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CENTERS FOR DISEASE CONTROL
NATIONAL INSTITUTE FOR OCCUPATIONAL
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ADVISORY BOARD ON RADIATION AND
WORKER HEALTH

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MOUND WORK GROUP

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TUESDAY,
JUNE 5, 2012

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The Work Group meeting convened in the Zurich Room of the Cincinnati Airport Marriott Hotel, 2395 Progress Drive, Hebron, Kentucky at 9:00 a.m., Josie Beach, Chair, presiding.

PRESENT:

JOSIE BEACH, Chair
BRADLEY P. CLAWSON, Member
PHILLIP SCHOFIELD, Member*
PAUL L. ZIEMER, Member

ALSO PRESENT:

TED KATZ, Designated Federal Official
TERRIE BARRIE*
ROBERT BARTON, SC&A*
RON BUCHANAN, SC&A*
JOSEPH FITZGERALD, SC&A
KARIN JESSEN, ORAU
JENNY LIN, HHS
JOHN MAURO, SC&A*
ROBERT MORRIS, ORAU*
JAMES NETON, ORAU
BILLY SMITH, ORAU*
JOHN STIVER, SC&A

*Present via telephone

T-A-B-L-E O-F C-O-N-T-E-N-T-S

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

9:00 a.m.

MR. KATZ: Is everybody in here ready to get going? Josie, are you?

CHAIR BEACH: Yes.

(Roll call.)

MR. KATZ: Very good. There is an agenda for this meeting. It's pretty simple. It's posted on the web and the Chair can go through that.

And there are also various documents related to this meeting, and they should be posted on the web as well.

And it's your meeting, Josie.

CHAIR BEACH: Okay.

MR. KATZ: And just to remind everyone on the line when you're not speaking to the group, please mute your phone. If you don't have a mute button, use *6. And then press *6 again to unmute your phone. Thank you.

CHAIR BEACH: Thank you. We are

going to go ahead and take off where we left off on our last meeting on April 10th.

We're going to start with tritides. The Agenda as Ted pointed out, is pretty brief. I didn't give any times because of that.

So, we'll start with tritides. We'll work into adequacy and completeness of internal dosimetry. There's a couple items on that.

We'll talk about Work Group recommendations, and then some action plans as how we'll proceed at our meeting in June in Santa Fe.

And then I did ask SC&A to put together the Site Profile issues for the last four to five years we've been working with the Mound. And we didn't really want to do anything with it other than just to get it on the table, give NIOSH a chance to look at it, SC&A, make sure that we're capturing all the Site Profile issues. And then make a plan of

how we're going to correct the Site Profile issues so that we don't lose any momentum there.

And then I did want an update, and I talked to Jim about it, it is not on the agenda, but an update on the radon issues that we discussed in our April meeting. So, just kind of where NIOSH is with those items.

And the last work paper that came out with tritides on May, was an SC&A White Paper. And I'm going to go ahead and turn this over to Joe and the SC&A Team to walk us through that paper.

MR. FITZGERALD: Okay. Thank you, Josie.

Went ahead and did a bit of a chronology which is in the paper, because this has a fair bit of history. And so, the summary at the deliberations piece is the more detailed account, but let me just sort of back up and just go through this a little bit.

This particular piece of the STC

or tritides review really started in July of 2010, almost two years ago. And we had spent time looking at different issues related to exposure potential and what available data there might be.

But at that point I think we had sort of reached a point where after a number of secure sessions and interviews, that I think the Work Group at that time felt it had a fix on the fact that there were support workers that might have been implicated, that there was an exposure potential for those support workers, and that there wasn't a clear pathway for dose-reconstructability. And I think at that particular Work Group meeting that's kind of where it came to.

And at that meeting, I think NIOSH alluded to having acquired about that time a lot of swipe data that I think there was some feeling that that might be applicable useful way to go forward on the question.

And at that point in time, I think

the Work Group wanted to see what would come from that analysis using swipe data as a basis for looking at inhalation using a resuspension factor.

Now, saying that, I think it was pretty clear we - meaning in this case SC&A and NIOSH staff - agreed to disagree on the question of whether that exposure was negligible or not. I mean, I think even two years ago we were having that sort of debate.

There was no question of an exposure potential. I think there was agreement that that potential was established, but the question really was whether that exposure was trivial or not.

And at that point we didn't have any data, but we agreed to disagree on that question.

In any case, what's been proposed, and this is going back, geez, I guess we first saw pieces of an analysis back in October of last year.

We saw pieces of a swipe-based theoretical model that would in fact as we found out in the December analysis, demonstrate that an exposure potential for the support workers based on that analysis was deemed to be very small and equivalent to negligible and that no dose reconstruction would be necessary.

In our analysis, we evaluated the pieces of that review and we had a Work Group meeting. I think it was in November. And we had an initial discussion then.

And at that meeting, I raised some questions on plausibility. Hadn't had really a chance to see the full analysis, but felt that at that very early stage there might be some questions on the overall plausibility of the approach.

That's where we kind of left it, and we did get the full analysis in early January, which was right before the January Work Group meeting.

And that first White Paper which was sort of taking the pieces we saw in October providing the full analysis, is what we now call the extreme case, which is - and I tend to agree.

It tended to take the variables and assumptions and use the - more or less the extreme values. And I think in that case, the resuspension factor is the most influential variable. In that case, the assumed value was fairly extreme.

And we did a review of that particular White Paper. But before our review was completed and before the last Work Group meeting, we got a second White Paper which proposed what I think we call in our review - well, NIOSH does too - the realistic case and used the case study using what was termed more realistic values.

And that was issued in - well, it was written late March, but issued in - we got it in early April. And that was right before

the last Work Group meeting.

So, we chose to withhold the analysis we had done on the first paper to sort of scrutinize and to understand what's happened in the second White Paper and to provide a complete analysis with that second paper in mind. And that's what this analysis is.

I mean, again we started this for the first one, but we augmented it to include the second one and the - an approach which is in that second paper.

And our evaluation in short, and we're going to go through this in some detail, so I just want to summarize, first reviews the adequacy and completeness of the swipe data.

We had told the Work Group at the last meeting that we would start there, look at the question of adequacy and completeness of the data itself again because this was the first time we had actually seen this data that was alluded to back in July of 2009 - or was

it 2010? I'm sorry. 2010.

And we would also look at the assumptions in the same light in terms of the adequacy and the completeness of the assumptions that were included in that model.

The second thing that we looked at, and this is something, you know, I went back and looked at the transcripts that were posted and this is something that we did discuss at the last Work Group meeting.

I mean, it wasn't written down, but I think I went into some detail as to some of the concerns we had relative to the uncertainties that would be associated with using a theoretical model and the variables that are in that model and what the implications might be if one was looking at a use of that model as a go/no-go for dose reconstruction consideration.

And that's the - sort of the source of the sort of two -- I put in quotation marks "policy implications," but

these are just sort of questions that arise above the technical questions which are, you know, given the nature of the model, how do these uncertainties affect that and would that in fact have implications for how it's being applied in this particular case?

And that's kind of what we discussed at the last Work Group meeting, but what's in the paper is really a written rendition of what I had to say at that session, some of the concerns I have in that area.

And that would be, I guess, the going-in summary of where we are today. We wanted to go ahead and try to be as precise as we can about some of the concerns that we expressed verbally in the past two Work Group meetings.

We never quite got to the written word. We're kind of responding to these two White Papers that came up right before the two Work Group meetings.

So, we did want to spend some time trying to as clearly as we could, write down - and some of this is a little nuance, but trying to write down what we thought were some of the implications that the Work Group ought to think about and perhaps query the data from that standpoint.

I think what we'd like to do, Josie, if you're agreeable, is since we did go through a fair amount of analysis, just to translate that and make it a little clearer by walking through that analysis.

The first one was looking at the adequacy and completeness of the swipe model itself and looking at the assumptions themselves.

That review was led by Bob Barton, who's on the phone. And what I'd like to do is just have Bob kind of walk through that as quickly and slowly as anybody wants to. And just to make sure that, you know, that's clear and that the conclusions are -

DR. NETON: This is Jim. I wonder if it might not be better to start off at this higher level, which is the policy implications and just get those on the table first.

Because if those can't be resolved, these little issues that you've identified to being smaller bit players in the whole - I saw nothing in the analysis -

MR. FITZGERALD: Right.

DR. NETON: -- the technical analysis that would preclude us from using the model.

I mean, there were issues about the amount of uncertainty and representatives of some of the samples, but by and large I didn't see anything that said this is technically wrong. I mean, but there are some policy issues about us being able to use the model.

In particular - well, is it okay to start with the -

MR. FITZGERALD: Yes.

DR. NETON: I just want to say a few things about the distinct impression I got from reading SC&A's paper that NIOSH was committed to not including these doses in dose reconstructions.

I think there was a little bit of talking past each other maybe at this last meeting, but I was pretty clear that I thought at the last meeting that anything that would exceed one millirem exposure would, our practice, be included in dose reconstructions.

I don't think that we were ever saying that - I think originally Brant may have started off down that path with this analysis. But it's become pretty clear at least to me and SC&A has demonstrated that for other case scenarios that one can evaluate, the doses can exceed one millirem for the lung. No doubt about it. So, we would propose that this be used to reconstruct doses for people.

Now, the staff at Mound that this

applied to is somewhat limited. It's recognizing that there's an SEC prior to 1980 in the tritium building where these exposures occurred. So, all those folks are already in the SEC.

This would only be applied prior to 1980 to those people who had non-presumptive cancers. In particular, the tritide exposures would only affect people with lung cancers.

CHAIR BEACH: Did you say prior to 1980?

DR. NETON: Right. Because we have an SEC up to 1980 for the SW building.

CHAIR BEACH: We have some time period between '80 and later years.

DR. NETON: Yes. Well, first I'm just trying to triage this a little bit and say -

CHAIR BEACH: Okay.

DR. NETON: - prior to 1980 these exposures are - people who have these types of

exposures are in the SEC.

There is the issue of the remaining non-presumptive cancers. But the way the tritide model was developed for this time period, re-suspended hafnium tritide in the air, assuming it was a hundred percent hafnium tritide and had people inhaled that amount of hafnium tritide, that would only be maximized where people have lung cancers. Because it would be - it would deliver a higher dose if they were to use their regular tritium bioassay, because then it would immediately go to the affected organs rather than being held up in the lungs and then slowly dissolve into the system.

So, prior to 1980 it only affects lung cancers. After 1980, it really only affects lung cancers, period. And it only affects people who worked in the SW building.

So, it does - it's a limited population of workers, but we would assign the doses derived from this model to those

workers.

So, I just want to make that clear that it's not an issue with us whether there's a de minimis dose here that wouldn't be included.

MR. FITZGERALD: Well, the reason we raise this and we did kind of make, you know, opened it up for revisiting it at this meeting because we weren't sure even though --

DR. NETON: Right.

MR. FITZGERALD: -- at the last two Work Group meetings we came back and expressly asked that question because, you know, again that's what we heard, but we wanted to make it clear in the answer. And that's why we used the quotes in there.

DR. NETON: Yes.

MR. FITZGERALD: The answer was that, you know, that these were essentially negligible, but what you're saying now is that's not the intent.

DR. NETON: Right.

MR. FITZGERALD: Okay.

MEMBER CLAWSON: Help me to understand.

DR. NETON: So, I wanted to make sure we got that clarified before we proceed.

MEMBER CLAWSON: Because my understanding was that you did this, that NIOSH did this test and that the reports came back and that they were negligible, but were not going to do dose reconstruction, that it wasn't needed.

DR. NETON: Well, it -

MEMBER CLAWSON: And that's why we went off and did this whole evaluation of what the uncertainty of it was and everything else was.

DR. NETON: I think that it was certainly -- the way it was originally drafted was an attempt to demonstrate that the doses were very small.

And in fact in a particular case example that was cited, it was -- I think that

the dose was less than a millirem or something like that and said, well, if they were that small, maybe we wouldn't worry about them.

But then it is clear I said that at the last meeting that if anything over a millirem would have to be included in a dose reconstruction, we cannot leave things on the table like that.

And I can understand the confusion on this issue. But our position as of today, you know, I think my position as of the last meeting, maybe it wasn't very clear, was that we would include this in dose reconstruction.

MEMBER CLAWSON: Okay, this is Brad again.

So, each one of these dose reconstructions are going to come in and you're going to do a test to them to see if they're going to get this dose or not.

This is kind of interesting, because I've never seen - I've never seen where we test the dose reconstruction first

and then see if they get a dose or not. And to me, you have to run this test to see if they get it or not.

DR. NETON: Well, that's not true. We do that all the time, Brad. We always will run the gamut of the scenarios that are out there that are plausible, which there may be some debate on this, but all plausible scenarios and pick the dose that provides the highest dose to the cancer that we're evaluating.

So in this particular case, in my opinion, for non - for support workers, we have bioassay on these people because they were all bioassayed when they went in the SW building, we evaluate the HTO dose, tritiated water dose, they're on bioassay. And also if they have a lung cancer, though, then we would do a tritide, a hafnium tritide dose because the water inhalation is typically going to be higher than a hafnium tritide.

Because what happens, the hafnium

tritide holds up in the lung, sits there, it irradiates the lung a lot longer, and then the tritium slowly bleeds off into the other organs.

So, you're better off getting a more soluble intake.

MR. FITZGERALD: I think the reason there's a little confusion is the model, you know, even the written White Paper expresses the approach as one to evaluate exposure potential versus an actual dose reconstruction method.

And that was surprising at one of the Work Group meetings. And we went back and said, you know, are we hearing that right? Because I think the Work Group had requested back in 2010, you know, to get a dose reconstruction approach and this seemed to be something a little different than that.

And that's why we are very carefully through the last two Work Group meetings, trying to clarify more than anything

else what exactly are we looking at.

And it was configured to be one that evaluated the dose from the standpoint of - and I even asked you that question as to, you know, is this a dose reconstruction method?

And the answer was, no, this was really one that would - I don't know whether the word would be "test," but this would be actually looking at whether it was a trivial or not dose. And the conclusion was as it turns out, it was a trivial dose.

So, this is definitely different than what's been portrayed in the two White Papers and the past discussions not to say that, you know, we're at a different place, but I'm just saying that's why we were expressing some concerns about that.

DR. NETON: I clearly said that anything more than one millirem would have to be included in the dose reconstruction. I know I said that.

MR. FITZGERALD: Oh, you did say that, but I'm -

DR. NETON: I thought maybe -

MR. FITZGERALD: We were trying to reconcile that with the context of what was presented before that.

DR. NETON: I looked in the executive summary of the tritide paper -

MR. FITZGERALD: Yes.

DR. NETON: - and the final sentence says the assessment demonstrates the exposures and inhalation of insoluble metal tritide at Mound were small, plausible and bounding.

Doesn't say not required to be included or anything. It would be the implication if all cases were that way, but it's true as SC&A has pointed out, that they're not all below a millirem.

That particular case study was, but there are numbers of other scenarios that one can come up with to put it over the

millirem -

MR. FITZGERALD: Well, again I think the - and not to beat this thing, but certainly the dialog over the last couple of Work Group meetings, and I've gone through transcripts and everything, I mean, clearly we were concerned about that interpretation and went back a couple, two or three times to clarify it.

And am I the only one - I think the Work Group felt that that was what we were hearing.

Now, saying that -

MEMBER ZIEMER: Well, can I just - while you're on that topic, just interrupt just for a moment if I might.

I think there was some confusion on the basis for the millirem value. And it came up again I think maybe in your paper, Joe, where you indicated you had gone back - there was some implication that IREP didn't handle anything below a millirem.

Was it you that - or was it NIOSH?

MR. FITZGERALD: No, wait, I think -

MEMBER ZIEMER: Said, no, they went back and it does. It will handle smaller doses.

MR. FITZGERALD: Yes.

DR. NETON: Yes, that's another issue.

(Simultaneous speaking.)

DR. NETON: But nonetheless, we would -

MEMBER ZIEMER: The millirem is not a magic number in any event. You're going to include it.

DR. NETON: Yeah, we'll -

MEMBER ZIEMER: I mean, do you -

DR. NETON: There are proximal implications for what one includes in dose reconstruction.

MEMBER ZIEMER: Right.

DR. NETON: For example, if you

have an environmental exposure that the first-year exposure gives you five millirem and then say IREP will calculate or IMBA will calculate doses ten to the fifth, ten to the sixth, ten to the eighth, ten to the ninth millirem out 30 years.

MEMBER ZIEMER: Right.

DR. NETON: And it's very unwieldy to keep including those type of doses. So, there's some practical limitations on what we include in --

MR. FITZGERALD: Right. And then this came up because --and Brant was sitting right there and he said, you know, categorically the doses to the support workers were not significant. And, therefore, there didn't need to be dose reconstruction.

I said, well, what do you mean by - how do you - what's significant? And that's when he threw the ball back at you and we were trying to figure out, you know, is there a definition of significant. And that's where

the one millirem came up as sort of a de facto, this is kind of a benchmark for what's considered significant.

And then that got into this discussion, well, where does the one millirem come from? And that's where we were talking about IREP and I - we, you know, as I said, I - you're right. That's not the important issue, but we kept hearing that sort of categorically that the doses to support workers - again, two years ago I think Brant was pretty clear that these were negligible. And that was pretty much the mantra all the way through this -

DR. NETON: I don't know. I mean, the first few analyses that you alluded to back in 2010 were coming up with doses that were something in the order of hundreds of millirem, if I recall.

MR. FITZGERALD: No, no.

DR. NETON: And they became more and more refined. As they became more - as

the process evolved and more and more data became available, it became more refined. They started to drop as you took out some of these very large overestimates.

MR. FITZGERALD: Now, the chronologies - and back in 2010, we didn't really have any numbers. What we had was data for the ten operators who we knew by name.

DR. NETON: Yeah.

MR. FITZGERALD: And at that point, you know, we had a number of renditions where we expressed some concern that there was more than ten people. That in fact these ten operators had to be supported by support workers, you know, maintenance people, HP techs and that kind of thing.

And we established that was in fact the case and that there was exposure potential based on the interviews with Mound workers. The Work Group was part of that discussion.

And at that point back in July of

2010, there was agreement, actually. I think we did agree that there was an exposure potential.

MR. KATZ: Oh, I can hear it. Someone is talking on the line, having a conversation at their own location.

Will you please mute your phone? Press *6 if you don't have a mute button, and that will mute your phone.

Someone who is talking right now. There is a woman talking right now. Please, if you're on this line, you shouldn't be talking on an open mic.

So, please mute your phone. Press *6 so the rest of us can hear each other. Thank you.

MR. FITZGERALD: Yes, let me just finish. So, in that July's meeting, we got to that point where we acknowledged that there was an exposure potential to the support workers. And, again, I think, however, the difference was Brant at that time felt that

that dose would be negligible and we felt that in fact that exposure potential is something that should be considered for dose reconstruction.

