UNITED STATES OF AMERICA

NUCLEAR REGULATORY COMMISSION

***

PUBLIC WORKSHOP TO DEVELOP AN SRP

FOR DECOMMISSIONING

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U.S.NRC

11545 Rockville Pike, Auditorium

Rockville, MD

Thursday, March 18, 1999

The above-mentioned workshop commenced, pursuant to notice, at 8:34 a.m.

P R O C E E D I N G S

[8:34 a.m.]

MR. HOLONICH: Why don't we go ahead and get seated and start the program today. Folks are probably wondering who this strange face is standing up here, and I figure this was a good opportunity to come down and introduce myself.

My name is Joe Holonich, and I'm the new deputy director in the Division of Waste Management. I was appointed to the division late last year. I come from the Uranian Recovery Branch of the division, where I managed that program for about five years, and as a result of a reorganization taking effect and some movement of folks, I was moved into the division slot, so I've been there about four months getting acclimated to a number of different things that the division does beyond uranium recovery activities, one of which, of course, is decommissioning work.

So I wanted to take the opportunity to come down and introduce myself this morning, and Nick asked if I would make the opening remarks today, so I'm going to go ahead and do that.

Essentially, we're here -- this is the third of six workshops on the NRC's standard review plan development process outlining our reviews of decommissioning activities under the license termination rule. The earlier workshops we had were in December of '98 and in January of this year, and those two workshops covered dose modeling and restricted use.

Today, we're going to look at dose modeling as well as complying with the ALARA provisions in the license termination rule.

I would like to start off by thanking all of our speakers, both from the industry and government agencies. I think they're going to have some very interesting discussions just based on the information Nick has given me as background to outline what's in the agenda this morning.

I would also like to thank Paul Genoa from the Nuclear Energy Institute and

Dave Colbersome from the Fuel Cycle Facility Forum for helping the staff get together and put the speakers together. Their help was invaluable in us being able to put together this agenda and outline the topics and the discussion we're going to have today.

What we're going to do is start off with two days, and this morning, essentially we're going to give you an update on the D&D screening model and sources of input parameters, and that will come from Walt Beyler -- is that how you pronounce it? -- from Sandia Lab.

We're going to follow that with some talks from Beverly Good of GPU and Bob English from CE on partial site release, and Joe Nardi from Westinghouse and Boby Eid from the staff will discuss conservatism and re-suspension factors.

The afternoon, Carol Hornibrook from EPRI is going to report on her work on the modeling imbedded pipe, and Boby Eid and Man-Sung Yin are going to discuss modeling of cesium and strontium.

Tim Harris from the staff is going to talk about modeling of former burials using RESRAD and D&D. Dick Sexton from Connecticut Yankee and Henry Morton will discuss problems with measuring alpha emitters, and then Claude Wiblin from WEM is going to discuss how to calculate indoor area factor.

Tomorrow afternoon, we're going to look at ALARA in a little bit of a different format. Chris McKenney from the staff is going to handle an overview of the ALARA provisions in the license termination rule, and we're going to have a roundtable exercise which steps through draft Reg Guide 4006 and the methodology in there for demonstrating that doses are ALARA.

After lunch, Chris is going to have a group discussion on when during the decommissioning process, you need to address ALARA. So it's going to be a little different format tomorrow than what we have today.

There are a couple of administrative things I want to mention. First is that the

workshop in June has been moved back to June 23rd and 24th. That's because this auditorium was taken the two days that we -- at least one of the two days that we needed it for the workshop on the 18th and 19th. So we need to move that back.

We recognize that's during the Health Physics Society summer school, but we wanted to get that workshop in before the annual Health Physics Society meeting and everybody went on summer vacation. So we've moved that back to the 23rd and 24th of June.

The June workshop is going to focus on modeling groundwater and is going to be held in conjunction with work that our research staff, in particular Tom Nicholson, is doing, and his workshop on the 22nd -- 21st and 22nd of the month on the same topic. So you have a number of days both on the research perspective and the program office licensing perspective on groundwater modeling.

Copies of the draft agenda are available in the atrium, so if you haven't got one, you can certainly pick one up for that workshop.

We're working on topics for August 18th and 19th. What we're thinking about is the first day, we would discuss comments we got on the draft standard review plan and go through what we see as those comments and what our responses are or what we'll be doing in the draft review plan or the final review plan. Also talk about comments we got in the D&D screen and on the draft Reg Guide EG4006.

We're thinking about maybe spending a day on issues associated with surveys. We're going to discuss the agenda for that workshop around 4:30 this afternoon, and what we wanted to do was kind of give you some preliminary thoughts of with our thinking was as possible agenda topics, and you guys can mull that over during the day, and then as we get into the 4:30 discussion, maybe you can add to it or help us focus better on what are those things we want to look at.

We would also like to add that the speakers from the industry, if they could come up and sit at the table and kind of sit around that side, that will help get a good mix up

here at the table rather than just the staff sitting at the table. So if the industry speakers would come up and sit around the table, that will be definitely a help.

You've got a full day today. Unfortunately, I can't stay for the workshop, I've got other commitments, but I did want to come down and say good morning in introduce myself. Unless you have any questions, I'm going to turn it over to Nick and let him get started on the workshop.

Okay. Well, welcome, and hopefully you have a very good day. I think you've got some interesting topics lined up. Thank you very much.

MR. ORLANDO: Thanks, Joe.

As Joe said, the first speaker is going to be Walt Beyeler from Sandia, and as soon as I get Walt wired up, we'll go ahead and get started.

MS. BEYELER: Good morning. My name is Walt Beyeler and I'm with Sandia Labs in the Environmental Risk and Decision Analysis Department.

We've been supporting NRC in the development of their decontamination and decommissioning program for a number of years. Specifically, my involvement has been in the analysis that was used to set default parameter values for version 1 of D&D. I have recently been working with Teresa Brown in the development of version 2 D&D, and this morning, I would just like to give you an update on how that's proceeding.

The project is really just getting started, so there is no progress to report. But what I would like to do this morning is to emphasize key differences between version 1 of D&D and the forthcoming version 2 which is sometimes referred to as Monte Carlo D&D.

I understand that Teresa presented in an earlier workshop some of the technical underpinnings behind both the parameter analysis and version 2 of D&D, and so what I would like to do this morning is focus on the operational differences, what the user would see different between version 2 and version 1, what practical advantages we believe is offered in version 2.

Basically, the new features will be added in version 2. I hope to implement

some additional steps of the decision framework that's defined in 1549. Not all of the steps in that decision framework are -- will be supported in version 2, and I would like to discuss just a couple of the limitations associated with version 2 in that connection, and then go over the schedule with you, explain when capabilities are anticipated.

The essential difference between version 1 and version 1 D&D is in the way the parameter uncertainty is treated. I should point out at the outset that that difference for a person who's doing a screening calculation is behind the scenes. A user would not notice that difference in the way the parameter uncertainty is treated. It really comes into play for users who are moving beyond the default screening calculation to do a site-specific calculation.

MR. EISENBERG: Your slide's off the screen.

MS. BEYELER: Oh.

MR. ORLANDO: Why don't we move the table back.

MS. BEYELER: I think that's got it. Thank you.

But in both cases, we're interested in making a dose calculation that can be used to make a regulatory decision if we don't have specific information about the site parameters.

The underlying idea is to look at the range or distribution of possible doses that might result from using site-specific parameters and to calculate a dose value that is appropriate for decisionmaking given the range of possible dose values.

The approach that's taken in version 1 of D&D is to use a specific deterministic set of default values that produces dose values in the upper end of that range for every source term, for every source nuclide that can be specified as input to the code.

The approach that's taken in version 2 of D&D will be to build that dose distribution directly given the specific input, the specific source term that applies for that site.

Again, that difference in the way the dose values is calculated is invisible to the user who's doing a generic screening calculation. In the case of version 1, there is a single

calculation using the default parameter values. In the case of version 2, there will be a set of Monte Carlo calculation that uses the default parameter distributions.

But the user will see basically the identical interface, will require the identical input, a source term for their site.

What they will potentially notice is less conservative results in the case of version 2, the reason being that the default parameters in version 1 are required to produce a screening does value in the upper end of the dose distribution for every conceivable source.

In version 2, that value is specifically tailored to their site source term. Satisfying the condition in version 1 means that the screening dose value may be above the set threshold for some sources. In the case of version 2, that additional conservatism that comes from having to satisfy the constraint for every source does not exist, and again, the important operational difference is that the user who's doing a simple screening calculation would see a lower does that does not include that additional conservatism.

They may also notice additional execution time. Again, version 2 will be building a dose distribution based on their input source term. That will require some hundreds of calculations rather than the single calculation that is done now. However, the dose calculation is computationally very fast, and so there may be some noticeable execution time as opposed to instantaneous execution time, existing version. But the intention of the design is to introduce very few changes for the user who's doing a simple screening calculation.

MR. GENOA: Walt, will you entertain questions now or do you want us to wait until you're done?

MS. BEYELER: Well, I guess let's --

MR. EID: I believe, yes, we have open forum as long as the questions are not too long, they take a long time, you go ahead and make the question.

MS. BEYELER: That will be fine.

MR. GENOA: Paul Genoa with NEI.

Walt, you've mentioned several times a source term parameter. Are the parameters also other parameters that would affect things like groundwater infiltration, TEDE values, all those other things, aren't they also affected by this? It's not just the isotope-specific issues, are they?

MS. BEYELER: I'm sorry, the -- I guess I'm not quite understanding your question.

MR. GENOA: In the deterministic method, if I were to -- you've determined groundwater or, excuse me, groundwater infiltration is an issue, and you have determined at the 95th percentile how fast that groundwater will move through. And in the new version, there might be distribution of the speed with which that would move through. Is that accurate?

MS. BEYELER: That's correct.

MR. GENOA: Okay. I wouldn't call that a source term. I guess that's where I was getting confused. When I hear the word source term, I'm thinking of radiological issues.

MS. BEYELER: Okay. No, I'm sorry. The issue with the -- or the -- where the source term considerations enter into it is that the default parameter versions in the current version of the code -- the default parameter values must -- are required to produce a dose value that is in the upper end of the dose distribution, and a separate dose distribution is calculated for each individual nuclide that can be specified as input. So there are -- the parameters values are required to satisfy some 60, close to 70 constraints. That is, they're required to produce high dose values for close to 70 individual nuclides.

The version 2 code will not -- the calculation will not be required to produce high dose values for every conceivable source term, rather for the particular source term that is specified for in the site.

MR. GENOA: Walt, can I take a stab at trying to clarify that?

MS. BEYELER: Yes.

MR. GENOA: The problem we've got right now, the current version of D&D has

a single set of default parameters, and that single set of default parameters is based on all radionuclides, and so as a -- because it's based on all radionuclides, you have some conservatism built into it because you, as Walt said, you've got multiple solutions that you can actually -- there's multiple solutions to the criteria, and you're picking a single set of -- single set of parameters, and that's causing some parameters to be extremely conservative for certain radionuclides.

What the Monte Carlo version is going to do is you can put in a -- you can put in your source term, and then it's not tied to other radionuclides, it's for that particular set of radionuclides. I don't know if --

MR. EID: This is Boby Eid. I would like to add more explanation. I understand your question regarding the other parameters like KD values, for example. Walt talked about the radionuclide, the specific radionuclides, and the question was, how about other KD values, I believe, other physical parameters.

I guess that are two parts in the conservatism that is there in the current version. One part is concerning the source term that is there. So because the source term considers all radionuclides, so there is one single default parameter for each physical parameter is there, that is conservative, that makes it conservative, and the PDF, it is above 90th percentile for all radionuclides. That's one part.

This means, you know, you have to get X more dose because you are considering the possibility that other kinds of radionuclides may exist at this site. It could be, you know, a billion, could be certain radionuclides not related to your site completely. So that's one part of it.

The other part, these parameters -- these parameters -- also, they do correspond to all these radionuclides like, for example, KD value. The KD value, if you look at the value that's selected, maybe it does not represent the 90th percentile for all radionuclides, could represent higher than the 90th percentile for that specific radionuclide that you are

interested in, because also should accommodate the other radionuclide that may not exist at your site because the screening tool for all kinds of radionuclides.

MR. GENOA: I guess my question is this. An example. Let's say that soil pH has an effect on the mobility of certain isotopes. Does this code artificially say that, well, for a certain isotope, it will migrate best in acidic conditions, so therefore, the input parameter for pH is acidic. However, on another isotope, the most -- it's most mobile in a basic environment, so therefore it's basic. So for one site, we're going to have an input parameter for ph that is both acidic and basic, and to me --

MR. EID: No, in this case, we are talking about site-specific conditions. So I believe currently the Code, you know, does not accommodate detailed site-specific conditions, and there is a question how you input the parameters based site-specific conditions for the current code.

For Monte Carlo version, which is version 2, so it will accommodate site-specific conditions, you may input KD values based on the condition that they pertain to your site.

MR. GENOA: Thank you.

MS. BEYELER: I'm sorry. There is -- the situation that you've described, there is a requirement -- the default parameter values must be the same for all nuclides. So the example you cite with pH, that's not an input parameter. But yes, for a soil moisture content, for example --

MR. GENOA: Okay.

MS. BEYELER: -- it is the same value of soil moisture content for every nuclide, and there is a, again, a common set of default parameters for all nuclides, and it is that requirement to find a common set of parameters that introduces this additional conservatism because the default parameters must produce a dose value near the upper end of the range for, say, a tritium source, for a cesium source, for a uranium source, for thorium source. For each potential source individually, that single set of parameters must produce a dose value near the

upper end of the distribution, and it is that requirement that there be one single set of parameters that adds this additional conservatism. The version 2 will make a specific calculation for your specific source term, so that the requirement that that same combination of parameters work for every source is not met here.

MR. GENOA: Thank you.

MR. BEYELER: Again the intention was to have in Version 2, something that is operationally indistinguishable from Version 1. There are some additional features for people who are moving towards a site-specific analysis that will be available in Version 2.

This is a list of some of those key features -- the ability to look at the distributions of parameters and to call up the text from NUREG-5512, Volume 3, or other sources of information that may be used to define those distributions -- the ability to look at the output distribution of dose and to look at individual pathway doses and peak arrival times, the capability to look at the parameter sensitivity, which parameters tend to be driving the dose distribution, and additional flexibility for modifying the input parameters in addition to the Volume 1 provision for specifying single values for parameters, Volume 2 because there are two -- dealing with the parameter distributions will allow the user to change the endpoints of those distributions, specifying modified distributions for those parameters.

There's also the source term as far as the model is concerned is simply another parameter. The existing version of the code requires a single number for the source term parameter as it does for the other parameters.

Version 2 can allow a distribution of source terms, so that there is a consistent and more comprehensive representation of the uncertainty in dose.

So what I would like to do now is just show you some proposed interface elements that will be available in Version 2. What we are intending to do is to take the new interface elements for Version 2 from an existing code SEDSS and so what I have here in the next three slides are some snapshots of screens from the SEDSS program. These are similar

but not identical to the D and D Version 1 screens but the information content is the same.

Is that -- I am not sure that that is quite legible. This is an illustration of how the parameter distributions might be displayed. The background panel, which is grayed out, is where individual parameter values are specified. Again this is for the SEDSS interface. The D and D has a similar interface where particular parameter values are tabulated and the values that are assigned to them can be entered here.

In Version 1 constant values can be specified for each of these parameters. Version 2 will retain that capability but then allow the user to pop up this overlaid display, which will show the underlying distribution for the parameter. It will allow the user either to assign different functional forms to that distribution and change the parameters of that distribution, again access the text that would support that particular distribution.

MR. EID: Walt, I have a question.

MR. BEYELER: Yes.

MR. EID: These numbers, they do not correspond to a D and D model, right, because the thickness of the Vadose Zone is very high, right?

MR. BEYELER: Yes, sir. Again, these --

MR. EID: So those are just demonstration, how you change the numbers?

MR. BEYELER: That is correct.

MR. EID: They are not real numbers for D and D model?

MR. BEYELER: That is correct. These screens again came from the SEDSS program, which has similar but not identical groundwater and exposure models in it, so these are sort of example displays showing the sort of information, the organization, the information that will be available in Version 2, but these are not literally the D and D screens as they would appear.

This next display shows an example of how the output dose distribution might be viewed. The left-hand panel shows a cumulative distribution function for dose. Again the

particular values here and the parameters are for a separate code, so don't focus on that, but rather the information displayed here is for different dose levels. The probability that the dose would be less than that value -- this is a cumulative distribution function.

There would be options to display the same information as a histogram of dose to select either the total dose or doses from individual pathways or to look a the distributions of peak arrival time.

There is also provision for looking at the sensitivity of the dose to particular parameters. On this graph the doses above a particular value are highlighted in red. The display on the right shows the distribution for a parameter that was used in this calculation, and in that display the values of the parameter that lead to calculations on the upper end of the distribution are similarly highlighted in that distribution, and so that sort of gives a quick visual indication as to whether there is a tendency for this parameter to control the value of dose.

If, for example, all of the parameter values of this part of the distribution were red and there were no red values in this part of the distribution, there would be a quick visual indication that that parameter is very strongly related to dose, and the same sort of comparison could be made either for the total dose or for individual pathway doses.

This next slide shows a little bit more about how that sensitivity information can be used and displayed. This just shows a set of distribution functions -- four input parameters, again to the model that is used in SEDSS. These don't directly correspond to input parameters to D and D -- just an example -- but each of these shows the distribution for the parameter values, again with the samples corresponding to high dose, highlighted in red, and it is also possible to look at the correlation basically between dose and parameter value and to highlight those parameters that have a significant correlation dose, and that is what is done in this display. The lighter backgrounds have been identified as being sensitive, so that as you can see in the case of partition coefficient for cesium for example low values of that partition coefficient in this calculation corresponded to high values in dose.

So again as far as moving towards a site-specific analysis, the real advantages in Version 2 are in the ability to identify sensitivities of dose to particular parameter values to get an understanding of what ranges of parameter values might lead to substantial revisions in the calculated dose, but there are other elements of the decision framework that are not, that will not be automated in Version 2 and there are important considerations in making data collection decisions at sites.

Specifically in looking at what kind of data collection might be worthwhile at a site it is not just enough to look at the sensitivity of the dose to the parameter but the practical considerations -- what experimental programs are available for collecting data, how likely are those programs to succeed, and what sort of changes in the parameter value are practically likely, what are the comparative costs for collecting different types of data, so the sensitivity information that will be available in Version 2 will be helpful but it is not all that is needed in order to make an optimal decision about data collection activities.

Also, the types of options that are contemplated in Version 2 cannot include revision of the D and D model because the code implements and is built on the default models in D and D, and so that is also a limitation of Version 2 with respect to more comprehensive implementation of the decision framework.

The second figure is really just there to emphasize the same point. If the screening calculation determines that there isn't enough information now to release the site, then the next step would be to define the options that are available for data collection, remediation restricted use, and to analyze those in terms of the cost of the time that it would take to complete them and the likelihood of success.

Again, Version 2 will assist in doing that analysis, but will not perform certain elements of the cost analysis, the likelihood of success.

The types of options that are contemplated can't again look at modified pathway models, more elaborate pathway models, simply because Version 2 is built around the default

models in Volume 1.

Just to quickly go over the schedule again. We are just sort of practically getting off the ground on this, so these dates are assuming an April 1st start. A month from then we are to have the requirements defined in the letter report to the NRC. The beta software -- again we are using a lot of preexisting code, combining that with preexisting interface elements for D and D so we expect that development to happen very quickly.

We will deliver a beta version of the code to the NRC in September -- sort of concurrent with the development of the code we will be working on the development of test plans and we will do some experimental implementation of the test plans between September and November 1st.

Revised documentation will be submitted to NRC in June and then the final document, revised documentation, in August of next year. I spoke with the NRC Program Manager about this and she emphasized that she is interested in distributing our beta version to users for their evaluation as soon after the September 1st date as is practical, and so I think the intention is to have some intensive NRC review to make sure that it doesn't crash anyone's machine, but then to try to get that out for your evaluations as soon as possible after that.

Are there any questions I can answer?

MR. EID: Thanks, Walt. I guess the floor is open now for any questions.

MR. MORTON: Yes. Henry Morton. Will the Version 2 be, if you input a distribution of nuclides or a mix of nuclides, will it generate the concentrations at the time of maximum dose or at the time at which 25 millirem is -- that is the mix that corresponds to 25 millirem?

MR. BEYELER: Well --

MR. MORTON: If, in short, the existing version seems to work in such a way that if you put in a source term you get the dose, but will it in effect derive in reverse?

MR. BEYELER: No, sir. It will not. It will not tell you the source concentration

for a particular mix that corresponds to a dose of 25 millirems.

I would say it would be easy -- well, you can get that information if you have a particular mix of nuclides and calculate the peak dose and then scale in effect the peak dose, 225 millirems. That same scaling factor applied to the mixture as a whole should give you a 25 millirem dose.

However, if there is some uncertainty about the combination of nuclides in that mixture, then that becomes a much more complicated problem and in any case the code will not be designed to give you the possible combinations of nuclides that would lead to a 25 millirem dose.

MR. MORTON: It would seem useful to have something of that kind since in the field you need to measure concentrations. You need to have some kind of -- you need to know beforehand what those concentrations would be in order to design and execute the cleanup.

MR. BEYELER: Yes. If I can, I think the complication is that there is more than one value of concentration. It is not a unique problem and so there are many combinations of concentrations amongst, say, two nuclides that would -- each combination would lead you to 25 millirems, and so I would say it is not a unique problem.

MR. EID: Mark?

MR. THAGGARD: Well, to answer his question, couldn't you put in like unit concentrations and back-calculate what the equivalent -- I mean what would be equivalent to 25 millirems by using the unit concentrations? I mean you know what the criteria, assuming you know what the criteria would be like, say you are trying to achieve like a 90 percent confidence that the dose isn't going to exceed 25 millirem, and so you know what the confidence is. You use unit concentrations, it seems like you should be able to back-calculate that fairly easily.

MR. MORTON: Yes, but the problem there is that the dose for different nuclides doesn't all occur at the same time, so if you put in unit concentrations and then mix them, you then overestimate the dose and therefore force a lower residual concentration that is allowed, so

if we are going to clean up we need to know beforehand -- and this seems to force one to clean up or do the calculation to see if you are clean and get -- the field people need some knowledge, some information about what concentrations they need to clean up.

MR. BEYELER: Well, can I ask, in cases where you have multiple concentrations, are you fairly confident because of process knowledge about the relative proportions of the nuclides, and is there some uncertainty about the -- or is there also uncertainty about the relative proportions of the nuclides?

MR. MORTON: So you have to go measure those.

MR. BEYELER: Yes, because again the difficulty is that if you have, say, strontium and cesium, if you're interested in what possible combinations of concentrations can give you a dose of 25 millirems, there are many answers to that question. And so it would not be possible to say you must have a concentration of strontium less than x, cesium less than x, because there are different combinations of concentrations that could potentially lead you to that 25 millirem dose.

And so I think what would be possible to do but what is not currently intended in version 2 would be to define what those combinations of concentrations might be. But it would not be a single number for each nuclide. Rather, it would be a family or a sort of curve or tradeoff function that would tell you for a concentration of this amount for this nuclide, that means a concentration of this amount for that nuclide.

So that's kind of the best way I think of answering that question. But again there was not an intention to do that automatically in version 2.

MR. MORTON: I'll have to see, but it isn't clear that that's what we need.

MR. BEYELER: Okay.

MR. EID: The next, please.

MR. DAUGHERTY: Joe Daugherty.

I was wondering if there was going to be any I'll call it tide parameter warnings

or something. If I go in there and change the annual rainfall will it tell me to look at say -- I don't know if this is true -- but soil moisture content? Or will it do that automatically? How will that be handled?

MR. BEYELER: There is no intention now to enforce changes in one parameter if changes to a second parameter are made.

So currently that requirement is not anticipated.

MR. ORLANDO: Okay. If that's it, move on.

The schedule shows that Chris Daily was going to give a little overview of sources of input parameters. That was something that had been requested at the last workshop. Unfortunately Chris had hoped to do that but isn't going to be able to make it, so Walt is going to fill in. So without further ado, I will turn it back over to Walt.

MR. BEYELER: Well, just one small ado.

MR. ORLANDO: Yes. We'll let you take a drink of water.

MR. BEYELER: Thank you.

MR. ORLANDO: But don't make it a habit.

MR. BEYELER: Again I -- shifting gears -- I'd like to talk about here are some of the issues surrounding defining input parameter values, site-specific input parameter values for the DandD code. There's a need in the standard review plan to answer a large number of questions regarding how that parameter replacement would happen, and there's nothing that is really been settled as far as the mechanics for replacing particular parameters.

There are a number of ideas that are being discussed. What I wanted to do today was to basically describe some of the issues we know will need to be addressed in the Standard Review Plan, to suggest a possible framework for organizing those issues, and to find out if there are some additional important questions that I've left out and that will need to be addressed in the standard review plan.

And what I'd like to do is to sort of look at a kind of chronological organization of

these questions, how they might -- the questions that might occur in the order that they might occur to someone who's trying to change input parameters to the model. That is, what are the sources of information that are available for changing the parameter, what sort of quality requirements or standards do they need or what sort of considerations are there for source quality, how is the information interpreted to define an input parameter for the model.

There are very few if any situations where an experimental program or study will directly go out and measure a value that is appropriate for the time scale and the spatial scale of the model. So there is some amount of interpretation that's required in going from the available body of information to an input parameter for a model, a number of questions associated with that.

Once the information has been assembled, then how is a revised parameter -- what is the mechanics of changing that value in the code? How is a deterministic value selected for the version 1 of the code? How is the distribution modified for version 2, the probabilistic version of the code? So this is sort of the order in which I've organized these questions.

The first has to do with the sources of information. What sort of criteria would be needed for including new sources of information? Is there some de facto acceptance of data from published peer review journals or from Government agencies? A sort of parallel question, what would be the criteria for arguing that studies that have been used in the past are not appropriate for a particular site? So that the idea is that the Standard Review Plan would ideally set out some specific guidelines for when a piece of information could be admitted for consideration when a particular piece of information is judged irrelevant for a particular site.

Perhaps these criteria would be different, depending on whether the information came from a site-specific data collection experimental program, whether it came from local weather station data, for example, regional data base, national data base.

Questions related to the required quality of information. Again, should there be a sort of a de facto acceptance of data from Federal agencies and refereed journals or selected

data sources?

Another question regarding the acceptable quality of information is whether there should be specific standards before data are considered or whether data having different degrees of quality should be all admitted to define the parameter value but given a different grade.

And with regard to site-specific data collection, are there additional considerations for experimental programs that are conducted for particular sites? Should there be some requirements to adhere to nationally or internationally established QA requirements, for example? These issues presumably would be discussed as part of the Standard Review Plan.

The next set of issues is again related to the fact that you don't often measure a parameter value. It's something that tells you something about a parameter value. Key things that should be considered in going from data to parameter values are measurement error, and then the difference between the time scale of the measurement and the spatial scale of the measurement and the time and spatial scale of the model. Just to pick an example, the latter Kd, there may be a large number of individual soil measurements for absorption coefficient. The model parameter is describing what's going on over a fairly large area. So how do we estimate the value for an average over that area given this batch of individual measurements at a very small scale? So that technical issue needs to be addressed in the SRP and some guidance provided as to how to make those kinds of interpretations.

And there are also questions regarding the modification of the parameter value. There are sort of two types of questions here. Version 1 of the code requires a single value for the parameter. That single value can either represent the fact that you -- there is an unambiguous single value for that site that has been clearly established, or -- and in which case the uncertainty about that parameter value has been completely eliminated.

