ORIGINAL ACNWT-0067

OFFICIAL TRANSCRIPT OF PROCEEDINGS

Agency: Nuclear Regulatory Commission
Advisory Committee on Nuclear Waste

Title: 49th ACNW Meeting

Docket No.

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27288

LOCATION: Bethesda, Maryland

DATE: Thursday, December 17, 1992 PAGES 1 - 211

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UNITED STATE NUCLEAR REGULATORY COMMISSION'S

ADVISORY COMMITTEE ON NUCLEAR WASTE

DATE: December 17, 1992

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UNITED STATES OF AMERICA

NUCLEAR REGULATORY COMMISSION

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ADVISORY COMMITTEE ON NUCLEAR WASTE

49th ACNW Meeting

U.S. Nuclear Regulatory Commission
7920 Norfolk Avenue
Conference Room P-110
Bethesda, Maryland

Thursday, December 17, 1992

The above-entitled proceedings commenced at 8:30 o'clock a.m., pursuant to notice, Dade W. Moeller, chairman, presiding.

PRESENT FOR THE ACNW FULL COMMITTEE:

D. Moeller M. Steindler
W. Hinze P. Pomeroy

PARTICIPANTS:

R. Major	G. Gnugnoli
L. Deering	M. Nataraja
C. Abrams	S. Schofer
R. Ballard	J. Linehan
S. Young	K. McConnell
G. Stirewalt	

P R O C E E D I N G S

MR. MOELLER: Good morning. The meeting will now come to order. This is the first day of the 49th meeting of the Advisory Committee on Nuclear Waste.

During today's meeting, the committee will be briefed by the staff on their evaluation of DOE's requested resolution of SCP Objection No. 1. We will be briefed by NMSS on the results of the geological cross-section balancing activities. We will hear a report by ACNW Senior Fellow Steven Mays on the November 18-19, 1992 DOE Workshop on the Use of Expert Judgment. We will hear a report by the Chairman of the ACNW working group on Total System Performance Assessment. We will review upcoming committee activities and future meeting agenda, and we will discuss and try to draft and complete several proposed ACNW reports.

The Designated Federal Official for the initial portion of this meeting is Richard Major, who is seated to my right. The meeting is being conducted in accordance with the provisions of the Federal Advisory Committee Act. We have no written statement nor have we received any requests from members of the public to make oral statements at today's meeting, other than those previously mentioned.

However, if there are people here who desire to make a statement, please simply check with one of us, and we will provide time to you to make such a statement. If you

do make a statement, please go to one of the microphones, identify yourself, and speak with sufficient clarify and volume, so that you can be readily heard.

Before proceeding with the first agenda item, we would like to cover several brief items of possible interest. Ms. Sertia Sanders is on a three-month rotational assignment to the ACNW ACRS Office as part of her intern program as a new NRC employee. She will be introduced later.

Another item of general interest is that the average collective radiation dose per nuclear powerplant of those licensed by the NRC showed a 24-percent decrease during calendar year 1991 as compared to 1990. The average collective dose per plant was 253 person REM, which is down from 333 from the previous year and represents a lowest average in 22 years.

MR. POMEROY: Dade, I read that, and I wondered. Do you know if there is some specific reason why that should change, or is that a slightly abnormal fluctuation?

MR. MOELLER: No. At least my observations of the trends have been that they have been decreasing quite continuously, year by year, and if you go back to the years immediately following the TMI accident, when all the backfits and so forth were required, that raised the dose tremendously.

MR. POMEROY: Right.

MR. MOELLER: Then, since then, there have been fewer backfits.

But, also, they have extended the life between refueling outages. I mean, it used to be regularly, what, 12 months or so? Now it is 18 or more months.

MR. STEINDLER: Is that number normalized to the number of megawatts generated?

MR. MOELLER: No. This is simply per plant.

MR. STEINDLER: Just per plant?

MR. MOELLER: Yes.

MR. STEINDLER: So you don't know whether or not they are functioning?

MR. MOELLER: Oh, they do have the other data, which are available and are calculated, and perhaps we could look up those data. But, yes, the doses have been coming down.

Organizations such as INPO sets targets or goals, and it is part of their policy of seeking excellence, and there is a lot of push -- constant push -- to have the utilities do a better job, do better planning, and so forth.

Other items of possible interest, you have been provided with a list. On No. 2, we were provided with a report from the Board on rad waste management of the National Research Council, and it was entitled "Review of

Analyses by the U.S. Department of Energy of Selected Technical Issues in the Environmental Protection Agency's Standards for High-Level Radioactive Waste," 40 CFR 191. This was a review of the WIPP standards, as I understand it, and I have not yet read it, but you are being provided with copies.

There was a memorandum of November 6, 1992 from the LSS Administrator on the status report of the work of the Center for Nuclear Waste Regulatory Analyses to help establish a priority loading schedule for backlog licensing support system material. The committee may want to learn more about this.

On the basis of the Administrator's review of the Center's report, he has concluded that this may not be the best method for determining priority categories, the method that the Center proposed. As I say, I know members of the committee are very much interested in this databank, and we may want to discuss that more.

Item No. 6 on my list is one that we have discussed before, this repository operational criteria analysis. At this point, I would simply ask if the committee wants a briefing on that, just to think about it.

On the third page of the list, Item No. 7, regulation of major materials licensees, the NRC staff is preparing a staff action plan for regulating major materials

licensees, and they initially were looking at whether materials licensees, NMSS licensees, might in a sense self-regulate themselves or self-evaluate themselves, much the same as INPO does for the nuclear powerplants.

Then, having made that suggestion, as I read the report, it said that the NMSS licensees are so diverse, and many of them are small in number; that it might not be possible for them to set up an independent evaluation group.

MR. STEINDLER: Let me just comment. If you recall, one of the problems that was faced when the near criticality incident happened at the GE Plant is that there was a difference in the viewpoint between the licensee and the NRC people whether this was a significant incident or not, if that is an example of the difficulties that exist in communicating the same set of regulations to people.

I think it might be quite premature to have these folks regulate themselves.

MR. MOELLER: We will talk about this more later. I really don't know where it fits in or what approach we should use. But if you look at some of the data reported, say, for 1991, it shows that -- and don't hold me to this number -- but there was something like 31 overexposure among licensees during that year. Not one occurred at a nuclear powerplant. They all occurred at NMSS licensees.

We have distributed it. There was a death,

several weeks ago, in Pennsylvania due to a misadministration of radiation in a medical facility. So, again, it is not rad waste, but it is certainly NMSS.

Other things. Item No. 9, residual contamination limits, there have been several reports recently issued, and the committee may want to follow up on these.

The Health Physics Society has a committee on scientific and public issues, and they have issued a draft position statement on standards for permissible radioactive contamination limits for site cleanup or restoration.

Offhand, I thought it was a very well-thought-out statement. It is only three pages long. We may want to discuss that with them or the staff may want to.

What kind of rulemaking is it?

MR. MAJOR: Enhanced participatory rulemaking.

MR. MOELLER: Enhanced. As the enhanced participatory rulemaking proceeds.

Then, one of the staff, NRC staff, published this paper. It is John L. Minz, who published this paper on disposal of slightly contaminated rad waste from nuclear powerplants. We have also distributed that to you.

Then, we were provided, six months ago, with this RES RAD code, which is a code for calculating or for implementing residual radioactive material guidelines.

Dr. Okrent, I guess, yesterday was saying to the

staff when we were talking about performance assessment to the DOE staff how confident are you in moving from radionuclide release limits over to dose.

Now, I am sure -- or I am almost sure, a part of this code, probably, would be directly applicable. They use the Ditty Code. Again, I don't know the degree to which the committee wants to get into it, but we could have someone come in for an hour some day and review with us the Ditty Code. I don't know anything about it.

I think I know something about it, but I am not personally familiar. Maybe we ought to have them come in and tell us about the Ditty Code.

The U.S. Council on Energy Awareness, Item No. 11, in late 1991 created a communications network for the nation's low-level radioactive waste generators. I don't know much more than what it says here. It says they will serve as a clearinghouse for information and is a focal point for an industry-wide network through which waste generators can share material strategies and techniques.

Now, that would mainly be, I think, nuclear powerplant licensees, because that is what the Council does. But maybe we ought to find out what they are doing.

MR. POMEROY: There is also a citing of low-level waste disposal facilities.

MR. MOELLER: Yes. So flag that as a possible

follow-up item.

Item No. 12 simply was my attempt in reading these reports of recent weeks about the different states abandoning proposed low-level or suggested low-level waste sites. We have heard from Connecticut and Illinois. Are these lessons to be learned? Perhaps the NRC staff is following that.

Under Item No. 14, these are just miscellaneous that I do hope tomorrow morning we can come back and discuss some of these.

Yes.

MR. STEINDLER: Let me just make a comment. Is it worth our while to talk about the Zero Release concept and what implications there are for ever being able to implement this? Clearly, if you follow the words and you extrapolate it, then the answer is you can't do that.

On the other hand, I don't have a good reading on whether or not things that are labeled Zero Release are, number one, that and, number two, whether they have a time limit associated with it.

It is unlikely to go away. It is certainly a populist, albeit, unscientific and irrational view. On the other hand, I think the Commissioners need to deal with it.

I would not be too startled to have one of the Commissioners point their finger at this committee and say

tell me what it is and tell me what we ought to do about it.

My view is that I think we ought to look at it and become sufficiently knowledgeable, one, and understand what the various states are saying and, two, identify potential strategies within the licensing framework, ignoring the fact that some of these are agreement states; that one might be able to use to at least overcome that aspect of it. I think it is a topic we ought to look at.

MR. HINZE: Is there a scientific rationale given by any of the states for this that would give us some meat and potatoes to chew on?

MR. STEINDLER: I don't know. I would guess that the scientific rationale is the one that says any radiation is bad for you.

MR. HINZE: It is very hard to investigate that kind of thing.

MR. STEINDLER: I will leave that to the Chairman whose business it has been for years to do this.

MR. MOELLER: I think that is a good suggestion. We have flagged it, and let's come back to it.

On page 7 under miscellaneous items, I just wanted to be sure you realized that the ACRS in a letter to the Chairman on revised regulatory analysis guidelines -- this was a letter or report of November the 12th -- has challenged the $1,000 per person REM. So, certainly, we

should keep up with that.

Then Item C, the DOE has given these four research grants to develop the application of robots or robotic technology to radioactive and hazardous waste management. I would be interested in what those people do or what they propose to do.

The Item D that I flagged, the NRC staff accompanied the Conference of Radiation Control Program directors when the directors went out and reviewed the Alaska RAD Protection Program. That was new to me.

We have been briefed on how the NRC goes to the agreement states and evaluates them, but the Conference of Radiation Control Program directors goes, and the staff goes with them.

I would like to know more about that. It was totally new to me.

On the last page, Item No. 15(b), the NWTRB is meeting on January the 5th and 6th on the systems implications of interim storage of spent fuel, which Marty has talked about many times.

You would have been pleased, Marty, on either Monday or Tuesday when the question was raised. I forgot who raised it, but it was what do we know about spent fuel, do we know its chemistry, do we know the integrity of fuel that has been stored in a pool for six years and so forth.

We should have thought of something like that at one time.

For (d), I don't think this an audience necessarily that we should attempt to appear before, but the university campus radiation safety officers, my guess is there are several hundred of them. But they meet every two years, and their fourteenth meeting is in Nebraska in June.

I flagged it, because I was just saying to myself is there anything that we would have to say to them should we offer to appear on the program. Offhand, I would question it.

Then, I gather, Item (e) has been delayed from what I wrote, Item (e) being the briefing on the RES High-Level Waste Research Program, which I now gather is delayed.

Let's plan to come back to those things later.

MR. STEINDLER: May I add one or two things?

You all may have noted the action taken by the Commission on approving the Fort St. Vrain D&D.

MR. MOELLER: Right.

MR. STEINDLER: The interesting thing to us, I think, is that process is going to generate two kinds of what I call interesting waste. Obviously, the D&D waste is one, and the tritium and the liquid waste, which is going to get diluted and dumped, is the other.

I have read very briefly by the second document

that, I guess, was the basis of the Commission action, and I find it a little bit surprising, because I guess I was operating under the general notion that dilution, in order to meet EPA or other criteria, was not a policy that was going to be allowed.

Unless I have misread it, which is certainly possible because I didn't have the whole decommissioning plan, that seems to be what they are planning on doing. I am wondering whether to not that is an issue we at least ought to look at to inform ourselves as to what the staff's policy currently is.

The other issue, eventually, I want to talk about is lack of progress of getting at the bibliography from the Center, which we requested three months ago.

I want to point out that, eventually, we ought to formulate as a new item some thoughts on how to approach a risk-based regulation principle for the disposal of waste in order to be able to have some input to a discussion between ourselves and the staff that, ultimately, will be translated into a position of the NRC through the National Academy in response to the requirements of the Energy Policy Act. That is going to require some study on our part in pulling together, I would say, the past five years or so of literature that we have, to say nothing of what has been printed elsewhere in the area on the topic of risk-based

regulations.

That is basically all I have.

MR. MOELLER: With respect to that last item, and I will have to search for it, the staff or the Commission, whoever it is, has put out something on risk-based, and it says specific attention will be directed to waste management activities. It is somewhere here in this stack. I have it flagged. It is another item. It is part of this thing that we need to talk about.

Any other comments? Bill or Paul, do you have any?

[No response.]

MR. MOELLER: Then we will move ahead with the first item on the agenda for this morning, and that is the NRC Staff evaluation of DOE's Requested Resolution of Site Characterization Plan Objection No. 1.

We have with us Charlotte Abrams and Dr. Mysore Nataraja. I gather Ms. Abrams will lead off.

Charlotte, welcome back. It is a pleasure to have you.

MS. ABRAMS: Since everybody has a copy of our Vugraph, is it all right that we sit at the table?

As Dr. Moeller said, I am Charlotte Abrams. I am going to lead off and do a little bit of introductory material, and then Raj here is going to actually give you

the main meat of the matter.

We are here to discuss our resolution or lifting of Objection 1 from our SCA.

MR. MOELLER: Excuse me. I have noticed in the recent staff writings that they now do, very straightforwardly, talk about closure: This item has been closed or closed out.

MS. ABRAMS: We are not using that term anymore, because the state actually objected to that term. So we are using "resolution." It is a matter of semantics, probably.

I would also like to clarify that this is just resolution at the staff level. This does not resolve anything at the time of licensing.

MR. HINZE: Excuse me, Charlotte. What does that mean?

MS. ABRAMS: That means that it was a staff concern, and it is resolved at the staff level. It is not something that we could hold the Commission to or the Licensing Board to. So, in other words, things like this could come up again at licensing.

MR. HINZE: I understand. In other words, there has been no joining of hands with the DOE; that there is no problem here.

MR. MOELLER: I guess, too, it is not a legal, because they are not a licensee. They are not an applicant.

MS. ABRAMS: That is correct.

In the second Vugraph here, page 2, I will just start with that. I wanted to give you and reiterate the definition of an objection, as we defined it when we wrote the SCA.

Essentially, an objection is a concern of such immediate seriousness to a particular area of the site characterization program that NRC would recommend that DOE not start work in that particular area until that objection is satisfactorily resolved.

DOE did commit to resolve objection-level concerns prior to proceeding with related site characterization work.

Now, I want to emphasize that the lifting of an objection means that concerns related to that objection at this particular point in time are considered to be resolved. The staff is also obligated to continue to evaluate activities related to an objection.

If we see that it is warranted to reopen an objection based on new information or analyses, we will reopen an objection.

I would also like to mention that Part 60 required that the SCP include a conceptual design of the geological repository operations area and the extent of planned excavations for the characterization of the site.

DOE still has that commitment on the books and

owes NRC a final design. This can be presented either in a progress report or the progress report can contain information on a referenced document that would have information on the design.

MR. HINZE: In view of this, would that be reviewed and comments made back to DOE?

MS. ABRAMS: Yes. We expect to review their ESF design. In their discussion, we expect to see how the design is going to tie into their testing program. We would want to see a discussion of the potential impacts of waste isolation and a discussion of the design of a repository and how it would tie in.

MR. HINZE: Aren't we getting very much locked into a design, though, by the very fact that they are starting to cut rock and prepare portals?

MS. ABRAMS: We are reviewing incrementally their design. For example, the staff has attended a 50-percent design review and a 90-percent design review of the portal. So the staff does attend --

MR. STEINDLER: I am sorry. Of what?

MS. ABRAMS: The portal.

MR. POMEROY: The portal?

MS. ABRAMS: Yes.

MR. POMEROY: Does that mean when you have attended these briefings that you have signed off on those

points?

MS. ABRAMS: There is not a signing off, no. We do review them. Technical staff attend these. The QA staff, the Quality Assurance staff, is also reviewing our observing DOE audits of the implementation of DOE's design control process. So we do have different mechanisms for looking at how they are dealing with it.

I will go into some of the chronology.

MR. HINZE: If it isn't out of place, I would assume from what you are saying then and the fact that they are proceeding that you have no objection in terms of the review that you have done so far and what they are doing at the portal and in the design.

MS. ABRAMS: That is correct.

MR. HINZE: Does that include the staff?

MS. ABRAMS: They are doing fault investigations at the portal, and we would expect that.

MR. HINZE: You would expect them --

MS. ABRAMS: We would expect that they would do the fault investigations, and you are right, at this time, they have identified potential faults in the area of the portal.

MR. HINZE: How close are you staying to that, so they don't waste money? I am sure that they are very much concerned.

MR. STEINDLER: That is not the function of the staff.

MR. HINZE: No, but they certainly want to make certain that they shouldn't proceed with the development of ESF, without the NRC's review of it.

MS. ABRAMS: We held a site visit to look at their trenching activities in the area of the portal and the potential surface facilities in September of this year. At that time, we did look at the portal area, too.

Our on-site representatives maintain, really, a day-to-day update on what is going on at the site. So we do maintain a constant vigilance.

MR. HINZE: Great.

MS. ABRAMS: I am going to go on and into some of the chronology behind Objection 1. I will go through this pretty quickly, unless somebody has a question.

In December of 1988, DOE issued their Site Characterization Plan. In July 1989, the NRC issued their Site Characterization Analysis, which Objection 1 was one of our concerns.

Also, not on this list, but in July 1989, there was a meeting between NRC, DOE, and the State of Nevada also intended, and it was a meeting to discuss what NRC expected with respect to the ESF design. The discussion included applicable regulations and NRC's approach to the review of

the design process at that time.

In December of 1990, the NRC received DOE's responses to our SCA, but at that time they did not request closure of Objection 1.

In January 1991, the DOE issued the Calico Hills Risk Benefit Analysis, which discussed in some way the analyses on the penetration of Calico Hills. This was really not a technical report. It, more or less, consisted of a multi-attribute utility analysis.

In July 1991, DOE issued the exploratory study facility alternatives, and in that they listed 34 alternatives. I will discuss those.

In September of 1991, NRC issued a letter requesting information on how the CHRBA, which is Calico Hills Risk Benefit Analysis, and the Exploratory Studies Facilities Alternatives Report address SCA open items.

Previously, we had received these two documents without a request to review them from DOE.

MR. MOELLER: When did DOE request that Objection No. 2 be resolved? It won, and I just wondered what --

MS. ABRAMS: I think it goes back the July 1989 meeting, but Ken Hooks, the section leader of our QA staff, may have a better date on that.

There was discussion of the resolution of that QA objection, actually, in July 1989. It was lifted in March

of 1992.

MR. MOELLER: Like in the December 1990 DOE responses, you say they did not ask for closure of Objection 1. Did they ask at that time for closure of Objection 2?

MS. ABRAMS: No.

MR. MOELLER: No? Okay.

MS. ABRAMS: I think Ken is looking to see if he can give you an exact date. I don't know if we can give you an exact date on that, because there were a lot of pieces to that.

MR. MOELLER: Right.

MS. ABRAMS: In September of 1991, there was a DOE/NRC technical exchange on the exploratory studies facility. At that time, DOE explained their design control process, and they provided samples of how they plan to implement it. They also presented some milestones for the exploratory studies facility work.

In November of 1991, DOE requested closure of Objection 1. But, at that time, we didn't feel there was sufficient information, and, in March 1991, DOE provided the additional information to support closure of Objection 1. That was in the form of a letter, and there was also what we would call a walkthrough that directed the staff to particular portions of the CHRBA and the exploratory studies facility alternatives that would address our concerns in

Objection 1.

So the staff initiated a review of those sections of those report and the supplementary information provided in the March 1992 transmittal.

In November 1992, the NRC staff concluded that Objection 1 was resolved based on the review of that information and also the information in the ESF report and CHRBA. At that time, they also determined not to review the entire Exploratory Studies Facility Alternatives Report and the CHRBA, as information in those reports with regard to the design and alternatives was really influx. The DOE is still revising their design.

At that time, they tentatively had a preferred alternative, but we had not seen officially what their alternative was.

MR. MOELLER: Excuse me. Help me with that. A few minutes ago, you said there were 34 alternatives?

MS. ABRAMS: That is correct.

MR. MOELLER: Then you are saying now they narrowed it down to a No. 1 and No. 2 or an A and B?

MS. ABRAMS: They narrowed it down to, I believe it was, Alternative 30, the preferred alternative.

MR. MOELLER: Of the 34, they chose 30.

MS. ABRAMS: Of the 34, they chose Alternative 30.

MR. MOELLER: As No. 1 --

MS. ABRAMS: As their No. 1 --

MR. MOELLER: -- choice?

MS. ABRAMS: -- choice.

Now, actually, they have modified that alternative somewhat at this time.

MR. MOELLER: Thank you.

MS. ABRAMS: But we have not officially seen it.

I would like to also emphasize that we plan to really remain abreast of what design changes DOE is conducting through a review of the progress reports, where they should be providing us that information and our participation in these design reviews.

The NRC staff will also participate at DOE audits of the ESF design review process to determine that DOE continues to implement its design control process.

We also plan to request to review DOE's selected design options and the rationale for the ESF layout, and we would expect DOE to have this information to us in a timely fashion, so that we can get our feedback back to them in a timely way.

MR. HINZE: Charlotte, has there been any change of the study plans or do you perceive any change or revision of the study plans as a result of the new ESF and modifications that are being made to the one that was selected?

MS. ABRAMS: Originally, the first five study plans with the NRC received were plans for work within the ESF, and they were based on the conceptual design that was presented in the SCP, and the NRC staff at that time elected not to review those, because that design was being changed.

We would presume that they would be changing those study plans to reflect their new design in the tests that they would be conducting in their new designed facility before they transmitted those study plans to us.

MR. HINZE: Do you have any idea when you are going to be seeing those study plans? Is that in that September schedule?

MS. ABRAMS: We saw a tentative schedule, and some of those will be coming to us in mid-1993, and I think there may be one coming in early 1993.

I also would like to add that one of them, we did get a revision of already, and we are electing not to continue to review that one, because it still does not reflect the new design.

MR. HINZE: I wonder if this is the place to ask: Is the staff satisfied that the tunnel-boring-machine approach is going to provide the kind of geological information that was anticipated from the more conventional drill and blast -- I shouldn't say "conventional" -- the more classic drill and blast procedures? Has that

evaluation been made on any review process or is this an ad hoc decision?

MS. ABRAMS: It has been discussed at length by the staff. We don't want to tell DOE how to conduct their excavation. At the same time, we do want to see them be able to gather the information that is needed.

I don't want to speak for the technical staff here, but there are tradeoffs with each method.

MR. HINZE: Sure. As I recall at our Las Vegas meeting, there was a discussion about this, and there was some talk of a report. Subsequently, we have received communication that there is no formal report by DOE on that topic.

I was wondering if you had a report from them that you were using to evaluate. Perhaps I am asking the wrong person.

MS. ABRAMS: We have seen no formal report, no.

MR. HINZE: Are you asking for one, so that you can make an evaluation on these study plans?

MS. ABRAMS: No, we have not requested one.

MR. MOELLER: Following up the comment by Dr. Hinze, time and tim again, we will see these statements that the study plans still talk about an exploratory shaft facility, and they have not been updated and so forth. I guess I don't have enough appreciation of how difficult a

task this is or how many people DOE has working on it.

How long ago was it that they shifted their idea from a shaft facility to the studies facility?

MS. ABRAMS: To the ramp concept?

MR. MOELLER: Right.

MS. ABRAMS: It has been a couple of years.

MR. MOELLER: Yes. Have you -- I know you have. In fact, they now say -- maybe it was in this material -- that we now have this statement that DOE will give you a monthly accounting or status report on all the study plans.

MS. ABRAMS: Yes.

MR. MOELLER: So, supposedly, maybe we will get a handle on this.

MR. HINZE: Might I request, as the staff proceeds with this concern about geological mapping that we be kept informed if you receive a report or you make some kind of review of the situation.

I think that Keith would like to make a statement, Dr. Moeller.

MR. McCONNELL: Keith McConnell, NRC staff.

There are two forms for our discussions in this area. One is the study plan on structural features within the site area, which we have inhouse. We actually have, I think, Rev 1 inhouse. But it addresses a shaft and drill-and-blast type of technology.

We have decided tentatively not to review that study, because there are indications from DOE that they are going to revise that study to reflect the tunnel boring machine. Therefore, they will be providing some level of detail about their methodologies for mapping in that revised Rev 2 of that study plan.

In addition, we have had internal discussions and also informal discussions about the pros and cons of tunnel boring machine versus drill and blast, and I think the conclusion that we came to is that both provide, I think, ample opportunity to collect the necessary data, but we are waiting on DOE's revised study plan to make a formal judgment on that.

MR. HINZE: Sorry for the tangent.

[Laughter.]

MS. ABRAMS: That is fine.

MR. STEINDLER: Let me continue to walk down that tangent for just a little bit more.

Is it clear to the DOE what criteria you would use to make comments about the acceptability of either a tunnel boring machine or the thing that is, I guess, called conventional mining engineering, drilling and blasting? In other words, what would you base your comments on?

MR. NATARAJA: Let me address that. I think the criteria, basically, depends on the waste isolation issues,

which we have raised earlier.

So the one that creates the least amount of disturbance and provides the required information is the preferred option. So that would be the basis for making comments.

MR. STEINDLER: Is that compatible with Dr. Hinze's question; namely, is that the mechanism that is suitable for getting optimized geologic-related information?

MR. NATARAJA: The preferred option, the 30 option, which is going to be modified, also has an additional shaft, if required, which can be added at a later stage. So, to satisfy the needs for the information that may not be obtained by the TBM method, they would have an additional shaft to gather the data.