And the source of that difference, and I don't want to put too much on this, was interviews with people familiar with the program. And, you know, we were getting into these intermittent glove-box failure which you tend to have when you're dealing with tritium.

And we talked to these folks and said, you know, when you're handling in these tritide operations, did you have the kind of glove-box failures you tend to have in tritium operations? And they said, yeah, of course.

And would the tritides figure in some release scenario based on that? And they said, yes, but, you know, it would be understandably small.

So, you know, that's all we had. Literally, that's all we had. So, we interpreted that to say, well, there's an

exposure potential that needs to be reviewed and looked at.

And I think at that point, Brant felt that even though there's an exposure potential, it would be negligible and something that would not be of concern from the programmatic standpoint.

And we went from there, Jim, and we got to this first December 2011 White Paper and that was the so-called extreme case.

DR. NETON: Right. That was the one I thought that was in the hundred millirem, 200 millirem -

MR. FITZGERALD: That got - yeah, that got to a couple hundred millirem.

DR. NETON: Right.

MR. FITZGERALD: And that was the first time actually there was a number attached to it.

DR. NETON: Right.

MR. FITZGERALD: And, you know -

DR. NETON: I don't think at that

time we were indicating that that wouldn't be included in dose reconstruction.

MR. FITZGERALD: No, it was called very small. But, again, if you go back to that particular meeting, which was January 5th, and we went into that issue as to - I think I've got the citations here, but we went into that issue talking about the significance and the question of whether or not this was a dose reconstruction method, or whether in fact it was just simply to look at exposure potential.

DR. NETON: Well, I think we're getting caught up in the difference between saying we can demonstrate that we can bound things versus do we have a refined dose reconstruction methodology.

Those things sort of always kind of go hand in hand. Just because you can say you put an upper limit on something, eventually you have to come to some way to apply that to the cases.

And in fact, I think this next iteration is exactly that, the one that came out a few months ago.

CHAIR BEACH: So, you're talking the March 30th, 2012, that paper?

DR. NETON: The most -

MR. FITZGERALD: The most recent iteration.

CHAIR BEACH: That's the most -

DR. NETON: Well, you know, as they became more and more refined, the doses went down and down and became smaller and smaller.

Brant's position was it became manageably small and I can understand that he was indicating that they were probably so small they wouldn't need to be included in dose reconstruction. It's clear to me that the dose is past some threshold where you'd have to include it.

So, what I'm saying here today, which I guess is probably the most important thing, is that we would include these in dose

reconstructions using this methodology.

DR. MAURO: This is John. I agree with Jim in terms of this is a clarification that we really needed because we weren't quite sure as Joe had pointed out, whether the case was being made that it's negligible, or the case is being made, no, we have a coworker model now that can be used to place a plausible upper bound.

And I think that we, you know, in our perspective, this clarification allows us now to focus in on the assumptions, the model, the approach that you have adopted and the degree to which you have sufficient data, swipe data, and that you selected are resuspension factors and other parameters that do represent a way to come at the problem and assign a plausible upper bound to some groups of workers that might have been exposed.

So, I think this is important. I'm very glad you brought that up, Jim, because we were not - quite frankly we were

operating on the premise that this was not being offered up as a way to do dose reconstruction.

MR. STIVER: This is John Stiver. I second what John just said.

I had asked Brant directly at the last Subcommittee meeting whether this was indeed going to be used as a coworker model. And I didn't get the point because this more realistic or not quite as bounding set of parameters that were chosen yielded doses that were, in his opinion, vanishingly small. He thought that it would be probably better just as a demonstration than just whether you'd need to be reconstructed.

But I think we're kind of incrementally getting to a point where we can see that indeed resuspension factor, the degree of solubility, the effectiveness of reasonable process and things of that nature. Oh, and uncertainties that were going to drive the range of plausible doses over a

millirem.

So, it does need to be reconstructed and Jim is presenting that now. I think that's a great -

DR. NETON: And the only thing I'd like to point out in addition to this unless I'm missing something here, is this would only be applied - this technique would only be applied to lung cancers.

We have bioassay data, tritium bioassay data for everyone else. And I believe that those would end up being higher to organs that are nonrespiratory tract organs using the tritium water model.

MEMBER ZIEMER: So, just to clarify, so pre-'80 if you have someone who doesn't qualify for the SEC in terms of the 250 days or whatever, if they have lung cancer you would use the tritide model. If they have another cancer, you'd use the tritium bioassay as -

DR. NETON: Everyone was required

to submit that worked in the SW building.

MEMBER ZIEMER: Right. If it's after '80, it would only be used for lung cancer cases.

DR. NETON: Well, we would run both -

(Simultaneous speaking.)

DR. NETON: The maximum dose would be for lung cancer cases. You would probably end up with a higher dose using the regular tritium model.

MEMBER ZIEMER: But that would be checked at least.

DR. NETON: Yes, we would check it.

MR. FITZGERALD: Yes, I was wondering wouldn't you just run it and see - you're just saying that -

DR. NETON: We could do both, but it seems to me that if you have -

MR. FITZGERALD: If you validated it, you -

MEMBER ZIEMER: It's likely only

lung cancer, but you would still run -

MR. FITZGERALD: You would still run it for everything just to make sure.

DR. NETON: Right. Because we may actually have higher tritium HTO intakes beyond the resuspension that occurred from the material on the ground.

I mean, so we would take the bioassay data and run it as if it were HTO.

MR. FITZGERALD: And the DR approach, I mean, let's call it a DR approach now since it's clearly not an exposure potential analysis, is the model as it's written. I mean, it's -

DR. NETON: Well, there's still some -- it is subject to debate about whether the 50th or the 95th percentile would be used. That's always open for discussion.

We tend to use the 95th percentile in these cases, because there's a lot of other uncertainty that you - SC&A has well pointed out.

And if we use this fixed resuspension factor, this approach would be totally consistent with what we've done in many other places and particularly residual contamination periods there. Resuspend the material, pick the 95th percentile value of the contaminant and assume that that is re-suspended in that concentration for every hour of every day that these people work. And I think it sort of accounts for some of the other uncertainties that are in there.

The alternative would be to run it as a full-fledged distribution of values, you know, picking a distribution about the resuspension factor, distribution about the concentration using the 50th percentile of that and run it through in that way.

MR. FITZGERALD: And this would be for all workers that -

DR. NETON: All workers that had -

MR. FITZGERALD: -- had tritium bioassay.

DR. NETON: -- tritium bioassay, correct.

CHAIR BEACH: So, if you look at Joe's point, the second point, the use of the conceptional model for which site-specific and empirical values of the SECs are lacking, so basically you lack site-specific parameters and there's still too many variables that I can see.

DR. NETON: Well, I mean, we have site-specific data. There are smears taken in all the rooms by year.

The resuspension factor is not necessarily site-specific, but this is exactly how we model residual contamination. This is a TIB-70-type approach that SC&A has reviewed and has not said is invalid.

CHAIR BEACH: Well, that's a technical discussion. I think Bob Barton is going to -

DR. NETON: Right. But what I'm saying is to say that the approach is not

valid, I would say that we've used this many times in the past and I don't know why it wouldn't be valid here to resuspend material into the air.

MR. FITZGERALD: Let me clarify. I think - in fact, we actually say this in the review, and I think Bob will second this in his more detailed discussion, is that we don't fault the analysis or the model itself.

DR. NETON: Yes, when I read that, I thought I was done reading.

(Laughter.)

MR. FITZGERALD: Right. No, no. The model itself is not -

(Simultaneous speaking.)

MR. FITZGERALD: I think it has a lot of history and all the rest of it. Clearly we're more concerned and have been from Day 1, on tritides. In fact, the uncertainties - this is a subjective thing, again.

And I think you said in one of the

Work Group meetings that applying models like this, it's not unheard to actually reflect the uncertainties - uncertainty ranges on some of these things.

That is where, you know, we had two concerns. And I think you satisfied the first one in your clarification.

But the second one is that when a theoretical model - and again this is - it's hardly one or the other. I mean, this does have some site-specific information and does have the tritium even though we don't know how much of the tritide is in the tritium.

It was done in the locations where the operations took place. So, you know, that could be considered site-specific.

On the other hand, we don't have the actual monitoring data per se for the tritides. And one has to make assumptions about all that, which is what we're talking about in the model. And we're just more concerned about the uncertainties that are

embedded in the model.

And this is a conversation that the full Board has had a number of times on models as to whether, you know, the uncertainties and the basis of the model in actual either empirical or site-specific data is sufficient to give one confidence in the application of that model in dose reconstruction.

I'll tell you that's not something that SC&A can offer. That's a study judgment call that the Board has to make on any model that's advanced like this. And it's not different than maybe the radon discussion at Blockson or some of the other models that have been considered.

It's a judgment call as to whether the uncertainties are acceptable or not acceptable, whether the site-specific roots of the model, the empirical basis of the model is sufficient.

And I, like I said, I think we

just wanted to present all the facts that we could in terms of the uncertainty issues and whatnot. And I think it's the Board that has to decide whether it in its judgement, has enough confidence that the model would support dose reconstruction with sufficient accuracy. And I think that's a judgment call.

I mean, I've been listening to the debate on the models in the past and I don't know what you can say about it.

DR. NETON: I'd say a couple things about this. It's not as unique, I think, as SC&A tends to think it is.

You think what happened here - the active use of the tritide compounds has stopped by this time.

So, what we're having here is essentially a classic period of there's no active airborne generators of tritium compounds during this period.

MR. FITZGERALD: I can't speak specifically to the time frame -

DR. NETON: I understand. So, if there are no active source generators going on, then you have a resuspension problem just like we have in many other sites.

The only way this model works and if that's true - now, if there's other issues that come out that it might be --

MR. FITZGERALD: The Work Group is familiar with issues that date past 1980 that would -

DR. NETON: Well, that's --

MR. FITZGERALD: - undercut that.

DR. NETON: - in the D&D era, I think, maybe.

MR. FITZGERALD: This is not D&D.

CHAIR BEACH: No, it's not D&D.

DR. NETON: Okay. Well, up until let's say - right now the model works if it's a resuspension because there's no active generators of material. So, you have a resuspension problem just like you have at many other sites. We have smear data. We

have re-suspended it.

The doses in resuspension periods tend to be very small because you're re-suspending a very small fraction of what's on the surface.

By nature of reconstructing small dosimetric quantities, the uncertainty goes large because any time you have a small dose, the uncertainty value as far as that, that's a given.

But we feel that it is small and is bounded by this approach. So, I'm not sure why there would be an issue with it. But I agree, you know, the Board certainly can weigh in on that, but I -

MR. FITZGERALD: Well, and I don't disagree with what you said. I think the model - I mean, this approach has been done before and it is - we weren't saying it wasn't relatively common.

I think what we're saying is that this deliberation by the Board on whether a

model's inherent uncertainties and its roots and site-specific data are adequate enough to support dose reconstruction, that part of it I think does happen and would need to happen on this one in the Work Group, but there's two issues.

Really, the first issue is obviously the operational status, this question that we can't really get into in detail, but the Board - Members of this Work Group are pretty familiar with that postdate 1980 in terms of generation.

The second issue is again because of the nature of the beast, this hafnium tritide, the - and we've had this discussion in the past. The source term can't - we don't have specific source term data. We do have the tritium data.

But I think again from the standpoint of the uncertainties that pushes you into, a judgment has to be made as to whether those uncertainties would be

acceptable or not given the uncertainty.

Now, not the mechanistic ones of resuspension, but just the ones where you're going to have to conclude particle size, you're going to have to conclude the, you know, in this case you're going to have to conclude a hundred percent tritide.

But the other issues that come into the uncertainties that we've laid out that there are a lot of uncertainties when you're dealing with theoretical model that has to be theoretical, because there isn't a whole lot of hard edges to it because of the nature of the analysis.

DR. MAURO: Joe and Jim, this is John again. Jim just said something that was very important to me in looking at the model that they're offering.

And that is I was always concerned that the resuspension model would be used at a time period when a person might be being exposed to both re-suspended material, but

also direct airborne contamination from leakage during operations.

And I just heard something that answered a very important question to me. And that is this resuspension model would only be used during time periods when the only way in which a person could be exposed to metal tritides is from resuspension and not from direct leakage.

That was, quite frankly, when I was reviewing the resuspension factor issue, you may have seen it, that - I was concerned that if you have direct exposure from leakage, the resuspension model is not going to necessarily do the trick for you.

So, I want to make sure that's confirmed here. I know this is a subject that was not directly addressed.

In fact, I remember asking Brant that question at the last meeting and they really for a variety of reasons, it was left ambiguous.

But it sounds like that their - Jim, your position now is that this resuspension model that you're offering up would only be used for people who might have been exposed to material that was literally re-suspended as opposed to direct injection.

DR. NETON: Yeah, I mean, I see no other way it is valid.

DR. MAURO: I agree with that and thank you. That's clarification Number 2. In my mind, that was really fundamental to everything we're talking about.

MR. FITZGERALD: Well, you know, I don't, you know, starting with - I'm beginning to agree with your premise of talking about this first.

I think this sort of leaves us with the question of a dose reconstruction method that save a decision on maybe a distribution, which is what you're saying, and some resolution of this generating - source of generation issue which -

DR. NETON: This is a new issue to me. So, I haven't been privy to what the -

MR. FITZGERALD: Right. Again, it's just difficult to talk about, but that actually was the whole source of the last year's worth of analysis of data if you look at the data, because the fabrication period was well before that.

DR. NETON: Yes.

MR. FITZGERALD: So, that's not an issue.

DR. NETON: Right.

MR. FITZGERALD: But the reason we're even talking about it in this context and not just D&D, is because of that issue.

So, that certainly is a question which we have basically done all we can with, actually. There isn't much more we can do with that one.

DR. NETON: Well, let me ask - I don't know if you can answer this or not, but is it safe to assume that up to 1980 this

would be valid, this technique?

I don't want to say "valid." There's no reason to assume that there's airborne generators -

MR. FITZGERALD: Well, there is -

DR. NETON: -- other than is an SEC already. And so, we're taking care of -

MR. FITZGERALD: Well, no, the problem is that you do have generators before '80. So, you couldn't apply the method.

DR. NETON: Well -

CHAIR BEACH: You mean after '80.

MR. FITZGERALD: Right.

DR. NETON: But everybody is in the SEC before '80 primarily.

MR. FITZGERALD: But the method for those who are not if it's -

DR. NETON: Right.

MR. FITZGERALD: The lung would not work for the generator because -

DR. NETON: Well, and then that had been - I know Brant's opinion and I have no

reason to doubt it, but he knows who were physically working with these materials in that time frame.

We would assume that their urine analysis would be based on tritide exposures and then -

CHAIR BEACH: But, Jim, isn't it true -

DR. NETON: -- maintenance workers would get the re-suspended -

CHAIR BEACH: Oh, I was going to say we couldn't identify the maintenance -

DR. NETON: All the ancillary workers would receive this.

MR. FITZGERALD: Right. And I would agree with that. They would be -

DR. NETON: So, through 1980 it seems like it's okay. I'm not - unless I'm -

MR. FITZGERALD: No, I think that's -

DR. NETON: I can't address the - what you brought up about after 1980, because

MR. FITZGERALD: That was the subject of many a trip to OST, Brant and I. So, that took a while to establish and there is a real - well, there actually was agreement on it, but we added rooms. Originally there was two rooms, and now there's four. And that's the reason there's four.

DR. NETON: Okay.

MR. FITZGERALD: So, yeah, that's an issue. And certainly that would be probably the - one of the bigger questions, technical questions - or one of the bigger questions that have to be resolved.

DR. NETON: Would that same situation apply if we knew the workers in that time frame - well, establish that they were the ones that get the high dose, then the same resuspension factors would apply to those workers, would that not work. Not knowing the circumstances of what you're talking about.

MR. FITZGERALD: Well, that's the

question we'd have to answer. Now, you know, are the personnel the same, or not?

DR. NETON: Do we know the personnel?

MR. FITZGERALD: Do we even know the personnel? But that would be the question as to whether you can make that bifurcation and apply it that way.

And you're right. Do you know the personnel for the second as opposed to the first?

MEMBER ZIEMER: You're talking about '80?

MR. FITZGERALD: Right.

MEMBER ZIEMER: That's what you're asking?

MR. FITZGERALD: Right.

MEMBER CLAWSON: I thought that we just got into that and Brant felt he had a good handle on it, and it fell apart.

MR. FITZGERALD: More on the support workers. I mean, knowing the

operators by name was clear-cut, but the it's like an iceberg.

Knowing all the workers who reported those glove-box operations, that wasn't as clear. That's why we're sort of into this you can't really distinguish who that population might be.

MEMBER CLAWSON: Right. That's what I want to make sure because we have never been able to do that. I mean, we have people come in that we changed out the glass in this, we changed out fans in this. It was an ongoing thing. It wasn't just cut and dry ten people.

MR. FITZGERALD: Yeah, we've actually sat in interviews and got to about 20 names because the operators could remember who supported them.

But at that point, you know, it's hard to figure out, you know, you've been in facilities. It's hard to figure out who actually all these folks are. There's a lot

of them.

So, but, no, I agree. I think if you had analogous to the pre-'80, you actually could identify operators versus others, then I don't see why you couldn't apply the same approach.

But of course then the overriding question would be treating -- or again the acceptability of the model from the standpoint of uncertainties and site-specific data again which, you know, beyond the mechanistic part, beyond this part is poor judgment. So, that's how I would sum it up.

CHAIR BEACH: Right. And I know we'll get into this later, but I know there's a lot of the swipe data that's missing in several years during that time period as well.

MR. FITZGERALD: I think Bob can go into that, but that's all - well, that gets into a question of whether you can extrapolate, but I think NIOSH does that quite often.

I think what Bob was pointing out, and he mentioned it at the last Work Group meeting, is that all these were two-month span of samples. And in translating that to annual dose estimates, that multiplier wasn't used.

So, he went ahead and came up with some really nice tables. He went ahead and made the adjustment.

So, I think that - is John on the phone? That's tractable. That can be adjustable. I don't see an issue there.

I think it really comes down to this one question of whether you can make it work post-'80. Another question as to whether or not the uncertainties can be - if that's satisfactory to the Board as a model.

And then, you know, we're left with this D&D issue, which quite frankly, you know, that was a new wrinkle. We had interviews that seem to suggest that there were tritide issues in the actual terminal cleanup of Mound.

I think Brant had done additional interviews which - that seem to come up with a different answer from some of the same people. So, we didn't have time to look at that, but that would be another question.

I think - I'm not sure the model would work for D&D per se. Although, I guess I'd have to think about that. It would be a different kind of -

DR. NETON: It would be harder to justify, but I got the impression from reading the earlier report that NIOSH put out that in the D&D era they had adopted a very different way of monitoring for tritides.

