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UNITED STATES OF AMERICA

NUCLEAR REGULATORY COMMISSION

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ADVISORY COMMITTEE ON NUCLEAR WASTE AND MATERIALS

(ACNWM)

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184th MEETING

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VOLUME I

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THURSDAY,

NOVEMBER 15, 2007

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The Advisory Committee met at the Nuclear Regulatory Commission, Two White Flint North, Room T2B1, 11545 Rockville Pike, Rockville, Maryland, at 8:30 a.m., Dr. Michael T. Ryan, Chairman, presiding.

MEMBERS PRESENT:

MICHAEL T. RYAN, Chairman

ALLEN G. CROFF, Vice Chair

JAMES H. CLARKE, Member

WILLIAM J. HINZE, Member

RUTH F. WEINER, Member

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

(9:03 a.m.)

CHAIRMAN RYAN: This is the third day of the 184th meeting of the Advisory Committee on Nuclear Waste and Materials. During today's meeting, the committee will consider the following items: First, the accounting for dose consequence and the state of the art reactor consequence analysis project, and a discussion of ACNW&M letter reports will follow. Neal Coleman is the designated federal official for today's session. We have received no written comments or requests for time to make oral statements from members of the public regarding today's session.

Should anyone wish to address the committee, please make your wishes known to one of the committee staff. It's requested that the speakers use one of the microphones, identify themselves, and speak with sufficient clarity and volume so they can be readily heard. It's also requested that if you have cell phones or pagers, that you kindly turn them off at this time. Feedback forms are available at the back of the room for anyone who would like to provide us with his or her comments regarding this meeting.

Thank you. I believe Mr. Sullivan, you'll start off, or Mr. Prato?

DR. BAHADUR: If it's okay with you --

CHAIRMAN RYAN: Please, Dr. Bahadur, we welcome your comments.

DR. BAHADUR: All right. I'm Sher Bahadur. I'm deputy director for the division of systems analysis. And as you know, the research got reorganized recently. This is one of the three divisions, the other two being the division of risk analysis and the division of engineering. Today we're going to talk about the state of the art reactor consequence analysis, as we call it, SOARCA. And the staff's focus is going to be mostly on the dose threshold as it's used for the best estimate for potential effects, consequence of an accident.

You will hear the initial thinking of the staff on various options. We are in the process of evolving thought, and with that in mind, what I've done is, I've invited my senior staff. Vince Holohan is sitting back in the audience. Giles Tinkler is sitting back there. He is another senior staff member. And Jocelyn Mitchell. I also have the branch chief for special projects branch. So we are here for

more of a collegial discussion on these options that the staff is currently reviewing towards the dose threshold.

With that in mind, Robert Prato is the project manager for this project. And Randolph Sullivan, who is from the office of nuclear security and response. He is the technical lead. And if you don't have any further questions, I'll ask them to start.

CHAIRMAN RYAN: Thank you, Dr. Bahadur. We look forward to the dialog.

MR. SULLIVAN: Before we start, if I might add -- this is Randy Sullivan -- our senior level technical advisor is also in the audience, Trish Milligan, and may be able to assist us with this issue.

CHAIRMAN RYAN: Thank you.

MR. PRATO: Good morning. I'm Bob Prato. I'm the project manager for SOARCA. And on behalf of the SOARCA team, I'd like to thank the committee for their interest in this project. Our objective today is we're going to give an overview and then we're going to get into the reporting of latent cancer fatalities. The overview will hopefully provide you

with an understanding of what we're doing and how we're doing it, and put Randy's discussion in a little bit better perspective.

SOARCA really has two goals. The first goal is to develop the state of the art, more realistic evaluation of severe accident progression, radiological release, and offsite consequences for dominant accident sequences. And as we go through the presentation, hopefully these facts will be coming out in the presentation. And a second goal is to replace analysis such as new reg 2239, which is the technical guidance for siting criteria development.

That was a 1982 report. That report, if you were to read the forward, you would see that it had a very specific application. That application was to help provide the staff with some background information for the development of siting criteria. In the forward, it's very specific that it should only be used for that application. However, because it does report some consequences, based on some very bounding and conservative assumptions, it has been used routinely in applications where consequences were of importance.

After 9/11, the staff has done a number of

different analyses, and we started to realize that early containment failure and early core damage was not a realistic outcome of many the sequences that we're looking at today. So the decision was to go ahead and do an additional -- a new consequence analysis to replace the old study that dates back more than twenty five years.

What you see in front of you is a flow chart for SOARCA, the process itself. SOARCA was never intended to be a risk analysis. Our intent was to develop the most realistic accident progression analysis and consequence analysis, and our intent was to use computer models of site specific computer models to come up with the realistic results. But however, one of the things that staff wanted to do was we wanted to focus on the sequences of greatest interest.

Therefore, we set an initial threshold for including sequences with release frequencies of 10E to the minus six. And because there are -- there was concerns that limiting the sequences reviewed may miss some lower frequencies, but higher risk sequences, we also included sequences for bypass events as low as 10E to the minus seven and greater. So to increase

our scope to make sure that we cleared those sequences that are a little bit more risky.

The other thing, we looked for assistance from the PRA folks. We took the initial conditions as defined in each of the sequences. We identified the containment -- the states of the containment systems, based on those initial conditions. And then we took a look at all the supporting systems that were required, and made a determination as to whether or not containment systems were available to mitigate any consequences.

From there we took site specific information from a design and operational perspective, and we took mitigative measures. We went to the site, we sat down with the licensee and went through each of the sequences, developed a time line, and determined what mitigative measures were available, and when they would be implemented. All of that information was fed into MELCOR. And MELCOR made a determination as to whether or not core damage occurred and when it would occur, and when containment failure would occur. Along with all that input into MELCOR, we also did site specific structural analysis to try and determine the vulnerability of the containments.

As a result of all this analysis, the output from MELCOR is basically a source term and time of release. And that was fed into the MAX code, the MAX code, along with meteorological data and emergency preparedness would determine the results, the early and latent cancer fatalities. Initially, the project plan was to do all 183 plants. Our intent was to divide those plants by plant design, and triple S design and containment. And we did that and we came up with eight different classes of plants.

And we were going to determine the sequences for each class of plants -- the dominant sequences for each class of plant, and do a representative MELCOR analysis for each class of plant, and then do a site specific consequence analysis based on those source terms. After we got into the project a little bit, we reassessed it, and what we decided to do was to do an initial scope, which included not more than eight plants. We were trying to get volunteers from one of the eight plants, each of the eight classes of plants, run the analysis for those eight plants. And then after those eight plants are done, we take the initial scope to the Commission and our recommendation as to how we would

like to proceed with the remaining sixty other analysis.