So what we understand is there are going to be two ramps and optional shaft, which may be included at a later date, if necessary.

So, if there is some information that will not be gathered by one method, that will be supplemented by an additional shaft at a later date. But I don't think they have made a final decision on whether they will have two ramps and a shaft in the ESF or not.

MR. STEINDLER: Is it clear then that this kind of strange three-dimensional tradeoff is going to maximize rather than minimize the amount of holes that are drilled

into the repository?

MR. NATARAJA: These holes will eventually be integrated into the holes that will have to be done for the repository.

MR. STEINDLER: I see.

MR. HINZE: That allows you to get off the hook, I think.

MR. BALLARD: I am Ron Ballard.

I would also add that I believe DOE is considering drill and blast techniques for some of their testing areas. They aren't just relying on tunnel boring all the way. So they will have alternatives in their plan.

MS. ABRAMS: That concludes my part of the presentation, unless you have any more questions. I will turn it over.

MR. NATARAJA: I am Mysore Nataraja, the Section Leader for the Geotechnical Engineering Section.

My presentation starts from Vugraph No. 6. I will be covering summary of concerns, expressing the SCA Objection 1, along with the bases for those concerns. I will also cover the NRC staff's evaluation of DOE's response to SCA Objection 1. I will briefly summarize what we intend to do in the future in regards to following up on the open items related to this article objection.

I will go to Vugraph No. 7. This is basically the

summary of Objection 1. As for the DOE's proposed design presented in the Site Characterization Plan, the exploratory shaft facility -- I will have to clarify one thing here. The acronym ESF stood for exploratory shaft facility and now has been changed to exploratory studies facility in order to make sure that not all documents will have to be changed. They kept the same acronym.

So, if I talk about SCP or SCA, that stands for the exploratory shaft, and if we talk about the current activities of the future activities, that refers to the exploratory studies facility. Even if you are a little inconsistent in the terminology, it won't make any difference to the discussions today.

The exploratory shaft facility, according to the designs presented in the SCP, would become a part of the repository. Therefore, one has to be very careful in designing the exploratory shaft facility, keeping in mind the design of the repository of which this ESF would eventually become a part.

However, our review found two major concerns with the ESF, and they are summarized on Vugraph No. 7. No. 1, the SCP and its references did not make a convincing case that the design control process under which the Title I design of the ESF was performed was adequate. No. 2, the SCP didn't convince us that they had an adequate Title I

design for the ESF either.

Let me explain. For example, since the exploratory shaft becomes a part of the repository, all the regulations considered applicable to the repository should be considered applicable to ESF also. But we found there were a number of regulations that were not considered in the design of the ESF.

We also found that the design control process allowed some of the critical technical data to be ignored, which led us to suspect that the process suffered from CDS deficiencies.

In addition, the design itself had some serious limitations in that the proposed facility could impose serious restrictions to gathering sufficient data for site characterization and also for the repository design.

So it appeared to us that the ESF layout would require some major revisions to satisfactorily address the staff's concerns raised in the SCP.

MR. MOELLER: Now you are going to show us that these particular problems have been resolved?

MR. NATARAJA: Have been addressed adequately.

MR. MOELLER: Or have been adequately addressed, okay.

MR. NATARAJA: The next Vugraph No. 8, I will very quickly go over this one. There is no reason to spend a lot

of time on this particular Vugraph, because I am going to go over each one of these.

As you can see, there were a total of six bases. We also had a number of comments and questions that supported these six bases points. We are not going into that level of detail in today's presentation. We are only going at a broad level to say how these six bases points are addressed in the resolution.

Vugraph No. 9 talks about the first basis point. This deals with the Performance Confirmation Program, the 10 CFR 60(f), which deals with the Performance Conformation Program, and alludes to the possibility of conducting in situ risk package testing and in situ seal testing.

There is a need for considering some early inputs to the license application from such testing, but the staff review showed that the ESF design, as presented in the SCP, did not address these issues adequately. That was our first basis point.

Vugraph No. 10 talks about the second basis point.

MR. STEINDLER: Excuse me. Can I go back to No. 9 for a minute?

MR. NATARAJA: Sure.

MR. STEINDLER: One of the interesting aspects of the relationship between the site characterization program and plan and the DOE objections is that, as the site

characterization plan changes and not by just renaming things from shaft to study, but also actually doing things differently, the objections of the staff don't seem to keep track.

For example, has the staff thought about whether or not in situ seal testing remains a valid commentary, when the whole geometry of this exercise has been shifted?

MR. NATARAJA: Yes. The seal question will remain whether the entry opening is vertical or inclined. That will not change.

MR. STEINDLER: [illegible] shown that doing the kind of in situ seal testing is even a sensible thing to do?

MR. NATARAJA: The question of in situ seal testing has to do with the selection of materials and the long-term impacts of seal performance.

MR. STEINDLER: Right.

MR. NATARAJA: And that is a question that will be valid regardless of what geometry we have for the ESF.

MR. STEINDLER: That is almost exactly my point. Why is it that the staff required in situ testing in the first place?

MR. NATARAJA: Either they have to perform in situ testing or demonstrate and make a case that is not required. We did not see either one of those in the SCP.

MR. STEINDLER: You gave them the option?

MR. NATARAJA: They have the option.

MR. STEINDLER: They do?

MR. NATARAJA: We don't have to give it.

MR. HINZE: Raj, my recollection from these items is that there was a concern raised regarding the possibility of gullying as a result of erosion on the east side of Exile Hill, as it might affect the portal and the sealing of a portal. Has that been adequately taken care of in your view?

MR. NATARAJA: The location of the shaft has been changed.

MR. HINZE: This wasn't a shaft. This was a portal that was on the east side of Exile Hill.

MR. NATARAJA: I don't know. We only raised that question with respect to the shaft location in the SCP. We didn't have a comment related to that.

MR. HINZE: In the original design, there was the possibility of a portal on the east side of Exile Hill, and my recollection was that you were concerned about it at that time.

MR. NATARAJA: Was that part of the repository you are talking about or the ESF?

MR. HINZE: Yes, the part of the repository.

MR. NATARAJA: This was confined to the ESF, the

objection was, and not talking about the repository.

MR. HINZE: I see. But if it was a concern in the repository, why isn't it a concern in the ESF?

MR. NATARAJA: The objection was on the ESF and not on the repository, but if that question has been raised, that will be an open question that would be looked at again. There are a number of comments and questions, which are still open. The one that you just mentioned might be one of those.

I have to go back and look at that. Do you have the number?

MS. ABRAMS: Seventy-two. It is Comment 72.

MR. NATARAJA: Yes. That is probably still open, anyway.

MR. HINZE: No, that is resolved.

MS. ABRAMS: It is resolved.

MR. NATARAJA: It is resolved?

MR. HINZE: I guess I am concerned about why it was a concern regarding the repository and then resolved for the ESF with a portal design.

MR. NATARAJA: We have not reviewed the portal design or the Title II design of the ESF. So it might come back again. We have not reviewed the ESF Title II design in resolving lifting this objection.

MS. ABRAMS: We can also reopen any comments based

on new information.

MR. HINZE: There was a concern about gullying on the east side of Exile Hill. That seems to be very appropriate to continue that unless it has been taken care of somehow or another in their design procedures.

MR. NATARAJA: Now we are on No. 10.

In response to the staff's earlier criticism of the ESF Title I design, the DOE presented a document known as DAA, Design Acceptability Analysis.

The purpose of this document was to explain why the Title I design as presented in the SCP was acceptable. However, the DAA, both the document itself and the process used for its preparation, either did not adequately address the staff concerns or raise some new concerns.

For example, some of the technical reviewers who performed the peer reviews were themselves involved in the preparation of the documents. So this raised some questions about the independence of these reviewers.

We also found some deficiencies in the thoroughness of the reviews. For example, we picked up some of the reports that were reviewed by the peer reviewers and found that some errors were left there unchecked, and we also found that the 10 CFR 60.21(c)(1)(ii)(D) -- that is the adequate for alternatives -- we found that there was not an adequate consideration of these comparative evaluations of

alternatives to major design features that are important to waste isolation.

We also found that a known anomaly, which existed close to the proposed shaft location, was not considered in their design. So these were some of the concerns either which were new concerns raised when we looked at the DAA or some of the existing concerns which were not addressed adequately.

MR. HINZE: Raj, if I may ask, the last item there, does this mean that this is acceptable now because the shaft is no longer a part of the ESF, or is it because the staff has accepted the --

MR. NATARAJA: We are looking at how they address these concerns. If they detect something, what do they do about it? How do they follow it? Do they have a process to take into account these anomalies? That is all we are looking at right now, and we are convinced that they do.

MR. HINZE: Does this, in any way, indicate that there is not a fault at the shaft location? Does it indicate that at all?

MR. NATARAJA: No, we are not talking about that at all.

MR. HINZE: I just wanted to make that clear. Okay.

MR. NATARAJA: We are now on Vugraph No. 11, the

Basis Point 3.

An important consideration in planning the underground test facility is to make sure that there is enough space to conduct all the required tests for the durations of interest and that these tests do not interfere among themselves or with the construction activities.

However, the ESF, as presented, in the SCP raised numerous questions regarding these issues. Most of these questions were either not addressed or insufficiently addressed.

For example, the calculations showing thermal zones of influence did not account for appropriate test durations. Some of these tests would potentially continue into performance confirmation periods, but they only looked at maybe, like, 12 months or 18 months, some limited durations in calculating the zones of influences.

Also, the uncertainties coming from some of those numerical models and calculations were not factored in appropriately. They took the same test space and proposed this space to be used for different tests that would be performed in some sort of a sequence, but the SCP did not provide the rationale behind these decisions, how they would be able to do these tests in the same space.

They did not list some of the required tests, and the information was quite sketchy in many areas. So we

thought that there was inadequate consideration of some of these requirements.

We are on page 12. We have already discussed this question of this in situ waste package and the seal testing and the potential impacts. Again, it is a repeat of the previous concern in a different context. The SCP did not go into some of these details. They did not show that the testing was not required, and if that testing was required, they didn't show what the impacts might be.

No. 13, Basis Point 5, this was another sort of a detailed comment related to designs. Some of the ESF design criteria were not sufficiently addressed. One of the examples was the seismic design basis, and there were calculations of the quantity of water in the bottom of the shaft, the assumptions, and some of the other details there were questionable, because there was a lot of liability on the long-term performance of these drainage, where we had several questions about where they would be clogged, whether they would perform the way they are assumed.

Also, there was insufficient details about the liner removal from the shafts and what they might do to long-term performance.

The last Basis Point No. 6 was, of course, sort of a very broad concern. The layout of the underground testing and the drifting as presented in the SCP, in our view, did

not show much promise for adequate site characterization, and there remained a potential for gathering insufficient data.

In summary, although most of these concerns expressed in the bases points, all the supporting comments and questions by themselves may not be very serious as individual comments or questions, but when combined and viewed as a whole, they indicated a trend, and trend of a flawed process that could lead to an acceptable design product. That was our overall concern regarding this ESF as presented in the SCP.

After the staff posed this objection, a number of things have happened. DOE prepared the exploratory studies facilities alternate study, ESFAS, and they have also prepared the CHRBA, the Calico Hills Risk Benefit Analysis, but that was in response to an earlier CDSCP objection, the consultation draft objection.

DOE prepared sort of a cross-walk that Charlotte mentioned in which they showed us how the staff concerns were being addressed in different documents, where and how they were being addressed, and we were invited to observe DOE audits and surveillances.

We were also invited to observe their design reviews at different stages; 50 percent, 90 percent, and so forth.

We were also invited to participate in the DOE NWTRB, the Nuclear Waste Technical Review Board meetings, and during these meetings, ESF design issues were discussed in great detail. DOE explained during some technical exchanges how the regulatory requirements were being considered and how technical assessment reviews were being conducted by DOE.

So let's now look at the Vugraph 15, which is the staff evaluation of DOE's responses. The staff has evaluated the DOE responses to the objection looking at both aspects; the two main concerns, the one related to the process, the other one related to the adequacy of the design.

So let's first look at the process part. Since the objection was posed, the staff has had a number of opportunities to observe DOE's surveillances and audits, as I mentioned earlier. During these audits and surveillances, sometimes the QA staff have been accompanied by technical staff, if the particular topic was appropriate for technical surveillance.

In general, the staff found that these audits and surveillances were conducted properly and were approved QA procedures and found them to be acceptable.

I am continuing with the design control process on No. 16. During a technical exchange that was held on

September 16, 1991 that Charlotte mentioned in the chronology, DOE explained its modified design control process. We found that DOE had in place, documented, and approved procedures for considering specific design requirements.

For example, the staff had pointed out that 11 regulatory requirements had not been considered during the ESF Title I design. Now, DOE presented the response. They responded by revising the requirements documents to include these 11 additional regulatory requirements.

In their Waste Management Systems Requirements, Volume 4, which is WMSR, one of the acronyms for another QA document, these are umbrella-type QA documents which govern all the participants of the project, and they also revised the project level requirements document and the ESF requirements document. All these missing regulations were now included as applicable for the ESF.

The staff's concern regarding the lack of integration of technical data was also addressed during the technical exchange. DOE explained how that technical support document management plan, what they call TSDMP, and the RIB, reference information base, addressed this particular concern, and DOE provided examples of how this particular process is supposed to work.

The concern relating to the geophysical anomaly

near the location of the shaft was addressed by performing an assessment review as per quality management procedures, and recommendations were entered into what they call CRS, the common response status relation database.

Technical assessment review, recommendations were also entered into the ESF design requirements. So the staff found these actions to adequately address our concerns as far as the design control process was concerned. It does not mean that what they have done is good or bad or anything, but we do know that there is a process.

Earlier we had a suspicion that the process was not working, but now we don't have that suspicion any more about the process.

Vugraph 17 basically concludes saying that we are satisfied that the Title II design activities are now being performed under an NRC-approved QA program.

Now let's look at the second part of the concern, which has to do with the acceptability of the Title II design. So we are on No. 18 now.

As a result of our limited review of the pertinent portions of the exploratory shaft facilities alternative study, the CHRRA, and particularly looking at the current preferred option, the staff has found that the DOE has addressed most of the concerns raised by us in the SCA objection, at least in a conceptual way.

One of the major issues raised by us related to the consideration of alternatives. This was a big, big deal. We were making a big issue about the fact that they did not look at the alternatives.

Now, the current approach explicitly considers alternatives; for example, alternative modes of entry such as shafts and ramps; alternative excavation techniques, such as boring machines; and several other mechanical excavation techniques; plus the conventional drill-and-blast techniques.

DOE also considered alternative locations for entry. Alternative repository design concepts were associated with each given option of the ESF. So, for a given ESF, there was an associated conceptual design of the repository. So, in other words, they looked at a number of possible options.

They also looked at a number of different test strategies, like different levels of testing, two or four different levels of repositories, and how to gather data, and different layouts for the drifting, and different extents of drifting.

So, with the new approach, the proposed approach, we also see that the in situ waste package testing as well as the early seals testing can be conducted, if they so desire. They are not precluded.

If you remember, I mentioned that, in the previous approach, we had many concerns that such in situ tests might have been totally precluded because of some other design deficiencies and limitations.

We will go to No. 19. We also found that the proposed option has a better potential for gathering adequate data for site characterization and repository design.

As you can see, there is an increased stage of drifting. Now we have something like seven out of eight times the originally proposed drifting. It used to be about 10,000 feet. Now it is about 76,000 feet.

These drifts will intersect major geologic features and provide data on these, and we hope this information will be available in the license application.

The concern related to the test interference was one of the big concerns in the design limitation. There were tests which were too close to each other, and there were tests which were located too close to the construction activities, and, thereby, which might impact the tests and the test data.

Now, they address this concern by practically doubling the main test level area from 400,000 to about 800,000 square feet.

The final point there is that the preferred option

has a phased approach. So there is a lot of flexibility as to which way you want to proceed. You can do as much or as little as necessary. You don't have to overdo it, or you don't have to underdo it. You can do it in phases, and you can design it in such a way that you can proceed in any preferable option, in any preferred speed, to gather the required data.

So this flexibility was one of the questions that we had raised, and I think the phased approach addresses that particular concern.

In summary, we have concluded that the design control process under which the Title II design ESF is being carried out is an acceptable one to the staff.

Also, we have concluded that most of the bases points discussed earlier during my briefing have been satisfactorily addressed. In other words, the preferred ESF option addresses in a conceptual manner most of the important consents raised by the staff in SCA Objection 1.

This is based on our review of portions of ESFAS -- that is the alternative study -- and the risk benefit analysis and the cross-walk, and we have taken into account the information and knowledge that we have gained during technical exchanges, observation audits, surveillances, and observation of DOE-designed reviews, and also out attendance at the NWTRB meetings.

I am on the last Vugraph now, No. 21. Finally, I would like to conclude my presentation briefly by mentioning about our intended future activities related to the review of ESF Title II design.

As I mentioned earlier, there are some open items related to the comments and questions that supported the bases for SCA Objection 1. We will continue to monitor these open items and continue to evaluate DOE's responses to them as and when they become available to us.

Perhaps some of these might be done during a review of the study plans.

We will also review major design reports that would be prepared by DOE in support of the ESF activities. We will provide comments to DOE as appropriate.

We will continue to observe DOE's design reviews of different design packages, the 50-percent and 90-percent design reviews.

We will, of course, have a number of audits and surveillances, and both from the QA point of view as well as from a technical point of view.

That is where my presentation is going to be concluded. Unless you have any further questions, I will hand you over to Charlotte for wrapping up.

MR. MOELLER: Dr. Hinze?

MR. HINZE: Raja, as I look back on our initial

view of the exploratory shaft facility, it seemed to me that the emphasis there was a study not only of the repository horizon, but there, very definitely, was a major concern regarding wha remains to be a fundamental problem for matrix versus the fracture flow, particularly in the overlying horizons.

In that ESF, there were breakout zones and critical areas, and there were provisions made for a test to be made as the shaft proceeded down.

Does your resolving of this objection indicate that the NRC staff is satisfied that the portal, which obviously has a vertical component as well as a horizontal component, which is primarily a horizontal component, is going to provide sufficient opportunity to evaluate the characteristics, the geotechnical, hydrologic characteristics of the overlying horizons, so we can answer some of these very critical questions that remain?

MR. NATARAJA: I think this is probably too delicate a question for me, and I can refer to hydrology's staff here.

Bill, would you like to take a shot at it?

MR. HINZE: Let me ask a question before you refer. Is this a proper question at this point in time? We have not heard from the staff on the ESF.

MR. NATARAJA: What I would say is any sort of

investigation is not ruled out by this approach. We do not have answers to the questions. But I am saying, using this approach, if somebody wants to do a detailed hydrologic investigation, it is possible to do it, but we don't know the details yet unless we review not only the ESF Title II design, but also their test plan.

MR. HINZE: I was pleased, and I think most of the ACNW was very pleased with the analysis that was performed by the staff on the SCP.

We have seen some major modifications of that, and rightly so, and perhaps the most important is in the change from ESF 1 to ESF 2.

I am wondering if the staff is going to take this opportunity to look at whether there should be objections, comments, or questions related to that. Are we proceeding in a fashion, so that we do have the opportunity to provide the same type of analysis that we had in the SCA, as we have now with these revisions?

One of the cases in point is this one of evaluating the characteristics of the overlying horizons.

MS. ABRAMS: We maintain the ability to provide an objection to DOE at any time, and we expect to see information in the progress reports that would update the information in the SCP. We expect to see this at six-month intervals.

If we see information in a progress report that requires an objection, we certainly would make an objection.

MR. HINZE: Or a question.

MS. ABRAMS: Or a question or a comment, yes.

MR. HINZE: Are they being reviewed in a timely and detailed enough fashion, so that that can really be done? You spend a lot of time and a lot of effort on the SCA. It was great work.

MS. ABRAMS: We have a commitment to turn around our reviews on the progress reports within three months Now, the problem in the past has been we haven't received progress reports in a timely fashion.

The DOE is working to improve that situation. In fact, we have just received Progress Report No. 6, and close on the heels of that, we are going to get Progress Report No. 7. So they are working to improve their turnaround time.

MR. HINZE: It raises some red flags to me that I see no objections, that I see no comments. I see no questions, at least that I know of, that come out of these updates of the SCP. To me, the break from ESF 1 to ESF 2 is a major break, and a lot of attributes have been gained by that.

But it seems to me that my question really is: Has there been sufficient analysis that we can feel

comfortable with the ESF 2?

MR. NATARAJA: Excuse me. Charlotte. I might also add that, in our review plan for the review of study plans, also it provides us opportunities for raising comments, questions, and objections. So there are a number of stages in which we can raise an objection if we have a big problem.

Of course, comments and questions can be raised at many, many stages.

MR. HINZE: If that takes care of the process, could we move on to the question that I asked then regarding does the resolving of Objection 1 tell DOE that you as an NRC staff are pleased with the analysis of the characteristics, the geological, hydrological characteristics of the overlying rock formations?

MS. ABRAMS: No. I mean, that is not what the objection really dealt with.

The objection dealt with did they have a design control process in place.

MR. HINZE: Charlotte, there are some places where there really are questions regarding the studies. I refer you on page 4-2, Item (g). It states that there are other tests that have not been yet completely defined, et cetera. Information has not yet been presented to indicate if any of these undefined tests would be the main test.

MS. ABRAMS: That got to the test interference

question that Raj was talking about.

MR. NATARAJA: Without knowing what the tests are, it is hard for us to say whether there will be interference or not. It was in that context that we raised that.

But I don't think the SCP would go into the level of detail. The level of detail was only at the investigation level, but to go into the level of detail of study plans would be inappropriate according to the guidance that we have given to DOE. So we couldn't be criticizing them for not giving that level of detail.

But the reason for raising that, as Charlotte said, was to make a broader level finding whether or not there will be some interference. But, certainly, we need those kinds of detail, and we will definitely review those.

MR. STEINDLER: I was going to go back to the process, but go ahead, John.

MR. LINEHAN: John Linehan.

I just wanted to go back over this. We are lifting the ESF objection for two reasons. They have got an adequate design control process. In a very broad sense, we are satisfied with the way they are proceeding with the ESF options that they have narrowed down to.

There is still a lot of specific questions out there. We have recognized we need to send a letter to DOE; in fact, to tell them, hey, while you have satisfied our

concerns at the general level, we have lifted the objection. You are heading in the right direction. There are still a lot of details that we have to worry about and we have to look at.

In July of 1989, DOE indicated that, as they proceeded, they were going to be sending to us packages for the various designs. While we want to see those and we want to review them, we have recognized that we will need a revised conceptual design, like we had in the SCP to make sure that we can answer all of these various questions, like the ones we had on the original SCP.

The SCP is a living document, and through the progress reports referencing a document, they can get us or revise conceptual design that addresses a lot of these questions.

With respect to things about the hydrology of the overlying material, there should be study plans, and those should be integrated with this conceptual designing. There is still somewhat of a void there. We have lifted the objection, but they haven't made -- I would probably term it, they haven't made the SCP totally well, and they still need to get this information to us.

MR. HINZE: Thank you. This discussion has been helpful, and I really wanted to see it put in the proper framework, and I think you have done an excellent job of

doing that for me now.

MR. STEINDLER: Let me raise a couple of issues. First, Charlotte, you indicated that you maintain the option of issuing additional objections; presumably, based not on the original site characterization plan and its 6,000 pages, but presumably based on the progress reports that are supposed to come every three months on time.

MS. ABRAMS: Six months.

MR. STEINDLER: Six months, yes.

I guess I have a couple of comments. The progress reports that I have seen contain significantly less detail by what I consider to be an important amount than the original format and content of the site characterization plan issued by DOE, and perhaps that is appropriate, but it gives me a little pause when that is the document against which you are now going to raise objections, because your ability to analyze the details of why you raise objections and whether or not now is the time to raise it in the particular issue seems to me has been significantly fuzzed out to be able to do that with the same kind of precision as you did in the SCA.

The second point that I would raise is that the original Objection 2 dealt, in part, with the quality assurance issue. It was a process that you objected to that either wasn't there or it was inadequate. Yet, the quality

assurance program objection, one, was closed out as being apparently satisfactorily resolved by DOE's QA program.

If that is, in fact, the right interpretation, I draw the simplistic conclusion that you folks closed out Objection 1 too fast. Is that the right conclusion to draw?

MS. ABRAMS: We would agree, there has not been sufficient information in the progress reports, and we are on record as telling DOE that.

They don't have to include all the information and data in the progress report itself. They can do it by reference to other reports. We haven't seen that, and we do agree with you on that.

We also expect to see a certain level of detail and study plans, and we, as Raj said earlier, can issue objections on the information in the study plans.

MR. NATARAJA: I was going to say that the QA objection when it was closed, we gave them three reasons. There were three bullets.

One of the bases for lifting the objection was that we were satisfied that there was a design control process in place. So the part of this objection was sort of subsumed or contained in the other objection, also. It is related, except that we were monitoring this particular objection separately, because it was related to the design control, because the two were so closely related.

We had a problem with the design, and we thought that this might be the result of a poor process, which was also being mentioned in the QA. They looked at the overall QA program, and they were satisfied that there was a QA program in place.

They also looked at the design control process as a part of both the objections. So, even though timetable-wise it came earlier in the calendar, the design control process part was being looked at even as a part of the lifting of quality assurance objection.

MS. ABRAMS: The QA staff through the observation of DOE audits will be looking at the implementation of the design control process, and, in fact, so far through one audit, they have seen examples of that implementation.

I don't know if that gets to your concern or not about your comment about resolution too soon, but --

MR. STEINDLER: It is more an observation than anything else, because I think the point may now be academic.

The notion, however, that you accept a quality assurance program that is adequate for the purposes that you, as a practitioner or as a practicing reviewer have to use, is based on a large amount of faith, which it turned out in this case was not quite well -- at least not timely enough taken. That was my only point.

The other issue that I would just raise by way of a comment is that, in the last Progress Report No. 6, which is sitting in front of me here, I tried to extract out of that in a fairly straightforward and simple way what they are doing in the ESF, and let me tell you it is scattered throughout this document, and I am not sure that I could pull it all together without being a specialist in the business, which I am not, which may be okay for you folks who live with this thing every day who know eaxctly what you are looking for, but I found it a difficult reading exercise.

It seems to me that, coupled with the lack of detail, ought to prompt you to, again, raise the issue a little more vociferously with the DOE, perhaps identifying for them the kind of thing they ought to be putting into these progress reports.

MR. MOELLER: Dr. Pomeroy?