In other words, they had a filter sample, a BZ sample that they were going to analyze with a scintillation counter, as well as looking at the gaseous form.

MR. FITZGERALD: Yes, and it may turn out that I've got to look at the timing, you know, the entire complex got alerted to tritides about the time that Mound was getting

through D&D. So, I don't know if that might have led to compensatory steps or something where that the exposure potential would have been pretty controlled.

DR. NETON: Right. You have to match up when the D&D activities actually occurred versus when they instituted these new protocols for tritide monitoring.

MR. FITZGERALD: But that, to me, is a different issue than whether or not the model would work as - in terms of implementation. So, that's more of a question -

MR. STIVER: This is Stiver. I remember now that basically the D&D activities were going on in the post-835 environment. And there was as Jim alluded to, a different technique employed they also used scanning electron micron to identify particulates.

And so, they had a technique by which they were able to identify the materials.

MR. FITZGERALD: It's just a matter of timing. I think Mound precipitated the attention. So, it may very well have been that the D&D was controlled from the get-go, because there was concern going into D&D that this would be -

MR. STIVER: But, I mean, the question is whether this type of a model would be applicable or -

DR. NETON: We certainly would use it if we had the type of data that I - it sounds like they collected -

MR. STIVER: Fill that gap --

DR. NETON: Any time you have a D&D and try to estimate resuspension factors -

MEMBER CLAWSON: This is Brad talking again. I remember something else about the D&D period.

Everybody wasn't tested for it. They took the stance of one out of 20 would have a BZ sample and then that was it.

So, you know, that's a whole other

- that's getting into -

MR. STIVER: You're getting into representativeness and data adequacy -

MEMBER CLAWSON: And this was brought out in many of the interviews and many of the people discussed that it was off, but I want to step back just a second.

So, we have a path forward. We actually have a dose reconstruction method that is going to be applied. I've been going for two years here and understanding that we have one, but it was more of a - just a general - so, the approach that you put out now is what NIOSH is standing on for a dose reconstruction for people.

There's no half a millirem limit?

DR. NETON: Sorry to confuse the issue.

MEMBER CLAWSON: No, no.

DR. NETON: I believe this would be the best -

MEMBER CLAWSON: You've got to

understand we've been dealing - we've been battling this back and forth. And even started out with slight data and this is how we're going to get here, but it doesn't really matter because it's negligible and we're back and forth.

And I personally coming into this today, did not think that we had a representative path forward with the dose reconstruction for it. And I guess I just want to make sure that that's clear that we have -

MR. FITZGERALD: And you have really, you know, it wasn't wasted effort, the analysis on the method, you know.

The only difference is I think some decision on the dose distribution guide 50th or 95th, but essentially the model is the same model that's reviewed in the paper.

MEMBER CLAWSON: Right.

MR. FITZGERALD: So, you're equipped to evaluate the model as a dose

reconstruction model, not as something else.

And as we were just saying, the only question really gets down to the adequacy and completeness of that model which is also analyzed in here.

And of course the remaining concern that we've expressed on uncertainties, but I think you've already heard about that. It's not the mechanistic. The actual model itself, the mechanistic approach is fine. It's been used in resuspension factors. That's all been pretty standard.

It's whether or not it's grounded enough, and that's a judgment call that I don't know how to say it.

It's just that you have to decide from a site-specific and uncertainty standpoint whether it's that famous sufficiently accurate or not to be used in dose reconstruction. And that's a Board call.

We, I think, pretty much have laid it out in probably excruciating detail as far

as what the uncertainties might be and what is the significance. They're all there.

DR. NETON: I don't have time to review all --

MR. FITZGERALD: Right, right. So, it's like - I don't know. And we can go through that as we proceed, but there's not much more that can be said. You have pretty much our full assessment of what those uncertainties are.

Some of the concerns over, you know, site specificity, which is kind of a term of art almost, but just what we consider some of the site specificity issues.

MEMBER CLAWSON: And this is why I bring it up because - and this is Brad again. I'm sorry.

The thing is as we came into this, I was looking at that as more of a test of the test's validity or -

MR. STIVER: Whether you need to reconstruct, basically.

MEMBER CLAWSON: Yes. And I have been on that premise for almost two years because it was kind of put forth to us this way. And now these gaps in the analysis, the way we look at this is a little bit more meaningful to me.

MR. FITZGERALD: And we say it's subjective, you know. I think, Paul, we were taking about trying to come up with some analogy. We're talking about the high-fired plutonium at Rocky because there's, you know, certainly the solubility question seems to be pretty parallel.

But there and again it sort of goes back to not necessarily the method as opposed to whether that method is grounded in either empirical data, in that case it's autopsy data, or grounded in site-specific information.

Of course Rocky had quite a bit of plutonium bioassay for both - for all workers. It was fence line to fence line practically.

So, I don't think that's the case here, but it's a matter of degree. So, it's not sort of saying white or black. It's just saying that the degree of supporting information and the uncertainty range is, I think, relatively higher for this one versus for the high-fired plutonium.

MEMBER ZIEMER: And I think it's important to realize that uncertainties per se don't dictate sufficient accuracy conceptually because general premise the bigger those uncertainties are, the more claimant favorable your decision is because it spreads that distribution out.

If you've got a 95th percentile, I would venture to say and I've done these exercises in class with students, the tighter your uncertainties are, the smaller - the lower the dose assigned is at the 95th percentile.

Sufficient accuracy means that you've bounded well enough to make any correct

decision on a claimant.

Usually sufficient - or uncertainties help the claimant. There may be an exception to that. I have not seen it yet.

Assuming you have a reasonable model, a model which is plausible which is important, it's got to be a plausible model, and certainly if you have site-specific data that that's built on, that helps you.

If you don't have that, then you're into other things like surrogates and so on. But I think it's important that we not think that uncertainties as they get bigger at a given site, tend to hurt sufficient accuracy decisions. The accuracy doesn't have to do with getting an exact dose. It has to do with getting a good decision.

MR. FITZGERALD: The only thing I would add is that what sticks in my mind is the famous stratification - radon stratification debate which was filled with uncertainties in terms of where radon would go

in a building.

And I think the representativeness of the model to a real situation, I sat through the debate I said, you know, I thought I knew how uncertainty would play in the model. Now, I have to assume that, yes, I think that's kind of a judgment call.

MEMBER ZIEMER: It is a judgment call. I think the model is pretty good, but the -

MR. STIVER: I think Paul hit it right on the - the crux of the problem here is that we're looking at - we're kind of defining "uncertainty" in different ways.

I mean, this is a classic definition of the uncertainty of the parameters that give rise to the distribution results.

MEMBER ZIEMER: Right, the definition of "uncertainty," yeah.

MR. STIVER: But what we're looking at here is just uncertainty and assumptions,

because we don't have site-specific data.

So, we have this one assumption, the percent of STCs ranges from zero -

(Simultaneous speaking.)

MR. STIVER: So, we have no way to benchmark this model that on the surface it appears to be a good model. We have reasonable parameter values drawn from the scientific literature, but you just don't have that link back to any kind of site-specific information where you can benchmark it.

DR. NETON: But the zero to 100 percent, I mean, SC&A has alluded in there that they believe that there was significant potential for tritide exposure in the workplace.

I mean, I don't know how you interpret significant, but to me that could mean as high as a hundred percent. It could be a spot, you know. We don't know.

MR. STIVER: This becomes a -

DR. NETON: I don't know. And

everything that one has to consider and I know it's hard to get your head around it, but these are small doses.

The uncertainty and the stratification for the radon was because we didn't know what the uncertainty was. We couldn't put a bound on, you know, I tried. I tried to say, okay, how stratified could it be?

Here I think you can bound the uncertainties because it's no more than a hundred percent, and the uncertainty in the resuspension factor can be easily quantified.

So, you've got an ability to put upper caps on these things that make some -

MR. FITZGERALD: I might add it was actually SC&A that enhanced that radon model.

(Laughter.)

MR. FITZGERALD: So, I'm not saying that -

CHAIR BEACH: Well, and I don't think that dose - how small the dose is really

matters, but it's how you're going to do the dose reconstruction.

And until today, we did not know that. We were left at the last meeting with that this was not a dose reconstruction. So, that does clear that up.

MR. STIVER: The magnitude of the dose isn't at issue. It's whether it's reconstructable and -

CHAIR BEACH: Exactly.

DR. NETON: What I'm saying, though, as the magnitude of the dose goes down, the uncertainty goes up. It's an inherent nature of reconstructing small doses.

CHAIR BEACH: Okay. So, are we ready to hear from Bob?

MR. FITZGERALD: Yes, I think that certainly -

MEMBER CLAWSON: About five minutes ago I started in onto this because Paul made a comment, and I agree with him on it, that when he was speaking that this - just because we

get uncertainties and there that it basically comes back to the basis and that is with site data, that all of a sudden we're coming in and we don't have good site data.

And then we're putting uncertainties on that and we're adding to this, you know, half of nothing is still nothing.

And this is - this is one of the things that I want to point out because personally looking at their data, they haven't got much, in my eyes.

DR. NETON: There are 60,000 swipes.

MEMBER CLAWSON: What's that?

DR. NETON: There are 60,000 swipes.

MEMBER CLAWSON: 60,000 swipes, but there's also very large gaps in it. The process that was going on with it there was questions in that there we start getting into uncertainties on that.

And this just compiles the issue and this is, you know, this is where we started off two years ago is to be able to with this swipe data and be able to look at this.

I do not disagree that when we have uncertainties that it makes the doses bigger or whatever else like that, but it is compounded by when we don't have good data to be able to track it.

If you go -- looking at it from just this, this is fine. But when we go clear back to the site and go through the process and there's holes and gaps, it makes it much harder to be able to do.

We have a hundred percent I'd agree with you, but we're not working in a classroom setting to where we can put this up there. This is a dose reconstruct - this is a compensation act for people.

When we don't have the data there, in my eyes, they set up an operation for us to

be able to take care of that and sometimes we really go a long ways.

DR. MAURO: Brad, this is John. I'd like to second - I think you are now moving into the - we've sort of set the framework with the problem now very nicely in terms of we know there's a coworker model in front of us and it's to be used for workers only exposed to resuspension. That was a very important boundary.

Now, we're in that world and I think you brought up the first and one of the most important questions. Does the data that we - the swipe data that's out there, does it capture the full range of exposure scenarios?

And what I'm hearing is that there might be some question whether that data is complete. Do we have enough data representing all scenarios and circumstances so that we have a degree of certainty, assurance, that we're not going to underestimate the dose to any particular worker?

And I think we're actually now into the substance of do we have sufficient data. And the first data point, and this is the only real data we're working with, site-specific data, that is the swipe data.

So, everyone says, okay, that is the rock we're standing on. Is that rock solid, or is there something about it that's a problem?

Later on we're going to talk about given that data are complete and reliable, then of course we can talk about the resuspension factor and other assumptions.

But I think, Brad, you've just nailed down the single most important question given the context we're in now.

Does the swipe data capture the full range of exposure scenarios from resuspension that we need to address, or are there holes there that we can't deal with?

So, I'm glad we got to that point. That's where we should be.

MR. FITZGERALD: Well, John, that's a perfect segue into Bob Barton's discussion of data adequacy and completeness.

DR. NETON: You guys must have rehearsed that.

(Laughter.)

MR. FITZGERALD: So, Bob, are you still with us?

MR. BARTON: I'm still here, John. Thank you.

Okay. So, I guess I'm going to start with the completeness data. For those of you following along, the report is actually on the website. That starts on Page 25, which is Section 4 of the report.

As Joe sort of mentioned at the outset of this meeting, there's been sort of an iterative process to this whole thing. And that goes for the data that was compiled, too.

And in my mind, it sort of went through three stages where Stage 1 was sort of the data we were discussing at the November

7th meeting. And that covered two rooms. It was the SW-8 Room and the R-108 Room. And that data started in 1985, and it was compiled through 1989.

Stage 2 was about a report released early in January of this year and that added additional for those two rooms. So, now the SW-8 dataset actually started in 1969, and the R-108 dataset started in 1983. So, more data was added in sort of a Stage 2 iteration.

And then there's been the most one, so I'll call it Stage 3, which was the report released in late March. And this one added actually two additional rooms to the original two. And that's Room SW-13 starting in 1974, and SW-150 starting in 1968.

So, that's kind of the dataset that we're at now. And because of how the whole process has sort of been iterative, so is the completeness analysis and how it was set up.

So, if it's agreeable, what I'd like to do is kind of start by talking about those first two rooms for which data was compiled. That's SW-8 and R-108. And then we can kind of discuss the final two rooms added at this latest stage at the very end.

And the reason I'd like to do that is so that anyone who's following along in the actual report can really go kind of page-by-page through this completeness analysis and hopefully not get lost along the way.

So, if we start with Room R-108, like I said, the data begins in about mid-1983 and goes up to 1989.

The intake periods that were defined off this dataset for this room and the corresponding number of samples are shown in Table 1 of Section 4.1.1, and are also shown visually in Figure 1.

It should be noted that no data had originally been compiled for 1987. And that's really kind of a two-year gap starting

in mid-1986 through mid-1988.

Any other gaps that were kind of noticed in the data were generally on the order of a few months. And this was the case for a lot of these rooms.

Moving on to the second room, SW-8, again the dataset was expanded in sort of the second iteration so that the data actually begins in 1969 and goes up through 1989.

Similar to the first room, you can see what the defined intake periods were and the corresponding number of samples for intake period on Table 2, and again shown visually in Figures 2 and 3.

There are several gaps for SW-8. They're listed on Page 27 in the sort of bolded form. I don't really want to read through each and every one, but it's worth noting that a lot of them are on the order of a few months.

Although in some cases such as in the early '70s, the gap could be up to two-

and-a-half years.

Sort of the next thing we did in relation to these two rooms is perform an SRDB search to see, all right, do we have any more available data that might be able to fill in some of these gaps?

And one of the types of reports we came across was what we called these HP trend reports. And what these are, originally it was preferable to use the raw datasets. That is you have essentially a map of the room, and you have a number in each area of the room where a swipe was taken and what the value of that swipe was.

Well, in the absence of the raw data there's also these trend reports which basically list out the week and will give you a high, a low and an average swipe result for any given day. They usually also provide the number of samples that were taken on that given day.

So, we found some of those. And

in particular we found one for SW-8 in 1980 which previously didn't have any, you know, data compiled for it.

And there were also several of these trend reports in the late '80s that could kind of fill in some of these gaps where you have a five, six-month period without any of the raw data, but then you could always use these trend reports to kind of supplement the dataset.

These trend reports were actually used for years prior to 1985 by NIOSH for these two rooms. So, that wouldn't be inconsistent with what has essentially been already done.

So, I guess the conclusion there is there is a little more out there in the form of the HP trend reports that could sort of bolster the datasets of these two rooms, you know, if it's determined that that's necessary to sort of flesh out the proposed coworker model.

Okay. So, I guess next along the line here in Section 4.1.3, we identify a sort of dose calculation inconsistency among the different years. Might be beneficial here just to briefly describe again what the model is.

You have a bunch of swipe data taken. Based on certain assumptions about the detector efficiency and that sort of thing, you can kind of get what the activity is on the ground. And you can use the resuspension factor to see, well, if that's the activity on the ground, what's the activity available to be inhaled in the air?

Then you take that and you apply a worker exposure time and a breathing rate and you can develop an intake, a radioactive intake for whatever period you want to define.

The way the calculational spreadsheets were set up, originally it was hoped that you could get a defined intake for each month of the year.

And if you have an intake for each month, you sum all 12 months and you get an intake for the year. That's kind of how mechanistically these spreadsheets were set up.

The problem comes is when you don't have an intake defined for each month of the year. So, for example, say you only had data for one month. You could take all that data, develop, you know, the 50th percentile, 95th percentile air contamination value and you can develop what the intake was for that month. But if you didn't have other months in the year, the, you know, hypothetical worker was only assigned an intake based on one month of exposure.

And that's not necessarily because he didn't have exposure, because that for the rest of the year it's more you didn't have the data to develop an intake value.

So, for situations where the hypothetical I gave where you could only

develop an intake for a single month, you're essentially underestimating the exposure potential by about a factor of 12 if you were going to extrapolate that to a full year.

It's not a real big deal. I mean, you can easily go in and sort of fix those errors and get it going. And we'll show a little later on how if you do go through and fix those errors with the most recent NIOSH case study, the doses change a little bit. But, I mean, again mostly on the order of about a factor of 12.

The very next section I really don't want to spend too much time on. Basically what happened was in preparation for the November meeting, we had performed our own data compilation of these HP trend reports just to see, all right, what's out there, you know, can these fill in the gaps, you know, how do these value shown in the HP trend reports compare with the raw data that has already been compiled and how might that

influence things?

And so based on that dataset that SC&A independently compiled, we were able to make a direct comparison to what NIOSH had compiled sort of in the Stage 2 where they added a lot more data for the first two rooms there, SW-8 and R-108.

The moral of the story there is any errors found, and errors could be a number was transcribed incorrectly or maybe it was transcribed twice or maybe it was just missed altogether, all those errors combined were very low. It was under two percent.

And even when looking at the magnitude as you went through and corrected all those little small, really, really, minor errors, it really did not affect the outcome of this dose model in any meaningful way at least in my mind.

And so, I don't want to spend a lot of time on that one because I don't think it's really important to this discussion.

So, I guess next we move on to the final two rooms which were SW-13 and SW-150 who had data added in the most recent iteration of the proposed method.

And one thing that was kind of different about the data for these two rooms is it wasn't compiled necessarily on a monthly basis. That is when they developed an intake value, kind of pooled all the data for a single year into one dataset. And then from there you could do log-normal fit and develop air concentration.

One concern that immediately jumped out to me when you do a model based on that, one, it's rather inconsistent compared with the first two-room analysis, because that at least attempted to do things on a monthly basis. But also if you're pooling all the data into a single year, there's always the off chance that the final result is unduly biased by a single month worth of data.

Hypothetically you could have a

month that only had ten samples that had -- 90 seemed to be only higher contamination, and then the next month have 300 samples and it was a lot lower. When you pool them altogether, it kind of muddies the water.

That's one thing that SC&A took a look at and said, all right, what happens if we take these things and weigh all the data by month? Let's weigh it by month. So, each month gets equal weight in calculating the annual contamination and how does that compare.

And it was generally favorable, you know. You don't see a very big difference for most months there. And I think there was a couple of - or most years there, there was a couple of years where, you know, if you had weighted all the data by month, that annual contamination value might increase by 25 to 35 percent depending on the room and year. So, that might be a consideration.

NIOSH might want to consider

breaking down the data on a monthly basis if for nothing else to be consistent throughout their dose model.

