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ADVISORY COMMITTEE ON NUCLEAR WASTE

In the Matter of:)
Fourteenth General Meeting)

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UNITED STATES NUCLEAR REGULATORY COMMISSION

ADVISORY COMMITTEE ON NUCLEAR WASTE

In the Matter of:

FOURTEENTH GENERAL MEETING

Wednesday,
October 11, 1989

Room P-110
7920 Norfolk Avenue
Bethesda, Maryland

The above-entitled matter came on for hearing, pursuant to notice, at 8:30 a.m.

BEFORE: DR. DADE W. MOELLER
Chairman
Professor of Engineering in Environmental Health and Associate Dean for Continuing Education, School of Public Health
Harvard University
Boston, Massachusetts

ACNW MEMBERS PRESENT:

DR. MARTIN J. STEINDLER
Director, Chemical Technology Division
Argonne National Laboratory
Argonne, Illinois

DR. WILLIAM J. HINZE

CONSULTANTS:

M. Carter
E. Voiland
P. Pomeroy

ACNW COGNIZANT STAFF MEMBER:

S.J.S. Parry

NRC STAFF PRESENTERS:

P. Justus
K. McConnell
M. Fliegel
G. Gnugnoli

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

CHAIRMAN MOELLER: The meeting will now come to order.

This is the first day of the 14th meeting of the Advisory Committee on Nuclear Waste. I am Dave Moeller, Chairman of the Committee. The other Committee members present are Martin Steindler and William Hinze. Dr. Clifford Smith will not be present at this meeting.

The consultants in attendance are Melvin Carter, Paul Pomeroy, and Eugene Voiland, and we also anticipate that Dr. Okrent will join us on Thursday and Friday.

During today's meeting, the Committee will discuss the following four topics--one, the draft technical position on tectonic models being prepared by the NRC staff; No. 2, the technical position on erosion protection of mill tailings piles; three, the low-level waste performance assessment methodology, and four, waste acceptance criteria for defense in West Valley vitrified waste.

The topics for the other two days of the meeting are posted at the back of the room. This meeting is being conducted in accordance with the provisions of the Federal Advisory Committee Act and the Government and the Sunshine Act.

Dr. Sidney J.S. Parry is the designated federal official for the initial portion of this meeting.

The rules for participation in the meeting have been announced as part of the notice that was published in the Federal Register.

We have received no written statements or requests to make oral statements from members of the public regarding today's session.

A transcript of portions of today's meeting will be kept, and it is requested that each speaker use one of microphones, identify himself or herself, and speak with sufficient clarity and volume so that he or she can be readily heard.

We want to take special note of the fact that this is the last meeting where Dr. Parry will assist us as a senior staff member. We thank him for his help and hope that his new position with the Nuclear Waste Technical Review Board will be challenging and enjoyable, and that his presence there will enhance the cooperation between this Committee and the Board.

At this time, it is also my pleasure to welcome Ms. Charlotte Abrams, who is joining us as a staff scientist. Charlotte, would you wave your hand so that everyone will know who you are?

Other items of interest include the expectation that Dr. Forrest Remick of the ACRS will be confirmed as a commissioner in the near term.

Additionally, there have been several newspaper

reports that DOE is seriously considering deferral of the sinking of the exploratory shaft for up to two years. This will permit a more detailed set of surface-based geophysical studies to be performed at the Yucca Mountain site, proposed Yucca Mountain site.

The proposed revision of the waste confidence decision has been issued by the Commission for public comment, and I also note that the staff is presently developing a Commission paper that will identify alternative approaches for evaluating the ability of the NRC to determine compliance with the EPA standard.

We understand that a copy of that paper will be provided to the Committee as soon as it is available.

With those opening remarks, let me ask if either Dr. Steindler or Dr. Hinze have comments or additional, any questions at this point.

Do any of the consultants have comments? Well then, we will move ahead with the first item on the agenda, which is the technical position on tectonic models, the draft technical position, and Bill Hinze will open our discussion, and beginning with a review of the working group meeting that we had yesterday on this subject.

DR. HINZE: Thank you, Dr. Moeller. I am going to report first on the meetings that we had yesterday afternoon in the working group, which Dr. Pomeroy was at and which Dr.

Moeller also participated in.

The first topic on the working group related to the volcanism issue at Yucca Mountain. As we are all aware, volcanism remains a troublesome, provocative topic related to Yucca Mountain, and if there is a fatal flaw at Yucca Mountain, certainly an excellent potential candidate is the volcanism problem.

This has received increasing consideration and awareness even by the public in view of the internal document that Dr. Trapp prepared for the NRC.

To gain further insight into the volcanism problem for the ACNW, we invited Dr. Bruce Marsh, who is professor of geological and planetary sciences and head of the department at Johns Hopkins University. Dr. Marsh I think brings to us an insight of having spent nearly 20 years working in a more systems approach to the volcanism problem than perhaps as is carried out by many of the other investigators.

He has worked both in the physical and the chemical side of the volcanology.

We--many of you heard his statements yesterday, and I think you were, I hope you were as pleased with them as I was, and I invite you to participate in anything that I leave out in this brief review of his remarks.

Basically what he did was review the state of the art in terms of magmagenesis, and the processes of volcanism.

He pointed out the sources of variability in volcanic rocks, and in volcanos, both there is the volcano themselves and their products, and in turn, how the variability can be used to study the source rocks, the magma origin, and the stage of volcanism, which is, of course, very important in the whole process of the volcanic prediction and the probabalistic aspects of volcanic occurrences.

One of the things that he pointed out that I thought was very interesting was that there is a tendency to over-emphasize the more recent aspects of volcanism that the process of erosion, and the process of burying, lead to a focusing on the more recent of volcanic products, and that in order to do a proper investigation of the volcanic process, one needs the entire stage, all of the stages found in the volcanism.

He also discussed in limited detail the cause of the volcanism in the basin, essentially backyard type of volcanism that is associated with the upper mantle material flowing and behind a plate which is diving underneath a continent. He didn't go into great detail in this, but he did point out several things as a result of his own experience, experiences, and as a result of reviewing the documents that Dr. Parry and others provided to him.

First of all, I think it is gratifying, must be gratifying to DOE to hear that he felt--and to NRC, that he

felt that the quality of the work done to date was of the nature that was very good work. He didn't leave it at that, though. I think his term was there are a lot of boxes left in the open, and specifically he felt that there has been a great deal of progress in the whole, in the whole understanding the state of knowledge of magmagenesis, and the volcanism over the past decade, and that there needed to be a greater integration and if you will a systems approach to the entire problem of volcanogenesis.

One of the things that I think Dr. Trapp should be pleased about is the fact that he said that he thought that Dr. Trapp was on the right track in terms of the process that was being commented upon, the conclusions, or the, the preliminary conclusions that Dr. Trapp reached, but he pointed out that the, that the integration of various types of data that John was using was absolutely the way to go, and I think one of things that I felt was very important that he said was that with the proper systems approach, that there was a strong likelihood that one could narrow the error parts on the probabalistic aspects of volcanos because I think one of the problems that the Committee has felt is whether we really have a chance in the next five years, the next decade, the next hundred years, of really making some progress on this, and it was great to hear an expert in volcanogenesis speak positively about this.

He also went on somewhat I guess at my prodding in a letter that I wrote to him to suggest how one might go about this, and he suggested that a small working group be in place for six months to a year to try to put together if you will a white paper on where we are in terms of our knowledge of the entire realm of problems associated with volcanogenesis in the basin range and to lay out some plans for filling in the holes, if you will opening some of those boxes and make certain that the boxes are full.

As Dr. Justus of the NRC aptly pointed out, that this was not a role of the NRC, but was part of the site characterization process that was at DOE's backyard, and that's certainly true, but I think the NRC and we in the ACNW should work toward seeing this type of effort and certainly be very certain that a systems approach, total systems approach to the volcanogenesis problem be affected if not in a small working group, that it be carried out in the study plans that are being developed, and I understand there are two of them that are in the process of study plans of DOE, incorporate those.

Paul, Dr. Moeller, you were there. I think perhaps I've said too much.

CHAIRMAN MOELLER: No. I think that has been very good. Paul?

DR. POMEROY: I think that's fine.

DR. STEINDLER: The implication that there is, from the fact that there is a need for a working group is that there isn't such a coherent summary available at this point and that the planning that DOE is currently doing is not in concert with what might come from a white paper of that kind.

Is that a correct inference?

DR. HINZE: Yes, sir. That's my inference from it. In fact, I would carry it a little further.

DR. STEINDLER: All right. Someone assumes that DOE is listening. They may well take up the notion.

DR. HINZE: Well, Mr. Kimball, branch chief of geoscience in DOE, was at the meeting, and we asked him for comments, and he said that DOE was considering these things in the study, but he felt it was very useful discourse.

And incidentally, I do recommend--the meeting was recorded, and I do recommend that you at least look over the comments, and Dr. Parry has a set of the overheads that were used, and I really recommend those of you that weren't at that meeting.

DR. PARRY: It is our intent to prepare summary minutes as are normally done.

DR. HINZE: Great!

DR. STEINDLER: Jesse knows we have the transcript. Perhaps there should be a copy of the transcript.

DR. PARRY: Besides the minutes?

DR. STEINDLER: Yes; in addition to.

DR. CARTER: Phil, I wonder if you can comment on three aspects of this. I have looked at the draft technical position on the tectonics and I have read the comments from the State of Nevada, USGS, and from DOE, and I guess there are three parts of it that trouble me a little bit.

One is a very rudimentary thing, but it would appear that this lack of consensus on definitions, and I mean fundamental definitions--what is a tectonic model, this sort of thing, so this is, it looks like to me it is quite basic. Amongst the various agencies involved in this, there is some real fundamental differences and rather simple things perhaps are very complex things. That's one of them.

DR. HINZE: One of them that really bothers me is the fact that there seems to be differences of opinion about predicted model. That's a real critical one, and I frankly thought the staff did an admirable job on that, yet there seems to be difficulty with that.

DR. CARTER: Well, that would appear to me to be a stumbling block, an understanding of whatever the problem. The other two probably are related, but one is the degree of conservatism, lack of conservatism. If you don't know something, obviously you try to build in conservatism.

DR. HINZE: Well, I've read DOE's comments about the conservatism. I, frankly I did not see that as a red flag in

the, in the technical position on tectonic models. At this stage, I would rather see in the site characterization of that that there be a higher degree of conservatism than to lean in the other direction, and I personally did not feel that there, that DOE's comments were warranted, nor did I feel as a result of the discussion that was held with, between DOE and NRC staffs on September 26th, a meeting which Paul and I attended, I don't think that point was made very well by DOE.

DR. CARTER: The other is again a generic kind of problem, as was brought out bring the ACNW, and that's the--and I'm sure we are going to run into it in a lot of aspects. It is appropriate--is the handling of uncertainty. This may take you a while to respond to.

DR. HINZE: Well, Mel and Dade, if I may, let me answer that question by making some general comments about the entire technical position.

The uncertainties get into the problem of, it gets into the problem of deterministic and proabalistic, and we do, we did arrive at some conclusions regarding the APES and UPES, and we will be very happy to go through those if I might.

It probably would be worthwhile to consider what the ACNW might do as alternatives to the draft TP that we have in front of us.

DR. CARTER: I think it is a matter of uncertainty and the limitations is going to be a continuing problem.

DR. HINZE: That's right, and of course, right now, we are at a stage where we do have information from the older studies, but site characterization has not started yet. Site characterization should be devised and the SCP and the SEA has commented upon improving that as to how we can cut down on those uncertainties.

We also have to reach I believe a proper mix of a more deterministic based upon observed facts and considerations and physical processes, and then we must develop the historical records so that we can put this, and models, so that we can put this into a proper context of, of probabilities and so we have to have--and I think that's one of the things that is brought out but not brought out very well I don't believe, in the TP of developing a proper mix between deterministic and probabalistic problems.

DR. CARTER: The State of Nevada commented on that extensively.

DR. HINZE: That's right, and this is much the same problem that has faced the country in terms of siting of nuclear power plants, the whole problem in trying to find a proper balance between deterministic and probabalistic techniques, and I think that, for example, the EPRI study did a, quite a good job in blending those together.

Paul, you were involved in that as well and in setting some of the technology for that.

DR. POMEROY: I certainly think that, that perhaps even that kind of a study would be extremely useful in several areas that we are going to get into.

As you point out, the uncertainty is going to be there and it is going to continue to be there for a long period of time. It is not clear at all, and I think you are saying that in the technical position, just how the mix of the deterministic and probabalistic ideas is going to occur, and I believe in the technical interchange meeting on the 26th of September, there was a great deal of discussion and DOE representatives stated reading between the lines, we think this is what you are saying basically, and representatives of the staff said please don't read between the lines. There is nothing there between the lines, so there is a, a clear area that needs to be resolved here because there isn't I don't believe--perhaps there is more of an understanding now between DOE and the staff as to what they meant by that, but clearly it is not expresred definitively in the, in the TP at this point.

DR. CARTER: Certainly my reading of the technical position and the comments on it is that there is some serious problems. That's the bottom line.

DR. HINZE: I think, I think that's right, and I think that in our role as the ACNW, we should make some recommendation pursuant to that, yes, sir.

If I can then, I would like to go on with a brief discussion regarding the technical position on tectonic models, and then I think that one of the things that we can do is ask the staff to remark about some of the comments that have come to them on the technical position.

First of all, one of the things that we all felt that in attending the meetings and discussing this further is that many things have, positive things have come out from the staff preparing this technical position, probably not the least of which is that it has helped to, the NRC staff to formulate its positions, and to get its definitions down, but it has also developed a communication dialogue between the various parties which I think has been nothing but helpful.

Let me say that there are several alternatives that face the NRC in dealing with this technical position, and let me go through a few of these alternatives.

First of all we can simply accept the draft TP as it is. We can suggest changes in addition to those that DOE and the State of Nevada and thus consider it after substantive changes.

There is also the possibility that the technical position could be downgraded to a guidance type of letter, or we could go to the point of suggesting that the TP and its materials not be released at all, it served its function, it has developed a dialogue, and then let's stop it at that

point.

There is more. One of the spinoffs from that might be in the more generic sense that some of the problems that you looked to, Mel, really come from the problems in 10 CFR 60, and by the things that we could recommend or continue to urge, and I understand that the NRC is doing this as a revision of 10 CFR 60 along the lines that would take out some of the ambiguities, especially now that we have much more experience with high-level waste.

Along that same line, we have seen a preliminary draft of the TP on seismic hazards which is based--the bottom line to it in brief is that it just says that we should use Appendix A of Part 100.

These are old, and to some people's thinking, outdated rules, and they certainly, one of the things that the Committee could do is suggest that we wait until those rulemakings are modified and brought up to date before more TPs become or are necessary.

Those are, those are, there are relevant sections of those that obviously relate to the technical positions.

DR. STEINDLER: May I ask a question? Do those problems that Mel and others have surfaced really come from 10 CFR 60, or could we attribute those to the state of tectonics as a science?

If you get the proverbial hundred tectonic experts

in the room together, would you in fact end up with a technical position and its confusion, or would you get reasonable agreement?

DR. HINZE: I think that you will always have--obviously you will always have disagreement, but also I think that--probability is pretty nigh, but the point is that I think you can come quite close to having some consensus on definitions. There may, but nonetheless, I think that we can come to some consensus, and I think that, I do think that some of the problems are, do go back to these rules, but certainly there is some problem associated with just differences.

Anyhow, those are the alternatives that are available to us as a Committee. I would like to briefly go through the major concerns very quickly that the small group of us focused in on.

I would like to say that in my view, there are no substantive changes that we see in--substantive in terms of there is nothing wrong with the document as such, but that I feel that it needs, and I think I'm speaking more for myself here, that it needs a great deal of clarification, and Mel has already brought up the definition problem. It also needs additional discussion, expansion if you will, and there are a few of these that I just wanted to comment on.

First of all is that there needs to be a better justification for the document as a TP, as a technical

position. There is very little said about this in the, in the technical position itself. When Keith McConnell made a presentation to us at the last meeting, he discussed why it was needed as a technical position. I think that Keith would agree that that could be expanded upon, and perhaps improved upon, and I would prefer not to come to grips with our decision upon the technical position until that statement is prepared in a more complete manner. That's my feeling about it.

We also certainly agree with Mel and with many others that, and with the NRC staff itself, that there needs to be a tightening of the definitions of all the terms, and even this term which keeps coming back to haunt us, the deterministic is a, is a tough term to apparently get agreement on, and yet it is used and used in a, in a very substantive way in the document, and we have to know I think what is really meant by the staff in those terms.

Thirdly, as I mentioned a few moments ago, that the relative role of deterministic and probabalistic methods of assessing events as they relate to tectonic models needs to be clarified. There needs to be a clarification of the relative roles of deterministic and probabalistic. Both of those are used, and there is a discussion in three on the use of probabilities and tectonic models. This really doesn't--I think there needs to be clarification of that.

Fourth, there is this old problem of the anticipated and unanticipated events that keeps reoccurring, and it occurs, it occurred in the discussion between DOE and NRC and also in the comments of DOE in their recent letter stating their problems with the technical position.

The working group felt that there would be advantages to waiting until the rulemaking is completed on the anticipated and unanticipated events, which would then--the technical position would not have to refer to a document really that is not even in existence but only in the process of being prepared. It would also help to clarify this, this whole problem of uncertainties, Mel, that you brought up, and help to define that better.

The working group saw no compelling reason for getting this document out in this, the near term, that there seemed, that at least from what we had heard, there was no reason for gettig it out in the very near future.

And along that lines, we understand from our discussion yesterday that this will be--at the present time, the NRC's position is that this will be our last time to comment on the TP. If there is some delay of a temporary nature in this, I think that Dr. Moeller would be willing to make available the Committee in a specialized way if necessary, to review this document as it is revised by the staff.

Fifth, one of the very first statements in the technical position is there is a need for the, for models performance allocation and performance assessment, but the connect between those has never been made, and frankly I think this is, I think this is perhaps if you will, in a substantive way is the probably the most difficult or the most compelling concern that I have is if this is really why the technical position is being prepared, then we need to have a better connection between them.

DR. POMEROY: I would second that, by the way, and I would hope that in the staff's remarks this morning, that perhaps they might address that issue for us.

DR. HINZE: As Dr. Pomeroy keeps telling me, the technical position is less than a page long, and actually the material on page 6 is the real technical position, so it is less than a page long. It is, it is in kind words, terse, and we don't want, we don't want to be inundated with words. I appreciate terseness in having to read graduate student theses day in and day out. I love to see terseness, but I think we have gone overboard here, and we do need an expansion an elaboration, so that there is really complete or probably a high degree of clarity regarding what is the position.

Now there are a number of other minor things, but those six items I think cover, Mel, the comments that you brought up, and I think that they cover our comments.

We are not in agreement with all of the comments of the DOE and the State of Nevada. I think that we, in our view, we have hit the high points.

DR. CARTER: Well, in my reading, it just looked like there was some fundamental problems or issues that we need to decide. I would suggest the other thing it would appear to me that there are so many things that probably need fixing--some don't, some do, it would be very appropriate for another review period when this thing is simply revised. I don't know if you said that or not.

DR. HINZE: I said that and it, it is very clear to us in the working group that this Committee should make itself available in whatever way. If we cannot delay the system, we should make ourselves available for another review, and I think that we through Dr. Moeller should do whatever we can to put us into that position.

DR. CARTER: I would like not only to reiterate that but support it wholeheartedly.

CHAIRMAN MOELLER: And I think, too, in our consideration, we need to know or it would be helpful for us to know who within the Commission is setting the schedule for this. I don't think that's the staff. I think they are apparently being told to speak about it.

We did have one other item, Bill, that I might mention, that came up, and this was just a thought, and I'm

not sure we have explored it with the staff, but the technical, draft technical position includes a statement that the criteria by way of a tectonic model will be reviewed and evaluated, will be presented in a review plan, and we wondered yesterday if it might be wise to put in a separate technical position or whatever is appropriate, the criteria through which models of any type will be evaluated, verified, validated and so forth.

I don't really know the pros and cons of that being in a review plan versus being in a technical position or somewhere else.

DR. STEINDLER: Let me comment on that. I'm not sure that the criteria for review, the technical review plan, the criteria for the review might well be on the technical position. The process for review is what I would expect to find.

Let me ask a question of Bill.

DR. HINZE: Could I comment on that point before--if I may, please?

I hear you. I hear what you are saying, but I am concerned that at least models are going to evolve, have to evolve, as the site characterization program goes on and the study, and I hesitate to get too definitive. I hesitate to see the staff get too definitive about the criteria because we don't know where, we don't know exactly where we are headed

with this. I don't want, I would not like to see these be too specific. I would if you are talking in a generic sense or in a generic way, yes, but I, I am concerned that the criteria become too constraining.

DR. STEINDLER: In the course of your discussions, did you have the opportunity to look at the draft that we got on this proposed rule, changes to 10 CFR 60? They do include some quasi-definitions of things like deterministic and so on and so forth.

In fact, as you were going through the concerns that you have on the technical position on tectonics, I kept referring back mentally to the document that we are going to take up later today I think, which at least addresses part of that, which in turn that leads me to ask isn't it perhaps wise to--one seems to reference--this is the one on anticipated. The nominal clarification I'm not sure is clarification, but--

DR. HINZE: What do you mean combined?

DR. STEINDLER: Combine those into a technical position on anticipated, unanticipated events.

DR. HINZE: Well--

DR. STEINDLER: To get models that define what these are and things to which they--

DR. HINZE: My view, there is no better way to do it tha in terms of a rulemaking because that has--

DR. STEINDLER: That's right.

DR. HINZE: And if we can do it that way in a timely enough fashion so that we don't inhibit proper scientific study of the site, then that's the way we ought to do it. The rulemaking as I understand, can go faster and even though it doesn't provide the legal bite, it does have guidance.

CHAIRMAN MOELLER: Dr. Parry?

DR. PARRY: I was going to mention to Dr. Steindler it was proposed that a thorough assessment of the Part 60 in general, obviously somewhat along the lines of what position might be taken by the staff and that a general revision of Part 60 be considered, so if you will rather than patch things up on a mandated basis go through and try and reassess the questions that have arisen during the past seven years and do that on time and possibly then the Committee would be able to get--

CHAIRMAN MOELLER: Well, I heard--yes. Go ahead.

DR. POMEROY: Could I just follow up on that comment? I think your approach is very appropriate because these questions are going to arise again and again and again the next several years as we review other technical positions and other papers associated with this.

You really have to be, they have to be addressed more broadly than simply in this document, whatever it finally comes out to be, and in my opinion, it would be more appropriate to resolve those issues in a generic sense that we

are going to face again and again rather than do them this time and do them the next time and again.

CHAIRMAN MOELLER: During the discussion yesterday, Marty, the question came up, and I sensed in some ways that you were addressing this question, it came up who is it that coordinates the integration of the, of the rulemaking on anticipated and unanticipated processes and events, the revision of 10 CFR 20 and all of the technical positions because we counted up--don't hold me to the number, but I think there are 23 technical positions either that have been completed or under development pertaining to high-level waste, and there are seven that they, well, three have just been, have already been issued this year and there are four more, you know, coming along, so there is seven really in process right at the moment, and the coordination of all of that would seem to me to be a monumental effort.

Now the staff assured us that they do have meetings and that it is all well coordinated.

DR. HINZE: An excellent reason for asking that question is the fact that we have received the technical position on seismic hazards, which as I mentioned, basically just refers back to Appendix A, but we also understand that there is in the mill a currying that we should get in about a year or so another one on seismic hazards dealing with the probabalistic aspects, and perhaps Paul would like to comment

on that, but to me, that, that looks like coordination is not perhaps what it could be or should be, because we get one TP and then turn around and approach it from another aspect. Those, that's another reason we really need to get deterministic and probabalistic brought together.

DR. POMEROY: Just to follow that up, I would like to strongly suggest that when the seismic, the deterministic seismic hazard technical position paper comes up for discussion before this group--I believe it is scheduled for January--that the staff be asked to indicate what other methodologies are coming into place, and in fact I would recommend that we listen to one or two speakers, outside speakers, discuss the issue of deterministic and the use of deterministic and probabalistic methods in the seismic hazard question for repository. .

CHAIRMAN MOELLER: In the discussion yesterday, the staff indicated to me, the way I heard them--we all heard them, but the way I heard them, that perhaps the questions of probabalistic versus deterministic approaches had been clarified. We might call upon the staff--is this a good time to call upon them, Phil Justus, to offer any comments that he believes are, would be appropriate at this time? Phil?

DR. JUSTUS: Thank you, Dr. Moeller. I have been making a list of points that I would like to comment on. I may not get to all of them. I guess we will be working

backwards from the last statement to the first.

With regard to the logic in which we are developing our technical positions on seismic investigations, that is to say the guidance we wish to provide to DOE on their seismic hazard investigations, the--while the timing of the technical positions may indeed not seem appropriate, the, there is a logic behind the development of the seismic hazard investigations position that you heard about from Lyle Blackford and the forthcoming probabalistic seismic hazard analysis technical position that you will be hearing about from Dr. Habil Blacker, and because the seismic hazard TP is scheduled for review in January, I think that would be a fine time for us to get into the details of the logic of the development of these two TPs that are compatible with each other, and form a set of guidance for DOE to consider in its evaluations of the seismic hazard pre-closure, post-closure, and for various uses in its assessment of the site such as for establishing design bases and for use in developing, structuring within performance assessments.

I would like to make one correction on the perception of the nature and scope of the seismic hazard investigations TP. I believe the statement was made that the bottom line or to the effect that the bottom line of that position is that DOE should use Appendix A to develop its seismic hazard.

That is not a correct perception that we would like to make. We have called upon Appendix A methods of investigation of the seismic hazard as being acceptable methods of investigation of the hazard at a repository. If DOE chooses to use other methods of investigation or if the methods of investigation enumerated in Appendix A are modified by some subsequent changes to Appendix A, DOE can likewise adopt in its judgment those further modifications. There is a--the methodology that we have deemed acceptable methodology is an existing methodology, and our guidance to DOE was that it may be used as they wish.

With regard to the use of the deterministic and probabalistic methods, we, we agree that the way in which we portrayed the, the need to conduct deterministic and probabalistic investigations with regard to development and use of tectonic models needs to be further clarified and enhanced.

This brought up the discussion of definitions. It is extremely difficult to unilaterally define controversial terms, but we have done so, and we will continue to do so, and hopefully in the interest of clarifying those terms or at least the intent, a good example is the definition of anticipated process and events and unanticipated processes and events, which our division is in the process of clarifying.

In fact, we are looking to make that clarification

through the process of rulemaking as you are all aware. We do not feel, however, that we need to await the completion of that rulemaking to develop guidance to DOE with regard to development and use of tectonic models or seismic hazard investigations, and we will hope to demonstrate that our approach is viable, reasonable, and workable with regard to facilitating DOE's progress in the program.

DR. HINZE: Could I interrupt to ask you a question about that, Dr. Justus?

Do you think that in the, what you anticipate coming out, and that is hard to do I understand, but anticipated rulemaking, you see that that will have any major impact upon the technical position and tectonic problem?

DR. JUSTUS: I don't, because we will be revising the tectonic models position in the, in the manner that you've suggested and others have suggested with the full knowledge of the definitions and intentions of the, of the requirements for DOE to characterize processes and events as anticipated or unanticipated processes and events, and the rulemaking on the matter of and anticipated, unanticipated processes and events, addresses, and I should say simply addresses the matter of classification of processes and events into those that are anticipated processes and events and those that are unanticipated processes and events.