MR. POMEROY: Help me out a little bit. Your Basis No. 5 for the objection, your first bullet says that some ESF design criteria are not adequately addressed, including the first sub-bullet, which is seismic design basis.

I understand, I think, that what you have said here is that, based on some process -- you haven't said anything about the seismic design basis. All you have said

is that the criteria for establishing those design bases are now adequate, and it is on that basis that you have said that this -- I think you have said all of these bases, including No. 5, are now resolved.

MR. NATARAJA: No. Actually, the seismic design comment is still open. Some of these comments have been pushed to another side. But the seismic design basis is still open.

MR. POMEROY: What comment is that? Do you have that offhand?

MR. HINZE: Question 58.

MR. POMEROY: Fifty-eight.

MS. ABRAMS: Actually, there is a Comment 121, too. SCA Comment 121 remains open.

MR. POMEROY: I will accept that, but I still have a problem. You are saying that this has not then been adequately addressed, because you are holding open the issue, and, yet, you say that you have resolved the basis?

MR. NATARAJA: What we did was when we pulled up this objection together, we not only looked at the broad concerns, but we also looked at any supporting comments and conclusions that might go to enhance or make a bigger case for us to be able to pose the objection.

So, in doing so, we put together any comment that had anything to do with the ESF. It so happened that the

seismic design criteria was also related to the ESF. I would say this, by itself, is not a big deal. You know, that is something that can be handled by making this comment, and they can go back and give us the seismic design criteria, and we can review it, and that will be the end of the story.

But I was trying to explain to you how we got that as one of the bases points.

MR. POMEROY: Yes.

MR. NATARAJA: There were a number of little things that added up and said, hey, here is something. When you put it altogether, it shows a very big concern.

There could be several opportunities for us to review this particular issue later on.

MR. POMEROY: Certainly, I agree with that.

I guess I am concerned, have you received any information from DOE that would help you in addressing that particular question?

MR. NATARAJA: No, not really. I don't think the ESF design has gone to that level yet.

MR. POMEROY: No, I don't either. In fact, I know they haven't.

Thank you.

MR. MOELLER: I wanted to comment on several things, because I find the process troubling.

John Linehan has said to us that, on the basis of NRC's observations of the broad approach being used by DOE, you feel confident that Objection No. 1 can be resolved or is withdrawn and is resolved.

Then, though, when we look back on what the two of you have said to us this morning, you find that your confidence that the comments and questions and so forth underlying Objection No. 1 -- your confidence that those things have been resolved is based on audits, observations, surveillance, and so forth. It is not really based on written documentation, or that is the impression I receive.

Let's just look at a few here that I read, which trouble me. I say trouble me in the sense of the lack of thorough documentation of what you are doing.

Comment No. 57 says, and I am quoting, "NRC's review of DOE's response to the SCA recommended that progress toward resolution of this comment would require DOE to submit study plan 8.3, et cetera. This study plan has not been submitted. The NRC staff considers this comment resolved."

I have trouble with that. It is contingent upon them submitting it. They didn't submit it, but it is resolved.

Let me just go on. Go to Comment 72. It says, "The SCB did not include analyses to evaluate the need for

seals and repository shafts and ramps. The NRC staff recommended DOE plan at sealing program on the basis that seals will be needed."

Then the second bullet says, "Although no details of the seal tests are provided in the ESF FAS, the information provided by DOE indicates that DOE considers that seals may be required." So they have sort of hinted they may be required, so this comment is resolved.

See, I don't hear them saying positively they will be used or not, but I hear a bottom line that the comment is resolved.

MR. NATARAJA: Can I address that? That made an assumption that it is not required in the original SCP. I mean, at least that is the impression we got is that the seals are not required.

But now that I am reading that seals may be required and then they have provisions for performing seals tests if they are necessary, in the original plan, we had the question whether they would even perform seals tests.

Now, we don't have that question anymore, because they have been listed as required tests that can be conducted. They have the space to conduct it. Their scheme allows them to conduct it. There is a recognition that it may be necessary. That is what we are looking for.

Now, we will look at whether or not they are going

to do it when we review the actual test plan.

MR. MOELLER: Okay. That didn't come through to me.

Comment No. 128 says that several applicable Part 20 requirements have not been considered in the ESF Title I design. Then you resolve it by saying the NRC staff proposes to evaluate whether or not design criteria based on Part 60 requirements have been developed when you review the Title II design.

So you are postponing it from Title I to Title II?

MR. NATARAJA: Again, it is the same situation. They had not even included them as applicable to ESF. Now all the requirements documents list them as applicable to the ESF, and, therefore, we sort of trust that they are going to include this in their concentration of the detail of Title II design, and that is what we are going to verify when we actually review the Title II design.

MR. MOELLER: Actually, you have, in a sense, resolved Comment 128 by moving it to Comment 130.

MR. NATARAJA: Right.

MR. MOELLER: It would have helped me to have had -- and maybe it was somewhere and I missed it -- to have had a chart that said 56, 57, listed each one, and then told me exactly the cases in which the resolution was accomplished by shifting it somewhere else.

Now, to go to your last bullet under 128, and I just couldn't understand the English, "The NRC staff recognizes that this comment is a special case of comment 130. Therefore, it will be tracked together with more of that general comment." Obviously, some words or something were left out.

But now, one that really troubled me is Comment 132, and it says, "The requirements of 10 CFR 60," and so forth, "in particular, have not been adequately addressed in the Title I design."

Then your second bullet of your response, the second sentence says, "The original SCA comment noted that the requirements were not adequately addressed. However, the consideration, description, and evaluation of major design features" -- probably should have said -- "are contained in the ESFAS."

In other words, the way I read it is you are almost like a teacher, and I see myself doing this as a teacher. I have a student that I know is smart, but he or she didn't really do too well on this exam. So I sort of hunt for the right answers to give them credit for it.

You say DOE didn't respond, but by hunting around, you have found what you are looking for, and so you are going to give them credit for it. That is the way I read it.

MR. NATARAJA: They also provided a cross-walk, as we explained earlier, how each particular NRC comment was addressed and where it was addressed. So we really didn't have to hunt that much, because they told us where it was. So we could go to that particular --

MR. MOELLER: See, I think if I looked only at the written word, I would be unhappy, but having heard about the audits, the surveillances, your technical exchanges, which backed this up, then it is a little better.

SCA Question 61, your second bullet said procedure PPO 317 appears to be sufficient. So you are going to resolve it. It appears to be sufficient. It either is or it isn't.

So my problem, to repeat, and I am fortunately -- or unfortunately, I am not involved in that, but if I were a lawyer or a technical reviewer looking for a good solid documented written case to back up your action, I can't find it. That is my problem.

MS. ABRAMS: Keep in mind, too, this objection was a very difficult one to deal with by virtue of the fact that we did reference comments and question as bases.

Many of the comments and questions alone would not make an objection by any means. The two major points that make the objection are really in Comment 127 and the concern about the design control process. The other things were

added information to document problems with their process. So, in a lot of cases, we have left the comment open.

Again, if DOE shows that they are going to address these requirements, then we have to assume they are at this time, but we maintain the option if we see they are not, we can produce another comment.

MR. LINEHAN: Dr. Moeller?

MR. MOELLER: Yes.

MR. LINEHAN: You make some very good points, and I think what we need to do in the future is, even though some of these things are somewhat open-ended and there has to be follow-up, I think we have to more clearly state why at a particular point in time, to document why we are satisfied and why even with the follow-up we can take the action we are taking at this point in time.

MR. MOELLER: I would accept a statement that says we have had many technical exchanges, observations, audits, surveillances, and so forth, and also that we have carefully reviewed the DOE plan or their program, and we have come to accept their broad goals and the broad way in which they are doing it. On the basis of that, we are withdrawing the objection.

I wouldn't object to that. But you have indicated to me you are documenting the withdrawal and that the withdrawal was based upon documented evaluations of separate

comments and so forth, and I am not happy with them as they are done.

MR. POMEROY: Could I just follow that concern? I think it is very, very real. I guess I am specifically concerned, for example, if you attended a technical exchange where you have no transcript, where you have no written record, where although other people could attend, they don't necessarily attend, and they can't recover what was said. Could you use that as a basis to assist you in closing?

MR. LINEHAN: One of the things that does exist is when there is a technical exchange, there is a record of the meeting.

MR. POMEROY: It is not a verbatim record, John.

MR. LINEHAN: No, it is not a verbatim record.

MR. MOELLER: No, but it is written adn signed by both groups.

MR. LINEHAN: It is a written summary.

What I was going to explain is that, if we are relying on something like this, we would document very well exactly what we felt happened and why we were or were not satisfied.

It is more of a case of observation of QA audits and design reviews. Particularly, with the QA audits where they have looked at the implementation of the design control process, we write fairly detailed audit reports that support

those. Those, I think, would very well document any conclusions, any findings we were making.

But, again, to someone that is just looking at this letter, I can understand without seeing those attached. It is not transparent enough what the total record is.

I think this problem is really confused because of the fact that the ESF has changed.

MR. HINZE: That really complicates the situation, because it is very difficult. It was really critiqued on the basis of a design, which would present in the SCP, and now that design is no longer valid.

MR. STEINDLER: It is altogether different.

MR. HINZE: That is right, and that confuses the issue. Right.

MR. MOELLER: I hope I fully appreciate the multitude of comments, questions, subparts that you had to consider and try to put it altogether.

I have something else that is confusing, and it is not your responsibility. But the NRC letters will say we consider such and such resolved, and DOE will come back, and they will list a whole bunch of things. They say we consider all of these resolved.

Good for them. It is confusing for me.

MS. ABRAMS: With respect to that concern, we intend to inform DOE that the decision on resolution of our

comments and questions is on our part.

MR. MOELLER: Right. Thank you. That would help a lot.

MR. STEINDLER: Can I comment? Perhaps I don't appreciate the importance attached to this whole process. Let me rattle off for you some issues that we have raised from time to time.

You folks went through and analyzed this 6,000-paged document that was laid in front of you and did, I think, a very commendable marvelous job. There are lots of open issues and questions -- well, used to be questions, and heaven knows how else you call them, that were initiated and gradually attacked, or, shall we say, resolved anymore.

What has never been made very clear is who cares, and that sounds a little callous, but let me translate that. I have not seen a sheet of paper from anybody that says it is necessary for the licensing process to have an applicant that has no outstanding comments, objections, or questions.

It isn't even very clear to me whether or not DOE must, compelled by something, answer all of these things. We have raised that question from time to time, but that issue, I think, remains open in some fashion or another.

So I guess I view that in the same category as I view the general notion of mandatory filing and approval by the staff of study plans. It seems to me that the process

is a lot more permissive than that. It seems to me that DOE has the option of proceeding without a resolution of objections, of proceeding without the filing of a study plan, of proceeding, having filed a study plan, without resolution of any objections you might have to the study plan at DOE's own peril; namely, you can come back later in the licensing process and say, "Fellows, you didn't do it right. You didn't do what we asked you to do. We told you it was bad, and now it is really bad."

Now, if that is correct, what that does is it throws the burden of technical analysis of their action back on DOE. If all of that makes sense, and I haven't strayed from the correct path, then I view the resolution of objections as useful for trying to minimize the twists and turns of a scientific or technical investigation that really plows new ground; namely, more heads are better than fewer, but that is it. That is the only important I can attach to this with an eye toward the licensing process.

It is a risk that DOE, it seems to me, has the opportunity to take if they so elect. That risk, if they come out right, is fine, and it doesn't have any negative consequences. If they screw it up because you, in fact, had a valid point here or there, then they have to take their lumps in 1998 or 2001.

MS. ABRAMS: 20004.

MR. STEINDLER: Is that a rational interpretation?

MS. ABRAMS: We are on record as saying that the comments and questions, if not resolved, may result in a less-than-complete license application. So DOE does run that risk if they do not resolve our comments and respond to our questions.

Now, DOE did commit, as I said, to resolve their objections prior to starting work in that particular area. That is not true for comments and questions, but, again, they do run a risk.

MR. STEINDLER: The point I would be driving at is while it would be useful to have a reasonably coherent recoverable record, independent of the persons who wrote it, of what actions you folks took and what conclusions you came to, that somebody 10 years from now might be able to recover, it isn't obvious to me that that is a compelling aspect of the licensing process.

If it is, then I think John is exactly right, and Dade's point is very important. One really needs to have a complete and recoverable record, if the resolution of your questions and objections can become a part of the licensing process.

MS. ABRAMS: We are putting in place what we call an open item tracking system. This is a computerized system in which all the SCA comments, questions, objections will go

and, actually, are being put into as I speak, and also all study plan concerns and any other comments, questions, or objections that we would have on any DOE report in the future.

The system requires that the staff track these concerns and give the various milestones on how we are dealing with these. So there should be a trackable record for all of these items.

MR. LINEHAN: The important thing, though, is the tracking system, while it does give us this status, say, how we close something out, I am not sure it gets to this question of documentation as far as clearly laying out what you looked at, what you reviewed, why you chose to close it out, what the technical basis was.

We are recognizing this in the program. We just took action recently to revise a procedure we have for trip reports. We had folks going out into the field, and, in some cases, we would get a two- or three-paged report that said we went to all these locations, observed a number of things, and then, six months later, they would say, hey, we are going to close out this partly based on what we saw.

We are trying to get them to lay out very clearly where they went on the trip, what they saw, what the significance of it was to our open items. These are points that I think we are better appreciating ourselves within the

division, and we are trying to take actions to make sure that documentation is there, because, indeed, if this is going to be relied on -- in the licensing process, anyone can open up these issues again, and if there is not a clear record of exactly what we did and why we did it, it is questionable what the ultimate benefit of this whole relicensing will be then.

MS. ABRAMS: Dr. Steindler, I would also say that, in our study plan reviews, we go back. The staff goes back and look at what SCA concerns are applicable to that particular study plan.

MR. STEINDLER: Sure.

MS. ABRAMS: So there is a follow-up there, too.

MR. STEINDLER: Thank you.

MR. HINZE: Then the technical review is performed on those study plans?

MS. ABRAMS: The technical review is performed on those study plans. Now, we do not close those comments or questions unless DOE particularly requests closure. They have to request and direct us to where the information would be provided. We do still look to see which ones are pertinent.

MR. MOELLER: Paul or Bill, did you want to ask about the fact that it is not clear in NRC's review why the setback distance from faults was not considered important?

MR. POMEROY: I think we do know the answer to that.

MR. MOELLER: Fine.

MR. POMEROY: We have a separate setback technical position, which says that there isn't a setback required. You can, in fact, site the repository on faults at your own risk, if you satisfy certain conditions.

MR. STEINDLER: At whose risk?

MR. POMEROY: At DOE's risk.

MR. STEINDLER: That is a strange way to put it, isn't it?

MR. POMEROY: No.

MR. STEINDLER: No?

MR. HINZE: The ESF is really designed and a very major part to it.

MR. POMEROY: It is really there.

MR. MOELLER: What do you need from us? Is this just a discussion or do you need a letter?

MS. ABRAMS: We don't need a letter. This was for your information to update you on what we are doing.

MR. MOELLER: Are there any other comments or questions from anyone on this topic?

[No response.]

MR. MOELLER: Hearing none, then let me thank you for coming down and meeting with us.

With that, I will declare a 15-minute break.

[Recess taken from 10:30 a.m. to 10:47 a.m.]

MR. MOELLER: The meeting will resume.

The next topic on our agenda is the results of the geological cross-section balancing activities.

Bill, did you want to comment?

MR. HINZE: No.

MR. MOELLER: All right. We are going to have a briefing on this, and I see we have a team here, Ron Ballard and Keith McConnell.

Ron, will you be introducing it?

MR. BALLARD: Yes.

I am Ron Ballard, Chief of the Geology and Engineering Branch, and in keeping with a pattern we have established, I will briefly introduce the presentations by describing in general terms how this modeling effort fits into our program.

I will be followed up by Steve Young of the Center, who is the technical lead in adopting geometric modeling techniques to Yucca Mountain repository block.

I would like to add that Gary Stirewalt has been working very closely with Steve, to my understanding, on that, and I am sure Steve will cover that.

Also, though the name isn't on the handout package, Keith McConnell, next to me, is Section Leader of

the Geology and Geophysics Section and is the key staff player in the program that supports this particular effort.

If we could go to the objectives Vugraph, I would like to say that today's briefing --

MR. MOELLER: Do we have this?

MR. BALLARD: Don't you? I assumed a handout had went to you. I will stand by for a second.

MR. MOELLER: Thank you.

[Pause.]

MR. MOELLER: You might note that this delay is a clear indication of our need for additional staff.

[Laughter.]

MR. BALLARD: We will go on to the objectives Vugraph that is in your handout.

This briefing is somewhat different than those that I have been introducing to you during the last year. It is designed to give you some perspective on the work that the Center is doing for us in the program area of analysis, methods, and development.

The primary purpose of this program is to develop analytical tools that can be used by the staff to address licensing issues for which key technical uncertainties, as identified by the staff, exist.

As you are all aware, the Yucca Mountain site has evidence of substantial structural deformation, and the

geometric model that will be described today by Steve is an early step in developing our capability to test the validity of structural deformation assumptions that are being developed by DOE at this time and will be coming in on their licensing application.

Another important objective of the program is to develop methods of forward modeling of structure deformation in the repository block for incorporation in the performance assessment models.

We believe that this effort will also be useful in characterizing the repository block for purposes of evaluating the design of safety-related structures, an issue which Raj Nataraja will be participating in.

If we could then go to the last Vugraph, the planned objectives or planned activities. This is intended to give the committee an idea of where we are in the terms of modeling.

Prototype testing of the computerized 2D geometric modeling approach was completed back in September of 1990. I believe that Steve may be able to follow up on this. I believe this is sort of oil industry techniques that he was reviewing, and then the initial phase of the application of this prototype work to Yucca Mountain was completed in a report, I believe, in November of this year.

That report, essentially, reflects the material

that will be presented here today by Steve.

Furthermore, this material was presented at the AFCE meeting in August in San Francisco, and I believe Dr. Pomeroy was at that one. So he will be hearing some of this, much of it for the second time.

I would like to emphasize, though, that this modeling effort is not intended at this stage, anyway, to reflect an evaluation of the Yucca Mountain site. It is primarily a demonstration of a modeling tool that can be used by the staff in evaluating DOE submittals.

To go on, there has also been some work done in this program on developing a geologic framework model of Yucca Mountain that was applied in the total systems modeling work.

We also plan to evaluate several rock mechanic models in the near future. You will probably notice, as Steve goes through his presentation, that geometric modeling that he will be describing can be useful, certainly, in attempting to decipher past processes, but it has some limitations for purposes of projections into the future. So we plan to evaluate these rock mechanics models in the next year in an attempt to apply more mechanistic principles to deformation processes.

Of course, our longer-term projection is for three-dimensional models, deformation models, that would be

most beneficial for performance assessments. That is an objective, we indicate in the Vugraph in the out years, and it has to await a little more development of the work we have ongoing right now.

MR. STEINDLER: Ron, can you, in three sentences, identify the problem you are solving here?

MR. BALLARD: There are multiple problems. First of all, DOE is trying to characterize the block, and from that characterization of past quaternary faulting how they expect this block to perform in the future, at least how the repository will perform in that block as it undergoes whatever geologic processes they come up with.

We will have to be reviewing that, adn this is one of the tools that we are trying to develop in this particular program to help us to get a feel for it and an independent view, if you will, and then to be able to judge DOE's assumptions.

The other aspect of it is we do have the rule, as you know, which is a performance objective-structure rule. When the rule is developed, I don't think people had in hand just what we really mean by performance assessments, and that, as I believe you have certainly heard Phase I work on total systems analysis, and those need modules that represent the block.

So this kind of work fits directly into that

activity, too.

I didn't attend it, but there was a committee working group meeting yesterday that sat in on and heard a lot of those things. Some of this work, we have some preliminary inputs that went into that from Steve to form the basis assumptions there, and it is more to refine that approach, too.

So, I guess, that is generally the best way I can describe it.

MR. STEINDLER: I don't want to delay this thing. But can I assume that what you are looking at is some way to determine the physical properties of the repository area from limited data either from on-site data or obtained in some other fashion or that this is an extrapolation problem?

MR. BALLARD: Yes, I would say so. Everything that we have done to date, and what you will see to date has been produced by DOE data on the site, and that is why I say it is not really an evaluation of Yucca Mountain because they have very preliminary data.

It is a system that is set up to be able to accommodate more data as DOE gets it, and we could get into the technical parts of it. I would rather defer to the technical staff for that.

MR. McCONNELL: Keith McConnell.

Maybe I could add. There are two aspects of the

problem we are attempting to address with this effort. One is within the 18-month period that the staff will have to review the license application. We will have to make judgments about the validity of the conceptual models on structural deformation that the DOE is going to present to us. That is one of the efforts.

The geometric modeling effort that we are attempting to develop will test those models and attempt to validate what DOE presents to us, so that the staff can gain the confidence that DOE is presenting an accurate picture of the deformation.

Second, we will also have to judge the scenarios that DOE presents to us with respect to the potential for fault displacement or structural deformation within the repository blocks and the consequences of that displacement, and that is the second, the more mechanistic modeling activity that we are now starting into is to develop the methodology to test what DOE provides to us as far as its accuracy of what may happen in the repository and what the consequences might be.

MR. STEINDLER: Thank you very much. That helps.

MR. MOELLER: It helped very much.

Could you, though, go back and tie the title of this discussion into what you just explained? Where did the title, "Geological Cross-Section Balancing Activities" -- I

must admit, that means nothing to me, but what you said means a whole lot.

MR. BALLARD: I guess I would have to apologize. The title was an early submission to the committee staff, I think, that we probably should have corrected.

If you go to the planned activities Vugraph, it reflects the underlying title there under activity, which is the geometric modeling of faulting at Yucca Mountain, and that is the relationship between the two. They are essentially synonymous.

MR. MOELLER: You are developing this model yourself independently?

MR. BALLARD: We are developing the methodology. The model itself, the data, is all from DOE.

MR. McCONNELL: Could I just follow up? The model we are developing, the data is from DOE. Yes.

MR. MOELLER: So, presumably, DOE is doing their own model or something?

MR. BALLARD: They are developing a methodology, also. We haven't discussed it with them formally.

MR. POMEROY: But, Keith, that is really my question. As we go along here, can Steve or you tie in what DOE is doing, to the best of your knowledge? In other words, I am concerned that we don't -- no one can fault the need for a tool for review.

What I am concerned about is we are not redoing something that DOE has done in a way that we would judge adequate and that our efforts are important to us for the review process and, at the same time, complimentary and not necessarily duplicating what DOE is doing.

So, to whatever extent you or Steve or Gary understand what is happening at DOE, could you leave that into the presentation?

MR. BALLARD: Yes. I would just add that, first of all, the independent assessment capability is much like performance assessment.

DOE is doing performance assessments, too. We didn't really know. They have just come out recently with some total systems performance models, but the staff went ahead there, too, because we feel that it isn't just worry about duplicating DOE's effort. We have a rule that we developed, and we really do need to have some comfort that the rule, as written, is an enforceable rule and there is a method available. So we, certainly, do try to avoid any duplication with DOE, but it is an independent effort at this stage, too.

MR. BALLARD: As I indicated, this is a November report. We are really very early in this effort, and we will certainly be working with DOE in the form of technical exchanges and all on these issues as they develop a little

more substantive reports.

MR. POMEROY: You might mention where, when, and so forth this might be peer-reviewed or published as well.

MR. BALLARD: Steve may be able to comment on that on peer review.

Oh, I would add one more thing in the form of an apology. The handout that you will get for Steve's presentation, we had every intent to have color photographs in it. There were production problems, and, unfortunately, the handout you have won't have those in it, but they are essentially in process, and we will be getting them to you very quickly. His presentation will have the photographs, though, for the screen.

MR. RUSSELL: I am John Russell with the Center.

I want to make it very explicit that the model we are using here are models which have been developed primarily for use in the petroleum industry, and we have not developed a new model in the sense of a numerical code.

We have been using data which has come from the DOE or any other sources. As we develop this further, it may be necessary to develop some numeric modeling in addition to what is commercially available.

That is not to say that we haven't developed some different conceptual models.

MR. POMEROY: This is a direct use, John? You

haven't modified that code in any way?

MR. RUSSELL: No. It is direct use.

MR. POMEROY: Thank you.

MR. BALLARD: If there are no more general questions, Steve, go ahead.

MR. YOUNG: My name is Steve Young. I am a Geologist at the Center for Nuclear Waste Regulatory Analyses.

I want to do a couple of things up front. First, I want to introduce our structural geology group: Dr. Gary Stirewalt, who is here in our Washington Office, and Dr. Alan Morris, who is Associate Professor of Geology at the University of Texas at San Antonio, and works with us on a consulting basis.

In addition to that, I want to apologize that you don't have copies of the color Vugraphs. I wasn't able to get those made in time. We had to make up a bunch of originals to reshoot them. So we will get them shot as quickly as possible and get you a package of those things.

MR. HINZE: There are more copies of the report that can be made available to the committee?

MR. YOUNG: Are there more copies of that? We have used up all of those, all of those reports. We can print some more.

Did each one of you get one of those?

MR. HINZE: No. As I understand it, the totality of the committee just received one.

MR. YOUNG: You guys got one together? You got two?

MR. McCONNELL: We will try to get you copies of the report itself, Dr. Hinze.

MR. HINZE: That would be great. Thank you.

MR. YOUNG: We will print some more of those, too, as well.

Some of the Vugraphs that I will show you today are not in that report in exactly the same form. We change them, depending on new work, new ideas, that go into that.

What I want to talk to you about today is an approach, and this is partially in answer to some of the questions of what are you doing and why are you doing it and why do you use the terminology, why are we using the terminology that we do.

You are going to make me hold two things. Good.

What we are engaged in here primarily is the development of sets of methods and an approach to essentially critically review tectonic models produced in support of DOE's high-level waste program.

In particular, the reason that we are doing that is because we believe at this time that assessments of geologic hazards, in particular, assessment of earthquake,

hazards due to earthquake seismicity, ground rupture, magmatic intrusion and volcanic eruption, distortional strain as a result of fault slip and potential result in changes in groundwater flow patterns to the extent that they are related to fracture fabric, we think that interpretation and assessment and conclusions that are related to all of those things are going to be strongly fundamentally based on tectonic models.

Now, some of which will have a strong conceptual component to them and may not be directly testable, there are models of deep subsurface faulting. It is very difficult to gather data to prove or to validate those things.