The second possibility is that there is still some uncertainty about the parameter value because the experiments are not completely determinative, and so the question is how is

the deterministic value then specified, given that there is uncertainty in the parameter value. In the case of the probabilistic version 2 of DandD, how is that uncertainty described as a probability distribution for input to the code.

This is just a proposal, suggestion, for a possible way of answering some of these questions or I guess providing a framework into which specific procedures can be dropped. But one possibility is that the NRC would maintain for each parameter a list of references that have gone through a review process and would also define some criteria for adding other sources of information to that corpus, to that body, bibliography of data. But each one of these studies would be available to all users, and it would be characterized by some measures that would help people select data that is potentially relevant for their site. Something on the generality. Whether it's a study that would apply to all sites, sites in particular regions, sites that met particular criteria. What was the connection between the values that are discussed in that study and the spatial and temporal scales of the model parameters? Whether a rating as to the quality of the information in that study. And also some specific criteria related to say site conditions that would establish when that study is appropriate for use in establishing a parameter value or when it is appropriate to disregard the results of that study.

And again there would be some procedures defined for adding new studies, site-specific studies to this data base that would then be available for use by users in general.

And then in general that -- the procedure for using this body of information would be to using site conditions to search through to identify the pertinent references to determine whether or not a parameter value is clearly established. There may be a single determinative study that was done for the site. There may be some body of ten studies that are relevant.

And so the parameter would either be a unique value would be chosen because there is a determinative study, the parameter may be uncertain because there is a single uniquely appropriate study, a site survey, for example, but it was impossible to determine an

unambiguous value on the basis of that study. The parameter value may be uncertain simply because there are a number of competing pieces of information or conflicting pieces of information in particular that might apply.

And the SRP would then ideally include some generic procedures for dealing with some of the common technical problems that show up in translating experimental measurements to parameter values.

Again one of those would be the question of differences in the spatial scale between experimental measurements and the parameter value differences in measurement time, how to go from a set of measurements over 24-hour periods to an annual average value, maybe standard procedures for defining those.

And also it might be possible to come up with some generic rules for using commonly available and commonly accessed data bases. For example, USGS regional coverages for groundwater, for example. There might be specific procedures defined for how those data sources would be properly used.

So those are basically some suggestions as to the types of information that we would anticipate supplying with the Standard Review Plan and some of the questions that those are --

MR. ROBERTS: Rick Roberts, Rocky Mountain Remediation Services.

In your presentation you talked about the uncertainty in the current environment, looking at uncertainty in environmental factors and the distributions maybe of things that could happen currently. What about the uncertainty in what's going to happen over the next thousand years? Do we need to take that into account, or is that something that we really don't need to take into account in looking at uncertainty?

MR. BEYELER: I guess my response to that would be that depth-to-water was perhaps an unfortunate example here, and that I think as far as a physical condition at the site that because it is meant to be protected for a thousand years that uncertainty in potential

variations of the physical conditions of the site should be considered. As far as uncertainty in behavior, I think that is stipulated by the specification of the critical group, and that those kinds of uncertainties would not be included.

MR. EID: I believe there are two issues. One issue is the possibility for changes due to the critical group conditions. And I believe the current critical group conditions, specifically when we talk about residential farmers, they encompass different kind of variations within a thousand years. So whatever kind of behavior we assume in that scenario, almost every kind of behavior and every kind of activity that you could think of is already included in the scenario.

However, regarding the physical parameters, you know, it's very hard to speculate what kind of physical parameters they could change, like Kd value. I mean, I expect you will see some changes, but the question how they could change, rainfall in 1,000 years, possibly the changes will be not that drastic. If you have specific examples about changes in physical parameters that could be significant, you know, those should be taken into consideration when you review and you look at the site conditions. I think it will be good if you now could give us some examples that we could pay attention to those physical parameters that may change in 1,000 years so we could look into that.

MR. ROBERTS: I guess I'm coming from the point of view that I would rather have the NRC come out and say no, use current conditions to model into 1,000 years because we really don't know the uncertainty over the next 1,000 years, but I've had questions come back to our group saying well, what about the uncertainty in the next 1,000 years? And I don't know how to quantify that in assessing cleanup levels. And I guess what I'm looking for more is some kind of statement saying no, you use current conditions to model 1,000 years in the future, and there really is no way to look at the uncertainty that may happen in the physical environment over the next 1,000 years.

MR. EID: I think when we talk about the uncertainties when you do a

measurement you could have different kind of variations because of the physical conditions. You may have different soil types at your site. So when you determine Kd values you will have ranges of soil types based on these ranges of soil types that you may have at the site. So this means you do not put one single parameter of Kd value for that site, you may put ranges of those Kd values because the conditions that you have, you may have sand lenses, you may have, you know, clay material and distribution of those, they are not homogenous because the nature, physical nature of the site is not uniform. So because of this you will have distributions of those, you know, parameters that they are specific for the site.

You may have also ranges because of the experimental determination. When you determine Kd value, you are not going to get a single value for the site, even for the same, you know, kind of if you have different samples. You will have a variety of values for the Kd. The question is then you will have range of distributions of those values for the site. Even if you do, you know, site-specific analysis for the site.

MR. McKENNEY: Chris McKenney, NRC.

Actually, you described it fairly well. Exactly what the background is on not only decommissioning modeling but also for high-level waste and low-level waste, modeling of the biosphere is based on present conditions. We don't model evolution, complete evolution especially, of whether there's an ice sheet or whatever or it turns into a lake. It's all based on present condition assumptions. There may be uncertainties in what the present conditions are, but we're not looking at what it will be in 300 years.

MR. ROBERTS: Thank you.

MR. DARMAN: I would like to ask one more question, I guess. Joe Darman, Maine Yankee.

In version 2 if we change one parameter, you know, we should look to make sure that if there was any tied parameters, you know, that -- look at those. But what about all the other parameters that it came up with as defaults. Can we just leave those all alone?

MR. BEYELER: In version 2?

MR. DARMAN: Yes.

MR. BEYELER: In version 2, yes. It changed to a single parameter. The consequences for the other parameters are in effect automatically included. If that answers your question.

MR. EID: I have a question. Now currently with the current DandD version, if the licensee, they have a value that you could trust and do a QC, physical parameter, can they input this value directly into the Code and they derive the dose for the site, or they have to run probabilistic, you know, Monte Carlo version in order to find the dose, the distribution of the dose?

MR. BEYELER: They I think need to provide assurance that by changing that parameter value that the decision criteria are still met so that the default parameter values provide that assurance. The Monte Carlo calculation will provide that assurance for a deterministic replacement. And for certain parameter values that assurance is also provided with the current deterministic version of the code. But it is not the case that you can replace arbitrarily combinations of parameters in the code in version 1 and still be assured that the resulting dose is in the upper end of the revised distribution for doses.

So did that --

MR. EID: No, it still is not clear for me what kind of vehicle, what kind of tool that the license could use to change the parameter. You are saying yes, they could change the parameter, but they have to look at the criteria? Yes or no? Can they change -- if they have a parameter that they could trust for their site, currently in the DandD code we understand you have the full parameters. Now they have one single parameter, say a Kd value, they measured it, we said that parameter is conservative, that fits the site, can they change it in the current version to say yes, that parameter fits our site? Can they change it, like Kd values?

MR. BEYELER: They can change certain parameter values, and a particular Kd

value for a particular isotope, I think it would depend on the particular isotope. But we have made some tests to determine whether or not replacement of individual parameter values with deterministic site-specific values provides the same level of protection, in effect, or provides -- satisfies the same decision criteria as the existing defaults do.

So there's a table of parameter -- basically parameter replacements that have been tested for that purpose that presumably would be part of the standard review plan or guidance for operating version 1 of the code. That is a set of parameter values and ranges and combinations that have been tested for the purpose that you describe. That would be my recommended way of proceeding pending the completion of version 2.

MR. EID: Okay. I would like just to say this is an issue we are still discussing. It is not final. I mean, this is a point of view, and maybe, you know, we'll discuss it further, you know, within the staff even.

Thank you for your answer.

MR. DEMODER: Steve Demoder, Department of Energy.

Relative to your process for identifying default parameter values, do you have -- I believe you have a document in progress that has a compilation of the default parameters and where that information came from and how you're going about deriving the distributions of those parameters. What's the status of that report, and, secondly, do you envision putting that information into an electronic data base, or do you already have such a data base?

MR. BEYELER: That report is due to be released in the next -- it's supposed to come out final in the next month or two, I believe. So it's shortly to be released. There is an earlier draft that I believe is still available on the NRC Web page that describes the methodology. The information sources that are described in that earlier draft have been considerably expanded, and that will be available in the forthcoming final draft.

I believe it is the NRC program manager's intention to make that available -- to make the report as a whole available on the Web. Whether or not there will be an indexed data

base or a searchable data base generated from that information, I don't know what her intentions are.

MR. DEMODER: Thank you.

MR. BEYELER: You're welcome.

MR. MORTON: Henry Morton.

In version 2 it would be helpful if on an individual parameter basis one would be able to select whether you put in or enter a probability distribution or a single value. Do you plan to design that into the version 2?

MR. BEYELER: Yes, sir. I won't go -- try to find a particular display, but the version 2 will allow the user to type in in effect a single value in the same exact way that version 1 does now. We'll also allow the user to double-click on that entry field and then bring up a display that would allow them to specify a distribution if they want to do that.

MR. EID: Any other questions?

[No response.]

MR. EID: Okay. Shall we proceed now?

MR. ORLANDO: Thank you, Walt.

MR. BEYELER: You're welcome.

MR. ORLANDO: The next presentation will be by Dr. English on partial site release, and before he starts talking I want to take a look at the throat mike a little bit here.

DR. ENGLISH: Good morning. I guess the men are displacing the ladies here regularly but Bev maybe will be able to answer some questions later on after I finish.

What I am going to speak about is how we are proceeding at Big Rock Point in decommissioning, the restoration project, and the examples of partial site release will be based primarily on what we are looking at at Big Rock and the condition of our facility.

This is the site map, which illustrated the protected area which within that maybe 10 or 15 percent has been utilized as a restricted area, so the protected area itself and the

adjacent developed areas represent probably only 10 percent of the total of 600 acres. We have maybe six acres that are developed. Of that, quite a bit less than an acre would be contaminated or covered by buildings that are potentially contaminated.

So the major portion of the site is basically non-impacted. The protected area is the area you can observe right around the sphere there, within that fenced area. We have a little more development at the left-hand side where we brought in trailers and that's the parking lot and so forth, but those again have not been impacted areas.

This is a view from the back side, an aerial photograph about half-way between the site boundary and the facility itself. It shows the slope of the land. It's higher behind the plant, sloping down to the plant. The area is basically undeveloped woodlands. There are just a few -- an old rail spur that ran down on the map that you saw going north and south and an east-west road that comes into the plant for access.

There are some fairly significant benefits of partial site release and not to mention the fact that there's a resource there, nearly 600 acres of property that's worth probably five to seven times what the original cost of the plant was when it was built.

The radiological control is certainly an area that we think would be benefitted by reducing the plant site size because there is always the temptation to utilize a lot of land for decommissioning activities such as laydown, shipping and receiving of goods and equipment that would cause you a lot more concern over the control of the area than if you reduced the size of the site, say to 10 or 20 acres.

Another benefit that we have of early partial release is that it requires us to identify those impacted areas early, identify the extent of them, provide early survey planning, implementation and fuel experience with the techniques that we are going to use for final termination, and then for the release, as I said, the financial asset recovery on the property, 7000 feet of Lake Michigan shoreline and the release of that resource for beneficial use.

One of the first considerations one makes of course when you go into

decommissioning is the rules under 50.82. Those rules are particularly applicable to the actual site termination and if you go through looking at release of nonimpacted areas, you can step through the requirements of 50.82 there as shown.

Do you need a dismantlement plan for undeveloped, unimpacted, nonimpacted areas, and the answer is no, it is not applicable. Do you need a remediation plan for that area? No, it is not contaminated, so no remediation plan is required. Should you need characterization data -- you will need characterization to define the extent of the impacted area but as far as the nonimpacted area, the recommendations in MARSSIM and Draft Guide 4006 both say that it is characterization of the nonimpacted areas based on historical site review as opposed to actual characterization survey and they definitely state that no termination survey is required for nonimpacted areas.

Do we need a plan for restricted use? Well, obviously no, if there is no radioactivity there is no question of restriction. Do we need a decommissioning cost revision? No, it is not a cost to release the site -- no cost impact.

Do we need environmental report revision? Again, no environmental impact to releasing the site that is not then affected by plant operation.

Public meetings? I don't think so, because there is no significant action or hazard for releasing, again, the site from the license, that portion from the license.

Do we need an approved termination plan? We are not terminating the license, so the license will remain. The restrictions on the control of radioactivity will remain simply on a smaller portion of the site so again there's no requirement, no usefulness to taking that path.

The final requirement of 50.82 is amendment of the license to control the continuing decommission activities and so, yes, that is the area that does need to be followed. If the site is described in the technical specifications a license amendment is needed to change that site description.

Now since there's license -- the last slide indicated that we have come to the

conclusion that it's a license amendment rather than the full process of license termination that is required for partial site release, one has a couple options. The first one is to move the site description from the technical specifications into the final safety analysis report and one uses there 50.36(c)(2)(D)(iii)(6) as a basis, which basically says where changes to the specifications in decommissioning facilities you are allowed on a case by case basis to remove non -- you know, requirements that do not apply to operating plants from the technical specifications.

Once in the FSAR, you would use the 50.59 process to then change anything to do with the site description. You would need an analysis to support 50.59 and Yankee Rowe has done the amendment change. They have not, however, changed the site yet through the 50.59 process but they did receive, dated February 3rd, an amendment change moving the site description from the technical specifications into the FSAR.

The second option is to change directly and by amendment the technical specifications, to exclude the nonimpacted areas, simply a new map and description of the site for the size to include the impacted area and any areas that you wish to, need to utilize to continue decommissioning in a safe and effective manner.

You need to provide the basis for that change and basically the reasons why it is not impacted and the things that immediately come to mind are emergency planning and security plan compliance, because one of the main reasons one needed the large site boundary distances originally was in terms of the emergency planning, the TZ and the productive action guides and so forth which decommissioning plants have recalculated and we have at Big Rock Point and basically have done calculations with FSAR currently showing that we do not exceed the PAGs at our protected area boundary -- that little small area directly around the plant site.

For either option again I would stress it is not important to not go overboard and try to restrict it to too small an area because you will need a significant facility area to complete decommissioning.

In terms of technical considerations for the nonimpacted area, one needs to look

at both the liquid and the airborne pathways. Liquid spill history, for example, surface water flows, aquifer flow, barriers to aquifer or to surface water flow, and the same things for the airborne contamination pathway -- surface spills. If you are in a more arid environment than we are at Big Rock, for example a desert type environment with wind erosion, one has to look at that and the surface and wind conditions.

Now Big Rock, as the slide demonstrates, the slope of the land is toward the plant. It is well wooded. It's been wooded since the time that the plant was built. There are basically only -- the access is by the rail line, a road coming in. There's a power line that parallels the road coming into the plant and there was a small, predating the plant, development to the left-hand side from the plant into Charlevoix. There was a little two-track, probably wagon trail or something from the old days that we used pre-site basically only for a meteorological tower.

We kind of had a novel situation. The pre-siting requirements for the facility required meteorological data and so we put up the tower and as soon as the plant was built we took it immediately down. We didn't have any tower until TMI days and we put up some meteorological equipment on the stack, so again that roadway was not used going back toward the Met Tower.

The review as I mentioned for contamination is highly important. Burial sites would exclude your ability to call an area nonimpacted. Rather it would be impacted, by definition. Any scoping identified contamination would automatically exempt you from being able to call the area nonimpacted.

Evaluations as identified in the MARSSIM manual are historical site assessment, scoping surveys is questionable. Draft Guide 4006 tends to speak more to characterization surveys. I believe the two are equivalent, although somebody from the NRC can say that MARSSIM talks scoping and DG 4006 mentions characterization. Either way, obviously, if any surveys show -- find contamination or your historical review identifies spills and

so forth in an area then it is by definition an impacted area.

There is some question and it probably needs more discussion between utilities or licensees and the NRC in terms of if you have cleaned up a spill, if you find a very small level of contamination somewhere in the site, can that ever return back to be called a nonimpacted area for purposes of release? The way the guidance is currently, it sounds as if not, but it sounds on a practical sense if you found one resin bead somewhere that it had dropped off of something, you picked it up and took it back to the plant and there was nothing else there, it's kind of envision why that would have to be called an impacted area and be classified as Class 3 or whatever, but that is maybe another point of discussion.

Our characterization -- it uses in situ gamma spectroscopy. We are running the detector at about 7 meters in height so that we can circumscribe the 10 x 10 meter square and running with the columnator in a circle allows us to move the arm around the truck and not have to move the truck every time and align it north, south, east or west, as a square columnator would.

The truck looks like it might be inside the circle. It's not. It's because of the angle of the shot. We don't include the truck in the analysis.

We did scoping surveys earlier with just a one meter off the ground but that is equivalent to sampling. It gives you a fairly small area that you have analyzed. Moving it up to 7 meters gives us a 159 square meter area.

In conclusion, we believe the regulations are in place to allow us to remove obsolete requirements under 50.36 and that the large land area is needed for emergency planning and/or for 10 CFR 50, Appendix I to reduce the offsite dose limits that are no longer required when we are not under power operation and we have had now at Big Rock a year and a half of decay, since shutdown.

The areas that are nonimpacted should be removed from the license to provide rapid asset recovery and beneficial uses.

The license termination surveys are not required for nonimpacted areas and therefore really isn't a license termination question. It is a matter of obsolete technical specification requirements and that the two options are the FSAR change, moving it, moving the description of the site to the FSAR or amendment of technical specifications and both of these -- we have representatives from companies that gone in both directions here.

Yankee Rowe did the move to the FSAR and Oyster Creek is in the process of attempting a change on their, amending their technical specifications to remove a portion of the site even though they are currently still operating, and so we think they are both viable options and we believe that we are in a process currently writing up the change. It still has to go through internal reviews but we will probably be submitting an amendment under Option 1 within the next month or so.

Questions? That is what the site is going to look like when we are done.

Okay -- and thanks to Dave Parrish down here.

MR. EID: Do you have a question, Henry?

MR. MORTON: It's a question actually for the Staff. This presentation dealt with partial site release for nonimpacted areas.

DR. ENGLISH: Right.

MR. MORTON: Could the Staff give some indication of what your considerations are for partial site release, impacted areas? For instance it's my understanding there's several applications in, in which the licensee is asking for partial site release for possibly impacted areas.

MR. EID: This issue is still not really fully explored and evaluated, however one area for the partial site release, is it to release it from the license completely or to release it just for use until license termination?

The question of potential groundwater contamination below that portion of the site is an issue. The direction of groundwater flow is an issue. So we cannot talk much now

about it. Maybe we could have a future workshop to discuss that issue, because it is a lengthy issue to try to address every aspect that pertains to partial site release specifically for affected areas.

MR. ORLANDO: Does anybody else have anything on partial site release? Any questions or anything you want to talk about? The idea was to use Dr. English's talk to stipulate discussion on the issue.

If we don't have anything to discuss, we can take a little bit earlier break and then come back and start up with some discussions of the conservatism in the existing data on resuspension factors. Okay. Thank you very much.

[Recess.]

MR. ORLANDO: I think we are going to try to get started again, please. If everybody could come on back in and sit down.

MR. EID: Good morning. My name is Boby Eid. I am with the Division of Waste Management, Performance Assessment and High Level Waste Branch. And I am giving this presentation also jointly with Steve McGuire. I guess all of you know Steve. Steve made the presentation of resuspension factor in the previous workshop and he made some recommendations. This is a continuation of Steve McGuire's presentation. Steve could not attend here because he's on travel. I will be taking the blame for any mistakes or any problems, and I would like to give Steve credit for doing the majority of this work.

With this opening again we have a disclaimer that this is a study. It is not final. We are still discussing within the staff. We did not make a decision or conclusions, final conclusions on that. They are just merely proposals and suggestions. So please do not take these values as a value that you could use in your assessment. They are only suggestions within the staff, that they need further discussion, they need further verification based on data that some of you engaged in collecting such kind of field data at your side.

So with this opening, I would like to start by referring to the previous workshop

and what was recommended in the previous workshop and the recommendation was to estimate the resuspension factor based on total activity rather than removable activity. I believe that's where we -- when we made the recommendation.

And we said we need to have more field data to support that recommendation.

Keeping this in mind, we'll proceed, and I would like to emphasize more on the reasons and rationale for considering, selecting the total activity rather than the removable activity for estimation of resuspension factor.

The first reason is because we believe that almost all residual activity at the remediated facility will be fixed. Most of it is fixed. A very, very small fraction of that activity is actually called removable contamination. Therefore we should look into the measurements of fixed activity rather than to emphasize on removable activity.

The second point is that because the resuspension driving forces from mechanical disturbances such as moving and other activities in the facility to be released is much greater than those involved in taking just a smear sample, putting smear sample on the floor or the wall and to say this represents the actual mechanical disturbance of that surface.

Therefore, the driving force will cause a deeper abrasion and gouging for the surface when you walk on the floor or when you have a vehicle or some kind of equipment that moves on the floor.

The third reason is because it is not exactly correct if we assume that only removable activity will be resuspended and fixed activity will not be resuspended. This assumption if we think that only the removable activity is not exactly correct because the fixed activity, whether on the wall or on the floors, will be potentially resuspendable. So it will be with time because of abrasion, because of any kind of other processes, ventilations, it will become resuspendable.

The other reason, as I guess I mentioned already, the abrasion of the driving forces will act primarily on the fixed activity because most of the activity, as we said, it's fixed.

The fifth reason is because the small fraction of removable activity will not substantially affect the total resuspension factor. As we said before, most of the activity is actually on the surface and at the present time it is not resuspendable, it's not removable, but could be removable in the future.

The sixth reason for concentrating on total activity, because the smear samples are difficult to interpret and reproduce. As you know possibly from your measurements, the production of the smear sample is not highly reproducible, so there are large uncertainties in the smear samples that you take to measure the removable activity.

Then the seventh reason is because the data for the suspension factor should be based on measurement of residual activity and rooms that are representative of the residual activity in the building occupancy scenario. So this means you need to compare your data based on site-specific conditions. That's what we were talking this morning, and these could vary from one place to the other.

We would like to visit again Steve's presentation before, and this is additional kind of data that we included, and we have new ideas about what can you do about the resuspension factor. This represents the data for different references that were used in establishing the current default value. The current default value is actually it is on this end of this curve, and what we are suggesting that the 50th percentile value of the resuspension factor to be considered as a default in the DandD screen.

And I will mention the reasons behind this kind of proposal or suggestion. Again, this is a suggestion, it is not final, it is not staff decision yet, it is just only to throw it for discussion.

So the suggested value will fall into this area, and I will talk about it. And even if we take the 90th percentile value based on reconstruction of the data so it is less actually than the current default value in DandD screen.

We are trying to collect additional data to support our suggestion. The

additional data we will use from Spangler, Dave Spangler, of BWX, and Dave is currently analyzing the data. The data was originally presented here and it was code data, so he's going through further analysis. That's why we would like to look into the QA/QC of the data in a formal matter so that we will have great confidence in the data that's submitted and say yes, it can be used in establishing the resuspension factor. So based on these data hopefully we'll try also to reconstruct the curve and to say this is the credible data that we could use.

In addition we will look into the weighting pattern for data. If there is lots of information it's credible to a QC so we may give it a weighting factor in establishing the PDF curve for resuspension factor.

Also Joe Nardi will be giving a presentation today. I'd like to thank him for volunteering to give his presentation about actual data at their site on the suspension factor, and we'll be interested to see how this data fits within the curve that I showed and whether our proposal is conservative or not conservative, whether again hopefully that this data will be credible, we'll look into it in terms of QA/QC of the data, and then we'll make judgments for inclusion of the data to construct the PDF curve for the resuspension factor.

Further there was a proposal from the previous workshop that DOE, they have lots of data for their decommission site. We did send a letter to DOE, and we requested to provide us with whatever data that they have on their decommissioning site such that we could include it in consideration or reconstruction of the PDF curve for resuspension factor, so that we could justify on a solid basis for changing the resuspension factor.

Now, I would like to address the issue of -- the suggestion concerning the 50th or the mean value for the resuspension factor when we established the PDF curve versus the 90th percentile of the resuspension factor distribution. The suggestion for the 50th percentile is because the default for physical parameter were based on the 90th percentile, because the default currently in the D and D screen is based on the 90th percentile prove against the function, that is to start with, that is currently what we have.

And then we are proposing to select the resuspension factor based on the 50th distribution, or the mean distribution for the following reasons. First, if we really analyze and look at the anatomy of the resuspension factor, it is not a purely physical parameter. It is not like a Kd, that it is for the site, corresponds for the site, and you determine based on actual physical measurements there at the site, although you do some physical measurements to try to derive the resuspension factor. It is a combination of physical conditions, at the same time behavior conditions of the critical group.

As I discussed earlier, that the resuspension factor will depend largely on the behavior of that critical group in that facility that will be releasing, such that behavior for the way they are working, the way the room is used, and the way the disturbance, that is used, the kind of ventilation is there. What is the room going to be used in the future, is it storage room? If you use the room for storage, of course, is different than when you use it and you consider, you know, a light industrial scenario, so you could make those changes.

So it depends somehow on the behavior of the critical group, and in our analysis, and currently in D and D, we accepted to use the mean or the 50th percentile values for the behavior parameters. For example, the building occupancy parameter, we used the 50th or the mean values for the way the critical group will occupy that building. The inhalation, for example, the breathing rate. So we accepted those, we accepted the 50th percentile, or the mean, somehow for these kind of behavioral parameters, we call them.

Based on our analysis, we believe the factors that they correspond to the critical groups will have significant or more impact on the resuspension factor value. In other words, the behavior of that critical group will do impact the resuspension factor value.

If we accept that assumption and that argument, then the question is why we do need to consider that the resuspension factor is a purely physical parameter and the 90th percentile confidence that to be selected. So that is one argument and that supports the possibility for selecting the 50th percentile of the resuspension factor.

The second point supporting the 50th percentile, in performing site-specific analysis using D and D Version 1.0, and then if we accept the 90th percentile vale, and then we ask the users to go ahead and then try to collect site-specific data for their site, in order to justify changing that 90th percentile value, the question is what the users, they use, what kind of procedures in order to go and change that conservative value, the 90th percentile value.

The question is, well, you go and collect data for your site. The question, to go and determine the resuspension factor for your room, the question is variability, a long time, or how much time you need to do, the sampling, again, the accuracy of the measurement, are they reliable or not reliable. So it is better to start with something relatively conservative, and then you say, yes, even I will accept for site-specific conditions rather than to say throw it on the users and say, okay, try to justify your resuspension factor. So, the justification for modification of the resuspension factor based on measurement, it will be, I believe, costly and could be also not easy to justify the specific values for resuspension factor.

Again, also it is difficult to predict, if we accept that, you know, the mechanical disturbance of the surface is a major cause for the resuspension factor, is difficult to predict what kind of activities that could be conducted for the facility.