It does not get into the matters of how they are to

be used in design or performance assessments. That's another matter, and because of the, let's say the narrow scope of our proposed rulemaking along those processes and events, we feel that the intent that we wish to convey and clarify by that rulemaking, we can pursue in the tectonic models position even prior to, to the final rulemaking being published.

Therefore, I don't believe, and I think we don't believe, that we need to wait a year or so for the completion, qualification of the proposed rulemaking.

I might add on a--we are aware that the rulemaking is a proposal, and can be changed drastically. If that's the case, and if we had assumed certain definitions or intentions in the tectonic model position, it is a simple matter to modify the tectonic position. The benefits of proceeding with it far outweigh those applicabilities that will come from delaying, delaying giving this guidance.

We are fully aware of DOE's and the State of Nevada and others' criticism of points in the tectonic models position, and those criticisms serve to point out to us two things at least.

One, we do need to clarify what we meant, but the way in which what we said was perceived indicates to us that there is a need for us to proceed with that clarification and not to delay or abandon such clarifications.

That goes back to perhaps one of the earlier

comments, points that were raised. With regard to--

DR. STEINDLER: Excuse me. Help me out here. Are you saying you have written a document that isn't very clear and somebody has pointed out to you that it isn't very clear and therefore you conclude that this isn't very clear and should be changed? Is that what you just said? I didn't quite follow that much of what you were trying to say.

DR. JUSTUS: I see the logic of your feedback, and perhaps that logic is quite clear. I think if I didn't go a step further, I should have, and that is to make the point that the, our intent, the need for NRC to clarify points it wished to make in the TP has been revitalized by the comments. We are not impressed with the arguments made by the commenters that points that they found unclear culminated in their recommendation that the technical position and the points that we were trying to make are not needed at all.

I would hope that Keith can develop some examples of the need for us to pursue this technical position based on some of the comments that we have received so far shortly.

There was some comment by the Committee regarding the criteria of or need for development of review criteria or develop methodologies and so forth, and whether the format for NRC establishing criteria is best made through a technical position or review plan.

Technical positions and review plans serve different

purposes. Both can contain acceptance criteria or statements by NRC as to what are acceptable methodologies. I would like to mention or reiterate NRC's distinction.

A technical position may include statements of acceptable criteria or attributes of what a good method, model or code may be. That's fair ground we think for technical positions, for NRC to lay out such criteria of acceptability, and the technical position will have public scrutiny on those points. Presumably consensus will be developed.

The review plan or review plans that NRC develops consider the criteria of acceptability and use those criteria to reformat those criteria into checklists by which the NRC staff can review DOE submittals bearing on those methods, models or codes, and in a review plan, the acceptance criteria are indeed reiterated but hey reformatted into essentially a checklist. Here is an acceptance criteria and here is how we check to see that DOE has abided by that redefined or republished criteria, so I hope this brief discussion of distinction may clarify some of the confusion on this document, kinds of documents that we use to give guidance to DOE as well as to help NRC staff.

CHAIRMAN MOELLER: On the basis of what you have just said, it would be clear to me that the criteria for the evaluation, verification, validation, of models should be in a technical position, not in a review plan.

DR. JUSTUS: It would be appropriate for the criteria to appear in technical positions and to be reiterated in review plans.

DR. STEINDLER: In a different format.

CHAIRMAN MOELLER: Right, and yet in this proposed technical, in the draft technical position, it says this, quote, this technical position does not address the criteria by which a tectonic model will be reviewed and evaluate procedures that are more appropriately contained in a review plan.

DR. STEINDLER: We assume that you will fix that.

DR. JUSTUS: Yes. We see that that is a point of confusion. Let me further clarify. This technical position, and there are others like it, do not propose acceptance criteria, so the next point I would like it make is that there are different kinds of things, technical positions have a variety of subjects or approaches to providing guidance to DOE. Initially I just mentioned that one type was the technical position that provides and develops acceptance criteria.

DR. STEINDLER: Are you shifting from acceptance criteria, from--to acceptance criteria from evaluation?

DR. JUSTUS: No. I'm trying to point out that there are different kinds of technical positions, different purposes, purposes other than to, than to present acceptance

criteria or acceptable methods to DOE.

The tectonic models technical position is one of those other kinds of technical positions. This technical position does not set out or try to establish what are acceptable criteria for the development of tectonic models. Some of our critics have assumed that by the title of the position, that this tectonic models position would have included such criteria. That pointed out to us the need, of course, for us to--and we agree with your concern. We need to be more explicit in laying out the reason that we are developing this particular position and what its scope is.

Let me if I may make this point again in a somewhat different way. This position does not give guidance to DOE on how it should, it should or could develop models such as tectonic models. This scope of this position and which we will make clearer in writing in the document itself, is to provide guidance to DOE on what we feel it should do whenever it uses tectonic models. There is quite a significant difference in scope, and we recognize that that was not sufficiently clear to our audiences.

CHAIRMAN MOELLER: Gene, did you have a comment?

MR. VOILAND: No. I'm just thinking that we received other pieces of paper. We received other documents here, technical position on the design of erosion, and this is full of absolutely intimate detail about how to do that, which

is far at the other end of the spectrum that he described.

CHAIRMAN MOELLER: You are absolutely correct. Mel?

DR. CARTER: You know, one comment, and then a suggestion or a question at least--I guess the comments that perturbed me the most probably were the ones from the USGS. They specifically cited a number of cases, very particular where there is differences of opinion in definitions and so forth, but it appears to me they are saying something a little broader than that.

They presumably are the federal agency with a large body of professional expertise in tectonics and related matters, and it looks like to me they are saying, you know, people are coming into our business and trying to change our definitions and this sort of thing. That looks like to me an implication that you could read into that.

The other thing that bothers me, they mention that they had commented earlier on the unanticipated and anticipated events and processes, and it had been some years since they had sent those in and they never had gotten any response, so this put them in some degree of difficulty in commenting at the present time. That's the comment.

The question I have is one of the things that would it appear to me is causing a considerable part of the problem is whether or not the NRC, that it wants the use of 10 CFR 100, Appendix A, and Dr. Justus has said they do not want

that, so the question is why not extract the methodology out of there if you would like to use it, isolate it essentially, and use that as guidance for the DOE as far as the repository is concerned?

DR. JUSTUS: That's a fair comment, and I would like to address that with a specific example, and we greatly respect the expertise of the U.S. Geological Survey, and we don't call that into question by planning terms or seeking to define terms that have regulatory significance that are technical differently from the way the geological community define terms. This is a necessity that is born out by the need to implement regulations.

For example, there are regulatory technical terms devised to implement our regularly requirements. I refer to a term in Appendix A called capable fault. Geologists never heard of a capable fault until they read Appendix A, Part 100. They know what faults are, but the NRC, in capable faults, refer to faults with a certain character that have displacements over various time periods that go back to about 500,000 years that enable an applicant to get a handle on what is a significant fault feature to use in its design basis for a nuclear power plant.

The lifetime of nuclear power plants is about 40 years, and capable faults were devised with that in mind, with a relatively short time period in mind.

In our part of the fuel cycle, our regulations, the target critical facility has a lifetime for regulatory purposes of at least 10,000 years. Capable faults could not be used by our definition because the rate--it is important, in fact there is a requirement for analysis by DOE in Part 60 of the requirements, the rate is the period of time about 2 million years, considerably more than 500,000 year time period for consideration of capable faults in reactors, so we needed to develop a handle for the Department of Energy to grasp when it was characterizing faults for purposes of repository design and performance analysis, so we couldn't use that existing term, and in fact, we didn't invoke any, any other specific terms other than what was laid out in 60.122 and other concepts in 60.21 that there is a fault, for example.

In the SCP, and other documents generated by the Department of Energy with regard to faulting, we found that various terms and concepts were being used such as significant quaternay fault. Significant quaternay fault is a DOE term. I would consider it an analog to a term such as capable fault. It has certain regulatory meaning. Geologists not familiar with the regulations wouldn't know necessarily what was meant, what were, the intention of the phrase significant quaternay fault was unless there was some regulatory term in mind, that he or she had in mind, so my--sorry for this long answer, but the problem with our, with definition in the regulatory arena

is that there are terms of art such as what is a fault or fault, that cannot be directly applied without some caveat because of the timeframes over which faulting mechanism and consequences need to be applied, and so there is this regulatory twist that is applied to technical terms for purposes of implementing requirements.

DR. STEINDLER: I wonder if--

DR. CARTER: We are obviously not going to define these terms today, but the USGS used several specific examples. They didn't use capable fault in one of them. They have used tectonic model, model, process, and bounding values as examples of their concern about definitions, and I would hope that you are not telling me that in every case, we are going to have two sets of nomenclature or units, one in the regulatory sense, and one in the technical community. I hope you are not saying that.

DR. JUSTUS: I hope that that is not the outcome.

DR. CARTER: I wonder if you would comment on my suggestion to isolate the methodology from Appendix A away from the thrust that a lot of people consider related to nuclear power plants which are obviously large energy storehouses, and the application of those to much more passive system like a repository.

Why not take that guidance, modify it as need be, and completely isolate it and separate it from 10 CFR 100,

Appendix A?

DR. JUSTUS: The concern that you raised certainly crosses many boundaries in NRC, and the answers would have to be essentially a joint answer as to why we, consider to be done--there are attributes of this existing regulatory requirement that can, we feel can be and should be--not should be, but necessarily should be, but can be applied to the program.

There were aspects of Appendix A with regard to design that are not necessarily applicable, so it is not a, we have not considered Appendix A in its entirety as something that is all-encompassing. There are existing methods of investigation latitude in Appendix A that are really generic in our view such as the seismic hazard investigation, and we have provided guidance to DOE that suggests that that, those methods still are viable. Other aspects of Appendix A are clearly not transferable to the high-level waste program. We haven't attempted to--

CHAIRMAN MOELLER: What Dr. Carter, though, is saying is you are saying that certain parts could be applied and certain parts are inappropriate for application, and Dr. Carter is saying if that is the case, then why don't you prepare using maybe Part 100 as a beginning point of Appendix A, prepare then a separate statement? It looks like it would be helpful to you, and it would certainly be helpful to DOE.

DR. CARTER: This matter has caused an awful lot of confusion, that people feel that there is comparison, a direct comparison between nuclear reactors and the health and safety aspects of the repository, and I don't think anyone would agree that that's the case, and I think you have essentially said that part of this, namely, the methodology, can be applied to the repository and perhaps you may need some modification, but I don't know why you just don't separate the two rather than laying the onus on DOE or someone else that here is some guidance and there is part of it that you should use and part of it you can ignore.

MR. BALLARD: Ron Ballard--you know, Part 100, Appendix A is a major issue in NRR, and they have expressed for a number of years the desire to, at different times, to revise it, to update it. It is a major--any change, that would have a major effect on the reactor program very likely, and we at NMSS don't feel that, that we wanted to take on the onus of, of revising it as was suggested at one time.

I'm not saying that you as a Committee suggested it, but certainly NRR staff would have liked for us to take a bite at it. We have one licensing action here, and I felt that our limited resources should not be focused on such a major effort, and so our technical position took the, recognizing the limitations that technical positions have, it merely excerpted and it is attempting to clarify.

Our only intent is to use the procedures used to gather data, that these procedures have been through, as you all know, well over a hundred licensing actions. It seems to work. Certainly the analyses that derived from it are major, have revealed major flaws, but we are not, we are not requiring or even indicating that, that the analytical procedures are important. It is merely, and the guide should clearly say, that it is merely the methods that have been established. They are in many licensing cases just a simple approach to going out and gathering geologic and seismic information for purposes of the licensing action.

We are, we have communicated with NRR closely on the preparation of that guide, and it reflects a lot of their, it of course reflects their comments and those of Research, too.

DR. POMEROY: I would like to come back, Phil. I know it is unfair to pick one word out of your statements, but I'll do it anyway. Forgive me.

And that was unilateral. And the, I think that much of the problem with Appendix A and with the technical position we are talking about now is related to that word in some way, and I would like you to just comment a little bit about it.

It is my recollection, for example, that Appendix A was largely devised in consultation with Jim Devine at the USGS in early 1960s I believe, and I wonder why it isn't useful or pertinent in this case to think about a broad-based

review so that questions like this, for example, don't come from the USGS.

With regard to what you mean by process, for example, there is no reason why that should happen if the proper steps have been taken to obtain perhaps not a consensus on these ideas because I agree with your statement that there needs to be a difference between a regulatory term and geological term, but nevertheless, there are a number of people in the USGS that do understand what regulatory term must be, and it would certainly be willing to provide the kind of input that I think some of these terms need.

Other terms, I have more problems with like full range and bounding values and so forth that I think need to be more clearly defined in your terms.

Could you comment, Bill, on the unilateral question?

DR. JUSTUS: Yes, and I think that's certainly fair. I didn't mean by unilateral that NRC would develop a definition such as one that goes into a rulemaking or a technical position entirely on its own.

CHAIRMAN MOELLER: Could you pull your mike over? Thank you.

DR. JUSTUS: What I did was that we are or NRC is responsible for clarifying what these rules and requirements mean, and that in defining or redefining or in asking the definition of terms in its rule, we feel that it is needed in

several cases. Then we, NRC, will proceed to make those clarifications, but I did not--as I said, this, this is done with consultation of DOE and other indirect parties, and an interesting consequence of a statement or a situation which you allude to with regard to the definitions developed in 10 CFR Part 100, Appendix A, in those days of regulation, the U.S. Geological Survey was an advisor to the Nuclear Regulatory Commission on power reactor siting, and in these days, high-level waste repository siting, U.S. Geological Survey is an advisor to the applicant, and we don't share the same relationship as we once did.

Nevertheless, the U.S. Geological Survey's input to definitions while yes, would be funnelled through DOE so that DOE can present a consensus, consensus if they wish, they will be considered, I'm sure.

DR. POMEROY: I would just like to comment then that I recognize that USGS, a portion of the USGS is indeed consultant to the Department of Energy.

Nonetheless, the USGS is, has a broad base of expertise and a very large number of people, and I would strongly encourage you to consider that the question on whether or not it might be possible to have some USGS advisors working closely or more closely with your group in terms of the development of this and upcoming tectonic models.

Obviously there is a potential problem of conflict

of interest. Nonetheless, there is a very broad area of expertise, as Mel has pointed out, that can and should be drawn on in what I consider to be the, probably the most important undertaking that the Nuclear Regulatory Commission is engaged in.

DR. CARTER: I think it should be pointed out, of course, that the USGS commented directly on this technical position, not through the DOE, but directly to the NRC. I presume that they would do similar things in the future.

DR. HINZE: There are also a number of other people alongside of DOE that can speak very intelligently about the problems, the tectonics of the region that we are investigating, and I don't think that it is a necessity that the expertise from outside come from the U.S. Geological Survey.

DR. POMEROY: Certainly not exclusively.

DR. HINZE: Amen!

MR. McCONNELL: My name is Keith McConnell with the NRC staff.

As a point of clarification, we received comments from the project-related USGS geologists through the project and then we received independent USGS comments via letter from the Reston office, so we got both sides of the USGS.

DR. POMEROY: Which one of those do we have? Do we have both of those? We have both? Thank you.

DR. STEINDLER: Let me ask a question. You indicated a sense of determination not to be diverted from the path of issuing and going forward with the technical position on the basis of some comments that said you guys are so unclear and it is so fuzzy that it is not worth having.

Aside from pride of authorship, which I assume is not an issue here, there has to be then some compelling, driving need that you see which would make issue of this technical position in the way of advantage.

Let me ask some questions that relate to that driving need. I can't understand, please, that I can barely spell the word tectonic. I don't really know very much about volcanos.

Is there some fundamental difference that you perceive between the methodology or the approach that you think DOE is using at the moment, it looks like they will continue in the future in arriving at their tectonic descriptions as they relate to both the models and their use, and the same process as done by the staff?

DR. JUSTUS: The answer to that is yes. There is fundamental differences. I would like if you will--

DR. STEINDLER: Don't. Let me pursue my point. It would be of no use to me personally for you to clarify where those are because I'm not sure I would understand.

Let me continue to ask some general questions. Do

you believe--I assume you believe, is it correct for me to assume that, that issue of this technical position will then clarify that difference and presumably demonstrate to DOE the logic of the staff's position so that DOE will shift its approach to be in concert with the NRC's? Is that your belief in the use of this technical position?

DR. JUSTUS: That's our hope.

DR. STEINDLER: That's your hope, okay.

DR. JUSTUS: Because, because we have indicated these fundamental differences to the Department of Energy when we commented on the consultation for outside characterization plan. We have pointed out these fundamental differences and some of our concerns, all of our concerns in our site characterization analysis of the SCP, the SCP did not address our earlier stated concerns, and now through the mechanism of the technical position, we are further stating, and we believe in a more generic way, using some examples from the project files of Yucca Mountain, to make the points again that we hope to make and will gain visibility in DOE because of the significance we feel the position has and at least should have, should have, in guiding DOE's site characterization program.

DR. STEINDLER: Okay. Do you have reasonable assurance by whatever use you want to make of that term, that DOE understands that there is this fundamental difference

between you?

DR. JUSTUS: We feel that based on the past two meetings that we have had with DOE, the past three, the first one on our site characterization analysis concerns, and then a public meeting on tectonic concerns in particular, and lately a technical exchange on tectonic models, that DOE does better understand I have said at these meetings I think a better understanding of our concerns, and I believe, and I think I'm not alone, in believing that DOE recognizes the fundamental difference in approach.

Their comments continue to reflect this difference.

DR. STEINDLER: Let me assume that there is a correct and an incorrect approach since you are adamant, sufficiently adamant to write a technical position in the face of some fairly severe criticism, and let me also assume that you believe your position to be correct; have you tested your notion against the rest of the scientific community?

DR. JUSTUS: Yes, through the methods that are available to our staff in its formal mechanism that we have, the staff has in developing technical positions, and to say a public, including the technical community is alerted to the presence of a draft position, and they are invited to comment on it. Also we can directly mail copies or solicit copies from specific groups and organizations.

DR. STEINDLER: We seem to have a response from at

least a couple of technical folks that Mel has tried to bring up to your attention and has in fact done so, and you have given me the impression that whatever criticisms are to be found in the position, that's--in the commentary that we have from USGS, for example, some of it is useful, some of it is apparently not to you, but you will proceed in the face of that criticism.

Is that an unfair characterization of what you are about to do?

MR. McCONNELL: I don't think so. Keith McConnell again from the NRC staff--we are considering the USGS' comments very seriously, and we plan to address them in a manner I think that will satisfy them when we redefine the various terms that they are talking about.

I think that the problems that developed with the definition, particularly with respect to tectonic models, was the result of either our not being clear enough in how we intend to implement them, or perhaps their not reading as closely as we felt people would when they read the tectonic position, so what we are, we intended to do is both redefine the terms that were brought up and were questioned as well as in the text be more explicit in the, how we intend to implement those concepts or terms.

DR. STEINDLER: Okay. Let me not continue this line. You obviously I think see what I am driving at, but let

me put it a little more crudely than necessary.

When you folks believe you're right, and you are also the regulators and in that sense you not only have to be right but better be correct, and there are people on the outside with staffs and expertise at least as significant as yours who I think are telling me that that may not be the case, and so to march what I would call blindly and head on into the teeth of that could--putting a position out which I guess DOE would have to live with, requires a certain amount of care, and I would hope that that care is being exercised and that care is being at least analyzed by the technical community.

DR. PARRY: Is it the staff's intention to check further with USGS as to whether they have, you have been successful in properly addressing their questions?

DR. JUSTUS: Yes. It is our intention to, to the extent that we can obviously in an open forum for such communications, we do intend to communicate with the USGS. They have verbally as well as in writing communicated with us, and are willing to work closely we hope that on developing those definitions.

Incidentally, we don't feel that anyone has pointed out that we are incorrect; only that we are unclear. The matter of us feeling that we are right, we do continue to believe that our interpretation and the intent in Part 60 that

we wish to convey guidance on, is right. We wish to make it clearer.

DR. STEINDLER: You should feel free to explain that fundamental difference in geologic terms to those people around the table that can understand it. Don't let me dissuade from you that.

DR. CARTER: Well, a couple of things on this--I think some of these were very specific, and I think that Dr. Steindler said I think the storm flags have been raised by a number of people, not only USGS, on that basis, but also by the statement, and I interpreted some of their comments I think there, they were saying that you were doing things that were either incorrect or that were in direct conflict with evidence and USGS may not agree with that, but I think there is a substantial amount of criticism from some reputable organizations, and I think the NRC certainly needs to think very, very closely about that.

CHAIRMAN MOELLER: Let's see--Paul and then Bill.

DR. POMEROY: Phil, going back to the question of needs--I know we commented on this late yesterday afternoon--again I would like to go on with it just for a moment. On the question of need, you did comment on the draft version of the SCP in regard to the matters that are in the technical position paper, you did comment rather extensively on the SCA, on the SCP that was presented on the same matters,

and now we are talking about a technical position paper.

Given that you were unable to reach the consensus with DOE with regard to these issues in either of the two previous comments, what gives you that quote, hope, unquote, that this is going to be the magic document that is going to change that, the difference that Marty so excellently pointed out?

DR. JUSTUS: Because we are tying this document to the requirements of Part 60 explicitly, and in our earlier comments, of comments on specific DOE documents such as the site characterization plan for Yucca Mountain, this problem is too fundamental for us to just let go without documenting in the form of position as a generic problem.

There are approaches that we feel DOE is not taking that are needed to be taken to fulfill the requirements of Part 60, and this position will--is aimed, designed to explicitly bring that home to DOE. Earlier comments, as I said, were on specific points.

DR. POMEROY: And you feel that perhaps we may have to go through the same thing again with other technical positions in the same area?

DR. JUSTUS: Yes. The reason that I say that is that we are delving into areas of guidance now that are the residual points of controversy or points of divergence with the applicant.

After years, several years, of discussion and interactions, we are into the point of developing or resolution to the most controversial or the most difficult issues, and therefore, the, these let's say various means at our disposal of resolving or at least seeing that DOE addresses these points of concern, are utilized as early as possible. Hence the urgency for this position.

CHAIRMAN MOELLER: Bill?

DR. HINZE: I understand that Keith McConnell has done some work in terms of preparing drafts on responses to these comments of the various parties, and I for one would be appreciative of hearing some examples of those because I think this will give us a flavor of where the staff is going with the revision on this document, and might help to--we are not meaning something that they were taking into consideration, so I would really urge that we do that.

CHAIRMAN MOELLER: I agree, and at the working group meeting yesterday, it was pointed out that the staff will prepare I believe the words were a set of response papers in which they will enumerate the criticisms or comments of each of the various parties, and then tell specifically how that particular comment was handled, and it's I think very important that we receive those as they are completed so that we can get a better handle on how you are responding.

DR. PARRY: I think my understanding was that those

comments would not be available until after the revision was available.

CHAIRMAN MOELLER: Correct. I understand that. Go ahead, Keith.

DR. JUSTUS: Before we begin, can we expect that we will have comments from you to be considered in this, by us in this package?

CHAIRMAN MOELLER: We plan to write you a letter or attempt to draft or complete a letter at this meeting commenting on this draft technical position.

DR. JUSTUS: Thank you.

CHAIRMAN MOELLER: Keith?

MR. McCONNELL: Thank you, Dr. Moeller. What I would like to try to do is go through some of the major comment areas that we got, have been more or less coalesced from all of the reviewers, and just give you some broad idea of how we intend to approach addressing those comments.

First is the overall need for the techn[illegible]osition has been talked about quite a bit here today. We think that in--we felt all along there was a need for the technical position, and I think that has been reinforced by the comments that we have gotten from the Department of Energy which does show a divergence in how tectonic models are considered and how they are implemented in site characterization and performance assessment, and I can give you a specific example

and that relates to the term predictive model which is included in Part 60.

The DOE response to the, or comments on the technical position seemed to imply that they consider predictive models as basically numeric models or codes of some sort that will assess performance in a quantitative way.

Well, we have no problem with that, but we expand the term predicted models to include qualitative, a qualitative sense, which is where we would include tectonic models, that tectonic models can be used in a qualitative or conceptual sense in preliminary performance assessments, not necessarily in the final performance assessments directly, but indirectly into the final performance assessments also via scenario development.

So we think that there is a fundamental difference between the DOE and the NRC staff in its use of tectonic models, and we feel that there is a need to clarify we feel the intent of Part 60 with respect to predictive models and the use of tectonic models as the NRC used them.

The second area that's come up today and quite often is the definition of terms. Firstly, I don't believe that we are that far apart from the USGS as far as our definition of terms. I think it, again it is how the terms were implemented or how we suggested they be implemented and what the reviewers perceived as what we were suggesting on how they should be

implemented.

Basically from the comments from the USGS, I get the impression that again, they thought that we were intending that tectonic models be used in a quantitative sense in the assessment of performance, and that's not what we were trying to imply.

And this may lead to another philosophical difference or misunderstanding between the NRC staff and the DOE in that we don't believe that performance assessment is something you do at the very end of the characterization process.

We believe it is something that you can be doing all along in the process, in a preliminary sense and in a conceptual sense to a certain degree, and that's what we were suggesting, that the tectonic models be used as that, in the draft position, that they be used in a preliminary and conceptual sense, not in a numeric or specific code, computer code type of approach.

Other terms like bounding value, full range, and process we agree could be expanded and we could be much more explicit on what we mean there, and we will attempt to do that and attempt to satisfy the comments.

Now the scope, we have been criticized that the scope is both too narrow or too broad. We intend to try to be more, again more explicit on why the scope is what it is, and

we have tried to, to tell the Committee why we have taken the direction we have with respect to scope. We wanted to basically outline the requirements and the implementation of requirements in Part 60 with respect to models, predictive models as it applies to tectonic models, and again we wanted to make it generic and not site specific, which was some of the, I guess many of the comments the state made--why is it more specific to the State of Nevada?

DR. CARTER: Let me interrupt. I believe the State of Nevada used the comments obtuse and philosophical in their criticism.

MR. McCONNELL: They did. It is philosophical to a point in that we are trying to get across the NRC staff's approach to the use of tectonic models which is basically or could be considered a philosophy. Now if it is obtuse, we will try to be more explicit and avoid some of that problem in the next draft.

DR. STEINDLER: Even people with degrees in philosophy would not consider philosophical a pejorative term.

DR. CARTER: There are very few of us present!

DR. POMEROY: Keith, can I--I might interrupt at this point, too, but you believe after the considerable commentary that you made in the SCA to DOE and that this Committee made with regard to the use of tectonic models in developing scenarios, for example, that they are not going to

do that? Is that your belief at this point?

MR. McCONNELL: Well, it is difficult for us to assess that at this stage. I think the experience that we have to draw upon is the period between the consultative draft SCP and the SCP in which many of the same comments were made, and the approach did not appear to change significantly.

DR. POMEROY: I don't know about the SCP, but I certainly know there was strong comments made from all points with regard to that situation. It would seem to me that once again DOE would be flying in the face of a great deal of potential problem that it chose not to consider, certainly in scenario form.

DR. HINZE: I think this is part of the problem that we have got ourselves into in terms of the difference between the SCP and the study plans and the amount of detail that is required in the SCP.

The kind--if I understand correctly, it is the lack of detail, the lack of precision, the lack of integration in the SCP that is of concern, and yet we are told don't panic, it is all going to be in the study plan, and I, I think that's, that's really the box that we are in, and I would hope that what the, what the NRC staff is doing is trying to, is trying to make certain that those things really do get into the study plans giving guidance on how it will be regulated or licensed.

MR. McCONNELL: Shall I go on?

CHAIRMAN MOELLER: Yes.