In addition to the initial eight plants, we decided to do one BWR and one PWR first. We wanted to do that to help exercise the process and to help work out any bugs in the process, and refine it so we can move forward in a little more efficient manner. So we've started the analysis on the first two plants, one BWR and one PWR. We're in the process of getting the initial results. After that, we plan to do -- we've started and we plan to continue a number of different sensitivity analyses. After that we're going to do an uncertainty analysis for the first term. And then an exterior peer review.

We are doing a number of interior peer reviews. We also wanted to do an exterior peer review. And then from there we're going to complete the remaining -- up to six additional plants to finish the initial scope and take these results to the Commission. The approach for SOARCA includes full power operating scenarios. We're not doing a low power and shut down risk type of scenarios, and we're not doing spent fuel scenarios.

We're using plant specific sequences,

including external events, where the core damage frequency is greater than or equal to E the minus six. And a core damage frequency greater than or equal ten to the minus seven for bypass events. We're going to consider plant improvements. And post TMI licensees made a significant number of plant improvements. We're going to include regulatory improvements, like station blackout, adverse rule, those type of things. We're going to take a look at operational improvements. Over the past ten, fifteen years, licensees have done a great deal to improve their operations and their overall performance.

And we're going to look at emergency response. In addition to that, we are also looking at mitigative measures, including new mitigative measurer that came out of the recent B5B analysis. We are doing a number of sensitivity analyses to assess the effectiveness of safety measures. The state of the art accident progression modeling based on twenty five years of research to provide a best estimate. That's our goal, is to provide a best estimate for accident progression, containment performance, time of release, and fission product behavior.

We're looking for more realistic offsite

dispersion models. We've done a lot to improve the MELCOR MAX models. In the 1982 study, the MELCOR model was non-existent. There was no computer model for plant design. And there was a very primitive model for MAX. Since then, MELCOR has been developed, has been significantly improved, and of course SOARCA has had a number of improvements, specifically for this analysis. Same thing with MAX, MAX has gone through a number of improvements. And for the purpose of SOARCA, we had a significant number of improvements to make sure that it's state of the art.

CHAIRMAN RYAN: Could you mention just a couple of what those improvements were?

MR. PRATO: Annual resolution, we went from sixteen to sixty four.

MR. SULLIVAN: Well, Jocelyn has a list of them behind you there, but the long and the short of it is, we increased the number of sectors that we calculate in to try to model wind meander a little bit better. We used the latest dose conversion factors from ICRP, I guess. We improved vastly how we can model emergency response. We can now have multiple cohorts, and we can change their speed and time and space. So this was a great opportunity for me to more

realistically model offsite emergency response, ie, how people are notified, how they're evacuated, what speeds they move in, and what directions they move in.

So MAX will -- there's a lot of things MAX won't do. But what it will do is move the population at risk, and estimate consequences to that population.

CHAIRMAN RYAN: Can you also address sheltering in place, doctor?

MR. SULLIVAN: Yes. And we do that for some of the cohorts. For instance, schools. The two sites we analyzed, this is not an important factor. But some sites that we studied and then didn't analyze, take two runs of buses to move their school population. We're analyzing the case of school days, because we think it's a more important case. I suppose that's not the majority of the time, but we thought that was the right analysis to do. In the case where there's two trips of school buses, well the children are sheltered in a substantial structure while that's going on. And we would model that.

MEMBER WIENER: Is your modeling of the accident conditional probabilities and severities and so on, is that still the same as it has been in MAX 2? Did you make any improvements in --

MR. SULLIVAN: I'm not sure that I understand the question.

MS. MITCHELL: I'm not sure that I understand that question either.

MEMBER WIENER: Maybe if we could -- if we could know -- you didn't make any changes in the basic way that MAX models the release.

MS. MITCHELL: Yes, we did.

MEMBER WIENER: Oh, okay.

MS. MITCHELL: We did. In that we have sixty four compass directions, as he mentioned. We can have shorter release of segments.

MEMBER WIENER: Okay.

MS. MITCHELL: So instead of having a relatively short one and then a very long tail where you accumulate hours and hours of release, we have the ability to break it up. The other things I wanted to mention, we have put in a KI model. That's exercise both for Pennsylvania and for Virginia. And we put in the ability to model parameter uncertainties, which we will exercise in the future in the SOARCA project.

MEMBER WIENER: Thank you.

MS. MITCHELL: And the alternative models for latent cancer dose response, you'll hear more

about later.

CHAIRMAN RYAN: Okay.

MEMBER WIENER: Thank you for the clarification.

CHAIRMAN RYAN: Thanks.

MR. PRATO: And finally, site specific evaluation of public evaluation, based on updated site specific emergency plans. Randy, do you want to get into that a little bit?

MR. SULLIVAN: Well, I presented to you broadly what we're doing, but we spent -- this was a great opportunity to more realistically model what's happening in the environs during a severe accident. This would be a general emergency under our emergency plans. These plans have been inspected, drilled, and tested for about thirty years. We believe that the licensee will implement these plans as their procedures call for. In the dominant sequences that we ended up with, there's probably a couple of declarations before the general emergency.

At some sites, the sirens are sounded at a site area emergency, and school evacuation begins. So at all sites, the general population is evacuated two miles and five miles downwind at a general emergency.

We were able to model these movements and the timing of these movements in MAX. So we took the specific population, we took the evacuation time estimates of the licensees, and we used that to model speed and direction. We did an analysis, based on the licensees' evacuation time estimates, of what are the cohorts? Because everybody doesn't move at the same speed. School children move different than adults and workers. There's always a slow moving tail that at some sites is farmers putting up their stock, or families reuniting.

But a large portion of the population moves fairly rapidly. And so we're able to break this population up into cohorts. We're also able in MAX to identify areas where traffic might flow rather smoothly, and the more likely case where traffic is moving kind of slowly. We can apply a factor in either direction for those areas if the evacuation routes go through those areas. So we can do a fairly -- well, this is not a perfect analysis. But it certainly is a much more detailed analysis than we've done in the past.

Now, for one of the sites that we analyzed, school children are not evacuated at the

site area emergency. Buses are called. And at the general emergency, they move with the population. At the other site, school children are evacuated at the site area emergency. So we dug that information out of their evacuation time estimate, and we moved those cohorts in accordance with their emergency plan. You know, we think we've done a state of the art analysis of how populations would move during the evacuations.