The third reason is because all of the data points shown in the previous graph that I showed the graph before, and you could look at those values there, sometimes highly conservative values on the other end of the curve, that these values, they have biases that they cause the measured resuspension factor to be large, is an experimental determination, somebody in the lab trying to have a research paper to be published in the journal, try to cause these conditions to be exaggerated for a disturbance of the floors or the surfaces of the walls in order to collect some measurable data. So disturbance, walking, you know, possible hundred steps per minute on the floor or having some kind of fan on the floor, try to agitate the surface, try to have conditions such that you could measure the resuspension factor, measure air concentration there for low concentrations on the surfaces or on the walls.

So, because of this, we believe even the data that was used could be somehow, you know, exaggerated in order to get some numbers for the resuspension factor. So, none of the measurements were done on a remediated facility. In addition, they were not actually done on remediated facility where you clean to that level, because simply you can measure after that, so they were measured on -- either you spray the floor or you put some kind of loose dust on the floor, then try to make some of agitation, then you do the measurements.

For example, the measured values for Breslin, which is the source which was used in establishing the PDF curve, and Eisenbud, they may have airborne activity from ongoing operation. That is different than the case when you are releasing the site and the operation has been ceased. So this means the source term is regenerated, or there is more source term actually is there.

So the question is, you know, why we use this data in order to establish our curve, see, we are very highly conservative. So the Breslin data was taken while exaggerating the movement of workers beyond what they normally do, even for light industry scenario, thereby increasing the resuspension factor, and the purpose of the measurement was to produce an upper limit of the airborne activity that could be produced by disturbing surfaces.

In conclusion, we would like to see an effort to be made to obtain more factual data on the resuspension factor for the building of the scenario and we are engaged with industry and with DOE. Hopefully DOE will respond to our Recourse 2 to provide us with additional data, so in order to have sufficient basis for modification of the resuspension factor.

Also we are proposing -- again it is a proposal, it is not final -- we are going through lots of discussion. We are suggesting to consider using the 50th percentile of the suspension factor distribution.

Also based on the current analysis, which still is raw analysis, it is not final, we are proposing to consider using a default value for D and D of .75 times 10 to the minus 6 based on total activity, so that is the number that we are proposing. Considering that number, currently

we have surface contamination limits for alpha emitters that is very low and in some cases you cannot measure it within the fluctuation of the background.

Here this table shows the Reg Guide 1.86 release limits for alpha emitters, specifically for U-238 and Thorium-232, and as you can see it is 5000 for U-238 and for Thorium-232 is it 1000, and --

SPEAKER: What are your units?

MR. EID: Those are DMP per hundred centimeters.

Now in the current default value of Version 1.0, the release limits are 100 and 7 DPMs. We recognize that dose limits here are very low and we said in our Federal Register that we are working to see what kind of parameters they can be changed and how these PDF values are justified.

If you will allow me just one thing -- just one more thing I would like say, and then we will open the floor for discussion. I am sure there will be also discussion, and using the recommended number that I just showed before, so we will have different values, which is for Uranium-238 surface contamination it will be 1800 and Thorium-232 it will be 140.

As you can see, still it is for U-238 specifically it is much below the release limits. However, it is measurable and it is much above the background fluctuation and it is, I would say it is somehow more reasonable but still it is a conservative number.

For thorium, still it is much below also, but it is possible detectable and is much above the current default values.

I believe this is a step toward trying to think about how we look more seriously into the current default input parameters and then to see if it can be justified or not. We are still in the discussion. Again, it is not final. Please do not take this as a value that we could go and just plug it in the code and quote NRC Staff. That is not the reason for that. It is just for discussion.

I would like to thank you again, and the floor now is open for any discussion.

Yes?

MR. SEXTON: This is Dick Sexton, Connecticut Yankee.

I guess in terms of looking at your resuspension factors, did you or could you look at operating facilities? Many operating plants certainly in the reactors have many air samplers that are located throughout the facility that typically don't have any removable activity and see a fair amount of action.

Have you looked at that data?

MR. EID: We did not have specific data in terms of numbers but if you have any data or anybody has data from the industry we would like to have it just to refer to us where this data can be obtained, but we did not go and look at every specific case to see what kind of data for example for the licensee that we have in monitoring, but if -- from the industry point of view, if you have any data like this kind of data that you are talking about and this data is reliable, QA/QC, or you could look at the surface contamination and at the air concentration and then the way to derive the resuspension factor would be very helpful for us.

MR. KULZER: That's one of the questions I had or very similar to it. I was thinking in terms of --

MR. ORLANDO: Could you identify yourself, please?

MR. KULZER: Ed Kulzer, Region III, NRC.

When you are talking about defining resuspension factors I was thinking more or less a similar process in asbestos removal where they take aggressive sampling, where they just go through the area with a leaf blower and suspend, put in suspension everything that can be suspended and then take an air sample and that way they get a pretty good example of how much is removable or resuspendable.

Has anybody got any data like that to support the findings or resuspension information?

That might be an easy way to do it -- what's ever been in the facility for 30 or 40

years -- that before you start decommissioning activities is going to be suspended on rafters, on beams, and by going through and aggressively sampling and monitoring you are going to have pretty good, accurate data to compare things with.

MR. EID: Okay. Any other questions?

MR. BEYELER: Walt Beyeler from Sandia.

MR. EID: One second. Henry?

MR. MORTON: Henry Morton. I think this is an evolution in the right direction and with respect to measuring and getting data for fixed activity, it would seem to me that in the process of getting data, particularly new data, it would be helpful to know the material compositions of the floor, for example, because it would seem to me that the actual source term is going to be a removal rate which would be a physical abrasion rate of the material times the activity, the aerial density activity that is fixed on that surface.

MR. EID: Thanks. Walt?

MR. BEYELER: Yes, Walt Bayeler from Sandia Labs.

I just wanted a small point of clarification on the behavioral parameters. Those in the current version of the code were set to the average values of the distributions rather than the 50th percentile and the argument behind that was that those values are meant to characterize the average member of the critical group.

MR. EID: That is why I am trying to relate that it is the behavior of the critical group that the codes -- also they have impact on the value of the resuspension factor so if we accept that as it is part, a portion at least, of that parameter is based on behavior parameters of the critical group then we could think about -- you know, the question is is it necessary to be that extra conservative to have it the 90th percentile or higher currently -- in the current D and D version actually it could go much higher because you are concerning all radionuclides.

MR. BEYELER: And that would be an argument then for using something closer to the average of that distribution rather than the median.

MR. EID: Yes.

MR. BEYELER: Okay, thanks.

MR. EID: Yes?

MR. SEXTON: Dick Sexton, Connecticut Yankee.

In establishing these limits, is there any consideration given to the feasibility of making these measurements in terms of demonstrating compliance? When I see a limit of 100 for U-238, I wonder about the feasibility of physically surveying and having current technology to make those measurements?

MR. EID: I believe that is a good question. That is one of the reasons we are trying to revisit, to see what kind of conservatism we have in the model, and as we said before, these values -- you cannot measure them, cannot demonstrate for some values -- 1.7, you know -- you cannot physically measure the dose, so the question is what is -- are we too conservative or because it is true, those are true numbers and they are risk numbers we need to care about, I believe we are trying to be a little bit extra conservative because of the screening process.

Therefore, this means that cost and money and time and effort that you spend in measuring those maybe is not quite justifiable, so if you have different numbers, it could be, yes, it is justifiable and the cost and the effort could be more justifiable to go on and do the measurements.

So the difference in the measurements -- if you are close to background, it's very difficult to measure. To be too close then you may not be able to measure or detect. If you go higher, then yes, there is reason for going -- trying to do the measurements. Yes?

MR. SAITO: Paul Saito, Combustion Engineering. I am kind of following up on what the last gentleman said.

This number I think is going to be much, much smaller because if you use a MARSSIMS type measurement at this point where you take source efficiency into account, this

number will go do from our current measurement. You are probably back down to about the 100 level again for an alpha emitter. This has to be taken into consideration when we start moving forward at this point.

We can't keep thinking of the old ways of measuring things, so when we start looking at this data, the resuspension factor I think should probably be an order of magnitude or two orders of magnitude lower than what you have suggested if we are going to move forward with the MARSSIMS type survey.

MR. EID: I think that is a very good point too, because when you do a survey you do not look at the DCGL value. You look at the value which is much below the DCGL value in order to measure it. You need to measure those low values then to justify releasing of the DCGL because at the end you are looking at the average, I believe, and in most cases you try not to exceed that DCGL value, so you are absolutely correct.

MR. MORTON: Excuse me. Henry Morton again.

I think part of the point here is the careful definition of those -- the units that are applied to these. The Reg Guide 186 numbers for the uranium and thorium series and the individuals -- it's actually the unit would be alpha per minute per hundred square centimeters, whereas for the derived value -- that is the value derived by D and D -- is expressed in disintegrations, that is, technically atomic disintegrations per minute per hundred square centimeters, so it becomes an issue if matching them up.

It turns out in this case that since you have derived the value for Uranium-238 without daughters and Thorium-232 without daughters then you have got a one-to-one correspondence in comparing them, but in terms of measuring, as Earl was dealing with, you need to differentiate between those two units.

MR. EID: Yes, you are right.

MR. DORMAN: Hi. Joe Dorman, Maine Yankee.

I would like to reiterate that too, about all this data on this graph, that these

surveys probably weren't done using the MARSSIMS technology, the source efficiency and back scatter and taking into account all the things that we have to take into account now and perhaps if they were the contamination levels on the floor, the fixed might have been a little bit higher than what they have reported in those reports, causing the resuspension factor to be lower.

MR. EID: Yes. Those references, as you can see, they are very old references, and now the question is you cannot go and call the person to see what kind of measurement they did and so on. It is, by the way, I guess one of the references that was used. They were also measuring that fill sampling -- and unfortunately those results they were not considered even in this analysis. They were taking samples, they were put at different levels which many of them they were very, very close to the floor, within a few centimeters from the floor, the resuspension factor, so there is a question even about whether these values they correspond to the briefing zone that those we are quoting or not.

I believe there is an additional conservatism for where the measurement was taken. Dave?

MR. FAUVER: This is Dave Fauver, RSI. I have to agree with the previous two comments. The new MARSSIM method with the source efficiency is just a different approach to assessment that it is easy to assume was not applied to any of this dataset that you have up on the board there, so I think it is a very strong case to simply make that correction to this data across the board when you would apply the MARSSIM approach when you determine the resuspension factor.

MR. EID: Okay, thanks. Any other questions?

If not, I guess we will move to the industry point of view regarding the suspension factor. Joe Nardi will be giving the presentation, real data from their sites. We will be looking forward to see how the data will support our assumptions or not.

MR. ORLANDO: While they are wiring Joe up, I just wanted to mention -- Bobby did say something but at the last workshop Earl Seito mentioned that there was some data that

was out there in the DOE world on resuspension factors and Earl sent me a list of questions that we needed to ask DOE and those were formed into a letter and we sent those out to DOE a couple of weeks ago, so as Bobby's slide indicated, we are out there trying to find some of this data.

Now I guess how we manage it and how that was collected needs to be looked at too, so just to give everybody an update as to where that particular request was.

I think the letter was sent out a week or so ago.

MR. NARDI: Okay. My name is Joe Nardi. I work for Westinghouse. What I'm going to be doing is trying to get two objectives: to review some operational data that we have obtained during the decommissioning of one of our facilities, and I'll describe the facility a little bit more in my presentation, and more so to really take a look at some of the factors and what I think are going to be the limitations in our ability to measure resuspension factors in facilities, that this relates directly to your question of whether we could use operational facilities versus decommissioned facilities.

This slide is not in your presentation. This was the one I handed out or showed on the board at the last workshop. My problem with the data -- this was very preliminary -- I was getting resuspension factors in the ten to the minus seven for total activity numbers in the ten to the minus seven values.

The problem I had with this data was that the air sampling was for a period of operation, and we really -- in operational facilities, we don't make total activity fixed measurement kind of things. We do lots and lots of smear measurements, but not total activity measurements on the floor. So I was really using air sampling from one period and fixed sampling of the surface activity for another period of time. So I wanted to relook at that information to really try to see what we could derive.

First of all, to make sure we're all together, the definition of the resuspension factor as given in the NUREG 5512 is just simply the ratio of the airborne concentration to the

surface concentration. If you look at their definition in the -- it's in units of picocuries per cubic meter, or picocuries per square meter. All my data is presented in microcuries for ML for air samples, DPM per hundred square centimeters for surface activity measurements, and therefore you need a conversion factor of 2.2 times ten to the tenth to get the equivalent resuspension factor.

To describe a little of the facility history of the operation that I'm talking about, this is a pump repair facility. It's been in operation from 1957 approximately to 1998. The primary isotopes within the facility are cesium and cobalt.

We are currently decommissioning the facility under the NRC's pilot program, and doing quite well in that sense.

The fixed head air sampling system we have left in place in the system. This amounts to 13 fixed head samplers on the perimeter of the building that we're going to leave in place as long as I can.

The final intent here is to decommission the facility to the point where we tear it down. We're going to demolish the building. But as long as I can, I'm leaving that fixed head air sampling system in place.

Related to the characteristics of the facility, there are really two buildings, it's like two oblong constructions sitting next to each other. The main building, the floor area is 142 feet by 40 foot wide given about 57,000 square foot, with a height of 30 foot on the main building.

The tank room, which is the most contaminated area, is a smaller adjunct and actually at a lower level, but in terms of size it's 48 by 12 foot with a height of 18 foot.

One of the problems in this facility is that we have no ventilation system operational any longer. It's basically static air conditioned. So I don't have the turnover of air in the facility.

Related to your comment, Henry, I didn't put it on here, but the floor is a concrete floor, painted, initially painted, and I'll show you some data. We've actually bead

blasted the floor, so now it's a raw concrete floor.

Getting into where the information I have with regard to air sampling, as I mentioned, we have 13 fixed head air samples, points around the perimeter of the facility. The change frequency for those air filters is one to seven days, depending on somewhat when they get around to it. The facility is empty at this point in not operational. We're not really working on it right now. So we've gone as long as seven days before the guys got back to change out a filter. But in general, we do it twice a week, three or four day frequency.

The flow rate of the units is 17 liters per minute. There is a substantial dust buildup on the filters. The counting time of that sample is 20 minutes, that's after a decay time of at least 24 hours and usually a couple days to decay out the background uranium thorium.

We're using a Tenelec counter, an automatic gas-proportional counter with a background of about two counts per minute in beta.

I've got this information in there. I'm not going to go through any of the actual numbers, but it is in the slide. These are the actual beta air sample results in microcuries per ML times ten to the minus 13 for the units.

The main point on here is I've broken it up into two blocks. The top block there is measurements that were taken during the period when we were physically working in the facility, and the second is this block down here is measurements that were taken when there was basically no activity in the facility.

Graphically, this data does not look like what you have in your handout. The difference is that on the handout, I had just a linear scale on the side, and in this one, I thought it looked a little better if I -- you could see it a little better if I did a semi-log scale.

But you can see the activity, the difference in air concentration depending on what's going on in the building. This first section here is prior to entering our phase 2 of the operation. Phase 2 was primarily just to go in, remove all the equipment from the building, not really do any decom, but we did do a little bit on the floor, we did a one-pass bead blast. So

what we were doing was waste packaging. We did do the period of shotblasting the floor, and then we've had this period where basically there is nobody in the facility except for the HP techs going in and doing the air sample changeout. So this last period is of very low use activity.

There is actually one piece of equipment that is still in the facility and still periodically used, so there is a little bit of activity. When I say its inactive, that's not quite true, but for practical purpose, it is.

The bottom line on those was that for the period -- I broke my -- I'm going to break my answer up into two pieces of data. Prior to and during the floor decom activities, the average air concentration is about 1.3 E to the minus twelve, and during that totally inactive period, it dropped by a factor of ten to about 1.3 times E to the minus 13 for microcuries per ML for the air sampling.

The air samples -- that graph really reflects the level of activity being conducted. As a point of comparison, the concentrations are very low. This is an operational facility but -- or was an operational facility, but even during the operation, we were not really much higher than that. That 1.26 E to the minus 12 was about the same as the prior six months of operation of the facility. We were actually during the prior six months where we were doing work in the facility, we were actually closer to one times ten to the minus 12, so this was a little bit higher when we were doing more aggressive packaging and bead blast.

Those are the results. As I said, it's about during that first section 1 times ten to the minus 12 dropping by a factor of ten.

The bead blast decom of the floor was strictly a one-pass operation, was not intended to decom the building. We just wanted to see how quickly the activity would come down on just one pass of it, and it really depends a lot on the construction and the age of the floor.

The section of the building -- the building is actually -- I only described it as one main building, but it's actually two buildings, the original building, which is about 40 percent of

the total floor area, and a new building, which is about 60 percent of the total floor area.

If you look at the new building, which is only a little over ten years old, which about one coat of paint on the floor, bead blast took it down in almost one shot.

In the old building, which was very -- had been painted many times, had been resurfaced a couple of times and lots of things going on, bead blast didn't come down very much on one pass.

Now let me move to the surface activity measurements, the other half of the equation. As I mentioned, the problem with an operational facility, and operational data trying to go back and look at it, is if I look at the last year worth of data that we have during the operational phase, we had thousands of air sample measurements and thousands of smear and zero fixed surface activity measurements, and that really becomes the problem when you're trying to take an operational facility, you're doing something, you're asking for more data than is normally collected in an operational mode.

The problem I have with my data at this point is that at this point in time, I really only have 29 measurements of total activity measurements, fixed -- you know, total surface activity measurement. I've got lots of air sample data, but I'm really limited on that. So that's one constraint to what I have. We used 100 square centimeter gas proportional probes with a one-minute integrated count to do those measurements.

Looking at the data, this is in your handout, forgetting about the whole thing, the bottom line really comes down to this: our average surface concentration during the period or the initial period and during the bead blast, using that data, that's really one set of data before we did everything. I used that to calculate the activity for the entire period, recognizing that as we're bead blast, the floor concentrations are going down. But we were at about 160,000 DPM per hundred square centimeters average over the facility.

After the floor decom, the one-pass bead blast activity, we dropped it down to about 65,000 DPM per hundred square centimeters.

With respect to the ratio of total to removable, supporting what Boby was saying, we've had very little removable activity in this facility, at least a ratio of about 200. Before the bead blast, it was about 256. After the bead blast, we had a little bit more dusting on the floor. That ratio dropped down to 130. But that's still less than one percent of the total activity on the floor was related to smear.

[Pause.]

MR. NARDI: No comments.

As I mentioned, we have very limited data during the normal operational phase with respect to total activity. We have lots of it on smear and air sampling.

I have limited data at this point. It's only 29 sample points that were based on a grid pattern; intended to try to be non-biased; just a grid through the facility to try to get measurements that would be a reasonable average.

The instrument calibration would not comply with MARSSIM. We weren't trying to get real good things, so I don't have all of the factors of back scatter and all of the other things that would be in a MARSSIM type survey data. So that's a limitation I have. As I mentioned, the removable to the total activity ratio is around on the order of magnitude of 200.

If I take that data, then, and break it out into the two periods, the one prior to and during the floor decom period with the air sampling and the surface measurements that I gave you, I come up with a resuspension factor in the order of 1.7 E to the minus seven. For the inactive affect the floor decom was done and during that period when we're basically inactive, you start calculating a resuspension factor in the order of four times ten to the minus eight.

Okay. Comments.

The operational data indicates that the resuspension factors should be closer to the range of ten to the minus seven, maybe down to ten to the minus eight kind of a range, not the ten to the minus six type of range that we're currently talking or potentially talking about.

They're certainly below the data that -- this is Boby's slide. If you look at that,

that means that I'm sitting on for the phase where we're doing some work in the facility, down around 1.7 times ten to the minus seven, which would be this point there, or four times ten to the minus eight would be way over here. So I'm way down below the range of the experimental data that was given in this information.

The data with four times 10 to the minus 8 has to be taken with considerations of the factors, but it really represents an essentially inactive facility. There's very little movement around there, so that perhaps represents too low of a range on what it can be.

Hopefully we're going to get some more additional data during the next phase of the decommissioning. As I mentioned, we'll be keeping the air sampling system operational as long as I can.

This gets me into the second part of the comments that I want to make, and that's measurement considerations, how good can we do.

As I mentioned several times, operational data does not normally provide the data needed to calculate the resuspension factor, and supporting what has been said today, I really believe that using removable contamination is not appropriate. But this whole exercise has really led me to another consideration, and that is that it becomes very obvious to me that the minimum detectable activity on your air sample measurements are very quickly going to drive the limitation of what you can measure.

And to give you an idea of this, this curve is nothing more than the mathematical equation that that resuspension factor, which is just a ratio of airborne concentration over total surface concentration, comes out for a family of curves where I'm looking at the lower end, 10 DPM per hundred square centimeters, there's the bottom curve going up to 3,000 DPM per hundred square centimeters surface contamination for the top curve. So basically what it's saying is if you measure -- if you have an air sample concentration of 1 times 10 to the minus 13, 3,000 DPM per hundred on your floor, that's going to be the resuspension factor that you calculate.

This is strictly calculational. No data in here. But let's add some comments to it. First of all, this is some data based on a facility that we decommissioned at Large, Pennsylvania. Building 9 was a uranium fuel fabs type of a structure. That's what its original use was for in the seventies.

We decommissioned this in the beginning -- early part of the 1990 time frame. If you start looking at what I could -- the average total surface activity measurements for alpha and for beta, you really begin to realize that we're at very low numbers on a decommissioned facility. Looking at that last curve, you're way down in the bottom part of that family of curves. You're not up high. And that's going to be what I've seen in all of the facilities that I've been decommissioning.

The only place we had some measurements were really in the pipe chases. Those when they decommissioned a facility in '72 the pipe chases they didn't do quite as good a job as the general floor, and we had to go back in and look at those and do a little bit more decommissioning remediation of those. So if you look at the beta activity, you're in the range of the 2 to 300 DPM per hundred. And that was the highest place we had in the facility. And those are not really accessible surfaces. Very easy to just simply -- we could have just poured concrete in them and closed them out if we had wanted to for that.

The site average for all of the facilities on the site, and there were ten buildings used for various purposes, but we had 5,000 measurements of total surface activity for alpha and 5,000 for beta. We're way down on total surface activity measurements.

So that gives you a little bit of an idea of where we would be on that family of curves, I believe, for a reasonable effort to decommission a facility.

So my initial conclusions are that the total surface activities in a decommissioned facility are going to be low. The alpha surface activities are generally in the noise range and more subject to confounding factors of surface condition and self-absorption factors that you would have to take into account if you were really going to try to measure in an

alpha-contaminated facility what your surface activity was.

And for the other part of alpha activities for alpha emitters is taking that curve again and adding onto it a reasonable range of what your MDA is for alpha measurements in air sampling. This is not surface contamination MDA, this is alpha air sampling MDA. These values derived from looking at what we get in our uranium fuel fab plant where we're only sampling for eight-hour periods, about the same sample flow rate, 117 liters per minute on the fixed head sampler. That's about what you get.

What it means is you're never going to be able to measure down into the 10 to the minus 6, 10 to the minus 7 range of resuspension factor. You'll always be below your MDA on your air sampling. And I don't know how to drive that one down much further. That one is a real tough one because you get dust loading on your air sampler, you get dust loading factors and other things that keep you from really driving the MDA down for alpha emitters.

So my conclusions then are that MDA measurements for alpha air samples essentially eliminates the possibility of using a decommissioned alpha-contaminated facility to obtain data for any resuspension factor of any value. Even using an operational facility to determine the resuspension factor may be very difficult. And as I mentioned, I derived that alpha MDA based on our operational fuel fab facility.

Looking at the same factors for beta contamination, these are the MDA -- the range of MDAs that I'm getting for the pump repair facility, and that drops it way down so I can begin to measure lower resuspension factors, but at this point in time my surface contamination is up in this level. That's about where you would get that. I can see very quickly that as I continue to decon this facility, it's not going to be very long before I'm going to drop below that MDA for that technique for measuring air sampling, and I won't be able to get any further data on resuspension factors. It's the driving force there.

So my conclusions on that are the MDA sampling for beta measurements of air essentially eliminates the possibility of using a decommissioned beta contaminated facility to

obtain any data on the resuspension factor. If you start looking at the 85 DPM per hundred that I showed you for our large facility, I don't know how I can drive my MDA down far enough to get a measurement on a facility like that. Using an operational beta contaminated facility to measure the resuspension factor is feasible, although it's complicated by trying to subtract out what are the contributions from operational factors.

At least operational factors, all they do is raise the resuspension factor higher than what it would be if it were purely turned over for unrestricted use. Considering handling contaminated equipment and packaging and repackaging and opening packages, all of those are going to raise your air concentration value, and therefore calculate a resuspension factor that will be high.

Even doing that, I am still getting numbers that are below those experimental numbers. So, my conclusions are that the recommended default value proposed, at least last month, is high by at least an order of magnitude. It is a little bit better today, given Bobby's presentation this morning.

If we are going to do this, I think we have to look for an operational beta contaminated facility to make the reasonable measurements, even though the resuspension factor is primarily important only for decommissioning an alpha contaminated facility. I don't think we are going to get by looking to alpha facilities.

The impact of human activity levels within the facility has been obvious to me, I can see if it plot out the data, and I really -- there is some further opportunity for me to gather more data at pump repair, but it is going to be limited in time, because we are proceeding to a point where we expect to have that facility demolished down to ground level by the fall.

Any questions?

MR. BURKLIN: Rich Burklin, Siemens. Jerry, you keep on talking about the minimum detectables. When you were doing your beta measurements, if you got a zero reading, do you use the zero or do you plug in the MDA?

MR. NARDI: All my measurements were above zero.

MR. BURKLIN: Were they above the -- well, let me rephrase it.

MR. NARDI: They weren't below the MDA. I have not seen any numbers on my air sampling that were below the MDA. So all of those numbers there are non-zero numbers.

MR. BHAT: Ram Bhat from U.S. Army at Fort Belvoir, Virginia. I have a question regarding this. You said that you used the gas per hundred centimeter square ADR gas proportional probe. Did you use the windowless or window gas proportional probe?

MR. NARDI: I'm sorry?

MR. BHAT: Did you use the windowless or window gas proportional --

MR. NARDI: No, no. That is a windowed gas proportional probe. That is like a Ludlum measurement.

Mike, do you want to get more specific about the model?

MR. BHAT: Is it a Ludlum model, Ludlum model?

MR. NARDI: Yeah.

MR. BHAT: You use the petane gas, petane gas?

MR. NARDI: Right. It is a 100 square centimeter opening.

MR. BHAT: I follow, yeah.

MR. NARDI: And with the mylar covering.

MR. BHAT: Yes.

MR. NARDI: And the petane gas.

MR. BHAT: Yeah. We had some problems, it was very much the temperature sensitive, and it was varying with the calibration, and we had to sometimes quite often calibration. The technology is not perfected. I think out of all the, Ludlum is the best.

MR. NARDI: Yes.

MR. BHAT: Compared to the other places I have seen. But, still, we had lots of radiation in the precision of the data and to the comparison. It was winter time, it was terribly

changing. And, secondly, when you did the surface activity measurement, what do you get, the total activity, and you are subtracting the suspension from the air measurement and, so, the difference you are calling it as removable activity, am I correct?

MR. NARDI: I'm sorry, no.

MR. BHAT: You have got -- when you took the fixed surface, you get a total activity.

MR. NARDI: That's right.

MR. BHAT: Yes. And then you are talking the air measurement and that is a suspension -- suspended activity. So difference between total activity and suspended activity, do you consider it as the fixed activity?

MR. NARDI: No. The definition of the resuspension factor is strictly the ratio of the measurement.

MR. BHAT: I understood that. I understood, sir. So then my next question is, did you get a chance to take the wipes to find out how much of the removable activity?