MR. McCONNELL: Then there was the major area of comment with respect to how the NRC staff processes tectonic models being used in performance allocation, and there were some fairly strong DOE comments on how the NRC staff may be going back on previous agreements that had been made, and basically what was stated in the position was what was stated back in April of 1988 at the alternative conceptual models meeting and where the NRC, where the NRC staff outlined its position on the use of alternative conceptual models in a general sense, so we are being I think fairly consistent in our approach to using models in general and in this case tectonic models in performance allocation.

We think that in developing an understanding of processes at the site, it is fairly important that you consider if you will--excuse me--the full range of alternative conceptual models, tectonic models supported by the existing data, and those cases you would then consider things like the Semansky effort and the John Trapp example as part of your characterization program. Those would be out there prior to the start of characterization, and you would go on and test for those, those models.

Now we don't say that you use tectonic models solely. You use other existing site data.

DR. POMEROY: Excuse me, Keith. Let me just try to clarify this in my own mind a little bit.

Do you feel that with the issuance of this technical position then that there will be some fundamental change in the preliminary performance allocation if your hope is satisfied that DOE takes note of this?

MR. McCONNELL: What we would hope or what we are, our intent is that the DOE consider the position in the construction of its study plans so that the study plans itself would then consider in its testing program all these models that might exist out there that are supported by existing data, but we don't see, and I'm sure the DOE doesn't see, any revision of the site characterization plan, anything of what we are expecting to see in the development of the study plan.

DR. POMEROY: You think DOE has a clear understanding that that is your intent?

MR. McCONNELL: Probably not from their comments.

DR. POMEROY: I would say that's true. I would hope that you would try to convey that because, in whatever this document becomes, because it seems to me to be a significant point, that clarification certainly would help.

MR. McCONNELL: The next area of comment is the role of tectonic models in performance assessment, and again, we don't consider tectonic models to be a mechanism for providing direct numeric codes or numeric models for performance

assessment. What we see them as is conceptual in nature, and qualitative in nature, and therefore, as to be used in preliminary performance assessments, and as input into scenario development, and we are trying to make that clear. I think that's the area that has the most confusion in it with respect to DOE's comments.

DR. STEINDLER: Let me suggest to you that from my very limited understanding of the word models as used throughout the Nuclear Regulatory Commission, all of those tend to be numerically generated large codes, small codes if you are talking about reactor activities, all of which end up with infinitely precise graphs on very fuzzy data, but nonetheless are numerical descriptions of what people perceive to be the upcoming real world.

You folks all of a sudden changed that definition, and while I have nothing personally against you trying to change the culture of the business, you ought to understand the magnitude of the task that you have inadvertently I think stumbled into, and the task that is before you in making that clear.

DR. HINZE: I think the geoscientists have precedence here.

MR. McCONNELL: Another criticism was often that we, on the examples that we provided, I think the DOE thought we were criticizing the SCP, and the state thought we were

over-simplified in some areas, and the DOE also thought that they were all negative, and we will just take those, take those comments into consideration when we revise the TP.

Then there is the role in identifying anticipated and unanticipated processes and events, and again, this gets into what appears to be a misunderstanding in how the NRC staff used the process of identifying and analyzing anticipated processes and events, and how the DOE sees it, and I think we have made quite a bit of progress in clarifying the positions between the two staffs with the interactions that we have had in the recent past.

The deterministic versus probabalistic criteria, we do intend to expand that significantly I think, and we also intend to provide definitions in the revised draft of particularly deterministic approach or deterministic criteria. We don't believe that we are that far from what the state has suggested or has basically quoted out of the American Geological Institute's glossary for the deterministic criteria.

And that's basically the major areas of comment. That's basically it.

DR. STEINDLER: If you covered it, I missed it. It seems to me that both, well, particularly DOE is, if I can condense it, it in effect said you guys are asking us to become deterministic only, and that doesn't jive with the EPA

criteria. And therefore, your, I mean the implication is that NRC is forcing the applicant into an analysis mode which doesn't comply with all the requirements, so why are you doing this?

You surely must address that issue. I was wondering how you are going to handle it.

MR. McCONNELL: That was not our intent with the position. Again, tectonic models can only provide input into scenario development which would then be used in the consideration of the EPA standard, and we recognize that the EPA standard is a probabalistic based standard, so we don't think that there is, there is much of a problem other than the fact that we need to be more explicit how we intend tectonic models to be used.

We--the area of disagreement I think at least in the area of misunderstanding is how you, you develop or identify and consider anticipated processes and events. We consider that in the identifying, it is a two-step process.

First you identified anticipated processes and events, and we suggest that you use primarily a deterministic approach in identifying those anticipated processes and events, and the second step, then you analyze those anticipated events that you identify, determine the likelihood of using probabalistic methods as well as deterministic methods, whatever mix the DOE seems to think is appropriate,

to come up with design bases events. Those are the ones that would go into the consideration of the design.

DR. POMEROY: I would like to see those definitions if you are going to expand that in that expansion in this document as well because there certainly seem to be confusion on the part of the DOE representatives at the technical interchange meeting with regard to whether or not anticipated events were indeed design basis events. I think you did clarify it there, but I think something has to be in the document.

DR. HINZE: I was trying to think of where we stand now in terms of working group, and I guess there are a couple of rather obvious comments that could be made.

One is that I think that what we have learned from Dr. Marsh has been very helpful to the Committee, and that we shouldn't just let it drop at that point, that we consider how these, how his suggestions are going to be implemented into the site characterization program, and we might think about in the future, perhaps bringing that up to the center on a visit down in terms of how they might get involved in that type of integration. I'm trying to find some kind of mechanism by which we can assure all of our, all of us that something is done about it.

The second thing is that we have a letter in draft on our comments regarding the technical position on tectonic

models, but it's apparent from the comment that we just heard from Paul and the many excellent comments that Keith has made here that it is in my view imperative that we do review this document before it hits the street.

DR. CARTER: The other thing, if I heard Keith correctly, earlier in his comments he indicated a new draft technical position would be prepared. Presumably it would be modified and so forth in the current one, and obviously we will consider respond and review comments when we see it. I think I heard that correctly.

DR. HINZE: That's what I heard, too, but I also heard yesterday that this would be our last opportunity to have any comments regarding the technical position. I guess my point is that just as Paul has said, we are very keen to know about some of these definitions that are going to be included, and that it is important I think that we get an opportunity to review that document.

CHAIRMAN MOELLER: Mel, they told us yesterday at the working group meeting that the new technical position will not be simply editing this one, but would be a completely new effort starting almost from scratch.

DR. CARTER: Presumably it will be available for review before it is put to bed. Maybe that's the point we should have addressed. Is it going to be reviewed?

DR. HINZE: I'm sorry I am not making myself clear,

but we were told yesterday that we would not be reviewing that. We would not have the opportunity to review it.

DR. CARTER: Nor anyone else.

DR. HINZE: Nor anyone else, and what I am trying to bring out on to the table is the fact that I believe that we need to do that.

DR. JUSTUS: May I clarify our position at this point?

DR. CARTER: Please do.

DR. JUSTUS: We, we expect that we will be following our current schedule for finalizing the draft position. We expect that to be completed, and available in final form by the end of December.

We also expect that there will be substantial revision, but I don't believe we said complete revision from scratch. We do expect, and I think you can appreciate from the discussion today, substantial revisions at least to some parts will be needed.

The review plan for the production of this final draft is the standard plan for technical position development, and we are on track to continue to follow the standard procedure, which means that we will consider very seriously comments that we have on the table prior to our going final in the next few months.

The current plan, however, does not call for another

review or additional reviews by the ACNW or other commenters.

DR. STEINDLER: In case you missed the point, I think it is our intent to sidetrack that current plan to the best of our ability.

DR. CARTER: I would like to ask the question that's a policy matter. When something like this has raised as many concerns as it obviously has, in the revision of one, that it is customary to not go through another review period? It sounds like to me that is a policy. Is that correct?

DR. JUSTUS: Yes, it is, and my branch chief is here to address that policy.

DR. PARRY: Before Ron speaks, I might remind the Committee that at least on three occasions the Committee reviewed a technical position from the low-level division and finally got out one--

CHAIRMAN MOELLER: Yes, jack's point being that we saw two or three completely different versions before we finally approved or you know, consented at least from our point of view.

Ron?

MR. BALLARD: Just some general comments on the concern you expressed earlier, Dr. Moeller, and that was coordination of TPs. Are they coordinated?

Well, we do have a formal process, and it relates to the standards which I'll mention about reviewing TPs.

As you know, SECY 88-265 is a pretty lengthy document that laid out the entire strategy of the High-Level Waste Division for the next few years, and trying to get this guidance out, as you indicated, there is a large volume of guidance that has been established as needed.

You mentioned some 23 TPs. There are a number of rulemakings which are much broader in scope, and they have all been, they are coordinated in that sense. They are laid out there. It's not obvious always when we, when we send out one draft TP that there is a, an integrated structure, but it is there, and we also have a system within the division of so-called policy documents, waste management policy documents, one of which is waste management Policy No. 46. It lays out the detailed procedures by which we, we are to develop and produce technical positions.

In that procedure, which has been approved at pretty high levels within the Agency, the standard approach is to have, have the ACNW review and advise us at the, prior to the release of the draft TP, and prior to the release of the final TP.

We've tried to adhere to that. I mean we always adhere to that requirement, and as you know, we have a limited staff, and we are on the milestone charts for producing these things, so we do indeed try very hard to retain those schedules.

I believe in the case of this TP, it was earlier mentioned by Dr. Hinze that they didn't see any major errors in this TP.

Now on the basis of that, I would say that clearly there is some need for clarification. That goes without saying based on the comments of both the ACNW and the DOE. We would take those into account, and normally if there is no major, major substantive change in the document, more clarification, no, we would try to adhere to the schedule and and contact you. I think the procedure calls for actually addressing to you how we disposed of ACNW comments, so that process is in place and we do resist primarily because of the vast number of other activities we have going, to resist changing the schedules, and I hate to look bureaucratic, but sometimes we have to.

Now that doesn't, that isn't meant to imply that we are cast in iron on this, and if there is some major comments and certainly from the ACNW specific point, why our management would certainly take into consideration those comments. We have done it in the past. We have revised, we have come back on anticipated processes and events more frequently than the schedule calls for. In fact, you will be hearing an extra one tomorrow, so I hope that helps to clarify our position on schedules anyway.

And one other comment--there was a number of

comments from the Committee on model verification and validation. If you scrutinize SECY 88-285, you will see that we do have on the schedule a technical position on model validation and verification which is in the pipeline. We have spread these out based on availability of staff, and as we are trying to bring on board through the center some additional staff to help us.

CHAIRMAN MOELLER: Thank you.

MR. BALLARD: One more little comment on the review plan--I heard a number of comments. I would like to refer back to the, one that many of you are familiar with--that's NRR standard review plan.

The staff hopes to come up with a similar review plan which would lay out exactly how we intend to proceed on our review, and key reference documents and all. Those plans, as in NRR, we intend also to [illegible]ch some of these technical positions. It's just a clarifying role. Technical position is merely an efficient means of getting out to the public and to the interested parties what we are thinking, our thinking is, and we hope to define it by that process.

That's about all I have.

CHAIRMAN MOELLER: Dr. Steindler has some questions.

DR. STEINDLER: Somebody pointed out that the technical position--if I could address a couple of questions there, and perhaps the question goes to somebody like Bill,

you have covered it, and that is item C calls for a statement that says that a full range of tectonic models supported by existing data should form one of the principal bases and so on and so forth.

I have two questions--one, does staff really understand what a full range is? Not have full, full? And two, this is a full range only supported by existing data? What do you suppose is meant by that?

DR. HINZE: First of all, let me not defend the staff, but my reading of that, if you will recall, at our September meeting, I took great exception--I think Keith will back that up--I took great exception to the term full range and I think it is much too ambiguous and I think I know what Keith means, but I don't think--I think it is important that the community understand what that is, so--and by existing data, I interpret that to mean that this was not just some crackpot idea, but that it was supported by physical processes that are standard in the scientific community and by data, and because we can always, there will always be that one more model that reminds me of the old comment I know of another company and there is always an item I can sell to another company--crackpot, and that was just to eliminate that, but that's my perception of what the NRC staff is saying.

DR. STEINDLER: I have one other question. I raise the issue--I'm not sure it is worth resolving because that I

hope is what will be covered by the efficiency folks.

My other question is you talked about natural analogs. In the sequence of things that can be used, page 2, where you say that analyses and models that will be used to predict future conditions and changes in the geologic setting shall be supported using an appropriate combination of, and you field test, NC 2, natural analogs--the concept of a natural analog is particular--in this particular case I am confused on.

Is that a generally accepted geologic community? Are natural analogs generally accepted basis of individuals in this concept?

DR. HINZE: I believe so, and it is a very important role.

DR. STEINDLER: All right.

CHAIRMAN MOELLER: I wondered on that why artificial analogs would not be considered? By that I would mean something like--

DR. STEINDLER: Laboratory tests.

CHAIRMAN MOELLER: The underground tests at Nevada or something like that.

DR. HINZE: Underground tests I guess--

CHAIRMAN MOELLER: Meaning after the test is over and the radioactive material is there.

DR. HINZE: How the earth reacts to nuclear tests

and that be incorporated as a result of those tests, I think that's part of the analog.

CHAIRMAN MOELLER: Okay.

DR. CARTER: It is a natural analog.

CHAIRMAN MOELLER: Okay. I hear you.

MR. VOILAND: What would be the situation of the pipes and what have you in the San Andreas fault? There is a lot of information in assembling that.

CHAIRMAN MOELLER: Okay. Any other comments? Well, if that--Mel, do you have something?

DR. CARTER: I think what Ron said, I didn't use the word. I think in spite of the policy, there is some flexibility in it.

CHAIRMAN MOELLER: Okay. I think what we will do is consider yesterday's meeting and this morning's session and this afternoon when we go into Executive Session, we will attempt to formulate, you know, sit down in writing our comments.

We have distributed this yellow draft version, but on the basis of what we have heard additionally this morning, I see several, you know, rather significant changes that need to be made in it, and if we have time, we will try to incorporate a few of them before Executive Session this afternoon.

Okay. With that then, let me thank the staff once

again for being here and spending time with us, and we will now have our break.

(A brief recess was taken.)

CHAIRMAN MOELLER: The meeting will resume. The next topic is the discussion--and in fact we will do this until noon--the discussion on the technical position on the quote, design of erosion protection covers for stabilization of uranium mill tailing sites, and you each have a handout on this, and our speaker will be Mike, is that Fliegel?

DR. FLIEGEL: That is correct.

CHAIRMAN MOELLER: With the Division of Low-Level Waste and Decommissioning of NMSS.

DR. HINZE: Could we have a spelling on that?

CHAIRMAN MOELLER: Okay--F-L-I-E-G-E-L, Dr. Michael Fliegel.

(Slide)

DR. FLIEGEL: Are you ready?

CHAIRMAN MOELLER: Yes.

DR. FLIEGEL: I'm Ron Fliegel. I'm the section chief for the Uranium Recovery Section in the Low-Level Operations Branch, and I'm going to be giving a brief overview of the technical position on erosion protection.

We had prepared a presentation last month but unfortunately, we were unable to give it because of the ACNW's schedule.

Our technical lead, the principal author of the technical position, isn't here today. He's inspecting erosion protection in Utah today, and so we really can't answer detailed technical questions. I will try and answer what questions I can, and talk in terms of general philosophy and overview of the position.

Our branch chief also apologizes. He is on his way to an NGA conference in Chicago today.

With that out of the way, our objective here is to obtain general ACNW approval. That is what we are hoping, is that after having read the position, you are going to be able to tell us that you have no major objections to it, and--

DR. STEINDLER: Is that microphone on? Whistle into it. Yes. You might want to try and raise that up a little higher.

DR. FLIEGEL: Our objective is to obtain general ACNW approval, no major issues or objections, and if you have any comments, we certainly will try and address them with the other comments that we receive.

With that as an introduction, let me just briefly go over some of the background legislative and regulatory framework where this position fits in.

As you may recall, we briefed the Committee in January of 1988 on the legislative and regulatory framework of the uranium recovery program, and in addition, just in

addition to what I'm going to say today, I'm going to then get into the erosion protection aspects of the position and then talk a little bit about the position itself.

(Slide)

DR. FLIEGEL: The framework of the position starts with the legislation, UMTRCA, which in addition to dictating a a role for NRC, gives the EPA administrator the responsibility to set the standards for both Title I, which are the sites that were out of operation at the time UMTRCA was passed, and Title II, which were the NRC license sites.

The EPA standards are in 40 CFR Part 192, and these apply directly to the Title I program. For Title II, we incorporated the EPA standards into 10 CFR Part 40, Appendix A, and these standards now conform to the EPA standards and these are what are applicable to Title II.

Now we are only concerned in this technical position with the erosion protection aspects of those standards.

(Slide)

DR. CARTER: Could I ask you a question about coverage?

I presume this technical position will address inactive uranium mill tailings, but what about the active mills when they have a pile that you know they are no longer adding material to--does it also cover those?

DR. FLIEGEL: Yes. The inactive sites are Title I

sites. The active sites are Title II sites. This position covers erosion protection for the remediation and restoration of both types of sites.

The first bullet here is a management position. Management position was finalized in January of this year, and that position is primarily to define and clarify some of the terms that we were dealing with, and I think that the ACNW has copies of that position.

The major aspect of the standards that erosion protection has to deal with is the concept of control, control of residual radioactive material, and in that concept, the most important aspect is longevity. The standards require control for a thousand years to the extent reasonably achievable, and at least 200 years.

Now in addressing control, the term for erosion, we are faced with two types of threats or aspects that we have to deal with.

One aspect is what most people had in mind when they were looking at erosion protection, and that's slow degradation. You have a package that over the course of years, a soil cover will, some of the soil will slowly come off or come off sometimes in events, but it is a continual process and we have to deal with that, and that part of the process is most relevant to soil covers.

Now in addition, there is another aspect, and that's

severe events, and that's concerned with rock covers, and it is simply the way that a rock cover works. When you put rock riprap on a slope, what you will find is that if it sees an event, water flowing by, if the event is below some critical level, nothing will happen. The rock cover will just stay there. It can see uncountable numbers of those events, until it sees an event that is greater than a critical value. At that point, the rock cover will unravel, and it will be--after that, even smaller events can degrade the cover, and so what that does is having to deal with that, we are led into looking at questions of probability, and the thousand year criteria will lead us rapidly into looking at probability, probabilities of severe events, not a low probability event next year, but cumulative probabilities in the course of a thousand years, and that's the reason that the standard looks to probable maximum events.

Now another aspect of the standards that, that the erosion protection standard has to deal with is the concept of no maintenance.

The standards don't really provide for, for maintenance of covers. This leads to great difficulty because most erosion protection is designed and built with the thought in mind that there would be some maintenance, yet the standard doesn't allow for that. The standard contemplates that you design it and build it and then basically take no credit at

all for your ability to go back and fix something if something goes wrong, and that's, that's difficult to deal with.

Finally, there are the EPA ground water standards, and that's here simply because it turns out that the ground water standards require you to do things that work against erosion protection. A lot of what is good for erosion protection is bad for ground water.

In designing a cover to protect against erosion, what you are trying to do is you are trying to keep water from running off rapidly. You want to slow it down, yet in dealing with the ground water standards, what you are trying to do is you are trying to prevent infiltration, and the way you do that is you are trying to get water off the cover as fast as you can.

While the position, while this position doesn't get deeply into the ground water standards, it points out that problem, and we feel, though, that both standards allow some flexibility, that in designing a cover, you cannot just look at erosion protection or ground water. You have to make some compromise as to, to handle both standards.

(Slide)

DR. FLIEGEL: Finally, about the technical position itself, it covers methods for several different types of soil cover designs, and primarily those types are soil covers, they are rock covers, and they are combinations.

The standard also talks about sacrificial slopes, which is a concept, if you are in a situation or a licensee or DOE is in a situation where they can't find good rock, and the soil cover is just impracticable to build, there are methods of putting a site cover that's assumed to degrade over the course of the lifetime of the facility, and if it's designed properly, we can conclude that it will last at least 200 year minimum, and there are methods for designing that.

And finally, we talk in the position about if nothing works, what you could do, and that is you can come back to us for some kind of an exemption, but you have to have a good case to be able to do that.

Talk a little bit about where we are with the technical position--the draft was noticed in the Federal Register last August. There was a fair amount of public interest. We've sent out on request 50 copies. Comments were due back this week. We've only received one comment unless there are a few that are in the process that we haven't seen yet, and that was generally favorable. We haven't received comments yet from DOE, but we know that they are preparing comments, and we have had one request for an extension of two to three weeks, which we granted, and I will be happy to answer any questions you have.

CHAIRMAN MOELLER: I would like to ask a few, and offer some comments. Could we have the lights?

In reading through the technical position, it appeared to me that there were certain consistencies with other waste problems and certain inconsistencies, and let me just discuss them and relay to you what they are as I see them and then obtain clarification if we can.

In terms of the EPA's standards for high-level waste, they say that they want to have the repository, you know--please bear with me, and then I'll get over to uranium mill tailings--but they say they want the, to limit the risk from projected release to the successful environment from the waste placed in the repository to no more than the comparable risk if the uranium ore had not been mined, so they are using in a sense a natural radiation background as their ultimate goal so to speak.

Now here in the case of the uranium mill tailings, they say that the permissible releases of radon 222 shall be no more than an average of 20 picocuries per square meter per second. This is in their 40 CFR 192, and I then looked up what the release of radon is from a typical soil, and it's roughly the same thing, so they are saying you shall cover these tailings so that the radon emission rate is comparable to typical soil, so here we have comparable approaches for high-level waste repository and uranium mill tailings.

Well then, though, if I go on, and I fully realize in one case we are in a sense talking about low-level waste

and in another sense, the repository, we are talking about high-level waste, but then when I look at these in more detail, I find that they are not necessarily the same, and let me see if I can find my notes on this.

Okay. Your stated goal is to provide protection for a minimum of 200 years and hopefully for a thousand years, and yet you are talking about a waste that will be radioactive for tens or hundreds or thousands of years.

If I look at--I realize the uranium has been removed to some extent, so that's out of them, but if I look at radium as the principal component, which is the mother of radon, then it has what, a 1600 year half life or something like that, so I'm talking, you know, thousands of years, not just one thousand years, you know, even in a thousand years, it will still be 80 percent or something or 70 percent of whatever it was at the original time, so I don't understand the selection of a time period here of 200 to a thousand years where for high-level waste, you're talking, you know, 10,000 to a hundred thousand years, so could you help me understand what I'm missing?

DR. FLIEGEL: I'll ask George if he can shed some light on that.

Before he starts, let me make a couple of points. First of all, I'm not sure that anybody at NRC is going to try and defend the EPA standard. We take it--

CHAIRMAN MOELLER: Did EPA have the thousand years?

DR. FLIEGEL: EPA could review residual radioactivity for a thousand years to the extent reasonably achievable or at least 200 years. That's a paraphrase of EPA's words, but--and in addition, NRC, when the EPA standards were promulgated, I think our standard had words that said thousands of years.

MR. GNUGNOLI: Actually the wording was several thousands of years.

CHAIRMAN MOELLER: In the EPA standard?

MR. GNUGNOLI: In our final rulemaking that we published in October of 1980, we had proposed a radon standard of 2 picocuries per meter squared a second, and the control was for several thousands of years.

Subsequently, EPA studies in terms of construction methods and reliance that could be put upon them concluded that it was sort of unrealistic to expect anything constructed to last that long, especially when no maintenance or minimal maintenance reliance was used, so if your design cannot be guaranteed or at least some reasonable assurance be given that your design can survive that long, then you basically have to modify what your design life can be achieved, what design life can be achieved.

It should not be thought that these sites are going to be totally abandoned. The idea will be that the Department

of Energy or the state will take over control, custody of sites, and that visits will be made during the, on a yearly basis.

However, in terms of making the design, determining what design could really be achieved, EPA concluded that, in their studies in 1983, that we couldn't really rely on more than a thousand years.

CHAIRMAN MOELLER: Well, I don't understand at all the logic of that argument, and I'm not asking you to defend EPA. That's another matter, but why on high-level waste didn't EPA say well, one thing we could do is leave it on top of the ground and put an earth cover, cover of earth over it and since we can only hope that that will last a thousand years, that's all you have to consider?

I mean you don't let the design of the facility dictate your time. You should look at the waste and decide what is needed and then design a facility to meet the need. It seems they are backwards.

MR. GNUGNOLI: Perhaps another consideration should be made. The tailings from the uranium milling process are not concentrated. There isn't anything generated that wasn't done naturally. The only problem, difference between ore that came out of the ground and tailings you have afterward is a change in the solubility, that the toxic and nontoxic things could be more mobile, and that, and that it is ground down.

DR. CARTER: I have got a problem. I dare say the average soil has maybe 500 curies per gram of radium in it, and as I recall the NRC model pile has an average of 500 picocuries per unit.

MR. GNUGNOLI: That was 280.

DR. CARTER: It was around 500. Of course that's an average. Some of them go considerably higher than that, so indeed in any particular place obviously there is considerable concentrated, natural.

MR. GNUGNOLI: No one said ore, natural ore. The original ore that was in the ground, it is not going to be any higher than the original ore.

DR. STEINDLER: Okay, but the argument, that argument doesn't hold if you address the issue of how fast can you get radon out, which is the central focus, and there the subdivision is critical.

There is I guess very large difference in the state of subdivision, so it is really fundamentally concentrated capability.

MR. GNUGNOLI: But it is ground down so it is easier for the radon to get out.

DR. CARTER: Remediation is based on limited escape of radon.

DR. HINZE: One of the concerns that's tangential to this discussion is the waste rock problem. The difference

between ore and waste rock is not a geological consideration, but it is an economic consideration, and we are dealing here only with tailings, and I wonder if what, what is said about the waste rock problem, and in terms of this, and the lengths of time available.

Is this addressed in another TP? Will it be addressed in another TP? Why isn't it included in this TP in consideration of waste rock because it, the waste rock is handled in mining in very similar way to the tailings, and really oftentimes are more subject to erosion than are tailings.

MR. GNUGNOLI: You are absolutely right about that. The reason why we don't have a TP addressing that is that our authority only begins at the uranium mill, so where we have source material or at the uranium mill, our regulation provides for it to be dealt with in the same way as tailings on site and closure.

However, the waste rock at the mine, we have no authority over it. We can't issue a TP saying we will do so and so, such and such. That falls under sort of a mixed bag with the Bureau of Mines and the Department of Labor, and MSHA, so we, although we may have some suggestions or ideas about how to deal with it, we are not in a position to say we will do this and that.

DR. CARTER: EPA actually handles these things under

air emissions.

MR. GNUGNOLI: Clean Air Act in some cases.

DR. HINZE: If I may while I have the mike here, in terms of the rock, there is quite a good discussion of the durability of the riprap and how to classify that, and I'm wondering how I translate poor, fair, good into the length of time that the riprap is going to be maintained in terms of years?

We have a specification here of 200 to a thousand years. What--if I understand the NUREG 4620, NRC has looked at this from the standpoint of the actual mines themselves. I am wondering what has been done to look at the durability of riprap types of rocks over extended periods of time and how that was brought into this TP?

MR. GNUGNOLI: Again, this is a--Ted Johnson would be better to answer the question. I do know that we sponsored a research program with Bechtel Pacific Northwest Laboratories which sort of pettered out with our money for it around 1982. However, from about '77 to 1982, they did do some studies about quality of rock. They did acceleration studies of chemical decomposition and stuff.

They also depended as much as they could on available literature, on that information. Much of this information was dovetailed into the resulting NUREG you are referring to by Colorado state. As far as I know, their

definitions of good, fair, and poor was as dependable or as precise as they could make it with that information.