MR. PRATO: Okay, that covers the overview. Randy, if you want to talk about latent cancer fatalities?

MR. SULLIVAN: I sure do. The Commission has charged us with creating a best estimate of consequences from these accidents. They've charged us with using the best risk communication techniques that we can develop, to communicate the results of our accidents, of these potential accidents. Now these are unlikely accidents. But nevertheless, this analysis can serve to focus resources and attention on the most important mitigative measures to reduce any consequences.

In the conduct of our analysis, it's become a concern to the staff that how we -- the staff believes that we must assess early fatalities and

latent cancer fatalities to align with and perhaps correct the results from past analyses. Now there's a whole other way to do this. We could have done risk to the individual. And perhaps that would a more modern technique. But that would not align with and correct past analyses that published early fatalities and latent cancer fatalities.

So the staff believes we must do this consequence analysis in a manner that speaks to what we've done in the past. Okay, so early fatalities is easy. You just -- if anybody's going to get fatal doses, you know, we ought to report those from these accidents. Latent cancer fatalities is more difficult. In dealing with latent cancer fatalities, we run into the issue of using collective dose to assess risk. And our analyses, our source term is -- well, you've already heard how we'd move populations. But beyond the evacuation zone, there will be a dose to members of the public who remain in place.

Now we expect things like a shadow evacuation beyond the evacuation zone and beyond the emergency planning zone and that sort of thing. But when you take all that into account, you still will end up with a large population receiving very small

doses. And the staff has to grapple with how do we assess latent cancer fatalities among that population. Well, there are options, as I'm sure you're well aware. We can use these options. You can use LNT and multiply millions of people times a very small dose and come up with latent cancer fatalities. You can use a range, if you wish, or you can use a threshold for the purposes of the staff's best estimate of risk consequences from this analysis.

That's a Commission paper that I've had the privilege of drafting and discussing with my peers in the agency at some great extent, as you might imagine. And we'll hopefully get that Commission paper up to the Commission for their decision. I'd be willing to discuss the advantages and disadvantages of each of the options, if that's of interest to you.

CHAIRMAN RYAN: Please.

MR. SULLIVAN: I sense that your background is perhaps deeper than mine, but nevertheless, at the risk of lecturing -- and I don't want to lecture, by all means, not to this committee. The range of thresholds has some advantages, it has some disadvantages. It would certainly present everybody's view, as to whether there is a threshold or there is

not. The problem is, it does not facilitate risk communication. We would have one sequence, a station blackout, for instance, and there would be five different answers as to the consequences.

This is kind of dissatisfying to the staff, because it doesn't represent a best estimate. It represents a range of possibilities. That's not the staff's best estimate. So there's problems with that, although previously the Commission directed us to use that sort of technique. The staff is questioning whether that's the most effective risk communication tool. Now maybe it is. And we're happy to take that direction if it is. We could use a linear, no threshold theory -- model, I suppose is a better word -- and simply run out, use collective dose indiscriminately, run out small doses to a large population, apply the factor, and say that's how many latent cancer fatalities there are.

That's certainly easy to do. There's some difference of opinion -- well, there's a great difference of opinion among the staff, but those on the team might not consider that a best estimate. But nevertheless, there are advantages to that. The disadvantages are that it could be a misuse of

collective dose. There's many opinions on this subject, as I'm sure you're aware.

CHAIRMAN RYAN: The committee is on record to tell you that there's not very many good uses of collective dose.

MR. SULLIVAN: Well, I have a personal opinion. But acting for the team, I'm just trying to present --

CHAIRMAN RYAN: No, no, I appreciate that. But in large circumstances, in a power plant or another licensee's facility where you're looking at process A to do a job of process B. And that's one measure of radiation protection success for method A versus method B. We sure see that as valuable. But when you get to micro doses to mega people, it completely discounts background. And when you completely discount any other contribution to background of risk, it becomes troublesome because you're only looking at one component of total radiation dose, where the other components of radiation does dwarf the micro dose. So we've struggled that for a couple of letters on the topic.

MR. SULLIVAN: Let me try to say it in the positive. There's been a lot of improvements to MAX.

And it is our tool and it's improving all the time, and we're going to make more improvements. It does not help us put in context the results of consequences. So for instance -- I'll probably explain this poorly -- the way we use MAX is it does many, many weather trials, collects the consequences, and then it'll give you a mean or a seventy five percent or a forty percent, you know, whatever it is you want, of the consequences. It's not counting population while you do that.

So if I use LNT and I develop some thousands of potential latent cancer fatalities, that number of latent cancer fatalities is indeed not detectable among the population question. But that is not -- we can't put that in context. We can't say fifty million people were involved in this test, and here's the potential for latent cancer fatalities using LNT. Now perhaps we can fix that in the future. I'm hoping that we can. But currently our tool doesn't allow us to put that number in context.

Which is a big detriment. The third option that we've talked about is we looked at this from many positions. We thought perhaps we would develop an expert committee and elicit a threshold

appropriate for this report. We are not looking for a cancer induction threshold. It's not necessary for this report. This report is a best estimate by the staff of consequences from severe -- unlikely severe accidents. We do not think we have to explore the threshold for latent cancer fatalities. There may or may not be a threshold, whatever your personal opinion is.

We're attempting to report the staff's best estimate. Now I think the staff has the right to present their estimate on what they think are the consequences of a given accident. We thought of retaining a committee. We thought of looking at what would be detectable, and it turns out the doses have to be pretty high to actually detect an increase in latent cancer fatalities in a population. The five rem of the Health Physics Society is likely not a level where actual increase in LCF would be detectable.

So the detectable idea was a very big number, and we didn't think that was appropriate. We thought we could develop this committee, and of course we became aware of the Health Physics Society position paper, which does this. It picks a threshold of five

rem current events and ten rem in a life time. The staff thought that was a reasonable option, and perhaps more expedient and involved the staff less than an expert elicitation panel would. So we thought that was a viable option also.

The problem with an expert elicitation panel is that -- well, let me say it in reverse. We didn't request this number from the Health Physics Society. We didn't ask them for it. I mean, I was aware of it, but the team was not. It was there -- it's external to the NRC. We thought that had a certain amount of attractiveness. So we chose that as our third option. We're going to have several more meetings on this subject. The senior level advisors from various offices are planning to get together next week or next month -- I'm forgetting -- and sort of talk about our views.