MR. NARDI: Right. Yes.

MR. BHAT: Did you do the wipes?

MR. NARDI: Yes. That is also on that curve, although I did not calculate any -- or not the curve, but on the dataset. These two columns are the fixed measurement on the surface.

MR. BHAT: Okay.

MR. NARDI: These two columns are the smear counted in the Tennelec.

MR. BHAT: Okay. Then next question I have is it is a little confusing to me, in the same table you are telling the -- in the last -- in the first column, the last one, total to removable is 256, and then in the next slide you say surface activity --

MR. NARDI: I rounded it. I don't --

MR. BHAT: No, that's fine. But you are -- it has become removable, removable

to total activity ratio is 200.

MR. NARDI: Right.

MR. BHAT: I mean has it been --

MR. NARDI: Yes. If you take this set of data and this set of data, you get that ratio of 256.

MR. BHAT: Yeah.

MR. NARDI: If you cite this set and this set, you get the ratio of 130 --

MR. BHAT: No.

MR. NARDI: -- and kind of visually average them and said about 200.

MR. BHAT: That's fine. In your next slide, you have a next slide, there you are telling removable to total activity as 200.

MR. NARDI: Oh, did I type that wrong?

MR. BHAT: A little confusing. Wait. Oh, I see. All right. Thank you then, sir.

MR. NARDI: I typed that wrong. I apologize, you are right. That ratio should be the inverse, it should be total divided by removable, yeah. I'm sorry, I did not catch that when I was looking through it. That is an error.

MR. BURKLIN: Rich Burklin, Siemens. Okay. Again, you said that the air in this building was basically dead air, whereas in a -- I need to have a facility that is going to be reoccupied, because we are talking about that type of scenario, you are going to have a change of air. In the modeling and so on, does the NRC consider this, because that ought to be taken into account, that the amount of activity in the air is going to be constantly swept clean by recircling air from the outside.

MR. EID: That's correct. When we look at this data, we will consider when there is activity in the facility and when there is no activity. So, most likely, we may consider the data, that they correspond to an activity, because our current scenario, which is called light industry scenario, although it is conservative because we did not consider all possibilities of

using the facility, as I said, it could be storage, it could be classroom, it could be lab, so what the current scenario is the light activity scenario. Therefore, yes, the activities, the measurements, when you have activities within the room, it will be considered rather than the measurements when you have static kind of air flow.

MR. NARDI: Yeah. In that facility, when we were doing the packaging and the other things, we did not have any building ventilation system on. Is that correct, Mike?

MIKE: Correct.

MR. NARDI: We only had the local ventilation, if needed for a specific task, which would be circulated internally through a HEPA system.

MR. EID: This has two actually factors. One factor is you may think that you do not have disturbances, mechanical disturbances to increase the resuspension factor. On the other side, you say, well, you do not have depletion of the source term, too. So the source term is not depleted by ventilation. So you have these two factors working against each other.

MR. NARDI: If I think about it from my standpoint, using an operational facility, like in this situation, not having air movement and air change-out to remove the airborne concentrations, dilute it down, in essence, means that my air -- my resuspension factor is still too high, and, yet, I am way down below the experimental data.

MR. EID: Yes.

MR. NARDI: I just keep coming back to every time I look at where my data isn't good it tends to mean that I am higher than I should be.

MR. GENOA: I just wanted to -- Paul Genoa, NEI -- amplify that last point so that it doesn't get lost. In your activity scenario, you had activity going on that could generate, or suspension --

MR. NARDI: Right.

MR. GENOA: -- and, yet, in general, you do not have ventilation that would remove it. So it is a very conservative number.

MR. NARDI: Yes.

MR. EID: That's correct.

MR. NARDI: Yeah. And that is the point I keep trying to make, is that, you know, based on what we have been looking at on experimental data, I really think that experimental data is up too high. I think it should be lower than that. And if I look at your last slide, at least with this, using a 50 percent, and everything you talked about, we are getting down into the range of where we can do measurements, but I think it is still conservative in terms of a dose calculation.

MR. EID: I think that is a very important point, we would like to know if it is still conservative concerning the 50th percentile value or not.

MR. NARDI: Right

MR. EID: Because we have some other kind of arguments here, well, you are not sufficiently conservative for screening analysis. So the data that you presented, you know, considering that the quality of the data is acceptable, I think is something to think about.

MR. NARDI: And the other bottom line conclusion of mine is that I really think we have got to go looking for facilities that are beta contaminated facilities if we are going to gather, you know, real data, not experimental data on it. I don't think we have chance with alpha contaminated facilities. Henry.

MR. MORTON: Henry Morton. And intuitively, Joe, I am inclined to think that the beta contaminated facilities, if anything, are going to tend to give more conservative suspension factors than the alpha contaminated facilities. And, basically, the basis for this intuition is that we know from experience in operating facility -- plutonium facility, and the thorium facility, or uranium facility, but, particularly, contrasting plutonium facility and a thorium facility, where, historically, the airborne limits, occupationally, for plutonium, low burnup plutonium, and thorium, natural, are within a factor of 10 of each other, and yet we know that you have to keep the plutonium facility orders of magnitude cleaner than the thorium facility.

And one of the things I looked at to try to see whether -- what might be responsible for the difference is to look to see whether the dust loading was beginning to be the limitation for the thorium on a mass basis, since suspension is really a mass phenomenon and not an activity. There is something that would seem to be different as a function of this vast difference in specific activity. But the air seems to be able to hold that much dust of either one, so it didn't seem to be that.

I don't really understand what's going on, but intuitively it seems to me that the suspension factor for the really low specific activity materials is going to be less than it is for the higher specific activity materials like some of these fission products that you've dealt with as beta emitters.

MR. NARDI: My last comment is if anybody can help me with an approach to drive that MDA down, I would like to hear it. But I can't see driving it down much further than what I am with a Tennelec type of approach. I am talking to the chem lab to see if compositing air samples for a long period of time and doing a gamma spec measurement may get me long-term averages. It won't get me individual sample measurements, because I don't believe that you'll ever see it on that. But it's really surprising if you start looking at an air sample of 1 times E to the minus 16 and you calculate out how many picocuries you're going to get, even taking a quarter's worth of samples and compositing them for 13 fixed head samples, I only came up with three picocuries of activity on that total collection of air samples, and I don't think I'm going to get there. I don't see how I can drive that detection limit down to the point where I can make measurements in a decontaminated facility ever.

MR. SEXTON: Dick Sexton, Connecticut Yankee. I agree with you that I think that is the limiting factor, but I guess operationally there are in many facilities areas that have fixed contamination --

MR. NARDI: Yes.

MR. SEXTON: Little or not removable contamination, and fixed contamination

levels at easy-to-monitor levels. And many of those facilities have fixed air sampling that is constantly being obtained. And I think --

MR. NARDI: We just don't normally do the fixed --

MR. SEXTON: Right.

MR. NARDI: Measurements.

MR. SEXTON: But could --

MR. NARDI: You could.

MR. SEXTON: You could go back even in a retrospective manner go back to those locations, obtain fixed readings, and see where the data might lie. And I think operationally we know that the resuspension factors are very, very low because we have years and years of data both with the fixed head and also with sometimes even lapel air samples for those type.

The other thing I was wondering is has anyone looked at any other data for other type of contaminants? Lead? Anything else? Any other -- to look at the resuspension factor on some other type of component that would have a similar concern? The first thing that comes to mind is lead.

MR. NARDI: I don't have anything.

MR. POTTER: Tom Potter. I can't answer that last question, and I almost hesitate to bring this up with respect to your situation, Joe, but as a research situation kind of one-time-only kind of thing, it might be practical -- I agree that you will not be able to do it with radiological analysis of the air samples, but conceivably something like ICPMS analysis of a large number of composite air samples might be a way to do it. You can detect about 50,000 atoms of uranium, for example. They're very expensive, a couple of thousand bucks a throw. But with single composites or a few composites of a large number it might be practical.

MR. NARDI: Okay.

MR. BEYELER: Walt Beyeler from Sandia Labs.

I was looking at the distribution that Boby had presented earlier, and I was just -- it seems to me that some of the difference perhaps between the results that you're seeing and this distribution is due to a difference in the ratio between fixed and removable. I was interested to see the high number of 1 in 100 in your case, and I was curious if you felt that that was typical of other decontamination facilities and how difficult it was to collect that information on a ratio of fixed to removable.

MR. NARDI: With respect to the ratio, I think that's very typical of every facility we've deconned. Invariably taking smear measurements on a decontaminated facility is essentially useless. I've said that in a workshop in the past. My smear activity measurements are almost always less than the minimum detectable on the counter. I just don't see it in a decon facility after remediation.

MR. BEYELER: So that ratios of 1 in 100 are fairly common then?

MR. NARDI: Well, 1 in 100 is probably too low. It is probably more like 1 in 250 or 1 in 500 I think would be realistic. If you looked at that data I showed for the large facility, it's like 1 in 85, but that is really fictitious because there's an awful lot of less than MDA numbers in that.

MR. EID: Any more questions?

[No response.]

MR. EID: That is an excellent presentation and thank you very much.

I guess we have some real data we would like to take a look at and we would like to have this further kind of discussion and generation of more data, as much as we can, so we could have more confidence if we need to change the resuspension factor or any other physical parameter.

Do you think it is the time to break now?

MR. ORLANDO: Yes. It is about ten of 12:00. We will break and come back at 1:00 after lunch.

[Whereupon, at 11:50 p.m., the workshop was recessed, to reconvene at 1:30 p.m., this same day.]

AFTERNOON SESSION

[1:09 p.m.]

MR. ORLANDO: I guess we can go ahead and get started again.

The first speaker this afternoon is Carol Hornibrook. She is going to talk about a project that she has been working on on modeling embedded pipe.

MS. HORNIBROOK: Thank you very much for the opportunity to make a presentation. I am going to be talking about dose modeling of embedded pipe and there's a lot more to this than I thought, so unfortunately I am not going to go into actual modeling this time. I am hoping to be invited back again.

What we are really going to look at is source term development for embedded pipe because as all of you are aware, there's two important things when you are trying to do some dose modeling. One is to have a decent source term that you put in and the other is to have a decent model that you can put that source term into.

So the first question would really be why do I need a good source term, and obviously what you want to say is because MARSSIM is here, meeting the DCLG for building occupancy and here it is predominantly cobalt is the major nuclide and also meeting the DCLG for rubblized field or landfill.

The way I looked at this for EPRI -- okay -- well, let's look at measurement techniques and our approach to this was looking at it the same way that we do in a sense in low level waste in terms of scaling factors. The first three techniques that you would consider for development of these scaling factors and they would be developed against cobalt in this particular instance, would be using smears, a crud scraping or total etch, and actually these three techniques have been used successfully at utilities who are decommissioning and the first two, the smear and the crud scraping, are used pretty extensively.

Once you develop your scaling factor, you need to determine what your real concentration of cobalt is because this first part here has given you what the actual mix of

nuclides are. Then what you want to do is figure out just how much cobalt you have, so then you can figure out how much of the other nuclides you have.

There are two possibilities for doing this. One is the dose to curie -- and I would ask you to correct this on your slide -- the other is gamma spectrometry -- it is not spectroscopy -- in order to determine what the actual Cobalt-60 concentration is.

Once you have this information, now you can actually calibrate the pipe crawler that you are using so that you can calibrate it for the actual mix of nuclides that you have in the pipe itself.

Now I want to stress actually when you use this technique -- when you use these techniques you don't have to do this for every single pipe, okay? -- you really just have to do it on a small number of them so that you can get a comfortable feeling with the source term that you have developed. It is not something I am encouraging people to do everywhere.

As I say, in the development of scaling factors, the industry has been doing this since 1981 when 10 CFR 61 came out so we have a lot of experience with it. We know it is a good approach to use.

Now in the dose to curie what we wanted to look at was with the utility that had used the etch technique, which is an acid etch -- what you do is you take a piece of pipe. I am a little bit lost because I don't have a pen in my hand. Could I borrow yours, Nick? Thanks. Now I feel more comfortable.

In this particular case it was a bleed line pipe and it was a 3 inch diameter pipe, a 3.5 inch long piece of pipe, so they took a little slice out and the wall thickness of the pipe was a quarter of an inch. What they did was acid etch that particular piece of material and what they came out with were the results here -- the concentration of the nuclides and I picked out the more important nuclides for this particular presentation.

They also did a smear of that particular pipe before they actually did the acid etch, and when they developed what their concentration was in the pipe, they summed these

two columns and came up with their activity sum here.

So their total was 4.71 E to the minus 2 microcuries per centimeter squared.

Now when we looked at this and wanted to use the dose to curie approach, as opposed to gamma spec, what we did was ask them for data on dose measurements, which they took for us at one foot and two foot intervals, and we took this mix of nuclides. We took this data and turned it into a relative concentration and relative amounts, and then what we did was put it into the microshield code and compared it with the doses that they gave us, okay?

What we found was that the actual, the concentration the microshield projected for this particular pipe was very similar to what the utility itself had gotten.

I am assuming you are riveted to this -- okay. Now one other thing that we wanted to do was we wanted to develop scaling factors also for these two, the smears and the etches that the utility collected and we did that just to see what kind of comparison we would get with the smear data versus the actual etch data, and as you can see from here it is actually very close, very similar data. Granted this is only one sample, but I thought it was significant and should point it out.

The last thing is if you look at the percent of total activity of these nuclides you will see, as I start off with my premise in the original opening of this presentation, that the majority of the activity is Cobalt-60.

Let's hope shorter is better. Okay. So, what my -- you know, and this is just the beginning of looking at this larger question, but at this point our feeling is that it is really important, as I said before, to develop a good source term. The methods that I have shown you so far, that I pointed out earlier in the paper, will really help you to calibrate the detector to the nuclide mix that you have in the pipe, and, as I said, it incorporates the same methods that we use for low level waste, that we have been doing since 1981, and also the calibration only needs to be done on a limited number of pipes. I really want to stress that. Okay. So hang with me here.

As far as future work, looking at source term development, what we would like to is more of these dose to cure conversions, get a little bit larger database that we are presenting on this. We also would like to look at gamma spectrometry, and do the same kind of calculations and see how well they match up. And, also, we would like to do more in terms of verifying this smear and crud scraping to look at the similarities that you actually get there.

So, I think we have a good approach here. I would be curious to have input from you. No, huh? Are there any questions at all from what I have presented, or pretty obvious?

MR. SAITO: Earl Saito, Combustion Engineering. I am a little confused, and maybe you can help me out. What you are trying to prove, if you look at the numbers you had up there, you are talking about a two feet -- about .9 millirem a day from this pipe. So that clearly will be over 15 millirem a year, if you are looking at just the pipe. So you want to know what is there, and then you will model the rest of it? I mean what is the end goal?

MS. HORNIBROOK: You are trying to model -- you know, if it were left in the walls itself, what the contribution would be. And you bring out a good point, we didn't -- I didn't show the background, the subtraction that would occur for background. I just wanted to look at the initial numbers and not confuse it with that discussion. Okay.

But what you are trying to do is get, for the building that is standing, to try to understand what exposure. So it is one component of the total dose modeling.

Man-Sung, would you want to add anything to that response?

MR. YIM: Well, I think if the question is more about the importance of what she just described, we usually have some samples of smears, or we have some samples from the crud, and, basically, that is all have, we have a concentration or some smear sample. And can we use those numbers to characterize your source term? What are the uncertainties you are talking about?

So we wanted to see if we can develop new approaches, the same approaches

that we use low level waste characterization and see that approach actually make sense with this embedded piping issue, too. And, basically, she demonstrated that makes sense. Low level waste characterization technique can also be applied to embedded piping.

MS. HORNIBROOK: And, also, when you are putting your pipe crawler through, you want to be able to calibrate that -- sorry. You want to be able to -- no, can't move that hand either. I feel those Irish dancers now, no moving the arms. You want to be able to calibrate that pipe crawler, you know, pretty reasonably, reasonably well.

MR. SAITO: Okay. So then the dose -- even though the dose from this pipe would be above the limit if this pipe was out in the middle of the room, you will then take that and use it in some future scenarios for this pipe will then get torn up and thrown somewhere else. Okay.

MS. HORNIBROOK: Right. Right. I am sorry if I was confusing on that. Good. That it? Okay. Very good. Thank you.

MR. ORLANDO: Thanks, Carol. The next couple of speakers are Bobby Eid from NRC and Man-Sun Yim from EPRI, and they are going to discuss the differences between D and D and RESRAD for modeling strontium-90 and cesium-137.

MR. EID: Good afternoon. I guess I do not need to introduce myself. I introduced myself this morning. However, if anybody is interested to contact me, I have provided here my phone number and my e-mail. I did not do that in this morning's presentation.

The title of the presentation is regarding the differences between DandD and RESRAD for dose modeling specifically for Sr-90 and cesium-137.

The presentation outline will be to give you some idea about the background behind the study and then the comparison of Sr-90 and cesium-137 dose values using default conditions for the two different codes, RESRAD and DandD.

Then also I would like to talk about dose analysis for specific pathways. This means what kind of contribution for different pathways in the dose modeling from these two

codes.

Then to talk about the dose analysis using different input parameters specifically for DandD. In case we change the parameters, what kind of doses we obtain.

Then talk about two specific parameters which as a result of this study I found are quite sensitive parameters, specifically for Sr-90, the soil-to-plant transfer factor, which is found to be a significant parameter, and the distribution coefficient for cesium-137.

Then try to say something about the causes of anomalies which was pointed out in the previous workshop regarding the doses obtained for Sr-90 and cesium-137 using DandD version 1.0.

And then hopefully we'll have some conclusions and some suggestions.

To start with the background, I would just like to refer that Sandia in their report did the comparison between RESRAD and DandD screen, and Sandia in their report indicated that there are differences in the dose results about 15 times greater using DandD screen when you compare to RESRAD. So, yes, there are some differences already in the two codes.

Then also during, as I indicated during the previous workshop, the industry indicated that there are some anomalies in the values of Sr-90 and cesium-137, and they requested us to take a look at those values. So we did. We tried to do our homework.

What we did, we conducted detailed pathway analysis and assessment of DandD parameters input that we may think that they may impact the Sr-90 and cesium-137 dose values.

We also conducted comparative analysis of the parameters between RESRAD and DandD and some sensitivity analyses of those parameters.

I would like to take a step-by-step approach so you can understand, you know, this work and what kind of conclusions we can derive from this work.

The first step, we like to look at the two parameters at the two codes that we are having, RESRAD and DandD. And then we say okay, forget about the models, what kind of

parameters differences between these two codes. So if you look at, you know, possibly about 300 parameters, you will get lost. But you focus on the most sensitive and most important parameters, and that's what I did in this table.

First of all, the contaminated zone area for DandD, I'm talking now about the default values for the two codes, is 2,400 square meters, whereas in RESRAD it's 10,000 square meters. So you have a much larger area for RESRAD.

The contaminated zone thickness for DandD is assumed .15 meters or 15 centimeters, whereas in RESRAD it is assumed to be 2 meters.

The contaminated and unsaturated zone density, they are more or less somehow similar, 1.5 versus 1.43, so we do not worry about the density.

The contaminated zone and unsaturated zone porosity, somehow there are some similarities, .4 and .46.

The infiltration rate, there are two now in the models, the one model assumes there is infiltration rate. The other one assumes there is a precipitation rate. So RESRAD will assume that not everything precipitated will go through. So there is, you know, there are some differences somehow.

Anyway, the infiltration rate assumed in DandD model is .25 meters per year, whereas the precipitation rate, it is between parentheses (P), for RESRAD is 1.0 meters per year.

The irrigation rate, also they are somehow slightly different, DandD assumes the irrigation rate for the whole year, and RESRAD assumes there is a period of irrigation. And for the whole year the assumption in DandD is .47, whereas for RESRAD it is .2. So if you consider the irrigation time possibly you may find some similarities between the two codes.

The unsaturated zone thickness assumed in DandD is 1 and now anyway you could change also this value, whereas in RESRAD it is 4.0.

The dust loading factor is specifically for DandD is for gardening, it is .0004,

whereas for RESRAD it is .0001. So there is a factor of 4 higher for DandD, although they are treated differently in the two codes.

The soil-to-plant transfer factor, which is the one I'll be talking about, is quite significant, so comparing between RESRAD and DandD you will find some significant differences.

For Sr-90, which is the first value on the left, for DandD it's 64. However for RESRAD it's .3.

For cesium-137 it is .018, whereas it's .04.

So we'll find some differences in the soil-to-plant transfer factors.

The plant mass-loading factor is .1. We believe this is still highly conservative in DandD, and we have general consensus that this value should be changed. Correct me if I'm wrong. This could be changed, right?

The distribution coefficient, differences between the two codes, you can see that for Sr-90 it's 31, for RESRAD is 30, they are almost identical, so we do not have much differences.

Now the distribution coefficient for cesium-137 is quite different. One is 10; the other one is 1,000. And I believe those are, you know, significant differences between the two codes.

So to make it simple, we need to see, assuming that the two models they are the same, what differences you expect because of the input parameters. I believe the first look you think that well, yes, the contaminated zone area is different, the contaminated zone thickness is different, those are the major differences, and the rest in these two physical parameters, the soil-to-plant transfer factor and the distribution coefficient.

So then the next step is, try to have the source term related to the contaminated area and the thickness of the contaminated zone, make those identical somehow between the two codes, and then you go a step further and see what are the differences.

So this is the comparison between the two codes. First of all using each code's own default parameters. In the first two lines, for example, for cesium-137 I used DandD default values and the results were 28.3 millirem per unit concentration, which is 1 picocurie per gram in the soil. For RESRAD the value is using RESRAD default values it's 2.3. So you could see there is significant difference between the two codes. For Sr-90 it is 59 versus 5, so there is one order of magnitude difference in the doses.

Now the next step you say okay, try to vary the geometry of the contaminated zone, make it .15 centimeters, you know, for RESRAD, try to be close to what DandD code they have, and try to vary the thickness of the contaminated zone -- sorry, in addition also the thickness of the unsaturated zone, make these variations to make them exactly the same. And what kind of results do you get using these two codes? So the next two lines you could see the differences that still DandD dose values per unit concentration, 28.3, and RESRAD slightly reduced to .1, but the reduction in the dose is not that significant. So it did not really impact, you know, varying the contaminated area impacted somehow the dose, but not that much.

For Sr-90 almost the same phenomena you find that you have 59.4 did not change practically when you vary the thickness of the unsaturated zone and the area of the contaminated area you will find the differences still they are somehow similar, although you started to get some reduction in the dose for RESRAD because of the different area and the different thickness.

Then we tried to think to say, okay, where is the problem? In order to analyze the problem, you need to think about what are the major pathways they are impacting the dose, or they are causing -- where the dose is coming from. So I try to look at D and D and RESRAD to see what are the major pathways using the peak dose for each code, regardless of the time. So the extent -- as you can see, if you look at the column with Sr-90 for D and D, the dose that is coming from agriculture pathway represent almost most of the dose, or the majority of the dose, which is 99.75.

If you look under RESRAD and look at Sr-90, you will find also the dose is coming from the agricultural pathway. So you will find consistency in the dose related to these pathways.

When you look at cesium-137, the story is different. So you find that the major dose for D and D is coming from the aquatic pathway, whereas, for RESRAD, the major dose is coming from external exposure pathway. They are completely two different pathways.

And then you start thinking, say, why you have these differences. The difference is because you have two different models, because you are having a model which allows very fast removal of the radionuclide or infiltration of the radionuclide to the aquatic or to the aquifer, because you have three box model, whereas, in RESRAD, it does allow for retardation in the unsaturated zone and in the aquifer, that is the major reason, is because actually RESRAD just delay the dose rather than try to reduce the dose. And I will demonstrate that in the next slide.

So the peak dose was actually, I believe, the first year or the first five years, in the first analysis. Now, I said I will look -- assume that after 100 years, what will happen. Do we have the same pathway, the same proportions of the dose related to those pathways, or we will find some differences?

If you look at the -- if you compare these pathways for D and D and RESRAD, you will, yes, the story is now is different after 100 years. So you have the proportions of the pathways that are from aquatic. All those, still it is high from D and D, however, you will find also some additional pathways, they start to appear, it is called irrigation pathways. However, for RESRAD, we are seeing more significant pathways now from, you know, from the drinking water, which is now the aquatic pathway and the drinking water pathway is becoming significant.

This means, you know, it took that time such that these pathways to become significant. So RESRAD, it deals with it in a way that will -- it will adjust for retardation and going through the unsaturated zone when it goes to the aquifer, whereas, in D and D, it goes much

faster to move the material from the contaminated zone to the aquifer, and that is the major reason.

Okay. The next step, we said, okay, let's try to look at what are these parameters, they cause, you know, differences in the doses. We looked at the -- certain parameters that we believe they could impact the dose, and, for example, the gardening dust, we try to change the gardening dust now looking at D and D code only, try to analyze what are the sensitive parameters. So the gardening dust, if you vary it by a factor of 10, you change -- you have no changes in the dose. If you look at the third column, with the default values in those millirem, versus the last one, you will find the dose is the same practically.

And now when you change the soil-to-plant transfer factor, you change it by only, you know, by having, instead of 64, to have it 32, then you will find the dose almost is reduced by that magnitude. So this means reduce by 50 percent, because you reduced the soil-to-plants -- to plant transfer factor by 50 percent.

So, immediately, you could realize that how this factor is important and quite sensitive. If you vary it by a factor of 40, you will find somehow qualitatively similar kind of variation. So you are getting from 59 to 3.52. So, immediately, we realize that this is a significant factor. So this way, actually, this is also to demonstrate how can you vary your dose based on some kind of information. You look at these parameters, that they could impact the dose and then you start thinking about them. Are they highly conservative, not conservative, appropriate the site, inappropriate for the site? That is the way the source of dose analysis, you look at and try to change your pathways, and to look at the sensitive parameter.

Then we looked at the partition coefficient for Sr-90. We find the partition coefficient is not that significant to change the dose for Sr-90. Remember, this is for Sr-90. Now, the story, it could be different for cesium. Then change the number of zone layers, instead of one zone layer, make it 10 zone layers, you will find, you know, the differences, they are small. Still, you increase by a factor of about 1.5 percent of the dose.

Then look at the fish bioaccumulation factor, you change it by, you know, you divide by two, then you find also not much differences. The unsaturated zone thickness, we vary it from meter to 10 meters, the dose did not really change much, is by 1 percent or 1.5 percent. The plant mass-loading factor for that case also did not change that much. So I guess these parameters will depend on the specific radionuclide. Certain parameters, they do impact the dose, but for that -- for the specific radionuclide, other parameters, they may not.

So now we look at the cesium and to see how the dose will change for the cesium. For the cesium, gardening dust did not change much the dose. The soil-to-plant transfer factor did not change much.

Now, the partition coefficient, or the distribution coefficient, which is the Kd, you can see that it is 10.5, it changed by a factor of 10. By order, one order of magnitude, you can see how significantly the dose changed. So, from 28.3 to 2.42 is one order of magnitude difference. So this means the partition coefficient for cesium, it is the -- one of the most significant and sensitive parameters when you try to assess the dose. This means you will go and review your input values, say, are they -- these values, they are good for the site, or they are conservative, not conservative, what are the bases for selection of those sensitive parameters?

Then try to change the number of zone layers. Again, there is some difference here in this case, as you can see, and the difference could be, you know, by about 10-15 percent. So it is, there is a difference, but is not as much as the partition coefficient.