So, they're really trying to break it down by month where you can. And if you can't, then you can extrapolate things to a full year.

And the last thing is this most recent case study which is essentially, all right, we have these derived intake values. Now, let's see what a potential dose situation might be like.

And when we define that is when we're going to have a worker who's exposed for two years, he's at two years with the highest contamination among all four rooms. And we're going to say, all right, he's exposed for two years, and then we're going to evaluate the dose ten years after that exposure period.

And what is shown in - I believe it's Figure - one moment, please, but those values were presented in Figure 1 of the

original NIOSH - or the NIOSH report for March.

MR. KATZ: Bob, Figure 9.

MR. FITZGERALD: Figure 9, Page 42.

MR. KATZ: Page 42.

MR. BARTON: Yes, there it is. Okay. So, that's the original values. And as you can see, this sort of bounding case - when I say "bounding," it's based on the 95th percentile air contamination value for SW-8, and the total dose evaluated ten years after a two-year exposure was about 0.48 millirem.

Now, we also got the source spreadsheets on that, and unfortunately the same error that I discussed earlier about extrapolating doses to a full year applies here.

It especially has an affect on those two latter rooms in which data were compiled, because again the original spreadsheet calculation only assumed for each intake a one-month exposure time.

So, since all the data was pooled into a single value for each year in those two latter rooms, again you end up with a dose that's approximately a factor of 12 below what it should be if it was actually extrapolated to the full 2,000-hour-per-year exposure.

So, as everyone can see in Table 16 on Page 42, which is just below the original NIOSH results, these kind of show how the doses would change if they were actually extrapolated to that full year of exposure.

And so the limiting case becomes - again this is bounding 95th percentile contamination. Room SW-150 comes out at about 3.7 millirem.

So, I mean, that's just, you know, one of those little things. That's kind of how it changes. Again, it's kind of a factor of 12 increase for that room.

And, you know, when you go through a fixed set error, that's the case study, you know. Assuming all the resuspension and all

these other uncertainties are out the window, we're just going to go with the original case study example and all those assumptions, this is kind of where it comes out. So, you're limiting cases up from 0.48 to about 3.7.

And I guess to kind of put a cap on the concluding statements, we didn't really feel that the data was incomplete or unuseable for this kind of application.

I guess where we come out on it is when there are gaps, for example, like a two-and-a-half-year gap in the early '70s like some of these longer gaps, you know, it can be established within the bounds of security concerns and whatnot to have a discussion to kind of verify that these gaps, these time periods without any data that it is appropriate to sort of use the temporal neighbor, that is the data before and after the period with no data, as representative.

I mean, as long as there's no reason to think that these periods that don't

have data are decidedly different from the periods surrounding them, then we feel that the data is adequate and complete for this purpose and that proper extrapolation is likely possible as long as that connection can be made.

So, I guess that kind of sums up the completeness and adequacy end. Does anybody have any questions? I know I kind of went quickly through that.

So, is there anything I can clarify or - am I still on the line?

CHAIR BEACH: You're still on the line. We're all thinking.

MR. STIVER: Everybody is trying to absorb what you -

MR. KATZ: You were actually very nicely clear.

CHAIR BEACH: Yes.

DR. MAURO: Bob, this is John. I think the question you're raising is something that really goes to NIOSH.

When you do have time periods, and notwithstanding the extrapolation 12-month business, certainly that's something that could be dealt with, though, but you do bring up a point that there are these gaps.

And you mentioned a two-year period where you don't have swipe data for a particular room, and really the question goes to NIOSH.

How do you deal with that? That is in the past when there are gaps, you know, somehow you have to convince yourself that the other data you have, like you said, the temporal data that's around it somehow can be used to place a plausible upper bound on the gaps, you know, and I agree. I mean, that's the question. And the question really goes to NIOSH. How are you going to deal with the gaps?

By the way, the other question that I'd like to pose to NIOSH is, the data that are out there that we have, the swipe

data, did that capture different kinds of operations like inside ducts, inside hoods if that in fact is applicable that I could envision workers operating in a setting where there's a potential for resuspension that's unusual?

And so, I guess given the summary you just gave, Bob, I have a couple of questions for NIOSH. And one is the gap, and the other is the scenario. I think that needs to be explored.

DR. NETON: Okay. This is Jim. I think I'd like to turn that question over to the Mel Chew folks that are on the phone who were responsible for putting this report together.

Anything you can put - Bob or anyone else on that end can comment on that?

MR. MORRIS: Um, we took the data as they were available. It wasn't that -- excuse me. Robert Morris talking. Ted, I'm sorry.

Do you hear me?

MR. KATZ: Yes, thank you, Robert.

MR. MORRIS: Okay. We took the data then as they were available. And we didn't exclude anything based on location.

What we found is extreme consistency. So, if it were special job coverage, we never saw it. We didn't find the kinds of things you would see swiping the inside of ductwork or something like that.

So, all I can tell you is that we don't have knowledge of scenarios that might have been unusual like that, John.

DR. NETON: Bob, is there any intelligence you can provide on why these gaps may have been there? I mean, were there maybe not ongoing activities in the room at that time, or would that just be speculation at this point?

MR. MORRIS: I have no personal knowledge. I wasn't privy to the kinds of conversations that were in classified

meetings. So, I don't know that.

DR. NETON: Okay. Well, I think SC&A has got a valid point. I mean, NIOSH needs to go back and evaluate why these gaps were there. And if there were ongoing activities, what was happening that might make them suitable or not suitable for interpolation between the available points -- or extrapolation, I guess.

MEMBER ZIEMER: And the related question, and I think somebody raised it was, are there any differences in the operations during those periods that would cause concern?

Extrapolating between or beyond, you usually have to have --

DR. NETON: Yes.

MEMBER ZIEMER: -- some assumption about --

DR. NETON: No doubt.

MEMBER ZIEMER: -- either things have changed or not. So, otherwise you're operating under the assumption that if you

have a lot of samples, they will cover the scope of the kind of work that was done.

I don't know how much we've seen of what the range of sample - well, we've seen in the charts what - there's a pretty big range in some of these samples.

And we've covered a lot of different scenarios, I presume, but that would certainly need to be confirmed.

DR. MAURO: This is John. One thought is you have lots of these monthly 95th percentile values rather than look at individual swipes.

If you have monthly 95th percentile values, and I'm not looking at the graph right now, but - and collect those, you start to get a sense of how variable the high end is.

Now, what I mean by that is the high - for a month, any individual swipe is - of course you're going to have enormous variability. Enormous.

But when you start to collect hundreds, if not thousands, of swipe samples that were collected in a given room in a given month, and you take them all and, you know, for that month and you get a 95th percentile, then you take the next month and then the next month, and then you start to look at those, that will start to give you a sense of how variable the high-end concentrations were over the course of a month.

And that will at least in my sense, is that will start to give you an indication whether, you know, what the variability on the high-end values from month to month could have been different by factors of - or by orders of magnitude. Then, you've got a problem.

But if you see that, you know, from month to month the 95th percentile values are clustered, then you start to get a sense that, well, is there any reason to believe the place where you have some holes might be, you

know, different. And you could argue, well, we haven't seen those kinds of differences in other months.

But then again as Paul pointed out, do we have reason to believe that there was nothing unusual happening, I mean really unusual happening in those months that have the holes?

So, I mean, I'm just looking for a way how I would come at a problem like this.

MR. MORRIS: This is Robert Morris again, please.

I think that if you look at the data as a whole, you will see that it's remarkably consistent without a lot of high swipe results in the set. It's a chronic low-level dataset. It's not characterized by wild swings.

Now, having said that we haven't, I mean, I can't give you number values on how to describe that right now, but we certainly could take that approach if it's worth doing.

DR. NETON: Well, clearly we've got some work to do on this piece.

CHAIR BEACH: Okay. So, I have two action items. I've got NIOSH needs to evaluate gaps in the data, and then Paul's point, was there any difference in what work was being done during that time period. Hopefully I captured that correct.

And then what about the table - or Figure 9 on Page 42 that Bob brought up? I didn't really hear any discussion on that.

DR. NETON: Figure 9?

CHAIR BEACH: Yeah, on Page 42.

DR. NETON: Oh, that was our own reconstruction of the doses. That was right out of Table 1 of our report.

CHAIR BEACH: But there were some mistakes there, and I guess I didn't really hear any discussion on what would -

DR. NETON: Well, I think the implication is that Bob used where there was only one month worth of data, he assumed that

there were 12 months of exposure, not one month as we did. So, his doses are larger because of that.

CHAIR BEACH: Right.

DR. NETON: It remains to be seen at least in my mind, whether it's justifiable to say there's an additional 11 months worth of exposure if it was - I'm guessing, but what if the room were locked up and nothing was going on there?

CHAIR BEACH: So, that goes back to the first two items.

DR. NETON: It all comes back, yes.

CHAIR BEACH: I just wanted to make sure that was covered.

MR. FITZGERALD: It's really a sort of campaign-based or routine operation.

CHAIR BEACH: Okay. So, where does that leave us as a Work Group then?

MR. FITZGERALD: Well, that's data adequacy.

CHAIR BEACH: Right.

DR. NETON: I think I believe it may be time for a break.

(Laughter.)

CHAIR BEACH: I was definitely going to suggest a break here. So, we'll go ahead and take a 15-minute break and then we'll recap.

MR. KATZ: Okay. So, it's about 10:30 now. So -

CHAIR BEACH: 10:33.

MR. KATZ: -- about 10:45.

(Whereupon, the proceedings went off the record at 10:33 a.m. for a brief recess and went back on the record at 10:53 a.m.)

MR. KATZ: Okay. Welcome back, Mound Work Group. We're ready here in the room.

Phil, do we have you on the line?

MEMBER SCHOFIELD: I'm on the phone there, Ted.

MR. KATZ: Hi, Phil. Good. Thank

you.

CHAIR BEACH: Okay. So, let's just recap on the tritides discussion. We do have action items for NIOSH as discussed right before the break.

NIOSH is going to evaluate the gaps in the data, and then maybe what work was going on during that time period.

Did I have anything else or do we need to add anything to that?

MR. FITZGERALD: For tritides in general or for -

CHAIR BEACH: For tritides in general.

MR. FITZGERALD: Essentially what we did in the report, what Bob Barton covered was the review of adequacy and completeness, as well as to look at the assumptions, essentially the model itself.

And after we received the March 2012, the very latest iteration White Paper from NIOSH, we actually wanted to take a

further look at the question of uncertainties.

And, Ron Buchanan, are you on the phone?

DR. BUCHANAN: Yes, I am.

MR. FITZGERALD: I'd like to have Ron outline the analysis we did, which just essentially looks at the variables, the assumptions which were embedded in the model.

Because as I was saying earlier, I think that was one of our original concerns over the model itself. So, I think it would be helpful for the Work Group to hear that review.

And after that, we also looked at a - sort of an analogous model which DOE put together and used in our handbook in 2008. And I think actually the two models are very similar, but that in terms of contrasting that we went ahead and did that as well.

So, Ron, can you walk us through?

DR. BUCHANAN: Okay. In the report, it starts on Page 60. And the reason

for this was we have the basic equation there on Page 60 that simply looks at the count rate that was recorded on the swipe, and then the conversion factors and some constants put in and that sort of thing to arrive at a dose.

And what our initial concern was, how does this vary, you know, since we don't have - we have some specific data for Mound, but we don't have exact data throughout all the years for Mound.

If you vary these parameters, does this affect your dose much? That's the general overall picture we were looking at here.

And so we see on Page 61 there, a list of about six factors that are in the main equation. And they're like detector efficiency, there's counts per minute, how accurate are those, the swiping of the surface over periods of time, resuspension factor of course which we can talk about more in this section, we just address it, but not discuss

it, the breathing rate, the time of exposure and dose conversion factor.

So, looking at these -- and this is all summarized in Table 23 on Page 62. And so, essentially what I tried to do and said, okay - and this is subjective, you know. What is a lower value, what's a higher value, what's median value?

And so, I looked at the value that NIOSH was suggesting to use in their 2012 value, which was more the reasonable estimate and say, okay, how much could this vary or how does this match up with what's published and stuff? And go on either side of that for low values and upper values within reasonable range. And I list the parameters there.

And then I said, okay, if you used all lower values or you used all upper values, how much would this change the median value that NIOSH put forth in their latest paper?

And so, you see I did it two ways. Since the resuspension factor was of major

concern here and had been related in the past, I did two analyses that you see in bold at the bottom.

I did it using a constant resuspension factor. And so, is resuspension factor the only thing that really matters here, or do other things matter?

And we see that if we were to settle on a resuspension factor, that the other variables within reasonable range would give you a dose that would range from 0.02 times the suggested value to about 135 times the suggested value.

So, essentially this illustrates that the other factors are of importance also in this case when you're selecting a model which you have to plug in parameters that weren't set necessarily by the site or you were using a range of these parameters to say, what I should use, what's the reasonable value here. So, we see that it does have an impact.

Of course in the line above that

you see that if you factor in the resuspension factor that it varies a lot more, 0.0003 to 8,000 times the median value.

And so in summary, you know, that illustrates that it does depend even if you're talking about low doses of millirem or so to an organ, you see that the values chosen -- the parameters chosen does have a significant impact on the outcome.

And of course the resuspension factor has the largest, because it has the largest range that we've discussed in the past.

DR. NETON: This is Jim. A good summary, but I'd like to point out I don't think - I don't suspect that SC&A was suggesting that one would use the high value for all the parameters in the dose reconstruction.

That does counter every piece of advice one gets in doing these types of calculations and not take the high end of the

value for each parameter and propagate that through the end result.

I think that would just not be good science.

DR. MAURO: And, Jim, this is John Mauro.

Bear in mind at least in the case of the resuspension factor where the range that we're looking at represents, you know, resuspension factor is observed and it's in the chapter on resuspension factors, you know, are quite variable.

But if we were to ask the question the average annual resuspension factor --

DR. NETON: Right.

DR. MAURO: -- it would bring this spread way down.

DR. NETON: Exactly.

DR. MAURO: So, that's an important point that is which one of these - which of these parameters would be the upper end? Would that represent a reasonable annual value

and where the variables from day to day may change. That's an important consideration.

DR. NETON: 80,000 times is not a realistic number.

DR. MAURO: No, and I also agree that if you were to do a Monte Carlo and you would say what's the probability that every one of the parameters would be at the high end, it would be, you know, the probability - it would approach zero.

DR. NETON: Right. So, and the other thing I see missing from this table would be the effect of using the difference between the 50th percentile, 95th percentile in the comp rate distribution which is one thing we would weigh in on as well.

MR. FITZGERALD: Yes, and I think it also should be added that - and we said this in the paper that we thought the change of the resuspension factor which by far is the most influential variable, was in the right direction.

We agreed it was overly conservative in the first paper. And even though it was changed two orders of magnitude, we thought the number had a better basis.

This goes back to what John was saying that we want to treat the uncertainties, but recognize that making the call as to where is the proper place to fall in the range is an important thing.

But given the fact it's a theoretical model, we just wanted to emphasize since it really didn't get treated as much in the two NIOSH White Papers, that somehow that had to be built into whatever final approach as to how you would treat that, what percentile distribution.

We didn't do a sensitivity analysis. I mean, that's clearly what this could have gone into. But, you know, frankly we just wanted to raise the question and to make sure it was clear that certainly these play into it.

DR. NETON: And again I just point out for consistency purposes, I think what we've done here is very consistent with TIB-70 approach. And it's nowhere that we have ever ended up using uncertainties about the resuspension factors in our calculations. We typically pick 95th percentile which we believe tends to bound the intakes.

We have some debate as John knows about the resuspension factor, but I think this one is quite reasonable. And I think there was some discussion in the NIOSH report as to why this one was selected.

But point taken, there is variability in these parameters. We have not selected a final model yet. Obviously we put a couple out there, the 50th percentile, the 95th percentile. And how we address against these other parameters I think we need to talk about.

So, I think that should be an action item for NIOSH which is to describe a

finalized approach for this model that would either incorporate or use the 95th percentile or do the full distribution, you know, whatever.

We've left a couple ideas on the table.

CHAIR BEACH: So, describe the final approach for the model.

DR. NETON: Right. Whether it's the 95th, 50th to full distribution or, you know, that sort of thing.

DR. MAURO: And, Jim, this is John. To help frame this problem within the things we're talking about within an SEC context is, you know, again when we look at the variability in the swipe data in just the numbers and we see how spread they are, there were also some, what I would say, important qualitative questions that we don't want to lose sight of.

The swipe data you're getting a, I guess, the total data count per hundred

centimeters squared. And it's important that we don't lose sight that what you're looking at in terms of the tritium whether you're at the 50th percentile, 95th or whatever, there are holes that need to be filled, et cetera, keep in mind what that data are.

And that is we're assuming it's all hafnium tritide.

DR. NETON: Right.

DR. MAURO: And I would like to alert everyone that there is a real - there is a plausibility question, in other words, and this is something that we have to deal with.

The swipe sample data, and it is a widespread value that would - the spread we're seeing and the holes we're seeing, we don't want to lose sight of the fact that we're making an assumption here that all those counts on the swipe sample are hafnium tritides.

And I think intuitively for me, that seems to be probably very unlikely that a

very significant fraction of whatever counts you're getting might very well be tritiated water.

And I know Ron may want to weigh in a little bit on that. So, I don't want to lose context. You can easily get lost into the numbers and forgetting about the context.

Same thing goes with the resuspension factor and the spread. I don't want to lose context on that.

The resuspension factor data that we summarize in the chapter, are data that really come from uranium, plutonium, dust itself, not radioactive material, and did not come from data that represent hafnium tritide.

I have no idea how it behaves, but certainly intuitively in this case we're really talking about hafnium as, I guess, some kind of metal, particulate metal of some size distribution that settled out.

The fact that it is attached to tritium, you know, we have to understand that

the numbers we have for resuspension factor literature are for very specific types of particulate material that settled out and particulate size distributions of that material.

How important that is in building a bridge and applying that to this particular problem related to tritides, we have to keep that in mind as a conceptual challenge and the degree to which we're comfortable making those assumptions.

MR. FITZGERALD: Jim, I think there was some commentary in our review about what exposure duration and latency period was used. I know we kind of raised that as a question, but is there a specific reason for the ten-year latency?

DR. NETON: No, I think -

MR. FITZGERALD: I mean, I'm just -

DR. NETON: I got a little confused when I saw your comment on that, because then I got to thinking about latency in risk

models, but it's really nothing to do with that.

It's just saying that the cancer occurred ten years after -

MR. FITZGERALD: As a hypothetical.

DR. NETON: -- exposure as a hypothetical situation.

MR. FITZGERALD: Okay.

DR. NETON: So, all it really meant was there was ten years' worth of exposure. You only construct the dose until you get the cancer.