The views of the committee would be much appreciated on this subject. But the staff hopes to provide a recommendation to the Commission, and for the Commission to provide us approval of that recommendation. And finish our study.

CHAIRMAN RYAN: Can we go into a little bit more detail on each of your options and how you would

use them?

MR. SULLIVAN: Yes, we can.

CHAIRMAN RYAN: Go ahead.

MR. SULLIVAN: Okay, so MELCOR gives us a source term, in kilograms, by the way. MAX -- there's a program that converts it to --

CHAIRMAN RYAN: Kilograms of what?

MR. SULLIVAN: Cesium, iodine. Radioactive species, isn't that beautiful?

CHAIRMAN RYAN: Oh, kilograms of each radioactive -- okay.

MR. SULLIVAN: It really is kind of funny, isn't it? But it's a mass balance program, I guess that's the right way to say it. And it takes the mass of the radio nuclei species in the core, and it examines the mechanical and physical --

CHAIRMAN RYAN: I'm not sure, but I imagine that it accounts for the fact that you only have so much mass to start with --

MR. SULLIVAN: Sure it does, sure it does. It starts with what's in the fuel. And it accounts for filtering through structures, and does many, many sophisticated things. But eventually that source term leaves plant buildings. In electronic space, it then

goes to a modifying program that turns, I suppose, kilograms into curies, and feeds that to MAX. MAX then calculates doses to a population that it can move, and consequences of those doses. MAX will accept thresholds. It makes the calculation a bit more time consuming, but we can put multiple thresholds, one threshold, or no threshold in MAX. And it'll report -- it'll do its thousand weather trials. We're looking at the mean of the consequences.

CHAIRMAN RYAN: So the result in the multiple threshold cases, you'll get number of doses and fatal cancers with a threshold of one, two, three, four, five, or whatever numbers you pick between zero and five?

MR. SULLIVAN: Exactly. In the multiple threshold case, I guess MAX would run its thousand weather trials with threshold A. It would do the same thing with threshold B and threshold C. You'd have three or six sets of results. You would then choose the mean or the seventy five percent of those results. It's based on weather trials, right? The actual weather. And then you would present three or five results for a given -- we're talking about one

accident. All right, one accident sequence. So you would publish -- I guess it'd be a table that would say, here's the -- well, we're just talking LCF. Here's the LCF for this sequence. And here's the threshold. So there would be a series of answers to the question.

There would be one answer. HP Society, there would be one answer. I'm not sure that answered your question.

CHAIRMAN RYAN: That's part of it. I'm just trying to understand if a multiple range of thresholds -- is it just an artifact of calculation. For example, does two give me one half the cancers of one? Because it's all linear. I mean, you don't have different cancer risk estimators as a function of dose, do you?

MR. SULLIVAN: Right. I guess it would be. Jocelyn?

MS. MITCHELL: We have a threshold. These are not linear, okay? So if you --

CHAIRMAN RYAN: No, no, no, that's not my question. My question is, if I cut off at one versus two, what's the difference in the cancer fatalities?

MS. MITCHELL: You can't tell, because you

don't know whether all the doses are 1.9997 rem, and so there's nothing above two, but the minute you say, okay, one, you pick up all of those doses. So you can't say that --

CHAIRMAN RYAN: Yes, but that's just a roundoff question.

MS. MITCHELL: It isn't roundoff.

CHAIRMAN RYAN: Sure it is.

MS. MITCHELL: It isn't roundoff.

CHAIRMAN RYAN: Well, if you're calculating doses to four significant digits, I want to see how you do that. That doesn't make any sense to me.

MS. MITCHELL: It was 1.5.

CHAIRMAN RYAN: So it's a bin. But I mean, roughly, in each bin, if you go up from one to two, two to three, three to four, are you adding the same number each time you capture the additional rem?

MS. MITCHELL: No, no. It's a threshold.

CHAIRMAN RYAN: Okay.

MS. MITCHELL: And depending if the doses are falling off with one over R squared as a function of distance, then you're not going to, as you add, you're not going to just double it if --

CHAIRMAN RYAN: Sure, it's got -- Okay, I'm

with you. I understand now. Thanks.

MS. MITCHELL: Okay.

MR. SULLIVAN: I guess the binning -- I mean, one would expect it to be linear, right? One, two. But the binning effects, perhaps it gets chopped off.

CHAIRMAN RYAN: I guess, you know, in a certain sector, up to a certain distance it would. But I understand the tail part of it too. Thanks.

MEMBER WIENER: Can I ask a question?

CHAIRMAN RYAN: Sure.

MEMBER WIENER: You're postulating -- I want to go back a little bit -- you're postulating one accident, is that correct?

MR. SULLIVAN: Yes.

MEMBER WIENER: And that accident has associated with it, I assume, a set of released fractions for each physical chemical group, and you bin your radio nuclides in physical chemical groups?

MR. SULLIVAN: Yes.

MEMBER WIENER: And then those that are released are responsible for the dose?

MR. SULLIVAN: Yes.

MEMBER WIENER: Now, does that binning --

is that a -- how does that binning and the selection of those physical chemical groups affect your dose and your threshold?

MR. SULLIVAN: Well, I can't -- we happen to have MELCOR experts in the room who can maybe address the binning of the chemical species.

MEMBER WIENER: What I'm trying to get at is, and it's a fairly simple answer, does that introduce a conservatism? In other words, with each bin you have associated a deposition velocity, you have associated a particle size distribution --

MS. MITCHELL: The deposition velocity is a function of particle size, and it isn't a function, necessarily, of the chemical bin. In MAX, each chemical bin can have a particle size distribution associated with it.

MEMBER WIENER: Yes.

MS. MITCHELL: By each one of these little plumes. So the first plume has one distribution, and the second plume, which comes from core concrete interaction as opposed to in vessel release, has another set. But the MELCOR is where the masses of the fission products, radioactive plus non-radioactive products are carried throughout the plant. The

binning in MAX is the exact same binning. So if the chemical element group in MELCOR included the following three chemical elements, the same chemical elements are in the same bin in MAX. Okay? So the whole thing is at least self consistent. It's not knowingly conservative.

MEMBER WIENER: Thank you. That was what I was trying to find out.

CHAIRMAN RYAN: One of the interesting things I think this discussion and my questions would help is that to really understand your three options that you're looking at, it would be helpful for us to hear a lot more of the details of how these calculations work.

MR. SULLIVAN: Okay.