The fish bioaccumulation factor, you could see that the difference is not that much. There is some small difference. The unsaturated zone, it is significant thickness. So if you have unsaturated zone thick of one meter, and thin meters makes a big difference in your assumption and in the dose that you calculate. So that is another parameter that you need to consider when you look at the site-specific information in the dose impact analysis.

Then I tried to look at the plant mass-loading parameter. Again, did not, for that specific case, did not change it.

From this analysis, so the conclusion that we need to pay attention to two specific parameters, the soil-to-plant transfer factor for Sr-90 and the distribution coefficient for cesium-137. So try to go, to take one step background and to see how this data, you know, was adopted.

For the D and D Version 1, the value that was adopted and we look at it in the input parameters, and you will find that the value for Sr-90 to soil-to-plant transfer factor for leafy vegetable, it is 64. For other kind of food diet, it is very small, that is why not bother to take a look at it, and it will have less impact. Whereas, for leafy vegetables, it will have significant impact. So it is a significantly high value, which is 64, and the question, is this value is justified to be adopted as a default value or not? That is really the question which I will be discussing.

We also looked at the old 5512 Volume 1, what kind of value that was recommended, and we called it -- this is the deterministic value based on best estimate or based on best judgment of the technical reviewers and the value that was selected is 1.6, so you can see a difference by a factor of 40 between the current value in the D and D Version 1 and the value in the original Volume of 5512, Volume 1.

But you could say, well, they are two different now because the Volume 1 is not probabilistic whereas D and D, 1, the parameters were selected based on probabilistic approach.

Sandia submitted a letter report almost a year ago or more in order to look at the soil to plant transfer concentration factors. They provided data based on lots of literature values and they listed data where they had the mean value and the geometric standard deviation and the geometric standard deviation plus the two sigma value or the two variation in the geometric standard deviation, so these values, the mean is 1.8. If you compare 1.8 versus 1.6, you will find them very close. Actually those are comparable values.

The question, if you use the mean value you will find some kind of result they are similar, but however we are not using the mean values. We are at minimum selecting the

probability distribution function to the 90th percentile value, and then it could be actually higher than the 90th percentile value.

Then in the SAR letter report also they provided frequency sample soil to plant parameter distributions for the SAR and the 90th percentile value for the specific radionuclide that was given -- now underline the 90th. It is 9.4 whereas currently we have in the code 64.

Go back and look at RESRAD, what kind of default value input for RESRAD. The RESRAD default soil to plant transfer factor is .3 It is also I believe the value of selected base on best estimate, I believe -- correct me, Dr. Charlie, if I am wrong, I mean how this value was derived, so the value that is quoted from RESRAD is .3; this is 5.81.

The Sr-90 literature value -- leafy vegetables and forage plants transfer factors, these values they are listed from different references. You can see it for all of these references, Napier and Kennedy and Strenge. This is the 5512, Volume 1, and those are significant references. They collected lots of data and the value that was proposed for them is 1.3 for the soil to plant transfer factor.

NCRP values, IAEA and for different kind of references it is .86 so this will give you perspective when you compare the number and the kind of default value, 64 versus what are the common values in the literature.

What I gave you -- remember, the current version of D and D is based on probabilistic selection, the 90th percentile or higher, whereas those are based on the best guess or best expert judgment, what is the best value to use in the dose impact analysis.

Okay. With this in mind we will move to the partition coefficient or distribution coefficient. The current version has partition coefficient for Cesium-137 of 10.5 milliliter per gram. The partition coefficient is the ratio between the concentration in the soil versus what you have concentration in the leached water.

RESRAD version 5.82 default values is 1000. See the differences. I believe it is a significant difference between 10 to 100, two orders of magnitude. In NUREG 5512,

Volume 1, which also was based on best estimate and tried to be conservative it is 272, so you could see the differences between currently what we have in D and D, Version 1, and what we proposed originally in 5512 based on deterministic assessment or best estimate of that value.

In the January 30th letter report from Sandia they looked into the -- in detail into the distribution coefficient and they said they reviewed the data based on the best estimate of individual measurements of Kd values. They based it on best estimate and variability of small scale experimental measurements in the field and they provided those references and also based on 11,000 experiment-based on partition coefficient values.

We have here lots of data on partition coefficient and those were used by the Nuclear Energy Agency, NEA, for its sorption database, so we have a sorption database.

So the letter report adopted an approach to develop 1 PDF for each element based on all data available. Now the current approach and the methodology to develop the default value, you do not differentiate between soil type, you don't differentiate between one geochemical condition which was raised this morning about the pH value. The data was reported that is an extreme -- you have pH value of 2 or 1.0 that causes immediate leach of the material, so you consider all kind of conditions, all kind of data available in the literature and you try to establish the PDF for partition coefficient.

In the letter report the presentation was to propose radionuclide partition coefficient for Cesium-137, then they said look at the sample, the number of samples that we have, 564 samples, and to use the data source. I believe Sheppard and Thibault are the major contributors to the distribution coefficient literature values, and they looked at the sorption database and it was concluded out of this that the mean Kd value is 447 and the standard deviation, 10.2, and the variance is 10.5.

Also it was indicated that the best estimate for Cesium-137 value range, this is the range -- you have high values and low values -- from the repository performance assessment study and the reference to McKinkley in 1991 and he did lots of work on the

distribution coefficient and he proposed the value range, the high and low values to be between 100 and 10,000.

Remember, we have current in the D and D screen the value of 10, so we are more conservative by one order of magnitude considering the low value or the low range value.

Also in the letter report the report presented cumulative frequency of sample Kd values and this curve showed that the 95th percentile, which here the 50th percentile, this means to be highly conservative in this case here, so you convert it -- 100, take away 5 and then it will be at the 95th percentile. You have a value of about 9, so currently the value that we have in D and D code does not represent the 90th percentile value for the specific radionuclide. However it represents almost the 95th percentile value for the distribution.

The 10th percentile value, which is now converted -- it will become the 90th percentile value. It is 30 milliliter per gram so if we need to add up the concept of using the 90th percentile value, assuming you have one single radionuclide, not the rest of the radionuclides, the value to you is as a default it is highly conservative, the 90th percentile level is 30 milliliters per gram, it is not 10, so you have at least a factor of three.

Of course the least conservative one is 10,000. This is not to say something about RESRAD but it looks like RESRAD is using 1000 and now the question whether it is -- whether it is conservative or not -- you may argue that, so I believe maybe the values should be higher than what RESRAD -- sorry, lower than what RESRAD is here, whereas for D and D, because we are going to the extreme to make it highly conservative value.

Now we go back to the issue of what causes these differences in doses and what causes to have 1 picocurie per gram of Sr-90 in the soil, correspond to 60 millirem dose. Is this really realistic or is it because of the current methodology that we are using?

So based on the review of the Sr-90 default dose values, as you recall soil to plant transfer factor in D and D Version 1 it appears that the current concentration ratio, which gives you for every unit concentration will get about 60 millirem and this is much larger than the

upper 90th percentile, so we looked at the 90th percentile value for assuming you have single radionuclide, which is Sr-90 only, and assuming those, so even by doing that you will find that at the 90th percentile value you do not get a dose of 60 millirem, you get a dose of about 15 millirem, so there is a factor of four or there is somehow excessive or extra conservatism in the current model because of the process, the procedure for using those parameters by minimum for this case, a factor of four.

When we look at cesium we will find also the same thing that for Cesium-137 the dose conversion factor or the concentration ratio it is 28.3 millirem per unit concentration. However, this corresponds actually to the 95th percentile of the dose for when you have one single radionuclide, so the 90th percentile of the dose when you have single radionuclide it's actually 2.3, not 28.3. This means you are having extra conservatism because the process and the methodology by one order of magnitude.

Okay. What are the causes. The causes, yes, you have numbers that input parameters, that they were inputted to the code based on the current methodology that you select that they produce those which is very high and the current default dose, the anomalies, they are mainly due to the artifact in the current methodology used in D and D Version 1.0 to select a solution representing a single default parameter set for all radionuclides. That is the crux of the point.

We are assuming the source term not in the single radionuclide -- all radionuclide. We try to ensure that any radionuclide on the list that we have it is guaranteed that it should be above 90th percentile value so maybe you will find for a few radionuclides they are causing this dose to be exceeded, and you do not have it at your site, you are actually getting higher dose because of those radionuclides.

But understand the screening methodology and we are trying to see where is the extra conservatism in the screening methodology. As Walt indicated this morning, we hope that the Version 2 will resolve this problem.

These dose anomalies can be minimized, as I said, using Monte Carlo version code, Version 2.0, by having input of the specific radionuclide or mixture of radionuclides that you have at the site. Also it can be improved -- it could be an improvement in the current approach in the statistical measure used to select the solution vector. We try to rethink also how you select the solution vector in establishing the default parameters for D and D Version 2.

Also we hope that certain parameters also will be changed. As I said, mass loading factor for the plant hopefully will change those, also the resuspension factor if we come to some kind of solid ground that we say yes, we need to move forward to change the resuspension factor. Those will be changed hopefully in Version 2 plus other parameters that we find that we have some extra conservative numbers.

Conclusions and suggestions -- the anomalies in Sr-90 and Cesium-137 in D and D Version 1.0, they are primarily due to artifact, as I said, in the methodology, so please when you give those values do not be scared -- it's just a screening process. We are working on it. Most likely that the dose that you will get does not represent the real dose that you have at the site.

The major parameters influencing the dose for Sr-90 and Cesium-137 when you try to look into it, the specific parameters for the site, you need to pay attention to the soil to plant transfer factor and the distribution of partition coefficient factor that you may have for the site, and you could change the dose significantly.

Comparing Sr-90 and Cesium-137, dose values using D and D and RESRAD, they show that the D and D values, they are significantly higher than RESRAD currently as is in the version. However, comparable dose values were obtained when similar input values were used. Now we are talking only about these two radionuclide. When you vary the parameters, specifically distribution coefficient for cesium and the soil to plant transfer factor for Sr-90, you will have somehow comparable results. Although the models and the codes they are different for that specific case when you try to have a source term which is similar in the area and in the

thickness, you will have it somehow similar to this.

And this is not unusual. You expect it because both of them use somehow similar pathways, all pathways and also they use the same dose conversion factors, and the difference in the factors are not that great, so you actually expect to see those results, although you will find the dose sometimes earlier is coming in D and D whereas it is coming, picking up later in RESRAD because of the nature of the difference between the three box model versus what is called the pulse release model in RESRAD.

In reviewing input parameters distribution it is suggested to consider Sr-90 soil to plant transfer factor as a sensitive parameter. Currently I believe -- Walt, correct me if I am wrong -- the soil to plant transfer factor is not considered a sensitive parameter -- some consider it a sensitive parameter because it does impact the dose.

Finally, the floor is open for any questions.

MR. PETERSON: Harold Peterson, Department of Energy. I have a couple of questions.

One, your accumulation factor for fish, for cesium, from .002 I think and .0002 are lower than anything I have ever seen. The lowest cesium accumulation factor that I am familiar with is in the range of 10 to 30 for marine fish and up to 44,000 for fresh water fish. Reg Guide 1109 uses 2000, so I think you might want to check that value. It almost looks like the concentration per gram divided by the concentration per kilogram in water, to get that number. Do you see the bioaccumulation factor there?

As I said, that should be in probably for marine fish something in the neighborhood of 10 to 30 and for freshwater fish Reg Guide 1109 would use 2000 and it could be as high as four or five thousand.

MR. YIM: As a side comment, Mr. Yim from North Carolina State University.

MR. PETERSON: Excuse me?

MR. YIM: Both uses 2000.

MR. PETERSON: RESRAD, right -- RESRAD follows, uses the parameters from Reg Guide 1109.

MR. YIM: I apologize. This is a typo.

MR. PETERSON: Okay, well, I am just pointing that out and --

MR. EID: It should be two times to the power --

MR. PETERSON: The comparison too of the soil bioaccumulation factor, again RESRAD follows Reg Guide 1109 and uses the value for wet weight, where your numbers are the value for dry weight. That could easily be a factor of five.

MR. EID: I am talking here about dry weight, okay? This is -- I converted those based on the dry weight.

MR. PETERSON: Right. But RESRAD isn't. RESRAD follows Reg Guide 1109. The BIV in 1109 is per gram of wet weight.

MR. EID: Okay, so the -- you need to multiply by a factor of four in this case?

MR. PETERSON: Factor of four or factor of five.

MR. EID: That's correct.

MR. PETERSON: If you want to include tomatoes it is a factor of 20.

MR. EID: If you multiply .3, I believe for RESRAD it is .3 -- multiply by a factor of four is 1.2. This means you will find consistency between actually NUREG 5512 Volume 1 and all of the rest of the values, so thank you for this point.

MR. POTTER: My name is Tom Potter and I think the problems that you have explored today are symptomatic on a microcosmic scale of the kinds of problems we are going to be pursuing in the future when we have the opportunity to go through this exercise at the same time deconvolving the various probabilistic distributions that we have used for various parameters.

I raise this question, second question, in connection with the importance of Kd here. The Sandia letter report seemed to lump all the Kd literature together to form one

distribution and when I read that I sort of questioned it from two standpoints.

One is a lot of the literature data like the TBO and some of that literature that broke it out by soil type, identified distributions that clearly did not overlap, and if that is the case we ought to be maintaining that distinction.

I haven't done much in the way of Kd measurement but I do have some experience with radioactive material spills I am not particularly proud of, but my experience is that for a variety of nuclides they seem to run through sand a lot better than clay, and I wonder if we ought to be trying to maintain this knowledge.

The problem of lumping all the data together is you get very broad distributions and you are basically driven to a site-specific research project for Kd.

MR. EID: Well, the answer to this question, I tried to point out two issues. One issue that the current methodology that we have in D and D 1.0, that we do consider it is not just only what is the Kd value for that specific radionuclide. It is what kind of radionuclide we took into consideration when we established the PDF curve to select the 90th percentile.

I believe that is one issue and this needs to be corrected, so if you have only Sr-90 for your site you do not need to be penalized because the D and D considered other radionuclides. Although I acknowledge that, we say that it is a screening problem, okay, and that is the methodology currently.

But also we agree that it is an artifact of the methodology that the code will give you higher doses because of consideration of other potential of having other kinds of radionuclides in the source. That is one issue.

The next issue is you need to look at the uncertainty in the values of Kd and the Kd values is true for the site that for soil is different than clay is different than loamy soil so there are some significant differences. Kd values also depend on geochemical conditions. If you have a pH value of one, two or three it is different than when you have neutral conditions. It also is impacted by the oxidation reduction conditions at the site. It is also impacted by the

particulate size of the formation that you have, so there are so many factors that you have.

The question is when you try to determine Kd for the site if you have the soil which is different. You do not have one uniform soil if it is sandy soil for the site, so currently more or less the Kd value you assume that you have more or less sandy soil because we try to be conservative because of the screening process, so I agree with you that the Kd value should not be taken as a single value.

The first thing you need to take a look at -- what is the characteristics of the Kd value that compares to your site conditions, so that is a very, very significant and important parameter, and this is the example for Cesium-137, so either you try to say, okay I'll need to use surrogate data based on literature value that you have for example sandy clay soil and you know this is the majority of the site who have sandy clay soil.

You go back and say I have a nuclear condition at my site. You look at the literature value based on experimental result determination that the Kd value for that specific radionuclide is such and such value.

So NRC staff, when you provide this value and this rationale for selecting your value, although it did not determine that specifically for the site, so maybe it could be acceptable, because you demonstrate that your sole pipe is different than what was proposed.

So I agree with you, you need to take a look at your site-specific conditions and then try to analyze what is the best Kd value that's appropriate for your site.

The next question?

MR. LITTLEFIELD: Pete Littlefield, Duke Engineering.

There are some of us out there now decommissioning sites which started when we had nothing else available to us except NUREG-5512 and RESRAD, and I just ask that when we get to the point we're actually demonstrating that our sites meet the dose criteria that the NRC will remember the discussions that we're having today relative to the progress that we're making in trying to define these parameters, because it's very difficult for us to be deciding on a

day-to-day basis what we ought to be using for parameters for our determining dose guideline levels and our dose assessments.

MR. EID: Thank you for the comment. I think we should hear what you are saying.

MR. SEXTON: Dick Sexton. I guess a followup on that. I'm with Connecticut Yankee, and we're in the process of selecting a decommissioning operations contractor, better known as a DOC, and this uncertainty that is being discussed relative to the modeling is one that I'm really struggling with both from evaluating the likely performance of that DOC, and I wonder about what is the DOC using to evaluate the cost of cleaning up these sites. If they don't know what model they have to use or what will be acceptable, I really -- I'm wondering what are they -- how are they estimating their cost.

Again in the reactor decommissioning clearly the one fairly attractive model is for a fixed-bid contract, and without these models to go forward and allow the commercial decommissioning community to make some assessment on risk and how clean they have to clean the site up, I think this is something that needs to be addressed in a fairly succinct manner, and I guess I would ask if you have any words about what both a utility and a DOC today would do to make those assessments as to how clean the site has to be.

MR. EID: You are raising a very important question and issue about the cost of cleaning, because when you try to estimate the cost for cleaning your site, you want to think about what kind of DCGLs you need to meet. And the way to find the DCGLs is to use dose modeling. That's the facts of life. You have 25 millirem and you do not go and measure in the field 25 millirem to measure concentration. So I guess the crucial issue is how you try to determine your DCGL.

The way that we have it now and the reason for this presentation we say okay, we have a very conservative model that is available, a tool to use, which is DandD version 1.0. Now we said this is only for these purposes, for the next two years, already almost a year past,

and try to evaluate the model and the code. That's what the Commission told us to do, trying to evaluate the input parameters, to evaluate the conservatism in the code.

The reason for this presentation we are telling you yes, there are some conservatisms in the code, and I'm trying to point out those. So this means you do not try to run the code and say yes, I believe what is there and I need to take these values and establish my DCGLs and take it to the field and the contractor and ask your contractor based on the current version say clean up to that level and calculate the cost for me.

For example, if you say, you know, we take this example, cesium-137. You say from these values that you need to clean up to -- sorry, not cesium, take strontium, for example, to .5 picocuries roughly per gram of the soil.

Now what we are saying, there is an excessive conservatism already in the model that you have, and you try to do something else. Not try to go and estimate your cost based on that. This is only a screening tool. It is highly conservative, and even in the methodology we have artifact in the dose estimate. That's exactly what we are telling you here, and that's the purpose of the workshop.

So please don't do that and calculate your cost based on, you know, on the current values that you have -- to derive. Try to do more. Try to consult with the staff. Try to see what is the realistic value for releasing the site, to establish the DCGL value.

So my advice is don't take currently whatever is in the code. It is under test conditions now. And try to calculate your cost for remediation, take, you know, one step further. Try to assess more realistically the dose that you have at your site. In order to be more precise in estimating how much -- how clean is clean and how much it's going to cost you to clean the site.

Harold?

MR. PETERSON: Yes, Harold Peterson from DOE again.

I'd just like to reemphasize something Tom Potter brought up, and that's the

problem of the variabilities of environmental parameters. And one thing to be remembered is that they are not random variables. They depend on environmental conditions. And if you can get the conditions specific to a site, you can considerably reduce the uncertainty in the dose estimates derived from those parameters compared to a broad distribution.

And the other thing related to that is it's all too easy these days to throw three or four points into a computer and it will fit a distribution to those points, and in many cases we don't have enough data to characterize the statistical distribution of the parameters that we're using. So --

MR. EID: I agree with you.

MR. BEYELER: I'm Walt Beyeler from Sandia.

I just wanted to mention with respect to the Kd distribution, we did in fact look at the potential for defining separate distributions depending on soil classification from the Sheppard & Thibault data, and the statistical analysis suggested that as a group there was not a distinct set of distributions for clayey soil conditions versus sandy soil conditions. I understand this is counterintuitive. But that was the -- we did in fact look at that and made that conclusions.

That might be something worth revisiting, and it might be a question as to whether for particular elements the Kd is demonstrably sensitive to a soil classification, whereas for particular elements it isn't. But in any case I wanted to mention that that was something that we did look at. There may be other classifiers such as pH that would be useful in helping define a narrower distribution of Kd's for a particular range.

MR. EID: Thank you.

MR. POTTER: Tom Potter. If I could address that just quickly. The problem is that that work is not very visible to us, and if we do go to the literature of that values of distributions there clearly is not a lot of overlap in those distributions, which seems to be in conflict with the conclusions that you're reaching. So there must be a disconnect there somewhere.

MR. BEYELER: Yes, and I think it's something that we do need to look at, and again my intuition might be that there is a distinction for particular elements, but there may not be for all elements, and --

MR. EID: Thanks.

DR. YU: Charlie Yu, Argonne National Laboratory.

Boby, I have a question on the -- your peak dose for cesium-137, what is the peak dose time, what time did the peak dose occur?

Also, why the fish dose is a dominant pathway for cesium-137, how do you calculate the fish dose? If fish dose is the dominant pathway, why when you do the sensitivity analysis it doesn't show that? When you change the bioaccumulation by a factor of ten, why the dose is not changed by a factor of ten.

MR. EID: Okay. The answer to the first question I do not remember exactly the peak dose, but I believe --

DR. YU: Time zero.

MR. EID: About, you know, zero time, I believe. Yes, that's correct.

DR. YU: Okay. If it's time zero --

MR. EID: It's about time zero or the first year, I would say.

DR. YU: If it occurs at time zero, how do you calculate the fish concentration? Does the water reach the pond?

MR. EID: Because the model in DandD, it will have three box models, very simple model, will allow you directly to leach the material and to go to the aquifer and then to the pathway for, you know, the water pathway.

DR. YU: So it doesn't matter how thick your unsaturated zone is, ten meters, one meter, it doesn't matter?

MR. EID: I tried to change the thickness and did not change it in that specific case. Changed it slightly, but not that much.

DR. YU: Okay. Then the second question is why when you do the sensitivity analysis it doesn't show that. When you reduce the bioaccumulation factor by a factor of ten, the dose is not reduced by a factor of ten.

MR. EID: That's a good question I did not investigate. I will look into that.

DR. YU: Thank you.

MR. EID: Thank you.

MR. SEXTON: Dick Sexton, Connecticut Yankee. Just one other comment. When you're performing the sensitivity analysis and also making some assessments on the probability distribution, is there any consideration for the ALARA analysis and the resulting cleanup activities and the risk associated with that? How does that get factored into these decisions?

MR. EID: For this specific exercise I did not look into ALARA and how it's going to be tied up with the cleanup. Possibly you could talk tomorrow about it. So I cannot talk too much about ALARA at the present time. But I believe the two together they should be, I agree with you, when you try to decide what kind of cleanup levels that you have, you need to look into the ALARA at the same time.

Henry?

MR. MORTON: Yes. Henry Morton. Yes.

There are a couple or three things here. One, I think this groundwater pathway model illustrates the complexity of these things pretty well in that when we tend to think in terms of the conservatism being in one direction only, that is, as we vary the value of the parameter, for example, Kd in one direction, we assume -- we may tend to assume that you'll move toward the peak dose, but for example in terms of Kd, keep the Kd high enough, the cesium's going to set on top of the ground, in effect, and deliver the external dose. If it's low enough, then it'll tend to move to the groundwater.

But the magnification of the effect of Kd is, for example, when you go to thinking

about the differences between very sandy soil and the clay soil, is not dependent alone on the Kd, but since the sandy soil will tend to have a higher transmissivity than the clay, that movement of the water will tend to make that move faster as well. So it seems to me that it's not a simple separation of these variables to get a clear answer out of it in some of these cases.

MR. EID: That's correct, Henry, we are trying to simplify the analysis now by taking one single parameter at a time, but typically when you do the sensitivity analysis, you combine all parameters to see when they are combined together what kind of dose distribution that you get. So in this case it is simplified analysis just to identify the most sensitive parameters that could impact the dose.

Any more questions?

Okay. Thank you.

MR. ORLANDO: According to the schedule, we are supposed to have a break at 2:15, but I would like to keep going, because, clearly, you all are not falling asleep or anything, given the quality of some of those comments. So I would like for Dr. Yim to go ahead and do his presentation and then we will take our break at 2:30 or quarter to 3:00, whatever it is.

MR. EID: I would like to introduce to Dr. Man-Sung and Dr. Man-Sung is a professor, University of South Carolina, right?

MR. YIM: North Carolina State.

MR. EID: North Carolina, sorry. And -- south and north, they are the same for me. And Dr. Man-Sung, he works for EPRI and we tried to do piece of the work that he did, and he did also the other piece, tried to exchange information to see, based on this exercise, what kind of data we have, are they -- we are getting the same data or different results. If it is different, we -- you know, we like to investigate it. Still, I have not look at his data, by the way. So, Dr. Man-Sung will give his presentation almost on the same issue, but I believe, talking to him, he said he looked into iodine-129 in addition to these radionuclides. So we will hear from Dr. Man-Sung.

MR. YIM: By the way, Man-Sung is my first name. Y-i-m is my last name, Man-Sung is my first name. People always confuse.

I work for EPRI as a contractor, so the program says I am representing EPRI, but I don't think EPRI would be happy to hear that. I want to put a little disclaimer of this, because the result I put in in my presentation is really a last minute work. For example, the code I am running for probabilistic D and D code, I got it really very recently, almost last minute work again. We had to compile it and we had to figure out to put inputs and how to read outputs and so on.

So, and then my original goal was for the probabilistic RESRAD analysis, I was hoping to have enough information for inputs that is going into D and D and use the same information for RESRAD to try to make some comparisons, but I found that they were a lot more involved in that, so -- involved in that work, so what I am presenting is really tentative findings at this point.

I want to start out with this table. As we saw Bobby showed the difference between the difference between the two codes. If you use one picocurie per gram, you need concentration in soil and use D and D 1.0 and then RESRAD 5.82, and you get this much difference, more than a factor of 10 difference. And Bobby showed where are those differences here. Basically, you have a big dose coming from external gamma for cesium for RESRAD, but then for D and D, you have a big dose coming from aquatic food pathway, which is basically fish consumption. Yes?

MR. BHAT: I am fundamentally ignorant on it. Can you tell me where the difference would be?

MR. YIM: Could you use the microphone, please?

MR. BHAT: I am fundamentally ignorant of the basic difference and assumptions between D and D and RESRAD.

MR. YIM: That is what I was going to get into.

MR. BHAT: Oh, I see.

MR. YIM: So, if you wait just one second. Well, at least we know what D and D 1.1 -- 1.0 is, you know, what RESRAD 5.82 is. And Dr. Yu asked Bobby about why getting this high dose from D and D 1.0. And that, I think -- and I am going to actually explain those things here. I think it is the combinations of the parameters that you are using and the difference in the model, combinations of both. And then when I say difference in the model, what are the main differences between RESRAD and D and D? Actually, I was going to put some information on this, but maybe not now. Maybe in other workshops when I have more conclusive findings.

But the main difference at this point we can say is D and D, as we heard, use a very simple three box model and basically you get into the aquifer region, there is no dilution of plume. You have put in some concentration into the aquifer, and then you will get the same concentration of materials out from the well. And at the same time, any of the surface water attached to this aquifer will have exactly the same concentration of whatever contaminant in the aquifer.

So the fish, for living in the surface water, they are exposed to the same concentration the groundwater. So if you have enough bioaccumulation factor working for you, you will have, obviously, a lot of concentration in the fish, by the fish, and if you eat fish enough, then you will get high dose.

So it is a combination of those modeling issues and then the bioaccumulation factor. By the way, for cesium, both RESRAD and D and D uses 2000, so it the same bioaccumulation factor, but the number is high enough, compared to other nuclides, so it may have an impact. Obviously, it will have an impact.