I don't, I can't give you that good as this kind of durability rating, this kind of--poor is this to that, and fair is this to that. A lot of that is a visual and take a hammer and hit it with, hit it at the site, but other than tha, t again maybe I shouldn't be even trying to answer your question.

DR. FLIEGE': Let me try and answer at least on the philosophy of what went behind it.

Based upon the, the research we had done and just upon historical evidence, it is obvious that there is some rocks that will last for a long period of time.

There is also criteria that the Corps of Engineers uses, for instance, for determining good rock, so the conclusion was that if you had good rock, what a geologist would call good rock, you would certainly be comfortable with determining that that good rock would last more than your thousand years. Then it became a matter of what it is you looked for, and again there were various different tests that the Corps of Engineers had designed to determine good rock, and we were able to actually do scoring on the tests. There was a cutoff point, and it was an arbitrary cutoff point, below which you didn't have quote, good rock, anymore.

Well, then you conceptually, well, if you just are

at that point, what are you going to do? It didn't seem to make sense just to have good and bad rock, so we put a category in the middle and we said well, at this point we are not quite sure whether or not it will, how long it will last, but to compensate for that, what you can do is you can use rock of a larger size than the calculations call for, and that will compensate somewhat for the fact that you aren't sure that the rock might not last the thousand years. It may degrade or erode. When you get below another arbitrary cut point, we say no, that's just poor rock. We won't even allow you to use it by oversizing, and those points are indeed arbitrary, but--and you have to make some decisions like that.

DR. HINZE: Well, I appreciate your comments. I was concerned about this high degree of quantification which I'm very much supportive of leading to these qualitative terms good, fair and poor, and without translating that into years under particular conditions, and error bars on those years.

DR. FLIEGEL: If you know of a way to do that, we would appreciate seeing that, but we don't, we haven't found anything that you can convert a score on a rock or ability test to amount of years it is going to last on a cover.

DR. HINZE: Well, it appears to me that you've used Ed Schuman in some of these studies, and my respect for him is exceedingly high, so I think the problem has been taken care of quite well, but I, we have been talking about analogs

earlier this morning, and one of the ways that geologists like to work is through analogs, and it seems to me that this is one condition where an analog could be put into a, into a framework of quantifying the number of years that you would relate to good, fair and poor.

DR. FLIEGEL: We have, we have seen at least one suggested argument where there was a request to use what appeared to be poor quality rock as riprap, and based upon the scores in our visual observations of the rock, we didn't think that that rock was very good.

We were told that to support the argument they would show us a building that had been around for 200 years that had that rock, which is good analog. You have to take into account the fact that the rock is cemented in place. It doesn't--but the argument was eventually dropped, but we do encourage if there is a disagreement, if somebody proposes a rock that scores poorly and yet wants to make the argument that it will survive, they can use old tombstones, building several hundred years made out of that rock as an analog.

DR. HINZE: Thank you.

CHAIRMAN MOELLER: Another thing that concerned me on this technical position, and it ties into our comments earlier today on the other one, on tectonics, and that is who is providing the overall coordination?

And I say that because it seems that this technical

position is a good example where the systems approach is not being taken, and you have already mentioned in your opening remarks one of the aspects. You pointed out that procedures for the prevention of erosion may increase the probability of increased infiltration and therefore ground water contamination, and while in the technical position you provide the following words of caution, and let me give the words of caution--you say, quote, the decision to use a particular reclamation strategy should consider all possible failure modes with respect to all the applicable EPA and NRC standards--well, you have cautioned them there, but then you go on to say that quote, the systematic process to address certain design aspects other than service water erosion is beyond the scope of this technical position, and is therefore not addressed.

And then you go on and you even admit that addressing only the concerns and criteria detailed in this position may not be sufficient to address the other features necessary to comply with other applicable regulations and standards, so you are saying here is how to prevent erosion, but we are not sure, having done this, or followed this position, that you are going to comply with the other regulations.

Well, who is going to assure compliance?

DR. FLIEGEL: Well, I'm--that's not quite what we

meant to imply.

What we meant to imply was that this position only covers erosion protection. There are other standards for other, other standards that cover other aspects of the design, and NRC will review those.

For instance, in ground water protection, we are in the process of drafting up positions on that and working. We put out a draft position on alternate concentration limits of ground water. All we are saying is that this one position can't tell you how to do everything, and--

CHAIRMAN MOELLER: But it also says even if you follow this position, you may be at odds with other related positions.

MR. GNUGNOLI: I think basically that the intent there was to avoid the impression that they were able to put a cover on, indeed met the one thousand year criterion. It doesn't guarantee that they, for instance, would meet the radon control. We would have to make sure they have had enough soil on there to protect against radon release. It is sort of a disclaimer to say just because you do this right doesn't mean you are going to, you're going to meet all the other requirements. Perhaps it is misleading. That can be changed.

CHAIRMAN MOELLER: It says if you follow this one, you almost assuredly will not comply with the others.

Now let's--

DR. FLIEGEL: That wasn't the intent.

DR. STEINDLER: To pursue that further, the recommendations of the technical position, from the standard point that if they are followed, does the applicant immediately get into serious irrevocable trouble with any other regulation?

DR. FLIEGEL: That's not the intent. The intent was--

DR. STEINDLER: I know that's not the intent. My question is have you guys in the production of this document asked the question can an applicant get into irrevocable trouble by following our recommendations in this technical position?

DR. FLIEGEL: Yes and no, in the sense that if you blindly follow this technical position, and try and design a site with the best possible erosion protection, without looking at other aspects, you may in the end find that you don't meet other aspects of the standard, but since the technical position talks about, about several different conceptual ways of designing erosion protection, and each one, there is an infinite number of variations, we are confident that in most cases, you will be able to meet the erosion protection standard and the ground water standard, and the radon protection, radon emission standard, but it just

cautions and says don't preliminarily try and take the best or the cheapest erosion protection cover. Look to meet this standard and in that way--and without looking at other aspects other aspects of the standards. It's like any design. You are always looking at compromises and what is best to meet one particular criteria may not be best in the overall design. This standard doesn't lead one to only one way of doing it.

CHAIRMAN MOELLER: Well, in going on, another area that I have found a little bit troubling or difficult to understand, there are many exemptions that grant--you say we are designing for the probable maximum flood or probable maximum precipitation, is quote, impractical. The staff will accept the standard project flow, and then you state where the provision of combined stable soils, top slopes and/or rock protected side slopes is quote, excessively costly, other approaches, at least you say may be acceptable.

It seems to me you are granting a lot of flexibility. Maybe that's all right. I don't know.

DR. FLIEGEL: That was the intention, to put the flexibility in there. We are, for instance, in the probable maximum flood or precipitation, we are saying that's an acceptable design. However, if you have got a situation just as a hypothetical situation, if you look at a design and you need 10 inch rock to meet, to meet the probable maximum precipitation to your calculation, and the erosion protection

requires 10 inch rock, and you look at the sources available, and the only sources nearby can give you 8 inch rock, and you do another calculation which shows 8 inch rock will protect you to 80 percent of the probable maximum precipitation, but it would require hauling rock two or three hundred miles, at much greater expense, you might be able to make an argument that No. 1, 80 percent of the probable maximum precipitation will give you reasonable assurance that you are beyond the 200 years, reasonable assurance for survival 300 years, maybe even a thousand years, and the cost benefit is not warranted in going through the larger rock.

We purposely try to put that flexibility into there, but that does not allow somebody to go in and start off and say I am going to start off with 50 percent probable maximum event.

MR. GNUGNOLI: In addition, that flexibility is sort of mandated by the Atomic Energy Act to meet the standards. They sort of both are in that door. We have to basically-- try to lay out the guidance for that.

DR. STEINDLER: Is there similar flexibility with the EPA rules?

MR. GNUGNOLI: The EPA rules for the Title I sites, the inactive sites, have a section of regulation referred to as supplemental standards where if there are a number of criteria that if in meeting the primary standards you end up

causing greater environmental harm than good, among other criteria, you can suggest other standards that would try to achieve or will achieve the same level of protection.

DR. STEINDLER: Economics is not an issue?

MR. GNUGNOLI: There is an economics issue.

DR. STEINDLER: There is still?

MR. GNUGNOLI: It is more--

DR. STEINDLER: The report said something about economics--

MR. GNUGNOLI: There are--most of the time, the provision applies to the vicinity property, sites off of the actual site, but there is a possibility of using that on what was previously the entire waste disposal side. In cleaning it up, you actually end up in reducing encapsulation, areas outside the encapsulation cell, still within the site boundary, you can use cost benefit and economics considerations.

Title II for the licensees is very clear. In the Atomic Energy Act, it says the staff can grant exemptions where you still achieve the same level of protection or no greater environmental harm is achieved.

DR. STEINDLER: My concern is whether or not the exemption, granting that, the NRC is in full concert with the EPA portion of the standards.

MR. GNUGNOLI: Yes.

DR. STEINDLER: If it is not, then I think the NRC--

MR. GNUGNOLI: The Title II standards for EPA I don't believe are--I have to check on the explicit revisions for economic consideration or for exemptions, but we did use the 84C provision of the Atomic Energy Act to put that in our standards which we conformed to EPA standards.

DR. STEINDLER: And I assume you have had somebody look to see whether or not those two regulations are--

MR. GNUGNOLI: We have, our OGC has looked at it and we have provided an analysis of, the Act requires us to provide analysis to EPA for their consideration.

CHAIRMAN MOELLER: Twenty picocuries per square yard, is that per second--per square meter. I'm sorry. It should have been back--is that above background?

MR. GNUGNOLI: It is above background, yes.

CHAIRMAN MOELLER: What is background? Where? In this area or where?

MR. GNUGNOLI: What happened is the EPA relied on, to some extent, on our studies back in the late '70s and early 80s. Fort Bacon gave us a number of--other contractors went out and sampled various areas in the west for radon flux, and Oak Ridge sort of pulled all that information together and they came up with a range that seemed to indicate that it, the natural range of background, like the upper 95 percent confidence limit, was out of the order of 2 picocuries per

square meter per second, something like 1.8.

CHAIRMAN MOELLER: From where?

MR. GNUGNOLI: Areas they studied in a lot of places.

CHAIRMAN MOELLER: Let me read--this fellow Robley Evans, I would take his word. He says the following--a typical value for the flow of radon from ordinary surface soils into the atmosphere is about a 10th of a picocurie per day per square yard.

Well, if you flip that around, it's about 20, as I recall, about 20 picocuries per second. Say there are 10,000. How many minutes--1440, well, we can work it out. We will, but I think it is about the same, so it is not jiving unless I made an error. It is not jiving with 2 picocuries.

MR. GNUGNOLI: I do recall that Dr. Evans was cross-examined during one of the hearings in New Mexico, and Dr. Vernon Rogers asked Dr. Evans had he been out lately to do any of these measurements, and Dr. Evans had indicated he had not, so we intended to go with Dr. Vern Rogers, that he has actually been out and doing it with more modern equipment as such. We felt more confident in what Dr. Rogers had come up with, and what EPA did to get the 20 was they did a cost/benefit analysis, and for the standard, and they found a break point to be--they considered 2, 6, 20, a hundred I believe were the numbers they considered, and they came up

with a 2 in their analysis.

CHAIRMAN MOELLER: Well, if the 2 is correct, then it makes the 20 as the standard almost with or without background. And that is 20 versus 22?

DR. CARTER: My guess is this number is somewhere reasonably close to background, but I've got a question that is more significant.

When you make these measurements, you make them over a small area; mill tailings--there is a tremendous variability from one place to the other. You don't have to go a hundred yards or anything else. You can go 6 inches over. You have got a crack or crevice, you get more radon, and the question is you have got this number now and it is to be used on an average basis over the entire disposal site for one year period.

How many samples do you have to collect to average, to get that kind of rate of confidence that the NRC uses?

CHAIRMAN MOELLER: And what year? Is this the 999th year or first.

MR. GNUGNOLI: We have all wrestled with that one ourselves, the fact that it is a, sort of implies an average over space and time.

What we have tried to do in implementation is answer the question of the variability and the reasonable assurance which as you know in the, earlier in the morning was a vague

term in itself, by dealing with moisture and we tried to look at the wilting point, say if you design the site to meet that radon limit, and it is a wilting point in the soil, we will feel that that gives us a reasonable assurance.

That may be too strict in some people's interpretation.

DR. CARTER: As I recall, when I read the position, I didn't read each of the appendices, but I gather the technical position that you know--

MR. GNUGNOLI: It is. They are supposing that you, you deal with the amount of soil or whatever material you used to protect against radon release, and that this radon or this cover is basically to try to rely on that barrier's stability, and so they don't address whether you need 3 meters, 2 meters or what for the radon. That is determined independently, and then after that you ask the question do you mean to keep it there?

DR. CARTER: I would think that would be a question that would come up in every case because obviously--average, economically that makes a tremendous difference.

DR. FLIEGEL: Well, another thing that is happening at the Title I sites is that in designing the radon barriers, it is turning out that the infiltration reduction aspect of the radon barrier is becoming more significant. The barrier that's used to prevent radon from coming out is also used to

prevent water from coming in, and when you design it for that, in most cases, the calculation will show you also address the radon infiltration aspect of it.

DR. CARTER: The emphasis I think is on preventing radon coming out rather than the other aspect.

DR. FLIEGEL: That turns out to be easier to show than keeping water from going in.

DR. CARTER: You can calculate that.

DR. FLIEGEL: Yes.

DR. CARTER: Rather readily.

DR. STEINDLER: Am I incorrect in making the assumption that this technical position in fact should be silent on radon? Is it strictly a wind and a water protection?

DR. FLIEGEL: Was the intention of this technical--

DR. STEINDLER: The radon discussions are somewhat incidental only to alert the reader to the fact that you have got some other things that you need to worry about, and don't lose sight of that when you are worrying about wind and erosion, which is what we are discussing here.

DR. FLIEGEL: That is correct.

DR. STEINDLER: Is that correct?

MR. GNUGNOLI: 3.64 on methods and techniques, that's provided to DOE and our licensees.

DR. CARTER: I want to ask you whether things have

been left out or not. Normally when you cover uranium mill tailing, in addition to the design objectives you list here, that you place or that you intend, one of them has always been as far as I know the reduction in gamma exposures to any population that might be nearby, and again, you list four objectives, but that's not one of them.

MR. GNUGNOLI: The primary standard for the waste disposal sites do not mention gamma. However, the off-site part of the standards do. Generally that's dealt with in terms of structures and soil that has been used for foundation materials, and there the thrust has been just simply to sand blast, clean, remove, replace, rather than worry about a cover.

It has been our experience that when we have designed a radon, the direct gamma from the tailings themselves are pretty well moderated as well.

DR. CARTER: I agree with that, but you are preventing radioactive releases due to erosion, providing long-term stability, designing for minimal maintenance, and meeting radon release limits. At the same time that you do those things, you are also ameliorating the gamma component and reducing any gamma exposures to the near-by people. I don't know why you just don't list that as design objectives you are accomplishing.

DR. FLIEGEL: Simply because it wasn't in the EPA

standard.

MR. GNUGNOLI: But that is something we can certainly take credit for in doing that, you are absolutely right.

DR. CARTER: It is a gimme. You might as well take advantage of it.

CHAIRMAN MOELLER: Let me go back, just so the record will be straight, I have recalculated using Robley Evans' number of the 10th of a picocurie per square yard per day, and if I'm correct on the number of seconds in a day, it is about 86,000, so say it is a hundred thousand seconds a day, it comes out using Evans' number that you get about 1 to 2 picocuries per day per square yard or per square meters, so the numbers are the same. I couldn't believe that he would be off.

DR. STEINDLER: Picocuries per second.

CHAIRMAN MOELLER: One to 2 picocuries per square meter per second, and so you are correct. Therefore, if you take that as background, then the 20 doesn't cause me concern, whether you include background or not. It's--no one is going to be that accurate. Okay. Well then, that's helpful.

DR. STEINDLER: I've got a couple of questions. One of the things that struck me in this, I went through the front end of this document and you have two interesting discussions, One, the comments in the succeeding paragraphs of one

paragraph starts out saying presently very little information exists on designing covers to remain effective for 200 to a thousand years.

It seems like a reasonable statement, and then you go on to say because of this basic lack of design experience, and technical information, we are going to give you folks some standard hydraulic design methodology, but no place do you make the connection that says that this standard hydraulic methodology, what you are going to lay on the applicants in what I consider to be painful detail by the way, in comparison, for example, to the technical position that we covered earlier this morning, nowhere do you make the connection that this standard approach will accomplish what you think it should accomplish if followed carefully.

Am I missing that connection?

DR. FLIEGEL: If it is not clear, we probably should say that.

DR. STEINDLER: Some place you ought to at least make the statement that you believe it followed--you can meet the criteria. Okay.

MR. GNUGNOLI: Think if I could add one bit of clarification there, the idea was to put out procedures with constraints so that you--the problem, depending on whose sources you use, you probably can justify anything, and so these were put down in terms of Ted Johnson's experience and

ability to sort of say did you do it this kind of way, you will probably end up with the same kind of conclusion we do, to design to the probable maximum flood and such, but yes, it is sort of I guess left there is a--

DR. STEINDLE": It is useful to make that connection, to give comfort to somebody.

You go through in the section you call design criteria, Section 3.2, and you give design criteria for various things and types of material.

It isn't clear from my limited reading of the appendices on a subject I don't know anything about, so I feel free to speculate, it isn't clear to me that the reader or the applicants would know when he has met your design criteria from what follows.

In other words, the criteria are not given in performance terms. They are given into why don't you do this kind of thing terms, and they are sufficiently vague so that I'm not sure that one would know that a successful following of your design criteria has been accomplished.

DR. FLIEGEL: Let me take a look at that section.

DR. STEINDLER: If that's what, if that's the conclusion, then my recommendation would be you might want to look at that section again, 3.2, and see whether you could add some comments that say if you do that, the cover will perform as follows in a testable sort of way, and and I think my last

comment is there are significant number of references in this technical position to all manner of documents from all kinds of sources. Who needs the technical position?

MR. GNUGNOLI: I think that I went through trying to follow Ted Johnson. The way he was pulling stuff from one document and another, believe me I wouldn't want to have to go through that again. It just would--what happens, one source will give the information about channels that are designed a certain way, and then the geometric configuration is different. You have to go to another source, and it just, it is like one painful hop after another. I think it's--in order to try to deal with all the various kind of geometry configuration flows, and drainage areas, this was very needed.

I went through and did the independent equation and mathematical analysis, and it was harder than the thesis. It was really difficult, so I think that this is a needed position, just if you only looked at it from the point of trying to get all that information from all those sources.

DR. STEINDLER: So your use of the term technical position, your use of the technical position is really a scientific document that pulls into focus all the existing literature and hauls all the things--

MR. GNUGNOLI: That's right, and things that--

DR. STEINDLER. This morning's discussion causes me some conflict as to what exactly a technical position is. I

will have to go back.

MR. GNUGNOLI: Our idea was just to give some people guidance on how to do it. That's really the point.

DR. FLIEGEL: It was basically to give guidance to our licensees, primarily the consultants, the hydraulic engineering consultants, as to how to design an erosion protection cover, and what procedures we find acceptable.

CHAIRMAN MOELLER: Going back to one of my earlier comments, which now would be substantially changed since I made an error in the factor of ten in estimating natural background release rates for radon, well then, if--I'm just asking if you have any thoughts on it--if for a high-level waste repository, EPA requires the risk be no greater than the original unmined ore, why are they willing to, for uranium mill tailings for the releases to be ten times what might have occurred? Of course I don't know what release rates are in the uranium--

DR. CARTER: As I recall, the uranium mill tailings originally--

CHAIRMAN MOELLER: Five.

DR. CARTER: Five--and they got so much flack on that that they raised it, and that's roughly comparable.

MR. GNUGNOLI: Part of the history of it is when we came up with the two, we looked at the data that Dr. Rogers had analyzed for us. At that time also, the industry was in

much more of a economically healthy situation. When EPA had considered various numbers and goals, the industry was already in decline and tectonic did a cost/benefit analysis to see what risks they were vulnerable to in terms of health risks and in cost to achieve them.

When NRC did this, we didn't do that kind of an analysis. We did a sort of an economic analysis. Because it wasn't cost/benefit, certainly perhaps the tolerance for a higher level of flux without significant impact was, didn't factor in as much in our decisions back then whereas it was much more significant an issue at the time EPA came up with their's.

CHAIRMAN MOELLER: I remember I guess Congressional hearing on this where they showed that there aren't that many people living next to the tailing piles and because of the atmospheric pollution if you went a kilometer away, you couldn't find it. Okay.

DR. CARTER: So they put out rigorous--Dave, I have got a couple of comments I would like to make. One of them deals with Appendix A, that equation there which calculates the critical distance, and that equation is obviously not pure from the dimension standpoint. Things are raised to the five, third power and so forth, and units are given for everything in there. They are identified with the exception of the roughness factor.

I was just curious where that equation came from and how you do wade through the units.

MR. GNUGNOLI: I think the whole process started with the the Horton reference. It's been quite a while since I doublechecked the math. The math works out, but there are some assumptions that are made in there. I think that again in one of the steps a geometric configuration is assumed, and I believe it is triangular, and there is another equation you use to get that.

DR. CARTER: I presume that's empirical.

MR. GNUGNOLI: I do believe that is the case.

DR. CARTER: The other--I thought it was kind of interesting, in reading it, there are several gratuitous comments, and one was on rock placement, and the statement is made that a 12 inch layer of 2 inch rocks is easier to achieve and a 12 inch layer of 8 inch rocks, and even Dr. Parry was--

DR. PARRY: Thank you for the compliments!

CHAIRMAN MOELLER: And also went with it the depth of one and a half times the size of the biggest rock. Okay. Well--

DR. CARTER: I just wish they would have put a 12 inch layer of 2 feet--extremely difficult.

CHAIRMAN MOELLER: Okay. We will have to tackle--are there any other comments or questions? Gene?

MR. VOILAND: Yes. I was just curious about the

historical period in the United States where there is experience with riprap. I know 50 years ago I was planning riprap on the Swami River in the State of Washington, and incidentally it got washed out every so often when you had a big rain and snow pack and so on.

I would be interested in that, and also what experience in Europe, because I'm sure that they have been doing this sort of thing. One of the nice things about this technology if you will, it has been around a long time. There is a certain empirical body of knowledge I was just kind of curious about

CHAIRMAN MOELLER: When they were designing the floating nuclear power plant in the breakwater for that, what was the word, Dolos or something, D-O-L-O-S or something, they used to finally in theory cover the tops, and they had lots of experience in California with that cover, and it works very well.

MR. VOILAND: The other question I had is that this deals, seems to deal solely with using rock, riprap. Is it used in conjunction with concrete?

I have seen that, and whether concrete is considered to be eligible for the 200, one thousand years criteria--the Romans put in aquiducts ducts in with concrete underwater. It is still around.

DR. STEINDLER: They didn't do that under a

licensing agreement?

MR. VOILAND: They had to put up with some problems.

DR. CARTER: Nor the lowest bidder!

MR. VOILAND: Another question--over a long period of time, do mill tailings consolidate at all, or do they still remain as flowery as they were?

MR. FLIEGEL: Do you want me to try and answer some of those concerns?

CHAIRMAN MOELLER: Go ahead.

DR. FLIEGEL: In terms of experience in riprap, when we first started looking at designing riprap covers about five years ago, it became evident to us that most of the work on design of riprap was designed for flowing water and stream channels, and the problem we were facing was somewhat different in most cases, that you were having precipitation falling on a pile and the water flowing through the riprap, and it wasn't clear to us that whether the same equations were valid because a lot of equations were indeed empirical, so we, we did some work and we did some theoretical work and we did some work in places in which we actually set up slopes with riprap and put water through, and it was from that work that the equations that you see here come from, so we did look at what experience, and primarily a lot of that experience is the Corps of Engineers.

In terms of the use of concrete, two comments--No.

1, concrete is more expensive than just native rock. Floating nuclear plant, you talk about a multi-billion dollar project, and that was a significant cost, and at the time there were arguments about whether or not it had to be reinforced or not, and in terms of concrete in general, it's not clear about the durability of concrete. Whenever you are dealing with some man-made materials, you can find instances where some of it has survived, but you don't--it is not always clear why it has survived one place and maybe ten other places that you don't know about that it didn't survive, so that's the problem, that we have had people propose asphalt covers, for instance, and generally our general experience is that concrete tends to crack. We don't expect any serious consideration of concrete.

MR. VOILAND: I was thinking in terms of you might have some small area which is really appropriate for the riprap that you could use the concrete to help you out. Cracks, it is going to prove--

DR. FLIEGEL: Well, eventually if it cracks, it is liable to crumble and break up. We just don't know of any way of assuring 200 to a thousand year lifetime under those conditions.

Part of the answer to that is also the answer to your third question about consolidation. At present, the going knowledge as far as we can get is that generally within three to five years after we stopped putting water in these

piles, they tend to go through the majority of their settlement and consolidation. However, we recognize the potential of differential settlement in the future as being very possible.

We tried to put stipulations in the licensees and the Department of Energy how they can distribute materials and tailings, how much organics they should be allowed to put in there if they are going to topple trees or things of that nature, so it goes hand in hand that potential for consolidation and differential settlement is one of the main reasons why we shied away from using concrete or--and asphalt covers as well.

It also causes some concern about artificial liners for ground water reasons, so after this was done, and this was again studied not only by NRC and its research program but was part of the R&D effort by DOE and they sort of came down to the same conclusion about the, I guess the more exotic kind of materials.

MR. VOILAND: Thank you.

CHAIRMAN MOELLER: I think we better wrap it up then, and we will take our lunch break.

Let me thank the staff for being here. We apologize in terms of last month. I know that was wasn't good, but apparently there was no way we could avoid delaying the review.

We will try to provide you something in the way of a written summary of written comments at this meeting.

We will take one hour for lunch.

(Whereupon, at 12:15 p.m., the hearing was recessed, to reconvene at 1:15 p.m. the same day.)

AFTERNOON SESSION

(1:20 p.m.)

DR. MOELLER: The meeting will come to order. We will pick up on our schedule. And the next item to be addressed is a progress report on the low-level waste performance assessment methodology. And this is mainly for information and for possible comment.

Our presenter is Dr. John Starmer, who is Section Leader for the Siting Section within the Division of Low-Level Waste Management and Decommissioning.

John, it is a pleasure to welcome you.

(Slides being shown)

DR. STARMER: Thank you, Dr. Moeller. I just throw this up to re-emphasize that this is a status report. It is an ongoing, if you will, project activity in the division, and I would like to give you a little background, talk to you a little bit about what we are doing in this area, and then offer up some of our observations on performance assessment to this point.

As you know, the regulation is --

DR. MOELLER: Excuse me. Could that be moved over so that we all have a chance to read the transparency?

DR. STARMER: Is that better?

DR. MOELLER: That is much better. Thank you very much.

DR. STARMER: Fine.

As you know, the regulation is somewhat unique in that it has performance objectives, poor performance objectives, and a series of technical requirements to support those performance objectives.

In addition, certain analyses are called out in the regulation to demonstrate to, you have to make analyses to demonstrate the performance objectives have been met.

The analyses are generally considered to be deterministic analyses. They are to demonstrate that, in particular for performance assessment, 61.41 requires that certain doses to the exposed individual be analyzed and shown to be less than specific values. And in 61.42, analyses have to be made to show that an intruder would not receive doses greater than a specific number.

If you wonder why I only have 61.41 up there, the magic graphics program that I used ended there for some strange reason, even though I could have, obviously on the diagram, gone over further. I couldn't fit any more on. But it is 61.41 and 61.42 that are the specific subject of performance assessment in the aspect that we are using it today.