CHAIRMAN RYAN: So if I could maybe ask that we get a further briefing on some of these details without you having to jump up and down and answer my questions on the fly. I'd like to see how you treat each one of the assumptions in the calculational scheme in a little bit more detail, that would be really helpful for us to help form an opinion.

MR. PRATO: Jocelyn, do we have this

information, or do we need to develop it?

MS. MITCHELL: Well, it's just that -- if you have three hours, I can start now. If you want a fifteen minute discussion, then it's going to take time to develop. Because I can just start talking about it and we'll get through it --

CHAIRMAN RYAN: That's not as helpful as an organized presentation.

MS. MITCHELL: Yes. I do want to mention that the threshold is applied after you calculate the dose.

CHAIRMAN RYAN: Right.

MS. MITCHELL: So there are what I call hidden LNT assumptions in the MAX calculation. For instance, if some of the people take KI and the other people don't, then that is lumped into an average dose for the cohort where say, fifty percent of the people take it and fifty percent of them don't. That really is a hidden a LNT assumption. In the same case, there are other hidden LNT assumptions, and that's an important issue.

MR. PRATO: Do you want a separate presentation on that, or do you want documentation that explains it?

CHAIRMAN RYAN: Both. I think if we had a little bit more detail of how the calculations work, that's important, I think, to us. To understand which, if any of these, or all of these, assumptions are rolled into your calculations and estimates of impacts, so we can really understand is that done the way we think it ought to be done and we can make suggestions on how to improve it and so forth. But without understanding the application of these three options, it's going to be real hard for us to form a well thought-through opinion. So I think hearing a more detailed and technical briefing on how the calculations work so we can get into some of these specifics that Dr. Wiener and I and I'm sure other colleagues on the committee would like to hear.

We'd sure like to just see that. That would help us a lot to understand, you know, your implementation of these three options, or the potential implementation. And surrogate calculations that just show how they work and how the mechanics and the calculations work are really what we'd be reaching for. We don't need specific cases or anything like that. Is that fair enough?

MS. MITCHELL: Sure. Absolutely.

CHAIRMAN RYAN: Thank you. Okay, great.

DR. BAHADUR: Then what we'll do is, we'll develop a fact sheet for each of the options. We will send it to the committee first, and then based on those fact sheets, then maybe we can schedule a presentation.

CHAIRMAN RYAN: That would be great. And we could react to that and maybe have specific questions and help shape the briefing and so forth. Does that sound like a plan?

DR. BAHADUR: Yes.

CHAIRMAN RYAN: Okay. I might just -- and that's Dr. Bahadur that was making the comment, and just for the record, if you could mention your name as you speak, it helps him to keep our transcript organized, so thank you very much.

MR. SULLIVAN: Are there any other questions on the process?

VICE-CHAIR CROFF: I guess I'd like to come back to a couple of fundamental things. First, is there any requirement for you to calculate latent cancer fatalities or collective dose?

MR. SULLIVAN: Only the staff -- no, to the best of my knowledge.

VICE-CHAIR CROFF: Okay. I guess secondly -- what you're trying to accomplish here in terms of latent cancer fatalities and thresholds -- at one point you mentioned the desire to compare to previous results --

MR. SULLIVAN: That's correct.

VICE-CHAIR CROFF: The old 1982 study. And then later on you focused a lot more on the risk communication. Is it both of those? Are both of those objectives?

MR. SULLIVAN: We think that's part of the risk communication, to be able to present this as the staff's latest, best estimate of consequences from these accidents.

VICE-CHAIR CROFF: I guess at this point I'm going to offer -- let me call it an interim comment, as opposed to a question -- my suggestion is, if you don't have to do collective dose and latent cancer fatalities, don't. In other words, communicate in terms of individual dose and distribution of individual dose, and doses across the population as a function of geography. And that avoids an awful lot of complications. I think if you feel -- if staff feel compelled to go to latent cancer fatalities or

collective dose, given that for many of the exposed population, you're below observable effects, and it's unknowable in that region.

And you're not likely to know at any reasonable time in the future. I think you would have to look at the range of thresholds. In other words, you don't know what the right answer is, you can't defend, as far as I can tell, any particular threshold. I mean, you've got an HPS opinion, but okay, it's their opinion. I think you're going to have to look at the range and portray the range out there. And basically say, "We do not know in this range. It could possibly be zero, which is LNT. It may be something else, but for these set of assumptions, here's what it looks like, and that's that."

That's just one person's opinion at this point. But that's what I come to after hearing what you've said so far.

MR. SULLIVAN: I understand. If I could just ask for the benefit of your views, I mean this is the staff's best estimate.

VICE-CHAIR CROFF: What is?

MR. SULLIVAN: The results of SOARCA are

the staff's best estimate. Does the staff not have a right to present its best estimate?

VICE-CHAIR CROFF: What is your best estimate of latent cancer fatalities? You've outlined three methods, and I'm not sure which one is your best estimate, if any.

MR. SULLIVAN: We don't know yet either. We will choose one. There will be a recommendation that we'll make to the Commission. And let's just say, hypothetically, we chose LNT or we chose HPS. I mean if the staff comes down and says, "Baloney, this is too difficult. Just go ahead with the most conservative thing, choose LNT." What if that's the staff's best estimate? Or, conversely, what if we say this is a misuse of collective dose, we believe a more representative estimate is five rem. I mean, is that inappropriate for the staff to do?

VICE-CHAIR CROFF: I don't see how any particular threshold, any singular threshold value can be defended in an unobservable region. That's where you are. You can't observe these effects.

MR. SULLIVAN: We're making best estimates based on the phenomenology of this accident. We're making our estimate of how --

VICE-CHAIR CROFF: Up to the point where you calculate radio nuclide releases and the distribution to the population and even doses, I can see going that far as a best estimate. But then when you start talking about converting into latent cancer fatalities, I don't see where there's a best estimate in there, because we don't know what the answer is.

MR. SULLIVAN: Without melting a core and over-stressing a full-sized containment, we're also making a best estimate.

VICE-CHAIR CROFF: That wouldn't help you answer this question.

CHAIRMAN RYAN: I think you're on two different paths here. Let me try to offer a -- I appreciate Allen's point that if you calculate a dose, that's a fairly straightforward thing where you can exercise lots of parameters in how you had the exposure and how you calculate a dose. But the latent fatal cancer is an extrapolation. And there's no way to test that extrapolation for its validity. Now the dose, there is. There's lot of cases. We have metabolic models, we have exposures on which those metabolic models are based, we have physiology, we have physics, and we have all that to calculate the

dose.