So what I did here was take a different twist approach, what if you use exactly the same input of RESRAD 5.82 and one -- D and D 1.0? And D and D gives very close numbers to what RESRAD produces. So this means maybe it is really the parameters that controls this whole thing, right. Almost identical, 2.29 versus 2.63. But then here you still have

this contribution from aquatic pathway.

By the way, this is external gamma, this is inhalation dose, this is agricultural food dose, and this is water independent, okay. Soil ingestion dose. Up to this is all water independent dose, and the bottom three are water-dependent pathways mainly coming from groundwater transport.

We do the same thing. Now you use RESRAD and then you use D and D default parameters, okay. So if you have a problem with D and D default parameters, that should show up. And that is -- I'll give it to you, it is shown here. Not really, I am sorry. Well, you use D and D input parameters and then you use RESRAD. Basically, you have -- yeah, external gamma still dominating, and then a little bit more agricultural food, but you don't really have this problem. So this indicates maybe the difference between the model. It is not just the parameters, but maybe the difference in the model, as we just discussed. There is no dilution in the aquifer, and assumptions we are making, surface water and groundwater have identical concentration, et cetera.

Now, Bobby had said if you use probabilistic D and D code, you may get realistic results compared to D and D 1.01 -- I mean 1.0. So that is what I did. You run probabilistic D and D code. This is all default. I am not changing any parameters, I am just using what is provided, and then the dose predicted from the probabilistic D and D code is very close to what RESRAD produces, or what D and D produces with RESRAD input, just because we are using more realistic representation of parameters.

If I use RESRAD probabilistic or it what it called uncertainty add in, again, that number is very close to this. One caveat to this, RESRAD probabilistic analysis and D and D both, it is very tricky. At this point I think RESRAD probabilistic analysis code is hard to use in some sense. It has user friendly features, but unless we are given prior information, you don't know what to expect. There is certain limit that you can change, number of parameters, they can vary. There are limits on your sampling, number of realizations. And without having those

information with you up-front, you are just trying everything out of blue, trial and error. If you are lucky, you will make it. If you don't, you will not make it.

But then later I found that it is up to 17 parameters that you can change and up to a hundred realizations you can do in your sampling with RESRAD uncertainty add in. But then that is maybe not 100 percent true for some nuclides, at least that is my experience. And the code is very -- at this point, the interface of the code with the rest of the system in the computer is very unstable. Interface is probably just not good, so at some times the computer says, no, it just cannot do it.

But what I heard, actually, yesterday from Dr. Yu, Charlie Yu, is they are working on this, and they will produce a revised version of uncertainty analysis code for RESRAD.

One more thing, when we say probabilistic analysis, I am putting standard deviation information, and you may wonder if you have mean value of 2.6 and standard deviation of 6-point-something, you are getting negative values, and that is just an artifact. Basically, you are getting something like this, load normal distribution, and we should use geometric standard deviation. I am just simply using a standard deviation.

DR. YU: This is Charlie Yu. Can I ask you a quick question on previous slides?

MR. YIM: Yes. You mean the picture?

DR. YU: No, no. The --

MR. YIM: Cesium.

DR. YU: The table.

MR. YIM: Okay.

DR. YU: The fourth column D and D using RESRAD input parameters for fish dose, how did you guess so low dose, what have you done to run D and D?

MR. YIM: You mean why are we not getting any fish dose here?

DR. YU: The previous column, the left column.

MR. YIM: Right here?

DR. YU: Yeah.

MR. YIM: Why are we getting such a low dose?

DR. YU: 10 to the minus 6, right.

MR. YIM: Well, I guess that is -- you may have enough -- that's D and D, it is D and D. So you still get some contribution, right, you still get some contribution. For the aquatic pathway, D and D assumes there is no dilution.

There is difference between D and D and RESRAD for the Kd value, that may be the reason. D and D uses 10-point-something, right, and RESRAD uses 1,000, it is a much higher Kd value. So you probably have much already significantly diluted concentration in the aquifer, so maybe the fish is exposed to relatively low concentration.

DR. YU: So D and D code also use the Kd to control the radionuclide leach to groundwater?

MR. YIM: Yes. In the unsaturated zone and in the contaminated zone, they still use Kd. So you will get some dilution and, you know, retardation in the contamination zone and unsaturated zone even with D and D.

DR. YU: Okay.

MR. EID: If the factor of 100, I mean there is two other magnitude difference between the distribution coefficient of RESRAD and D and D, and RESRAD, if the default value is 1,000, so when he input 1,000, instead of 10 in the current code, and then you could see the difference such that you will have retardation, although you have thick zone, but, still, it will account for the retardation when you put this value.

DR. YU: I see.

MR. EID: It is becoming now more significant.

DR. YU: Thank you for explanation, but I want to make one comment. Maybe this is appropriate time to make the comment.

Suppose the site measured the Kd. The Kd is 1,000. But DandD, you still insist to use 10. This

will generate results showing the dominant pathways, fish pathway, which is incorrect.

MR. EID: Well, I think we are talking about now site-specific analysis. There are screening analysis and site-specific analysis. When you say that you measure it for the site and you find it 1,000, and you are having 10, this means you have justification for changing that parameter. The question is how you change it currently in version 1. That's the issue. But you do need to change it, of course, because you have site-specific parameters that are different from the default value.

DR. YU: Okay. Now does the licensee have the option to turn off the pathway if he can show that we don't have a pond on site? Can they turn off the fish pathway? In other words, they just consider all other pathways and based on your parameters if they turn off the fish pathway, they can get a very low dose and very high cleanup soil, cleanup --

MR. EID: I'm not sure if it is possible currently.

Walt, can you elaborate that? That if you can eliminate certain pathways currently in DandD codes?

MR. BEYELER: Walt Beyeler from Sandia.

There isn't a switch that will turn the pathways off, although there are certain combinations of parameters that have that effect.

MR. EID: So in other words you could put very low values or zero values for those parameters impacting the pathway.

DR. YU: I think my point is that if they can do that, they can turn off the fish pathway, but your code will show that the external dose is not a dominant pathway, you will get a very low dose.

Do you know what I'm trying to get?

MR. EID: Yes.

DR. YU: So essentially the licensee can show that we don't have a pond, we can get much higher soil cleanup criteria, which may not be true, which may not be right.

MR. EID: Current dose becomes important with even DandD if you use RESRAD, that's 70 percent.

DR. YU: Anyway, let me go through the rest of the stuff.

For the cesium, Boby already mentioned that soil-to-plant transfer is the important parameter here, and as we can see, it's agricultural food, both -- even though the magnitudes are different, it's the same pathway, okay? If I use DandD and then use RESRAD input values, the same pattern. You have this agricultural food pathway dominating, but then the numbers become very small.

And now if I use RESRAD with DandD 1.0 input values, same pattern, agricultural food dominates, but higher dose. So it's probably more of a parameter issue than a model issue, especially with strontium I guess basically with small contributions from a waterborne pathway, the difference in the model is not evident here in the results. Which means if you have better control on the parameters, cesium probably will be taken care of.

If you do the same kind of probabilistic analysis, the results from DandD is very close to what you get from RESRAD with their defaults or the default probabilistic analysis.

These two nuclides represent one of the more interesting ones according to what we saw in our last workshop. Then actually we wanted to do a little bit more study on this. I mean, is it just a cesium and strontium issue, or are there some other nuclides that may be a potential issue here?

So actually what we did was we actually ran all the nuclides, sort of a base case simple analysis, and then basically try to identify what nuclides may potentially produce these kind of differences. And we found that there are a couple more nuclides, but actually for this we pick the most important four nuclides, and that includes iodine-129 and plutonium-239. Okay. So you will see big differences if you just use default approaches between DandD 1.0 and RESRAD 5.82.

The numbers are relatively high, both of them, but then differences too -- higher.

And there is a big difference in terms of its pathways, controlling pathways. For RESRAD it is drinking water, and in DandD it is irrigated food or aquatic food. Again, the fish pathway, and then some irrigated food from groundwater dominates. Because both RESRAD and DandD use these very small Kd values. DandD uses .1. RESRAD used .283. So both of them relatively very small. And so the small Kd values again, the difference in the model is not amplified and groundwater pathway becomes important in both cases.

If I run the same case, DandD with RESRAD default, we produce much smaller values, and you see changes in the importance and you see more importance in drinking water. So it's sort of coming closer to what you see from this.

If we do RESRAD 5.82 with DandD, we again see the same pattern, increase the dose significantly and it becomes almost identical between RESRAD and DandD if you use default parameters.

And in this case same aquatic pathway becomes important even with RESRAD. So it's again a more parameter controlled problem. So with the bioaccumulation factor and the small Kd values, you have plenty of contamination in the groundwater and enough for bioaccumulation. You will have higher concentration in fish, and that produces dominant pathway in these groups.

And the same thing with probabilistic DandD and RESRAD. But then interestingly RESRAD produces much higher dose with probabilistic analysis, and that's sort of an anomaly. The way RESRAD probabilistic analysis is done here, as I said, is default. The true comparison should be based on equal distribution functions you put in as input, and I found actually it was difficult for this, even the limitations in time, et cetera.

So I'm using whatever the code is giving as a distribution. And in the case of RESRAD uncertainty analysis, the Kd value the program gives you is very narrowly defined. Default Kd value is .1, and then distribution of Kd is even -- the maximum pair of the Kd value is even smaller than what's in the DandD. So we've got very small Kd value obviously RESRAD

will produce higher dose. That's basically I think it. But that doesn't mean that that will happen if we had realistic distributions.

Yes.

MS. HORNIBROOK: Carol Hornibrook, EPRI.

May I say I'm getting a little bit lost. Are you trying to say that depending on the nuclide depends on whether it's a parameter or a model?

DR. YU: Um-hum.

MS. HORNIBROOK: Aspect.

DR. YU: Um-hum.

MS. HORNIBROOK: Okay. And then beyond that, by knowing that, how does that help you or where does that take you when you look at this?

DR. YU: I mean, obviously even though it is changing, you know if you're talking about iodine-129 you always know that it is water pathway. If you're talking about cesium, it is obviously more, you know, external gamma dependent. If you are talking about plutonium, it's maybe waterborne pathway controlling. So even though it's changing, we already know what pathway is important, et cetera.

So if you have a nuclide which has potential for groundwater contamination, and then subsequent high dose from that pathway, then you need to look at both the model differences and parameter differences. If you have a nuclide that doesn't really have much waterborne pathway involved, then it's probably easier. It's just maybe the parameter issue.

MS. HORNIBROOK: Carol Hornibrook again, EPRI.

So that helps you determine the pros and cons of the two models.

DR. YU: Um-hum.

MS. HORNIBROOK: Which would then maybe be better for the nuclear power industry or more representative of the nuclear power industry?

DR. YU: Um-hum. Actually I think before I did this analysis people said well, it's

really the parameters that control the differences. So if we use identical parameters, they should probably produce very similar results. And that is true, partially true. But then for some nuclides where again water-dependent pathway is important, the model differences pops out, and then even if you use all the same values, you will still see a significant difference between the two.

This is plutonium. Big difference. High dose with DandD and small dose with RESRAD.

Obviously, with plutonium, you have relatively large Kd values. By the way, the Kd value used by RESRAD is 2000. For D and D, it is 13.6. There is a big difference in the Kd value. And if you know that plutonium will be controlled by waterborne pathway, the result shows that there will be a big difference. I mean there will be a big difference, and results, basically, supports that.

In the case of RESRAD, again, you will have high concentration in the groundwater because you have a relatively small Kd value and then no dilution in the aquifer and no dilution in the surface water, so you have fish or all the food, or drinking water, contaminated with plutonium. That is really driving the whole thing. Also, RESRAD, Kd 2000 big enough, no contamination of aquifer. So everything stays up there and then, basically, you get very small dose.

If you use RESRAD input values, high Kd values, basically, it is the same thing. Even though you still get some contribution from waterborne pathway, just never --over contribution, so it is almost, actually, showing the same thing. But then if you use D and D input for RESRAD, you would start seeing this but not as magnified as this, because you have better control in the -- describing the dilution of your plume in the aquifer, that makes difference. So, again, this shows the difference in the model contributes to difference in the results. Yes.

MR. GENOA: Paul Genoa, NEI. If I could try to summarize my understanding of the parameters. If I understand it right, the models have two issues. The two different dose codes have two issues to deal with. One is the structure of the model itself. The second has to

do with the selection of the parameters. Both are important in different situations.

And EPRI has been asked to provide some comparison data, and if I understand it right, you are handicapped by not being able to select a generic list of input parameters for distribution input parameters to run into both codes so that you can compare the results. Once you are able to do that, you then can start to refine which are the important -- or which two codes adequately model the results for the aquatic pathway. In other words, you have to be able to put in the same parameters first to do a comparison.

MR. YIM: If you are doing -- you are talking about probabilistic analysis?

MR. GENOA: Yes.

MR. YIM: That is true, yeah.

MR. GENOA: Okay. What is holding him back from --

MR. YIM: Actually, the problem is it is not him holding me back, or anything, but the timing of all this. The information I got from Walt, for example, required processing of data using Fortran programs, reading all the data, sorting it out and then try to actually read the data as it is and then eventually you have to fit those data to figure out what kind of distribution they represent.

Actually, the way the program is set up right now -- I am sure they are in the process of changing it, as they described this morning, I will change this, but the way I have -- the program is set up, or the way I am running it now is -- it is two tiered approach, I think. First, you have LHS program, a sampling routine and then you put in distribution functions for different parameters that you are changing. And then you basically select 580 points from your sampling for all the nuclides -- for all the parameters. There are some exceptions, but for those parameters, they are selected from the LHS program.

And then you use that 580 data points for all the parameters that you are changing as database, it becomes big data table. And then that becomes an input to the second phase, that data becomes an input to your simulation, repetitive simulation, using each set of

parameters, you run simulations 580 times, so you get dose representation for each realization and then 580 data points of the dose predicted will give you that distribution.

And since the distribution is already dictated by the first phase, you are using just the data, you cannot change your density functions at this point, unless you go back to the first phase and change your PDFs, run LHS again, produce new data table and then read it again.

At this point, since it is already -- I mean put into this data table and some -- for some parameters, it is difficult to know, it is not simple, as simple as this is normal distribution with just standard deviation. This is normal distribution with this much -- or minimum, maximum or geometric standard deviation. There is no such thing available for some nuclides, some parameters. So the only way you can really characterize the solution is now plot all the distributions, fit the data, and then get distribution functions, and that is just time-consuming process. So that is why I couldn't really compare the two codes, probabilistic codes, in true sense, using --

MR. GENOA: But looking forward, are there limitations, other than your work and time, to being able to do that for the D and D code on the one hand and the RESRAD code on the other.

MR. YIM: I think if we work closer with Sandia, that could be maybe relatively easily done. If we have a better communication or working system.

MS. HORNIBROOK: Carol Hornibrook, EPRI. What do you mean could be easily done, overcoming this particular problem?

MR. YIM: In other words, for some of the things, they have already done, I think, information is already there maybe, instead of we trying to do the same thing, we just get those data and we just focus on running the codes.

MR. EID: I think -- this is Bobby Eid. One area also to look into when you try to establish your PDF and you go to site-specific analysis, these distributions won't be that wide.

Here you are using practically every piece of information in your distribution, whereas, in fact, when you look at site-specific conditions, you need to reduce those distributions so you will not get such kind of dose values, because you will have information about the sites. An example is given about the Kd, you may have uncertainty in the Kd value because your conditions at the site, they are -- could be clay conditions, or sandy clay conditions. This means you not need to use this kind of wide distribution that you are having here.

MR. GENOA: Paul Genoa again. If I understand this right, what we are hoping to do is characterize perhaps two three generic cases, come up with what we think would be a generic case and then run it against two codes, get the answers and compare it.

MR. YIM: Actually, that is exactly what I am doing at this point, cold data.

MR. GENOA: Okay.

MR. EID: That is what we call --

MR. GENOA: And I guess I am asking, I thought I heard you say that there were some limitations. I am asking you if you will be able to accomplish that. Are there limitations in your access to the code, or your ability to mess with the code to stop you from actually doing that?

MR. EID: That is what we call the refined screening approach. In the refined screening approach, you try to group these kind of conditions and then to narrow the distribution.

MR. GENOA: So what's the answer, yes or no? Do you think you can do this?

MS. HORNIBROOK: Carol Hornibrook. The question, Man-Sung, is can you do it by September, that is the question.

MR. YIM: Yes, I think we can do it.

Mr. BEYELER: This is Walt Beyeler from Sandia. I think the obstacle is really that Monte Carlo D and D right now is not user-friendly. I think you would agree. And so it is a matter of us communicating to overcome some of the non-user-friendly features of the code, and I think we have been fairly good in being able to do that. It is just that it takes a bit of time to

explain some of the peculiarities of the file formats and work through some of those issues.

MR. YIM: Basically, if you want -- if anybody wants to use it, it is basically, you have to read the source code. You have to understand how inputs are going, how to read it out.

MR. GENOA: No, I understand this current limitation, and I just want to make sure.

MR. YIM: One additional problem is with RESRAD, I understand that RESRAD is in the process of revising their uncertainty analysis version, and with the current version, I think -- I would say it is very difficult to do meaningful comparison between RESRAD and D and D.

MS. HORNIBROOK: Because of the code -- Carol Hornibrook, EPRI. Because of the code, Man-Sung, or because the difficulty in your running it?

MR. YIM: Because of the code. Because right now RESRAD can only specify five different PDFs, only five different PDFs, and up to 17 parameters for some nuclides, and only up to 100 sampling realizations versus D and D, you have 580 sampling, you are changing maybe more than 100 parameters at the same time, and you have 20-some different PDF functions built in.

MS. HORNIBROOK: So you are -- Carol Hornibrook, EPRI. So your comparison would be limited to what RESRAD can do.

MR. YIM: Yes.

MS. HORNIBROOK: And as big as what the D and D code can do.

MR. YIM: Yes.

MS. HORNIBROOK: Carol Hornibrook with a question. If the D and D code won't be ready for easy use and release until the year 2000, the question in my mind is how useful is it for us to use that code when it is not in a stable -- I mean I know codes are rarely in a stable point, but is the -- I mean my question is, do you think -- well, don't worry, Carol, we are going to get a certain amount of information out of this analysis that will then identify some

changes that could be made, or, you know, or you are right, which is where I thinking I am heading. I am concerned I am working with a code that is not pinpointed somewhere in time, you know, that you can say this is what is going to represent the final code.

MR. GENOA: Paul Genoa, NEI. You are working on two -- you are working -- we are trying to compare two unfinished codes that are not out for use yet, is that correct?

MR. YIM: Yeah. That is exactly right.

MR. GENOA: And one of them is beta and one of them isn't?

MR. YIM: Actually, that is what I am trying to say here, my observations. Just let me first finish this part. With the user of same input values, the difference between the two codes are much smaller, okay, as we already saw, indicating that differences in the full input parameters dominates the difference at this point. At the same time, as we can in the cases of plutonium-239 and cesium-137, the difference also comes from the model. So model difference contributes difference even with the same input parameters and if you use probabilistic D and D code, obviously, you had much smaller dose and maybe much more realistic.

And probabilistic D and D codes results also is very close to what is predicted from RESRAD in D and D 1.0.

Neither of these two codes exist in usable form. The probabilistic D and D code is still under development, not user friendly. Maybe it is an alpha version -- beta final release it's not known -- today. With the conversion the user cannot change the PDF and et cetera -- you just cannot just play with the code other than what's given.

The probabilistic RESRAD code is a beta release version. With the current version it is difficult to perform defensible probabilistic analysis because the code is very difficult to use, sometimes it just stops. You don't know what you can change, what you cannot change. There is no manual available. Maybe it is available -- that information may be available at Argonne but not to any member of the users. As I said, the number of premises that you can vary and the interface problem all very basically makes it difficult.

But then Dr. Yu said they will be releasing a new version. Maybe it is very soon, which is supposed to be a much improved version. Maybe we may hear from Dr. Yu at this point.

[Laughter.]

DR. YU: This is Charlie Yu. It is true that we are going to have a new probabilistic RESRAD code release soon and this new version dose limitation measured by Dr. Yim will be removed and we are going to have 20 parameter distributions in the code and we'll have more user friendly interface and so on.

We will put this information -- when the code is available we will put it on the RESRAD website and you can send an e-mail to us and then we can send it to you.

MR. EID: I would like to add also that NRC's thinking, it is not final yet, of funding a project to Argonne, Charlie Yu and his group, Dr. S.Y. Chen, for probabilistic RESRAD based on the same methodology that Sandia is using, so we will have the comparison that will be more precise than currently that we are having.

Now the question is when this is to be completed. I would say it will take some time so I agree with you now. The question is what to do in the interim period. I mean this is an issue that we are discussing so far and then we could discuss it further and we would like to have suggestions from you regarding this question that was mentioned.

MR. GENOA: I guess -- Paul Genoa with NEI -- we have two questions but clearly we have a question about what to do today and we fully want to support development of codes that we can use in the future to construct and I see two agencies, two efforts that aren't well coordinated to move forward and we are trying to help validate the success of either one and I guess it sounds like we have an opportunity here where both codes are in development, but perhaps there is an opportunity to identify what are those things you would like to see developed or input into those codes so that they would be more user friendly in the end.

I guess we are giving you input. Maybe we are giving you input. I am not sure

but I believe that we should -- there is an opportunity to sit down together and to see where we need to end up. If we begin with the end in mind maybe we will get there. Right now I don't see us doing that.

MR. SEXTON: Dick Sexton, Connecticut Yankee. I guess I'd take that a step further from a licensee's perspective. Why are we trying to compare two codes? Why do we not just select one code and continue with it? If we are going to continue on with this comparison of RESRAD and D and D and it looks like ultimately we are going to have two codes that are the same. I think that is just going to extend the time at which the licensees don't have a code to go to to make some decisions.

MR. YIM: I think there is some comment.

MS. TROTTIER: Cheryl Trottier, NRC. I want to clarify where we are on these codes.

First of all, the Office of Research is overseeing the development of both, so it is not two agencies working and no communication. Argonne is producing a version of RESRAD for NRC use and regarding what code we are going to end up with, this is a testing period.

We have D and D on the street. It seems foolish to just put it in the trashcan. It seemed like during this testing period it would be appropriate to give people an opportunity to use the codes that are out there.

If the decision is made at the end of the two year period once we have a RESRAD version that is probabilistic, similar to what D and D is, if we decide that it is meaningless or counter-productive to have two available codes we will make a decision at that point but the reason there are two out there now is that they already exist.

It just seemed worthwhile to give people an opportunity to test the codes that are out there. I realize that the Licensing Office has to make a decision on cases that come up between now and the end of the two year period. We in Research can't make that decision. All we can do is provide the tools.

If it turns out that doing it this way is not useful, then we will rethink that but that was our thinking at this point, that it was better to continue with what we had during the testing period and then make a decision at the end of the two year period, which is now at the end of almost a year period but we are getting close to the two years.

MR. EID: Thanks, Cheryl. I guess that is the right comment from NRC point of view.

MR. PETERSON: Harold Peterson, DOE. Just a point that Ms. Trottier said about the two codes. DOE of course has one code and will stay with one code, RESRAD.

MR. EID: I would like to say that, to add to this, that the two codes, D and D currently is based on probabilistic input parameters. We understand it is conservative for screening and we understand that these parameters need to be changed and we are working on it.

Also, we will have the probabilistic version of the code.

RESRAD also is a good code, as was demonstrated here, so when you adjust the parameters to make them somehow identical, you are getting more or less closer results, so I guess maybe they will be meeting at one point so at the end, you know, if the industry, if they are somehow similar and there is sufficient justification for selecting the input parameters, I don't see any reasons that if you use D and D or if you use RESRAD at the end you are getting some comparable results, and with sufficient justification and sufficient conservatism to ensure the public health and safety on this.

MS. HORNIBROOK: Carol Hornibrook, EPRI. I am not sure who I direct this question to. Now I am feeling almost more uncomfortable because I feel like after we fund this comparison of RESRAD probabilistic and D and D probabilistic that yet another code, NRC's RESRAD probabilistic, will then be ready and I am having a hard enough time with just those two codes and where they are now, so I guess I am looking for some input here.

MR. EID: What we are saying that, and as Cheryl said, that we are thinking

about funding a research project for Argonne to develop RESRAD and to look at the methodology that we are using in D and D so we can compare and then to look at the conservatism in the two models and codes and look at the parameters, and at the end either we say one code is more suitable, more appropriate, the source term is better than the other one, and then we say okay, D and D, it is, you know, is history -- which I don't predict that -- I predict that both of them could be useful -- or we can say that at the end that we have comparable codes. If the licensee would like to use the RESRAD and they provide sufficient justification we will go ahead with that.

Now currently because RESRAD currently, as we heard, and when you talk about the probabilistic situation, does not have the appropriate probabilistic way of analysis, as Dr. Yim indicated, so they are working on it. We do not know when it is going to be ready. Also, it needs to have some testing period.

At the same time we are working on the proper way of probabilistic version of RESRAD similar to the current D and D version which we hope that it will be somehow producing comparable results. If they are not and there is reason for believing one code more than the other, then we will say that code is to be used rather than the other code.

MR. ORLANDO: Carol, do you mind if -- we can take this up at the end of the day. We are running somewhat behind and we just had a couple power surges and have lost the regions and I think we might have lost the PowerPoint, so if everybody wants to take a real quick break I can get the telefolks down here and we can pick this up a little bit later on.

[Recess.]

MR. ORLANDO: We are going to try and reconvene.

MR. HARRIS: Are you ready to go, Nick?

MR. ORLANDO: Yes.

MR. HARRIS: My name is Tim Harris, and I'm going to talk more about the same. I would really like to thank Boby and Dr. Yim for getting everybody invigorated, excited

about discussion of the two codes.

An overview of my talk, I want to talk about -- it's a former burial in Alabama. I'll give you some background on the site, site features, dose modeling that was done by the licensee. We also subcontracted the evaluation of that dose model to Oak Ridge and National Laboratories, so they did some dose modeling.

Since that happened, staff produced some interim guidance, which was back on the back table, which was principally developed by Mark Thaggard, and we did some additional dose modeling in-house. So we've modeled this site a lot.

I'll talk about the results of D&D, RESRAD and then hopefully try to draw some conclusions which aren't controversial.

This site was operated by the Tennessee Valley Authority. It's a former burial authorized under 20.304. The site location is Muscle Shoals, Alabama, which is in northwest Alabama. It was predominantly fertilizer research, but they did do some munitions research back in World War II. Maybe not if '66.

Basically, it's 40 individual burials at this site, which is a relatively small area. It's probably the size enclosed by the tables. The burials are two feet in diameter. They're basically caisson type geometry ten feet deep with a four-foot soil cover, and the average space in between the holes is about twelve feet.

The site features, being in northern Alabama, it's underlain in a relatively epikarst topography with limestone and chert. Relatively humid site: annual precipitation, 52 inches. The aquifer is, as I said, is an epikarst topography. The unsaturated zone thickness is relatively thick; it's about 18 meters. And with epikarst topography -- some of you may not be into geology -- limestone essentially dissolves with water and you end up with these big underground rivers or conduits.

The groundwater flows towards the Tennessee River and also towards Tuscumbia Spring -- very high transmissivity rate.

Here is a picture of the site. The burial is down kind of in the lower middle area. You've got this in your handout. The Tennessee River I guess would be towards the top of the screen and Tuscumbia Spring would be over in this direction.