As a matter of fact, there really are only two methods that are proven to gain you any substantial information at all in those processes, and those are reflection seismic and drilling. For the most part, key parts of the subsurface fault system are outside the drilling range as well.

But just because some of these key parts of the fault system are deep, it doesn't mean that they don't have a substantial effect on what goes on at a shallow crustal level as well.

So the reason that we are doing this is to be able to critically review and assess tectonic models produced by

the Department of Energy, because we believe that those tectonic models will serve as the foundation for hazard assessment of processes related to tectonics; in particular, volcanism, fault rupture, and earthquake seismicity.

This is a summary of the purpose of the task. Initially, a development of methods for review and assessment was the primary driver.

We realized fairly early on that, in order to determine the value or the utility of these modeling methods to assessment of tectonic models at Yucca Mountain that we are actually going to have to go a little ways down the road of producing some models.

So one of the early purposes was to produce some preliminary models. Ultimately, we want to determine the implications of the geometric models for performance assessment. We have not contributed substantially to performance assessment so far, but we think that, when we get the models to the point where we have some supporting deformation mechanics work done, then we can start and have enough confidence to contribute in an important way to performance assessment.

MR. MOELLER: Perhaps you are going to explain it later, but earlier we heard from John Russell that you are using these models that were developed by the petroleum industry.

Now, I don't know much about the petroleum industry, but I presume they are not projecting out thousands a year. They are looking at the hear and now.

MR. YOUNG: That is correct. But the oil industry throughout its history, depending on how you define this, has been in the prediction business in the biggest way possible.

I will tell you, basically, the problem that we are setting out to solve -- somebody said this earlier. It is an extrapolation problem. It is a blank paper problem, and I will show you an illustration of that.

We have relatively detailed geologic maps of Yucca Mountain and some pretty good geologic cross-sections that have been made of Yucca Mountain. However, the depth to which you can extrapolate information that you measure at the surface, it is uncertain how far you can ago with that and still be credible and still have confidence in what you have done.

What we have intended to do here is to use the methods that have been in use to solve the blank paper problem, to use what we know about the structural geometries that are measured directly at the surface, to extrapolate fault geometries deeper in the crust.

Now, I want to reiterate that the main reason for this is to develop approaches and methods to review existing

models. Our primary focus is not to develop an inhouse model, is not to develop a model that we would say this is it, this is the way things are at Yucca Mountain. We do not intend to do that.

However, we must study Yucca Mountain to do these problems. In order to exercise the modeling methods, we have to use the data from that particular site.

MR. HINZE: Steve, before you go on, I would like to note that many of us are very interested in how your results were really used in the performance assessment, the IPA II, and what you have learned, indeed, from that. As you go through, if you can give us specific instances of how you were involved -- quite specific -- that would be very helpful to us in evaluating the IPA.

MR. YOUNG: We have not supplied substantial direct input to IPA to the extent that we have said the probability of a certain event is this. We have not given them quantitative answers. We do not have quantitative answers with respect to any of the stuff right now.

However, what we do have is some pretty good working conceptual models, some of which have some quantitative aspects, components to them, and we are pretty far down the road on a good three-dimensional geologic framework model that is tied directly to the structural models.

Now, that model is being overviewed and used by the performance assessment people. I mean, they are looking at that effort to determine exactly what they have to do to distribute subsurface rock properties, et cetera, and things like saturation porosity, hydraulic conductivity, and things like that.

The model that we produce will be essentially the tank for that. It will be at least an initial step on how to model.

But I will point out specifically what we think the implications are for performance assessment.

MR. HINZE: Have you received anything back from the performance assessment in terms of helping you to clarify, to modify your models?

MR. YOUNG: Not directly, no.

MR. POMEROY: Let me follow up just a little bit, Steve, on that. One thing I think you might do for the benefit of the people, we are talking about models and models and models here. John was talking about some cross-section codes and the extrapolation codes and also used the term "models" in doing that.

You are talking about constructing a model. You are talking about bringing in a geometric model of the site. Your geometric model doesn't necessarily come from the cross-section codes; does it or doesn't it?

My only point there is just, if we are talking about lots of different models here, let's distinguish them somehow.

MR. YOUNG: I will show you pictures of those.

MR. POMEROY: But then I understand you to say that your one constructed model was then used as the basis for IPA?

MR. YOUNG: No, no. It is not used as the basis for IPA. I don't even know that it is our intent to have our models used as the basis for IPA.

Our intent is to provide the performance assessment effort with a good geologic framework model, and that is the critical stratigraphy, the faults, and the proper structural configuration of the units. That is what we want to provide to them to performance assessment most directly.

The 3-D model is what we want to provide directly, However, there are other things that we want to provide as well, and those are things like guidance on the comparative risk assessment with respect to earthquake seismicity on the Yucca Mountain faults with respect to faults that might be nearby.

I mean, one of the things that you realize right away in reviewing the literature on earthquake seismicity and faulting in the Great Basin and the Basin and Range

region is that fault geometry matters when it comes time to assess.

The relative seismic hazard of a fault system -- the basic question is some fault systems are a seismic. Some are seismic. Seismic slip is periodic. We want to try to learn something about Yucca Mountain in those areas, so that we could use that to provide some guidance to the two performance assessments as well. So we want something quantitative about that.

MR. POMEROY: Excuse me. My point was narrower than that.

MR. YOUNG: Okay.

MR. POMEROY: You are providing one model. You want to provide what you consider to be the correct model. I don't think that that is the concept that some of the staff people worked with in terms of alternate.

MR. YOUNG: The 3-D model that we provide them with is basically going to be the best pass that we can make at it, right off the bat.

However, anything that comes out of PA or that is learned at the mountain subsequently, we can feed back into that.

Now, with respect to alternatives, we are looking at alternatives very actively, and we had a specific task to do that, to look at alternatives.

Now, we have identified a slightly different approach to what the alternatives may actually be, and I will talk some about that, how we approach that issue of looking at alternatives.

MR. McCONNELL: If I could break in here, there are three different types of models that we are looking at in developing methodology to evaluate. One is to look at models or the methodology to assess the hazard. In other words, the geometric models that Steve will be talking about are looking at the hazards and what implications the existing data has with respect to the hazard.

The second are the framework models that Steve is talking about, which basically are a 3-D depiction of the data that exists, the stratigraphy, the faults, where they are, and how they are oriented.

Finally, we are looking at the mechanistic models to start talking about how we can directly input into performance assessment calculations by providing some estimate of the risk involved should fault displacement occur.

So we need to keep them straight. As Dr. Pomeroy mentioned, we have models going all over everywhere.

MR. POMEROY: Thank you.

MR. YOUNG: Let me just set the stage for you a little bit. This is a topographic map of the Yucca Mountain

area.

Yucca Mountain is here. It is basically a fault-controlled set of ridges; that is, a volcanic outflow apron from a large area to the north. But the important things to note here are the Yucca Mountain ridge system itself with respect to the valley, Crater Flat Valley adjacent to it, Bear Mountain over here, and there is a little valley system over here with Fortymile Wash. The drainage that runs through this valley is called Fortymile Wash.

All of the topography that is in this area is controlled by or strongly influenced by regional and local scale tectonic and structural features. These elongated valleys are probably fault-controlled. The location of all the ridges are fault-controlled. There are faults in front of most of the mountain ranges, and there are faults that cut through Yucca Mountain.

Our intent at this time is to take existing cross-sections of Yucca Mountain and, first, to evaluate those cross-sections to see how geologically reasonable they are, and we will show you some of the assumptions and the constraints and the limitations of that procedure.

But as part of that procedure, we have computed or modeled fault trajectories based on the structural geology at the surface at Yucca Mountain.

Here is a geologic map of Yucca Mountain. Yucca Mountain is comprised primarily by fault-bounded ridges, most of them east dipping. The faults are west dipping. So most of the outcrop areas that you see here are discrete fault blocks.

There are the hanging wall blocks or, basically, the downward displaced side of these major normal faults, and the proposed repository block would sit right in this area. These are lines of cross-section that follow the cross-section lines that were produced at the time that Bob Scott was doing some mapping in here, and these are the lines of the cross-section that Bob constructed.

This is the basic database that we have used to do the first round of models, and that basic data are: the dips of these faults that you see in here, which are west dipping; the dips at the surface; and the stratigraphic information; basically, the contacts of the formations that we can pick off here.

The major ridges over here we think are also fault bounded. Some of the models indicate there may be some faulting out here, and I'll show you that.

[Slide.]

MR. YOUNG: Now, I'm showing this to address the issue that came up earlier, that of the extrapolation problem or the so-called blank paper problem, mapping at

Yucca Mountain yields information that's about at this depth right here. Depending on where you are on the mountain and how deeply the drainage has incised into the existing ridges, you can map these geologic formations. Virtually everything that is below these has to either be extrapolated between bore holes that are drilled, and these are the bore holes, USWH-5, USWG-4, the UE25A-1. There are a fair number of bore holes at Yucca Mountain, but you see also the depth extent of the bore hole control. It's also no more than a little over a kilometer or kilometer-and-a-half or so deep.

What we're really interested in are what do these faults do as they go deeper. Up until just recently, there really have been two main fault models at Yucca Mountain. People have suggested that Yucca Mountain is part of a so-called low angle detachment system, which is a system of faults that basically curve and they flatten as they go deeper, so that the mountain essentially rides on a very low angle, or an almost sub-horizontal fault.

The alternative model has been that the faults at Yucca Mountain are essentially planer, and they're planer to depth. They go all the way down to the brittle ductile transition. It's probably -- it's good to keep in mind at this point that it's important what those faults do. It's important for hazard assessment and probably even for a lot of aspects of performance assessment to figure out what

those faults are doing.

Now, I want to point out an important limitation, and I'll point this out in a few other areas as well, and that is that what we're modeling, when we model, the deeper level default trajectories based on these shallow structures, we're modeling a snapshot of the net accumulated deformation at Yucca Mountain. However, what we want to be able to do is predict. We want to be able to tell what's going to happen to the mountain if those faults slip in the future. What's the deformation going to look like? We want to be able to critically review tectonic models that the DOE would use to base those kinds of conclusions on.

MR. STEINDLER: Can you give me a three sentence description of what the difference is between the faults that level out at some depth and those that go down to the brittle ductile transition? I mean, why do you care?

MR. YOUNG: Seismic capability. Empirically, in the great basin, large earthquakes seem to occur almost exclusively on large planer faults that extend to depth. Most of the large dip slip and oblique slip main shocks in the Great Basin have focal mechanism depths, focal depths, or 15 kilometers plus, and in addition to that, may cause ground rupture at the surface.

On a global basis, fault systems that are interpreted to be, based on fairly strong evidence, or fault

systems that are known to be low angle, to have dips of less than about 20 degrees are generally aseismic. Now, that is not -- it is not a conclusion on our part at this time that the faults at Yucca Mountain won't support a large main shock. However, it is important to be able to assess models that will emerge that show Yucca Mountain as one type of fault or the other. We've gone a substantial way down the road towards taking the existing data and developing it in a way that we can use to tell the difference between those two faults. right now, the important difference I see is as seismic risk.

MR. STEINDLER: Do one or the other of those fault related earthquakes have a reasonable -- does it have a definable impact on the repository horizon? In other words, would you expect to get damage at the repository horizon, or if you want the surface, from one kind of quake, I mean from the low angle fault in comparison to the faults that are at depth. Is there a difference between those?

MR. YOUNG: Perhaps, but it's more in the so-called seismic capability of the fault. If the low angle detachment systems are aseismic, then the deformation near the surface may be better characterized as creep. If the faults are planer and they extend to depth, then the ground rupture is seismic. So, that's a substantial difference in the deformation at the surface. In other words, another way

to say that as in general, fault scarps are believed to be almost exclusively related to seismic slip. Fault scarps are not generally accepted to be due to creep mechanisms. You have to be a little bit careful with that because erosion rates play into that some, too, if erosion can keep up with the fault slip or not.

MR. McCONNELL: It may be more important with respect to preclosure and determining the dehazard two facilities important to safety. Seismic hazard will probably be most important under those circumstances.

MR. STEINDLER: Thank you. Steve, on one of your cross sections, will you treat any fault you might think of as the site of the mountain earthquake?

MR. YOUNG: Yes --

MR. STEINDLER: Since that's the largest --

MR. YOUNG: Yeah. I will say something about that, and I have some slides where we can talk about that.

MR. STEINDLER: Okay, fine.

MR. YOUNG: The approach, then, that we have taken to first, assessing the geologic validity of the cross section, and the way that we do that is we retro-deform the section to see if it will go back to an undeformed state with no substantial inconsistencies. In order to do that, we've also had to compute fault trajectories based on the Hanging Wall deformation up here. The general process that

-- the framework within which we do that is couched in terms of structural balance.

The fundamental assumptions and the underlying thesis here is that geologically, the real world balances. Undeformed rocks can be deformed. They go from undefaulted states to faulted states, and our thesis here is that we should be able to recover the undeformed states, the pre-deformation states, and that if a particular geologic interpretation, a particular tectonic or structural model, if that model cannot be retro-deformed to a pre-deformation state, then something is wrong. You don't necessarily know what's wrong where it is. It could actually be in the method. However, it is a flag to look for something. The Hanging Wall -- I'll show you this a little bit later, but the stratigraphic units that are offset across the faults, if you remove the slip on the faults, those should fit back together again. If they don't fit back together again, something's wrong.

Some of the assumptions -- I'll show you some of the assumptions that you have to make in order to do this, particularly in two dimensions, but what it involves is retro-deforming, or undeforming the deformed state to see if it will go back to a geologically reasonable pre-deformation state. Of all of these things that you can do, what we have mostly focused on is removal of fault displacements, removal

of fault slip. We essentially have been -- we first compute fault trajectories, and then we run those faults backwards to see if the undeformed states are geologically reasonable.

At some point, we can start to pay a little bit more attention to these other things, to the extent that folding exists at Yucca Mountain in the shallow rocks, we think we have that handled within the fault models. We have not dealt with compaction , erosion, obtrusion, or we have not dealt with so-called growth sedimentation, which is sedimentation that occurs while the fault is slipping. We also have not dealt with distortional strain yet, but we have a plan to do that.

[Slide.]

MR. YOUNG: This is an important assumption or constraint on the cross sections that you have to use to do this work. Since the methods that we use are based on an assumption of plain strain, which means that in order to subtract the slip or the displacement from the cross sections, the line of section has to contain the slip factor. To the extent that the line of section does not contain the total slip vector their error is built into it, and this is an interpretation problem. Geology involves a lot of interpretation. How do you know what the slip vector was? It's based mostly on attempting to orient the cross sections such that it is a true dip section that contains

the maximum dips, and based also on any indication in the field of what the slip was in the faults. There are slick insides, or slip lines, that also occur.

So, we have taken the cross sections that are oriented such that they contain the slip vector, or at least our interpretation that they contain the slip vector, and we're working primarily with normal faults. To the extent that we deal with strike slip faults at Yucca Mountain or in the Yucca Mountain area, we'll have to expand our approach, but right at the moment, we think that the faults that we're dealing with have evolved primarily in a dip slip mode, and that we have at least two cross sections that are pretty close to true dip.

[Slide.]

MR. YOUNG: The basic assumptions involve here, the basic assumption that we have worked primarily with is conservation of area, and that is that between the undeformed state and the deformed state as the fault system evolves, if the cross section is a true dip section and if it contains the slip vector, then the area of the deformed state should be the same as the area of the undeformed state. In other words, no material should be lost or gained in there.

[Slide.]

MR. YOUNG: Now, the fundamental principles here

are these. For movement along a normal fault, basically you have a situation where you essentially pull the hanging wall away from the foot wall. The foot wall is on what will be the so-called upside of the fault. The hanging wall is on the so-called down side of the fault. So, if you pull the hanging wall away, if no deformation occurred in the hanging wall, a gap would form. We know that in the crust, the crust will not support gaps like this. So, what happens is that the hanging wall collapses on to the fault or onto the foot wall, and the deformation mechanism, the mechanism by which the hanging wall accomplishes this collapse, is the connection between the deformed state hanging wall and the underlying fault.

So, the key to this method is that the deformation mechanism, which is something that also has some interpretation to it, that the deformation mechanism gives you a direct connection between the structural geometries in the shallow rocks and the underlying faults. That's how we predict -- not predict -- model the underlying faults. It's an interpretation of the deformation mechanism, and then we can compute the fault trajectories directly from that.

In general, our approach is to use generalized incline shear. However, most of the models that I'll show you today were used -- were developed using vertical shear, and basically the deformation mechanism geometrically mimics

a kind of a deck of cards model, such that as the hanging wall is pulled off of the fault, pulled along the fault and off of the foot wall, it collapses back on to the foot wall along the slip surfaces.

[Slide.]

MR. YOUNG: This is a geologic cross section of Yucca Mountain produced by Bob Scott. The things that are important to notice in here are first off, the west dipping major normal faults, the east dipping hanging wall blocks, and perhaps most importantly and what are the physical basis for the kinds of models that we do, are these relatively closely spaced small fault systems that are primarily clustered in the hanging walls of these fault blocks.

These may be the deformation mechanism for the major fault blocks at Yucca Mountain. To the extent that the small fault fabric at Yucca Mountain is the deformation mechanism, and to the extent that we can learn something about the geometry of these small faults, we can use that to take the deformed state of the hanging wall and model, directly model, the underlying fault trajectory.

MR. HINZE: Is there some reason, Steve, why some of the deformation is taken up by a single fault as indicated there, and in other cases by the multiple faults?

MR. YOUNG: Yes. The difference between the deformation on the entire fault block system is that the

major slip is localized along a main fault trend. That also can be a zone, but there's a lot of slip localized along that zone. These small faults up here are not directly accommodating the slip on the main fault system. These things are simply allowing the hanging wall block to conform to the shape of the underlying fault, and that is a key difference.

MR. HINZE: And wouldn't we expect those small faults to occur at each one of those major faults?

MR. YOUNG: Yes. The question is do they?

MR. POMEROY: And what's the depth scale?

MR. YOUNG: There is no depth scale. Why? How come there's no depth scale on these cross sections? Here's one over here, but these are very general. They don't know. This is the depth scale right here. The answer is -- how deep are they? I don't know.

MR. McCONNELL: You could also start to conceptualize, I think, the risk to the repository whether you have a single fault like a Ghost Dance Plain cutting through the repository or an anastomosing sequence of smaller faults cutting through the repository and the implications with respect to risk that you would get from that.

MR. HINZE: Are they anastomosing, or are they imbricate?

MR. YOUNG: They're anastomosing. The imbricate was a simplified. The imbricate picture was simplified. Bob Scott and the people that are doing the mapping out there now, John Whitney and Rick Spangler, have said they look like this. They're not just tabular. They're not just simple slip surfaces all lined up. They're complex. They're complex in map view, and they're likely to be complex in cross section. Of course, we don't have very much of the cross section view of them.

MR. HINZE: Yeah, that was my next question. What's the evidence for it?

MR. YOUNG: The evidence is simple, and that is that the anastomosing character, the anastomosing fabric of these things is evident in outcrop, and the conclusion that they are anastomosing at depth is an interpretation based on that.

MR. HINZE: So, that cross section would really be a plainer section as well?

MR. YOUNG: If you stood up on top of the outcrop and looked across it, it would look just like that.

MR. POMEROY: And would you expect the snapping and the Ghost Dance --

MR. YOUNG: Yep.

MR. POMEROY: Where you've got a finite width will show the same thing?

MR. YOUNG: Yes, it is, as a matter of fact. This is critical. The point that Keith brought up is perhaps very important, and that is that these things, these relatively closely spaced small fault systems, if they are the deformation mechanism for the hanging wall block, they are going to be highly concentrated in the regions where the fault is curved. Those things drop off rapidly in areas where the fault goes flat. The hanging wall block is moving across that fault. It doesn't have anything to conform to. It doesn't have any shape. It doesn't have to change shape to move across that piece of the fault. However, it has to change shape to move. If this fault moves, the hanging wall block has got to change shape, and when it changes shape, that's a deformation process, and that is a reflection -- our interpretation is that that's a reflection of the mechanism. That's the mechanism that the hanging wall block is using to change shape as it moves across the fault.

If you get additional movement on these faults, can those things grow? Can the zone get wider? Yep, the zone can get wider. The mode would suggest that the zone can get wider. What's the Ghost Dance? Is the Ghost Dance really one of these things? The implication here is that the Ghost Dance is one of the bounding faults, but there's no deformation in the hanging wall, or is there? Spangler says yes, there is, and there's a lot more than what was

mapped.

We did not model -- I'll show you this in the models. We didn't model the Ghost Dance fault, and that's the reason right there. The first reason is it didn't have very much displacement on it in the location that our cross sections were, so he said well, it's too small. We're not going to model it. In addition to that, it didn't have any well developed rollover geometry on it. He said okay, no rollover, couldn't model it anyway. Let's not. So, we didn't. So, we didn't.

However, it's very important to interpret what the Ghost Dance is, and the key questions, for example, would be this. If the Ghost Dance is actually an element of one of these, if it's one of the internal slip systems that's just a little bit farther out on the block, can it grow into one of the major bounding faults, is it one of the major bounding -- there's an interpretation now, Spangler and Whitney and even Scott think that this is possible -- that the Ghost Dance is the northern terminus, the northern propagating tip of one of the major bounding faults that exists to the south. It's the abandoned wash in particular. That's possible.

The implications there are that deformation with future slip on the Ghost Dance system that the zone of deformation in the hanging wall must grow into the hanging

wall, and there's good evidence now from Spangler's work, that there is a wide zone of deformation.

Now, I don't want to spend too much time on this. There's also some deformation behind it. So, the situation is not simple, but at the very least, slip on the Ghost Dance would suggest that deformation in the hanging wall will propagate into what would be the repository block, and that would be important.

MR. HINZE: Steve, for the record, your conservation of area does not permit you to predict the location and the extent of those anastomosive faults, is that correct?

MR. YOUNG: Yeah, that's a safe way to put it. We actually have to assume the geometry of those things. We can -- after a fashion, we can model the extent that those things may have to exist. We can model the aerial extent through the block. However, we can't predict their existence. We have to measure that in the field and use that as data in the model. Is that safe? You guys feel free to speak up if you don't like what I just said.

[Slide.]

MR. YOUNG: I need to set a couple of terms for you here so that I can explain how we generate these models. Foot wall, hanging wall, and what do we know and what do we want to know? For a normal fault system, I said this once

already, the foot wall is on -- here's the normal fault, here's the upside, there's the down side. This is the offset, or the displaced horizon. At Yucca Mountain, here's what we know. We know that right there. We know the so-called foot wall cut-off, which is where a specific stratigraphic horizon in the foot wall is cut by the fault. We know or can reasonably interpret the hanging wall cut-off. There's some leeway, some interpretation that has to be done there, and we can get good estimates of the initial dip of these faults.

What do we want to know? We want to know the rest of that fault, and the way that we do that is that we use measurements of the deformed state of the hanging wall block. We use an interpretation of the deformation mechanism, and that allows us to directly determine the shape of the underlying fault, the point being that the shape of the hanging wall is directly tied to the shape of the fault. I'll point this out later.

You can also go in the other direction with that. The forward modeling part of that is that if you know the fault shape, you can model a future deformed state for the hanging wall using basically the same approach.

[Slide.]

MR. YOUNG: This is one of the models. This is a composite model of two of the cross sections, and it is

built to be basically the longest true depth section in there. We had to put two cross sections together to get this, but this is what we think is the best representation of a true depth cross section or a cross section that contains the dip slip vector.

Now, again, what you know at Yucca Mountain is this, and to a somewhat lesser extent, that. Just simply as deep as the bore holes will go. The control drops off naturally because you have wider spaced control points at depth, but these are the structural geometries at the surface that we use to do the model. So, for this particular model, we use the vertical shear deformation mechanism, and we used the deformed state shape of the hanging wall block at the level of the Topopah Springs. We used the initial dip of the fault and all of the rest of the fault trajectory is modeled based on the shape of the hanging wall blocks.

MR. POMEROY: Steve, could you show me where a nine kilometers depth is?

MR. YOUNG: Nine kilometers depth. Here's minus six elevation. There's another kilometer above sea level, so that is seven kilometers deep here. So, 7, 8, 9 is at the black mark at the bottom of the screen.

MR. POMEROY: Thank you.

MR. YOUNG: That's nine right there.

MR. STIREWALT: You might point out the Ghost Dance on there, too, just for the Committee's sake.

MR. YOUNG: That's a good point. The Ghost Dance fault is right here, and again, as I say, we didn't model that -- this is just a generalized depiction of the small fault fabric. We took most of that off of the cross section. However, most of these blocks have a fairly wide zone of those closely spaced small faults.

Now, in creating these fault models and attempting to come up with a balanced solution, if you restore all of these blocks to their pre-deformation shape, you can see that there is some residual east dip left in the entire model. Our interpretation currently is that that east dip is supported by a fault or a zone of faulting that is east of Fran Ridge, basically east of the Yucca Mountain fault system, and that would lie in the sub-surface somewhere out here.

This fault is the Paintbrush Canyon fault. The Paintbrush Canyon fault is a major fault in the Yucca Mountain system. However, if the only thing that you did was restore all of the slip on all of these faults up to the Paintbrush Canyon, you would still have substantial east dip built into the section. In order to account for that east dip in a way that is consistent geologically with what we see in the field, we've interpreted that there's a fault

east -- this is Fran Ridge -- east of Fran Ridge. We have variably referred to that as a 40-Mile Wash fault. It may or may not lie directly in 40-Mile Wash. There has to be a slip system out here somewhere that accounts for that, but --

MR. HINZE: Could that also be associated with some doming, perhaps if Crater Flat were a Caldera?

MR. YOUNG: If Crater Flat were a Caldera, it is possible that some doming on that scale could be associated with it. However, we don't interpret Crate Flat in that way. We don't think Crater Flat Valley is a Caldera. We don't think Crater Flat is a Caldera. In addition to that, our key mapping unit, the Topopah Springs, which is mapped here in Fran Ridge and Busted Butte and also throughout the south part of Yucca Mountain, that unit is encountered in a bore hole here in 40-Mile Wash. I believe that's J13, but it comes back to the surface here. There's also another bore hole sitting out here. I believe that's J11, isn't it? Or is it 12? It's 11 or 12.

There's another bore hole sitting right out here that encounters the Topopah Spring at about the same elevation that it exists here and only a little bit lower than here. So, with substantial east dip up to 15 or 20 degrees of east dip from Fran Ridge, it's back to the surface again here. There is a structure in between here

and here, maybe more. There are structures between here and here that allow all of this east dip to be brought back to the surface to essentially be taken out, and the Topopah Springs come right back to the surface again.