MR. FITZGERALD: Right.

DR. NETON: So, they could have easily just said let's assume the cancer occurred ten years after exposure. Latency really didn't play in there.

MR. FITZGERALD: Right. So, I mean, in other words it's just an example -

DR. NETON: It was an example of a case study which was put out there to attempt to demonstrate that the doses were indeed -

MR. FITZGERALD: And clearly suggested that you did the -

DR. NETON: Did the 20 years, 10 years --

MR. FITZGERALD: Right, right.

DR. NETON: -- five years, six years of exposure.

MR. FITZGERALD: Okay.

DR. NETON: And that's when, you know, I realized you could start getting into doses that far exceed a millirem because this was the one isolated case study. It was illustrative though, which demonstrated the doses are indeed in the millirem range.

MR. FITZGERALD: Yes, I don't think there's any debate about the fact that they are relatively small. It's just a question -

DR. NETON: And I think throwing in latency got me all confused because I immediately start thinking of risk model, the apportionment of latency between zero and ten and the S-shaped curve and all that kind of

stuff. It doesn't even come into play.

MR. FITZGERALD: Ron, do you have anything else?

DR. BUCHANAN: Yes, I did want to address Jim's statement about the - no, I was not suggesting we use the 8,000 upper limit or anything like that.

This came about simply for two reasons. Number one was we wanted to give the Working Group an idea of how things change. That this was an equation that you could get an exact answer depending on the parameters putting in. That was to illustrate that.

And number two is that, you know, we were at the time, the mind set was we were looking at this one millirem magic number and we wanted to illustrate that, you know, depending on the parameters, you could come up with less than a millirem or more than a millirem.

And on the committed dose and the latent -- the exposure period and the latent

period, we just went into that area to see if it was really important in their case study.

Your scenario had two years of exposure and ten-year latent period. And so we said, well, is this important? Even though it's not in the first equation, is it important?

And so, we looked at it and we said, well, you know, it's kind of intuitive. If you increase your exposure time, double it, you get about twice the dose. If you half it, about half. If your latent period is greater, you know, you'll get not quite double the dose and stuff.

And so, we found that those were parameters you chose to illustrate the case, but it wasn't really influential on our overall umbrella analysis of the situation.

DR. NETON: Appreciate that. My only concern with the 8,000 is someone can read that and say the doses could be 8,000 times higher when in fact I don't think anyone

would agree that they could be that much higher.

I admit there's a lot of uncertainty there, but it's not that great.

DR. BUCHANAN: Right. I was looking at what more could it range and did these parameters, really, the details in there, what should we be concerned with, you know?

We don't want to worry about breathing rate and time. Those don't have a big influence. And, you know, it's the factors that influence the outcome the most that we want to spend the resource on.

MR. FITZGERALD: And I think the other cautionary note is that you see some of these dose estimates, two or three significant figures, and I just sort of realize that we're operating in a realm where we say several millirem. That's probably as precise as one gets.

And that was a little bit, you

know, I didn't want there to be construed a level of precision that doesn't exist when dealing with this much -

DR. NETON: Agreed.

MR. STIVER: This is John Stiver. I'd like to kind of weigh in a little bit on this.

You know, back when they were kind of grappling with how to present this, we thought about possibly doing a full-blown uncertainty analysis and doing Monte Carlo, Crystal Ball simulations for all the different distributions and we thought it would probably be better just to give more of an illustrative example.

But this is something I was kind of concerned with that putting out the extreme values out there could be misconstrued as to being realistic possibilities as opposed to what you might actually get in an uncertainty analysis.

MR. KATZ: This is Ted. In general

I think down the road not for this particularly, but generally when SC&A does these, I think it would be better to use reasonable assumptions to give a sense of the range of uncertainty instead of sort of theoretical limits or whatever that has been used here, which is giving a wildly broad range of uncertainty.

So, I mean, it's unreasonable to those choices you're making if you're going to try to illustrate to a Work Group, you know, how much uncertainty there could be in these figures realistically as opposed to tweaking every parameter to an extreme.

MR. FITZGERALD: Ron, anything else?

DR. BUCHANAN: No. That was it.

MR. FITZGERALD: Okay. I don't know if we -

DR. MAURO: Joe, this is John. I just - I'd like to just bring one thing up I guess with Ron.

I think we talked about it before, but it would be good to put it on the table here.

Am I correct when we take those swipe samples, am I correct that it's difficult to judge what fraction might be hafnium tritide and would you - now, this would be just your experience in this matter or anyone around the table, around the phone, or would you expect that most of that count that you would get from the swipe is tritiated water?

MR. BARTON: Well -

DR. MAURO: You may not be able to - no one may be able to answer that. I don't know.

MR. STIVER: I think that the questions we're grappling with is what is the fraction --

DR. NETON: I mean I point, John, to your report that actually says that a significant fraction of the activities could

be tritides in the room. I mean, if that's true then there's some significant -

DR. MAURO: Perhaps for some of them, you know, I don't - my problem is I don't know, you know.

When you have a swipe taken in a room where there may be some tritides and there was also tritiated water, that was, you know, and you take a swipe there, I have no sense whether there may be certain time periods and locations where it's predominantly the tritide, or maybe it's not, you know.

Something tells me, and this is terrible to say, but instinctively something tells me it's probably dominated by tritiated water. But, you know, and there's a - and this goes toward the uncertainty that Joe brought up in the beginning, you know.

We build a model, we try to probe it and say, well, listen, is this a good way to come at the problem? And I think it's important that we all understand the embedded

assumptions even in the spread and in uncertainties that we just talked about, you know.

We're being quantitative here, but in reality there are these issues that we are troubled by.

In my mind, quite frankly, there's no doubt that by using the upper 95th percentile for a given time period where you have data and you use the upper 95th percentile and assume it's all hafnium tritide, there's no doubt in my mind that for the purpose of that month of exposure you're off-the-charts high, you know. That's how I come at this.

Now, so I believe there are some issues here that the Board will have to struggle with. That is, you know, once you recognize that this could be an off-the-charts high characterization of how much tritide, namely hafnium tritide, was on surfaces in a given time period using the data that we start

with, I mean, this is something we have to struggle with, all of us.

DR. NETON: But, John, don't you agree though even if that's the case and the doses come out to be three millirem that it's -

DR. MAURO: Oh, I got to tell you -

DR. NETON: You got to take that in consideration, I think.

DR. MAURO: Oh, yes. Very important. I'm glad you brought it up. You're absolutely right. That is that, you know, by assuming it's all hafnium tritide, I would say that it's an extraordinarily conservative assumption.

And even then, I agree with you, you're coming in with doses that are relatively low.

DR. NETON: I would think if you were in very high doses where it could put someone on the borderline, you know, factor of ten would make it 70 percent TC versus a seven

percent TC, I mean, then you've got some issues there.

DR. MAURO: That's very important to put out on the table, and that's why I'm bringing this all up.

DR. NETON: You have to take into account the magnitude of the source term, I guess, is what I'm -

DR. MAURO: Yes, yes.

MEMBER CLAWSON: Well, John, this is Brad. I'm glad you brought that up because my question now leads into this.

The swipe data that we have, do we really have any swipe data that calls it out, this is tritium?

CHAIR BEACH: No.

DR. MAURO: No, isn't this all tritium? I mean, Jim, or, Ron, this is what's -- how is this counted? I assume this is counted in a way that -

MR. STIVER: John, this is counted in a PC-5 gas proportional counter, but it's

adjusted and calibrated to -

DR. NETON: It's assumed that it was all in one particle, John. So, the efficiency was based on that.

DR. MAURO: Oh, I see. Okay.

MEMBER CLAWSON: And we're looking at hafnium because it's the worst actor, right?

DR. MAURO: Yes.

MEMBER CLAWSON: We're not looking at any of the other tritides that -

DR. MAURO: No.

DR. NETON: We have urine samples that would indicate that the HTO component, and that's what we would use to calculate doses to the organs if the tritides wasn't bounding.

So, we have both ends of the spectrum. We have actual biological bioassay data that we can use, or we can use the tritide intake. That's our choice depending on whichever ends up with the higher dose.

MR. STIVER: Is there a situation where you would use them both? I mean where you could have people who were being exposed to tritiated water in addition to the tritide? So, it's kind of -

DR. NETON: Well, I would think you have multiple cancers maybe.

MR. STIVER: Yes.

DR. NETON: So, I guess there's a little bit of a conundrum. We've run into that before where you have two cancers and you can't be exposed to two different sources at once. I'm not sure how we would handle that.

MR. STIVER: Well, this situation would be, I mean, you have tritiated water basically permeating the work space, but you also have this other component of this -

DR. NETON: Well, we would maximize one way or the other. Tritiated water would bound the dose - assume tritiated water bound the dose. We would use that. If tritides bound the dose, we would use that.

MR. STIVER: Yes, but to such a small increment, mostly.

DR. NETON: It depends on the cancer, I think. I think mostly it's going to be lung cancers, but I did notice that the lower large intestine tend to be irradiated more over the long term because of the -

MR. STIVER: Yes, insoluble particles being cleared -

DR. NETON: Yes.

MR. STIVER: -- through the digestive tract.

DR. NETON: So, yeah, we were doing both models to get the higher of the two. So, we've covered both exposure scenarios, I think, or the extreme end of exposure scenarios.

CHAIR BEACH: Did you have anything more, Joe?

MR. FITZGERALD: Yes, Bob Barton, are you still on the phone?

MR. BARTON: I'm here, Joe.

MR. FITZGERALD: Can you spend a few minutes just summing this thing up relative to the DOE handbook 2008 method just to contrast that quickly?

CHAIR BEACH: Which is on Page 67 if anybody is looking at that in the report.

MR. STIVER: Actually, I did that section there.

MR. FITZGERALD: Oh, I'm sorry. Never mind, Bob.

CHAIR BEACH: Thanks, Bob.

MR. BARTON: No problem.

MR. STIVER: You can relax now, Bob.

Basically what we wanted to do is find a paper out there that would be kind of a benchmark study that would help to validate the NIOSH report and we did find one.

This is the 2008 DOE report called the DOE Handbook, Tritium Handling and Safe Storage. And there's an appendix in there, and I think it was Appendix E that describes a

method of calculating dose to the respiratory tract from these insoluble tritiated particles.

And so, we looked at the DOE model in comparison with the NIOSH model and they both use the same basic approach.

The DOE and NIOSH both take a look at this self-absorption factor. And what this really does is when you're looking at particulate forms of insoluble tritides, you're looking at an average beta energy of about six keV.

And so, the fraction of beta particles that actually escape the surface of that particle could be quite small and be limited to the surface area.

And so, the actual observed activity compared to the actual activity in the particle can go down quite significantly as particle size increases.

And so, to account for this using a liquid scintillation counter, basically any

- almost any beta particle that makes it into a cocktail is going to be registered as a count.

And so, what you're looking at really is this idea of observed activity. And NIOSH took kind of a slightly different approach than DOE. I can kind of talk about that a bit.

What they did was they corrected the PC-5 counts, basically the gas proportional counts by calibrating those to the liquid scintillation counting efficiency in the first paper.

In the second paper, they looked at this self-absorption factor for energy, and they basically corrected the PC-5 by dividing that by the absorption factor to get the total activity for the - that was in that particular particle.

And then from that, went through a series of calculations. And then at the tail end of the calculation, they then corrected

for the observed activity for the respiratory tract doses by taking a look at this distribution of these self-absorption factors for energy. I believe the geometric mean was about 0.12.

And so for the lung dose or any of the respiratory tract doses, mainly lung in the ICRP 66 model, they went ahead and multiplied that back by the 0.12 to account for the fact that only the particles that actually escape the surface are going to be able to interact with the tissue in the effective dose.

The DOE paper took a kind of similar approach, but with DOE they were really concerned with effective dose as opposed to individual organ doses.

And they used the same basic construct. They produced a self-absorption factor which was about a factor or two higher than the NIOSH calculation.

I think NIOSH used a method by -

in a paper by Knopf, I believe, in 1988, I believe, but that's kind of beside the point.

DOE then went through and they graphed everything into the tail end in their dose conversion factors for effective dose.

And so, what they did was they accounted for all these things in the intermediate steps. And then for the component for lung, they went ahead and added in, they multiplied by their self-absorption factor. And then those individual components were then weighted by the tissue weighting factors in some to yield the effective dose component.

But when you look at the individual organ doses for lung for NIOSH versus the DOE construct, the weighted values come in with about a factor of two to each other.

And this really gets back to just - the scale is almost exactly by the self-absorption factor for energy. I think the DOE

value was about 0.26 and the NIOSH value geometrically is about 0.12.

And so, this kind of gave us a fairly higher degree of confidence that this particular approach NIOSH is taking is indeed a reasonable one.

We thought that it was based on our initial reading of it. It seemed to be perfectly scientifically reasonable.

And by being able to benchmark it against an existing study which is a fairly comprehensive study, we felt pretty strongly that they're kind of on the right track here, but there really are big issues in terms of the methodology that were employed.

So, that's really it in a nutshell. Are there any other questions about it?

MR. FITZGERALD: I mean, in terms of self-absorption factor, which way would be preferable or is there even a difference really?

MR. STIVER: Between the beta and the energy?

MR. FITZGERALD: Yeah.

MR. STIVER: Basically, it - NIOSH felt that -

MR. FITZGERALD: It's just a judgment call.

MR. STIVER: Yes, the fraction of beta at the surface. In any case, you're going to get a potential with that. And obviously for dosimetric purposes, you want to look at the energy that escapes those particles.

So, I think we're on pretty good grounds there.

CHAIR BEACH: Any questions, comments on that?

(No response.)

CHAIR BEACH: Okay. Phil, are you still with us? Any comments or questions?

MEMBER SCHOFIELD: No questions at this time.

CHAIR BEACH: Okay. So, then we're ready to move on to the adequacy and completeness of the internal dosimetry.

And I know there was some comments, questions, there are a couple papers out. What's left here is the thorium issue, the early time period, the February '49 to September '49 polonium issue, and then of course the tritide issue that we just discussed.

Let's see. So -

MR. FITZGERALD: Do you want to maybe broach the thorium because -

CHAIR BEACH: I was just going to say let's look at the thorium. Yes, let's look at the thorium.

So, we had several papers on thorium. And the latest one was sent out May 30th, by SC&A. And it actually captured SC&A's comments, NIOSH's comments and then SC&A's replies.

So, if you have that, we should

work to that.

MR. FITZGERALD: Yes, we were trying to keep this in real time in the sense that with this meeting coming up that we wanted to at least provide some reaction to the report that we got from NIOSH. I guess it was May 8th.

And I think what it comes down to is for want of a better term, you know, whether or not one is confident on the reliability of the program that was in place because it sort of comes down to that in a way that there isn't - this is reminiscent of a lot of the other internal dose issues.

And if Brant was here, we both would wince because we went through this for a couple years and I don't propose we go through it again.

CHAIR BEACH: Well, can I say, Joe, to that -

MR. FITZGERALD: Yes.

CHAIR BEACH: -- this actually

goes back before that to the January 8th, 2012 White Paper.

MR. FITZGERALD: Right.

CHAIR BEACH: Which had a table that had a lot of different open items, which is what we asked Brant -

MR. FITZGERALD: On thorium.

CHAIR BEACH: On thorium.

DR. NETON: Thorium was one of those.

MR. FITZGERALD: Was one of those, right.

CHAIR BEACH: One of them. So, we had actually given your - SC&A's recommendation was to totally close everything, but we wanted to tie all these up and make sure -

DR. NETON: I think those were considered to be dose reconstruction Site Profile issues, is my understanding.

CHAIR BEACH: But there was a couple SEC issues embedded in there that we

were trying to -

DR. NETON: I understand. I thought Joe's memo that came out most recently clarified that the thorium is the only remaining issue. The other ones were Site Profile issues.

CHAIR BEACH: Well, and then but there's also the polonium in there -

DR. NETON: Well, the polonium one I can address.

CHAIR BEACH: -- as well.

MR. FITZGERALD: There's three issues. And I apologize. I think the preface to that matrix was not crystal clear. But we did say in that preface that there was three SEC issues outstanding; the tritides, the polonium was the early years, and this thorium issue.

And the other ones which clearly we need to wrestle with a little bit is the baseline for the Site Profile issues. But beyond those three central SEC issues at least

from SC&A's standpoint, we didn't see anything that stood as an SEC-significant issue. The Work Group may disagree, but we kind of went through that and that's where we came out.

And this analysis of course is a response to the thorium White Paper that we received not too long ago. And we had some questions, and we went ahead in real time and posed those questions back to NIOSH and we got a response. And this is sort of a response to the response.

So, I think we pretty much have wrestled this as far as we can. I want to - not to be glib, but again I think where we came out in terms of what actual data and evidence is available, it does come down to accepting that the oversight and controls were adequate and working in terms of who got urinalysis, who did not.

I mean, there's no way that we can really pin that down too well.

DR. NETON: Exactly. I don't want

to cut you short, but I think it comes down to whether or not it's a believable scenario that Mound actually did have appropriate administrative control.

I went and looked through all the data I could find in the last week or so to try to have a fresh pair of eyes on it. And Brant was looking at it as well and -

MR. KATZ: Jim, sorry. There's a conversation going on, on the phone. Please, someone on the phone is talking. Two people are talking on the phone. Can you put your phone on mute, please?

The lady that's speaking right now, can you put your phone on mute? *6. Thanks.

DR. NETON: And this has been discussed by Brant before, but I went and looked, went back and looked at the Herb Meyer reports that talk about redrumming being done on a periodic basis. Personnel were assigned, were provided contamination control equipment,

clothing and monitoring surfaces.

This is Meyer summarized this over a period of years, and then I went back and looked at the - there's a lot of quarterly Health Physics reports out there that span from 1948 to 1960 something.

And each of these reports, at least the ones I was looking at in the 1960 time frame, have a very nice statement that I'd just like to read that says: Personnel working with radioactive isotopes or in areas containing radioactive materials are required to submit urine samples. The urine specimens are analyzed quantitatively for radioisotopes, to which employees may have been exposed, and results are used to estimate employee's body burden.

And they go on further and explain what happens if there's what they call a hot sample.

Each of these reports have that statement, and then they go through and report

on all the various activities that occurred and the monitoring that was done in that quarter.

And in the periods that we know that redrumming was done, we see that there's a report on thorium analyses in the report that there were, in this case, three 24-hour urine specimens were analyzed for thorium content. The maximum concentration was 0.7 dpm, that sort of thing.

So, there's a consistent body of documents out there that points to the fact at least in our opinion, that the workers were monitored.

And the Meyer document also talks about a small number of workers being involved and we see that in the quarter reports, where there are anywhere from three to four or so people monitored per quarter for thorium in urine, which is very unusual.