When the regulation was passed in 1981, there was a great deal of emphasis put on the groundwater pathway. In terms of exposure to the public, this was shown to be one of

the major manners in which someone might become exposed if the facility did not operate as was predicted.

At that time, there was not much emphasis put on things like the air pathway. It was felt that air pathway codes or analysis methods such as that in air dose or the codes that were used for uranium milling, could be adapted. Direct radiation exposures to the intruder were considered to be covered by methods that were outlined in reg guide 1.109. So that a great deal of emphasis was put on groundwater pathways and the staff actually identified about six groundwater codes of varying complexity that they suggested that the applicant could use.

This was publicized more or less by word of mouth. People would say well, what codes would be use? And a list was generally available to tell people what might be expected.

In 1895, Congress passed the Low-Level Radioactive Waste Policy Act Amendments. And the amendments, one of the aspects of that was that staff would be required to do their review, not including the hearing, in 15 months. This forced staff to look at the review procedures. We took several actions at that time. We developed a set of review plans and updated a standard format and content guide. And, among the other things that we did, we looked at what it would take to do licensing and

what sorts of review we would have to do that might include pathways analysis, performance assessment.

We decided that we did need to define a methodology. The definition of the methodology was not just for us. But it was to make sure that an applicant had a pretty good idea of what staff would find acceptable, and also staff would have the components of that methodology available and ready for use.

We also felt at that time, on looking at where we stood in terms of, there are lots of models, there were many models in the computers, that it would be very useful first of all not just to have groundwater pathway models but where appropriate have models for other pathways and other exposures, and to make sure that the methodology was defined for all pathways.

DR. CARTER: I presume that there was some concern about air pathways, because you've got, of course, 75 millirem in as a dose to the thyroid. And I presume that is probably where that would come from.

DR. STARMER: That is true. I think that what they did, when they did their analyses, they found the groundwater pathway was very important. What they said was there are approaches we can take to estimate the air pathway exposures.

Our review in 1985 said yes, they are available,

but many of the staff don't have them, they aren't readily available, they are on somebody else's computer. We wanted to have them ready. We also wanted to be able to define for the applicant and to help the agreement states find out what we would expect at least as the national regulatory agency, just to define it.

It was not that we de-emphasized it and would not have considered it. It was certainly to be considered.

I think what we felt was that we have a lot of hydrologists, and there is a lot of concern for groundwater, that they needed to be made aware and make sure that that methodology included all pathways. And I will actually talk a little bit about some of the types of models and codes that our contractor has come up with.

DR. STEINDLER: When you say "they," are you talking about the original Ford, Bacon, Davis Utah study?

DR. STARMER: No. I'm talking more about the analyses that were done for the DEIS and then for the updates of the impacts analysis methodology.

It took someone a while to get a contract in place. But in January of 1988, we let a contract, or started an interagency agreement with Sandia National Laboratories to provide us with what we call an integrated performance assessment methodology.

Just to make sure our terms are understood, this

is in effect an approach to be able to do pathways analysis and to be able to look at all pathways.

We recognized Sandia's work in the high level area in terms of code implementation and integration. And we asked them to find codes, not develop codes, but to find codes that we could use. If there were no codes they would come to us with that finding and we would decide how best to develop codes. But there were so many codes, it seemed like there should be good codes. And so we asked them to implement and integrate the codes that they found so we had a package that was available that sort of would set a standard and would allow us to do check analyses.

Also, as typical with many of these contracts, we asked them to provide us some technical support. They recently did a review of the performance assessment calculations and methodology that was used in the below ground vault plasar. You were briefed earlier on the EMCV plasar, the earth-mounted concrete bunker plasar. We did not have staff available. And the contractor was very busy at that time putting together, doing the preliminary work to put together the methodology so we didn't analyze there or didn't look at their performance assessments. They had done that for us. If we had problems with a code or something, we would be able to ask them for some help.

I give you a little bit better idea of exactly

what they are doing, this is very broad. There are about, I guess, eight or ten tasks. If you look at the inter-relation between these, it is a little bit more complex than here. But the main aspects of the project were to define pathways that were possible from a low-level waste site.

Now, again, we are not trying to reinvent the wheel. We are more trying to document the invention process and make sure that we have a documented analysis of pathways that have been used before and have looked at them to say is this a reasonable set that covers all pathways.

We asked them to prioritize pathways. And what this really amounted to was looking at change of pathways, and to come up with qualitative estimates of where you could cut off and say I think, for example, if I remember correctly, they said unequivocally, by the time you get to about four chain pathways, there really isn't much need to look at it, because you've had so much dilution by that time of a very small source term.

Now, you can say that if they didn't do a lot of calculation, maybe that is not reasonable. But I think what they have done is they have documented their reasoning process and the approach they took to give us a firm background to say these are important pathways.

DR. CARTER: When are they scheduled to report under that contract?

DR. STARMER: I was going to get to that. We will have documents which we can distribute to you as NUREG documents on the background material. I will get into it a little bit on the schedule.

DR. CARTER: Fine.

DR. STARMER: They are to define and integrate models.

Now, I would like to make what may seem to be a small difference or to emphasize a small difference. We wanted them to look at the various ways of modeling the transport along various pathways and to look at exposure models, rather than computer codes, again, trying to go step by step with a rational basis so that at each point along the way we can say: here was the basis for choosing this model; we looked at this type of model for air transport, a Gaussian plume versus a finite difference model, and here are the pros and cons; and here is why we suggest, as contractors, that this is the approach to take.

Once they had identified and defined models, they were to also look at how difficult it is for say chained pathways, or when you have to take the output of the groundwater model and do calculations for example on exposure due to intake of groundwater or irrigation type of exposure for an agricultural scenario, what would be necessary to integrate those models to come up with

something that gives us a dose in the end.

After they were to do that, they were to use that and come up with, identify codes, and implement those codes, not all the codes that they identify, but selected sets of codes that they would suggest that we could use for our analyses to check the analyses presented by an applicant.

Now, then, the final task is to develop what is called a self-teaching curriculum. We used that approach because it was successful in a high-level program where they developed several of these so-called self-teaching curricula which allow a staff member, who is reasonably computer-literate and hopefully modeling-literate -- you can't just take someone necessarily off the street and give them this, but -- someone who is familiar with modeling and computers and things, and stepped them through he process, developing the input files, running the code, hopefully some information on interpreting the results that come out.

To date we have a draft report which is not being finalized on pathways identification, one on prioritization of pathways, one on the definition and integration of models that are applicable to this problem, and a code identification task report.

Those reports that I mentioned are in the process of being prepared for NUREG. They have to be put in a specific format. There were some minor editorial changes

that had to be made. And those will be available at least by the middle of November, is our expected date now. I am hoping it will be sooner than that. It is all mechanics at this point.

DR. MOELLER: Now, are you going to cover the work at Brookhaven and PNL?

DR. STARMER: That was going to be my next.

DR. MOELLER: Oh, fine.

DR. STARMER: this is a Sandia contract.

DR. MOELLER: Right.

DR. STARMER: I am now going to go into as much detail as I would like to on all of the contracts everywhere. But there are some in the next slide.

DR. MOELLER: Okay.

DR. CARTER: Let me ask you a question about this one before you leave it.

DR. STARMER: Sure.

DR. CARTER: I presume the first four of these in essence are literature review, based on literature review.

DR. STARMER: They are based on literature reviews and the contractor's expertise in the area of code. We have some very good actual code people, people that are, you know, like FORTRAN programmer types of people. We have people that are hydrologists. They brought in some of their meteorologists, for example. And they also took input from

our staff. We have experience, for example, in some of the direct exposure pathways for fuel cycle facilities and materials licensees.

So they have a broad expertise. It is in this area. And yes, the point was, we did not ask them to develop new models and things. We thought there were enough there. And so far it has turned out to be true. There is some controversy as to which one is the right one and how much is enough. But there are codes it seems for all the pathways, some very good codes, that are available.

I have a list, by the way, just at least, the last slide. So maybe if you are interested you can look at that and make some observations.

DR. STEINDLER: The work of Sandia was for the staff as its customer. Is that right?

DR. STARMER: Yes.

DR. STEINDLER: Was there any thought that there may be other customers who may be passionately interested in that subject? All the state compacts?

DR. STARMER: Yes, there was. There was a great deal. The thought is this. Once we have defined a methodology, and series of codes, if people are going to say gee, that looks like codes that NRC, or NRC's contractor finds are adequate, do a good job. And one of the things we did, for example, we didn't specify, but basically the

criteria or the finding was that the types of models that are defined in reg guide 1.109 are good models as far as they go, and so then once they had the models, they said these are fully adequate, they've been used, industry accepts them, the public accepts them, the professionals accept them, they have had public scrutiny. There is no reason to look for other models.

What they did, though, is to look at a couple of different implementations of those models as codes. So there are different codes that have been used.

DR. CARTER: Have they identified, for example, anything else in the way of radionuclides that would need to be monitored, in going through this review process?

For example, iodine 129, just as an example. It is obviously in some of the low-level sites. As far as I know it has never been measured. But was there any indication in this that things of that sort weren't routinely monitored around low-level sites?

DR. STARMER: Not really. Because this has been more of a mechanical operation to look at methods of prediction rather than methods of monitoring.

DR. CARTER: I realize that. But they didn't get to that when they were taking a look at source terms and pathways and so forth.

DR. STARMER: No. No. John?

MR. SURMEIER: John Surmeier. Just to provide a little bit more clarification, Dr. Steindler, we have also interacted with our regulators, state regulators in this area. They have seen the draft material that Sandia has had. And there is also a technical coordinating committee which is for the states who are going to be the host states. And they have also participated to an extent in knowing where we are.

So we are trying very hard to keep them all informed in this area.

DR. STEINDLER: That helps. Thank you.

DR. STARMER: I just thought I would mention some of the other work that is going on or is planned.

When we originally looked at the integrated methodology project, we concentrated on the releases post-closure. We had to prioritize and look at what we could do with the amount of money that we had available at the time.

We have identified an area of the exposures due to operational and accidental releases, and we plan work that will begin after the beginning of the year. The self-teaching curricula is due in January, towards the end of January. And Sandia will at that time go through the same process that they did for the post-closure releases for operational and accidental releases.

One of the things that you get by looking at

pathways analysis, but was not exhaustively looked at, were scenarios and reasonable and unreasonable and which kinds of scenarios you would use to get releases along the various pathways.

We are going to have some work done. I believe the research office is in the process of developing a contract to do an analysis of scenarios. And hopefully at least they will go through looking at the types of scenarios you could expect for release and then do some prioritization in that area.

DR. POMEROY: Excuse me, before you leave that.

DR. STARMER: Yes.

DR. POMEROY: Can I just ask a question?

I bolieve you need a complete set of scenarios in order to do performance assessment.

I wonder, can you give us just a little bit of the thought process that goes in before that, into how you ensure, in this instance, that you have a complete set of scenarios?

DR. STARMER: Well, if I were to do it or my staff were to do it right now, we might be flying a little bit blind, in that nobody, as far as I know, has taken any source of a coherent, intense look at what are the possible scenarios, how you could get it.

I think it is not an intractable problem. But we

just don't have the staff. And how a contractor would approach this would be I hope one of the ways that we would look and see whether we picked this lab or that lab or this company, what is their approach to define what is a complete set of scenarios.

I think you could probably raise the same, or make the same observation about well, which are important and which are not. It is a matter of the sorts of things that can happen, and trying to be complete. But right now I would say that we would probably be looking, right now the way we would look at it, we would look at what the applicant did. And then we would say well, are there any things we can think of that they didn't deal with, any sorts of things that we could think of happening. And then, would they be important and is it likely that they are going to happen or not.

So I think that would be one of the criteria by which you judge a project that was to look at scenarios.

DR. POMEROY: Thank you.

DR. STARMER:

DR. STARMER: Dr. Moeller, you may have been referring to some of the BNL work on source term modeling.

DR. MOELLER: Yes.

DR. STARMER: This is, in our opinion, one of the two basic areas where there are issues. There are probably

some issues involved in just determining what the inventory is. You have to do some sort of modeling. And I think it almost becomes econometric type of modeling that would say what your inventory, what inventory do you project. Do you look at current generation, and you try to project that?

The next thing, though, is how is that inventory packaged, what is its form or what is the waste form, and then defining how the waste form behaves, and the mechanisms for release.

Now, we have had a contract going at Brookhaven now for about three and a half years, I believe, looking at, really at detailed mechanisms for release. They have come up with a preliminary, what I would consider a research code, which would allow you to investigate methods of release and compare them to data on release from various types of waste form.

I say, it is a research code. It is pretty big. I think it is a good code, but it is fairly big and fairly unwieldy, takes a lot of information.

For actual running and performance assessments, to say this is the availability factors, or something like that, it is probable that you want something that is a little less detailed.

The way the code is set up, or the Brookhaven code is set up today, it deals really on a waste package by

waste package basis. And our approach right now would be to try to lump waste packages of a kind based on the projected inventory for that kind of waste package, then develop a release scenario or release mechanism, release curve, for that sort of material.

Take a, an example might be a 55-gallon drum of cement solidified waste, power reactor waste, say resin beads. What are the sorts of mechanisms? The current code will allow you to look based on the water availability, and then look at the waste package, which is considered to be the drum, how it would degrade. It is called the breach part of the scenario.

And then once the drum is actually breached, how the waste material inside, the treated waste, would leach the leached part of the scenario. And there are several models within that, and those models that have been put into code.

As I mentioned, it is a fairly complex code. But I think it is quite useful. Some of the states have looked at it and feel that it really, really is certainly a step in the right direction.

Research is continuing that work, and we are going to provide some funds to Brookhaven to provide us with a more generalized code, which can take groups of waste containers and try to predict performance for individual

waste units using the concept that is similar to the waste stream concept in the EIS and the DEIS and the FEIS.

DR. STEINDLER: Do you intend to investigate or have investigated for you essentially all sources that are possible?

For example, right now -- I'm going to give you a silly example. Right now, the amount of decontamination waste that is being generated is fairly modest. It is not unreasonable that that might rise. But at the moment, it is not a big deal.

Would that be included in some sort of a source term analysis?

DR. STARMER: It should be, yes. That would be one of the sorts of things that we would be reviewing in an applicant's submittal: have you looked at? And that's what I was saying is sort of predicting of what do I see, for example, for the industry, or what do we see on the horizon as new ways of treating waste, for example, what would those effects be?

DR. STEINDLER: Do you intend to look at NARM waste?

DR. STARMER: NARM waste?

DR. STEINDLER: N-A-R-M? You know, the natural and accelerated --

DR. STARMER: The stuff that we have no

responsibility to look at?

DR. STEINDLER: Right.

DR. STARMER: Right. No.

DR. STEINDLER: You don't. Okay.

DR. STARMER: But I would point out, I don't see that it would be -- You can use these sorts of models, the whole performance assessment methodology could be used by a state who is responsible for disposing of NARM.

So it is another type of radioactive waste. And in the impacts analysis update, methodology update, which was published about four years ago, they actually did look at two types of NARM sources. So they looked at discrete NARM sources, such as radium sources. And they looked at, which are still sort of discrete but are larger types of things, which were resin columns used to clean up water which is contaminated with radium. For example, natural radium, to bring the drinking water down to a level, can be cleaned up with ion exchange resins.

Well, then you have now a NARM waste, which has to be handled. And they looked at that. So you can look at that and use the same sort of methodology.

Two other areas that go hand in hand are work on barrier performance and on concrete degradation. Our research office has a project going at Idaho Engineering, National Engineering Lab, on barrier performance. There

have been a couple of interim reports which I found quite interesting. And they are again taking a very step by step, first step at a time type of approach, to look at mechanisms, to look at models for mechanisms, the various types of mechanisms for concrete degradation that they have identified, and also then to look at, eventually, to look and see what sorts of codes could be used to predict behavior of concrete barriers.

On an even more fundamental note, there is work going on in concrete degradation at NIST. We have some work going on there and so does Research. This is really basically looking at mechanisms of concrete degradation on the molecular scale, almost, very detailed work, and over long periods of time something that hasn't been done, since concrete is only about 100 years old, as we know it, the Portland cement type concrete.

And then finally, we have some work going on at PNL on groundwater transport. They have done some things like look at some of the existing sites. And that work is coming to fruition and I expect reports to be available from that in the near future, this Fall or early Winter.

In particular, they looked at Beatty. They found that there was very little information on Beatty. The idea was originally to look at a dry site and then to look at a wet site.

They found that there was quite a bit of information available. And we have looked at the Sheffield site. And we have also looked at the West Valley site. And there is a recent publication from the West Valley site which is basically describing the information that is available on West Valley.

In the future, what we would like to have them do is to look at some of the problems with modeling groundwater transport and groundwater performance, and looking at the groundwater pathway, in particular in the amount of detail that is necessary to provide adequate basis for making predictions.

As you probably all know, you can look at a simple Darcy flow or a simple 1D type of model analytical solution, or you can get very complicated models which take into consideration all aspects of the substrate through which the water is moving and may use very complicated solution techniques like finite element analyses and that sort of thing.

DR. CARTER: Is the BNL work primarily related to groundwater transport or leaching or both of those?

DR. STARMER: Right now it is primarily groundwater transport and release from the packages. And as I say, the PNL would be looking at, the expertise that we expect to have from them is in the groundwater transport

area. They are looking at things like dimensionality of models or will be looking at dimensionality of models, how much data is needed and how much data is enough, and trying to actually exercise some of these models on given data sets, varying the dimensionality, varying the amount of information that the modeler has given.

This is a follow-on type of work that comes from a Chalk River study which I think was presented to you about a year ago.

DR. STEINDLER: How do you integrate your source term models with for example the groundwater transport studies?

Source term models, aside from identifying nuclides, which is just identifying chemical elements, also identify the ionic and non-ionic environment in which those nuclides exist, or begin their migration process. And thereafter, groundwater transport, both kinetics and the actual mechanisms, are a strong function of what source term is. But that also has implications or inferences from the barrier and its performance and the degradation of concrete, and a number of other things that you may be studying.

How do you go about integrating this thing into a coherent product?

DR. STARMER: Well, right now, we use basically very simple and, if you will, conservative estimates of what

is going on there.

There are methods on the horizon. We see more and more of what are called coupled codes, that actually couple geochemistry with transport. But at this point, that is state of the art or a little bit beyond for the types of problems that we are dealing with.

I think the best we can do is to look at things like if you have a large amount of concrete or cement in a waste disposal unit that you are going to at least be looking at very high PHes, probably very high calcium content, probably high carbonate, bicarbonate. And you can use some fairly, well, I wouldn't say simple, but straightforward modeling to look and see what sort of effects that would have.

We do know, for example, that some of the transuranics, and uranium, are strongly influenced, their transport properties are strongly influenced by the ionic character.

So if we look at that and see a strange type of groundwater, we could maybe take that into consideration.

This is a very interesting sort of thing to discuss because the National Academy of Sciences is coming out with a document which looks at regulatory use of groundwater codes.

And at least the version, it should be impressed,

the version that I saw, asked NRC how we could one, use very simple models and two, asked for very complex or very complete site characterization information.

One way that you would use that would be to look at say the groundwater characterization, the type of groundwater, the ionic constituents, look at the interactions that you would expect between that and the waste, and try to make some predictions, have a basis for using a very simple model, for example, or to say this is a very complex system and will require more complex analyses before we can actually accept your predictions of performance along this particular pathway.

But right now we take a fairly simple approach.

DR. STARMER: It might be useful if you flip two slides. There is one called performance assessment strategy. What happened was that unbeknownst to me the xerox machine slipped one. And when I was looking through it, I said gee, this performance assessment strategy should have been first, and I got it back in place wrong.

So if we could look at that one first, we will just talk a little bit about the sort of thing that we have been telling people that they should be doing in terms of performance assessment.

I think that you might possibly come up with a semantic problem. Just because it sounded good, I talked about pathways analysis or pathways and scenario identification. Pathways analysis I believe as most of us normally use the term would be what we are talking about here in terms of performance assessment. That we would do a pathways analysis and compare it against the standard. So just to make sure that that is clear.

The concept though was to say people you should be looking at your pathways, at the potential pathways. You can use our if you will global analysis of pathways to look at which ones that you should pick from. You then should be looking at which ones are the ones that are the most important and which ones are the likely ones. Those are the ones that if you had failure that that would cause a

potential dose. Those are the ones that you should concentrate your analysis on.

Not to say that you do not address other pathways but that you concentrate on the ones that may cause problems. This comes down to whether the pathways will differ for example between an arid site and a humid site. At the arid site, the important pathways are most likely to be air pathways and possible accident scenarios. Wherein at the humid site, degradation of the waste may turn out to be more important. So this is what we are trying to tell people to do under that heading.

DR. HINZE: Excuse me. In your document here, you talk about that the low probability pathways are sets of pathways that can be eliminated, but that the method of eliminating these should be documented.

Could you provide any guidance on this whole subject?

DR. STARMER: Well, the contractor took an approach which I guess by accepting the contract product that we accepted as one way to do it. Again as I mentioned doing qualitative elimination of pathways is difficult, but there are things, for example if you say -- it has been quite awhile since I went through that. But you may find a pathway that dead ends and that has no logical output.

Or you may find a pathway that by the time that

you cycle to let's say the fourth step in a pathway that there is just no way that you could expect say fallout dust raining into a surface water stream not to have been diluted to a point where any reasonable person would have reasonable assurance that that is not an important pathway.

So it is somewhat of a rationalization. You go through the steps in determining what is reasonable. And as I said, our contractors looked and tried to define all possible pathways and change of pathways, and backed off and explained how they came to back off on various pathways, so there is an example at least.

And we analyzed that and accepted it, so I guess that you could say that that would be at least one approach that would work. It is an exercise more in logic than in quantitative analysis.

And that is why I was saying then that the next step is to actually look at the pathways that have a potential for yielding a dose to the public or to an intruder and do quantitative analysis of those pathways and come up with numbers.

Now we believe that a simple approach to modeling is the way to go. Our belief is that if you can demonstrate that the simple model adequately represents the system that it is generally easier to explain how that model works than a very complex model.

DR. STEINDLER: Excuse me. Let me go back to Phil's question. If you were the person who just accepted that contractor report that gives the examples of how to eliminate low probability pathways and you are still around when the next compact comes in for analysis, then I think that everything is in good shape.

On the other hand if you are not around because you have been transferred to some other position, perhaps that acceptance is no longer a valid regulatory posture. And the absence of written guidance on how to go about selecting out the low probability scenarios, that absence now could give the next applicant a fair amount of pain.

Is it not perhaps worthwhile to codify an acceptable process that is acceptable to the staff on an institutional basis?

DR. STARMER: Maybe my management could speak to the personnel problems that that involves.

DR. STEINDLER: Well, I do not need an answer.

DR. STARMER: I would point out that there is an interesting article in Government Executive this month that suggests that we have a government that is hollow due to the austerity programs that we are undergoing. I have a great deal of difficulty keeping with the day to day licensing that we have to do particularly now that we are involved in decommissioning activities.

I agree with you that if guidance is available that it can be a much more efficient licensing procedure. Because first of all, the applicant has a much greater chance of giving the right type of analysis in the first place. And in the second place, since we have already basically stated that this is acceptable if you do it this way and show acceptability.

It really boils down for us at least in a small division with a lot of responsibilities of one of not having adequate means. And it seems like every time that I come up here, I am saying something like that, but that is just the way that it is.

DR. STEINDLER: Okay.

DR. STARMER: Again as I was saying, I think that the simple models tend to be more defensible. You have to be able to demonstrate that the simple model does adequately represent the physical system and the way that it behaves. And that usually will include some more complicated if you will site characterization modeling or measurements of the properties of the site and the way that the site behaves. And then last which is very simple I guess is to compare it to the performance objections in Part 61.

Now one of my last slides is more about what we would review, the sorts of things that we would look at in a general manner, not the sorts of modeling that we might do,

but sort of general things. And one of the points that I make there is that it is very interesting or something that we need to look at is how is that comparison done, and I will talk a little bit more about that one when I get to it.

In terms of what we sort of have defined as what we wanted our global method or our integrated methodology to look like, we wanted it to be global. That means that it is able to treat all pathways. We wanted it to be modular. And I think that this is an important difference between some systems models which are available and have been used for say standard setting by EPA or regulation development by NRC like the IMPACTS code and the PRESTO group of codes.

We wanted to be able to look at intermediate results, the results in concentration for example at the end of the ground water pathway which is being put into the surface water pathway. And to be able to look at important pathways and not important pathways, and where the problems might be developing.

Because if you were getting fairly large doses that were coming from one pathway, you want to spend a little bit more time looking at that. On the other hand, the applicant may want to do a little bit more sophisticated modeling of a particular pathway that seems to be particularly important.

That also comes out of requirements in 6113 that

you be able to allocate performance between the engineered and waste package if you will, the source term and perhaps this barrier that people are proposing, and maybe infiltration, and the limiting covers that they put on disposal units, and the natural setting as a requirement in the regulation.

Again we prefer simple models. The sorts of things as one dimensional stream tube models coupled with estimates of travel time along a flow path developed by just using Darcy's flow if it can be shown to adequately represent the situation.

If you have got some real problems with chemistry, Dr. Steindler, I think that you may not be able to take that approach. But I think that those are the sorts of things and demonstrations that you would have to be able to defend using the simple approach.

The simple approach does one other thing at least for the applicant is that it allows him to look at a lot of different scenarios and a lot of different "what if" type of things in a savings of analysis time. So there are some, I do not want to say economic, but it is more efficient to use simpler models and look at more variables and sensitivity.

DR. MOELLER: Excuse me, on that as you talk, I am wondering. You know, I could come at you and tell you that your complex model to me is simple, and I want a more

complex model developed to assure me that what you consider to be your complex model is accurate or accurately represents the situation.

Is there sort of a rule of thumb or something that tells me which are simple models and which are complex?

DR. STARMER: I think that when we are talking about simple versus complex here is that there are probably two aspects of it. One is the development of a physical model, how much detail do you put into that. If you basically say that I have a potential, and I have a resistance, and I have a through-put, that is a pretty simple model. And it basically only has three parts.

However if I have to describe some of those features over a long flow path where things change, you may need things like looking at variability through space. And an example that you see in the high level waste repository situation which is causing complications of fracture flow versus matrix flow.

And it would be difficult I think to show that a simple one dimensional stream tube model adequately demonstrates that. But I think that this is an area that does not really lend itself to simple rules of thumb. It is an area that is practiced by experienced practitioners. And I think that where the problem comes is for them to explain why they found something adequate.

If I used a stream tube model, how did I get to that. And usually what they will do is a more complex model to show that there is little lateral dispersion for example, that there are no lateral gradients so that they can use the stream tube, and that it is a valid representation of flow along a particular path. Then they can look at the complexities of transport in one dimension. But it is not a simple analysis to show that the stream tube is complex enough to represent the system.

DR. CARTER: You can go about it in several ways. One is just the tools that you need to do the analysis. Whether you can use a hand calculator or something like that. And the other one is the professionalism that goes into it. You know, do you need the meteorologist or do you need the podomologist and all of this sort of thing, or can one guy do this thing for a reasonable period of time.

DR. STARMER: Well, yes. And again one reason for having a modular system would be if you found a particular pathway that required more complex analyses to be demonstrably representative of the system, you could insert a more complex model. Maybe our original modular simple system has all 1-D models. It might be that for the ground water pathway for example that you cannot use that and represent the system.

If you found that to be the case that this was

your site of choice, and maybe for example for political reasons the only site, that you could then demonstrate that it is still an adequate site even though complex and requiring a more complex model and then still get a license. So it is true.