I don't want to hit this point too hard right at the moment, but what happened at Little Skull Mountain? What's going on there? We don't know, but early on when we were putting some of these regional models together, we projected some of the faults from farther north through here in order -- and I'll show you this in just a minute -- in order to connect them to a seismic reflection line that is sitting out here in the Amargosa Desert with some substantial structures on it. There it is.

On the west end of the Amargosa Valley 1 seismic reflection line, there are some large structures that appear to be normal fault related structures, major basin bounding fault system out there. There's very little evidence on the AV-1 line of sideswipe or defraction, and so it looks like that the AV-1 line is crossing the structure at a fairly high angle. In other words, the structure that is crossing this reflection line is not coming in at a highly oblique angle. It's crossing it at about right angles. It's about orthogonal to it.

You might see some similarities between these interpretations and what we just showed for Yucca Mountain.

Let me point out again what we know about -- from this seismic line. Here's what can be reasonably determined. These fault block dips in here and the fault dips themselves, everything else on the line has to be modeled, except for this major structure on the far west end of the line. This is also an indication that there are large fault systems on the east side of the 40-Mile Wash Valley. Let me have that --

MR. POMEROY: Steve, you might just point out in the depth of that section for the benefit of the Committee.

MR. YOUNG: Okay. Let me show you this, and then I'll show you the depth scale.

[Slide.]

MR. YOUNG: So, some structure crosses the west end of this line, and the focus of the earthquake at Little Skull Mountain was at around about 9 to 12 kilometers below here. The focal mechanism, which is an indication of what the slip direction was and what the depth of the faults may be, has the two focal plain solutions, which are choices for the faults that could have caused that slip. One of the focal plains projects to the surface about right here. The other focal plain projects to the surface over here in the Rock Valley area.

So, there's a low angle focal plain and there's a high angle focal plain. One of them comes to the surface

here. The other one comes to the surface here.

MR. POMEROY: And what's that sense of motion, Steve?

MR. YOUNG: It's a dip slip main shock, and so this focal plain, the focal plain -- I don't want to indicate that we think that's the fault necessarily. That is an interpretation. However, the focal plain that projects to the surface at that particular location dips about 60 degrees east, and the other one that projects to the surface over here in rock valley, it dips around about 30 degrees west.

Now, what I wanted to indicate here before we go too much farther is how Yucca Mountain, how the fault style at Yucca Mountain might be a little bit different from some of the other fault systems around it and how we might be able to fit that together into a reasonable regional model. So, what we've done is we've interpreted the AV-1 line in order to get some additional feel for how to include reflection data in Yucca Mountain interpretation, but we've also found this structure on here, which may project northward along the eastern plank of 40-Mile Wash.

Then we've constructed a regional model across here that shows the relationship between the Yucca Mountain fault system, Crater Flat Valley, Bare Mountain, and the Bullfrog Hills extended region. We have developed, in

reference to the earlier discussion on alternatives, the -- it looks to us at this time that the alternatives are not just strictly are they planer or are they curved. There are also alternatives that are variations on the curved fault detachment model.

[Slide.]

MR. YOUNG: One of those models, one of the alternatives, came directly from interpretation of the AV-1 reflection seismic data. This is an interpretation. We intend to use it to further examine and investigate Yucca Mountain, to further model the Yucca Mountain fault systems. However, what's interesting on here is that the AV-1 line shows first a relatively large east dipping half grabin, basically a large grounding fault here, where we could not create good, balanced solutions for these smaller fault blocks that would sole into the deeper one.

Basically, these are all balanced fault trajectories. This model will go back together -- if you run it backward, the entire model will go back together again to an undeformed state.

MR. POMEROY: Steve, let me ask my question again. Now will you tell me what the depth scale is and what the horizontal scale is? I can't read them from here.

MR. YOUNG: On here?

MR. POMEROY: Yes.

MR. YOUNG: Yeah. The initial model, as we digitized it straight from the seismic section, is in two way travel time, and the two way travel time, this is three seconds to here. So, it's basically the datum, which is near the surface, one, two, and three seconds of two-way travel time, and we've converted that to depth using the velocities from the coincident refraction survey. We've used these interval velocities and converted it to a depth section, and we're at the bottom of this model is at minus six kilometers elevation. So, what is that, roughly that's almost seven kilometers deep.

It's interesting, too. One of the things that we did initially on the AV-1 reflection line is to simply model this fault trajectory using the same deformation mechanism that we used at Yucca Mountain, and a very direct and simple interpretation of the hanging wall geometry of this block and the detachment depth for this big fault system came out pretty close to the Yucca Mountain detachment depth. It came out pretty close to the detachment that we had modeled at Yucca Mountain.

Now, further to the inconsistency of these small blocks with that deeper detachment, we computed some balance fault trajectories on these things, and a best interpretation at this time is that they would sole out around about here. One of Alan's very innovative

interpretations is that the structure that is on the west end of that line is essentially a hanging wall syncline. It is related to the ramp that connects the two detachment systems. This is a fundamentally new model for the Yucca Mountain system. It's one that hasn't been exercised at all to explain that anything at Yucca Mountain.

Our thoughts currently are that perhaps Crater Flat Valley could be worked into the hanging wall syncline model. So, this is a good indication of how you can use information from nearby to gain additional insight on how to interpret structures in the Yucca Mountain area. So, this --

MR. McCONNELL: Steve, can I interrupt just a second?

MR. YOUNG: Uh-huh.

MR. McCONNELL: We've been going about an hour-and-a-half, and I'll leave it to the Committee to decide whether we need to speed this up or what, if that's okay. I don't know what your schedules are.

MR. MOELLER: Well, we're flexible, but what do you fellows --

MR. POMEROY: I'd like to hear it.

MR. MOELLER: Let's go ahead and hear it.

MR. HINZE: My problem is that I think we can go here for the rest of the day, but we won't.

MR. YOUNG: I speed through them a little bit quicker.

MR. MOELLER: Could we aim at 12:30? Is that too soon?

MR. YOUNG: Fine.

MR. MOELLER: Let's aim at 12:30.

MR. YOUNG: 12:30 Is good. Okay, so at any rate, we have a new -- but this is the direction that the alternatives are going in. We're finding alternatives that we didn't anticipate initially through this modeling effort.

[Slide.]

MR. YOUNG: Now, on the other side, west of Yucca Mountain, here's a model -- gee, he's going to ask me about the depth scale again. There's no depth scale on here. Oh, it's one to one. Okay, good, it's one to one.

Here's the Yucca Mountain system here. This entire model is basically forward modeled, and the intent here is to show the relationship between the strongly extended Bullfrog Hills area, and this interpretation is based on the work of Florian Maldinado, the USGS, and to show how Bare Mountain fits in and what the potential difference in structural style is at Yucca Mountain.

So, here's Yucca Mountain interpreted as the detachment system with the detachment system persisting eastward. We haven't modeled what's going on over here yet.

We intend to do some work on that, but the interpretation here is that the Bare Mountain fault is one of those high angle faults, that it does extend to depth, and that some of the uplift at Bare Mountain is isostatic uplift associated with development of the Bare Mountain fault.

[Slide.]

MR. YOUNG: Now, one of the things that we wanted to do originally as well is to take the balance structural interpretations to take good base case or well controlled structural models, and to use those as the basis for the development of three dimensional solid geologic framework models, and what I mean by solid is that each one of the units or the layers in here is a solid block. Therefore, the model can be sliced through in virtually any orientation. Any of these layers can be stripped from the model so that you could look at the layer underneath.

Furthermore, within each one of these layers, rock properties, any kind of parametric data can be gridded in 3-D and displayed within these layers. So, this tool which we envision to be an important performance assessment tool, this is going to be the tool that is going to allow the performance assessment people to have a picture of what the mountain looks like under the ground, based on virtually any kind of data that can be acquired can be built into this.

Currently, we have it as solid stratigraphy. We

haven't gridded any of the rock property data into it, but the intent is to use this as a framework model.

[Slide.]

MR. YOUNG: What I wanted to show with this is that one of the real values of these 3-D framework models, and you run into it all of the time, is when one person will come in and say gee, we think the faults are doing this and that the fault log goes that way and it dips this way, and everybody is left with hmm, I wonder exactly what that looks like. Even if you draw maps and cross sections, it's still difficult for people in a group to have a common vision or a common view of what you mean. What does it look like?

Well, what we'd like to see out of this tool is the ability to show someone what you mean. When you say the faults do this, the fault blocks do that, a certain stratigraphic unit goes this way or that, you can show a picture of it directly. Then everybody can either agree, disagree, or determine how to proceed from there. In this particular case, this is our current interpretation of the geometry of the Solitario Canyon fault within this particular model. So, we've left the surface one. We've left the Tiva Canyon unit on here. We've taken out the Topopah Springs, all the way down to I think the Bullfrog Hills, and you can actually see the fault surface in there.

So, that's what we think the major value of this

is going to be. It's going to be in distribution of rock properties, a tank to hold the performance assessment model, and a communication tool. Okay, we can skip that one.

[Slide.]

MR. YOUNG: Overall, we see the results as being basically that we have a reasonably good interpretation of Yucca Mountain, and it is overall a detachment model. That doesn't mean that there aren't alternatives. We are exploring alternatives, and the interpretation from the AV-1 line is an example of that. The alternatives seem to be clustering in the detachment style. We have probably tested a planer fault model in more detail and with more rigor than anybody else has done.

We did a task to go in and test the so-called domino style of faulting, which is basically where you would envision a stack of dominos just laid over. Planer faults, all the way to the brittle ductile transition, and there really is no -- as far as we can tell, there's no combination of deformation mechanisms that we can use for modeling that will result in the geologic structures that are mapped at Yucca Mountain, and in particular, the features that we're interested in are the rollover geometries, which are the folds that are developed in the hanging walls of the faults, and the relative -- the variation in the fault block dip. None of the domino models

will account for that, so from the point of view of alternatives, we're moving towards looking at variations in the detachment style as being probably more productive.

The existing cross sections, in particular the ones by Scott and Bonk, can be reasonably well balanced. They can be balanced pretty easily. In other words, they did a good job of mapping. Those things will go back together pretty well. They're reasonably good geologic interpretations. You can retro deform the sections based on computed fault trajectories, and they'll go back together fairly easily.

The interpretation in the fault models from the AV-1 seismic line suggest that we should be looking at multiple detachment models at Yucca Mountain. We didn't do that. We started out to produce the simplest model that we could using most of the geologic information that was available. I think we would now subsequently go back and look at that again and see if one of these nested or multiple detachment models might be appropriate at Yucca Mountain.

Furthermore, there's indication from the models that what we would refer to as the Yucca Mountain fault system, which is that fault system that comprises the local curved fault and detachment system, persists somewhat eastward into the 40-Mile Wash Jackass Flats area. How far

eastward does it persist? We don't know. We haven't done any models out there at all, and we don't have very much information on that.

MR. POMEROY: Let's go back to two things. First of all, the Little Skull Mountain earthquake which occurred at 9 to 12 kilometers in depth. On your cross sections or the AV-1 line, I saw nothing going down to that level. Are you assuming that there is some other kind of faulting taking place at greater depth associated with the Little Skull Mountain?

The second part of the question is, consider the fault plain mechanisms, fault mechanisms, of the micro earthquakes, admittedly of limited number in the Yucca Mountain vicinity, fairly large vicinity, and could you speak to the character of those solutions relative to the basic assumption here?

MR. YOUNG: Yeah. Question number one is on the interpretation of the AV-1 line that I showed, we didn't show any faults going down to the focal depth of the Little Skull Mountain earthquake. We do have several interpretations with those faults on them. The reason I didn't show those here today is I wanted something that pertained more directly to alternatives at Yucca Mountain. However, we do have interpretations that show deep planer faults on the west end of the line.

MR. POMEROY: Excuse me, is this the preferred solution?

MR. YOUNG: Boy. Is that our preferred solution? It's not -- no, it's not. What is really is is a pointer to go back and look for such a thing at Yucca Mountain. The deformation on the far west end of the AV-1 line, that last big normal fault, there's not enough hanging wall to model a fault, and the deformation that we showed, those little dipping horizons that were in there on the west end of that line, those are all forward modeled. The fault on the west end of the line is not modelable. However, it looks planer.

Why did we do the multiple detachment model? Because the small fault blocks would not reasonably detach as deep as the large fault block. So, basically, there are like two characteristic fault block wave lengths on that line. One is this great long half grabin, and it detaches deep. The other are these little small blocks. It's very difficult to get those to detach at the same depth, so the implication was gee, we should look for a multiple detachment model of some sort, so we did. Alan forward modeled one. So, do we have interpretations that do have faults going down to seismic depths? Yes, for that line.

Now, to speak to the other issue, what is the compatibility, what is the relationship between interpretations of detachment faulting at Yucca Mountain and

seismicity that is deeper than that where the focal mechanisms range from dip to oblique to strike slip?

MR. POMEROY: With a lot of strike slip.

MR. YOUNG: Well --

MR. POMEROY: That's arguable.

MR. YOUNG: Yeah, because you don't know what the slip is, because they're all pretty small events, and the focal mechanisms are not well constrained. There are a few good focal mechanisms under Yucca Mountain. However, the point is how can you reconcile deep seismicity below Yucca Mountain with an interpretation that Yucca Mountain evolved as a low angle detachment system. That is a problem all over the basin and range. The answer is I don't know. I don't know how to do that right at the moment.

MR. McCONNELL: One of the solutions to that is what they're modeling is a miocene series of structure that's being overprinted by the quaternary tectonic regime that might be substantially different than I think, if nothing else, this modeling activity is starting to point that out, that what we may see at the surface may not be a good indication necessarily as far as tectonic models are concerned. It's what may happen in the future.

MR. STIREWALT: I think that point is well taken. What we've shown you, in fact, is the genesis of the features and not necessarily what could occur X thousands of

years from now, so I'll reinforce the point that Pete just made.

MR. POMEROY: Right. What I'm concerned about getting to the bottom line is the first set of bullets on this slide, because you're talking about evaluation of earthquake seismic hazard, and yet the model you showed us doesn't deal with the largest earthquake that perhaps has occurred there in numbers like 100,000 years.

MR. YOUNG: Yeah. The fault that the Little Skull Mountain earthquake occurred on, my interpretation at this time would be that fault is not part of the Yucca Mountain. It is not part of that detachment system that comprises Yucca Mountain. It is something else. It is like Bare Mountain. It's one of those kinds of features. It is one of the genuinely planer, deep cutting faults that definitely exists in the basin and range. They exist along with the detachment systems.

Now, I do not want to downplay the importance of reconciling modern seismicity with the observed extensional strain history of the basin and range. That is an important -- it's a paradox. There's a big inconsistency there. However, there is one thing I'd like to reinforce, and that is that everything that we see at Yucca Mountain in the structural geology of the mountain is consistent with these lystric detachment models.

In addition to that, the same faults, from faults that we've modeled, which almost certainly began to form in the miocene, the late miocene, those faults have quaternary slip on them. Not only that, but Spangler says they have a holocene slip on them. Now, you're left with making some interpretations or some decisions or some choices here. Have new faults cut through the system that took exact advantage of the surface trace of the old detachment strands? I don't know.

Is it possible that the detachment system is active in creep and that the lower crust is seismically active? We talked about this the other day. Alan has suggested that there's some strain rate dependence on that. Both mechanisms can be working. However, all of the geological -- in all of the structural geometries at Yucca Mountain, they do nothing but point at these detachment models. That does not mean that there's not something else going on. There demonstrably are deep planer faults directly adjacent to the mountain. It just doesn't look like the ones at Yucca Mountain are of that type.

Bare Mountain is an excellent example of one of these deep planer faults. So, really there's no good answer for that. We're up in the air on that issue right now.

MR. POMEROY: Right. Don't take any of my remarks in a critical sense, Steve, because I have great admiration

for this work. I'm just confused myself about --

MR. YOUNG: We're just as critical about it. I mean, we almost every day talk about that. How do you solve this problem? What are we going to do about it? Don't know. What are we going to do to approach it? We don't know yet. Some of what we're going to do to approach this is going to have to be done on a regional scale. We do have some models that are regional models that speak to the potential relationships between faults like the Northern Death Valley Furnace Creek fault zone. We have some pull-apart models put together for Yucca Mountain that may be able to address that issue, but what we need to do is we need to make good, detailed maps and cross sections of the seismicity. We need to run cross sections through with the seismicity on it. We have not done that. We haven't done any of that stuff yet.

MR. POMEROY: What I'm thinking of also there is you might be able to delineate fairly clearly what you think is needed in terms of data to more accurately constrain what --

MR. YOUNG: Yeah, and a good example of that is those small fault systems.

MR. POMEROY: Yes.

MR. YOUNG: If anything can be done to improve the information or our knowledge about what those things are

doing, what their distribution is, what the geometry is, what the overall fabric is, to the extent that anything can be done to improve that, we can improve our modeling effort. We can improve the way that we examine those models.

MR. POMEROY: Can you get that kind of information back into the system?

MR. YOUNG: Oh, sure, sure. Yeah, we can do that. We can rerun the entire model with a new mechanism, in short order, just like that. But yeah, that does address these issues.

There are some choices to be made, some interpretations to be done, and some important assessments to be done with respect to fault style. I'm not sure how to sum that up other than to say that fall geometry is important. With respect to a lot of these hazards, fault geometry matters. It matters what shape the thing is. The ground rupture hazard is related to the seismic hazard. In that ground rupture is probably, in this area, would be more likely to be associated with a seismic event.

Now, there is -- and you've seen these. You've seen some of these trenches. There's some basaltic ash in some of the fault zones. That's an indication that those fault zones were open. That's rupture. That's ground rupture. What happened? It's hard to say. We're working on a pull-apart model, a regional pull-apart model, whereby

response of the Yucca Mountain fault system to large earthquakes on the Furnace Creek fault would be examined. If Yucca Mountain is a pull-apart system, if it is inside of the Furnace Creek fault zone pull-apart region, then a big event on Furnace Creek would be expected to pull it apart, and it might have done.

An example of that is the Landers event, is the recent Landers earthquake in southern California in the Mojave Desert. We plan on watching the literature on that very carefully to see what the pull-apart effects were on that fault. The volcanic hazard, what's the relationship between faults at Yucca Mountain and the location near the ascent and the location of erupted vents.

If it is a detachment system, it seems unlikely that that detachment system has a substantial influence on magma ascent from the depths that the magma would be generated from, the genesis depths of the magma. The best estimates are that the depths that these erupted magmas are coming from are 20 to 30 to as much as 50 kilometers. We don't think these faults go that deep. Some other crustal scale structure is controlling that.

However, it seems equally likely that the shallow location, the surface location of some of these vents is related to the fault system. So, there's a coupling. There are deep crustal scale structures that somehow influence

tapping of the magma in its source region, control the ascent up to a certain depth, at which point the shallow fault system exerts some influence, may be a strong influence.

[Slide.]

MR. YOUNG: Here's what we're going to do. We want to do some forward modeling, and the purpose of the forward modeling is an attempt to predict what's going to happen to the hanging wall block that holds the potential repository. Our approach to that currently is that we want to do forward models based on what we know about the fault geometries currently in order to predict potential zones of distributed deformation. Those are the small fault fabrics, to specify we're going to use the existing models, the existing geometric models to help specify displacement boundary conditions for the deformation mechanics, and ultimately what we want to do is we want to map stress and strain due to the fault -- due to potential fault slip itself, and that basically is an effort to superpose stresses and strains that are due to fault slip on to the in situ stress state in order to assess the effects of fault slip on the major bounding faults on those faults that may be closer in to the repository, specifically the Ghost Dance.

The question is if some slip occurs on the

Paintbrush, the Boundary Ridge, the Solitario Canyon, what will be the response? In the repository block on a distributed basis, and what will happen to the Ghost Dance fault? That's the intent. That's the main intent of the forward modeling effort.

Now, the forward modeling is not going to be restricted simply to the detachment models. We'll do the full range of domino models, just as we've done with the geometry to see what the effects are there as well. Then ultimately, we want to do -- as soon as possible, we want to do the dynamic models that parallel the two-dimensional geometric models. We're going to continue with construction of the 3-D framework model. That's a display. We call that a model. It is a model. It's a geometric model, but it's built up of the other structural models and other stratigraphic models. It's built up as a display, a communication, and a performance assessment tool.

We want to conduct research on the 3-D geometric methods. Probably the single most important or significant criticism of the models we've done is that they're plain strain. They're 2-D. What if they're strike slip? Can't handle it. We cannot do that. We don't have any methods for doing that. So, we're going to pursue that some.

The 2-D dynamic models we're going to pursue that straight away. We have codes lined up to start that work as

soon as possible, and then from here, from the 2-D dynamic models, then we want to do some plate models. We'll try to look at maps of stress and strain and three dimensions.

MR. HINZE: These kinematic models, and particularly in the 3-D area, are these off the shelf models that you're going to purchase? Where do they come from?

MR. YOUNG: The modeling system that we're using right now is a system that was originally developed by a company called geologic systems. It's now being run by cognicized development, primarily in support of oil and gas exploration work. That's an existing supported package. It's an existing supported system.

We're not sure what we're going to do about this yet. We're either going to do research on our own -- we do not have the resources to build these things from the ground up.

MR. POMEROY: Major problem.

MR. YOUNG: That's right. We are thinking about some way to either start to get a collaborative effort going with an existing development company or to watch their development efforts and to take advantage of that as soon as they're advanced enough to be useable, or to do some combination, to do some in house work and watch their work, et cetera, and try to decide when the best time to do that would be.

MR. HINZE: Where is the USGS in this case?

MR. YOUNG: USGS is not very far in this area. That's not a criticism either. If detailed assessment, kinematic assessment of tectonic models is not something that you do all of the time, you wouldn't necessarily have the systems in house. They're sometimes expensive to support. They're expensive to buy. The USGS is starting to do some of the two dimensional balancing work. They talked to -- I guess Keith mentioned that they are going to buy some sort of system to do that in house, but they've not done yet really. They've not done very much work in this area.

MR. HINZE: I really like your regional models. Are any of those going to be regional models? I think these are extremely important. Are the deformational models, are they going to be regional?

MR. YOUNG: The models that we think are easiest to take on to a regional scale are these, the kinematic models, both the 2-D and the 3-D kinematic models. To do -- see, there's always a problem when you try to do the dynamics. When you try to do the dynamics, when you try to do deformation mechanics on a regional basis, because then you've got grid cell sizes that are so large you have to really wonder to what extent you are reliably or credibly representing the actual conditions.

MR. HINZE: Is the model system that you're using requiring that you use large grid size?

MR. YOUNG: The system that we're using right now to do the geometric and the kinematic work is not grid based. However, all of the systems we would use to do the dynamic work are all finite elements.

MR. HINZE: And so there should really be no limit to what --

MR. YOUNG: No, no, except for the practical limits, the computational limits. That's right. It's machine and computation efficiency are the limits at that point.

MR. POMEROY: Steve, you must have talked some to the oil companies.

MR. YOUNG: Problem definition is, too.

MR. POMEROY: I know some people in the oil companies arguing some of this kind of work, and it's probably proprietary, but have you talked to them at all and determined whether or not you might be able to work something out with them with regard to the 3-D modeling?

MR. YOUNG: We've thought about that, too. Cognicize is -- I don't know if I'm supposed to say this or not. Cognicize is actively working on a 3-D modeling system, and it is possible that we can take advantage of some of that, that we can either become involved in it or

watch it very closely, but you know, they hold that stuff pretty tight sometimes. So, I don't know. I'm not sure what we can do there yet, but there are some possibilities that would be really helpful to us.

MR. STEINDLER: Is this work supported by research or by TA?

MR. YOUNG: Both. It's primarily to this point been technical assistance, but when it's going into a developmental mode, it's going probably to be funded mostly by research. The TA work will take a different direction. One, it's more directly tied to design and performance concerns.

MR. STEINDLER: And what has been the size of the effort?

MR. YOUNG: The size of the effort to date has been the expenditure of probably in the neighborhood, I would say, of -- and this is just an off the top of my head guess, of several hundred thousand dollars at this point, and this is over a period of approximately three years.

I would like to clarify one thing and get back to the question I think Dr. Pomeroy asked, and that is the potential for duplicating work that the DOE is doing at this time. The staff has taken the position that in some cases there may need -- they may need the ability to do independent modeling, and this independent modeling and the

judgment that leads to that decision is based on our identification of what are called key technical uncertainties in the systematic regulatory analyses. This effort is tied to key technical uncertainties that have been identified in the SKA process. So, while we may be duplicating DOE's effort, there is a recognition that in some areas, particularly when the uncertainty is very inattractable, that we may need an independent modeling capability.

MR. POMEROY: Do you keep track, though, of where DOE is? I mean, does Steve talk with --

MR. YOUNG: I do. I watch him real close. I'm interested in what they're doing, and they're not.

MR. HINZE: Let me ask, Keith, what's going to happen to this?

MR. McCONNELL: Basically, we consider that report to be input, again to our analyses with respect to helping refine the key technical uncertainties. Right now as far as the staff is concerned, there probably will be nothing done to that report in particular. We're using it as a stepping stone to go further.

The center staff is free to submit the results of their working efforts in that report to journals or conferences like international high level waste conference.

MR. HINZE: Is the seismic line that is suggested

here pretty much a duplicate of what the USGS is currently receiving proposals on?

MR. McCONNELL: The seismic line that we --

MR. HINZE: I have not read this report. I have just been handed it. You suggest that you want a seismic line across Yucca Mountain. Is that being taken care of by the USGS line that is now being proposed?

MR. YOUNG: Spangler says they are looking pretty close in the regions that we would think would be appropriate for those kinds of lines.

MR. HINZE: The crust has created a flat in Yucca Mountain --

MR. YOUNG: Crater Flat right between Red Cone and Black Cone and right up against Solitario Canyon just to the south of the mountain where the Aluvium goes up in there, over the mountain and then right out into Fortymile Wash. That's the -- I think what we resolved to do in that report is to first look carefully at existing and emerging study plans on the seismic reflection program, and then to look again at that blue cover report on the results of the existing work, and to just keep a close eye on it and to take as quick advantage as we can. We'll try to do some modeling work on it.

MR. POMEROY: Steve, just for the Committee, could you comment, as Keith points out, you're free to publish

this material. Could you point out what you have done and what you are doing in terms of publishing the material other than in that form?

MR. YOUNG: Yeah. We've been putting this -- most of this is going into the high level waste conference. However, we have been chatting about putting something together for something like GSA or JGR.