I have not seen this level of thorium in urine monitoring on a routine

basis. I can't think of any other site.

And, in fact, there is a database out there that had 350 total thorium and urine samples that were taken at Mound. And Brant actually went through for that last White Paper and picked 20 of those workers and did some dose reconstruction.

So, I don't know whether we just end up agreeing to disagree on this, but in our opinion it appears that the thorium project was monitored pretty well.

I'd also point out most of the thorium activity where there was - outside the drumming, the original refinery-type project that was done back in the mid-1950s all occurred during the - prior to or during -- just at the cusp of the original SEC that stops in 1958.

So, there were some thorium activities that were not redrumming that occurred. But if they would have occurred in the original SEC period, those people are in

the SEC already.

So, any reconstruction that we're talking about for thorium, in my opinion, is going to be either redrumming or there's just one - as far as I could find, there is one miscellaneous piece where they did something else which was using thorium, coating thorium with molybdenum or something as a surrogate for the plutonium-238 microspheres.

And the thorium particles in that particular experiment were a hundred micron in size which is respirable, to my knowledge.

So, I mean, that's where we're at. I don't know, you know, maybe this is one of those glass half empty, half full situations.

MR. FITZGERALD: I suspect that's the way it's ended up with internal.

Ron, short of going through these one by one which I think the responses are before the Work Group anyway, is there any - you spent some time on this.

Do you want to add anything?

DR. BUCHANAN: Yes. This is Ron of SC&A.

Essentially it boils down to we looked at the dose reconstruction model and we don't have a problem with that. We do not have a problem with what NIOSH has said.

It's just that we don't have any assurance one way or the other. We don't have any red flag saying, hey, we've got a group of workers saying that they worked with it and weren't bioassayed. On the other hand, we don't have anything to say, yes, you know, it's like an operating - if you got a reactor operating accelerator, you can say, okay, how long did it operate or were people monitored, who was there, were they monitored, and you can go back over some of the claims and stuff.

In this case, we really can't prove a negative. We can't prove that people worked with it or were inadvertently exposed to it that weren't directly connected with redrumming or some other use of thorium that

weren't monitored.

So, you know, like Joe said, we've done about all we can do on it. And if they were monitored, you know, the procedure is there to assign them the dose. And so, we don't have any way one way or the other to prove that some people worked with it, weren't monitored.

CHAIR BEACH: As the report states. Paul, anything?

MEMBER ZIEMER: Well, one part of this, the concept is accepting that oversight was in place. The existence of those samples tells you that there was some oversight in place.

I suppose you can always argue that could there have been someone working there that didn't have monitoring, but that's - you're probably going to raise that issue anywhere.

And I suppose if someone made the claim that they did redrumming as part of

their operation in their CATI or something and they said, yeah, I redrummed thorium, I worked with those folks, we can always, I don't know, do a coworker model or what would you do?

DR. NETON: Well, we have 350 samples. There are also some air concentration data. Although, most of the data I saw were in the 50s and they were fairly low.

There was a lot of high activity, but I don't think those were necessarily the redrumming operations. I couldn't really quite tell. That was in Brant's report.

So, I agree with you. You really don't know and then what do you do? Do you add a Class of people who weren't monitored?

Those who were monitored are not in the Class, and then those who weren't are in the Class. It could be an issue.

CHAIR BEACH: That takes us into most of our internal and why we are where we are with just thorium left, because -

DR. NETON: Yes.

CHAIR BEACH: -- it's the same case.

MR. FITZGERALD: Well, because we've gone through the saga of trying to go through the Meyer report and the King report. And basically there just isn't any other information that can pin this down. So, I think we agreed to disagree to some extent, but also agreed that you would need something that would be clearly corroborating. Otherwise, you would get into the same scenario Jim suggested that he would -

MR. KATZ: Excuse me, Joe. Please, there are people on the phone that are carrying on conversations.

If you don't want to listen to this, then I would suggest that you disconnect. But you're interrupting everyone who's trying to listen to the discussion here, including other people on the phone who may have a harder time than the people in the room

hearing what's being said.

So, please, mute your phone or disconnect.

CHAIR BEACH: Thank you.

MR. FITZGERALD: But again that avoids the circumstance that Jim just alluded to that otherwise you're recommending SECs for periods where that data is lacking, but you have to, you know, you don't know one way or the other what it means whether it's operationally there wasn't anything or whether in fact the monitoring wasn't done.

So, I think that's the circumstance here, but there is information which actually there's more information on thorium than we found for some of the exotics.

CHAIR BEACH: Phil, are you still on the line? Do you have any comments or - hopefully you were able to hear the discussion.

MEMBER SCHOFIELD: I notice that you were talking about the size of the

thorium. How long has that material been - will the records show -

CHAIR BEACH: That was just one operation. One separate from the redrumming.

DR. NETON: Which one?

CHAIR BEACH: The size. The particle size.

DR. NETON: I read that in a report. There's a report titled "Uses of radionuclides." I forget the author, but they talked about the particle size that were used to coat these microspheres that were 100 microns in diameter.

But those people were also presumably under the monitoring program as well. Because like I say, Mound is a little different in the sort of sense it's not quite in my opinion seemingly expansive as some of these other large DOE sites.

There were a number of buildings, but the operations were, I don't know, somewhat - not as many individual operations

going on at the time.

There were other campaigns with other small things, but I don't think anything like you would see at a larger, more national lab-type situation.

One of the reports -- I mentioned these quarterly Health Physics reports I think are particularly instructive. This is just out of a quarterly report that was issued in 1960 again.

And I'll read this section called Other Areas, which is sort of outside the polonium/plutonium ones. And this statement reads: The thorium redrumming work was undertaken again this spring. Approximately 2,500 drums of thorium will be redrummed yet this year. Work is being done in the area close to the railroad spur west of the oil pump house. A portable change house has been set up in the area. Personal monitoring will be carried out as in the past, you know.

So, there's clearly an awareness

of what's going on and an indication that workers were monitored. And in fact, we have 350 samples.

So, it's a nice, little tight package there, at least in my opinion, based on what I've read.

MEMBER CLAWSON: Well, that 250 samples -

DR. NETON: 350.

MEMBER CLAWSON: 350, excuse me.

It covers how many years?

DR. NETON: It covers a number of years out through -- maybe 20 years. Something like that.

The thorium, remember, we're only worried about thorium reconstruction necessarily after '58 and the material was actually put into the Building 21 configuration. I believe in '64 they actually dumped all the drums.

They got tired of redrumming, in fact. They redrummed all the drums three

times. And then they finally developed this - - it's sort of an igloo, in my opinion, like an open structure where they dump in all the drums into the igloo.

And after that point from the records I read, it pretty much sat dormant until 1975 when it was removed.

CHAIR BEACH: A company bought it or came in and -

DR. NETON: Someone bought it. And Gray and Associates was in charge of the shipping operations. I'm not even sure Mound was involved in the removal of the thorium.

So, there was about a period, you know, '58 to '64, six years or so where there was active outdoor -- well, actually it's probably late '50 to '64 active outdoor drumming in the good-weather months outdoors removed from the site - or onsite, but in a remote area of the site.

MEMBER CLAWSON: So, basically about five or six - well, six years of

monitoring for that.

I appreciate the paper that you read that this is how it is at the summary, but I'll always caution you of what goes out in the site is sometimes very different than the way it really did.

I hate to use that as this is how it was run, because today I still chuckle when I read the reports that go out.

So, I just caution some fan of, yes, that's the way it is.

DR. NETON: I appreciate that, Brad. I'm aware.

MEMBER CLAWSON: I know, and they didn't -- well, that's what we can go with.

CHAIR BEACH: Okay. So, SC&A's recommendation to the Work Group is to close this item. And I guess I'm going to throw that out to the Work Group what your thought is on that.

MEMBER CLAWSON: I don't have a warm, fuzzy feeling on it myself. I've

listened to too many different interviews that contradict what was said, but that's just my personal opinion.

My opinion is that is that they've got 350 - I'm still - the people, are they exactly called out who was actually involved with -

CHAIR BEACH: Brant said at the last meeting that they could identify the 20 people that did the redrumming effort. I do remember that from -

(Simultaneous speaking.)

CHAIR BEACH: Yeah, they have the samples.

DR. NETON: He identified 20 people, 20 claimants that had -

(Simultaneous speaking.)

DR. NETON: Out of the three hundred and 50 or so samples, I believe about a third of them were positive.

MEMBER ZIEMER: One third.

DR. NETON: Were positive.

MEMBER ZIEMER: That represents how many workers?

DR. NETON: I don't know how many workers. There were 350 samples. I didn't do that count. I was surprised there was that many positives, to be honest with you.

Thorium is - inherently it doesn't excrete very well from the body. Only about 10 percent, by the old models. I'm not sure about the new ones.

But anyway, so, there was clearly positive exposures measured from them.

CHAIR BEACH: I have Brant's report here. And he said this report presents internal dose estimates for 20 workers involved in the thorium operations at Mound.

DR. NETON: Right.

CHAIR BEACH: And I would tell you the date, but it is not listed, as you pointed out at the last meeting that it would be nice if NIOSH would put dates on these.

So, I guess this just goes back

to, I mean, this was kind of for all the internal - you might - you're not comfortable with it and I agree with that, but then where does that take us or where does that lead us for a recommendation?

I guess I would have to say that I would take SC&A's recommendation to close this. That would be my vote.

MEMBER ZIEMER: I would agree with that. This is a case where we have monitoring data, we have identified individuals, we have a description of the work site and the restrictions not entering and so on.

It's not like some of the others that we've had and I think it's Oak Ridge Hospital, where there's no indication that there's any control about who went in and out.

I mean, you can only speculate that someone might get past controls, but at least they existed here and it's much tighter than we've seen in many of these.

The SEC to me becomes very clear

if you can't show it, if there was some - at least a reasonable level of control.

I don't think it excludes - but the idea that anyone on the site is going to be wandering into this area.

And the only other thing is that on these with what apparently is in these drums, I'm not sure.

DR. NETON: Very high percentage of thorium by weight.

MEMBER ZIEMER: Was it?

DR. NETON: Yes, I was surprised it was that high. Not all of them. Some. There was a mixture, but I know a large number of them were monazite ores. I don't know if it was Brazil or India.

MEMBER ZIEMER: But I'm wondering if the ingestions were actually inhalations versus oral.

DR. NETON: That's possible.

MEMBER ZIEMER: If it's oral, you get a very different excretion pattern than

inhalation.

DR. NETON: The f1 value for thorium -

CHAIR BEACH: Well, I think in this case from what I remember from Brant's report, was that the redrumming was done in the summer months and that uses of respiratory equipment was maybe a little haphazard.

Sometimes they wore them, sometimes they didn't, based on how hot it was.

DR. NETON: How many were exposed?

CHAIR BEACH: And I think we captured those particular workers. I agree that based on the urine samples, I think what we were really grappling with was the ones that weren't within those 20 people and how do you pinpoint those.

DR. NETON: Well, there were more than 20 people that were monitored. I mean, the 20 that Brant selected -

CHAIR BEACH: Right.

DR. NETON: -- were just the ones that he picked out of the population to do some case studies.

Dr. Ziemer has a good point. Out of 350 samples, I don't know exactly how many workers that covers.

CHAIR BEACH: Right.

DR. NETON: Presumably it's more than 20.

MEMBER CLAWSON: Well, and this is - my concern with it was we have nothing to let us know that the hundred percent of the people over these time periods were done. We've got 60 different people, but -

DR. NETON: Well, again, you know, and you could argue they do follow their own procedures, but they set up a change house, they cordoned off the area.

I mean, when you have controls like that, it's a little different like Dr. Ziemer says -

MEMBER CLAWSON: And that's true.

DR. NETON: You're restricting people coming into the area. You know who's been in there. And at least there's a proviso in there that everyone working with radioactive material is supposed to get their sample done.

It can't be proved a hundred percent here, but it appeared to me that there was a fairly good for that time frame, health physics practices in place for this operation.

I've known a lot of people who worked with thorium early on and they had zero monitoring. It's unusual to see this many samples for a thorium operation.

MEMBER CLAWSON: And I agree because partly I think that Mound was the reason for a lot of this because the issues could -- have arose with the thorium.

DR. NETON: I mean, if you look at the report and personnel descriptions, I was actually impressed that for the quarter they had six man-months of bioassay support for

bioassay programs in that 1962 area, which I thought was pretty good for, you know, that time period.

MEMBER ZIEMER: Well, these people are for the most part already in the SEC unless they don't meet the criteria, right?

DR. NETON: No, prior to '58 they're in the SEC. We have an SEC through 1980, but they would have to have also worked in the SW building handling tritium.

MEMBER ZIEMER: Oh, okay.

DR. NETON: But as I -

MEMBER ZIEMER: You don't know that the -

DE. NETON: The early thorium activities - the thorium program started in the mid-1950s and there was an intent to make like a pilot plant to purify the thorium.

That only lasted less than a year, I believe. So, that was sort of a chemistry pilot plant operation and then the project was terminated.

And so, the only activities associated with thorium here would then be the redrumming operation.

MEMBER ZIEMER: right.

DR. NETON: They weren't doing the processing of the thorium as they had intended to in the early years.

MR. KATZ: I think we need Brad and Phil's final words on this.

MEMBER CLAWSON: You know, it's fine with me. I just, you know, I'm not always going to feel a hundred percent good on it. I have no problem with closing this following SC&A's request.

I just wanted it to go on the record that I don't - I personally really don't think it's that clear-cut, but I'll go with the rest of the Board.

MR. KATZ: Phil.

MEMBER SCHOFIELD: I'm good at this time.

MR. KATZ: Thank you.

MEMBER ZIEMER: I think you sort of weight where it is. Is it past the tipping point in your mind. For me it is past the tipping point.

MEMBER CLAWSON Right. And I understand, Paul, and I'm not questioning it.

CHAIR BEACH: That's kind of where I'm at too and you can't, I mean, where would you go from here? That's what I was grappling with.

So, Phil, what were you saying?

MR KATZ: Phil said okay.

CHAIR BEACH: Phil, okay. Okay. So, thorium then will stay as closed.

And, Jim, that takes us to - you said you had a report on polonium.

DR. NETON: I'll be very brief.

CHAIR BEACH: No, please take your time.

DR. NETON: Let me take the easier one first. The two years where we're missing polonium logbooks, we are actively in the

process of developing an 83.14 for that.

CHAIR BEACH: Okay. You're talking the radon.

DR. NETON: The radon, right. I forget which two years those were.

CHAIR BEACH: I have it right here. So, let's make it very clear because I think one of the reports was incorrect for radon.

It is -- the missing logbooks were September 1st, 1972, through December 31st, 1972. And for January 1st, 1975, through December 31st, 1976.

Because I think the other report just says '77, which was wrong. So, okay. Thank you.

DR. NETON: And I spoke to LaVon Rutherford who is the keeper of the SECs. And he indicated to me that we intend to present this at the September Board meeting to be the next Board meeting after -

CHAIR BEACH: So, an 83.14 for that time period.

DR. NETON: An 83.14. It would be all workers who worked in those two time frames would be eligible to enter the SEC because we can't - we have no definitive way of documenting potential for exposure in the SW building.

As far as the early period for polonium between February '49 and September '49, this is the era when Monsanto transferred polonium work over to Mound.

CHAIR BEACH: Right.

DR. NETON: And it perceived the initiation of the SEC Class at Mound. We intend to add that piece, but I checked last week. And as of last week we have no claimants that are affected by this.

CHAIR BEACH: Okay.

DR. NETON: So, you know, we will be monitoring for what we consider to be a litmus case, someone who would be eligible to file for an 83.14.

We keep our eyes open. I think

there's probably computer checks as they come in to make sure we don't miss someone.

But at such time as we get a case in, this will remain suspended until -

CHAIR BEACH: So, are we talking about an 83.14 that will -

DR. NETON: It would be an 83.14, but we can't proceed unless we have a case that's affected.

CHAIR BEACH: So, is there going to be any - I don't know. We're probably dealing with survivors possibly in this case.

DR. NETON: That's true.

CHAIR BEACH: So -

DR. NETON: You mean an advertising campaign or something of that nature?

CHAIR BEACH: Yes.

DR. NETON: We haven't done that. We don't normally advertise. We could put the word out through the Board, notify - I have notified the Department of Labor about our intent to add an 83.14 for the logbook error.

I did not bring up with them the polonium error. I can mention that in our interagency calls and ask them to distribute that information however they can.

We just sort of let our ombudsman know, 'identifying information redacted', of our intent to be soliciting --

MEMBER ZIEMER: What are the years on that?

CHAIR BEACH: February 1st, 1949, through September 30th, 1949.

And then the other question is, is that going to be a Monsanto or a Mound?

DR. NETON: That would be a Mound.

CHAIR BEACH: It would be a Mound.

DR. NETON: Mound was in operation and they transferred that to Monsanto - or Monsanto transferred that operation to Mound.

CHAIR BEACH: Okay.

DR. NETON: And there's no reason to believe that the procedure was any less messy than it was at Monsanto when it

transferred at that time period. They were irradiating slugs and dissolving them.

The other thing that was going on at Monsanto, as I mentioned a few meetings ago, is Monsanto is becoming a DOE facility.

CHAIR BEACH: Right.

DR. NETON: There's also a campaign out to - Jenny, correct me if I'm wrong, but I think we need to do an 83.14 there to solicit anyone - well, there's a potential Class of workers out there who were contractors that had worked at Monsanto who were not eligible for the SEC at Monsanto by nature of it being a DOE facility.

So, an 83.14 could be done to recruit - we can't just change the Class definition. There's a Class out there already as an AWE. Now, it would have to be a DOE Class.

And so, we're working on that aspect as well. So, there's a few things in the early periods that are going on.

CHAIR BEACH: Okay.

MR. FITZGERALD: You mentioned just to go back, you mentioned the logbooks in the context of SW.

Are you talking about the - anyone who got a tritium bioassay, it wasn't to SW per se, was it?

DR. NETON: Well, what we're saying is we don't have any logbooks for tritium monitoring in those years. So, you have no way of establishing if they worked in the SW building or not.

MR. FITZGERALD: Well, I'm just saying, though, the Class Definition was broader than SW. I think it was fall of - or anyone who got a -

DR. NETON: Yes.

MR. FITZGERALD: Right, right. When I heard you say SW, I wasn't quite sure if -

DR. NETON: Well, what I meant was that anyone - we don't know who had tritium bioassays in those years.

MR. FITZGERALD: Right.

DR. NETON: So, therefore, everybody on site is in the Class -

MR. FITZGERALD: Right.

DR. NETON: -- by definition.

MR. FITZGERALD: Right.

DR. NETON: But you're correct. There was another building that had tritium samples that was sort of brought into the Class.

(Simultaneous speaking.)