The complexity of the model -- I guess that is what I was saying from Darcy's Law to finite elements to whatever the next step is is an indication of complexity.

Models that we consider deterministic models as the way to go. We are dealing with deterministic regulation. And I started using a new word that they should be robust. Conservative has such a bad connotation. There was the National Academy report that really sort of comes down on regulators who constantly are using ultraconservative estimates.

We have always tried to use what we considered conservative but realistic. In other words, erring on the safe side. But that is easily interrupted incorrectly. What I am saying is hopefully you would have a modeling technique or use a modeling technique that when it errs that it errs on the side of safety, but it is as realistic as you can make it and it is strong.

I got the idea from Tukey and Mosteller and their idea of robust statistics, and it is similar. Because it is a matter of the amount of data, using small amounts of data

to get good estimates of what is happening.

DR. STEINDLER: The term robust is often used in an altogether different context at least in chemical and analytical fields. You need to be careful that you define that for the poor chemists among the audience that you address.

DR. STARMER: Okay.

DR. STEINDLER: In fact where it is used very often is in analytical methods. The term applies to methods that are somewhat insensitive to screw ups. If that is what you mean, fine.

DR. STARMER: In a way.

DR. STEINDLER: That may not be bad.

DR. STARMER: I would not say that that is the only concept that is involved there. Maybe it is in fact a little bit new twist. But hopefully that would be if you err that you err on the side of safety. If you err, you hope that it does not cause much problem. So it is insensitive if you make a slight mistake in an input value or something. I think that it is valid.

MR. VOILAND: I think that it also has a specific meaning in statistics, but I am hard pressed to tell you what it is.

DR. STEINDLER: That is different though I think.

DR. STARMER: It does, but robust statistical

methods are able to make good predictions of things like central tendency and spread of a population from very small samples.

DR. CARTER: I was going to suggest that we have a contest between the staff and the committee. You guys say robust and we will say less filling.

(Laughter.)

DR. STARMER: Okay. I thought that it might be interesting just to talk a little bit about the sorts of things that we would look at when we look at a license application. At the end of the strategy was a comparison to the regulatory limits. There are limits in Part 61, or if EPA promulgates their standard or when they promulgate their standard, the EPA standards.

The sort of things that we would look at is the definition of the physical system that is involved, and that gets back to complexity. Did they look at the things in the system, the natural setting and the barriers, that are important to making predictions. And that is the first step in determining whether or not the analytical methods that they propose, or the models, and the codes then that would implement them are adequate. So that would be one thing.

DR. CARTER: Excuse me, if I could ask you a question.

DR. STARMER: Yes.

DR. CARTER: I notice that the numbers of course in 61 I guess coincidentally conform to 40 CFR 190 as far as the 25 to the whole body, and 75 to the thyroid, and 25 to any other organ, millirem per year.

I presume that you folks are tracking what EPA's low level waste standard will probably be, is that correct?

DR. STARMER: Yes.

DR. CARTER: How does that fit in with what you have got at the moment?

DR. STARMER: I personally am not tracking it. But I can tell you this. If the standard is different than the numbers in Part 61, we will conform the regulation. You are right. I was not involved in setting those limits at the time. But I know that there were a lot of discussions with EPA could you live with 25, and they said at the time that yes we can live with 25. It might be less, but it would not be so much less. So there was a lot of discussion at the time, and we are watching what they are doing now, but my group is not watching it right now.

We would look at their integration of the system and their subsystem models. How are they looking for example at partition of dose between different pathways. Are they getting the right kinds of outputs from one model say in concentration units and putting them correctly into say dose conversion models that might be used.

We would look at the basis for the selection of their model input, any model validation and verification. It might be better to say model calibration for example on data gathered at the site. And make sure that the models that they use have been verified. And make sure that they have applied the right models to the right problems.

Remember one of the things that we have asked Sandia to do and they have done is to look at which models and which types of models are appropriate for the different pathways. And we would be using information of the type gathered by Sandia and put down by Sandia to see that they have used the right types of models. This would be referred back for example to the physical system, do those models actually represent adequately the physical system.

I think that one of the most important things that we would be doing would be looking at the analysis of the uncertainties and the sensitivities of the modeling. Now some of this is if you will generically known. There was a study done I guess probably almost six years ago by Dames and Moore where they looked at the sensitivity of the systems model, the IMPACTS model, and various components of that and how they affect output.

But we certainly would be looking at uncertainties in input variables. And we would be looking at the sensitivities of the models to those as part of our

review. And then we would be looking at what the results were obviously, but also how they used the results.

And I think that this is an interesting thought. I think that it is something that staff has to come to grips with and something that we will probably be talking to you about. Take a case where there was a postulated say 24 millirem exposure for several years at some peak period say 1000 or 2000 years in the future. Now 24 millirem as a number is less than 25. And by definition, Mr. Surmeier would happily sign the license maybe.

I think that it goes back to looking at all of these other things and particularly looking at the uncertainties and sensitivities involved. And this is what we mean by looking at how you use those numbers to support your license application.

If you look at all of the scenarios, and if you look at all of the pathways, and all of the possibilities and ways that you could get into it, for example the air pathway, and come up with this particular way of disposing of waste. For some reason it has no potential for release to the air, and I predict a zero exposure. I am not sure how you demonstrate that. But if you came up with that, then I have a much better, if I can believe all of your rationale, I can have some reasonable assurance that you indeed have met for that particular pathway the 25 or the 75

millirem.

On the other hand, if you come up with 24, does that mean by definition that I give you a license. It may well mean that. But I think that it also means that that is something that you better have a very, very firm handle on. Or I may still give you a license, but I may have to limit your inventory for that particular radionuclide, or put special requirements on packaging, or do something that will give me some confidence that it is not 26, which obviously by the number is not acceptable.

DR. CARTER: Do you have the caveat in of as low as reasonably achievable?

DR. STARMER: We do, yes.

DR. CARTER: That probably answers your question.

DR. STARMER: I think that what we would be looking for is a demonstration of that. You know, that we have done everything including limiting inventory and it is still 24. It might be 26 or 28, but we have got analyses that say that it is that low. So you know, that is what we are trying to say. That we do not look for just a simple number against number necessarily. In those models, the numbers are probably somewhat questionable but I will give them some credit.

DR. CARTER: Sure. You are going to have some uncertainty in all of those numbers.

DR. STARMER: Right. But what we are trying to emphasize is that they are licensing tools and they are performance objectives, and we are looking for demonstration and we are looking for ALARA. So that is the sort of analysis.

DR. MOELLER: But you also say that they should estimate the maximum and minimum values for the dose estimates.

DR. STARMER: Right.

DR. MOELLER: Now what is the 24?

DR. STARMER: The 24 I would assume is a conservative best estimate.

DR. MOELLER: Okay.

DR. STARMER: And what I am going to say then is what we would be looking at would be those ranges.

DR. MOELLER: Sure.

DR. STARMER: What is an upper and why would it be that bad, what sorts of conditions lead to a non-performance if you will. And at that point, you have to make some decisions as to what to do about that.

This is not an really an overhead slide, but I will put it up just to make sure we are looking at the same slide. This is actually a list. I'll tell you what. We will look at some of the top ones and I'll side it. And this is not so much for discussion as specific models, but to just show you some of the sorts; first of all, the variety that Sandia identified and some of the sorts of models that we are dealing with.

I think the first groups there, the so-called infiltration water budget codes offer very useful example of the variability in the types of codes. Codes like CREANS, which is a USDA code; HELP, which is a code put together by the Corps of Engineers for EPA, are so-called water balanced or water budget that, our favorite word, there water budget. Water balance codes that look -- they basically are subtracting the rainfall flux from runoff flux and a few other things to come up with infiltration.

What you are dealing with are large numbers being subtracted from a large number to get usually a fairly small number. They are inherently difficult codes to really validate. But if you could use something which is more complex, not much, it's really a one-dimensional model, but it take into consideration some more mechanistic models of infiltration. For example, how the roots suck the water out and some of these sorts of things for transpiration, for

example, and it shows that you can use something like HELP or CREANS, and it is all right in that situation. Then you can go back to what's more simple, just a balance type of, mass balance type of code to do your infiltration analyses.

The BLT code here is the code that Brookhaven has put together. There is a leak code and container degradation code and a barrier code, which is a proprietary code by EPRI. You can see there is a lot of groundwater flow and transport codes. And they generally are fairly complex codes.

Of the codes that we are looking at, this is a code that is proprietary. It was developed for NRC. These are the codes that we have been basically using. There is no real reason not to use them. I have been told that SUTRA, for example, carries an awful lot of extra baggage in terms of being able to do a lot of calculations you wouldn't need for low-level waste. This code, in my opinion, is way overblown for a low-level waste site.

TRANSS and NEFTRAN are one-dimensional flow tube models. They are a little bit more -- they do transport and they are a little bit more complex than just concentration times a velocity gives a volume.

UNSAT2 is an unsaturated code. Again, surface water codes. These are codes that implement Reg. Guide 1.109 approaches. The air codes are accepted, XOQ, DOQ is

an NRC, it's actually chi over q, but is an NRC gaussian plume-type model. I'm not sure of the basis for these, but these are EPA codes which are accepted codes.

And these are some of the codes that are used for dosimetry and food chain. One that is not on here is one that's called MAXIE1. That's because this GENII code from PNL has sort of pushed -- it's a next generation of the MAXIE codes.

And again they are codes -- the GENII code is particularly interesting. There is 1800 pages of documentation in just one of the volumes on that code in terms of verification and showing that it actually does the calculation. It all was developed and benchmarked and tested under NQA-1 procedures. So that seems to be a code of the future. That's as PNL code.

But that's just to sort of give you an idea of sort of where we are. No decisions have been made on which of these codes are the ones that will be in the methodology. We are expecting a brief letter report and then a visit from our people at Sandia within about two weeks to talk about actual codes.

One of the things that is interesting, one of the things that they ran into was to some extent just mechanics of coupling these codes, getting the right output from one that would be appropriate for the other, and so they did

quite a bit of running of the codes. They did some sensitivity analysis and that sort of thing.

As I say, we expect a self-teaching curriculum in January. We will be doing some work to bring the integrated methodology up to speed by looking at operational and accidental releases through the rest of FY-90, and should at that point have a good sound basis on which we can improve as we find improved modules to put into the methodology.

DR. MOELLER: Okay, thank you.

I think if that completes it, we will open it to questions.

Let's see, Mel Carter.

MR. CARTER: Yes, I had one. Most of this, of course, is written in terms of position as far as performance assessment and so forth. And yet, in the summary there are a number of places where you talk about what you may do.

An example: It says NRC staff reviews may be supported by systems modeling to confirm the design performance of low-level waste disposal facility. And if determined prudent by NRC staff, the systems may such and such.

And I just wondered why you sort of switched to from what you know to what may happen. It's almost a

predictive thing.

DR. STARMER: Well, I would make one point. That if the licensee came in with what I would call an absolutely transparent license application in terms of performance assessment and we could follow every step, used all the codes, the inputs were correct, we might not necessarily do any calculation.

There is a misconception that you have to do a complete performance assessment, an independent one, to issue the license. I don't think that that's true, and that's why it says "may" use the systems code to do this.

And that's why when I started out I said we are doing this for our own purposes to do two things. One, is to make sure that we can do that sort of analysis. And two, is to indicate to other people what analyses should be done.

MR. CARTER: Why not take the tack though that if these conditions prevail, then we will do thus and so?

DR. STARMER: I don't know.

MR. CARTER: I guess you can put it in a different language, a little more positive rather than speculative. It could be interpreted almost as a threat. You know, you guys jump through all these hoops. When you finish those, if we aren't satisfied, we've got another set of them we will pull out of the drawer.

I'm not suggesting it was written that way, but

you can interpret it that way.

DR. STARMER: I guess -- I can see your point. I was just wondering -- it certainly was not meant as a threat of, you know, always bring me another rock no matter what you do.

As I said, if we got an acceptable, on-the-surface acceptable, and we can do some check calculations, simple, that showed that this is a good analysis, there really is no reason to do a lot of analysis, or to ask for any more analyses.

MR. CARTER: I don't disagree with the philosophy. Like I say, I would suggest you look at the way it's worded because I think it could be worded a little bit differently and leave out those --

DR. STARMER: Sure.

MR. CARTER: -- possible interpretations, or eliminate them.

The other thing I can't pass your earlier comment about budget austerity. I'm surprised that anyone would have the audacity these days when the budget is so damn high to talk about austerity without some kind of qualification.

DR. STARMER: Whose budget are you talking about? Not NRC's budget.

MR. CARTER: I think you have got to be pretty specific about that. I could be talking about my budget.

You mentioned austerity as far as federal budget, and the government might be --

DR. STARMER: Well, we have a division that was originally designed for 65 people, which is at 42 people now. And as I understand it, and I should let me management speak to this, I understand that there is no imminent increase in that.

MR. CARTER: No, no. If you are talking about your budget, then I have no problem with it.

DR. STARMER: I'm talking about what I'm giving to a job --

MR. CARTER: That's as probably a austere as you want to make it.

DR. MOELLER: Paul.

DR. POMEROY: I just had sort of an informational question with regard to we've dealt rather extensively here with the high-level waste performance assessment. And I wonder if you could tell me a little bit about the level of interaction between the low-level waste performance assessment group and the high-level waste performance assessment people, recognizing the differences, of course. And is that an appropriate level of interaction, if there is any?

DR. STARMER: Well, let's put it this way. First of all, they are on the same floor. I am probably

over on their side of the floor at least once a day talking to one of them or the other. At that level I think there is a lot of interaction.

I've provided their staff, Seth Coplen's staff with our staff thoughts on performance assessment. They are aware of the Sandia project One of the reasons for going to Sandia was to try to take advantage of what Sandia may have learned in their process of developing a methodology for high-level waste.

I think there probably are some matters of scale that are somewhat different, but a lot of the sorts of things that you have to consider are similar.

I think the big difference -- there are two big differences -- well, there are a lot of them, but two big ones: heat and total inventory of not absolutely monolithic type of waste, but a waste that is pretty easily characterized in terms of what it looks like. I'm not sure they can characterize it so easily as how it behaves under stress, but we have a very, very diverse source term to deal with, and we don't have deep geologic barriers, but then we don't have some of the problems that they come into in terms of long distance transport.

But we do talk and I am aware of what they are doing.

DR. POMEROY: Fine. Thank you.

DR. MOELLER: I had a few questions.

We this morning were looking at draft technical positions and then we heard about guidance documents or guidance letters and so forth. Well, see, this doesn't have any title at all. It's not a technical position. It's not a regulatory guide. It's not a guidance letter.

DR. STARMER: That's true.

DR. MOELLER: What is it?

DR. STARMER: It's a publication done by the three staff who at the time made up the entire performance assessment team, which was signed off on by our management. It is not a technical position. It is our thoughts on performance assessment. Any comments on that would be welcome, but it isn't a position.

Again, it's a matter of we don't have any real resources to do much more than that. Now a lot of that, by the way, was done by me and my staff on our own time. Not, you know, patting myself on the shoulder, but that's just the way things like that get done.

DR. MOELLER: Well, I think, though, it deserves to be dignified with some sort of classification.

MR. SURMEIER: This was provided to the DOE Low-Level Waste Management Conference of two years ago. I think it's a good document. I think NRC, Division of Waste Management, Low-Level Waste, thinks it's a good document.

But that was as far as it went with the limited time, and all three of the staff, including Dr. Starmer, really did this on their own time. Did not have time to budget it themselves, but thought that it was important. And it was invited paper.

DR. MOELLER: Do you have -- what are your plans for it?

And if you want comments from us, which I gather you do, meaning formal comments, what is it we are comment on and why are we commenting?

MR. SURMEIER: The document which was sent to you was provided to you as background information for this briefing. We would appreciate any comments you may have, but it really wasn't sent down here for you to critique it as much as for background information given the nature we think performance assessment and low-level waste is an area that we would like you to at least be aware of and understand where we are.

I think we have, even though we have a limited staff, I think we have a darn good staff in this area, and I think that Dr. Starmer has really spent a lot of time trying to pull together a good team and also good contractors along with Research.

So again, any comments you want to have on this session we would certainly appreciate, but the point is that

it was really to come down here and give you an understanding of where we are in performance assessment, because we know you have been briefed on high level, and there is a difference between the use of a probabilistic type and approach because of EPA's nature in the high-level area as opposed to the deterministic area that we were talking about here. So we just thought it would be useful for you to year.

DR. MOELLER: All right. On some other subject. Mel Carter has already mentioned the dose limits which are archaic at best. Some day EPA will move to ICRP or the risk-based approach for setting organ dose limits and the whole concept of effective dose equivalent.

You really are powerless, I guess, to do anything until they do that. But one thing you could do, and I don't know whether this committee is the right one to push it within NRC, but that is, to move to SI units.

I mean I have read the report that was issued a few weeks ago on how much it's going to cost for NRC to move over. But it stated that, in terms of low-level waste, that you hoped on a gradual basis to move over and you discussed the Agreement States and so forth.

But I would think you could move relatively quickly and I don't know enough about word processing, but maybe you could have all of the NRC word processors set up

so that when you put down 500 millirem, the machine would print out 5 milliceverts. I really believe we need to move to SI units, and you could give it in both forms, you know, English and SI. But I would like to see you move.

Now, this morning we covered the uranium mill tailings covers, or erosion protection. And there we were looking toward a minimum of 200 years and a maximum of 1,000 years. Well, in this document you are proposing to do estimates for "thousands" of years.

Why on uranium mill tailings do we only have to do it up to 1,000 and here we have to do it for thousands?

DR. STARMER: Because the regulation specifies 200 to 1,000 for mill tailings.

DR. MOELLER: Is that EPA now or Congress?

DR. STARMER: EPA standards.

DR. MOELLER: That's an EPA standard.

DR. STARMER: Neither the EPA standard for low-level waste, nor Part 61 specify a time. But I think you can use a reasonable approach to determine what that might be by looking at things like half-life, radio toxicity, potential for release and transport to come up with some reasonable approaches.

DR. MOELLER: Sure.

DR. STARMER: But there is no specified time.

DR. MOELLER: Okay. And the last item that I

have, and this again is one I mention every time, and it goes along with SI units.

And on page 6 you say, if I can find it.

(Pause.)

DR. MOELLER: No. Page 7 in the summary, Part 5. Your first, the performance assessment should consist of, number one: identification of defensible pathways and scenarios through which radionuclides may escape from the disposal facility or by which intruders could be exposed to radioactivity.

Now, there is no way in the world anybody can be exposed to radioactivity. That is like being exposed to humanity or something.

Radioactivity is a property of a radioactive material, or some radioactive materials, which in turn leads to the fact they are radioactive, and therefore they emit radiations.

So what you are really trying to protect the intruder against is exposure to radiation and/or radioactive materials.

Do we have other equally significant comments?

DR. STEINDLER: At least.

(Laughter)

DR. STEINDLER: Let me point out that the whole question of SI units does not generate unanimity. The Academy of Sciences', actually the NRC's, Committee on Nuclear and Radio Chemistry has said that SI units are desirable except for the curie and the rem and the rad that should be maintained.

DR. MOELLER: Excuse me for interrupting.

DR. STEINDLER: So take your pick.

DR. MOELLER: But that is not the prerogative of, who is it?

DR. STEINDLER: That is a recommendation.

DR. MOELLER: I mean, I think the groups that should make the decision on the rad and rem and so forth are the radiation protection, professional radiation protection societies, nationally and internationally. Here you have Mel Carter, with the President of ERPA, and former President of the Health Physics Society, and so forth. And those groups, you know, they haven't given it a second thought. They are miles down the road with SI units, all by themselves.

DR. CARTER: But I think if Albania can do it, we ought to be able to do it.

DR. STEINDLER: I think that's right.

(Laughter)

DR. STEINDLER: Let me get to a slightly different point.

Down on the bottom of Page 4 you indicate if, however, an applicant projects waste production for a state or compact different from the analyses used by the NRC staff to develop the waste classification requirements of 10 CFR 61, the applicant may need to consider new scenarios and

analyses to demonstrate compliance with the objectives of 61.42.

I assume here you are in fact talking about the Ford, Bacon, Davis study, the NUREG --

DR. STARMER: Pardon me?

DR. STEINDLER: -- the NUREG 580, the classification, the original classification document.

DR. STARMER: There's waste classification in 61.56, I believe it is, which gives you the Class A, B and C wastes. And if you read the statement of considerations, which we have been looking at, actually because of how long should you model, one of the statements there, and I think if you think about it, it is probably true, is that the analyses that were done made certain assumptions about amounts of waste that Class C, Class B and Class A.

DR. STEINDLER: And that is to be found, what I am saying is that that is to be found in NUREG 580, which was the original classification document, which ultimately led to Class A, B and C.

DR. STARMER: Okay. I see what you are getting at.

DR. STEINDLER: But you indicate here that if the state or the compact finds that its projections for waste production are different, then the scenarios have to be different.

What 580 did, as I'm sure you know, is it took a typical site and tried to derive, for the first time, and I think in that sense was an outstanding document, some method of classifying wastes into the classes. But it took a generic site, literally a generic site, single kind of exercise. And here you are saying if that generic site doesn't fit a particular site, you have to go through and re-examine all the scenarios.

It by definition isn't going to fit all the particular sites.

DR. STARMER: Again, the reference is to the classification system in Part 61, which is derived, at least the concept --

DR. STEINDLER: No, that is not what you say here. Let me read you again what you say.

DR. STARMER: I didn't refer to NUREG 580, though.

DR. STEINDLER: I know it. That is exactly my question.

DR. STARMER: I referred to the DEIS, FEIS --

DR. STEINDLER: No. Doesn't even do that.

DR. STARMER: -- and the classification system in 10 CFR Part 61, which sets certain limits on certain radionuclides.

I could give you an example, a real example. It

is a little bit off to one side. But let's make an assumption that all the low level waste from West Valley Demonstration Project, rather than being the responsibility of DOE, was to be disposed of in the New York State radioactive waste disposal facility. It is approximately 50,000 drums of material, with TRU contents on the order of 50 nanocuries per gram.

We have a classification system. This is a Class A waste, and does not need special treatment, if it is below 10 nanocuries per gram. It is Class C waste if it is less than 100 and greater than 10.

The assumption there was that there would be a small amount of commercial waste, and it is documented in the FEIS, only a small amount of waste that would have concentrations of transuranic waste or elements, in that range, only a small amount.

When they did the analyses, making that assumption, they found that, well, if it is contained, as Class C waste should be, then it will be acceptable for disposal, and we can accept small amounts.

Now, if I took 55,000 71-gallon drums, this is not what was expected in the, is not what was analyzed in Part 61, to show that this would be acceptable for near surface burial.

And it should really, then, be treated as a

special case and analyzed to show that indeed this can be safely disposed of. It is just a word of caution. Another example.

Much of the Class B waste is disposed of at Barnwell, comes from the Cintichem, Tuxedo plant in New York. And they are very hot, but they are a very specific type of waste. They are scattered all through the site, and analyses, I assume by the licensee for the State of South Carolina, have not indicated a problem.

But remember, that is through a very large site and it is a very small proportion of the total load that is being deposited at that site, the total radionuclide inventory.

Now, if that all goes to New York only, it somewhat changes the mix of radionuclides and amounts that was analyzed in Part 61 and should at least be given some extra attention to make sure that no problems develop because of that.

It turns out that both New England Nuclear and Cintichem are in New England. Now, if you had a New England compact, they would be asked to take much of the Class B waste from those types of facilities, that is, from the manufacturing facilities, that is generated in the country, which is now spread, all the New England Nuclear that I know of, a lot of tritium goes to Hanford, while the Cintichem

goes to Barnwell. It is sort of split and it is spread around the country.

So the generic analyses sort of made assumptions that the waste would be more or less evenly spread around the country. And the only thing that phrase was meant to bring out was that special situations may need special analyses.

DR. STEINDLER: I hear what you are saying. I am not sure that these sentences, at least as I read them, would have given me that same view.

DR. STARMER: I'm sorry. As you probably know, many times when you try to write something, one more sentence might have done it.

DR. STEINDLER: Yes. You're right.

DR. STARMER: But we are probably too close to the subject.

DR. STEINDLER: I think it goes back to Dade's point. And that is what do you expect to have done with this document? Is this going to be used as guidance for the various folks who are in charge of compact analyses?

DR. STARMER: It was effectively published a year ago. I thought it might be useful for you folks to see. We may have changed some of our thoughts at least slightly. I think it is a good, basic presentation of where we are. And that was why it was provided.

However, I appreciate your taking the time to read it and look at it and point out things like that, because the next time, if I need to explain that to somebody, I sort of had to run through it and we will certainly be sensitive to it if people start talking about what analyses do you need; what indicates the need for special analysis?

DR. MOELLER: Gene.

MR. VOILAND: Whether it is a formal document or not, it still is technical advice to somebody who is in this kind of a business. There is no question about that. I was, I think, very well impressed by the flow of the document and what it covers. It addresses technology which has been in existence for a long time, so you have had the benefit of looking at a lot of different kinds of approaches to performance assessment here. I think it certainly shows.

I guess in my own mind I have been wondering how much of this is applicable to the high level waste, which is really the question you asked a little earlier, Paul, because there is some good stuff here.

DR. STARMER: Again, my feeling is that the approach is to some extent similar. I think there are some complicating problems in a high-level waste repository. You are dealing with a much larger volume, longer distances, a much larger source term. Maybe, as I said, more

monolithic, more one type of material in a way. Yet, it has the added complication of heat, which when I was working in high-level waste, we always felt that if the heat wasn't there, a lot of the problems, I wouldn't say went away, but they were a lot easier to handle.

DR. MOELLER: One other question. Where did the 500 millirem, 5 millisevert limit for an intruder, what is the origin of that?

DR. STARMER: I am not absolutely familiar. But I believe the reasoning goes something like this: that we would accept that sort of, for an occupational exposure, for a brief exposure, you wouldn't want to see it but it would be acceptable.

DR. MOELLER: So that is not in EPA standard or in 10 CFR?

DR. STARMER: No. I really don't know if the new standard has an occupational, or not an occupational, an intruder --

DR. MOELLER: Sort of an accidental situation or injury?

DR. STARMER: Yes. You are trying to protect someone who doesn't know there is something there, and accidentally digs into it. Hopefully, rapidly realizes that there is something wrong or different or that he should try and figure out what this stuff is, maybe goes and looks up

records or something like that.

But it is to protect someone who doesn't know what they are doing and gets involved in this.

DR. MOELLER: And on that, it gets back to something I was mentioning earlier.

If you would go to the effective dose equivalent approach, you wouldn't have to say anything about organs or thyroids or anything. You would get one number.

DR. STARMER: My great proponent of that is sitting back here, Dr. Shum, who you have met.

DR. MOELLER: Yes.

DR. SHUM: Dr. Moeller, I think we have gone through this with our EPA. We really would like to see the 25 millirem be changed to effective whole body equivalent.

DR. MOELLER: Sure.

DR. SHUM: And now there is some reason that, according to EPA, for occupational exposure. We like to do it with the effective whole body dose equivalent. We were told from EPA that for environmental radiation standard, they would like to see it if they want to set a standard. They would like to consider ALARA themselves, to set that for NRC. Then NRC would like, why don't you give us 25 millirem, a single risk? We will consider ALARA ourselves.

EPA says no. We would like to set 25 millirem to the organ dose, that we will consider in doing our job.

So there is some argument back and forth. I can see us changing right now, as you see that the Clean Air Act, I was trying to persuade the EPA people, it is not very easy for NRC to demonstrate 25 millirem to the bone and lung dose.