So the difficulty is is that it's an extrapolation from high dose regions, typically. And this committee's had lots of dialog with folks. We've had the French Academy of Science's folks in, who've said that they see a ten rad threshold. And then of course the BEIR Report, BEIR VII, sees no threshold. The policy for administrative purposes in the agency is to use LNT. But that doesn't rule out the fact that we live in a sea of radiation from natural and manmade sources.

Last year at the annual meeting of the NCRP there was a presentation that medical exposures are going up by quite a large factor. So everybody in this cohort's receiving some number between 300 and 600 millirem per year. My problem, to add to Allen's problem, is how do I evaluate the cancer risk of an incremental small dose, and let's say it's half of background, or less, on top of an existing background. Which also has a cancer risk, I might add. You can't rule out the cancer risk associated with the background and medical exposure. Those exist. They're real.

MR. SULLIVAN: Well, I agree with

everything you're saying.

CHAIRMAN RYAN: So you're counting part of it when you do this calculation, but you're not saying, "Well this is a cancer risk incremental to what?" Is it incremental to background that population receives? I don't imagine you count for that.

MR. SULLIVAN: No.

CHAIRMAN RYAN: So it's artificial in a sense. So it adds an uncertainty, and I think part of the risk communication to me is it doesn't really communicate the complete profile of risks for latent cancer. It only picks on one, and it probably picks on it inordinately because it's not accounting for all the other doses. So let's say we did an epidemiologic study in a population. I mean, do we account for background plus these incremental environmental doses? Radon. I mean radon's a very big contributor to dose. Particularly in some areas of the country. So cutting out the dose from the event is a little bit cleaner on having a metric -- I use metric carefully -- as opposed to trying then multiply it by a risk factor where there's a lot of other things that contribute to that same risk that you're not

accounting for. You with me?

MR. SULLIVAN: I am. But -- actually, I'm not trying to argue. I'm attempting to get the benefit of the committee's views.

CHAIRMAN RYAN: Absolutely.

MR. SULLIVAN: Really.

CHAIRMAN RYAN: And we appreciate that.

MR. SULLIVAN: And I'm sorry if it comes across differently, but every step of this project is the staff's best estimate. Every step. We chose -- well, we didn't choose -- the Commission directed us to choose a ten to the minus six cut off on core damage frequency. We had developed models based on expert opinion. We have assessed mitigative measures in a way that is the staff's best estimate. We have looked at operator error in the same way. We have looked at how containment fails on the basis of the staff's best estimate.

I'm attempting to use that same modus operandi in the LCF area. I'm suggesting that we're considering using that same --

CHAIRMAN RYAN: And I think what we're telling you is taking the dose and multiplying it by a cancer risk estimator is not a best estimate. You

don't account for background, you don't account for variability of background, you don't account for age dependence. Potassium, for example, age dependence is critical to thyroid cancer induction and so forth.

MR. SULLIVAN: Yes.

CHAIRMAN RYAN: How do you account for all that? And if you don't account for it, it's not a best estimate. So those are things you know you can account for. So I think what we're doing is, we're not arguing the best estimate approach all the way through. I think just using that simple multiplier of dose times risk factor for latent cancer gives you a number you can now examine.

MR. SULLIVAN: Yes.

CHAIRMAN RYAN: We're thinking that's -- and we've thought about it in other contexts -- is maybe not as risk informed as it might be.

MR. PRATO: Yes. Just a point of clarification. Risk communication isn't communicating of risk information. In this application, it's communicating of any detailed, technical information in a manner in which you can get a common understanding with all the stakeholders. And when we looked at the range, and we looked at past practices,

when we reported multiple results for a single sequence, we don't get that. We don't get a common understanding. We get certain people who would prefer to use the more favorable results, which may be low, to use staff results. Those people who think the more favorable results are the high results, they use those results.

That's what initiated this. We wanted to report SOARCA in terms of one result for each sequence. If we were to turn around report four results and say, "The staff favors this result, because we believe that this is our best estimate," then why did we bother reporting?

CHAIRMAN RYAN: How about this as an idea. This is maybe out of the box thinking, but if you reported a stratified table of doses, this percentage of the exposed population in these sectors by miles out or however you want to do it, received -- 500 millirem to a rem, and 100 to 500, or less than 100. Aren't you accomplishing that single picture?

MEMBER WIENER: Could I ask --

CHAIRMAN RYAN: You don't have to answer that this minute, but that's an alternate view that takes out all this complexity of trying to turn that

very clear dose calculation into a stratified estimate of fatal cancer risks. I'll give you a simple example. In the fatal risks, do you account for smokers and non-smokers?

MR. SULLIVAN: We have no -- I got numbers.

CHAIRMAN RYAN: See, that's my point. And so the best estimate part, unless you tested and vetted those factors, you can't guarantee that's the best estimate. That is one estimate. But the dose part, up to that point, I think -- a lot of work by this agency and other agencies have gone into those kind of dose calculations. That may be something to think about as maybe a fourth version of how to look at things. And stratify it, so you can see what the categories are. Then those kind of things can be prepared.

MR. SULLIVAN: How would the public interpret that, though?

CHAIRMAN RYAN: I don't know. I mean, that's something that can be the next step. What I think we're focused on is -- and I appreciate your willingness to come back and lay out the calculations in a little bit more detail for us. What are the technical calculations you're doing, where do we think

we agree with you on your using the best estimates, and your using risk-informed techniques to get those estimates, and where do we think they may not be so risk-informed? I think Allen and I are expressing the view that when you use these cancer risk calculation numbers, we're raising a question mark at this point of how those are risk-informed, and where they come from. So with that, Ruth?

MEMBER WIENER: I wanted to, first of all, say that I think the committee -- Allen has made my point very well. But I'd like to add to it, and respond to what you said about risk communication. When you report latent fatal cancers, no matter how small the number is, in comparison with any other number, what the non -- relatively less-informed public takes away from this is, NRC says that this accident is going to give you cancer. That's what they take away. That's what you're communicating. And one of the problems with communicating in terms of latent cancer fatalities is that that is what the public hears.