DR. NETON: So -

MR. FITZGERALD: Okay.

DR. NETON: It gets confusing at times.

CHAIR BEACH: It does.

DR. NETON: Because there's tritium samples to cover radon exposure. I mean, that right there tells you how confusing --

CHAIR BEACH: So, this will be all workers.

DR. NETON: All workers who were

onsite during that time period.

CHAIR BREACH: So, I would suggest that we take our lunch break and then give anyone on the phone a chance if there's any public comments before we get into Site Profile - or maybe we should - is there anything else with data adequacy?

We do have that list May 29th that NIOSH sent out. And I was trying to go through it briefly to see if there was anything missing.

MR. FITZGERALD: That's going to get into Site Profile issues.

CHAIR BEACH: That's what I suspected.

MR. FITZGERALD: That would be better after lunch.

DR. NETON: I think we should ignore NIOSH's response in those areas because they're redundant to what's going to be in -

CHAIR BEACH: In the Site Profile, okay.

DR. NETON: We were somewhat confused as to which were SEC and which were -

CHAIR BEACH: Absolutely.

DR. NETON: Site Profile issues. But I think if we go over this entire list, it's going to take a little while especially because I need to refresh my memory on some of these. I wasn't intimately involved with these as much as I am going to be now.

MR. FITZGERALD: We can use this as an opportunity to do that because I think some of us haven't actually looked at these in a couple of years either.

CHAIR BEACH: Right, right.

DR. NETON: This just came out recently. I haven't looked at the gamut of the issues in a while.

CHAIR BEACH: Okay. So, essentially other than the tritide issue, we have cleared up all the SEC issues.

And can I ask a time frame? You knew I was going to ask that on the tritides.

DR. NETON: Can I get back to you on that, because I have not worked with this - I don't know what's on people's plates and I know this is a working product of one of our contractors. I can't speak for their time frame.

CHAIR BEACH: Right.

DR. NETON: I don't expect it would take long, but I'll -

CHAIR BEACH: Okay.

DR. NETON: -- take commitment. It's one of my action items to get back to the Working Group within a week or so with a time frame. I just want to get a chance to talk to the people -

CHAIR BEACH: Sure.

DR. NETON: -- that are actually going to do the work.

CHAIR BEACH: Okay.

DR. NETON: I'm very good at giving short commitments and then learning that -

(Laughter.)

CHAIR BEACH: Right, right. And we'll revisit recommendations and timing and stuff after lunch.

So, let's go ahead and break for an hour. Perfect timing. 12 o'clock.

MR. KATZ: So, thank you, everyone.

(Whereupon, the proceedings went off the record at 12:56 p.m. for a lunch recess and went back on the record at 1:03 p.m.)

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

1:03 p.m.

MR. KATZ: All right. We are back after lunch. This is the Mound Work Group of the Advisory Board on Radiation and Worker Health.

Let me check on the line and see, do we have you back, Phil Schofield?

MEMBER SCHOFIELD: Yes. Yes, you do.

MR. KATZ: That's great. Thank you.

And while we have everyone else on the line at the outset, let me remind you again we had a lot of problems with people carrying on conversations on non-muted phones during the morning session. So, please, everyone, basically everyone except Phil and the SC&A staff, should have their phones muted. And if you don't want to mute your phone, then just cut out when you want to have a discussion, and dial back in.

And to mute your phone, you just

press *6. Every phone has a *6. Press *6. And then to unmute it, press *6 again. Thank you.

And, Josie, it's your meeting.

CHAIR BEACH: Okay. So, where we are in the agenda is Work Group recommendations. And the only thing I want to say about that is we already closed out thorium. We know where we are with the tritide.

As far as this Work Group is concerned, I will do a presentation at the Board meeting in June in Santa Fe and basically just lay out what we've done over the last four to five years.

I did a presentation, I was trying to remember the date, but it was quite extensive a year or two ago, also.

And I'm hoping, and of course Jim is going to give us the okay on that, is to report out on the tritides issue in September at our next Board meeting after this one in

June. So, that's what I'm hoping and shooting for.

DR. NETON: I'm going to report out, or --

CHAIR BEACH: No, you're going to tell us when you're going to give us the tritides model. So --

DR. NETON: I should have that date before the June meeting.

CHAIR BEACH: Hopefully, yes. And then we'll decide on the next Work Group meeting and -

MR. KATZ: But our aim would be to be done before the September meeting. That's what Josie is saying, Jim.

CHAIR BEACH: The final --

MR. KATZ: So, our aim would be to be -- to wrap up tritides before the September Board meeting.

DR. NETON: I would hope so.

MR. KATZ: If that's possible.

DR. NETON: It doesn't seem to me

to be that complex an assignment on our part.

CHAIR BEACH: Right. Okay. So, that's just the star I'm shooting for basically.

Okay. Is there anyone on the phone that has any comments or questions, would like to make any comments?

(No response.)

CHAIR BEACH: If not, then the next item on the agenda is the Site Profile issues. I asked SC&A just to give us kind of an updated Site Profiles matrix.

The reason we're going to do that now is just to kind of go through the items and get some clarification. We're not going to solve anything, I don't imagine, today, but just to rehash the past four or five years and see where we are with the Site Profile matrix to move this forward.

MEMBER ZIEMER: And in that connection, what is the latest version of the matrix?

CHAIR BEACH: Joe --

MEMBER ZIEMER: The 26th. May 26th. Let me double-check here.

DR. NETON: I can send it to you.

MEMBER ZIEMER: No, let's see. It was sent out on the 25th?

DR. NETON: Yes, I believe so.

MR. FITZGERALD: Yes, pretty close.

MEMBER ZIEMER: I must have moved it into my Mound file. Let me see.

CHAIR BEACH: If you're like me, you have two Mound files.

MEMBER ZIEMER: Oh, wait. I'm actually looking at the wrong file. I'm looking at the inbox instead of the Mound file.

CHAIR BEACH: Okay. And, Joe, are you going to --

MR. FITZGERALD: Yes, let me give a little background. This is really two efforts.

One, you know, we did a status summary of outstanding issues associated with the internal dose side of things. That was back a couple years ago: October of 2010.

That was when we were combining all these White Papers and all these issues into a consolidated internal dose item. And I think at that time the Work Group asked for, you know, what is the status of all these.

So, we didn't lose anything when we consolidated all these White Papers and we came out with that matrix. And I -- and we never did anything really with that.

We got involved with the -- just closing out SEC questions. So, that sort of stood as a status that was two years old essentially.

So, I started with that and added to it the items that fell out of each of the SEC discussions. There was a number of SEC discussions where certain things were put in the parking lot, so to speak, as likely Site

Profile issues and kind of verified that through the transcripts and made sure that we didn't lose anything in that process.

So, combining those items with what was in the October matrix is the source of what you see today.

Now, saying that and I think as Jim and I have discussed, it's kind of complicated in the sense that there were issues raised and the response to the issues sometimes were broader than the questions raised. Sometimes they dealt with a couple of the questions, that kind of thing.

So, when we go through this, some of the clarification's just to figure out if in fact some of these have gone away by virtue of the broader treatment of the issues, but some of the specific ones stand as outstanding Site Profile questions.

So, what you have is a combination of what's come out of the SEC discussions, and also what came out of the consolidation of all

those internal dose White Papers, which were quite a few of them.

And as I said earlier, this is -- I'm sure this could stand some scrutiny. And that was the intent was to give the Work Group and all of us a chance to go through this and just make sure that this is a reasonable baseline.

CHAIR BEACH: Joe, let me be clear. The earlier one you were talking about, was that the actual Site Profile matrix, the one that came out March 10th, or is that something different?

MR. FITZGERALD: Well, back -- there was an October 2010 document called Mound Internal Data Adequacy and Completeness, Issue Status Report. And I think the concern there was that, because we were going to consolidate all these different internal dose-related issues into one, which was this omnibus internal issue that the Work Group was going to deal with, your concern was not to

lose anything in that process. Because, quite frankly, these White Papers had a combination of clear SEC questions, as well as issues which were clearly Site Profile in nature pointing out perhaps inaccuracy questions, questions of consistency and those that are clearly more Site Profile-related.

So, we wanted to sort of divide that up and we have since dealt with the SEC, central SEC questions, but trying to pull out all those Site Profile questions is what we did in that October 2010 document. I have a copy of it, by the way.

And that's -- you know, the internal piece of this matrix, you know, leans heavily on that piece, but I'll be the first to tell you that again is a combination of clear SEC issues -- I'm sorry -- clear Site Profile issues, the ones which are sort of in between.

So, rather than making the judgment on priority to leave stuff out, I've

left more in. So, some of this is going to have to be one where we distinguish, try to go through and figure it out.

CHAIR BEACH: I'm sure it wasn't an easy task.

MR. FITZGERALD: Yes, it was a little complicated, but mostly on the internal side, I might add. The rest of it was much clearer. Internal was a bit of a nightmare.

So, do you want to go through these and just --

CHAIR BEACH: Yes.

MR. FITZGERALD: One by one?

CHAIR BEACH: Yes, if that's okay with the rest of the Work Group.

MR. FITZGERALD: Starting with Issue 5, this is one of the earlier ones, plutonium-240, -241 in which we closed out. However, there was an action. This came from transcripts for NIOSH to confirm the bounding intake for Pu-241. It was just a to-do that was in the transcripts.

And other than that, I tried to give a little bit of a picture of the issue that was the basis for the action being levied by the Work Group. And that's what the basis and source means.

In terms of plutonium-240 and -241, that particular issue, that was closed out as an SEC issue, but the Work Group agreed that there was some question about discrepancy and the relative concentrations of the isotopes, the 240, 241. And NIOSH offered to confirm the bounding intake for 241 that would be in fact used in the dose reconstruction program.

And that would be included in the TBD if it weren't there, and I guess it wasn't there.

DR. NETON: From our perspective, I can only agree that we will pursue this and close it out.

I don't know that anything has been done. It may have been worked on and

closed, to my knowledge, but I haven't had time to go back and look at it.

But this has to do with the amount of plutonium-241 that could have been the isotopic mix. Could have been -- looks like it could have been higher 241 which would I guess increase your accumulation of americium-241.

MR. FITZGERALD: Yes, I think it was just the detail that --

DR. NETON: I can just accept this as an open item on our part. And like I mentioned earlier, I'll be taking this back to the Working Group -- I mean the Site Profile folks who handle these type things and go over this list. And then hopefully we can get some sort of a time commitment.

This will go onto our -- what we call our Gantt chart -- actually, we don't call it the Gantt chart. We call it our tracking matrix.

MR. FITZGERALD: And as far as

background and the source of the item, I tried to be very specific about the transcript itself and the reference and the page numbers.

CHAIR BEACH: Yes, I was going to comment on that.

DR. NETON: That's very good.

CHAIR BEACH: That is good.

MR. FITZGERALD: So, you can go right back to the actual citation and find that particular loose end.

Okay. Well, the sixth one is tritides. And the only issue there is something that came up, actually, several years ago when we got into this distinction between hafnium and the insolubles and the intermediates.

And a comment was made that there was a lot more intermediates that were being handled at Mound than -- actually, the hafnium was a small fraction of what they actually dealt with.

Then the question came up from the

Work Group, well, these are clearly not as insoluble as hafnium. But nonetheless, you know, would there be any need to perhaps apply a solubility factor beyond what is being added now?

And I think the offering was NIOSH was going to look at that and see if it was necessary to include that into the revision. And that was the item.

DR. NETON: It seems like that right now we've got a situation where we've bounded the extremes, the very soluble and the hafniums. And I'm not sure what benefit there would be in adding this intermediate Class of which we would not know the fraction anyway.

So, I don't really see a need at this point to do that unless I'm missing something. But it's either -- again, we have the two extremes. I don't know that it would make any difference in our dose reconstruction.

I do vaguely recall, though, that

there was a -- some of these nuclides did not have a determined solubility class. And there was some research being done at the time by -- I want to say Savannah River was contracting Lovelace or -- I try -- to do some solubility studies, but that's all I recall.

MR. FITZGERALD: And I think the other issue, and again the transcript discussion's illuminating, you know, it's -- there are some extremes. But I think in terms of the intermediate, some of them clearly aren't hafnium, but they do have -- it's a continuum and they do have --

DR. NETON: Right.

MR. FITZGERALD: -- a degree of insolubility which it would look -- it would be useful to see whether or not any adjustment would be claimant-favorable or not.

I don't know. I think we left it that way.

DR. NETON: I don't think so because, you know, I looked at the lung model

this morning to confirm that the tritium lung model -- the way we handle tritium is the matrix is inhaled, the metal that the tritium was bound to, we actually account for the tritium dissolving off of the metal and becoming systemic.

And so once that happens, then all you would do is reduce the lung dose if you had a more moderately soluble material. So, I don't see that it would really affect anything.

But I'll tell you we will take that up, we'll go and run that to ground and just respond to it.

CHAIR BEACH: Yes, I was just going to suggest that.

DR. NETON: To get it in writing or in a more formal piece of communication.

MR. FITZGERALD: And whether there's any particular, whether it's titanium or some of these that fall just short of hafnium whether there be any value to applying

it, because I think that it is a bit of a continuum for some of these. It's not one over the other.

DR. NETON: We'll put some kind of formal response --

MR. FITZGERALD: Titanium is one that comes to mind, but there may be some others that fall in that upper range.

Okay. This is an old favorite, Issue 9. Brings back fond memories. The high-fired Pu-238 and Type L excretion model.

And that was simply -- I think we -- after we kind of banged that thing down, I think NIOSH agreed that, okay, there might perhaps be a Type L that might come up on occasion, but we always have those excretion curves if we need to. And we will apply it if the phenomena shows up.

And I just put that down as a -- just to acknowledge that that was the commitment to add a Type L and make it available through dose reconstruction if in

fact that phenomenon shows up in terms of the urinalyses results that they kept. I think that's how we left it.

DR. NETON: I think I remember recently adding a proviso in the Site Profile indicating that this type possibly does exist and we're aware of it.

MR. FITZGERALD: Yes.

DR. NETON: Don't try to force it in one of our standard models if it doesn't seem to fit the basic.

MR. FITZGERALD: Right. Exactly.

DR. NETON: Strangely, I do remember that.

MR. FITZGERALD: Right. So, that was kind of it and there was a couple of cases that we conveyed it back and forth.

MR. KATZ: So, that sounds like an issue that's, in effect, in abeyance. It just hasn't shown up in the TBD.

CHAIR BEACH: Yes, and there's several meetings and then I know that you

wrote up kind of a status in the matrix.

DR. NETON: Yes, I would want to go back and review the material. We're all based on recollection here.

CHAIR BEACH: Yes.

DR. NETON: So, it's better to go back and, you know, it's going to take some work on our part to go and more definitively outline what -- who said what and what we're going to do. It's got to be done.

DR FITZGERALD: Okay. Those are the easy ones. Now, we get to internal dosimetry data completeness: 11, 12 and 13. That was consolidated.

On A, uncertainties and low recovery for polonium bioassay procedures, I think that's one where I would say that would be one of the things to take a look at specifically. I don't know.

It would be a value to go through and repeat some of the discussions that we've had. But the citation that's in Section 3.1,

that's one of our White Papers.

So, I went back and looked at the NIOSH White Papers that came back in response and did not see that treated specifically. It was in broad responses, but I think that would be one where -- unless you could find the particular citation.

DR. NETON: I might have it right here, actually.

MR. FITZGERALD: Okay.

DR. NETON: Section 3.2.

MR. FITZGERALD: Do you have that one?

DR. NETON: Yes.

MR. FITZGERALD: Which response --

DR. NETON: Well, you list it as Section -- this is the 2009, April 2009 NIOSH Internal Dosimetry Data Completeness.

MR. FITZGERALD: Yes, 9A.

DR. NETON: The polonium response I have is Section 3.2, not 3.1. Maybe this is not the right one.

MR. FITZGERALD: Let's see.

DR. NETON: This talks more about the availability of records and not the recovery. So, that was just fortuitous that it seemed to line up.

Never mind. That's not the one.

MR. FITZGERALD: Yes, this is the section that's labeled Uncertainties in Load Recovery for Polonium Samples.

DR. NETON: Which one is that?

MR. FITZGERALD: This is on the April 2009.

DR. NETON: Okay. There were two pieces here.

MR. FITZGERALD: Right. I cross walked it with the responses we've gotten afterwards. And there were general responses, but that specific question I couldn't find in the -- but granted there's a lot of paper that came afterwards.

So, I went through and didn't find it. But if it's there, then that's fine. We

can go ahead and put that down.

DR. NETON: Yes, we can address this. This is a matter of what recovery is used.

MR. FITZGERALD: That reference is correct. It's 2009,A, Section 3.1.

DR. NETON: Yes, it's the other document. There's a completeness, and then there's an adequacy.

MR. FITZGERALD: On B, I got your response on that and actually I went back to double-check that and I think the first two bullets are responded to in the general framework.

I mean, you almost have to step back because those issues are a little broader, are answered by the White Papers, but not specifically, but in general on this question of 95 percent of the data was found for selected individuals collected in 1990 and later, you know. This gets to the gross alpha, gross beta.

DR. NETON: Right.

MR. FITZGERALD: Sort of radionuclide-specific versus gross alpha and -

DR. NETON: Yes, yes.

MR. FITZGERALD: And when I went back and thought about it and read that thing through, I said, okay, this is really that issue and we are pretty much satisfied on that.

And the same thing with the next one that the majority of pre-1990 results again even though the original White Papers focused in on radionuclide-specific, I think as the dialogue went on we accepted the gross alpha and beta. So, those issues were responded to.

Now, the next ones I did not -- these were a lot more specific and I think clearly were Site Profile in nature to begin with in terms of the units and the radionuclides didn't match.

DR. NETON: Right.

MR. FITZGERALD: The volume corrections were not possible in a number of the cases that were identified on that particular element from Section 3.6. I think that's clearly a Site Profile and how we'll improve the document type of thing.

On C, this was actually a collection of what I would consider Site Profile-specific issues. When Kathy was going through and doing a QA on the database, she was finding these discrepancies.

The question is are they, you know, are they ones that require any kind of adjustment in this TBD or some kind of explanation, or they're not significant or not, but these were sort of her QA check of the database found basically questionable issues or issues that raised some questions. And this is in Section 4.6.

There was a broad response. But I didn't in reading that response, find a -- again, because these are Site Profiles, didn't

see a specific response to some of these discrepancies that were raised.

So, again I went back and checked that given your -- when you came back with your response.

DR. NETON: Right.

MR. FITZGERALD: So, anyway, that can be found in this 2009 C, which is a different White Paper. That's why I said the crosswalk is important. That's where the reference is.

2009 C, this is -- this is the QA document, Mound Internal Dosimetry Data Quality Assurance. That's April 2009. Same dates.