The source term is relatively large number of radionuclides, 54 radionuclides. These are the predominant ones, and of these, carbon 14 is the main contributor to dose. Here's a list of some others. Relatively small amounts of uranium and thorium, pretty low concentrations of these other isotopes.

Dose modeling, as I said, TVA modeled the burial site using RESRAD and came up with a dose of .6 millrem per year. When they did their model, the hydrologic parameters that they used assumed that the water released from the site got into one of these large underground conduits with high transmissivity, and basically you got a lot of dilution at that point, thereby getting low doses.

Oak Ridge, when they modeled the site, they said, well, you may have a chance where your groundwater doesn't get into one of these conduits, and what would happen if a well was sunk in the saturated zone that was basically similar to a normal aquifer. So they used different groundwater parameter.

The doses that they -- that Oak Ridge produced using RESRAD with most of the RESRAD defaults, with the exception of obvious things like the burial geometry and, like I said, the revised groundwater pathway, resident farmer was 15.1 millirem.

They also looked at changing the parameters as suggested in PG 8-08, and if you do that, you get slightly higher doses. They also looked at a worker scenario, and obviously there, you're going to get lower doses with the cover.

They also looked at an exhumation scenario, which they said what happens if all the waste is dug up, and when they modeled their source term, they assumed that the waste was mixed with the cover material.

For all these dose models, they assumed 1981 was the starting point, and with

RESRAD, the peak dose occurs relatively quickly.

As I mentioned, staff just recently released, actually this week, interim guidance on evaluating dose assessments, and again, if you need a copy, you can contact me. If you have questions, please contact Mark Thaggard.

When we looked at their additional dose assessments that we did internally, we used D&D for screening with all the defaults, which was a mass balance approach. We did a number of things -- the mass balance approach, which I'll describe in a minute, also a single simulation approach.

We also did a pseudo site-specific analysis with D&D where we changed the unsaturated zone thickness and varied the number of unsaturated zone layers. Essentially, D&D has a box model and the number of unsaturated zone layers are divisions within that box.

We also looked at site-specific analysis using RESRAD. The interim guidance suggests that there is a laundry list of parameters which they suggest that you input into RESRAD, which are basically the D&D defaults, and we'll discuss here in a minute what the impact of that is.

As RESRAD, we modeled the resident farmer scenario, and also an exhumation scenario. When we looked at this, we didn't mix the radionuclides with the cover soil, so the concentration for the resident farmer scenario -- that is, the material in the ground and the exhumation where it was brought up was the same. We didn't mix it with the cover soils. And this is pretty close. The exhumation scenario is actually very similar to the single simulation scenario with RESRAD -- I mean, with D&D, excuse me. We also looked at a parameter sensitivity to see which parameters were sensitive.

The mass balance approach using D&D essentially says you take the activity from the site and mix it into 360 cubic meters, which is the D&D default; that is 15 centimeters thickness over a 2,400 square meter area.

The single simulation approach is essentially the same thing, where you're

modeling the waste on the surface, but here, you're conserving the volume where you're keeping the thickness of the layer 15 centimeters, but allowing the area to be larger depending on the size of the contaminated area.

Here's some results with D&D. The mass balance approach, you get 190 millirem, and again, that's mixing the entire source term with a relatively small volume of material, 360 cubic meters. If you look at the single simulation approach, you get 152 millirem.

Then we looked at changing one single parameter, which was the unsaturated zone thickness. We used 18 meter, which is the site condition, and also ten meters, to see what the difference was, and we varied the number of unsaturated zone layers. As you can see, when you change the unsaturated zone thickness, it dramatically changes the dose.

One of the surprises was when we changed the number of saturated zone layers what the curves looked like. The upper two curves, the upper curve is for the unsaturated zone thickness of ten meters. The lower curve was for 18.2 meters. And we also looked at changing the number of unsaturated zone layers for the D&D default, which is 1.22 meters. You can see you kind of get a little checkmark there, and the doses are sensitive to changing the number of these unsaturated zone layers, and essentially it has to do with the amount of mixing and dilution that you get. Is that right, Mark?

MR. THAGGARD: That's correct.

MR. HARRIS: The RESRAD results for the resident farmer -- again, this is with the changed default parameters. If you recall from the previous slide, you're getting doses in the range of 15 to 19 millirem with the RESRAD default parameters. When you look at -- I'm sorry. Yes, with the RESRAD default parameters. When you look at using RESRAD with the D&D default parameters, those doses go up from 68 to 157 depending on the groundwater model. D&D allows you to choose two groundwater models. One is a mass balance approach, which shouldn't be confused with the mass balance scenario that we're proposing for D&D, and a non-dispersion case.

We also looked at the exhumation scenario, as I said. Here, the doses between the two difference groundwater models were relatively significant, almost an order of magnitude different. In all cases, the key radionuclide was carbon 14 with the drinking water pathway. The key radionuclide with the D&D results was also carbon 14, but the predominant pathway was the aquatic pathway.

We also did a sensitivity analysis with RESRAD and found, as you probably would expect, that the contaminated zone density was sensitive, the saturated zone affected porosity and hydraulic conductivity were sensitive parameters, that the hydraulic gradient was sensitive, that the length parallel to aquifer was slightly sensitive, and watershed area was sensitive. Also well depth and KD.

One of the big differences between RESRAD and D&D, RESRAD assumes a KD of zero -- basically you have no retardation -- and D&D assumes a KD of 4.3.

Some conclusions. Carbon 14 is treated differently from a model standpoint between the two codes, as I'm sure everybody is happy to hear. This is demonstrated by different principal pathways and the different time to peak dose, which is largely a result of the different KD values.

D&D doesn't always produce the highest doses. I think what we saw when you changed the unsaturated zone thickness, you were getting doses -- I hate to say this, and Mark will probably hit me later -- you were getting doses similar to using RESRAD with the RESRAD default.

D&D is sensitive to variation in the number of unsaturated zone layers. I guess Boby saw that a little bit in strontium. I think it was probably more pronounced here with Carbon 14. And again, that's probably going to depend on your radionuclide mix.

Using D&D default parameters in RESRAD as suggested by the interim guidance does produce higher doses than using RESRAD with the RESRAD default parameters. The RESRAD doses were sensitive for Carbon 14 to the groundwater model you

pick and also to variations in KD.

The big question is what do we learn from all this, running different models, different simulations, different parameters. I think what you come away with is that varying the parameters and models can lead to a wide range of doses that you predict, and that when you're selecting your model and your parameters, you need to think about properly justifying it. I don't think that using different models -- I think it's good to use different models. I think you can expect to get different answers. But as you go through and determine which model is appropriate for your site and you look at the default parameters and changes that you make to those parameters, your going to need to adequately justify those.

That's basically it. I hope everybody was questioned out before, but I'll be happy to answer any questions.

MR. SAITO: Earl Saito, Combustion Engineering.

I had a question here. Could you help me understand your results a little better? The site closed in '81.

MR. HARRIS: Correct.

MR. SAITO: And you have the maximal dose occurring within five years. So has the maximum dose already occurred at this site? I mean, what's the --

MR. HARRIS: Hold on, Mark. That's true for RESRAD. For D&D, where your KD is a little bit higher, the maximum dose occurs in the -- I think it's 75-year range. So while that's true for RESRAD and one could make that argument, which I don't think Mark will buy, but if you use D&D as your code of choice to release the site, the doses do not occur until later on in the future. But I'll let Mark address the argument that doses have already occurred.

MR. THAGGARD: Well, I think the problem with the argument is that we're not really modelling reality here. I mean, we're trying to bound the dose estimate. So we are not properly modeling the groundwater system out there. We don't know what the KD is. So we can't say that we are anywhere close to modeling reality here. All we're hoping to do is to bound

the estimate so that we're making sure that we're not under-estimating the dose. That's really all we can say about these dose calculations.

MR. SAITO: So as a licensee, if I took this data and then I went out and I put a well in right near the burial and could show that the doses -- that the concentrations in the water aren't even that high yet, we can say that the model is very conservative, and therefore these dose estimates are good and we should be able to move forward from that point? Would that be a reasonable path to take?

MR. HARRIS: Well, I think if you had a well and you modeled doses during operation and you saw that the stuff was leaking out, that you could argue that it had already -- that the contamination had already been flushed through. But if you put in a well at the end of operation, I don't know that that tells you much.

MR. SAITO: Well, it's going to tell you what -- if you're saying it's already flushed through, then you're dose has already passed. If you're saying that it could --that it's flushing through at this point -- I mean, at some point, it's nice to be able to ground it into something that you can go out and measure that's measurable.

MR. THAGGARD: Well, yeah, if you could validate these numbers, I mean, if you could somehow validate your model with real slight data, then I think you can make that kind of argument. I think what we're saying is we're a long way from validating any of these numbers here, so all we're hoping to do is to bound the estimate.

But yes, if you've got real data and you can somehow use that real data to make an argument that you're modeling the system correctly or appropriately, then I think that would be the kind of argument that you would want to make.

MR. SAITO: Okay. Thank you.

MR. EID: This is Boby. I believe this question is quite important, and one of the models -- I'm not sure which one -- does allow you to place the source at the earlier date and then to recalculate the transport. So I believe your question is quite valid that all of the source

was placed some time ago, and the question is what kind of dose that you are getting from the period that you are having now later on. But if you have any information that there is already contamination in the aquifer, for example, because of the placement of the source, you need to take this into account.

MR. HARRIS: Any other questions? I saw Henry get up.

If not, thank you.

MR. ORLANDO: Thanks, Tim.

In case anybody is interested in what's going on, there's apparently some pretty severe wind outside, so we are experiencing some power surges here. Apparently we had some problems with some of the elevators, too, so use the stairs.

MR. HARRIS: I went through that pretty fast, I know that, in the essence of time. If people look at the slides later and have questions, please feel free to call me.

Thank you.

MR. ORLANDO: Next we're going to hear from Dick Sexton and Henry Morton talking about problems with measuring alpha contamination.

MR. SEXTON: Good afternoon. My name is Dick Sexton. I apologize for the video folks running in and out, but again we keep losing the regions, and they're wondering what's going on, and as you can see, some of them are getting ready to throw their controllers at the screen. So we're trying to keep them happy too.

Good afternoon. And I do appreciate the opportunity to address this crowd. My name is Dick Sexton, and I'm with Connecticut Yankee Atomic Power, the Haddam Neck plant.

Just as a background, the Haddam Neck plant is currently shut down and is in the process of being decommissioned, and as one of the issues related to this that I was asked to give a brief presentation on is alpha, and during the plant operation the facility did encounter a number of fuel failures, relatively significant in discussing our alpha situation at Connecticut Yankee it appears that I'm not saying we have the highest level, but certainly we have residual

alpha contamination that does make it fairly difficult to monitor.

One of the first things that we did when we made the decision to permanently shut down was to initiate a site characterization, and some of this discussion that I will present today is really related to that site characterization that we performed.

The first step in any site characterization or scoping survey is the historical site assessment, and it was during that period of time that we looked at the fuel problems that they had, and we also looked at some of the historical data that they -- historical survey data that they had on the facility, and that was fed into our site characterization.

Our site characterization typically focused in on the areas that we had the least amount of information. So they didn't necessarily focus in on the areas of the plant that actually had the highest level of alpha contamination. But during the course of the site characterization we did develop some scaling factors. And these scaling factors also help us in our radio protective processes. And they're simply the ratio of the alpha contamination that we can't easily monitor to the relatively straightforward beta-gamma contamination that we have. And we use these scaling factors for both characterization purposes and also for protecting the workers from the more difficult to monitor alpha.

And what we found when we did this is that Connecticut Yankee's ratios are fairly dissimilar from what we found at other reactors. Our ratios -- typically the other reactors that we talk to are in the 2,000 to 1, 3,000 to 1, and we found areas in the plant that are 1 to 7, 1 to 20, or even 1 to 50. So what that all means is that in our site it's fairly difficult to just monitor the beta-gamma contamination and ratio back and know reasonably what your alpha ratios are.

And so this required us to make some modifications in some of our instrument types and some of the sensitivities that we had to utilize.

I think one of the most significant challenges that we are a reactor, since we kind of have a mix, typically reactors that go through final survey and even scoping, they're using the scaling factors, and they're using those to essentially say that the presence of alpha is not

there based on their results from their beta-gamma contamination. I don't think CY across the board is going to be able to do that, and I suspect there's other facilities that will fall into that category.

I think the other point that I wanted to make is that with alpha contamination I think it's going to require a somewhat different sampling and analysis process. And I think it's most troubling or most difficult in areas that -- the areas that are impacted, that you know are impacted, you're going to decontaminate, remove the pain. The most difficult areas in a reactor are those areas that you don't think were impacted, they could have been, and that they've been painted over. And then you have a very difficult monitoring process. And I think that's where the innovative sampling and analysis techniques are going to be needed to use and aren't really well described in MARSSIM. They are alluded to, and I think this is an area that we'll be looking at, trying to come up with simple yet defendable ways to monitor this.

I think the technique probably that we're going to use is really just to go into the facility where the paint remains and remove portions, sample the paint, do an analysis, and then allow us to calculate what is actually remaining in the paint, if any.

From what we've seen so far I think we've had a lot of discussions today on dose modeling. I think this is an area that we see as a licensee that both of the codes seem to be somewhat weak in defining the exposure scenarios for alpha contamination.

And the other thing that I'll talk a little more about is background, just the physical monitoring of alpha contamination, with gas flow or proportional-type counters you encounter significant background variations and typically or in many cases these background variations can exceed the limit that you're trying to look for, and it creates a very difficult monitoring sequence.

I guess I was asked to also just talk quickly about some background issues, and this is kind of a generic not just related to alpha contamination but also background. And I'm just going to really quickly touch on cesium-137 and some of the experience that we've had recently

at Connecticut Yankee, some material backgrounds, and also some regulatory guidance that may or may not be needed.

Part of my presentation is related to our experience with some material that during the course of the plant operations at Connecticut Yankee some material was inappropriately released offsite, and we've spent approximately the last year to year and a half recovering that material. And as part of that process to recover the material, we really had to look very hard at the cesium-137 background. We had to go to properties, and our criteria for cleanup was no detectable. So we had to spend a lot of time looking at what the natural background was. And what we found that there is a high variability, I'm sure that's not a surprise, and that cesium-137 is easily measured, so it will be an issue for all soil, and it's out there in fairly significant quantities.

And I guess the other thing that we found as we went through this process there seems to be a very high interest in background evaluation, and I think we have looked at that interest and we see frequently that licensees are making the decision really just to accept the background in their -- as licensee added, and many times we're not so inclined to do a detailed evaluation of background. In many cases I think that's appropriate, because what we see is that at least if we get the code squared away that the 1 or 2 picocuries per gram of cesium that you can see out in the environment doesn't contribute that significantly to dose.

Some of the data that we collected, this was again to support our offsite cleanup efforts that we had to conduct at Connecticut Yankee, and as part of that we went out and collected about 80 samples, and these samples are from the general area of Connecticut Yankee, but they were collected in regions that were not impacted by plant operations.

We looked at where did the wind -- what's the primary direction of the wind, we got a certain distance away from the plant, but we didn't go so far away that the general background from weapons fallout was that dissimilar. And you can see the range of data that we got. Again, our criteria for this offsite cleanup effort was no detectable activity contributed

due to Connecticut Yankee activity. So we really had to find out what is the background.

After much discussion with both of our regulators, which includes Connecticut Department of Environmental Protection and NRC, we worked out a technical basis document in which we established 1.68 as our default background value for evaluating our cleanup of these sites. We also beyond this we actually have up to 2.4 picocuries per gram. We are able to look at that and see if that could potentially be due to some specific condition at the site.

Another issue that you see in background is that background -- this is more going now to background issues related to surface contamination. I think we've talked a little bit about measuring total activity, and there's very significant issues related to actually going into a site and monitoring the surface contamination. We find that background can vary significantly within facilities from one building to another with the same material yet they can have fairly significant different backgrounds.

We also find that material background can depend on time of construction and certainly what type of material was used, and I guess the most troubling part of a final survey process relative to background and materials is many, many rooms or survey units can have many, many backgrounds, and it creates a very complex survey process. I don't necessarily have the solution, but I think this is an area that we really need to have more consistent application of regulatory guidance as to what is an appropriate range of background. It's not unlike the situation that we see with some of these input parameters.

What is an acceptable range for these type of parameters? I have a slide here that shows an example of some issues that we ran into. This is actually from Yankee Rowe.

What this is showing is that this was in the same survey unit so you have a technician surveying block walls and surveying poured concrete and you can see that while the value is significantly below the guideline value, and that is the limit that they are utilizing there, you can see that there is some variation and that is not unexpected. It really is not necessarily an issue, but I would say that our experience or my experience has been while it is not really an

issue, it takes a tremendous amount of time and effort to resolve because I think there is an interest in both the technical folks that are doing this type of work and the regulator to resolve difference in background. Again, my experience is it takes a tremendous amount of effort.

I guess in summary we believe that right now there is no clear practical guidance available to address the large variations in background. There are some techniques in MARSSIMS but the practical implementation of going into the room, scanning the floors, the walls with different material backgrounds -- that is not something that has been totally resolved so that it is simple and straightforward.

We expended significant resources resolving these backgrounds as you are going through and in many cases we believe that that effort really isn't justified. It does make for very pleasant data packages but we believe that it doesn't necessarily add value to the final survey process.

We see that in situ gamma spectroscopy is a viable technique. We think it needs wider use by the licensee and acceptance by all parties that look at the final survey, and I guess finally we see that as some states and some other regulators, EPA, as they reduce the limits that we're trying to achieve, that the complexity of actually going out and physically demonstrating compliance gets more and more difficult and these issues related to background really start having a tremendous impact to your efficient surveying of the site and achieving compliance.

That is all I have. Are there any questions?

[No response.]

MR. SEXTON: Okay, thank you.

MR. ORLANDO: Thank you, Dick. One comment. We are thinking about trying to have a workshop that talks specifically to surveys and I think a lot of the things you brought up here might be addressed then or that would be a good place to do it, so I would hope maybe you could come back and do that again for us.

MR. SEXTON: Sure.

MR. ORLANDO: Are you ready, Henry? Henry Morton is the next speaker.

MR. MORTON: I have been asked to speak to issues involved in surveying, measuring alpha emitters on surfaces. Since we have heard basically more about the reactor side, I am going to tend to speak more to the remainder of the fuel cycle and other materials.

In this regard what I will focus on are key radionuclides, the properties of interest, alpha emitting and for the key ones I will focus on the longest lived ones in these chains and then it will become apparent why somewhat later, but those for the most part are the Uranium-238, Uranium-234, Thorium-230, and Radium-226 in the uranium series; the U-35 in the actinium series, Thorium-232, Radium-228 and Thorium-228 in the thorium series; the transuranics and even Uranium-233 in a case or two.

Basically these are found and we are going to encounter these problems in a number of the processes ranging through these power reactor fuel, mixed oxide fuel, some uses of U-33 -- I am going through these more quickly -- and some other processes, materials, extraction processes where from ores are extracted some of the specialty metals and the materials licensees then encounter -- these natural series emitters; then also the Yellowcake U-06 conversion and the fuel cycle.

In dealing with the issues, basically we need to match up the issues and match up the needs and basically we have a measurements problem. Measurements basically I see as a signal to noise problem and the question, the spread of the signal increment due to the contamination, is greater than some multiple or some numbers of standard deviations of the noise of that measurement system.

The other portion of the problem is one of compliance and I see that as a signal versus a limit problem. How does the signal increment due to the contamination compare with the signal increment equivalent to 25 millirem per year.

Basically then, this boils down to the issue of minimum detectable signal

increment due to contamination needs to be less than the signal increment equivalent to 25 millirem per year. So we will need to look at these two parts of the equation, the minimum detectable aerial density on the surface of this alpha emitter and the aerial density of the same equivalent to 25 millirem per year, and compare these and see where we stand.

What basically we have here is I have taken D and D, and for the default cases, for the building occupancy scenario, calculate for these individual nuclides and change the total effect of dose equivalent that corresponds to this unit input. In other words, for one dpm per hundred square centimeters, first, what kind of dose does it give, that is the output of D and D. And the way the code operates, it basically likes to do it on a per unit parent basis. Then, because these -- that is, basically, then I convert that to the maximum acceptable average aerial density of the radionuclide in units of disintegrations per minute, per hundred square centimeters that gives a 25 millirem per year dose. So these then, for these nuclides and chains, would be the limit.

In this case, by virtue of the way the code operates and the calculation goes, a disintegration in this case is, in effect, it is the technical definition, that is, it is an atomic disintegration. So we see that some things that may seem to be different from what we have been used to seeing in the distant past, for instance, radium-226 alone seems to be elevated somewhat, but look at the thorium series, we get quite low numbers, 7 disintegrations per minute per hundred square centimeters for thorium, and you add the chain in, basically, it drives it down to 6. And on the -- in the right column, these are the main contributors to dose. So if we look at those, we are basically seeing that what D and D is telling us is that -- for, of course, uranium-238, or uranium natural, it is without daughters grown in, of course, that is going to be the uranium.

But, otherwise, looking down the list, basically, it is mostly thorium and, in the case of the individuals, the radium and thorium in those chains. So those become the main -- the key nuclides, and the limits expressed in disintegrations per minute per hundred square

centimeters.

Then comes the question of which parameter to measure to begin to assess compliance with this, if we are measuring this material on the surface. So it becomes a question of whether to measure the total activity on the surface. That may be required for the direct radiation exposure pathway.

Then there comes to me the question that has been discussed today and previously, whether to try to measure removable. And there is that question of whether, depending on the model that is used, whether removable may be necessary to measure, or then the alternate question, or, finally, whether to have to measure both of them. The migration of the conversation today seems to be toward measuring total activity on the surface. And because that seems -- that is basically my preference, so the remainder of the talk, I am going to deal with the measurement of, in effect, total activity on the surface.

Then in doing measurements on the surfaces comes the question of what -- which of the radiations to measure. Alpha -- well, alpha, measuring alpha has the advantage of having a low background in the instrument or the measurement system, and, thus, you can get, relative to the other two, a relatively lower LLD. Disadvantages, it doesn't penetrate very well from a rough surface or a dirty surface, or a painted surface, or a greasy surface. Because of that, that is a prominent problem with measuring alpha.

The materials backgrounds can vary by 15 times or so according to the data in NUREG-1507. And then I have some other data later that you can see the range that we saw in particular materials. Other disadvantages, the detector windows have to be pretty thin and fragile, and in the work that we have done in the past, when you get to these low levels, trying to measure these, the lowest levels we can, we eventually abandoned gas proportional detectors, the field survey instruments, because they simply varied, they slowly varied too much. And so when I basically discuss most of the rest of these data, they are going to be oriented toward alpha simulation rather than gas proportion.

Another radiation that might be measured or surveyed to detect the alpha emitters on the surfaces might be to measure the beta. Advantages here, from my perspective, it has almost -- of the three radiations, relatively, nearly the optimum penetration framework the surface. It is less variable in its dependence on surface roughness and on self-absorption. The materials backgrounds seem to be less variable and the source efficiency factor is better than for alpha and is less variable.

The disadvantages basically are for the enriched uranium, or the purified uranium sources, the uranium tends to emit weak beta and so surveying by beta is generally not done because of that. Measuring beta at the exclusion of alpha and gamma requires to measurements rather than the one for alpha or gamma. It does have a higher MDA than for alpha, and if there is a prominent amount of potassium-240 around, or may be, then you may need to account for that at these low levels and have to subtract it out.

When I think about using gamma, the advantages, the only one I can -- the one I really think of at the moment is the detector can be more rugged in the window than the others. The disadvantage is that basically the gammas tend to be strong enough to penetrate from down in the substrate and so you may be measuring something below the surface, particularly the background that you don't really want to be measuring and so that may tend to make the background high and variable and for that reason or for those reasons measuring gamma would not be my choice.

Now when I put the numbers up for the derived limits with D and D these are expressed in atomic disintegrations per minute per hundred square centimeters, but we don't measure that with the instrument. We really measure one of the radiations, so there is a translation that needs to be done here and in looking at these key nuclides and the change of these, basically when we go count the number of alpha or the number of beta and internal conversion electrons per parent disintegration we get these numbers -- that is, Thorium-230 which a lot of the instruments calibrate with, has one alpha per disintegration, but the uranium

series you can get, when it is in equilibrium, 8 alphas per disintegration of the parent, U-38; likewise, Radium-226, five; thorium natural and the chain, six; and so on.

Similarly, if we go count detectable betas, and these numbers happen to be ones that I concluded were the detectable betas -- that is, that would penetrate the window that we had on the beta instrument -- and that included the internal conversion electrodes.

Looking at the ratios of these we basically see in terms of detectable radiation you have got an advantage for the alphas -- not many betas are better detectable, so that affects the conversion and the derivation of the detectable radiation in alpha per minute per 100 square centimeters or beta per minute per 100 square centimeters.

Now when account for these detectable radiations and then compare it with the old numbers that we are used to, whether we think of it in terms of the same table from the Reg Guide 186 or FC 8323, remember that the tables for these natural chains in Reg guide 186 are really expressed in terms of the radiation per minute per hundred square centimeters. The practice has been to interpret the other nuclides in the same way because the other ones that we are interested in except for one basically emit an alpha per or beta per disintegration.

So when we look at the comparison of what D and D expressed in alpha per minute per hundred square centimeters, how it would compare with the old numbers, we basically see for example that the purified uranium has gone down by a factor of 50, natural uranium down by about a factor of 50, and this is -- the natural number is particularly I think interesting to fabricators because the number for enriched uranium is not going to be much different because basically the dose limits are not all that much different and these basically don't have the chain to add more alpha particles, so it is going to be pretty close to a one to one comparison with natural and enriched within not very many percent.

Uranium with daughters present, 152 compared to 5000 -- down by a factor of 33; and going on down the chain the things that are interesting to see is that the radium for example, Radium-226 went the other way; radium with daughters is about the same; thorium, it

went way down; thorium with daughters present, 28, it goes down, and so on.

So the numbers, the differential, is not quite as wide as you might think without making that radiation to disintegration translation.

I have taken some real cases, that is here are four buildings which we went in, took scrapings off the walls and floors, did alpha spectrometry to get the ratios of the nuclides, and this is a process that extractions and the ores had both uranium and thorium series in the ores, so this is among the more complex cases in deriving numbers that we'll see -- the uranium series, the thorium series have all six of the key nuclides and they are not necessarily in equilibrium. Fortunately it's old enough that the thorium series, the three key nuclides in it pretty much have to be pretty close to equilibrium. In fact, the measurements, the ability to measure shows itself to be perhaps not as good an indicator as the calculation from the decay chain and the age.

In this case for this mix, taking the values that are the minimum -- I'm sorry the Maximum Acceptable Average Aerial Density that we derived using D and D, gives these activity limits. They range basically from 10 to 19. I take those and convert them to an alpha limit and I have drawn out in this case the average number for these four mixes, the average number of alphas per disintegration.

Then because that is the case the conversion calculated differently but the conversion gives you in effect a limit of the 68, 134, 83, 111 alpha per minute per hundred square centimeters, so this would be in effect the compliance value that you test individual measurements against or test your measurements against to go into the building with an alpha survey instrument.

Similarly, I have done the same thing for the betas and those limits are somewhat lower because you have fewer betas per disintegration for the same source.

So basically keep these kinds of numbers in mind when we go to look to see how well what is the minimum detectable aerial density that you can see with an instrument.