MR. POMEROY: I think that would be really useful, and also it would get it to a wider -- expose it to a wider review.

MR. YOUNG: Get a bigger group to hammer on it, it's true. Yeah, we have plans in that area. The very first thing we're going to do is put another little paper in the high level waste symposium on the alternatives work. This is going to go directly towards gee, how come the domino models don't work? Well, this is how come. That's what we're going after right now.

MR. McCONNELL: I would also, I guess, try to prepare you because I think you'll be getting more of these center reports like this one. These are intermediate milestones generally that are used to monitor the contract to make sure that the staff is getting basically what it wants, but we also don't want our center contractors to parrot back what we want to hear. Therefore, they are independent contractors and there is independence in their

work, so we would expect to see conclusions made in these reports that the staff may not necessarily totally agree with. There is a disclaimer in the report that notes that it is an independent product of the center, and if sometime in the future we intend to adopt it, then we would come out with our own, a staff report on these activities.

MR. MOELLER: Other comments or questions? Well, let me close out, then, by thanking Steve and the Center staff as well as the NRC staff for being here with us and providing this briefing. You've certainly clarified many things for me, and I'm sure for all of the Committee members. We'll take a one hour lunch break, and then we'll resume with the briefing on expert judgment, followed by the report of the chairman of the working group for total systems performance assessment.

Thank you.

[Whereupon, at 12:43 p.m., the hearing was recessed for lunch, to reconvene this same day.]

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

(1:50 p.m.)

MR. MOELLER: The meeting will resume. The next item on our agenda is a report by Senior Fellow Steven Mays on the November 18, 1992 DOE Workshop on Use of Expert Judgment.

Steve, it's a pleasure to welcome you, and we appreciate the written report which you have submitted and which we have seen.

MR. MAYS: Thank you. I guess what I would like to do is just give a real brief summary of my impressions of the DOE Workshop on Expert Judgment, and then try to answer any questions you may have about any specifics or things I wasn't necessarily able to put in the memo about the workshop.

As I noted in the memo I wrote to you, there were two very fundamental impressions I came away with on the workshop that I think were important. One of them has to do with the issue of the entire discipline of decision making under uncertainty, and that involves both decision analysis and the use of uncertainties, and determining the best courses of actions to take in a process. And the other concerns the elicitation of expert judgment in order to determine what those uncertainties will be when there's an absence of data.

There is an obviously growing and continuing discipline of decision making under uncertainty that has been a remarkable progress, and the presentations from the workshop on those particular areas in that field I was impressed with.

There were several people in the presentation list who were also major contributors to the process that started, as far as the NRC is concerned, with the expert elicitation and expert judgment of NUREG-1150, and some of those applications have been followed on in other areas with the DOE as well.

There was also another aspect of the meeting which was entirely appropriate but was less technical, and that was the issue of the political/legal/social implications of using that kind of a methodology. And that particular aspect of the meeting carried over in almost all of the presentations, especially in the legal aspects.

I was impressed with Dr. North from the Nuclear Waste Technical Review Board's presentation and his presenting of the issues that Dr. Pomeroy wrote to him in his letter, since he was unable to attend the meeting.

I think there is a general consensus among all the people there, both the DOE, the decision analysis people, the expert judgment people, the State of Nevada, the other people; that the key element in this whole process is the

legal, social, and public acceptance of how you go about doing expert judgment and what processes and decisions you apply it to.

That is a bigger hurdle to be overcome than the particular technical hurdles of how do you combine differences in probability distributions derived by various expert judgments and expert panels.

So those were the key highlights, as far as I was concerned, from the meeting.

I noted in the memo, in particular, that Dr. Hora made a compelling discussion about how he has applied decision analysis and expert judgment in the commercial field for corporations. And he noted that the successful application of expert judgment and decision analysis in his fields was almost always dependent on what he called a commitment to action, and that there was actually two tracks of responsibility and decision making that apply.

One was the body or persons who had both the resources and the commitment to action to make something happen, and the second track was the people involved in eliciting and determining expert judgment and making the decision analysis.

He indicated that he was impressed with some of the decision analysis people at the conference, but he was wondering where the commitment to action was and who was

going to be making those decisions. And I think that was a very key point in the process that we've seen so far.

In addition, there were some very good presentations about some of the legal aspects of the conference. In particular, there was a paper that was presented, and is in the package that I sent to you, by a gentleman by the name of McGarry, who I believe was with Winston & Strawn, a law firm, who talked in great length about the Atomic Safety Licensing Board process and how they tended to view expert panels; not just expert witnesses but expert panels at licensing proceedings and what the difficulties and problems that they encountered with that process were.

I'm not sure that anything was conclusively decided at the meeting other than that a lot more work on this whole area needs to be done. But it was a free exchange of ideas, and I think it was good from that standpoint, that it brought a lot of people together so they could talk about different aspects of what they had been doing.

MR. MOELLER: Was the NRC staff represented?

MR. MAYS: Yes, sir. Dan Fehringer, in fact, gave a presentation which was similar to the one he gave yesterday to your working group. I noted that I saw Lee Abramson, who is a statistician with Research, was at the

meeting, and there were some others there as well.

MR. HINZE: Steve, were there any ideas presented on how one can achieve the legal, social, and public acceptance of expert judgment? Can you expand on that?

MR. MAYS: There were a few. I would say the one that was most addressed to that question which was raised by Dr. Pomeroy and Dr. North at the meeting was a gentleman who had some experience in the low level waste licensing arena. And he presented -- it's in the slides there, I've forgotten his name now.

But he indicated that a good portion of the legal problems that have been experienced in the low level waste would be where the developer of a site would create a set of models and assumptions and analyses, and come to a conclusion and would follow that train.

The regulator would go out and develop a set of analysis assumptions and solutions and come up with an answer from that train. And an intervenor or an opposer would come up with a set of analysis assumptions and come up the thing, and when they came to the end and none of the answers agreed, they would go to court. And that was the model for which those things were being cited.

And he indicated that he thought probably a better way to go about the process was to get all the parties involved at the start, and have the parties decide what were

going to be the models, the assumptions, and the processes.

So that when you came down to the bottom there would be one set of analysis, or a group of analysis, that people had gone through the experience and the trouble of agreeing to, so that when the case came to litigation or the case came to a decision maker it would be less confrontational and perhaps more amenable to the process.

There are a lot of issues involved with that kind of approach as well. I'm not sure which is the best way to do it. I think there are -- from my standpoint, there were two fundamental issues from a technical standpoint about this whole process.

One is the whole concept of decision making under uncertainty is a relatively new discipline in our society. It's no more than about 30 or 40 years old in terms of active use and commercial and governmental processes. It's not something that people are fundamentally, intuitively attuned to. It follows more of the subjectivist, Baysean type of approach to looking at and analyzing problems.

And as the decision analysis people will tell you, their paradyne in decision analysis is to maximize utility. We are often involved in the regulatory business of trying to regulate individual outcomes. And so there's a fundamental difference in that mentality that I think contributes to the problem of dealing with decision making

under uncertainty.

Over and above that, there is also the problem that if you accept decision making under uncertainty in the decision analysis framework, what information goes into the models. And that's where the expert judgment problem comes in, when you have a sparsity of data and you have to rely on degree of belief or subjective probabilities as your input models, as opposed to, say, rolling dice where you can construct a very nice distribution for what the probabilities of the individual outcomes of rolling the dice would be.

So there are two issues there that I think are fundamental problems and that with respect to taking that discipline and gaining the kind of public legal acceptance that we would have to have in order to make that process work.

MR. STEINDLER: You are making the assumption that if everybody got together and agreed in advance that they would by definition come up with the same answer?

MR. MAYS: No, sir. I wouldn't make that assumption. I think that's the fundamental problem with the approach I just described with respect to trying to get everybody together and come up with one approach. That was the only novel approach I heard at the meeting towards trying to solve the public legal problem in terms of the

technical answer situation.

My personal belief is that it's more fundamental than that and it goes to getting people to the point where the acceptance of decision making under uncertainty and making a policy as to whether or not you want to really maximize utility or regulate outcomes. That's a fundamental policy issue that I think has not been resolved at the levels that would be required to make this process work one way or the other.

MR. STEINDLER: On a more detailed level, were there any discussions evident of how to reconcile quantitatively disparate outcomes of collections of expert judgment?

MR. MAYS: Yes, there were some. The predominant philosophy that I came away with, or at least my interpretation was that most of the people who were experts in soliciting these kinds of things felt that bi-model outcomes, or that kind of result from expert elicitation was usually the result of either poor training ahead of time or a lack of feedback mechanism within the training and solicitation, to go back and say here's what the results are; now go back and see if these make sense based on what things we may know or may be able to test.

During the presentations at the meeting I noticed a distinct absence of direct reference to this iterative

feedback mechanism in the elicitation process. And I spoke to several of the practitioners during breaks and said I think there's a serious issue here.

They all indicated to me that they really believed strongly in the iterative feedback; let's test the hypothesis that we've now come up with as a result of this distribution that we've solicited. And they all believed that that was an appropriate and proper thing to do.

But I don't think it gets talked about as much as the more theoretical things, such as what kinds of biases are there and how can you measure them and how can you hopefully try to adjust for them. But I do think that's an important problem.

Now, there is one international program that's going to go on that Steve Hora is involved with, with the European community, where they're going to try two different methods of soliciting some of this judgment, and they're going to look at different ways of either weighting or not weighting and combining distributions; whether they're going to strive for consensus or they're going to strive for individual inputs that will somehow be put into a model and averaged.

So there may be something that comes out of that in the next year or two, but there is still no hard and fast rule for how to do that, coming out of the decision analysis

people.

MR. POMEROY: The operative word was "averaged." And we've argued at length as to whether that's even a sensible approach.

MR. STEINDLER: And the other one was trying to achieve consensus, which is not my conception of how you - -

MR. POMEROY: That's not an adversarial methodology that's normally applied to intervenors.

MR. MAYS: There was a considerable amount of discussion on the consensus issue. It seemed to me that most of the decision analysis professionals and elicitors seemed to think that rather than striving for consensus, what you should be striving for in your elicitation is the broadest possible representation of what the uncertainties are, rather than the narrowest possible agreement for which everybody would agree.

In other words, they wanted to categorize the uncertainties of the expert population as best they could to its largest degree rather than get the most narrow distribution for which everyone would agree to. That seemed to be something that was underlying a lot of the discussions, but it wasn't something that you could nail down firmly.

MR. STEINDLER: Are you saying that people were

more interested in the uncertainty than the answer?

MR. MAYS: Yes.

MR. STEINDLER: That's a fine "how do you do."

MR. MAYS: As a matter of fact, a lot of the --

MR. STEINDLER: Is it because it's easier to get?

MR. MAYS: No. I don't think it was because it's easier to get. I think it was because some of the studies resulting on biases indicated that one of the most significant problems they had with expert elicitation was an over-confidence problem with experts who tended to think they knew the bounds of a problem more closely than they did.

They found this in terms of asking people in their almanac sessions or in their generalized training and probability sessions to estimate specific values, and they also asked them to estimate ranges. And what they found often was that people who knew something about a subject were usually pretty good at getting close to what the value was, but were often -- had very, very narrow ranges applied to their estimates.

I guess you could look at that as either a confidence in their knowledge of the subject or you can look at it as some of them do as over-confidence in the subject.

There are some very interesting formal processes that they go through to try to determine how people are

biased one way or the other with respect to confidence, and it has to do with both asking them to estimate the single or the mean value of a parameter and asking them also to determine what the bounds, or a range in which their 90 percent confidence, the real answer lies.

And so there were several discussions of that at the conference.

MR. MOELLER: Paul, is it appropriate to move into and discuss at this time some of your plans, or you had talked about a workshop --

MR. POMEROY: I think so. I think he should lead us right into those.

MR. MOELLER: Fine. Thank you.

MR. HINZE: Could I ask one more question of Steve? Steve, in discussing the legal aspects of this, was there any consideration that one might go to all kinds of lengths in terms of having scientific engineering and technological acceptance of the expert judgment, and having this fail at the legal level?

MR. MAYS: Absolutely. As a matter of fact, as I mentioned, Mr. McGarry's discussion of how licensing boards have proceeded in the past and what one might consider to be if the past is an indicator of the future, that there was no guarantee; that even with a consensus of the expert community and people going into a proceeding, that that was

going to be necessarily acceptable to the licensing board.

And, in fact, one of the key points he pointed out, and is in the paper, is that his experience with licensing boards was that expert witnesses and expert panels were extensively and gruelingly examined at these meetings, and that the licensing boards tended to, rather than sit in judgment of the cross-examination of one side and the other, tended to be the more grueling of the examiners; and that their examinations tended to go more towards process and what they thought of the people's integrity and their competency to make a decision, rather than on any specific outcome of a specific set of judgments; and that the licensing boards, in his opinion, tended to be sometimes more critical than either of the parties, and that they, the licensing boards, would not rely solely on the counsel cross-examination as their basis for deciding whether to accept the expert panel's judgment or not.

MR. HINZE: I suspect that's because of several reasons. But lack of precedence, the distrust of the scientific bureaucracy, technological technocrats?

MR. MAYS: He didn't give any particular reasons or offer any judgments as to why he thought that they did it that way, other than to say that this is the way the experience was and it would be reasonable to expect that experience to continue.

MR. STEINDLER: Well, you know, in the cold light of morning, if you think about it, how else is credibility as far as the three-man panel is concerned, or three-person panel, how else is credibility established? Certainly not by the color of the tie the person is wearing. And that's the only mechanism that that panel has, I can tell you from limited experience.

MR. MOELLER: Well, thank you, Steve. And we'll hear now from Dr. Pomeroy. Stay with us, so you probably will have him --

MR. POMEROY: Right. This certainly ties directly into the meeting that Steve was at and it ties in, to a certain extent, our next topic this afternoon as well.

You have, I think, a copy of a letter that I wrote to Warner North that he, as Steve indicated, summarized in the meeting and, in fact, distributed half of, I believe, after the meeting.

In that letter I cited some personal views, although two aspects were aspects that we had discussed before, namely getting some guidance issued on the question of methodology or extraction of expert opinion, and ultimately, perhaps, more than that.

And the second one was simply that I thought both the Department of Energy and the Nuclear Regulatory Commission should pick one particular area, not necessarily

the same one, and investigate it in great detail, as we have a commitment from the staff to do in Phase 2.5 of IPA.

I quoted extensively from Judge Mims' 7th Circuit Court ruling on a case that was rather straightforward, I thought. But he had done an excellent job of summarizing the court's feelings about expert opinion. And so I wanted to bring those to Warner and I wanted to suggest to him that this could be potentially a very serious problem.

I'm not sure, in fact I have great confidence in the Hearing Board within the Commission and I have great confidence, of course, in the Commission itself in making decisions in the face of a large degree of uncertainty that is support in some way by expert judgment.

I am concerned that in this particular instance, in the repository situation, this will certainly be fought not only through the hearing process and through the Commission, but it will be fought in the legal system where we get to the strict application of Federal Rules of Evidence and other doctrines that were cited in the 7th Circuit Court.

MR. STEINDLER: I'd like to get back to this question of the Federal Rules of Evidence sometime.

MR. POMEROY: Right. I'd be glad --

MR. STEINDLER: Whenever you want.

MR. POMEROY: Let me go through this and then I'll

do that. Remind me.

I wrote to Warner that I thought that perhaps -- and I'll try to find it. I thought we should try -- consider the possibility of having a meeting between representatives of the interested parties, the overview people and legal specialists in the area of expert judgment, and plan for the use of expert judgment in the legal framework of the licensing process.

What I'm concerned about there is not reaching a consensus on how it will be used, but rather getting -- transferring information from the legal community to the technical community and some sense as to what they're going to face when we get to this situation in court.

It seems to me that's a first -- one possible first step as an ongoing -- in an ongoing discussion of the expert judgment question.

This meeting that Steve went to, I might say, went on for three solid days. So it was an extensive set of presentations.

I did get a letter back from Warner North, and I believe you have copies of that also. He indicates the following, and let me just read a few sentences here. "I would like to invite your suggestions on how we," that is ACNW, NWTRB, NRC, and/or DOE, "might proceed further in this area."

And then he goes on to discuss one route might be to involve legal scholars in carrying out a review of relative legal principles, but he doesn't think that's a very good idea, and I agree with him. I don't think we have the expertise or the time to develop expertise in the legal areas.

"It certainly, however, is within all of our charters," this is Warner speaking, "to point out the need for this expertise. Second, it is my impression from the workshop that relatively few of the scientists or managers on the DOE program have had substantial experience as expert witnesses."

NRC staff have had extensive experience, and perhaps expensive also, extensive experience in adjudicatory hearings on complex scientific issues. While some presentations at the workshop provided an indication of lessons learned from such experience, much more could be done to familiarize the DOE program with the situation they will face in the licensing hearings.

And I wondered what the Committee's thoughts would be to the idea of pursuing jointly or separately, as the political and legal considerations dictate, with Warner convening a working group, in essence, or a subcommittee meeting, in their case, a combination of the key technical people and some key legal people; involving not only legal

people from our -- say, somebody representative from our OGC and the DOE's OGC, but also a legal representative of the State of Nevada, a legal representative of one of the counties or other interested parties out there, as well as, perhaps, Dan Riker from the natural resources group.

I think it would have some merit in assembling and transferring their feelings with regard to what's going to be faced and how we might proceed in the most effective manner.

I'd like to think that we're getting to the point where we could evaluate what we're going to face before we get into the actual licensing process. I have brought that question up, and Warner's response simply was, "Learning by doing in the first ever licensing proceedings for a high level nuclear waste repository could be very expensive for our country." And I certainly agree with that.

I would like to hear any comments you have on that subject.

MR. MOELLER: Well, let me begin with a question. Have the NRC and DOE staffs had a technical exchange on this subject or any type of -- between the two staffs?

MR. POMEROY: As far as I know, and perhaps we could ask Margaret back there, there has been no technical exchange other than this workshop on the legal aspects of the problem. Margaret?

MS. FEDERLINE: That's correct, to my knowledge. Any discussion --

MR. MOELLER: The recorder can't hear you. The microphone there is working.

MS. FEDERLINE: Margaret Federline, NRC staff.

To my knowledge, there have been no formal exchanges between the legal staff of -- discussions have primarily focused as a subset of performance assessment discussions between the technical staff.

MR. POMEROY: Right.

MR. MOELLER: Well -- so that was just a question in my mind. The second comment I would have is several times we have discussed joint NWTRB/ACNW meetings and generally have concluded that's not the way to go. Not that it wouldn't be useful, but it apparently is very difficult. So I presume that says we should either encourage them to do it and we would sit as observers, or vice versa.

MR. POMEROY: Right. I would like to try it one more time. We've certainly been encouraged --

MR. MOELLER: We probably should.

MR. POMEROY: -- to try to do that, and by an earlier chairman. I think now there is a different chairperson at the NWTRB who might be more receptive to that kind of interaction. But it's certainly possible to do it by either one of us and have the others simply as active

participants.

MR. STEINDLER: Well, I've got a lot of problems with what I guess I just heard. First of all, let me get to this question of Federal Rules of Evidence.

It is presumed by bringing the situation up at all that the results of an expert solicitation -- solicitation of a group of experts for a value for a concept, or for whatever, is definable as evidence in the context of the Federal Rules of Evidence.

It isn't at all clear to me that that's necessarily true. In fact, if you look at the definition of the Federal Rules of Evidence in those two documents that define it, which blissfully are not very big, I think the results of expert -- solicitation of expert judgment are almost by definition excluded from being called rules of -- from being called evidence in that context.

I have no training in this field, so you know what that's worth.

The other issue, though, that I'm concerned about is that I think somehow or other we're confusing a number of significant points here. One is the methodology of the process and the other one is the ability to apply it. And the ability to apply it comes again in two groups.

One is, is it allowed or is it going to be allowed and, two, does it fit into the particular technical issue

that we're currently talking about, and does it fit; can you adjudicate two disparating views, et cetera, et cetera.

The major uncertainty that I have is that I can't see my way clear through that first question; namely, is this a methodology that people are going to accept. And I don't know what I mean by "people." The Atomic Safety and Licensing Board or the Commission has experience in this business and I would guess are more comfortable with it than a lot of other people I can think of.

And so I would expect in the Commission circle the notion of accepting the outcome of an expert solicitation process to be not a very difficult issue. I think that would probably fly.

It's not at all clear that, to me at least, the court system, if we are to pay attention to them, are going to view that with the same level of charity. That is the driving force, it seems to me, because at the moment there are no backups to that system.

If we can't use expert judgment to arrive at some parameters, to arrive at some information that we can't get any other way, at the moment there is no other way that I'm aware of that is likely to fly.

So the court, then, it seems to me, if the court elected to throw out this whole methodology and say, no, folks, you can't use this process for one reason or another,

no matter how well couched; if the court throws that out, then the court in effect is saying that the process that we're trying to do, namely license a repository, can't go forward.

I don't know how that's going to play out. But before we spend an enormous amount of time worrying about sharpening up the process of expert solicitation and all the nuances and socio-psychological issues that are involved in getting six people in a room together, or whatever, it seems to me we ought to determine first whether -- get somebody to determine first whether this whole thing is going to be worthwhile.

If we can urge anybody to do anything, that strikes me as the number one issue that is necessary to be resolved, but not sufficient. And then we can move further.

I don't, quite frankly, see what we can gain at this juncture for having a group of folks from the Technical Review Board meet with us and talk about this. It isn't clear what the outcome would be that would move the ball forward.

We've had a session, we've had two sessions of significance. Transcripts are long and we can get useful information out of that, the process, the methodology, the quality of the outcome, et cetera. I don't think that's where the problem is.

MR. POMEROY: Maybe I didn't make myself clear there. There are people over at the PRB, namely Warner, at least, that are concerned about this particular problem. We are -- I, at least, am concerned about this particular problem and I know you are also.

What I was hoping was that we could have not only a few of the Overview Committee people, but that's sort of almost secondary to having a few of the key technical people from the -- some of the people that we could identify, namely DOE, NRC, the State of Nevada, and any other potential intervenors, talk with and hear presentations by, for example, somebody from our OGC saying what is acceptable from the standpoint of the NRC's legal system as to expert judgment, and what is my opinion of what is acceptable outside of the framework of the NRC.

And I'd like to hear -- you know, the concept here is a set of -- small set of lawyers talking to a small set of technical people, not necessarily talking to a few overview people who might be sitting there.

MR. HINZE: I'm confused about what you really want to accomplish. Is this training of the technical people in NRC and the DOE bringing them up to speed on this, or is it trying to reach some generalities that we can move forward?

MR. POMEROY: In essence, it's the first, a

modification of the first, Bill. It's not in any way trying to reach any sort of consensus. We'll never do that. It's an education process to ensure that all the parties that are going to eventually be involved in this are going to be aware of the positions of the individual agencies involved with regard to the acceptability of expert judgment.

MR. STEINDLER: But it's all very well and good for a few lawyers to talk to a few technical people, but none of those parties are decision makers. Decision makers, either the court in D.C. or Supreme Court, that's where it's going to end up.

MR. POMEROY: That's right. And all we can do - -

MR. HINZE: And we're not going to be able to ask them.

MR. POMEROY: No. And all you can do is get information -- as far as the courts are concerned, all you can do is get any thoughts that the lawyers for the various groups have in respect to such questions as under the Federal Rules of Evidence or under the Frye Doctrine what is allowable; what is their expert judgment, if you will, on what is allowable within that court system that's going to make that decision.

Certainly, you're not going to -- I'm assuming that our lawyers, for example, will advise the Commission on

what they feel is acceptable in terms of expert judgment. So I don't think --

MR. STEINDLER: As far as I know --

MR. POMEROY: -- you need to involve decision makers within the agencies.

MR. STEINDLER: I agree. But, again, I would go back to Margaret. As far as I know, our -- that is, legal counsel of NRC has not addressed this issue, is that correct, as far as you know? Or Dan, or whoever has looked into it?

MS. FEDERLINE: Dan is really the best one to ask because he's been working directly. But he's been working very closely with our general counsel in comparing -- they've been reviewing any material that we've been developing, and reviewed his presentation that he did at the expert judgment workshop.

MR. STEINDLER: Okay. The question I would pose to you is, supposing we address a question to the General Counsel and say give us your judgment on two questions. One is the results of -- are the results of expert judgment likely to be allowed in a licensing board hearing, and if the answer is yes, please answer the same question regarding the federal courts.

And if the answer is no, then give us an alternative on how to introduce -- you know, what's the

mechanism that we need to develop. Is it rule making, for example?

And I can perceive of Section 2 where procedures are outlined, in 10 CFR 2, have a section in which it says rule making will be a process that's allowed if it follows a particular protocol. And we've been talking about having the staff develop a protocol that would go along with that.

And if the answer, which is more likely than anything else, "I don't know," then our question ought to be, how can we find out.

MR. HINZE: Haven't we been using expert judgment all along with these boards? It's the --

MR. STEINDLER: Well, except for the two cases -

-

MR. HINZE: Well, we've been using --

MR. POMEROY: Excuse me. I --

MR. HINZE: -- individual expert judgment. This happens all the time.

MR. POMEROY: Right, absolutely. There is a significant amount of case law in the McGarry paper that Steve cited. There are hundreds, literally hundreds of NRC cases where expert judgment has been involved, we've been involved; certainly has been used in various ways.

It's been used in various ways. It's been used as individuals testifying, with a group of other experts

standing or sitting beside them without being sworn in and without being involved in any way, but simply counseling the person who's testifying.

And I don't -- what I don't think we've done is, I don't think we've gone through the process of looking at large panels of experts testifying in a given area with exactly the same technical basis; that is, all of the material that has been developed in the cite characterization program, coming to some apparent conclusion of this one group and having three or four other people, three or four other panels, rather, who base their opinion on the same set of evidence and who get to a different conclusion.

I think that's where the real problems are going to lie, and I don't think that that's been really addressed.

MR. HINZE: In dealing as we have with expert judgment and being involved in the firing line, my concern was the experts on expert judgment controlling my decisions and weighting those decisions. And it's not the experts that I worry about, but it's a concern about how the elicitation is performed and how it is weighted and presented.

So we're really putting a group here between the experts, like we have for centuries, and trying to come up with some commonality, some -- as you put it, a large group

coming together and having that brought as a decision.

MR. POMEROY: Right. And I don't know how the courts -- the courts will say that -- one of the things that Jim Wolfe, I believe, would say is we'll take each individual panel, one person at a time and find out -- go through the whole process and find out the process by which he reached a conclusion; what the conclusion was, and whether it's based adequately on the acceptable scientific principles of the day, basically what the Federal Rules of Evidence try to say.