And you're sending -- the public does not say, "Oh yes, but I'm way more likely to get cancer from smoking cigarettes, or from, you know, getting my

teeth x-rayed, or whatever." The public says, "Yes, this accident, which is a horrible accident, Chernobyl, is going to give me cancer. And how do I know that my Aunt Susie's cancer did not come from this accident?" There is a real risk in reporting that way, and I would second, whole-heartedly, what the Chairman just said. Doses are reported everyday in the popular media. Rem is defined in Webster's Collegiate Dictionary. People are used to seeing dose. You're not talking an arcane language here.

I think the Chairman made an excellent point. If you reported a table of doses --

MR. SULLIVAN: And we'd still move the population -- you mean people doses, not fence-post doses?

CHAIRMAN RYAN: Sure.

MEMBER WIENER: Sure.

CHAIRMAN RYAN: You know, we'd have to, I think, understand more of the details of how you've done it by sector or group or time dependence and those kind of issues that give you any further insight. But just that general idea that dose is where you end the thinking, or end the reporting, might be something to think about as an alternative.

And again, please take that as just a dialog at this point. We're going to continue the conversation with further presentations.

MR. SULLIVAN: We appreciate that.

CHAIRMAN RYAN: Professor Hinze, do you have any questions?

MEMBER HINZE: Well, I had nothing to add on dose threshold. I think my colleagues have covered it well. I'd like to ask a question on the process, if I might.

CHAIRMAN RYAN: Sure.

MEMBER HINZE: And that is, I understand, Bob, from what you said that you have eight classes in the selection of your test cases.

MR. PRATO: Yes.

MEMBER HINZE: And I assume that those are eight classes of reactors. And my question is, have you considered population demographics, weather conditions, meteorological conditions, etcetera, in selecting these eight so that you have covered really the breadth of conditions.

MR. PRATO: Meteorological distribution, I don't think we've really considered that.

MR. SULLIVAN: See, each site that we

analyze will have site specific meteorology used.

MEMBER HINZE: Sure.

MR. SULLIVAN: And so we'll draw from their 8700 --

MEMBER HINZE: But there certainly are areas of the country that have much different meteorological conditions --

MR. SULLIVAN: Sure.

MEMBER HINZE: On an average basis as well as on a vary time-specific basis. And those may enter into this whole process in a very real manner. And I'm just wondering if you've really given a complete view of the accident scenarios.

MR. SULLIVAN: Right. I think I got your point, I think it came through. The answer would be no. But walk with me for a second on this. We had to choose the eight classes of plants based on the technologies. We tried to -- if we get our -- well, the eight classes of plants are our way to slice the technology. We tried to parse that in terms of population density. You know, we don't want all low-pop sites. We want low pop, high pop, medium pop, if we can get it.

MEMBER HINZE: Yes.

MR. SULLIVAN: We have not looked at the weather question, as you proposed, but if the Commission directs us to expand this study out to all sites, we would use site specific weather at those specific sites. So we would encompass that if we get that far. I mean, we may -- it's up to the Commission.

MEMBER HINZE: Right.

MR. SULLIVAN: We may stop at two, six, or eight. But if we go to the full fleet, then I think we would adequately address your question. But no, we weren't sophisticated enough to look at the eight and parse for weather. And some have sea breezes, you know, others don't. So I think I get your point.

MEMBER HINZE: And in the in meteorological conditions, have you considered severe climatic conditions? Tornadoes -- what happens if one of these accidents during a tornado? Extreme conditions? Is your probability analysis including those tails?

MR. SULLIVAN: Likely not. We're using real weather from a given year.

MEMBER HINZE: I'm from Indiana, and we have a lot of tornadoes, and we don't have many nuclear power plants. I think severe weather

conditions are one of the things which impose themselves upon your analysis.

CHAIRMAN RYAN: If we get this follow up, we can get some of the details.

MEMBER HINZE: Right, right. I'm just trying to think of -- these are some of the thoughts that ran through my mind as you were making your presentation. Of just what might be the concerns here.

MR. SULLIVAN: Yes. Interesting.

MEMBER HINZE: Thank you very much.

CHAIRMAN RYAN: Some other examples, Bill, that I know are more realistic but maybe not as realistic, a couple of plants in Florida have dealt with hurricanes. Jim?

MEMBER CLARKE: I was kind of going to go there to, wondering if each of those eight classes had sub-classes for different site specific conditions. Your more realistic offsite dispersion model, is this a model that you built? Or is this a better model that somebody else has?

MR. SULLIVAN: No. We now have -- we can dissect the plume more. We have more sectors to account for. Wind variation. And of course we're

using site specific meteorology.

MEMBER CLARKE: Okay.

MR. SULLIVAN: So we think it's a better model.

MEMBER CLARKE: So the model you used before has been improved.

MR. SULLIVAN: Yes.

MEMBER CLARKE: Okay.

MR. SULLIVAN: Better model than it used to be. It still doesn't do everything we'd like it to do. It does have the terrain factor. Not complex terrain. But it does have a terrain roughness factor.

MEMBER CLARKE: All right. That's all I had.

CHAIRMAN RYAN: Thanks Jim. We had a request from Mr. Ed Lyman from the Union of Concerned Scientists to make some comments.

MR. LYMAN: I'm Ed Lyman from the Union of Concerned Scientists, and I appreciate the opportunity to make a few comments. I didn't prepare remarks in advance, mainly because there's so little public information yet about a number of the details of the SOARCA program. And to that end, I appreciate that this meeting was open to the public, and I think

everyone can appreciate that the discussion here was not something was appropriately withheld at this point, since it seems to be just a free exchange of discussion of scientific approaches to this program.

As the only member of the public here, I believe, I'd like to address the risk communication issue. And in our view, the best way to communicate with the public is to present an honest assessment of the scientific data and uncertainties including different approaches to discussing the concept of the consequences of a severe accident. And so to that end, I don't think it would be necessarily confusing to provide multiple cases, as long as each one was appropriately explained and the scientific basis for each case was presented with appropriate peer review documentation.

So for instance, we don't believe that there is peer reviewed documentation to support at this point using thresholds for radiation protection purposes. We have the outcome of the BEIR VII study, and people had the opportunity to convince the panel otherwise, but they were unsuccessful, so right now you are faced with an international radiation protection community and the recommendations of

agencies that there should be no threshold. So if you are going to run calculations with thresholds, you need to either document why that number would be appropriate with peer reviewed scientific evidence, or explain why that isn't available.

And I appreciate that you're planning external peer reviews, and I might suggest that you might seek actually submitting a summary of your methodology to a journal like Science Policy Forum or something. That, I think -- going through a peer reviewed process like that would add enormous credibility in the eyes of the public to what you're doing. But I don't agree with the approach of trying to couch the results in a way so that you don't frighten the public. You should be honest about what you're providing.