I wanted from this point on to take two sources of information, one that is available and one for which there are already published data. In this case for the alpha I am taking these numbers out of NUREG-1507, where Eric Abelquist and the people at ORISE did these measurements and unfortunately this is the limit of what we have -- for some cases point sources and the efficiency tends to be high for a point source relative to a distributed source so when we have actually got distributed sources the source efficiency data that we have here overestimated somewhat probably, but the point is one of the values of parameters we will need is the source efficiency and here are the data for alpha I am going to use.

I put blue marks by a couple of them, one for scabbled concrete. That one is probably high for a couple reasons. They scabbled it and then put the source on. Secondly, it is a point source not a distributed source.

The other one I don't have an opinion about it, but I call it to your attention, because here's carbon steel with a source efficiency of .22 for alpha.

[Pause.]

MR. MORTON: This may have real significance by a factor of five relative to what you have been doing particularly for example for the power reactor work where you may be working on a lot of the steel plate.

Basically these numbers are based on Thorium-230 alpha, which is basically representative for these series for calibration for these series and talking about it there was a 74 square centimeter zinc sulphide alpha scintillation detector. There are one minute counts and these data are in NUREG-1507.

So what I was interested in is basically in looking at this is what is the best we may be able to do, so one of the things -- no. Before I go to that let's see what these numbers give us in comparing what the minimum detectable is as reported in NUREG-1507 for this instrument compared to the Maximum Acceptable Average Aerial Density derived with the D and D for defaults.

Fundamentally, this is the comparison. Probably a couple of general conclusions that one tends to see. Maybe you can see something for the really slick surfaces like stainless, formica, something else but the LLD or the minimum detectable is not below the derived limits for the other materials, particularly the rough surfaces, the concretes, the untreated wood, the carbon steel in any of the cases and in particular I have noted the natural uranium because again that carries over to a similar number for purified uranium or enriched uranium.

For the others, for the chains, look at the uranium series when it is in equilibrium, it is 152; thorium series, 36; and then of course when you have the both of them, as in the numbers I showed earlier for the four buildings, these combine and you get those limitations.

Now the next thing I wanted to do was see how well I could do with a better instrument. One of the things I looked at, and I had these data from some previous work that I'd done 3 years ago doing the same thing with respect to measuring beta as well as I could, and the same -- one of the things we have to look at is well, if these organized data were counted for one minute, how much better can I do if I count a longer time or measure for a longer time.

Basically you see that you get a knee in the curve between around 2 minutes, 2-3 minutes, so there's really not much need to measure more than 2 or 3 minutes with these instruments. You're just spending more time without getting much more advantage.

So what I did then, take an instrument that was better. A, get a real clean instrument with a low background on it, and in this particular case about a count per minute per 100 square centimeters, 100 square centimeter window, instead of 75, count longer, and then count the background longer.

In this case basically what I did was for these data coming up a sample count time of 3 minutes, a background count time of 10 minutes, this instrument with a lower background, less noise, also had somewhat less efficiency than is sometimes seen, .15 for the

instrument efficiency, and then I used the source efficiency numbers, that I've just showed out of NUREG-1507, and derived the minimal detectable aerial density for this alpha scintillation detector for these cases.

And basically what they did for me was to take this instrument and go out and measure these different materials. So this accounts for the material background which is used in deriving the MDAD.

So this is probably the very best that one can do, as far as I'm concerned within reason the best you can do with a portable field instrument of this kind. Basically what you begin to see is slick surface like the Formica desktop, which I used, .5 as the source efficiency in that case.

When you look down through these you basically say the only place I've got any hope of a minimum detectable that's below the limits for a slick surface I can clean up anyway, and that will -- or a Formica top or something like that. The instrument's just not going to do it for any of the rest.

You get if you look down through here numbers and well, you've got, I use the slick surface number for the linoleum and the Formica and so on. I suspect the source efficiency is going to be lower for the concrete and thus lower for the concrete slab, the sidewalk, the bare brick, the fire brick, and so on, these things that are not coated with a lower source efficiency then they're going to have a higher detectable limit correspondingly.

More likely the instrument may more likely have instead of one count per minute may have two or three. If it has three, to give you an example, the minimum detectable with no substrate that is in the air would go up from 20 to 30. So a lot of these numbers would go up not necessarily linearly but they would go up more with an instrument that's not basicly a pure, clean instrument.

Now, some other things that are problematic with measuring alpha, although you can get the good minimum detectable, that is, relative to the other methods, Ablequist and

Loris had looked at the effects of paint and the effects of dust, and you see a couple of things in here in these data. Basically, once he started adding -- once he started looking at greater thicknesses of paint, the minimum detectable goes up pretty fast. So, by the time you get to, say, the 2.77, you are already up at 135, up at three -- 223, so the 223 is above everything, all of these other limits we have seen, and that 223 at three milligrams per square centimeter, that is only 30 grams per square meter.

By contrast, one of the real advantages of measuring beta begins to show up. It is not much affected because of the penetrating ability of the beta or the internal combustion. Not much affected through this range.

Similarly, Loris' work, the same kind of thing in looking at dust on the surface. You can tolerate quite a bit of dust with the beta measurement, but not much dust and still stay below the limit, or have any chance of it for any of these for dust on the surface. So, 30 grams a square meter or so.

At that point, beyond those, I begin to summarize some of these issues and mention some others. The first question or issue, whether to measure total activity, removable or both, and the evolution of the migration today seems to be toward total, which is what these slides have dealt with.

This has raised the question of detection sensitivity versus the limit. Fundamentally, with the field instruments we have got for these derived values, it is really not going to do it.

Suitable background material. The multiple backgrounds in a single survey unit and in buildings, which is -- this is a pretty prominent issue. I remember a survey unit that we were characterizing that had steel plate concrete floor, part-block wall, brick wall, Celtex ceiling, and glass windows and steel window frames. So after we had mentioned this multiple materials in one survey unit, there is some dealing with that that the MARSSIM Committee operated on.

Determining source efficiency. This is not a trivial issue to get it and get it right,

because it does require going and measuring. There is another issue I haven't addressed yet, and that is, for surfaces, if you have a radium to radon decay in the chain, do you get emanation that changes the number of alpha per apparent nuclide.

I'm sorry, I was talking about the multiple materials in one room as the third point. The suitable background material is sometimes just simply being able to find a suitable background material. We have had, in order to try to find some, taken bricks out of the building and sawed them in two and cleaned up the interior half, taken concrete bricks, sawed them in two to look inside the concrete to get a -- taken floors, sliced them in three slices so we could see the middle slice.

I'm sorry, let me see if I can switch pockets. Okay. And then some more problems. The dirty surfaces problem. At these low kinds of values, a couple of cases we have seen where, for some reason, the window sill looked like it was contaminated. Eventually, it turns out, we found out it was just dirt on the window sill. The top of a large storage tank looked like it was contaminated when the side was not, so we were able to find another one just like it two or three miles away, the same problem, it must have been dirt on the top of it. At these low levels, these are the kinds of things that are going to show up. Ordinary dirt and dust is going to have enough of a change in the background relative to some clean low background surfaces to trip you over the limit.

Finally, one of the provisions of MARSSIM is for an elevated measurements criterion. We need to be able to derive that and it appears that for the building occupancy scenario, for the building scenarios, it appears that D and D needs to have this, to have floor area parameters programmed into it so that you can vary the fraction of the floor that is contaminated in order to be able to derive these.

And, finally, if these instruments don't work well enough, then what is the measurement technique to be done or actually the right answer is work on the models to get the derived values to be more realistic and working on the suspension factor I think is the step in the

right direction, because all of these numbers are dominated by the suspension inhalation pathway.

Finally, because of the timeliness rule and because the staff is encouraging not to quit and wait, we have had no choice but to continue to do the characterizations and the surveys and the decommissioning plans based on the best that we can do. So if these are not now good enough relative to minimum detectables, the fundamental question is what to do with the data. It has basically been -- not much choice except to keep going and we need to be able to use these data, both from an expense standpoint and from a time standpoint, not to have to regress a year or so to do our characterizations again.

That's it. Any questions?

MR. DARMAN: I'll ask a question. Joe Darman, Maine Yankee.

Did you look at the effects of changing the resuspension factor? It looked like one of the big problems was the DCGL was below the MDA, right, of the instrument.

MR. MORTON: In this particular case.

MR. DARMAN: The resuspension factor we're down at 1 E minus 6. Does that get rid of that problem or not?

MR. MORTON: It would probably get rid of the problem. I didn't attempt in this case to tackle that problem. That was, you know, the earlier part of the workshop. The part that I tried to deal with is the ability to measure, how well can we measure, and how does that compare with the default values. I didn't try to work on changing the defaults or anything in the code.

MR. EID: Okay. If there are no more questions, I believe we will move to the last speaker. We apologize for the last speaker because we tried to be on time, but because of the lots of discussions and questions, so I guess that's the purpose for this meeting.

And the next speaker, please, next.

MR. ORLANDO: The next speaker is Claude Wiblin from WEM, and he's going

to talk about demonstrating how to calculate the indoor area factor.

MR. WIBLIN: It's a shame we can't hear the sound. We have great applause in this.

I've been asked to speak on calculating the area factor when you might want one and when you don't want one.

First of all we need to understand primarily that the second bullet, DCGLs, are based on the area of contamination. Henry started talking about this topic last. But anyway, if we have a DCGL for this entire room as far as clearing it in the final status survey, we should also be able to calculate an area much with a higher DCGL that's much smaller. All right?

This is important to us because it will allow us to release a Class 1 area with contamination levels in small areas with greater than your quoted DCGLs. And of course once you get through with it, you have a hot area or an elevated area, the rest of the room has to be somewhat downgraded in contamination levels, and the unity rule will apply.

First of all uniform concentration, as soon as this thing gets built up -- you guys are still awake, right?

This is basically a fairly representative uniform concentration, evenly spread, kind of what you'd like to expect after you've spent all of your money deconning the place. This represents an elevated area of contamination. It can be much higher than your average contamination, but when we average it using the rules and the formulas in DG4006, we can actually release this building, room, site, with some of the area having much larger contamination limits.

The importance, one of the areas you have to worry about, if your MDC scan is greater than your DCGL, you're going to have -- you may have to take more measurements in a Class 1 survey unit. Of course for all of us we think that's bad because at this point we've already spent our money, we've deconned the place, we took truckloads of dirt off, concrete, cinder block, whatever. And you do have to consider your area factor in every Class 1 survey

unit whether you use it or not. You have to start looking at the MDC for your scanning survey instrument versus your DCGL.

But up here are some example comparisons of the MDG and DCGL. These values really come from a draft from MARSSIM. MARSSIM has some tables in there. I used RESRAD to come up with these numbers. Expect your actual DCGL to vary widely, of course, from site to site, area to area. No way any of us can come up with one that will fit all. But you're going to start getting into trouble when you're scan MDC is greater than your DCGL. These are the values that DG4006 alludes to that you can use and take as automatic as far as your scan MDC when you're walking around out in the farmer's field looking for that cobalt-60, cesium-137. If this is your given scan MDC, you can't see in some cases all that you're looking for.

I'll give you an example as to why this is important to you. In MARSSIM they use a triangular kind of a base for calculating the number of points. I'm still a square kind of a guy. All the survey units that I've put out and looked at so far to date have been squares. We have not dealt in triangles and double triangles this difficult.

Anyway, their example on page 5-39, they call it Example 2, you're given an area of 1,500 meters squared, a DCGL, an MDC of the scan, and by their other calculations that you do to get to this point, they came up with 15 data points, probably 15 soil samples that they have to collect in this area. You go through your square pattern calculations and determine how big this area is. Of course it's 100 meters squared.

And moving on, you have to start looking at the area factor, and this is from that Table 5.6 in MARSSIM I just told you about. The area factor is 1.2. So with this area factor of 1.2 AF, you multiply it by your DCGL, which is 3, you come up with 3.6. Well, you can't see that. There's just no way that you can measure that walking around with a sodium iodide detector in the field.

So you go back and redo your calculations and you come up with 50 points that you have to go back and measure instead of 15. That's a lot of dollars, an awful lot of dollars,

it's a trap at the end of the survey cycle that you don't want to get yourself into. You want to look at that survey instrument before you go out there, try to find a survey instrument that's equal to the cause. Because you're probably out of money.

I put these up just as a reminder. New NRC acceptable screening levels for unrestricted use. These are, of course, in dpm per 100 centimeters squared. Everybody familiar with these numbers? I like to put them up because most people start to gasp at the 10 to the minus 8th.

So one of the things that we have to start looking at to reduce our errors towards the end of the survey is how do we handle these MDC scan numbers. What is good, what is bad? Of course, we can look at the background. This new formula, surveyor efficiency, your instrument efficiency, and your source material efficiency. That, you can probably play with, mix it on your site and get maybe a 10 percent, 15, I forget what the number actually is, above the ISO standard recommended values.

The one I want to talk about later on is this one, surveyor efficiency. These are what all those little things are -- you have the handouts, I hope.

One of the things that I have been doing is we are using data loggers. We are also controlling the speed of the data logger. Routinely, you are supposed to take this thing and push it up, and if the guy hears something or sees something, he is supposed to back up and wait about four seconds, right. One detector width per second. He stops and waits four seconds and says, yeah, was that a hot spot or was it not?

I really just like to set that thing and let it go. That takes the worry from me is that guy surveying too fast, is he surveying too slow. It gets the job done. At the same time, we are recording the data. No longer do I have to ask the surveyor to write down what his high and low, minimum, his gas average type thing. I have it recorded. And we can make a report of it. In other words, the gross gamma, subtract the background away, number of measurements, this is on the gamma, this is the alpha beta, the minimum, the maximum, the average. We have got

it all. And the scan values are coming up to be very close to the direct measurement values.

But the question is, right now I am only allowed to use a value of .5 for surveyor efficiency. Because that is in the denominator and it is a square root, what do I get now? Do I go up to .75? Can I go up to 1? When I am approaching 1, I am getting real close to a static measurement type MDC. So, anyway, I think this is a point that needs to be negotiated with the NRC and find out where do we stand with those.

And, of course, it is important because in Class 1 we are going to doing a 100 percent scan.

Reg. Guide 4006, draft guide, it says you can use those. As soon as you do, one of your technicians is going to walk in and say but I have got a three by three, what is my MDC? So you have to go back and start looking at how they derived these equations.

A distance of 10 centimeters above, this is out of MARSSIM, they claimed to have used, I guess they used Microshield. These are the values of the parameter inputs that they had. I am kind of rushing through this, I have a lot more I could say.

The area factors in RESRAD, they are given to you, by the way, an area factor is equal to the ratio of the dose rate per unit concentration at 10,000 square meters to that of that of other areas. In other words, they ran their RESRAD program for 10,000 square meters, then they ran it for 3,000, 1,000, 300 and so forth, then they compared doses. So when you have a small area, you are allowed to have more contamination.

And they published a table similar to this. I ran the code over, the same numbers came up. This is the way it looks on a 3D kind of thing, comparing all of them. Look at the area factors I can get. For a real small area, wow. That means I can have some pretty heft contamination levels in small areas out there in the floor. Wait a minute. I'm sorry, this is outdoor, out in the farmer's field.

They did the same thing with the -- except they used RESRAD Build and this time they started at 36 down to 1. Then they compared the doses. I believe this table was

published in MARSSIM also. And this is the way they look. This means that for some small areas, I am going to have some pretty hefty multiples that will permit me to apply, at least, to release the building.

MARSSIM also said that you could interpolate between the points if you needed to. What we did, we have got a computer modeling code, came up with best fit equation. Just another tool that you can use if you have to. If you are close to the limit, you might want to take the effort to look at that.

Then of course in the final status survey you have to come up with the numbers to the evaluations to make everything right and for this elevated area you are not just allowed one but you can have several, okay? Again I think I said that the unity rule applies, and that is where you have to start working with this unity formula.

You have your average residual rate of activity for all sample points in the survey unit that are outside the elevated area over your DCGL and plus your average concentration in the elevated area over the area factor times the DCGL and it also says that if you have more than one include a separate term for each.

So anyway, when all is said and done, we've scabbled, we've power sprayed, we've washed and everything else, hopefully not very many of us will run into an elevated area but if we do the tools are in MARSSIM for us to look at, consider, and try to convince the regulators that the site is acceptable.

MR. MORTON: Yes, Henry Morton. A question. When you derived the area factors for the indoor scenario, what did you use as a basis for those? Did you use --

MR. WIBLIN: RESRAD. Straight RESRAD. Pull it up on your screen, select your nuclide and go with it.

MR. MORTON: Indoor parameters, indoor scenario, indoor pathways?

MR. WIBLIN: RESRAD Build instead of RESRAD, but those aren't mine. Those are MARSSIM. This is the work of MARSSIM people.

MR. ORLANDO: Okay. Anybody else?

[No response.]

MR. ORLANDO: Thank you, Henry. That concludes all of the presentations that we had for today.

Traditionally at this point we open it up for anybody to sort of bring up anything they want to talk about or any discussions that we have been having earlier today -- start them back up.

Does anybody have anything they would like to say about any of the stuff we have talked about today?

MR. GENOA: Yes, thank you. Paul Genoa with NEI.

Appreciate your indulgence all day on some of my questions. I wanted to kind of focus on I guess five points that I picked up today, the first being that the earlier presentation on the available input parameters. I think the presentation was very well done, probably should have commented at that time that you asked all the right questions.

My point would be when do we get the answers and what are people doing today? I hope that we can continue to decide how to answer that question -- what parameters are out there, what do you currently use, what do test on the codes, what are some ideas and let's keep open communications because these guys are out there trying to run these codes and come up with the right input parameters.

Second, related is that the D and D code that you pointed out with the probabilistic functions added to it, it looks like it is more than a year away from availability, so again help us work on what people do in the meantime if there are ways to use existing Rev. 1 D and D code or other tools, things to do, because I heard some ideas that we discussed around that may help us work around that in the meantime until the new codes are available.

The third point -- EPRI and the industry has been asked to help support some of these comparisons and we are certainly willing to do that and just speaking on behalf of EPRI

and Man-Sung, he is doing good work. I think he's working at somewhat of a handicap and Walt, I heard you offer that you are willing to be open in communication and you can perhaps help him work through this. I just hate to see him beat his head up against the wall if some of the information is out there that he is trying to understand, so if you want us to work for it, please help us or help him get that data, get the work and make it available for him, anything that might be helpful, and I just appreciate your openness because we will get the answers sooner, that's all.

Third, a point that Dick brought up, I want to point out, ultimately we'd like one tool to get one answer to justify to the public. Two tools, the codes, probably results in two answers. Two answers result in adjudicatory hearings that judges want to ask why I shouldn't take the worst case. So ultimately as we get down the road, we probably want one code, one answer, or certainly a very clear understanding of why the two codes have different purposes, different applications, et cetera. And with that in mind, I'd like to help work towards kind of getting the effort in that direction so we ultimately end up with something that's very defensible for the industry.

Finally, I don't know how to put this, but as all good engineers and scientists, we start with the end in mind. If your end is developing a tool that helps you assure compliance with the 25 millirem plus ALARA standard, I want you to recognize that in the greater world out there there are other agencies that have other ideas about what that dose criteria should be, and they continue to try to impose their will. If it was only another agency, that wouldn't be so bad, but in fact there are agreement States that are implementing your authority that also appear to have some different ideas about what the dose ought to be.

I only raise this issue because as you're developing your input parameters for a code, you have a certain comfort zone of what your conservatism should be, and the conservatism to beat a 25 millirem number is one thing, the conservatism that will meet a 4 millirem water pathway is a totally different thing. And I want you to be sensitive to that, because

that is the reality.

And I guess those are my points. Thank you.

MR. ORLANDO: Anybody else?

MR. POTTER: Yes, Tom Potter.

I'd like to follow up with one comment that's a little bit related to that.

I actually spent some time in the probabilistic risk assessment area, and it appears that we're kind of evolving into that kind of approach here. But one problem I have is that we're evolving in a limited way. We're only looking at the variability or the risk or the uncertainty in one piece of this.

I think, for example, where the cases that are going to be the real tough nuts for the decommissioning regulation, the restricted release situations, for example, in the back of our minds, maybe not explicitly, but at least in the back of our minds we need to be thinking that these uncertainties are really contingent probabilities, contingent probabilities, for example, assumption of a probability of 1 for loss of institutional controls, which is not a reasonable assumption.

If you really wanted to explore through the whole or apply the uncertainty analysis to the whole process, you would assess the probability of losing institutional controls, which I think is low. We don't really have to carry it to that extent as long as we have in mind that we've got these extra conservatisms in the form of noncontingent probabilities back there.

One way of accommodating that kind of thing is to focus more on the center of the distribution for the piece of the problem we're analyzing rather than the 90 percentile or 95 justified.

MR. ORLANDO: All right. Just to throw out a conversational or comment on that, I addressed the National Academy of Sciences a couple of weeks ago, and one of their -- they were looking at institutional controls, and we got into a rather lengthy discussion because one of the individuals on the NAS panel felt pretty strongly that none of the institutional controls

that have been used for restricted sites up to this point, principally in the EPA world, really worked. He cited, you know, Love Canal and some other places.

So I hear what you're saying and I think we have to look at that. But there are others out there who think that the probability of institutional control failure always is 1. So --

MR. POTTER: If you really believe that, do you want that part of your regulation?

MR. ORLANDO: Oh, I don't necessarily believe that we can't do it. I'm just saying that when we say definitively that the probability of the loss of institutional control is low, I think one of the things that we have to look at, NRC has to look at, is the institutional controls or the restrictions to try and make sure that that probability does get smaller and smaller.

MR. POTTER: There is one other --

MR. ORLANDO: Why don't you come sit at the table, Tom? You're getting more exercise than I've seen you get all year.

MR. POTTER: Well, this is the last of what I think. But there's one other protective factor here, particularly for these tough nuts, and I'm talking uranium, thorium situations. I believe that we're talking here about a handful of sites that we're working on, and I believe there are lots of other sites in the country where these materials in comparable concentrations often are not regulated at all. So in a sense the worst we're doing is allowing for a small -- even with loss of institutional controls -- allowing for a small incremental increase in the number of such sites. That's another way of looking at the problem.

MR. ORLANDO: By the same token, too, you know, there is a dose cap if there is a loss of institutional control, and that would be below the public dose limit anyway. So, I mean, the concern that that individual on the NAS panel had, and when you have a loss of institutional control or something like that at perhaps a hazardous waste site, the consequences I think are probably higher than if you have one under restricted use that NRC's put together where there is the dose cap in the event institutional controls fail.

One thing that I want to take away is that Paul didn't mention was that I think you all are looking for some guidance on partial site release, whether it be either in the reactor world or in the materials world. We weren't able to do too much discussion of that today, and I guess there's multiple facets of that, both the release of unimpacted areas, large areas that have power reactors and some discussions I had with some folks during the breaks, release of sites where perhaps it's been remediated or you can just come in and carve off a chunk and say look, this was never used, and we've surveyed it out, and now we don't want this on the license anymore. I think there are ways to do that on the materials side through license amendments and what not, but it looks like that might be a little more difficult on the reactor side.

So that's something I'm going to take to NRR and see if I can't maybe in one of the subsequent workshops have somebody come down and at least talk about what they're thinking about along those lines.

Does anybody else have any observations, thoughts, or what not about what we've talked about today?

MR. MORTON: I think what -- Henry Morton -- what you just mentioned I think has three facets, at least three facets. One was the breaking the nonimpacted from the controlled area. Second is a case in which you might want to separate the impacted area, part of a site, from the remainder of the site, also impacted, under the new guidance.
Both under the new guidance. And then the third possibility is to separate part of a site under a grandfathered condition from the remainder of the site under current regulation.

MR. ORLANDO: Do you mean grandfathered in that 20.1401 sense where it would be released under SDMP criteria?

MR. MORTON: Yes.

MR. ORLANDO: We are looking at that one, at least on the material side right now. There are some questions associated with that.

Paul, was there something you wanted to say?

MR. GENOA: I was just trying to clarify. I think Dr. English pointed out two different options currently viable.

MR. ORLANDO: Pull the microphone --

MR. GENOA: Yes. I'm sorry. Paul Genoa, NEI.

Clarification. I believe Dr. English's presentation pointed out two different approaches we currently believe are viable and are fairly straightforward in the amendment of our existing Part 50 license to describe changes to license boundary that would allow for the early release of non-impacted areas. I'm not sure that I -- and if it was an impacted area, that's a different story, and perhaps we could talk about guidance on that, but I think we understand how you would do that as well. So we can talk about it some more, but I think we understand the -- currently the release of non-impacted areas from a facility.

MR. ORLANDO: Okay.

MR. GENOA: Operating or not operating.

MR. ORLANDO: Well, maybe, then, it's something we just need to look at more on the material side.

That's about it, then, for the discussion today. I just have a couple of little housekeeping things for everybody.

First of all, there was a request made last time to try and put the transcripts of the meetings on the Web site. I looked into that and apparently there are some memory problems with doing that, so what I'm going to look into is having the transcripts available on diskette, and if you want some -- if you have a particular one you want to listen to, we'll go ahead and make copies and send it to you. I have to check into that. That's not a promise; that's one way I'm trying to make sure that everybody can see everything that was said.

There is a preliminary guidelines for evaluating dose assessments that was posted outside. I think we're going to try and put that on the Web here pretty soon. Everybody can take a look at that. That's what Mark used to -- well, Mark's gone -- as part of the

development of Tim Harris' presentation.

Let's see. The last thing was just a discussion of what's going to happen over the next couple of workshops. The next one is groundwater modeling. I'm not sure if Tom Nicholson needs or is looking for any volunteers, but I will check with him and let Paul Genoa and Dave know, Dave Colbersome know, if Nicholson is looking for anybody to participate. I looked at the agenda and I think it's pretty -- there's a lot of folks on there already. But if you're interested in groundwater modeling, the week of the 23rd, here's the place to be.

As far as the August workshop, folks from the CRCPD had indicated that they had hoped to be able to say something at that workshop, so perhaps the August workshop, and we'll try and talk this up a little bit more, may look at perhaps surveys and agreement state issues, and maybe we won't have a day that's focused just on dose modeling the way we normally do.

I throw that out for consideration. The agreement state folks wanted to be able to come in close to the close of the comment period on the draft guidance so that they could verbalize concerns that they had.

Also, I'm not sure if we need to have one on surveys, but given some of the things that came up today, I they it's appropriate, plus -- oh, to give you a little bit of information, also we lost -- as you know, Dave Fauver is gone or has left NRC. He's sitting in the back somewhere; he's not -- there he is. And so we've had to -- Dave was putting together the surveys portion of the SRP, and to kind of give you an idea of Dave's value here, we had to mobilize to do it. So we're going to have -- Tim Vitkus and Eric Ablequist are going to be working on that for us.

We also don't have the individual who was working on the financial assurance section of the SRP. He rotated out. So we're going to have ICF come in and do some of the work on that. So we're hoping to -- as we lose staff, we're hoping to try and replace them using contractors or other staff. So we're still hoping to get the SRP out in the same time frame. But if

we do have a surveys module workshop, I think that will involve bringing Eric and some of those folks down because I think they're the ones who can speak about it the best.

Does anybody have any thoughts or observations or anything before I kick you out of here?

[No response.]

MR. ORLANDO: No? Okay. I want to thank all of the speakers for speaking, and I want to thank all of the participants for participating, and I want to thank everybody else for staying as late as they have. One of the down sides, once you get rolling on comments, you don't want to shut people up, but by the same token, you want to make sure everybody gets out of here. So thank you all very much.

[Whereupon, at 5:17 p.m., the workshop was recessed, to reconvene at 8:30 a.m., Friday, March 19, 1999.]