I think the Federal Rules of Evidence are very reasonable, in fact.

MR. STEINDLER: For evidence.

MR. POMEROY: For evidence, yes.

MR. MOELLER: Dan, I'm sure, has some remarks, but I guess the part I find myself confused on is that we had a working group meeting on expert judgment. DOE has spent, obviously, a lot of money. Remember Bonanno was in here and had what I call, or what to me is the bible on expert judgment. I guess that was -- was that a NUREG document?

MR. POMEROY: That was a NUREG document that somebody had contracted.

MR. MOELLER: The NRC paid for that. And doesn't even Part 60 have words about expert judgment? Am I dreaming?

MR. POMEROY: Dan?

MR. FEHRINGER: Dan Fehringer of the NRC staff. Part 60 does have some words talking about reasonable assurance and recognizing that uncertainties will exist in any demonstration of compliance. I think that infers that judgement will be a major part of the demonstration of safety.

MR. POMEROY: Right. I think the problem goes beyond that, however, though. Those were good words, and I think they are good words. But the whole question of the identification of those uncertainties and what is actually going to be allowed in the court system is another question.

Bill's question really relates to Marty's first point, mainly the methodology needs to be out there. We certainly need to encourage that to happen.

MR. MOELLER: Where I was confused, I though I was hearing the basic question as to whether expert judgement will be used and whether it will be allowed.

MR. STEINDLER: You mean by licensing board or --

MR. POMEROY: My suspicion is that individual expert judgement will be allowed by the licensing boards because it has been. There's a great deal of precedent, and the legal profession lives on precedent.

I'm not sure how they might deal with the question

of panels of experts --

MR. MOELLER: Okay, sure.

MR. POMEROY: -- because we've often been told here in the meetings you cite that after all, we'll look at the underlying scientific basis, the technical data that underlies the decision, and then we'll make a decision. This is the staff speaking.

That's a good statement, but if it's the same set of technical data that underlies a number of different opinions that all diametrically opposed from one another, the decision-making process is thrown to the next step, it seems to me. I don't know how that's going to happen.

I think it's worthwhile trying, using some methodology, to communicate in a straight-forward way to the Department of Energy what kind of obstacles they're going to run into at an early point in time.

In fact, we've talked about other methods of jump simply running through a whole mock system to try to go through the whole system once with expert panels, with people sitting as adjudicators to see what might transpire.

This alternative is another way to try to do that. I'm looking for alternatives, though, gentlemen. I'm certainly not advocating more work here for myself.

MR. MOELLER: Let's hear from Dan again. You're correct. Then I think we need to decide what is our

objectives. What do we want to do?

Dan?

MR. POMEROY: Let me just say that I think we could ponder that question for a month also, as an alternative.

MR. MOELLER: Sure, sure.

MR. FEHRINGER: In preparing the talk I gave to you yesterday, I was lead to believe that it's very important to distinguish between the admissibility of evidence on the one hand versus the weight that a particular item of evidence will carry in formulating a decision on the other hand.

The standards for admitting evidence to the NRC hearing are not really great. They're the two that I described yesterday -- show that an expert truly has expertise and show that the evidence he wants to offer is relevant to the issue that's being discussed.

Whether or not the board will place great weight on that particular evidence, it remains to be seen. It depends on the quality of evidence and the reasoning that underlies it.

I've been lead to believe that the reasoning is really what is important and that the process by which evidence is developed is largely irrelevant. So, a formal elicitation process would be of value to the extent that it

helps an expert identify his reasoning, document it, and articulate it as he presents his evidence.

The process by itself probably has no real value in convincing a board to accept the particular evidence that's offered.

So those are a couple of thoughts that you might want to pursue with actual legal counsel rather than a member of the technical staff if you develop this kind of workshop that Dr. Pomeroy is suggesting.

Oh, one other point that I wanted to offer, I've also been lead to believe that the Federal courts will not review an NRC licensing decision on the basis of its technical merits. There's supposed to be a precedent by the Supreme Court telling the appeals courts to stop doing that, that the purpose of the NRC is to make those technical findings and the court should defer to the NRC's technical expertise.

Of course, creative lawyers can always dress up a technical argument in legal grounds and get it reviewed in the courts. But that again might be an area for you to investigate with legal counsel just what are the limits of the Federal court's ability to delve into technical matters versus sticking with strictly legal or procedural matters if a decision is appealed to the courts.

MR. POMEROY: Could I just speak to that for a

second? Of course, as you are well aware, more than I am, the real purpose of the formal elicitation of expert judgement is to clearly delineate and document the thought process that goes into there. I stress the word "document."

I believe we should utilize some sort of formal elicitation of expert judgement every time we make an insert of expert judgement. We're tempting to do it implicitly. I think we should do that explicitly. I think we should document that very carefully.

So, that would be my rationale for agreeing on some methodology that meets some sort of current consensus in the community, but would provide a sort of uniform basis for inserting expert judgement into the system.

This is a non-lawyer talking to a non-lawyer, now, I do agree that courts don't tend, as far as I understand it, to turn over technical decisions based on technical issues.

However, I think, as I said yesterday or whenever, the courts could be persuaded, I think, relatively easily if the Federal rules of evidence have not been utilized by the Commission to reconsider the Commission's decision based on the fact that the evidence that was introduced was faulty. It would not qualify as evidence in a Federal court.

That's not a technical issue. That's a matter of law.

MR. FEHRINGER: Yeah. I think that's an area where legal counsel is really needed.

MR. POMEROY: Right. But I think somehow this needs to get conveyed in some way. This whole set of arguments needs to get conveyed in some way to all of us, but particularly to the Department of Energy because it seems to me, as Marty said, before we go too far down the primrose path, we should find out whether or not this is a potential showstopper or not.

MR. MOELLER: Steve Mays.

MR. MAYS: I would suggest on that issue of reviewability of technical decisions that we have a recent example that might tend to make you think about it again and that is the recent court decision in which the Federal courts overturned an EPA decision on asbestos, primarily on the basis of subsequent expert testimony, the basic legal argument being that the decision of the Administrator at the time was capricious.

So, there was an example in where the technical judgement of the legally-appointed body at the time was subsequently challenged in a court, not on the basis of procedural error, but on the basis of fact.

MR. POMEROY: Why don't I suggest that we think about it for a month? We don't have to make a decision. Sooner or later I would like to get back to Warner and talk

with him more and perhaps I will in an informal way.

MR. MOELLER: Sure. We may even have time tomorrow. We'll see.

MR. HINZE: Would it also be reasonable to see whether our OGC has the resources, the interest, whatever, to conduct this kind of a study that we might suggest to the Commission? Before we get too far along the process, what kind of interest is there in the OGC?

MR. POMEROY: Again, Dan can probably speak a little more to that. I would think there would be a great deal of interest in the OGC simply because I'm sure that Jim Wolfe knows. He'll be faced with these issues. He's been faithful in attendance at some of these meetings to recognize the issues that we're discussing.

MR. STEINDLER: Yes, but he's also no spring chicken. Remember this is a very slow process.

[Laughter.]

MR. POMEROY: Are you speaking for yourself?

MR. STEINDLER: Yeah, I am speaking for myself.

MR. FEHRINGER: I would just say that if you do make a request to general counsel, it's probably better to go to the hearings attorneys rather than the rule-making attorneys. They have a little more experience in the proper areas. Jim Wolfe is a regulations lawyer rather than a hearings lawyer.

MR. POMEROY: Thank you. That's a good point.

MR. STEINDLER: Is it likely that if you got a whole bunch of folks together in a room -- well, that's almost biasing the case -- but to address the question: What is the optimized methodology for eliciting expert judgement -- is it likely that the staff, for example, could write a set of ground rules to do this and not get an enormous amount of flack from the rest of the community on that process?

MR. POMEROY: I suspect if you limit the discussion to people within the United States, that there is a pretty good unanimity at this point in time simply because there is only one predominant methodology since all of the practitioners have gone to the same school, in essence, and have been trained by the same professor.

That's not necessarily the best methodology. On the other hand, Dan of course, is quite correct that it's not the methodology that does anything for you in this game, it's simply a matter of making it simpler in the licensing process to identify what's happening.

MR. STEINDLER: Bill raised, I think, some interesting points which ought to be raised by almost every expert who's been asked to participate on panels. What is this methodology going to do to my ability to make stick my view? That's a very good point.

The other question I would ask is whether we ought not to be thinking about back-up methods. Suppose this whole thing falls apart for one reason or another and conflicts can't be resolved between the European model and the American model, as is true in the case of the weather forecasting, do we have another way to get at the kind of answers without flipping coins?

MR. POMEROY: I don't know. My feeling is that you can get at the answer either way there. I think it would be very nice to have consistent methodology.

MR. STEINDLER: Do you get the same answer?

MR. POMEROY: Well, we never get the same answer. You know that.

MR. MOELLER: Okay, we'll certainly return to this. The discussion has been helpful to me in beginning to have a broader view of the subject and some better understanding.

The next item on our agenda, and the final formal discussion this afternoon, is the Working Group Chairman's report on the meeting on total system performance assessment. We will go ahead with that then, now, Paul.

MR. POMEROY: Dave, what I thought I would, if it's acceptable to you, is I thought I would review some of my impressions of our meeting yesterday. I've talked informally with a few of the other members of the committee.

They have, of course, their own perspectives. I'd like to try to gather all the perspectives of the Committee together.

Then, I will listen while other people talk. From my perspective, this was a very important meeting in that it allowed us the opportunity to do a side-by-side evaluation of both the NRC and DOE programs in performance assessment. I don't think we are going to have an opportunity like that, unless we force it, for at least a period of another two years. That is, I don't think there will be TSPA-2, or even an Iterative Performance Assessment Phase III completed before a period like two years.

My first statement is a laudatory statement. Both of these programs have made major significant progress since my individual last reviews, and our last review, and both of them deserve a great deal of commendation.

My second point is, that with regard to what they said they were going to do, in general, they have accomplished the purposes that they set out before they began. They have done that work, and we are going to have a lot of comment on that shortly, but they have also laid out reasonable goals for themselves, although extremely ambitious goals for themselves in the next iteration of their performance assessment.

The third point that I want to make is that I

believe, in particular, the NRC Staff should continued to be provided the resources necessary to continue their PA work. Enhanced computer capability is on its way, I understand, and other appropriate resources should be considered.

However, I am concerned about the totality of what is being done by the NRC Staff, and I believe that a careful evaluation of NRC's need to develop codes, models, should be carried out and, where possible, codes developed elsewhere in the community, be it national or international, should be utilized. There are economies to be realized. It is not, in my mind, necessary that NRC develop every one of its own codes.

My fourth point, and perhaps my most important point is, my principal concerns, and they are generally shared by our consultants as well, involve two things that this committee has been involved with for a long time, namely the treatment of uncertainty by both groups, and secondly the use of expert judgment.

Let's not belabor the question of expert judgment, although I think it is perhaps even more important. We have discussed it to some extent, and we can do more, if you would like to do so. I have great difficulty, when I sit through three days of performance assessment and listen to both groups, to get a clear picture of the totality of the uncertainty that exists in any given result, or even any

given set of models. Why is that?

Well, the models are certainly difficult in themselves, but they are not impossible. I believe that there are a number of problems. One is that you have a simple variable uncertainty that where you have data you can get different people to provide you a distribution on the data. When you step outside that range, and that, of course, is primarily where we are right at the present time, because we have so little data, then you get into this question that Steve brought up, and that I have brought up before this committee before, namely the degree of belief probabilities.

And if you ask six experts, you may get six different ranges of a variable distribution with little or no overlap. That should be documentable, however.

What I am concerned about is that there are conceptual uncertainties underlying many of the calculations that we have looked at in the past several days, and let me offer you one example of that, which may or may not be an important example.

The flow and transport models of both groups are bounded by some physical boundaries, and both of them tend to exclude any possibility of feed, for example, from the Solitario Canyon. Linda Lehman, who works for the State of Nevada, has prepared a paper based on what I think is a

reasonable dataset, although I haven't had a chance to review this in great detail, offering an interesting hypothesis that there could be a focusing of flux into the repository system from the West. That is precluded by the models because of the choice of boundary conditions and so forth.

Her statement, which was made very carefully, I believe, was, that might involve three to five orders of magnitude difference in what you consider, and she asked simply that that be considered.

I think that is a reasonable request. I think that three to five to orders of magnitude uncertainty, and those calculations ought to be considered and assigned some probability.

I can't identify, in critical areas, where there are similar types of uncertainty from the presentations that we have had, and from what I have read. I think that the two problems of uncertainty and expert judgment are extremely difficult problems that we really need to continue to investigate at some significant level of detail.

You know of the SECY documents that have been prepared on the treatment of uncertainty by the NRC Staff. I don't believe that adequately addresses the problem. I certainly don't know what the Commission thinks about that. I think we should continue to pursue uncertainty and find

where these sources of uncertainty are.

I have a number of other comments, Dade, and I would like to run through them. None of them, perhaps are as potentially dangerous as those two items, namely the uncertainty and the use of expert judgment.

The next one is, neither the NRC nor the DOE provided many examples of the changes in performance assessment brought out by other groups within the same agency, nor changes in actions of other groups resulting from the PA. We got lists of those. A lot of those lists, at least from the NRC's perspective, were changes in models one way or another. Those aren't the types of changes that I was thinking about, at least when I posed the question.

As Felton Bingham pointed out, there was a rather communality of interest and results in the lessons learned. By and large, DOE could concur with many of the lessons learned by the NRC Staff, and hopefully from Felton's comments, they have learned some of the same lessons.

There are some fundamental problems in definitions of scenarios, and CCDFs, that the staffs have agreed to work on. However, it is somewhat disconcerting -- I guess that is a good word -- that this late in the performance assessment process there are differences in the NRC's concept of those issues versus the DOE's concepts of those issues.

It isn't clear to me that other priorities within DOE, as they were outlined by Dr. Boek, will allow a timely completion of the TSPA-2. In fact, I believe, from what he said, that it had the lowest priority of a number of other issues. I understand that, and I am concerned that portions of performance assessment within DOE will be -- I am not concerned that they will be used in other areas, but I am concerned that that use will detract from a continuing systems performance evaluation.

Both groups should be encouraged to maximize their input to the Academy of Sciences Committee that is in the process of being formed. Particularly, the staff, in the past, has conveyed their ideas on a risk-based assessment, as you brought up this morning, I believe, Marty, they should certainly continue to express those views to the Academy.

I think most people would like to see the word "validation" go away. It is a terminology that is preferred, certainly by the Europeans, but many people would like to see it replaced by something like, adequate to the purpose or intended use.

Because I am concerned about uncertainty, I am concerned about the use of a "turnkey" sensitivity and uncertainty capability. That is the development of a code that you plug in certain parameters at one end and you

inject the sensitivity, I am not as concerned about that. I am concerned that the right numbers come out in the terms of uncertainty at the other end.

Although I didn't ask that question during the presentations, I think that that is something that I am personally going to look into in some detail as far as the staff is concerned.

Margaret Federline brought out a point at the end of our discussion that the NRC IPAA, if I understood her correctly, will produce, to paraphrase, a group of good PA analysis, and it is importance as a training tool certainly should be continually emphasized and encouraged.

Individual dose calculations should be encouraged by all of the groups involved. Certainly, the concept has been around now for a significant period of time, yet what we have seen to date in terms of individual dose calculations is certainly minimum, and we should be seeing more of that.

There are a large number of comments that I could make about codes. Let me just make a few. One is that the NRC developed a modified codes. If we are going to develop codes within the NRC, we should try to achieve some sort of a peer review, a formalized peer review of some of those codes to make sure that we have identified all of the possible holes in their construction and, in fact, of

course, as always, their use should be peer reviewed also.

One thing that was very useful to me yesterday was the tree models that Felton Bingham used to map out the PA modelling efforts. I know NRC does some of that, but I think that these road maps of the modelling structures certainly might be considered by NRC as we develop any of our own codes.

I also have about ten pages of comments from one of our consultants, Dade, which I need to review further before I present them, and I would like to simply ask that I have the opportunity to do that before we talk about that.

Let me stop there. I do have some thoughts about where we should go from here, but I would like to hear what the other members have to say.

MR. MOELLER: Bill.

MR. HINZE: Could I ask a question, as you were talking about your two key things, uncertainty and expert judgment, I wasn't able to be here the first two days of it, was anything said about the use of fractiles in trying to minimize the uncertainties in physical hydrological parameters, has anyone approached that?

It seems to me there is a piecemeal of indefinite detail that one might be able to use to try to get at the scaling problems here, and I am certainly not an expert on fractiles. I have sat with Don Turcotte and tried to

understand them, but I wonder if anyone has touched up on that?

The fact of the matter is, we were going to have a working group on fractiles at one point in time, and it seems to me that there are better statisticians than I would want to be in the room, and I would be interested in whether there are some approaches here, or perhaps we could be thinking about a working group meeting that might help us to find out what the proper role of fractiles are.

MR. POMEROY: First, let me say I agree with you 100 percent. There is a great deal that we might learn out of that process, but certainly not in the last three days, nor in my associations with the performance assessment groups have I heard of people actively considering applications of fractiles to this, but perhaps Georgio has another thought.

MR. GNUGNOLI: If I recall correctly, I believe, I can't remember whether it was Mike Wilson or Eslinger, discussed that they did use fractiles in terms of looking at the fracture flow, and I was curious that that is what stuck to me, but they did, and there was a very short mention of it. I do believe that DOE, either at PNL or Sandia are looking at fractiles for that purpose.

MR. HINZE: We are seeing a lot of papers on that topic now in terms of fracture, what might lead to fracture

flow using fractiles.

MR. POMEROY: I certainly missed that reference.

MR. STEINDLER: Fractiles won't qualify under the Federal Rules of Evidence.

MR. POMEROY: Yes, sir. I am aware of that. Chaos might.

[Laughter.]

MR. HINZE: Some of the problems in seismic prediction are not too different than what we are dealing with here in a small sample determination of properties of a block of ground, and fractiles are being used in that area, not notably successfully, but they are coming along.

MR. MOELLER: Paul, I have a few comments, but let me begin, though, by complimenting you on your summary, and I was think, you know, if the committee, and I don't think we are headed this way, but if we were to write a letter on this, it would certainly be a long letter, not that it is negative, but it is just that there are so many things to comment on.

I would begin by repeating your compliments to both the DOE and NRC Staffs, and what really impressed me was the way they communicate. They are all seeking the very best scientific effort that they can accomplish. It almost reminds me of the neighbors where the children play and get along well but the parents don't.

At this level in the staff, the communication is obviously there, and either they are good actors -- and I don't believe that, I think it is sincere. There is no holding back. They really open up, they lay their cards on the table.

I believe, in line with that, again, Margaret Federline is to be complimented on organizing it, and on this strategic plan that she is having them develop. That is excellent.

I would repeat your comment that, in terms of defending the NRC effort, don't forget training and education of the staff, and getting their hands and feet dirty so that they know what it is all about. That, to me, is as important as almost any other aspect.

Let me just zip through a number of things, most all of which will reveal my ignorance. First of all, we heard that they will be publishing this in NUREG-1464. I presume, and they can answer it later, that they will hold up NUREG-1464 until they can do some runs. I don't know that I heard that, but I hope that is true because they said the runs were imminent.

Again, taking these in no set sequence, just the way I put them down, they showed that they had the four-by-four matrix for the scenarios, and they showed that 12 of the 16 classes that you end up with in the four-by-four

matrix have probabilities of occurrence below this rationale that even EPA requires that it be considered.

So my immediate response was, then are only four scenarios survive, so you need to go back and create a new set. Then they said, those that are thrown out, or that would be rejected have subclasses that wouldn't be rejected.

That is fine, but if there is some step in the total scenario that rejects it, then I still -- I don't understand why I wouldn't throw the whole thing out. So that is where I need education.

To hit a key point, this one leaves me troubled, and I don't know how to address it, and I hope I am not misquoting either the DOE or NRC Staff, but DOE said, at the moment we are not following QA practices, we will work those in later when we get over the hump and are really getting into the real final production stages.

Then the NRC said, again, revealing my total ignorance, said, whereas DOE must follow QA procedures, the NRC Staff doesn't have to. Someone said, well, they met only QA-1. Again, that doesn't help me too much.

So I think we need to ask or have clarification on that. The DOE asked -- and, again, Margaret Federline, throughout the meeting was making a list of major topics for further or for new forthcoming technical exchanges, but DOE did request several times for guidance or criteria for

developing scenarios, and apparently they do not, although they seemed in the exchange to better understand, but they did have questions.

One thing of interest to me was the fact that several of the DOE groups showed that they, in order to overcome this problem, if we go to a dose limit for individuals, in order to overcome the problem of a dry site giving too high a dose, one group was going to assume a 1 million gallon per day consumption of water, and that would be for everything, and factored into the DITTY Code and come out with doses.

Then, as I recall, another proposal we had was to put limits on the aquifer in which the release would be diluted, and assume that that is a standard biosphere aquifer. Others can explain more about that.

This one troubled me somewhat. I heard and I guess this obviously was DOE. They said although plutonium dominates the estimates of the dose they are not going to give it any special treatment or they have not. A more accurate way of saying it is they have not up to this point given it any special treatment. In other words, they just treat it like anything else, I guess, and I would think if plutonium does indeed dominate the dose then, over whatever else dominates it, then those nuclides should receive special treatment.

MR. STEINDLER: What do you mean?

MR. MOELLER: Well, I mean apply solubility factors or transport factors or release limit -- you know, treat it separately to define as carefully as you can the behavior of that particular nuclide.

They pointed out, I guess it was DOE, and I found this interesting, that again if you look at human intrusion and want to keep the dose to a minimum, you want the repository to leak as much as possible to spread and uniformly spread out the waste so that when you drill into it you don't hit a hot spot. That was facetious but it is, you know, it gives you pause for thought.

They are, they did define or list lessons learned. They do appear to be using their performance assessment to help plan site characterization. Steve Frischman of course, from Nevada, urged that they get on and devote more time to data collection and less to PA. I don't necessarily agree but it is in line with certainly using the PA to determine what your data needs are.

I, too, shared the question and again it's my area of, one of my many areas of ignorance but David Okrent asked them time and time again, you know, how are you dealing with uncertainties? Are you quantifying the uncertainties, and so forth, and I think at this point they are really not doing too much in it but then in the very end on Tuesday

afternoon we see that they have a computer code, as you mentioned. You just plug everything in there and it tells you all the uncertainties. You don't need to know anything.

Well -- you know --

MR. POMEROY: That's what I am concerned about.

MR. MOELLER: They also raised several times how useful or valid is the Kd concept and of course I don't know. Other people will have to consider that.

Oh -- they did show that in the CCDFs that whereas in the main the individual ones comply with the standards, they did show that this 10 to the minus fifth, Part 60, release, violates I think the CCDF or, you know, the EPA standards in several instances, which is interesting.

Then their bottom line of course of the technical conclusions, those are very interesting. Even if qualified, they are beginning to give us perspective, so I think with that I'll quit.

MR. POMEROY: Can I add one thing in there that I did forget? It bears some again on this uncertainty question and that is the process of abstraction concerns me as part of the uncertainty problem.

We see a pyramid of very detailed discussions and there may be a careful consideration even of the ranges of uncertainty down at that bottom level where the detailed calculations are carried out, but my uncomfortable feeling

comes from the fact that when you abstract and what I call simplify that process that those ranges of uncertainty don't get adequately translated into the abstracted or simplified calculations that you are doing -- at least I can't see where they are carried through in their entirety.

Somehow that should be a process that is clear in the process of abstraction. Excuse me, Marty.

MR. STEINDLER: I don't know why I'm next. You know, all the things that have been said about the first two days I guess I'm sorry I didn't, I couldn't get here. Sounded like it would have been quite useful.

However, you know, I have already basically made some of the comments. One of the things that we keep addressing indirectly is the whole issue and expert judgment is a good example of what is going to be acceptable, not so much to the technical community but this thing has to be palatable to the world-at-large.

I have some real problems with the way codes are currently assembled. They are fundamentally opaque. They are even opaque to the user, which troubles me probably most of all and when somebody says, oh, gee, I have discovered this great way of compressing this code so I can run it in two minutes of Craig time instead of 20 hours, you know, it's mandatory to know what that does.

In that same vein I think it is important to make

sure that the experimental data that served as the underpinning to a model or a code are in fact relevant to the scenarios that are being described by that code. That is not always the case. In fact in many instances it is not the case and, as you know, we have made comments to the Commission concerning repository relevant experimentation, which is quite scarce.

I suppose during the first two days somebody addressed this issue and I don't know whether that is true or not but I hope somebody talked about the question of when are the answers good enough. That comes perhaps under the heading of the treatment of uncertainties but at some point refining the model, for example, to look at the impact of U-238 decay daughters on the dose at 10 to the fifth and 10 to the sixth years out strikes me as rearranging the deck chairs on the Titanic. I don't think that is a very useful exercise and it all comes under the heading of what is it you really want to know from the PA results? When should you quit?

I am troubled by the general notion that if we can't make sense out of what goes on we are going to try and arrive at a fundamental understanding of the mechanism of whatever the process is, and it seems to me by now it should be evident that we don't have the time or the resources to get at the mechanistic underpinning of that complicated

system called the Yucca Mountain area -- or for that matter any other repository, yet I keep hearing it as being the target, the goal. I think that's a mistake.

I am not very charitable about the impact of the answers from performance assessment exercises on research and development. It wasn't clear yesterday that that was a significant issue. Now it may have been for the first two days when people talked about details but I couldn't see where the results of performance assessment influenced the start or the stop of research into new or old areas.

I thought at one time there was a general agreement that PA ought to be in fact driving the direction of the programs. I didn't see that. If it was there, I would be pleased to hear about it, but I didn't see it.

You know, that and the other things I have mentioned before, that's basically it.

MR. POMEROY: Could you comment though on the Carbon-14 issue?

MR. STEINDLER: Yes. The Carbon-14 discussion was interesting in several ways. In one case the detailed steps that were followed were quite instructive and I thought they were reasonably well done until we got to the end discussion and then it turned out that a fundamental difference between the two models -- remember the one that somebody commented on the fact that the drying out process of a heated waste

emplacement forms a layer of essentially fully saturated rock impermeable to gas which ought to totally bar transport. That's such an obvious fundamental issue that I was a little bit surprised that the first group didn't consider it.