And to that end, there was one thing I heard that was troubling. I do appreciate that there seems to be an in-depth discussion on these issues, and you're not just responding automatically to the Commission's dictate, but I did hear that you said that you're going to report the mean consequences of weather trials, and as someone who's very familiar with these MAX -- I've run the code for years -- and

is also familiar with the political history of the CRAC2 study, I'd like to maybe remind people that what made CRAC2 front page news back in 1982, was not the results, per se, but the fact that the public results were only the mean of the weather sequence distribution. And that someone leaked to the press the fact that there was actually an outlier, and the maximum consequences, which were the order -- at least one order of the magnitude greater than the mean.

It was that fact that lead to making the front page news, as opposed to something that was buried. So you might want to consider that in recording results, and don't simply include mean consequences that also discuss some of these sensitivity cases for other, more severe weather related trials. So again, I do appreciate it. I look forward to more open discussions of these matters in the future, so the public does have the sense that you aren't cooking up a study to give the public only selective bits of information. Because then you're going to go through a three year study and spend a lot of money and that's something that the public really is not going to accept. So those are my comments.

MR. SULLIVAN: Could I ask you a question

or two?

MR. LYMAN: Yes.

MR. SULLIVAN: Thanks so much for your comments. I just want to reiterate -- I just want to make very plain that the staff is being honest. This most complete analysis we've ever done. It is the staff's professional work, and our reporting will be honest. It will be the staff's opinion. And there's no attempt to hide these consequences. What about the suggestion of risk to the individual? Is that something that can be communicated to the public with success? Maybe we should do that too.

MR. LYMAN: You run MAX and you do find you can generate the individual doses to people. Now I guess if you're more sophisticated, spacial modeling is a function of the evacuation pathways. I think as an additional data point, why not? Actually, I find when I run MAX -- and I admit, I've used scenarios involving early containment failure that you seem to be excluding, which is one issue I'd like to take up separately with ACRS. But the individual doses, especially thyroid doses to children, are quite significant in themselves.

MR. SULLIVAN: Well, it would look

something like an individual living at five miles has a personal risk of ten to the minus something number -- is that meaningful?

MR. LYMAN: That would be multiplying by the CTF, you mean?

MR. SULLIVAN: Yes, sure, why not? Well, and the ten to the minus four per rem --

MR. LYMAN: You can't use that method. That's population factor. That's not individual.

MR. SULLIVAN: Okay, so --

MR. LYMAN: Well, you can -- if you can calculate the individual dose, either the whole body dose or thyroid dose of an individual at a certain distance and say -- well, using a linear coefficient, you can say that that would lead to a ten to the minus four or a ten to the minus five. That's, in fact, the way the safety goals were written.

CHAIRMAN RYAN: If you start with a dose, whatever context you're in, whether it's this calculation or any other, that's a better line in the sand from which to then say, well what does that mean? And there might be different ways of assessing the meaning of that, whether it's a group, a child, an adult, a healthy adult, a person with a lot of radon

exposure. You know, all those other kind of things can then be assessed in the context of a given dose from a given activity.

MR. LYMAN: Yes, but in general, I think more information is better than less, and I think you have to be afraid of overwhelming people if it's poorly presented. I don't see any reason why you couldn't report --

MR. SULLIVAN: What about the context issue?

DR. BAHADUR: Whether more information is used or less is debatable, and depends on the context. Sometimes more information may cause more confusion. As the discussion shows, this is not an easy -- if we were to make a regulatory decision, we would have taken the most conservative approach, which we have taken, and would have gone LNT. This is the object where the staff is trying to come to a best estimate for a particular project.

There are a number of ways by which it could be done, and there are different values each time you take a certain approach. Which approach needs to be taken is not an easy answer, and that's why you overheard the discussion that you have. The

idea is not to couch the results, as you said. The idea is not to hide from the public, as you implied. The idea is to get to the best possible estimate the staff can make, based on the best scientific information which is available to us.

There is an ICRP and NCRP recommendations, there is the Health Physics Society, and there are other professionals who have come up with various opinions on this issue. And the staff is looking into those issues, coming up with the best estimates, presenting it to the Commission, and eventually to the public. The idea of keeping the results of SOARCA away from public is not the issue. The idea is when to make it public. Sometimes it is better to complete the study and then show the results, rather than giving the results at every step of the way.

MR. LYMAN: Well, I guess I would respectfully disagree with that.

DR. BAHADUR: So maybe it's a debatable issue at a different forum.

CHAIRMAN RYAN: I think we've agreed that we're going to have a follow up briefing that probes some of these details a little bit more fully. And I think that'll be helpful. That'll help our discussion

with you, and it will help us shape views that we can then offer to you down the line a bit. So I appreciate the fact that we've had a good discussion this morning. Thank you very much for coming, and we appreciate your further participation down the line. And we've got a good discussion of your views, and certainly a good start on what will hopefully be a long, productive conversation. So with that, unless the members have any additional followup questions --

MR. FLACK: This is John Flack with ACNW staff. I realize this is not a risk assessment but a consequence analysis, but I'm trying to understand how much risk you're actually capturing. Because you are screening at ten to the minus six and ten to the minus seven. Thank you.

MR. SULLIVAN: Right.

MR. SHERRY: Richard Sherry, research. It's true that directions in the SRNs for performing this project did not direct us to capture some fraction of risk associated with operation at any of the subject plants. We did as sort of a site calculation have a recent level two analysis results from one of the plants, and we looked at the sequences we selected. And we believe we captured, for at least

that plant, the risk significant sequences, okay? We didn't have that information for the second plant, so we can't make that statement, okay? And that's sort of the best information I can give you about whether we captured the risk dominant sequences using the frequency threshold that we were directed to use.

CHAIRMAN RYAN: Thank you. Any other questions? Again, I think it's helpful that we're involved with you early in this process. You're clearly in the midst of a work project. You're not presenting a final work product, so this early dialog, probably we're raising, I think, more questions than we might have started with today. I think it's ultimately going to be productive. And I hope you feel the same.

MR. SULLIVAN: I do.

CHAIRMAN RYAN: Because it's helpful for us, and I think the dialog maybe can further enrich your analysis in your work, and hopefully we'll learn more to further advise the Commission as well. So with that, hearing no other questions, I'll close this open session.

(Whereupon, the above-entitled matter was concluded at 10:20 a.m.)