There are three documents of the same date just to make things more complicated. One was Internal Dosimetry Data Accuracy, the other was Internal Data Completeness, and the third was Dosimetry Data Quality Assurance.

DR. NETON: And this had to do, I

think, with the MJW.

MR. FITZGERALD: Yes.

DR. NETON: MJW did their post -- pre-1986 or whatever dose reconstructions.

MR. FITZGERALD: Right.

DR. NETON: And Kathy identified some issues with the data in the database. And so --

MR. FITZGERALD: Yes, it was the question of if one is going to rely upon that MJW evaluation, should one reflect the fact that there were some issues that I think MJW itself raised.

A lot of these weren't issues that we originated. These were issues that MJW acknowledged in their report or were issues that they had dealt with.

So, I think the question in general was how does NIOSH see the report, MJW database, given some of these issues or questions.

DR. NETON: Right.

MR. FITZGERALD: How do you reconcile those issues in terms of making use of that database?

And that was the broad issue that she raised and these are -- these are actually more specifically some of the illustrative examples of things that she thought NIOSH should treat in its TBD or at least acknowledge.

DR. NETON: Don't disagree.

MR. FITZGERALD: Okay. On D, I think this has been addressed already, tritium, missing tritium logbooks for --

CHAIR BEACH: The only thing I want to point out here is that the dates are wrong.

MR. FITZGERALD: The dates are wrong, okay.

CHAIR BEACH: Yes.

MR. FITZGERALD: I carried this over. So, I guess we got that wrong. '72 and '76.

CHAIR BEACH: Yes, December '72

through '75, and then -- or excuse me -- '72 through, and then '75, '76.

MR. FITZGERALD: '75, '76.

CHAIR BEACH: Do you want the exact dates, Joe? September 1st, 1972, through December 31st, 1972. And then January 1st, 1975, through December 31st, 1976.

MR. FITZGERALD: 1976, okay. Yes, we've carried that over for a couple years now that way.

Okay. But anyway that's -- I think that's encompassed by the action that's being addressed. So, I don't know how you want to treat that. You can maybe remove it from the Site Profile list as that's being addressed explicitly.

Moving on to E, tritium, this gets to tritium bioassay in general. There were a couple of issues in two different reports dealing with the early dose calculations in terms of algorithm and compounds.

Now, compounds other than HTO is -

- that drops out. Okay. That's tritides, basically. So, it's just the -- it's the first issue in that particular session.

DR. NETON: I'm not sure why we don't have an algorithm for those calculations. I'm not sure what --

MR. FITZGERALD: Yes, I think it goes with the context in the actual report

DR. NETON: I'll have to look at the document. For tritium HTO we definitely have algorithms. I don't know what this is.

MR. FITZGERALD: This is -- becomes the early dose calculation. It may have to do with the availability of the data there.

DR. NETON: Or maybe the Mound calculations that calculated the dose, we don't have the algorithm, but we're not using that.

MR. FITZGERALD: Well, that may be the response. I mean, some of these like this one in particular came from that early October 2010 listing. So, that may have been

responded to.

MEMBER ZIEMER: Excuse me. What's going to happen on D; did you say? I mean, sorry to back up a minute. On those missing logbooks, what --

MR. KATZ: Where at?

(Simultaneous speaking.)

MR. FITZGERALD: So, that would disappear from our Site Profile matrix as something that's being addressed explicitly.

CHAIR BEACH: The answer, too.

MR. FITZGERALD: Yes.

MEMBER ZIEMER: Yes, that is.

MR. FITZGERALD: Two years ago, it was sort of an open question. That's been addressed.

MEMBER ZIEMER: All right.

MR. FITZGERALD: E, I think, is a matter of checking back. This was an early finding that we're not too sure about, but may very well have been addressed along the way as well on tritium bioassay data accuracy in the

early years.

So F, plutonium data comparison, this is another Site Profile question that was embedded in some of the analyses and again a question of some gaps in the sources as far as information for dose reconstruction for claimants essentially.

The same thing with G for polonium, and this came from a data completeness review and raising questions about potential gaps.

DR. NETON: Yes, I don't know if this has a gross alpha issue with it or --

MR. FITZGERALD: I didn't get the sense. Like I said, I have the documents right here. We can go back and check, but I think this is different from that.

DR. NETON: Okay. I'll look.

MR. FITZGERALD: H gets into fecal bioassay data, the question of -- this is going back quite a ways now. Few results in PURECON, poor overlap in logbooks, notion of -

- I'm not even sure what --

(Simultaneous speaking.)

MR. FITZGERALD: Right, right.

MEMBER ZIEMER: You're not using it anyway, are you?

MR. FITZGERALD: So, that may be the answer to the observation in the data completeness review.

I, again on tritium data comparison. Two individuals from the data completeness evaluation -- this is the evaluation of a sample of the claimant database that had bioassay data not reflected in the MESH tritium database. Again, sort of a very specific sampling issue that was done.

And I think because it was a limited sampling, the question was does this reflect a broader question --

DR. NETON: Now that we've reproduced the entire set of tritium logbooks, I think that this might be addressable. We'll look.

MR. FITZGERALD: J, again this -- I don't know if there's anything we did not address, but the thorium bioassay data -- yes, this is a little different than what we just did. This is more Site Profile in nature in terms of procedures and the uncertainties.

This Super S or YY thorium is one that's come up before. In that particular case, I think --

DR. NETON: YY is the first time I've seen it.

MR. FITZGERALD: Yes, I know. I'm saying it should be Super S maybe. But there was one scientific paper, I think, that was raised in one of the White Papers saying this sort of broaches this question.

And the response was, well, but the authors sort of downplayed it because there was a limited sampling where they found this phenomenon.

And I think the NIOSH conclusion was, well, because it was qualified that way,

that you couldn't -- you couldn't really use it as a reliable source for this question of whether this was a prevalent issue or not.

This seems to keep coming up and I don't --

DR. NETON: We just responded to this for another site last week.

MR. FITZGERALD: Yes, I'm not quite sure. It almost is more smoke than fire. But anyway this came up in the White Paper, that one should at least address whether or not the Super S thorium, the high-fired thorium was a dosimetry question.

DR. NETON: I think we just addressed this very issue at another site. I remember looking at it and we'll just incorporate the --

MR. FITZGERALD: It's an old question. It was one that came up two years ago.

Anyway, that's all contained in this one section. These are issues that are

clearly not the same issue we just discussed, but ones that have the thorium bioassay in general.

And that's it for internal dose. This carries over from what was generated two years ago, updated it, tried to weed out as much as possible things that were covered in the SECs.

CHAIR BEACH: So, we didn't have anything on exotics? Nothing that would have been a Site Profile nature?

MR. FITZGERALD: No.

Now, keep in mind the exotics figure prominently in the SEC discussion. I mean, in the memo from January, it says right here, deals with the exotics and the fact that, you know, after much hand-wringing one could not figure that out.

(Simultaneous speaking.)

MR. FITZGERALD: I think that was -- that was a large part of the discussion on the consolidated internal issues that the memo

addressed.

CHAIR BEACH: Yes.

MR. FITZGERALD: Nothing left from that, but these two are companion pieces. Because in essence, this hands off to what Site Profile issues are left.

The matrix that was attached to that memo in January is in essence this list from the internal side.

Okay. On neutrons, Ron, did you ever -- Ron Buchanan, are you still here?

DR. BUCHANAN: Yes, I'm here.

MR. FITZGERALD: My God. Okay. Actually, I forgot that you had -- I had Ron take a look at -- because he had been very much involved in the back and forth on neutron issues, to try and scrutinize what would be left on that.

DR. BUCHANAN: That goes way back.

MR. FITZGERALD: That goes way back, but also there were a number of Site Profile questions that were parked because of

discussions on the -- remember the 12-inch MCNP and all that?

DR. NETON: Oh, yes.

MR. FITZGERALD: So, Ron is the reservoir of that institutional memory. So, I'm going to rely on him to walk us through that portion.

DR. NETON: That's when I was still young.

DR. BUCHANAN: Okay. Well, you know, we addressed the common problems on neutron monitoring and NTA film. And we came to a solution where the threshold issue and NIOSH did some MCNP calculations, SC&A did some, we discussed them and we came out in agreement to incorporate those correction factors in the recorded neutron dose to compensate for the neutron dose missed because of the threshold of the NTA film.

And so that's A, Item A under Number 15. And so, we came to agreement on that. We just need to have that incorporated

in the revised TBD.

And some annotations follow at the bottom of the page there or a couple pages down that explains the interchange of papers. There's quite a few papers went back and forth between NIOSH and SC&A. And it was discussed in several of our Work Group meetings.

So, you know, I don't think that we have further discussion on it. It just needs to -- we just need to see it in the revised TBD.

MR. FITZGERALD: As Ron pointed out, we did put a couple, two, three pages of annotations in the back of this matrix just to try to reconstruct the history because it's a little hard to understand unless you know the history. So, that's what that is.

DR. BUCHANAN: So, Jim, is that your understanding as to --

DR. NETON: Yes.

DR. BUCHANAN: -- correction

factors will be in the revised TBDs?

DR. NETON: Yes.

DR. BUCHANAN: Okay. So, Item Number B, Item B is that neutron-photon ratio values were not consistent.

In the first TBD-6, the N over P values in one place is two-to-one, in another place is one-to-one.

And so, again that's -- the action item on that was to get the appropriate value in the revised TBD.

Okay. And then Item C, this was a quality factor in the original TBD. And the values listed in that came from Meyer's work notebook and such papers, but they listed variations in the number of neutron flux that provided 300 millirem per week.

And the first explanation was that, well, if you had a 40-hour week or 50-hour week or you had one calibration source or another. However, if you went back and looked at the calculations, we see that this wouldn't

account for such a wide swing in the number of neutrons that created the weekly dose. It ranged from 30 to 250. So, that's a factor of five between 1947 and 1969.

And so, what needs to be done now? Perhaps this doesn't affect the way NIOSH creates, reconstructs the dose and that's fine, but we need to document it that NIOSH uses a method that doesn't depend upon those conversion factors. Or if it does, that it comes out in the wash. It comes out correctly.

And so, that was an issue that needed to be addressed and I assumed it would be either responded to or in the revised TBD to correct that.

DR. NETON: Now, does this have a - does this have a reference where we can look?

DR. BUCHANAN: Well -

DR. NETON: Some of these are - I'm going to have to go back. I mean, the other ones had like sort of a reference of where the

issues came from.

MR. FITZGERALD: The annotations do, I think.

DR. NETON: Do they?

MR. FITZGERALD: Ron, your annotations, do you indicate or identify the source? It looks like you do.

CHAIR BEACH: He does.

MR. FITZGERALD: Yes, in the annotations.

DR. NETON: Yes, yes, under neutrons.

MEMBER ZIEMER: I don't understand the concern about Item C. I mean, those numbers vary with the energy of the neutrons.

Is the question here that we don't know the energies, or what was -

DR. BUCHANAN: Well, the original TBD in 2004 states that Mound Lab used between 30 and 150 neutrons centimeters squared per second per 300 millirem per week between 1947 and 1969.

And the question was, why did it fluctuate back and forth? And it fluctuated several times in there -

MEMBER ZIEMER: For a specified energy or -

DR. BUCHANAN: Well, that's what we're trying to find out is -

DR. NETON: There was a difference in source term or the energy of the source term or -

MEMBER ZIEMER: The conversion factor, I mean, you can find tables of these. And they go from about 30 for real fast, up to, I don't know, over a thousand. I don't remember the number, but this looks like it's an energy-dependent issue.

DR. NETON: And we'll take a look at it. I don't recall this one at all, really.

DR. BUCHANAN: And the point is, you know, if you're just quoting what Meyer had in his document, but it doesn't influence

the dose reconstruction, then it just needs to be explained that way in the revised TBD.

DR. NETON: We pull out - the quality factors were actually pulled out, and then added back by us because the modern quality factors are not reflective of the historical quality factors.

So, I've got to look and see how we dealt with this.

DR. BUCHANAN: Okay.

DR. NETON: And then actually they get pulled out again in a distribution assigned in IREP.

DR. BUCHANAN: Okay.

DR. NETON: I'll look into it. I don't recall this one very well at all.

DR. BUCHANAN: That was one of the original ones way back. A number of years ago.

Okay. And then Item D, which was NTA film fading, and we had a lot of discussion, probably too many to keep track of, on this issue. And it got included in the

Monte Carlo correction - not in the correction factor, but was being addressed at the same time. And we came up with the agreement of when it would be applied and - as such as illustrated in the annotations.

The original TBD addresses on Page 30, it recommends 33 percent fading per week and 56 percent for two weeks to NTA film between '49 and '76.

However, then in the SEC evaluation and in a 2009 paper it says, okay, we'll do fading correction at nine percent a week.

And so, you know, we agree with the original TBD and to apply that fading factor, because that came directly from Meyer document.

The nine percent came from a related document, but it wasn't really Meyer's work.

And so, what we would like to see in the revised TBD is the original value and

not have it changed to nine percent.

MR. FITZGERALD: Okay.

DR. BUCHANAN: Okay. And then Item E was the coworker database, okay, for people that didn't have neutron dose recorded that meets the assigned neutron dose.

There was a coworker database that was created using categorical data in one of the papers referenced there. However, this was like somebody had a dose between zero and a hundred millirem, another one had a dose between a hundred and 200 instead of exact numbers.

MR. FITZGERALD: Right.

DR. BUCHANAN: And if you look at the data, there is NTA-recorded neutron doses available to create a database of individual results. And I believe that was in Table 4-4 of the '09 paper.

And so, what we would like - we recommended was that that be used to create a coworker database as opposed to using

categorical data.

DR. NETON: I remember this.

DR. BUCHANAN: And I know we discussed it, but, you know, it had never been done. And so, we wanted to keep that on the books.

CHAIR BEACH: Ron, this is Josie. Didn't we have something also on the inches for the glove boxes? Didn't that end up being a Site Profile issue?

DR. NETON: I think that was resolved.

DR. BUCHANAN: I think that was resolved. Brant's latest paper on that agreed, okay, it doesn't make much difference. You hit kind of a plateau between eight and 12. We'll use the 12 and move on.

CHAIR BEACH: Okay.

DR. BUCHANAN: I don't think there was a further issue on that.

CHAIR BEACH: Okay. Thank you.

DR. BUCHANAN: And so, that is

where we stand on the neutron issues.

MR. FITZGERALD: And you also went ahead and addressed Issue 16, which is the next one which deals with shallow dose which was one of the early ones that was sort of taken off the SEC list, but I think we had a remaining issue on that too, didn't we?

DR. BUCHANAN: Yes. Number 16 or shallow dose Site Profile Issue Number 8, that was - the problem was originally there was beta dose could not be reconstructed in the early days because there was no reliable dosimetry records.

However, we found out that it needed to be extended to a further period up into the '70s before beta dose is actually recorded and dosimetry was verified.

And so, in past discussions that was agreed upon to extend it up to the DOELAP accreditation in 1991. And that's quoted there in NIOSH's paper in 2009.

And so, again that's a bookmarker

we agree with and NIOSH agree. We just need to see that that's done in the revised TBD to extend that up to a later date.

DR. NETON: Yes.

DR. BUCHANAN: And so, that was all on that issue.

MR. FITZGERALD: Okay. Thank you, Ron.

The last item is Issue 20 which we haven't talked about in eons, but has to do with the Environmental Occupational TBD and the wording in that TBD in terms of ambient environmental internal dose.

And we had this what seems to be an obscure date now, but the question of whether site-wide contamination existed and whether there needed to be a statement removed that Mound did not experience site-wide ambient contamination.

And maybe that was the peace offering, but I think that was just an item for TBD to remove that one statement that

factor might be in fact instances where you had some broader contamination to the site. And that was that one.

CHAIR BEACH: I mean, I do remember that discussion.

MR. FITZGERALD: It seemed like it was a long discussion.

(Simultaneous speaking.)

MR. FITZGERALD: It seemed like a long discussion to get to a point where, yeah, okay, we'll take that sentence out, but that was the resolution. I think weariness stepped in at that point.

DR. NETON: Well, we went through them quickly, but there's a lot of work embedded -

CHAIR BEACH: There's a ton of work.

MR. FITZGERALD: But that represents a pretty good scrub based on the transcripts and the midterm analysis done on internal.

So, I think it's a pretty good - like I said, it might, you know, once NIOSH goes through it there might in fact be some things that were missed. And that will take care of some of those issues readily, but that's pretty much it.

CHAIR BEACH: Thanks for pulling that together on short notice.

That's the end of our agenda unless - and we can't really try to schedule another meeting.

MR. KATZ: So, do you need any discussion about the presentations in June or do you -

CHAIR BEACH: I actually have a start on the presentation. Bill put one together for me. I looked at it and I was asking what we were going to do with tritides.

We'll send it out in the next week.

MR. KATZ: Do you need any support from DCAS on that front?

CHAIR BEACH: I think just if there's questions.

MR. KATZ: Okay.

CHAIR BEACH: I'll definitely send it to Ted, and then he can send it out. And if there's any -

(Simultaneous speaking.)

MR. KATZ: Why don't you just go ahead and send it to the whole Work Group for everyone to take a look at.

CHAIR BEACH: It's pretty straightforward.

MR. FITZGERALD: Well, I think the tritides might require some consensus on how the -

DR. NETON: But when it's ready as soon as you feel it's finalized, if you send it at least to me so I can get it to Chris Ellison because she needs to -

MR. KATZ: Well, that's at the end of the process.

DR. NETON: Well, but it's getting

close to the end.

MR. KATZ: It is. We have a couple weeks.

DR. NETON: Once it's done.

CHAIR BEACH: Well, and hopefully by the end of next week maybe, Joe, between us -

MR. FITZGERALD: And LANL.

(Simultaneous speaking.)

CHAIR BEACH: A week. Well, and I've got some notes and that's what I'm going to work on the rest of the day. And then -

DR. NETON: Is there a LANL Work Group?

CHAIR BEACH: No.

MR. FITZGERALD: No, but it's a presentation because it's in Santa Fe and -

(Simultaneous speaking.)

MR. FITZGERALD: Mark's been in Australia. So, there's a little bit of -

MR. KATZ: And Mark wanted to do a presentational update in this case.

CHAIR BEACH: Well, let's go ahead and pull off there then since we're done with Mound and - unless anybody has any other comments or -

MR. KATZ: No.

Adjourned?

CHAIR BEACH: Yes.

MR. KATZ: Thank you everyone on the line for bearing with us.

CHAIR BEACH: Thanks, Phil.

MR. KATZ: Thanks, Phil.

MEMBER SCHOFIELD: Thanks.

MR. KATZ: Thanks everyone at SC&A too and have a good afternoon.

(Whereupon, at 1:51 p.m. the meeting was adjourned.)