Sometimes we cannot use a simple model. For example, we have to use environmental measurement even for a fuel cycle facility. On some of those environmental assessments, we have to add up to do uranium solubility, at the air particulate, and that affects the organ dose by a factor of 10 if you look at the dose conversion factor at ICRP 26 and 30.

And even we cannot afford the particle size. It is very tough to enforce the 25 millirem, one dose, which is a very small risk if you translate it into effective whole body equivalent.

So we would like to see that now it is changing, as you see that from the Clean Air Act, we express the idea that we cannot enforce 25 millirem bone dose or lung dose, as you know that in a certain time frame they change it to 25 millirem effective whole body equivalent and 75 millirem to any other organ.

So they somehow provide a uranium fuel cycle facility, a break by a factor of 3. But now, with this new EPA standard, we change from .03 to whatever, 10 millirem.

I think they are changing to the effective whole body equivalent.

And I also want to make a comment on that 25 millirem. Actually, we try to, at EPA, really, to change it on a 25 millirem. Now, the organ dose, I can have 25 millirem whole body and 25 millirem bone, lung, and still meet the standard.

But if you are summing it up, actually, it is higher than 25 millirem.

We would like to see them at least set it at 25 millirem effective whole body equivalent. And for the 10 CFR Part 61, it is not easy to demonstrate that low organ dose.

DR. MOELLER: Okay. Thank you. Do we have any other comments or questions on this topic?

(No response)

DR. MOELLER: I see none. Let me thank the staff for being here and meeting with us on this subject, and we will take a break then and resume at 3:15.

(Whereupon, a brief recess was taken.)

DR. MOELLER: The meeting will come to order.

The last topic for the day is a discussion of waste acceptance process for defense and West Valley waste, and Dr. Parry will introduce this subject for us.

DR. PARRY: I have circulated to you a copy of the memorandum that I believe is dated July 24th.

It's a cover letter from me to you all referencing and attaching a letter from Ralph Stein of DOE to Mr. Youngblood, Deputy Director of the High-Level Waste Division.

Members may remember that during their visit to Savannah River a question was raised at some length about the waste acceptance process. This is a somewhat awkward term for quality control or quality -- not assurance -- but quality control of the product of material coming from the defense waste processing facility, DWPF.

I first became involved with this about 12 years ago with Dr. Ed Hendley who was project manager for the DWPF -- what became the DWPF. The basic question was: how should sampling be done of the product material? Should it be sampled on a regular routine basis? Perhaps twice for each canister as it was produced? Or should the control of the product be determined by careful process control as opposed to product sampling and retention of the samples for background of history for historical purposes?

This was a longstanding discussion between he and I. I was interested last year to see that it was still a matter of some discussion. And as you may remember during the presentation in January the State of New York and DOE, again, referenced the question of the waste acceptance process.

So when I saw this letter from Mr. Stein I wanted to bring it to your attention. Mr. Rick Weller who is section leader for materials in the division will address, I assume, both the staff's interpretation of Mr. Stein's letter and possible staff's response.

MR. WELLER: I can get to that, Jack.

Actually our presentation is a bit broader than just the focused issue and your note for the ACNW. So why don't I run through an overview of the WAP process first.

DR. MOELLER: Can you shorten the length on that microphone.

Thank you.

MR. WELLER: Why don't I go through an overview of the WAP process because I do touch on those issues in terms of the acceptance criteria for regulatory requirements.

(Slides being shown.)

MR. WELLER: As I mentioned this is going to be an overview of the waste acceptance process. Let me give

you a little bit of the history first.

The genesis of this was a letter from DOE in August of 1985 which described a waste acceptance process. And even though there are interactions and correspondence that predate that, this is really the genesis of the term and the formalization of the waste acceptance process.

Its purpose is really two-fold. It formalizes the waste acceptance activities within the DOE office of Civilian Radioactive Waste Management. And its other purpose is to ensure the acceptability of the waste form generated at the vitrification facilities at any reactor, any repository site, rather.

As you can see, since 1985 the focus of the WAP has narrowed considerably. It was originally applicable to all repository sites. And in 1986 three sites were approved for characterization at Yucca Mountain, Deaf Smith and Hanford.

And then the amendment to the NWPA in December 1987 further narrowed the focus to just the characterization of Yucca Mountain.

So it really simplifies the WAP focus and the related issues considerably.

Now, in order to fully understand the waste acceptance process I think you have to have an understanding of the related documents that the DOE will generate and that

the NRC staff will be involved in, in review.

The first document in that chain is a document called "The Waste Acceptance Preliminary Specifications" or WAPS. That document will specify the properties and requirements for high-level waste forms produced at DWPF and West Valley and at Hanford, presumably, and out at Idaho in the future.

It's primarily rule driven. You will see specifications in there which are intended to satisfy the performance objectives of 60.113 and the design criteria for the waste package in 61.35. You will see canister specifications for size, length, et cetera, chemical stability, subcriticality and things like that, and even transportation in the future. It will address Part 71.

I do have a copy of those specifications I was going to pass around just to give you some familiarity about the content if you have never seen it before.

The next document in this chain is a document called "The Waste Form Compliance Plan" or WCP. It describes the methods and programs for demonstrating compliance with each of the specifications in the WAPS. So this tells -- this describes how they will satisfy each of the specifications that they have generated in the WAPS.

DR. MOELLER: Now, excuse me. What is the nature of each of these items like the first one, the waste

acceptance preliminary specifications; is that a technical position?

MR. WELLER: This is a DOE generated document.

DR. MOELLER: DOE generated document.

MR. WELLER: Yes. It's their specification for the waste form.

DR. MOELLER: Okay.

MR. WELLER: And as I mentioned earlier, the overall purpose is to onsure that the waste form that they produce will be acceptable at a repository site, recognizing that they intend to generate glass to produce waste forms prior to even the completion of the design of the waste package, the entire waste package.

DR. MOELLER: And the WCP is also a DOE document?

MR. WELLER: Yes. Yes, it is.

DR. MOELLER: Okay. All of them.

DR. PARRY: Have both those documents been approved by the NRC staff and accepted?

MR. WELLER: No, we've had some involvement in the past in that the DOE has provided to the staff draft copies which we have commented on. And the last submittal, for example, for the DWPF was in September of 1988. We're expecting shortly corresponding submittals of both the WAPS and the latest waste compliance plan for West Valley this month or shortly.

The next document in the chain is a -- actually it's a series of documents. This is really a report package in that there are nine, ten, or a dozen or so reports; it's called a waste qualification report or WQR. And that document will publish the results of the test and analyses that were conducted under the waste and described under the waste compliance plan.

Another document that -- or product that we have asked for from DOE is a process control program which will define the parametric bounds of the vitrification process. That is, the ratios of high-level waste to glass frit, to any chemical addition to change Ph or whatever, to ensure a high quality product.

This is really a QA issue here, but it's something I know Dr. Moeller and others are familiar with. It's something that has been utilized and been a necessity for all the low-level waste producers at reactor sites for a long time. So it's certainly applicable here.

DR. STEINDLER: I'm sorry, you indicated that that was a QA issue, did you mean that?

MR. WELLER: It's QA in the sense that the intent is to produce a consistent high quality product.

DR. STEINDLER: Okay. That can be applied to the whole world. The process -- it seems to me that the process control program is a series of, in effect, tech specs

equivalent on the operation of the glass plant.

MR. WELLER: Yes I kind of view it as a recipe that you will use when you generate that waste to ensure a high quality product.

DR. STEINDLER: But it's got only a limited relationship to what is currently the quality assurance program.

DR. MOELLER: To help me because when I first looked at this it was really not on what I -- it didn't emphasize the aspects that I thought it would.

The first basic thing you have to have is to show that if you have good QA that the process itself is capable of producing a product that is acceptable. And somebody has to have decided whether that product, if placed in a repository, will provide the proper barriers to releases and so forth that are needed.

Who is doing that or is that all in here, also?

MR. WELLER: Well, the waste acceptance process, its purpose is really kind of narrow. Primarily to ensure that whatever product they produce in a stainless steel core canister will be satisfactory for emplacement at any repository site.

DR. MOELLER: But before -- and good QA will assure uniformity and high quality in the product that's produced. But somebody has to have said, you know, 20

percent frit and so much in temperature, et cetera, is capable of giving us an acceptable product.

MR. WELLER: That's right. In all of their cold tests that they will conduct and provide in the results of these reports will provide the process control program. As a matter of fact, they will put the PCC in one of these waste qualification reports.

DR. MOELLER: Mr. Bunting.

MR. BUNTING: The same kind of questions you have we have sort of reflected over here on chart six, we'll get there shortly.

DR. MOELLER: All right, I'll wait.

MR. BUNTING: What he's trying to lay out for you here is how DOE advertises these things to be as opposed to whether we accept them on their merits or not.

MR. WELLER: Now, one of the last things that we've asked for out of the waste acceptance process is that DOE conduct a preliminary performance assessment and we're calling it preliminary because we recognize that they will, if they stick to present schedules, they will produce these glass forms well before even the license application is submitted. And well before the waste package design is completed.

So we don't want them to just view the glass production with tunnel vision; we want them to think about

the context in which that glass form has to go into.

So whatever characterization has been done at the time. Whatever advances in waste package had been done at the time. And whatever other information they had to conduct that performance assessment, we want them to do something which will give us greater confidence that indeed that glass waste form will be satisfactory for a repository.

Let me just go over a quick chronology of significant WAP interactions that we have had since it was formalized back in 1995. As I mentioned earlier, the correspondence and interactions actually date back to 1982 and open issues were generated back then but it wasn't called the waste acceptance process. The WAP was formalized in August of '85.

There have been a number of what we call technical exchange meetings: one in July of '86 which discusses the waste acceptance process; and the waste acceptance preliminary specifications for both West Valley and Savannah River, draft specifications we had at the time.

The rest of these technical exchanges have been preliminary to discuss the status of activities and progress at both those sites, December '86, February '87, April '87, February '88, and the last big one in September of '88 for DWPF.

Generally we've had about one technical exchange

per year; that's about how they have been running.

DR. MOELLER: And you did not meet in this calendar year?

MR. WELLER: There have been planning meetings but no, what I would call technical exchanges.

The significant correspondence that has been generated out of the WAP beginning with their issuance of the description of the WAP in August of '85, NRC provided comments on that document.

In May of '86 we provided a review of the draft waste acceptance preliminary specifications for both DWPF and West Valley.

In September of last year I mentioned that DOE issued a revised WAPS and WCP for the defense waste processing facility.

On October of last year we issued a list of all WAP open items. We had a smaller meeting at West Valley in September of last year and what we recognize is that nobody really had a complete understanding of what open items were out there and had been, you know, accruing since 1982.

So one of the tasks out of that meeting was for us to sit down and provide a list of all the open items we're aware of and we asked the DOE to do the same thing. So we issued our list in October and a week or so later the DOE issued their list.

In December of last year we requested an integrated schedule of all WAP related activities. Let me explain that. In the fall, I believe in the October '88 time frame, DOE requested that we review the latest draft WAPS and WCP for DWPF. And that was about the time frame in which we were gearing up for the SCP. So we wanted to know, you know, we had to plan for resource allocation, so we wanted the WAP activities integrated into the repository activities. So that was one reason we wanted an integrated schedule developed.

And I brought down a number of copies. It was too small to make a viewgraph out of, but let me pass those around, just to give you a sense of whatever else goes into that integrated schedule.

MR. WELLER: In April of this year, DOE integrated the schedule of WAP Activities. And In August of this year, DOE published a consolidated list of both our open items and their open items, and they put them into one package.

DR. MOELLER: Was there pretty much agreement on the open items?

MR. WELLER: I think so. There was a lot of combination of, you know, some open items have been kind of carried forth over the years and were repeated in subsequent correspondence. We've listed everything to start with, just to be complete. And the DOE also put them in subject brackets. So it is better organization rather than just a chronological listing of everything that has been generated since 1982.

Inasmuch as that integrated schedule is hard to read, I picked out some of the near term deliverables or meeting milestones from that integrated schedule.

In August, as I mentioned, DOE provided the consolidated open items list and we have received that. They also have on the current schedule revised WAPS for West Valley and revised WCP for West Valley. We have not yet received either of those.

There was also to be a meeting on waste form performance assessment. And we have not really had any

further planning discussions with DOE to settle on a date for that meeting.

In November on that schedule we are expecting perhaps a package of waste qualification reports for the Defense waste processing facility, and also in November a process control document for the DWPF.

Let me talk a little bit about general staff concerns, because I think they relate in part to some of the concerns that the ACNW has.

One of the concerns is that there is the potential for an adverse interaction between the waste form product and the container, inasmuch as the DOE has already chosen a medal that is going to be an austenitic stainless steel. And there are other types of metals, for example, the copper family, that are under consideration for waste package. So there is at least the potential there for some kind of galvanic or adverse interaction. That is something the DOE will have to look at.

We are also concerned about how well the glass will actually perform in the waste package environment and that is one reason why we asked for the preliminary performance assessment.

DR. STEINDLER: Excuse me. You say the potential for adverse interaction between the waste form and the container?

MR. WELLER: When I say waste form, that really includes the pour canister, even thought he DOE kind of ignores it.

DR. STEINDLER: Oh. Okay.

MR. WELLER: They ignore it to the extent that they don't assign any performance allocation to the forecast, or since it is heat stressed.

DR. STEINDLER: Right. So you are really worried about the stainless steel copier issue and the presence of some kind of an electrolyte.

MR. WELLER: Yes. That's our concern.

One of the positions that the staff has taken in prior meetings is that the vitrification systems design at West Valley or Savannah River must be designed for hot sampling capability.

We are not going to put our full faith or complete faith and trust in just the process control program. We want those facilities to be designed for hot sampling so that they can go back and conduct leach tests and do the same kinds of tests that they do on all the simulators, the cold tests that they run.

DR. STEINDLER: Why did you take that position?

MR. WELLER: Primarily because I don't think you can place complete faith in a process control program. There have been some relatively bad experiences with cement

on the low level waste side. Cement swelling and things like that. And those were all processed with process control programs that should have worked. And for some reason they didn't.

DR. STEINDLER: Well, we looked into that problem I think from time to time here and I would certainly not share the same level of enthusiasm about the process control, the quality of the process control program in the cement manufacture in comparison to what I think is going to be done at West Valley and Savannah River.

The operation of a hot cell or a hot canyon in a facility that size and magnitude and the requirements for, if nothing else, control of materials, seems to me strikingly different.

The consequences of requiring a hot sample are not so simply dismissed as simply laying it on the board as well, you know, you guys ought to be able to sample this thing hot in some fashion or another.

It isn't at all clear to me, at least I have not seen a reasonably presented staff position on this. Do you intend, does the staff intend to turn out a rationalization for that position in some fashion or another?

MR. WELLER: Well, the rationalization is that --

DR. STEINDLER: I mean, you can say that you

don't have faith. On the other hand, you can say that about almost anything.

MR. WELLER: Yes. We recognize that there is difficulty in even establishing homogeneity in a huge tank. It may have sludge that is difficult in the bottom to sparge or mix.

You are going to have some variation in your enfluent. That is going to result in differences in the product.

DR. CARTER: Let me follow up with a couple of questions.

I'm like Dr. Steindler now. I guess it would appear to me that the decision is being made without data. That is the first thing.

I presume we have some analogs from other countries that are producing more silicate glass. That is one possibility. I am sure we have had cold runs in this country. And I would presume there will be test runs before the process is put into operation.

I guess my question primarily is what variability in the characteristics of the glass are going to be acceptable, because I am like you. I am sure there is going to be some variability.

MR. WELLER: That is one of the concerns that, one of the specific concerns that we have identified down

here.

Yes. What is the variability allowed in the test results. In other words, if they, let's say they make a dozen runs and they conduct leach tests, and they get numbers all over the place. What is the acceptance criteria?

DR. CARTER: Let's say they get numbers fairly close together. Are those numbers going to be acceptable or not?

It would appear to me you could run this thing a while and then make that determination, whether it is going to be necessary to do two per canister or some other thing, because two tests of that sort I am sure is quite an undertaking.

MR. WELLER: We have not specified the frequency of sampling. That is an open issue.

But we certainly want the capability designed into the system for hot sampling.

DR. STEINDLER: Well, you have repeated the point that you made before. But I am not sure I understand the rationale.

I assume you are familiar with the document that West Valley turned out, if memory serves -- they turned it out fairly recently -- in which they have looked at the very question that you addressed I think at the bottom of that

slide. And the answer is that if you control the process, you can control the product.

So the issue of how do you determine what the product looks like and how it will behave can then be pushed back readily and that is a matter of, I think, experimental verification, and I assume that is do-able both at West Valley as well as Savannah River, back into the process parameter control. Once that is, you know, if you agree to that, then your staff position that you need hot sampling capability becomes even less defensible.

I could ask a whole series of questions. I won't. But I could ask a whole series of questions of where do you want samples and how do you know that your sample taken today in a one-ton run is going to be representative -- I assume that the samples are of modest size -- representative of that whole -- it isn't one ton, it's three tons -- three ton run? Et cetera, et cetera.

I think it would be worthwhile, although I am not in the position of suggesting that you do more work and I would not want to do that, but I think it certainly would be worthwhile for us to understand the fundamental rationale that allowed you to come to the conclusion that you must have hot sampling.

MR. WELLER: Primarily it is because we viewed this as a somewhat complex process. There is chemical

addition. There are going to be adjustments to Ph. There is going to be variation in the enfluent. In fact, if my memory serves me correctly, I think that even in cold runs, the DOE produced an outlier which did not have the characteristics of the glass.

DR. STEINDLER: I assume by hot sampling you are talking about glass?

MR. WELLER: Yes, actual product.

DR. STEINDLER: That's a product, not, you know, there is obviously sampling of feed and fret and whatever else.

MR. BUNTING: It is before my time, but it was my understanding in talking to others at the division that one of the reasons for insisting on the sampling capability being built into the plant was the fact that they were going ahead with the plant, and a lot of these analyses that you just referred to were not available at that time.

So it wasn't to say that we are going to insist that we have sampling throughout this entire process while it is going on. But not having the data to prove, to be able to see that the process, as you indicated, if you control the process, you control the product. And I have it at evidence, if they were going to go ahead with the design of the plant, then our compromise was build in a facility where you can at least sample something.

That is not meant to say that the position would be that you have to have the sampling program going on continuously. We haven't gotten to that point yet. We don't want you to interpret it that way.

MR. WELLER: Dr. Steindler, we also kind of view it as a verification program, and in a sense it is kind of a test that you are producing in the kind of quality product that you think you have produced, just on the basis of a PCP.

And I don't know of any other way to do that.

DR. STEINDLER: Well, I sound like I am arguing against motherhood and the Flag, in the technical domain. And I don't want to give you that impression. I'm in favor of motherhood and the Flag.

But the thing that I have trouble with is the blanket staff position, without what appears to me to be at least a visible underlying rationale that has been analyzed in depth. That staff position is likely to have a significant impact on the applicant, and the kind of provisions they have to make, both at West Valley and presumably coming up at Hanford, and whatever they have done at Savannah River.

MR. YOUNGBLOOD: Would you recommend that they go ahead and build the system and produce all of the glass without any sampling capability?

DR. STEINDLER: I am not in the business of recommending how Savannah River runs its operation.

But my view deals entirely with the rationale behind the staff position. If the staff says look, we need hot sampling because we have reasonable cause to believe that the product variability is undetectable by process control, then my answer is, fine.

MR. YOUNGBLOOD: We have not said they need hot sampling. But we said if they are going to build a plant or start running it before they come in for a license application, they probably should think about the capability of being able to do that.

But once they get it hot, they can't go back and make those type of changes to it.

If they can justify it not having sampling, that is a prerogative that they have. It is at their risk that they are doing these things. But West Valley is apt to be through with their 270 or 80 logs before they ever submit a license application. And they are going to have to be able to say that those 250, 300 logs out there meet whatever criteria they are going to take credit for.

DR. STEINDLER: Yes, that's true.

MR. YOUNGBLOOD: And when we asked them, well, if instead of 250 logs you make 500 logs, does that change the ability of the glass to perform certain requirements? And

they said well, we don't have any idea what impact that has on the ability of the glass to withstand certain leaching criteria and other physical properties. So without that information, we can't lay anything specifically on them until they come up with what they are going to take the credit on.

DR. SIEINDLER: I think I've said all I need to say. I was looking for some kind of a staff rationale that says hot sampling requirements, as you say, must be designed for. My question was why. It remains a question.

Go ahead.

MR. WELLER: I'll go on, then.

DR. MOELLER: Gene. Excuse me. Another question.

MR. VOILAND: I would just like to follow up on Dr. Carter's comment here. The French have been solidifying glass for really, waste, for quite a while. I guess it would be interesting what their experience has been and whether they sample and what kind of methods.

There was a waste solidification demonstration program at Hanford that went on for a number of years and I think they had onstream sampling capability. I don't think it is a big thing to incorporate it. But I believe also that it is almost impossible afterward, or very difficult.

MR. WELLER: If you don't design it in in the

beginning, I don't think you ever will, because the canyon area is going to be virtually inaccessible.

DR. CARTER: Are there any data from the French and others that are available on process control versus sampling of the borasilicate glass?

MR. WELLER: No. I can only relate to my experience with low-level waste products which I always viewed as being less complex. There was, for example, there was an effort up at Three-Mile Island to solidify some of the waste that was collected in the aux building up there. And they got a number of vendors onsite to make, they made up a bunch of simulated wastes with dust, and dust in the road and cement dust and a bunch of other stuff, and had the various vendors come in and solidity that stuff. And they cut all these things open and about half of them did not make good products that they thought they would make in their little test tube samples.

And there have been other wide experiences, the cement that swelled, et cetera, that I mentioned earlier, that presumably were made with a process control program that for some reason didn't work. We learned something new out of the process.

DR. CARTER: Well, I think that is a bad analogy myself. I don't think you can compare the two. I think it is an apples and oranges kind of thing. But I daresay some

of the other countries that have been doing this, it is likely that they have some of this sort of information that might be available.

It sounds to me like you have not really looked into that aspect of it. Is that a fair assumption?

MR. WELLER: From a programmatic viewpoint, what that tells me is that you cannot rely completely on a process control program. That is the simple conclusion that I draw from that, whether it is a good analogy or not.

MR. PARRY: Rick, has either DOE or the NRC staff visited the French facilities or researched, I'm sure, numerous papers that the French have put out in the area, that look at consistency of the product?

MR. WELLER: No. I can't say that we have done anything in that area.

MR. PARRY: I might mention as an aside, one reason that originally, before hot sampling was conceived of, was to maintain an awareness of the condition of the condition of the processing equipment, so that if the refractory started breaking down, we could start picking it up in the sample. But that was a prior thought.

MR. WELLER: Let me go on with the viewgraph then.

When we get into the details of the waste acceptance process and start reviewing in detail the waste

acceptance documentation, some of the questions that we were going to be asking, what are the definitive interface specifications for the waste form which flew down from the EBS specification, this relates to that marriage, that the waste form, including its support canister, have to make, ultimately with the waste canister that will be described in the license application.

What is the waste form system design qualification specification, and how will it be demonstrated?

What is the waste form acceptance test specification, and how will it be demonstrated?

What are the requirements for test reproducibility? For example, leach testing of simulated glass waste and the resultant statistical validity? In other words, how many cold runs should they make in a sampling regime?

What is the variability allowed in test results and how will it be ascertained and controlled?

Let me discuss now the waste form requirements, because I think this really gets to the essence of the issue that Jack had in his note.

The waste form requirements are embodied in 60.135, entitled "Criteria for the Waste Package and its Components."

And they state essentially that the waste form cannot have or should not have any explosive power fork or chemically reactive materials which might compromise the performance objectives of 60.113.

It should have no free liquids which can also compromise those performance objectives.

It shall be in solid form. In other words, you can't take zeolite resin and just throw it into a canister and satisfy these requirements.

Particulate material will be consolidated, like zeolite resin or some of the material they are using up there at West Valley to concentrate and volume reduce the wastes that they have up there.

There cannot be any combustible material which will compromise performance.

And this might be a good place to discuss the ACNW concern, because note that these are primarily design criteria. Let me mention that there is no waste form performance criteria per se. And I guess I am a little bit surprised to hear that that was a concern to the folks at Savannah River because the DOE has assigned allocation to the waste form. And indeed, that allocation is what those folks should be designing that waste form product to.

Not having waste form, specific waste form performance criteria gives to you a greater flexibility in

waste package design. If they don't want to assign any allocation for performance to the glass, they don't have to. They can put all their eggs in the other components in that multicomponent system of the waste package.

Sop if they are complaining about not having waste form performance criteria, I guess I am a little bit surprised because it would reduce the flexibility that they have in waste package design.

DR. STEINDLER: How do you interpret that first one about no chemically reactive materials which compromise performance objectives? Or in fact, two questions.

For example, in the case of UO2, UO2 is certainly chemically reactive. The product of the oxidation would certainly give you a material whose performance is significantly different than the UO2.

Would NRC interpret that as being, spent fuel being a chemically reactive material in that context?

MR. WELLER: I think that is a good example because if you simply put the spent fuel in the container, without an inert environment, it can go through oxidation, over a period of years.

And if that affects performance, and I'm not saying that it does, but if that affects performance, then the DOE will have to consider that in the design.

As it turns out, they are planning on putting

argon, I believe, in the container, as an inert environment, perhaps for that purpose.

DR. STEINDLER: That's true even if there is no particular performance objective assigned to the waste form.

MR. WELLER: The real key is if compromises or affects performance.

In other words, if say U308 is a product which has an adverse effect on a container in some fashion, and affects the performance objectives, then that design would have to be considered, in light of these requirements.

MR. BUNTING: The performance you were talking about is the performance of the waste package itself, not the waste form.

This would be an inconsistency between the waste form and the waste package.

DR. STEINDLER: Okay.

MR. WELLER: Let me also talk about FY '90 staff resources for the waste acceptance process because I think this, too, relates to the note from Jack and also the letter from, the recent letter from DOE which somewhat emasculated our environment in the waste acceptance process.

There was a planning meeting which I didn't attend -- and I don't think you attended, either, did you, Joe? -- in which we were simply going to tell DOE that fiscal year 1990 resources for involvement in the WAP were

limited, and somehow there was a misinterpretation about that. And we have recently issued another letter or another memo -- another letter, rather -- which explains that we will get involved, as much involved as we want, and those would be management decisions, in the WAP process, as we determine. And we will have to see what other programmatic developments there are in the DOE program, because as everybody knows, it is in a state of flux right now.

So the point we want to make is that money in dollars that we would like to have had for FY '90 were not forthcoming in the budget process. We are certainly going to read and review to some extent everything that the DOE submits, including WAPS, and waste compliance plan, other WAP-related documents, and that we will reallocate as management determines resources, to get as involved as we want to in the WAP process.

DR. STEINDLER: We have a copy of a letter from I guess Ralph Stein to Youngblood, this thing that was referred to, dated in June, in which Stein says that on the basis of a meeting you folks must have had on April 20, his staff was informed that the NRC does not plan to review the technical documentation, but rather, limits its review to the QA documentation.

Is that a correct statement as of today?

MR. WELLER: That was not the intended message

out of that meeting.

DR. STEINDLER: Thank you very much. That helps a lot.

MR. WELLER: If we don't review the WAP-related documents that I described earlier, we are not involved in the process.

DR. STEINDLER: Yes. Okay.

MR. WELLER: You have to review those documents to be involved.

Because as you can see from the waste acceptance specifications I passed around, there is very detailed technical information in that document.

DR. STEINDLER: You are preaching to the choir.

MR. WELLER: Including specifications intended to meet release rate limits in 61.13. That's really the essence of the WAP. If we don't review those, we're not involved in the WAP.