Now what does that tell you? If you are willing to extrapolate, it tells you that there are some real problems in scenario definition that need to be done first and ought to be in fact the subject of a lot of internal discussion between DOE and NRC and whoever else is involved.

So once the scenarios are outlined, then models can be constructed by arithmetic issues that everybody has their own way to attack. But at least the fundamental generic issues of a model can be outlined and perhaps agreed to.

I looked at the chemistry and all the other things that were going on in that carbon 14, there are some fundamental assumptions that are probably okay, but they are not demonstrably okay.

I have no idea whether or not carbon 14 and circ alloy comes out as carbon dioxide. In fact, I have some doubts about it. Circ carbide, for example, is a fairly stable material. I would expect to sit in there as a carbide rather than something that can be easily leashed as carbon dioxide.

But it may not make any difference. So, you know, that's not that big a deal. But I thought it was a good example of some difficulties.

I have trouble with a linear model. As I mentioned, my intuition somehow or another got bent out of shape when somebody said that the travel time from a 1,200 foot repository depth to the surface is only measured in tens of years by a process of breathing due to the fairly small delta T between the waste itself and the surface.

Somehow I would have guessed, to show you how little I know about it, that would have taken in units of hundreds of years or longer. But, you know, I can't verify the thing one way or the other.

MR. POMEROY: Thank you.

MR. MOELLER: A quick question on that. You know Steve Hershman mentioned that the heat from the waste would be a driving force to send radon out. As you talk, or as I am thinking as I talk, radon, of course, has only a four-day or whatever it is, half-life. So, radon very deep in is never going to reach it. So, it would only be if it influences the upper meter or so of the soil that it's going to be of any consequence.

MR. HINZE: Well, it's hard to add to those profound and excellent statements. But I would like to add a few comments based upon my own perspective. I guess it's

time that I came out of the closet.

I really have been very concerned about PA because I've been concerned that the quantification that one gives to it lends a respectability to the result that perhaps may not be justified for a number of reasons.

From that aspect, I was pleased with both the reports that I heard from the DOE and the NRC in that they caveated their results and tried to specify the assumptions, although I guess everyone could sit back and think of many more assumptions that should be taken into account.

I agree with Paul that the groups have come a long way, but as I said here and looked at the trees and the lack of inter-mixing of the trees, and the number of problems that really needed to be developed, I think we have a long way to go. I think there's a long way to go, and perhaps longer than we think even at this time. I think with that in mind the whole idea of resources is terribly important.

The question that I have and asking myself is what is the role of the PA analyst. I had a perception about that and I'm going to try to answer that question, but I think we ought to be thinking about it because what I heard in yesterday's session was -- dependent upon the group, even to the point of lip service -- in terms of the interaction with the scientific community, that are directly involved.

I think this goes back to some of Marty's

statements regarding the opaqueness of codes. I think that the only people that really can deal with that properly are people that are experienced in the science that those codes are trying to deal with.

They have to, if you will, properly cull and exercise them. They have to play with them. They have to exercise them. Gosh, I became very concerned when we hear statements such as, "Well, we're not going to take this out and show our results to the hydrologists."

It seems to me that the PA analysts have really had is the PA analyst is the person that provides an interface with the computer expertise, and also interfacing different disciplines because, to me, the PA analyst should not be running the hydrology codes. The hydrologists ought to be doing that.

What the PA analyst should be doing is making certain that we have the coupling of these processes. I hate to use the word "coupled" process because it's perhaps a little overused.

But I truly believe that in this process that one plus one can equal three or 300. I think as we kept our eyes open yesterday, we could see where that interplayed, where the vulcanology and the dykes and the orientation of the dykes could have a swamping out effect upon the hydrologic characteristics.

Who cares what the uncertainty is because you're going to flood it, depending upon the orientation of where that dyke might be.

So, it seems to me that there needs to be a pretty clear definition of the PA analysts role. I, for one, am reluctant to give that person the role of what I call the working scientists in the field.

I thought the carbon 14 results of the NRC were very interesting, but I would have very much liked to have seen how they compared with the DOE. I mean, I thought that was what it was all about.

I think that there needs to be much more critical consideration of the comparisons of the results and how they got there. I guess that goes to even results that come out the same. Just because they come out the same, as we heard yesterday, doesn't mean that they're going to be right.

But if they come up the same way -- use different codes, it's telling us a very great deal. There must be something in that modeling, in that analysis, that we can gain knowledge that we can gain by that kind of comparison.

I think as various iterations are performed, we really have to understand why we're getting different results. Sometimes I think that's rather opaque.

Nothing new to modeler is the problem of the density of grid size to cells in a spacial way. That very

much enters into the uncertainty problem and can be really minimized with that, if you approach it from one way. Certainly there's the spacial density. But there's the temporal density.

My own studerts do a lit of modeling. They wouldn't be permitted to start with a modeling procedure until they had defined what kinds of density that they needed in their models.

Also, the boundary conditions. How far out do they have to go? You mentioned Linda Lehman's concern about leakage from west. Do we go out beyond the Solitario Canyon? Do we go out beyond Bare Mountain? Does Bare Mountain fault make that contact?

Well, I think there needs to be evaluation of those things. I guess that really leads to the next point. I heard this particularly from NRC from Norm about simplifying the codes and speeding up the codes.

Maybe it's my lack of understanding, but it seems to me that the codes are going to get much more complex. If they don't, we're all in serious trouble. What we shouldn't be talking about is speed of codes, but simply the efficiency of codes.

Can these be run on a smaller machine and run for a week? Some of the things we do, we handle that way. We don't have a huge machine, but you can let it run for a

period of time. I guess I'm kind of concerned about that.

The expert judgement sounds really interesting -- the 2.5, I guess it's called. I would like to learn more about that because we're thinking a lot about expert judgement. I would like to have a better feeling of how the staff -- and this may be helpful to us -- of how the staff is going to take this elicitation of global change and put that into trying to reach a decision regarding any guidance.

It's important to carry that through with one process. That was, after all, our suggestion, I guess. But there have been a lot of other approaches to this. I'm sure that they're going to take those into account. They need to be, certainly.

Well, those are some of my comments.

MR. STEINDLER: Let me make a couple of comments on one of them I forgot.

You know, you asked the question as to whether or not we should send a letter or we should communicate with somebody about it. I think the one short-term issue that I think we might raise is to perhaps address a letter to somebody within the NRC to ensure that continued support is provided for the folks here -- You know, to give resources and funds for people but as well as the -- you know, make sure that their computer needs and their interactions needs are not set in the second level, down below someplace. That

may be it.

MR. HINZE: Could I add to that? A major resource is access to the working scientific groups within the Agency. Maybe they have that but the PA people have to have that.

MR. STEINDLER: That would be I think one of the few reasons I would think that it would be worthwhile to at least communicate in some fashion or another and I would assume we'd have to do this by letter. I guess that's it.

MR. POMEROY: Dade, one of the things that we talked a little bit informally about might be worthwhile bringing up here. We have discussed the possibility of writing a letter but I believe it was Marty's suggestion that one thing that could come out of this -- two things should come out of this.

One is that there should be some documentation in addition to the transcript of our lessons learned out of this working group but the second is -- and could be contained in that document -- some list of the critical issues and I see about four or five here that could be really critical issues to the entire process.

One suggestion may be that we might want to consider actually a working group on each one of those issues and again I am not looking for work but I think those, there are several of these issues that need to be

pursued further than we could in a one day review session of two major programs.

MR. MOELLER: Are those different than Margaret's list?

MR. POMEROY: They are somewhat. Bill's issue and concern about the analyst's role --

MR. MOELLER: Okay.

MR. POMEROY: -- what we do with that. There were a number of concerns that he cited in there. The expert judgment and the uncertain question I think is one that we really would like to pursue, we should pursue because if indeed there is a problem, a significant problem, with clearly defining the uncertainties in this, that is another very dangerous aspect. One could make very short statements about the usefulness of performance assessment if those can't be adequately addressed.

MR. MOELLER: Back on your list and I meant to ask it earlier, Paul, when Linda Lehman made the presentation about focus flux, I thought I understood it but I then found I was left with a question.

If you could somehow because of, you know, rainfall coming down and then going into streams and puddling or pooling in a lake, if that lake were on top of Yucca Mountain then I would be concerned. I saw the lake as being down in the valley so how does it then affect the

repository horizon if it is well below that horizon? Or could it be above that horizon?

MR. POMEROY: Let me say one thing first. This is an episodic kind of treatment.

MR. MOELLER: Correct.

MR. POMEROY: Most of the treatments that you saw yesterday and the previous two days are steady state treatments and so any way of treating her hypothesis or her assumptions would have to be treated in an episodic way and that isn't readily available right at the moment but it can be done.

There could be flow -- this would involve some almost, some sort of a flooding concept but you could conceive of flow into the west side of the mountain from Solitario Canyon. That could horizontally flow.

I haven't read her paper so --

MR. MOELLER: Is that below the horizon though?

MR. POMEROY: That could be below, partially below and partially above the horizon.

MR. MOELLER: Okay --

MR. HINZE: But you can get ponding above.

MR. POMEROY: Yes.

MR. HINZE: There are a lot of opportunities and the fact of the matter is that Alan Flint I think it was that made a pr[illegible]entation to the TRB a couple of years ago

that I sat in on that was talking about some of these high flux times when you have the storm activity and the backup and the net result.

I mean if you look at percolation, water flux into the earth, anyplace it's on a very episodic basis. I mean there are only a couple of periods of the year when it really happens.

MR. POMEROY: But you can have the purged water situation under certain circumstances and I would like to read her paper. I think she's going to submit that for publication in the normal process. She's used totalled data for the past 20 years. I think it bears looking at but I used it only as an example of the boundary conditions that you choose affecting the outcome of the problem, perhaps making it an artificial outcome.

MR. HINZE: Are we each going to get a copy of her paper incidentally?

MR. POMEROY: We have a copy.

MR. HINZE: Okay, it will be distributed then. Linda Lehman.

MR. MOELLER: Yes, it was distributed, if not yesterday maybe it was the day before..

Okay, well, does the Staff have any comments before we wrap this up?

Do you have any comments?

[No response.]

MR. MOELLER: Okay, the Staff has no comments. Well, then, we are reaching the conclusion that we may write a letter. The letter would have a paragraph saying this is important work, please support it, and then it might even say in addition to the list that the Staff is compiling we have this list of topics that may be worthy of consideration for future working group meetings or even DOE/NRC technical exchanges.

Okay, we can say something like that.

MR. POMEROY: I think there should also be a paragraph in there however, somehow -- we don't always have to compliment the Staff but they have done an extremely large amount of work in a very short period of time, relatively speaking and I would like to see something commending their efforts.

MR. HINZE: But let's also say that there is a long way to go.

MR. POMEROY: There's a long way to go, I certainly agree with that.

MR. MOELLER: Giorgio?

MR. CNUGNOLI: I guess in that first part about the importance of the work is where you would address the fact that they should get more resources or maintain the resources.

MR. POMEROY: Right.

MR. MOELLER: Okay. I think then with that we'll wrap up, to repeat, the formal portion of today's meeting and in fact this concludes the recorded portion of the day's meeting and we will now have a break and after the break the Committee will plan our January agenda, future working group meetings and begin to prepare some of the reports that we have been talking about.

Let me express again our appreciation to Steve May and the NRC Staff for being with us this afternoon and helping us to recollect what we've heard over the past three days.

Thank you.

[Whereupon, at 3:46 o'clock p.m., the recorded portion of the meeting was concluded.]

REPORTER'S CERTIFICATE

This is to certify that the attached proceedings before the United States Nuclear Regulatory Commission

In the Matter of:

NAME OF PROCEEDING: 49th ACNW Meeting

DOCKET NUMBER:

PLACE OF PROCEEDING: Bethesda, Maryland

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

Official Reporter
Ann Riley & Associates, Ltd.

PRESENTATION TO THE
ADVISORY COMMITTEE ON NUCLEAR WASTE
DECEMBER 17, 1992

NOVEMBER 1992 RESOLUTION OF SITE CHARACTERIZATION ANALYSIS OBJECTION 1

DIVISION OF HIGH-LEVEL WASTE MANAGEMENT, NMSS

SCA OBJECTION 1

INTRODUCTION

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STAFF'S REVIEW OF OBJECTION 1

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SCA OBJECTION 1

OBJECTION:

Concern of such immediate seriousness to a particular area of the site characterization program that NRC would recommend DOE not start work in that area until it is satisfactorily resolved.

DOE committed to resolve objection-level concerns prior to proceeding with related site characterization work.

SCA OBJECTION 1

Lifting of an objection means that concerns related to that objection have been resolved at that particular point in time.

Staff is obligated to continue to evaluate activities related to an objection.

Staff has an obligation to reopen any objection when warranted by new information or analyses.

CHRONOLOGY OF OBJECTION 1

DECEMBER 1988 - SITE CHARACTERIZATION PLAN ISSUED

JULY 1989 - SITE CHARACTERZATION ANALYSIS ISSUED

DECEMBER 1990 - DOE RESPONSES TO SCA (CLOSURE OF OBJECTION 1 IS NOT REQUESTED)

JANUARY 1991 - CALICO HILLS RISK BENEFIT ANALYSIS ISSUED

JULY 1991 - EXPLORATORY STUDIES FACILITY ALTERNATIVES STUDY ISSUED

SEPTEMBER 1991 - NRC LETTER REQUESTING INFORMATION ON HOW CHRBA AND ESFAS ADDRESS SCA OPEN ITEMS

CHRONOLOGY (Continued)

SEPTEMBER 1991 - DOE/NRC TECHNICAL EXCHANGE ON ESF

NOVEMBER 1991 - DOE REQUESTS CLOSURE OF OBJECTION 1

MARCH 1992 - DOE PROVIDES ADDITIONAL INFORMATION TO SUPPORT CLOSURE OF OBJECTION 1

NOVEMBER 1992 - NRC STAFF CONCLUDES OBJECTION 1 IS RESOLVED BASED ON REVIEW OF INFORMATION PROVIDED BY DOE RELATED TO THE REVISED ESF DESIGN AND DESIGN CONTROL PROCESS AND OBSERVATIONS OF QUALITY ASSURANCE AUDITS

SCA OBJECTION 1

- Concerns
- Bases
- NRC Evaluation of DOE's Response
- Future NRC Activities

SCA OBJECTION 1

- ESF to become part of the repository
- SCP & references do not demonstrate
  - The adequacy of the ESF design control process
    - Not all applicable 10 CFR 60 regulations considered
    - Inadequate integration of technical data
  - The adequacy of the ESF Title I design
    - As designed, ESF may not permit needed tests for sufficient duration
    - Design and site characterization activities may need significant revisions to resolve the identified problems

THE 6 BASES OF OBJECTION 1 CONCERN

1. Early performance confirmation and seals tests

2. The Design Acceptability Analysis and how it addresses NRC concerns

3. Test interference in the test area

4. Potential Impacts of long term performance confirmation testing

5. Design criteria - seismic, drainage, and shaft liner

6. Data requirements for site characterization and the repository design

BASES FOR SCA OBJECTION 1

Basis 1

- SCP design of the ESF does not adequately address the issues of
  - Need to consider starting confirmation testing as early as practicable during site characterization. (Example: possible early waste package testing)
  - Preliminary data on in situ seals tests as input to the License Application

BASES FOR SCA OBJECTION 1 (CONT'D)

Basis 2

- Design Acceptability Analysis did not address many NRC concerns, such as
  - Independence of technical reviewers
  - Regulatory considerations in the ESF Title I design and the Design Acceptability Analysis
  - Adequate consideration of starting performance confirmation testing during site characterization
  - Consideration of alternatives to major design features important to waste isolation (10 CFR 60.21(c)(1)(ii)(D))
  - Thoroughness of document and data reviews
  - Consideration of a known anomaly in the vicinity of the proposed shaft location

BASES FOR SCA OBJECTION 1 (CONT'D)

Basis 3

- SCP does not adequately address whether tests can be accommodated without interference
  - Incompatibility of tests with adjacent construction and operation activities
  - Thermal zones of influence were underestimated because of insufficient time considerations
  - Lack of rationale for sequencing of some tests
  - Lack of consideration of uncertainties in the calculation of the zones of influence
  - Lack of identification of needed tests
  - Lack of information on coupled testing
  - Lack of sufficient underground space for testing
  - Lack of consideration of existing boreholes

BASES FOR SCA OBJECTION 1 (CONT'D)

Basis 4

- Potential impacts of long-term performance confirmation testing are not adequately addressed.
  - Insufficient demonstration that in situ waste package testing is not needed
  - If such tests are needed, their impacts are not addressed

BASES FOR SCA OBJECTION 1 (CONT'D)

Basis 5

- Some ESF design criteria are not adequately addressed, including
  - Seismic design basis
  - Drainage volume and drainage reliability
  - Effect of shaft liner removal

BASES FOR SCA OBJECTION 1 (CONT'D)

Basis 6

- The ESF in the SCP may not provide sufficient data for
  - site characterization
  - repository design

STAFF EVALUATION OF DOE'S RESPONSE

Design Control Process

Observed surveillances and audits conducted by DOE and found them acceptable

STAFF EVALUATION OF DOE'S RESPONSE

Design Control Process (Cont'd)

Reviewed DOE's modified design control process, and found the following to be acceptable

- DOE's process for considering specific design requirements
- Revisions of DOE's 'Requirements Documents' so that they address applicable regulations (Systems Requirements, Waste Management Systems Requirements, and ESF Design Requirements)
- DOE's process for integrating technical data
- DOE presentations on examples of how the design control process is being implemented
- The process for technical assessment reviews for proposed design changes

STAFF EVALUATION OF DOE'S RESPONSE

Design Control Process (Cont'd)

The NRC staff is satisfied that the current ESF Title II design activities are being performed under the NRC approved QA program

STAFF EVALUATION OF DOE'S RESPONSE

Title II Design

Staff found that

DOE analyzed alternative design features important to waste isolation

- Both ramps and shafts are considered
- TBM's and mechanical excavation; not all drill-and-blast
- Considered alternative portal locations
- Considered alternative repository design concepts
- Considered alternative test locations and strategies

In situ waste package and seals tests are not precluded

STAFF EVALUATION OF DOE'S RESPONSE

Title II Design (Cont'd)

Staff found that

The preferred ESF option has the potential to gather adequate data for site characterization and repository design

- More drifting; 76,000 ft vs. 10,000 ft

There is less potential for test interference and more space for additional tests

- Larger Main Test Level; 800,000 vs. 400,000 ft^2

There is flexibility because of the ESF phased design approach

SUMMARY OF NRC EVALUATION OF DOE RESPONSES

Based on

- Reviews of pertinent portions of the ESFAS and CHRBA
- A review of DOE's 'cross-walk' that addressed the SCA open items
- Consideration of previous DOE responses, technical exchanges, observation audits, and surveillances

NRC concluded that

- There is an acceptable design control process
- The majority of the bases for Objection 1, and the supporting comments and questions related to Objection 1 are acceptably addressed
- The preferred ESF option addresses conceptually most of the SCA concerns

FUTURE NRC ACTIVITIES RELATED TO THE ESF

- Evaluate DOE responses to remaining open items
- Review major design reports
- Review the Title II design and design control process
  - Observation of 50% & 90% reviews
  - Observation audits

GEOMETRIC ANALYSES OF FAULTS AT YUCCA MOUNTAIN -

APPLICATIONS TO THE HIGH-LEVEL WASTE REGULATORY PROGRAM

A PRESENTATION TO:

THE U.S. NUCLEAR REGULATORY COMMISSION -
ADVISORY COMMITTEE ON NUCLEAR WASTE

by

Stephen R. Young

Center for Nuclear Waste Regulatory Analyses
San Antonio, Texas

Bethesda, Maryland
December 17, 1992

PURPOSE OF THE TASK:

i DEVELOP METHODS FOR REVIEW AND ASSESSMENT OF STRUCTURAL GEOLOGIC MODELS OF A POTENTIAL HIGH-LEVEL WASTE REPOSITORY SITE AT YUCCA MOUNTAIN, NEVADA

ii DETERMINE IMPLICATIONS OF ALTERNATIVE GEOMETRIC MODELS OF EXTENSIONAL FAULTS FOR PERFORMANCE ASSESSMENT

iii USE EXISTING REFLECTION SEISMIC DATA TO ASSESS STRUCTURAL GEOLOGIC MODELS OF THE YUCCA MOUNTAIN REGION

OBJECTIVES:

- TEST EXISTING STRUCTURAL CROSS SECTIONS OF YUCCA MOUNTAIN FOR GEOLOGICAL BALANCE (RETRO-DEFORMABILITY)
- CONSTRUCT A WELL INTEGRATED SET OF FAULT TRAJECTORY MODELS OF YUCCA MOUNTAIN *(A BASE CASE)*
- CONSTRUCT ALTERNATIVE GEOMETRIC MODELS OF FAULTS AND ASSOCIATED DEFORMATION
- MODEL FAULT TRAJECTORIES DIRECTLY FROM EXISTING REFLECTION SEISMIC DATA
- USE BALANCED 2-DIMENSIONAL STRUCTURAL CROSS SECTIONS TO CONSTRUCT 3-DIMENSIONAL SOLID GEOLOGIC FRAMEWORK MODELS OF YUCCA MOUNTAIN

RESULTS

- YUCCA MOUNTAIN IS UNDERLAIN BY A LINKED, LISTRIC NORMAL FAULT SYSTEM THAT MERGES INTO A SUB-HORIZONTAL DETACHMENT FAULT (OR ZONE) AT DEPTHS OF 6 TO 7 km (-5 to -6 km elev.).

- ALTERNATIVE GEOMETRIC MODELS OF FAULTING AT YUCCA MOUNTAIN SHOW THAT PLANAR (DOMINO) FAULT GEOMETRIES ARE NOT CONSISTENT WITH 'ROLLOVER' FOLDS MAPPED IN THE MAJOR HANGINGWALL BLOCKS.

- EXISTING GEOLOGIC CROSS SECTIONS (Scott and Bonk, 1984; Scott, 1990) ARE REASONABLY WELL BALANCED - THAT IS, THEY WILL RETRO-DEFORM ALONG BALANCED FAULT TRAJECTORIES.

- INTERPRETATION AND FAULT MODELS OF THE AV-1 REFLECTION SEISMIC LINE SUGGEST MULTIPLE-DETACHMENT MODELS SHOULD BE CONSIDERED AS A POSSIBLE PARADIGM FOR FAULT GEOMETRIES AT YUCCA MOUNTAIN.

- THE YUCCA MOUNTAIN FAULT SYSTEM PERSISTS EASTWARD INTO THE FORTYMILE WASH - JACKASS FLATS AREA.

IMPLICATIONS

EARTHQUAKE SEISMIC HAZARD

- DETACHMENT FAULT SYSTEMS ARE GENERALLY INTERPRETED TO BE ASEISMIC.
- IN COMPARISON, SEISMIC SLIP IN THE 6.0+ RANGE MAY BE MORE LIKELY ON THE BARE MOUNTAIN FAULT OR ON FAULTS PROJECTED ALONG THE EAST FLANK OF THE FORTYMILE WASH - TOPOPAH WASH VALLEY.

GROUND RUPTURE HAZARD

- SLIP ON THE MODELED LINKED, LISTRIC SYSTEM IS LIKELY TO BE DISTRIBUTED.

VOLCANIC HAZARD

- MODELED LISTRIC-DETACHMENT SYSTEM MAY INFLUENCE THE LOCATION OF SURFACE VENTS, BUT DOES NOT INFLUENCE DEEP-SEATED MAGMA ASCENT.

FORWARD MODELING

- PREDICT POTENTIAL SHAPE CHANGES OF REPOSITORY FAULT BLOCK DUE TO FUTURE FAULT SLIP.
- PREDICT POTENTIAL ZONES OF DISTRIBUTED DEFORMATION.
- SPECIFY DISPLACEMENT BOUNDARY CONDITIONS FOR DEFORMATION MODELS.
- MAP STRESS AND STRAIN PATTERNS DUE TO POTENTIAL FUTURE DEFORMATION.

FUTURE WORK

3-DIMENSIONAL STRUCTURAL/STRATIGRAPHIC FRAMEWORK MODEL OF YUCCA MT.

RESEARCH ON 3-DIMENSIONAL GEOMETRIC/KINEMATIC MODELING METHODS.

2-DIMENSIONAL DYNAMIC (DEFORMATION MECHANICS) MODELS OF YUCCA MT.

3-DIMENSIONAL DYNAMIC (DEFORMATION MECHANICS) MODELS OF YUCCA MT.

PRESENTATION TO THE ADVISORY COMMITTEE ON NUCLEAR WASTE

DEVELOPMENT OF METHODS TO ADDRESS STRUCTURAL DEFORMATION IN CHARACTERIZATION AND PERFORMANCE OF A GEOLOGIC REPOSITORY

PRESENTATION TO THE ADVISORY COMMITTEE ON NUCLEAR WASTE
DECEMBER 17, 1992

DEVELOPMENT OF METHODS TO ADDRESS STRUCTURAL DEFORMATION IN CHARACTERIZATION AND PERFORMANCE OF A GEOLOGIC REPOSITORY

Geology and Engineering Branch
Division of High-Level Waste Management, NMSS

OUTLINE OF PRESENTATION

Introductory Remarks	Ronald L. Ballard, Chief Geology and Engineering Branch HLWM (504-3462)
Geometric Modelling of Faulting	Steve Young, Geologist Center for Nuclear Waste Regulatory Analyses (210-522-5247)

ACNW 17DEC92

OBJECTIVES

- TO DEVELOP METHODS TO TEST THE VALIDITY OF GEOLOGIC CROSS SECTIONS SUBMITTED IN ASSOCIATION WITH A LICENSE APPLICATION
- TO DEVELOP METHODS WHEREBY FORWARD MODELING OF STRUCTURAL DEFORMATION IN THE REPOSITORY BLOCK CAN BE CONSIDERED FOR INCORPORATION INTO ASSESSEMENTS OF REPOSITORY DESIGN AND PERFORMANCE

PLANNED ACTIVITIES

Activity:	Status:
Prototype Testing of Computer Assisted Geometric Modelling (2D)	Completed - September 1990
Geometric Modelling of Faulting at Yucca Mountain (2D)	Initial Phase Completed - November, 1992 Iterative Efforts Based on Data Provided by DOE
Geologic Framework Models of Yucca Mountain	TBD FY93/94
Mechanistic Modelling of Deformation at Yucca Mountain at Repository Scale	Planned FY94/95
True 3D Models of Deformation at Yucca Mountain	Under Consideration FY95-97