MR. PARRY: Has the essence of your comments been transmitted to DOE?

MR. WELLER: Yes, in a recent memo. Recent letter, rather.

MR. WELLER: We essentially explained that we can get involved as much as management really wants to, depending on available resources.

And that was only for fiscal year 1990. So

inasmuch as the schedules for hot operations are well out into the 1990s for both DWPFN and West Valley, I'm not sure there is great harm in limiting the oversight in any one fiscal year. But that will have to be factored into that integrated schedule.

And the last viewgraph that I did make up did deal with the ACNW comment to Chairman [illegible]ch, identifying the need for acceptance criteria for vitrified high level waste including the testing procedures to demonstrate conformance with that criteria.

I guess I never looked at this as identifying a need for waste form performance criteria, for the reasons that I stated earlier, that that is not a part of Part 60 right now. That would involve rulemaking, and that would also reduce the flexibility that the DOE has in waste package design.

Let me give you an example. If their leach test, for example, of the glass products that they are intending to make with borasilicate glass indicate that the performance was poor, that the matrix dissolves, et cetera, and they want to reduce the performance allocation for that glass, they have the flexibility to do so. And if they want to compensate by boosting the allocation of the container or some other component of the multicomponent waste package system, they have the flexibility to do that. And that

flexibility in design was always intended.

DR. STEINDLER: On the other side of the coin, having assigned a specific performance, allocated a specific performance to the waste forms, one would assume that you would be interested in knowing whether or not the waste form in fact performs in that fashion.

MR. WELLER: Yes. But as I mentioned earlier, inasmuch as the --

DR. STEINDLER: Whether it is in the regulation or not explicitly. Right?

MR. WELLER: Yes.

As I mentioned, the DOE has assigned allocation to the glass. So as far as I am concerned, I would view that as the criteria the glass factory should be addressing their product to.

And I will tell you that they have concerns about whether they can satisfy that rather stringent criterion. It is 1 part in 100,000 per year. In essence, it is the release rate limit of Part 113 by itself, imposed on the glass.

And the producers of that product will tell you they are not sure they can meet that. And we are not going to hold them to it if they can't. They have flexibility and design.

DR. MOELLER: When you say you are not going to

hold them to it, you mean they can make up for it?

MR. WELLER: When I say that, we're not going to say hey, you put 1 part in 100,000 in the SCP, we are going to nail you to the wall with that requirement. We're not going to do that.

This is an iterative process. Waste package design is an iterative process. And if they find out through their research and testing program that the glass is not going to perform, like they think, let's say it can only meet 1 part in 100 or 1 part in 1,000, they have the flexibility in their waste package development program to reduce the allocation over here and perhaps boost it over here with some other component.

DR. MOELLER: Again, do we have any experience from in France or anywhere on how successful they have been?

MR. WELLER: I'm really not all that familiar with the French program, I'll be honest with you.

MR. YOUNGBLOOD: They have not told us how compatible their program or how the designs vary from the French programs. They are still working on those things. So regardless of what France did, what DOE is doing might be something different.

DR. CARTER: I think a big difference, of course, the French as far as I know have been making borasicate glass out of their high level waste for what, roughly ten

years? They started in 1978?

DR. MOELLER: That's why I'm asking, just out of curiosity, what their experience had been.

DR. STEINDLER: Well, leach rate data for French glass had been published. The issue before the house here though is are those data of any use to us? I think they are MCC-1 tests which means that they are basically in deionized water, and the issue internally within the high level waste community in this country is, is that test worth anything when you talk about a repository, or is it in fact what it was designed to be, namely, a screening test of various compositions of glass to see which one is best or worst?

DR. CARTER: I think the other thing, Marty, is what is their experience totally?

DR. STEINDLER: Yes.

DR. CARTER: In other words, have they had experience with process control as far as the product, and these sorts of things?

And like I say, they have certainly had 10 or 11 years of experience. And if we don't know what that experience is, it is obviously is not going to help us any.

DR. STEINDLER: I think Joe is right. They have not really as far as I know, published a great deal of their detailed operating. I mean, we know they make a lot of glass. But I've not seen any data that indicates changing

glass attributes is a function of process condition change or chemical composition change, or the things which in fact were addressed, I believe, by both the recent West Valley report, which addressed the very thing that you were talking about.

I think that is a worthwhile report for us to have a look at. And I think somewhere buried in the Savannah River documentation there are attempts to look at compositional variations as they influence the property of the glass, which is really what you are looking for.

Then they are going to feed that back into the process, which is a kind of a standard approach.

MR. WELLER: That's all the viewgraphs that I have.

DR. STEINDLER: What about the lack of resources, and I don't know whether this is the kind of meeting in which one ought to be doing this, but what was the requested dollars and how does it compare to the dollars received for fiscal year 1990?

MR. WELLER: I'll tell you how it fell out. It came out as an over budget item, along with some others that we were interested in doing. But one of those other items, important items, we thought, assessment methodology development was another one that was somewhat cut out of the budget.

I guess the total staff in the division is what, around 63, 64, in the entire High-Level Waste Division? The materials staff is small right now, a section of about three people, although we are intending to add to that a little bit.

It turns out the waste acceptance process was unbudgeted for 1989 and in previous years, and we still had those involvements. So I'm not sure how significant it is to have something that is not budgeted.

DR. STEINDLER: What size effort, in terms of number of staff or some other unit that you would care to use, would you think is required to reasonably track and react to DOE's activities in this area?

Is that an unfair question?

MR. BUNTING: Not a bit. But I want to qualify what I am going to state. It is purely a reactive kind of an effort.

DR. STEINDLER: Yes, I hear you.

MR. BUNTING: Okay. It doesn't involve going out and looking at what the French are doing, using other data bases, but basically, by the seat of your pants, what capability you have.

We talked about something on the order of $120,000 to $150,000 of technical assistance, and within our own staff, on the order of 35 to 40 staff weeks.

DR. STEINDLER: Thank you.

MR. BUNTING: Based on what we saw on the schedule that they would be coming in with, those documents that Rick mentioned to you.

DR. STEINDLER: That helps. That helps a lot. Thank you.

DR. MOELLER: Other comments?

DR. CARTER: I would just like to point out that some people in the NRC believe that small is beautiful.

DR. STEINDLER: Yes. But not vanishingly small.

DR. MOELLER: What do you need from us on this particular topic? Were you looking for a letter this time or is it mainly informational?

MR. PARRY: Basically, responding to us.

DR. STEINDLER: For the briefing. Okay.

DR. MOELLER: For the briefing.

MR. WELLER: Part of this briefing was intended to address the ACNW comment, because the kind of acceptance criteria that I thought you were getting at was at least in part addressed in the waste acceptance preliminary specifications, because there a lot of detailed specifications in that document, and there was a lot of acceptance criteria. And the waste compliance plan describes how they intend to satisfy that criteria.

So those parts have been developed. And I guess

in that sense maybe I misread your comment to Chairman Zech, because I did not realize at the time that it was more focused towards waste form acceptance criteria rather than design criteria.

DR. MOELLER: I think your response has been very good. You have looked at a broad picture and we should hear about it. I have no problem with your presentation.

DR. STEINDLER: Let me ask another question that was brought up in the document that we have.

Is it the staff's view or is it your view that for example West Valley or for that matter, Savannah River, should be required to obtain explicit approval from the NRC before they start making hot glass waste forms in the pour canisters?

MR. WELLER: Well, there is a West Valley Act. There is no Savannah River Act.

We don't have approval authority. I don't think we can stop them from making glass if we wanted to. Let me tell you what I envision out of the waste acceptance process, because I think from DOE's point of view they want some kind of end result or conclusion from the NRC. They ought to get something, and we ought to deliver something out of all this interaction in these reviews.

And that should be something perhaps akin to our SEA, perhaps a new objection type of letter. In other

words, we don't object, if the process is successful, and we agree to them making glass. And that is one reason why we asked for that preliminary performance assessment because that is kind of the final end product that we will review to draw that conclusion, that indeed, to the best of your ability, we can assess at the time that this product is likely to perform reasonably well in the repository, or perform how you expected it to perform.

DR. STEINDLER: So presumably then there is a method that you can envision, that I guess all of us can envision if we walk through the steps, which in fact can probably be written down, which, when transferred over to DOE would say, if you guys follow these steps and get these results, then you can go ahead and run this plant and make glass that we think is likely to meet the criteria required by the repository.

DR. MOELLER: That is what DOE has said to us that they would like to have.

DR. STEINDLER: Yes. There are a number of sticky points. Obviously, there was a list of what, seven or eight here?

MR. BUNTING: I don't believe you could make that kind of a statement in the absence of understanding the entire concept for an engineered barrier system.

In other words, what is that system going to

consist of.

DR. STEINDLER: Or at least you have to fix it.

MR. BUNTING: Well, but you may --

DR. STEINDLER: At the risk that was indicated.

MR. BUNTING: That's right. You at least have to have, to me, an allocation, perhaps your materials selection at least for your waste package, and know that what you are doing in the glass factory is not incompatible with what you plan to do with the waste package.

DR. STEINDLER: Yes. Exactly. If all of those steps are met and documented at least to the extent that they can currently give you some documentation.

MR. BUNTING: The point of not knowing what you are doing in the waste package and then going to a hot operation, there is some risk in my mind to that.

MR. YOUNGBLOOD: We will be asking OCRWM to give us all of that information. We won't be asking Savannah River, we won't be asking West Valley. Because the only licensee we may have in the future is OCRWM.

DR. STEINDLER: Yes. That is correct.

Do you believe at the moment that that is a process understood -- I shouldn't ask you what you think somebody else understands, but -- do you get any indication that your view of this process with whatever final approval documents you think you can write or transmit is reasonably

well understood by DOE? I mean, do you two folks have the same idea of what it takes to close that loop so that the glass plants can begin to run? Or is there still some argument about whether or not one or the other of your objections are worth addressing?

MR. BUNTING: Well, we are having some trouble right now getting together with DOE and understanding the waste package.

DR. STEINDLER: I see.

MR. BUNTING: And with that still as an open item and not understanding how people are going to go about to make decisions on waste package, I can't very well say I agree with what you are doing on the glass side.

There may be nothing wrong with the glass side. I just don't have a basis of knowledge.

DR. STEINDLER: Okay.

DR. MOELLER: Gene.

MR. VOILAND: Is the implication here that the glass that is prepared at West Valley will be put in a temporary container and that ultimately will be put in a container that will be compatible with the requirements of the repository?

MR. WELLER: No. It is going to a stainless steel core canister, as they call it. And everybody recognizes that it will be a part of the waste package, but the DOE

does not intend to try to take any credit for it because it is going to be thermally stressed.

MR. VOILAND: It is just something to hold the glass.

MR. WELLER: It is just a convenient vehicle to hold the waste form.

MR. PARRY: You do expect there to be an overpack?

MR. WELLER: Oh, yes. That is why there are canister specifications for length and diameter and size that's got to fit in the overpacking.

DR. MOELLER: Any other questions? One item that has come up twice, at least twice today, is the matter of staffing and the adequacy of resources within, I guess this morning it was the Low-Level Waste Branch or Division and now it is the High-Level Waste Division.

The Commission and Commissioners have specifically asked us, you know, to the extent that we see problems, to comment on them.

What is the committee's feeling, belief on this?

DR. STEINDLER: I'm convinced that we should comment on it.

DR. MOELLER: All right.

DR. STEINDLER: I guess, I don't know whether we are in a freewheeling discussion or not, but my view is that

unless and until these issues are tracked and in a sense signed off on, by NRC, DOE is going to continue to fire sheets of paper around the world and cut down trees to do it, but it won't mean a whole heck of a lot. I mean, they won't be able to close the issue.

And it doesn't strike me that 40 staff weeks less than FTE plus 150 K for TA represents a horrendous bending of the budget in considering what is involved.

DR. POMEROY: I would be curious, though, in that sense, Martin, to worry about why it was an over budget item in the first place.

It seems to me that this activity is a fairly essential activity, and to have it end up in an over budget category, if I understand what an over budget category is, implies that it has less priority than everything that is within the budget.

And I would have to investigate, if I were in your position I would have to investigate why it was in that position in the first place.

MR. BUNTING: That is a question we can ask Bob Browning.

DR. MOELLER: I see he is here and I don't want to put him on the spot.

DR. HINZE: It is rather obvious, too, that there is a potential saving in the long run by taking advantage of

what can be gleaned from the foreign experience.

It seems to me that this is pennywise and pound foolish not to be up on that situation.

DR. STEINDLER: I think the staff should, the NRC staff should kind of track that to the extent they can. It isn't that big a deal.

On the other hand, I think it is DOE's function to put together the background case that justifies their particular conclusions, using whatever the information is that they have available.

And I know for a fact they know what is going on in France. It isn't that they are blind.

DR. MOELLER: Any other questions or comments on this subject?

(No response)

DR. MOELLER: Well, thank you, Rick, for your presentation.

Do you want to comment?

MR. YOUNGBLOOD: I think our budget is fairly consistent with the program that they gave us from a schedule that they gave us, so that we could work it in.

Now, that is OCRWM, who has to deal with us on that. Now, if you want to go to West Valley and ask them what they think we ought to do, I'm sure they will up that by a significant amount, and what they think they think they

are going to do with their budget and so forth is maybe inconsistent with what OCRWM is saying.

But nevertheless, we still have to work through OCRWM on these and we can't go off spurious to whatever the other entities are saying that they want to do.

So I think the manpower we've budgeted has not been inconsistent with the schedules that OCRWM has given us.

DR. STEINDLER: I think therein lies the problem.

MR. YOUNGBLOOD: And if they give us more than what they thought and they beat some of their schedules, we will reorder our priorities and so forth, and have to accommodate these things as they come along.

DR. STEINDLER: Do you react to the DWPF schedules the same way?

MR. YOUNGBLOOD: Whatever schedule they have, we have to get it through OCRWM.

DR. STEINDLER: You do.

DR. MOELLER: Well, and we need, of course, to be careful on this. Mr. Youngblood has already pointed out that the allocation was based upon what they anticipated to be the workload. And if we make a comment, I think there is a fine line between saying well, they need more money, which we don't know, or they need to do internal readjustments.

And I doubt, or at least I am not in a position

to tell them what to cut back on, in order to expand this.

But there is probably something we could say that would be constructive and useful.

In other words, we don't want to hurt them in trying to help them.

Okay. Well, if that wraps things up, then we will conclude with our formal, with the formal portion of our meeting today and will take a short break, and then the Committee will go into Executive Session, open to the public, anyone who desires to stay. And our goal will be to review what we have heard today and begin to formulate opinions and draft letters commenting on it.

Let me thank our Reporter for being with us and being patient in trying to hear what is being said. And with that, I will declare the meeting adjourned for today.

(Whereupon, at 4:25 p.m., the meeting was adjourned.)

CERTIFICATE

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

Name: Advisory Committee on Nuclear Waste--Fourteenth General Meeting

Docket Number:

Place: Bethesda, Maryland

Date: October 11, 1989

were held as herein appears, and that this is the original transcript thereof for the file of the United States Nuclear Regulatory Commission taken stenographically 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.

/S/ Catherine S. Boyd

(Signature typed): Catherine S. Boyd

Official Reporter

Heritage Reporting Corporation

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/s/ Joan Rose

(Signature typed): Joan Rose

Official Reporter

Heritage Reporting Corporation

Staff Technical Position
Erosion Protection
ACNW Briefing

October 11, 1989

Overview

* Legislative/Regulatory Framework
* Erosion Protection Aspects
* Staff Technical Position

Legislative/Regulatory Framework

* UMTRCA
* 40 CFR Part 192
* 10 CFR Part 40, Appendix A

Erosion Protection Aspects

* Management Position
* Control
  - 1000 years
  - 200 years minimum
* No maintenance
* EPA groundwater standard

Staff Technical Position

* Several cover types
* F.R. notice - Aug. 11

Insert #1

NRC STAFF PRESENTATION TO THE ACNW

SUBJECT: LLW PERFORMANCE ASSESSMENT METHODOLOGY -- PROGRESS REPORT

DATE: October 11, 1989

PRESENTER: Dr. R. John Starmer

PRESENTER'S TITLE/BRANCH DIV.: Section Leader, Siting Section
Technical Branch
Division of Low-Level Waste Management
and Decommissioning

PRESENTER'S NRC TEL. NO.: 49[illegible]-0589

SUBCOMMITTEE: N/A

TO BE USED ALL PRESENTATIONS TO THE ACNW BY NRC EMPLOYEES

LOW-LEVEL WASTE PERFORMANCE ASSESSMENT INTEGRATED METHODOLOGY DEVELOPMENT

Status Report -- 10/11/89

Division of Low-Level Waste Management and Decommissioning
R. John Starmer

LOW-LEVEL WASTE PERFORMANCE ASSESSMENT
Background

Regulation -- Performance Objectives and Analyses

- Deterministic -- Pathways Analysis — 10 CFR 61.13(a)
- Dose Limits — 10 CFR 61.41
- (Emphasis on Groundwater -- 1981)

LLRWPAA of 1985 -- 15 Months for Review

- Need for Defined Methodology
- All Pathways

Contract With Sandia National Laboratories -- January 1988

- Integrated Performance Assessment Methodology
- Code Implementation and Integration
- Support Staff

LOW-LEVEL WASTE PERFORMANCE ASSESSMENT

Sandia Contract (Post Closure)

Define Pathways

Prioritize Pathways

Define and Integrate Models

Identify and Implement Codes

Self-Teaching Curriculum

LOW-LEVEL WASTE PERFORMANCE ASSESSMENT

Other Work -- Future and Ongoing

Operational and Accidental Releases

Scenario Analysis

Source Term Models

Barrier Performance

Conrete Degradation

Groundwater Transport

LOW-LEVEL WASTE PERFORMANCE ASSESSMENT

Integrated Methodology Characteristics

GLOBAL

MODULAR

SIMPLE

DETERMINISTIC

ROBUST

LOW-LEVEL WASTE PERFORMANCE ASSESSMENT

Performance Assessment Strategy

PATHWAYS ANALYSIS
- Scenario and Pathways Identification
- Ranking
- Selection of Defensible Set of Relevant Exposure Modes

QUANTITATIVE ANLYSIS of FACILITY PERFORMANCE
- Consider Relevant Pathways (incl. Intruder)
- Simple and Defensible
- Include Uncertainty Analysis

COMPARE TO PERFORMANCE OBJECTIVES

LOW-LEVEL WASTE PERFORMANCE ASSESSMENT

Staff Analysis -- NUREG-1200, Chapter 6

DEFINITION of the PHYSICAL SYSTEM

INTEGRATION of SYSTEM and SUBSYSTEM MODELS

BASIS for SELECTION of MODEL INPUT, MODEL VALIDATION and VERIFICATION

MODEL APPLICATION

ANALYSIS of UNCERTAINTIES and SENSITIVITIES

USE of RESULTS to SUPPORT APPLICATION

SOURCE-TERM RELEASE CODES

- WATER BUDGET (INFILTRATION) CODES
  - CREAMS
  - HSSWDS
  - HELP
  - SESOIL
  - UNSAT-H
- CONTAINER DEGRADATION AND LEACHING CODES
  - BLT
  - BARRIER

GROUND-WATER FLOW AND TRANSPORT CODES

- VAM2D
- FEMWATER
- FEMWASTE
- MASCOT
- SUTRA
- TRACR3D
- TRANSS
- NEFTRAN
- TRIPM
- UNSAT2

SURFACE-WATER TRANSPORT CODES

- PATH1
- LADTAP II

AIR-TRANSPORT CODES

- AIRDOS-EPA
- AIRDOS-PC
- XOQDOQ

FOOD-CHAIN AND DOSIMETRY CODES

- GASPAR II
- PABLM
- GENII

Insert #2

OVERVIEW OF WASTE ACCEPTANCE PROCESS (WAP) TO ACN

OCTOBER 11, 1989

RICK WELLER

WASTE ACCEPTANCE PROCESS (WAP)

- GENESIS
  - DEVELOPED BY DOE AUGUST 1985
- PURPOSE
  - FORMALIZES WASTE ACCEPTANCE ACTIVITIES WITHIN OCRWM
  - ENSURES ACCEPTABILITY OF WASTE FORM AT REPOSITORY SITE
- WAP FOCUS
  - ORIGINALLY APPLICABLE TO ALL REPOSITORY SITES
  - MAY 1986, 3 SITES APPROVED FOR CHARACTERIZATION (YUCCA MOUNTAIN, DEAF SMITH, AND HANFORD)
  - DECEMBER 1987, NWPA AMENDED TO CHARACTERIZE ONLY YUCCA MOUNTAIN
- WAP FOCUS, RELATED ISSUES, NARROWED CONSIDERABLY

WAP DOCUMENTS

- WASTE ACCEPTANCE PRELIMINARY SPECIFICATIONS (WAPS)
  - SPECIFIES PROPERTIES AND REQUIREMENTS FOR HLW FORMS PRODUCED AT DWPF AND WVDP
  - RULE DRIVEN (60.113, 60.135)
- WASTE FORM COMPLIANCE PLAN (WCP)
  - DESCRIBES METHODS AND PROGRAMS FOR COMPLIANCE WITH EACH SPECIFICATION IN THE WAPS
- WASTE QUALIFICATION REPORT (WQR)
  - DOCUMENTS RESULTS OF TESTS AND ANALYSES IDENTIFIED IN WCP
- PROCESS CONTROL PROGRAM (PCP)
  - DEFINES PARAMETRIC BOUNDS OF THE VITRIFICATION PROCESS (I.E., RATIOS OF HLW, GLASS FRIT, CHEMICAL ADDITION) TO ENSURE HIGH QUALITY PRODUCT
- PRELIMINARY PERFORMANCE ASSESSMENT
  - ASSESSES HOW GLASS WASTE FORMS WILL PERFORM IN CONTEXT OF ENTIRE WASTE PACKAGE AND YUCCA MOUNTAIN ENVIRONMENT

CHRONOLOGY OF SIGNIFICANT WAP INTERACTIONS

- CORRESPONDENCE, INTERACTIONS DATE BACK TO 1982
- WAP FORMALIZED AUGUST 1985
- TECHNICAL EXCHANGE MEETINGS

  -- JULY 1986 - DISCUSSED WAP AND WASTE ACCEPTANCE PRELIMINARY SPECIFICATIONS FOR WVDP AND DWPF
  -- DECEMBER 1986 - DISCUSSED STATUS OF DWPF
  -- FEBRUARY 1987 - DISCUSSED STATUS OF WVDP
  -- APRIL 1987 - DISCUSSED STATUS OF DWPF
  -- FEBRUARY 1988 - DISCUSSED STATUS OF WVDP
  -- SEPTEMBER 1988 - DISCUSSED STATUS OF DWPF

o CHRONOLOGY OF SIGNIFICANT WAP INTERACTIONS (CONTINUED)

- SIGNIFICANT CORRESPONDENCE

  -- AUGUST 19, 1985 - DOE ISSUES DESCRIPTION OF WAP
  -- DECEMBER 16, 1985 - NRC PROVIDES REVIEW AND COMMENT ON WAP
  -- MAY 30, 1986 - NRC ISSUES REVIEW OF DRAFT WAPS FOR DWPF AND WVDP
  -- SEPTEMBER 2, 1988 - DOE ISSUES DRAFT WAPS (REV. 1) AND WCP FOR DWPF
  -- OCTOBER 13, 1988 - NRC PROVIDES LIST OF ALL WAP OPEN ITEMS
  -- OCTOBER 24, 1988 - DOE ISSUES ITS LIST OF ALL WAP OPEN ITEMS
  -- DECEMBER 5, 1988 - NRC REQUESTS INTEGRATED SCHEDULE FOR ALL WAP RELATED ACTIVITIES
  -- APRIL 4, 1989 - DOE ISSES INTEGRATED SCHEDULE OF WAP ACTIVITIES
  -- AUGUST 3, 1989 - DOE ISSUES CONSOLIDATED LIST OF WAP OPEN ITEMS

DOE INTEGRATED SCHEDULE

NEAR TERM DELIVERABLES/MEETINGS

- AUGUST 1989 - CONSOLIDATED OPEN ITEMS LIST (RECEIVED)
- SEPTEMBER 1989 - REVISED WAPS FOR WVDP (NOT YET RECEIVED)
- OCTOBER 1989 - WCP FOR WVDP (NOT YET RECEIVED)
- OCTOBER 1989 - PLANNED MEETING ON WASTE FORM PERFORMANCE ASSESSMENT (NO FIRM DATE)
- NOVEMBER 1989 - WASTE QUALIFICATION REPORT (WQR) PACKAGE FOR DWPF
- NOVEMBER 1989 - PROCESS CONTROL DOCUMENT FOR DWPF

STAFF CONCERNS

o GENERAL

- POTENTIAL FOR ADVERSE INTERACTIONS BETWEEN THE WASTE FORM PRODUCT AND THE CONTAINER
- ACTUAL PERFORMANCE OF THE GLASS DURING POST CONTAINMENT

STAFF POSITION - VITRIFICATION SYSTEM MUST BE DESIGNED FOR "HOT" SAMPLING CAPABILITY

o SPECIFIC

- WHAT ARE THE DEFINITIVE INTERFACE SPECIFICATIONS FOR THE WASTE FORM WHICH FLOW DOWN FROM THE EBS SPECIFICATION
- WHAT IS THE WASTE FORM SYSTEM DESIGN QUALIFICATION SPECIFICATION AND HOW WILL IT BE DEMONSTRATED
- WHAT IS THE WASTE FORM ACCEPTANCE TEST SPECIFICATION AND HOW WILL IT BE DEMONSTRATED
- WHAT ARE THE REQUIREMENTS FOR TEST REPRODUCIBILITY (E.G., LEACH TESTING OF SIMULATED GLASS WASTE) AND THE RESULTANT STATISTICAL VALIDITY
- WHAT IS THE VARABILITY ALLOWED IN TEST RESULTS AND HOW WILL IT BE ASCERTAINED AND CONTROLLED

WASTE FORM REQUIREMENTS

- 60.135 CRITERIA FOR THE WASTE PACKAGE AND ITS COMPONENTS
  - NO EXPLOSIVE, PYROPHORIC, OR CHEMICALLY REACTIVE MATERIALS WHICH COMPROMISE PERFORMANCE OBJECTIVES
  - NO FREE LIQUIDS WHICH COMPROMISE PERFORMANCE OBJECTIVES
  - HLW SHALL BE IN SOLID FORM
  - PARTICULATE MATERIAL WILL BE CONSOLIDATED
  - NO COMBUSTIBLE MATERIAL WHICH COMPROMISES PERFORMANCE

NRC STAFF FY90 RESOURCES FOR WAP

- NRC STAFF AND $ RESOURCES REQUESTED FOR FY90 WAP WERE NOT APPROVED

  - SOME LIMITED COGNIZANCE WILL BE MAINTAINED
  - RESOURCES MAY BE REALLOCATED BASED ON DOE PROGRAMMATIC DEVELOPMENTS

ACNW COMMENT ON WAP

- JANUARY 26, 1989 LETTER FROM ACNW (DR. MOELLER TO CHAIRMAN ZECH) ON WEST VALLEY DEMONSTRATION PROJECT
  - IDENTIFIED NEED FOR ACCEPTANCE CRITERIA FOR VITRIFIED HLW, INCLUDING TESTING PROCEDURES TO DEMONSTRATE CONFORMANCE WITH CRITERIA
- STAFF HAS NOT YET FORMALLY REVIEWED LATEST (SEPTEMBER 1988) WAPS AND WCP FOR DWPF TO ADDRESS ACNW CONCERN
- REVISED WAPS AND WCP FOR WVDP ARE EXPECTED SHORTLY