U.S. DEPARTMENT OF HEALTH AND HUMAN SERVICES
CENTERS FOR DISEASE CONTROL
NATIONAL INSTITUTE FOR OCCUPATIONAL
SAFETY AND HEALTH

+ + + + +

ADVISORY BOARD ON RADIATION AND
WORKER HEALTH

+ + + + +

SUBCOMMITTEE ON PROCEDURES REVIEW

+ + + + +

THURSDAY
AUGUST 28, 2014

+ + + + +

The Subcommittee convened via teleconference at 11:00 a.m., Eastern Daylight Time, Wanda I. Munn, Chair, presiding.

PRESENT:

WANDA I. MUNN, Chair
JOSIE BEACH, Member
PAUL L. ZIEMER, Member

ALSO PRESENT:

TED KATZ, Designated Federal Official
MATT ARNO, ORAU Team
BOB BARTON, SC&A
HANS BEHLING, SC&A
KATHY BEHLING, SC&A
RON BUCHANAN, SC&A
BOB BURNS, ORAU Team
HARRY CHMELYNSKI, SC&A
DOUG FARVER, SC&A
STU HINNEFELD, DCAS
LORI MARION-MOSS, DCAS
STEPHEN MARSCHKE, SC&A
JOHN MAURO, SC&A
JAMES NETON, DCAS
STEVE OSTROW, SC&A
SCOTT SIEBERT, ORAU Team
MATTHEW SMITH, ORAU Team
DANIEL STANCESCU, DCAS
JOHN STIVER, SC&A
ELYSE THOMAS, ORAU Team

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

(10:58 a.m.)

MR. KATZ: So this is the Advisory Board on Radiation and Worker Health Subcommittee on Procedures Review. There are materials for this meeting that are posted on the NIOSH website along with the agenda under the Board section under meetings, today's date. So people can also follow along with some of the materials that we'll be talking about which are posted there.

Let's do roll call. As far as conflict of interest is concerned, there shouldn't be any material that relates to conflicts in today's agenda. But I just remind my Board Members to just keep that in mind in case something comes up related to a site you have conflict with.

And so let's go with the roll call for Board Members.

(Roll Call)

MR. KATZ: Very good. Wanda, it's your agenda.

CHAIR MUNN: Thank you much, and thanks to everybody who is joining us today, and thanks especially to those of you who submitted papers for us to deal with today. After that long hiatus, I hope you're all ready to attack these issues again.

And let's start with very quickly taking a look at what Steve has up on the screen for us. Thank you, Steve, for being johnny-on-the-spot getting the material in front of us to look at.

And let's take a look at what you and Lori have posted to the BRS that we discussed at our last meeting but was going to be taken care of offline. Steve, you want to lead off on that and Lori if you can follow afterwards?

MR. MARSCHKE: I'm not aware of

anything that was really we needed to be posted, Wanda. Let me see, up to the last --

CHAIR MUNN: I just thought there were one or two things that were so wordy that you were going to take care of it after the meeting. But perhaps you did it before we ever got offline last time.

Lori?

MR. MARSCHKE: If I did I -- I'm sorry, Wanda. I mean if I did, I did it right after the meeting which was, you know --

CHAIR MUNN: So long ago.

MR. MARSCHKE: So long ago that I've forgotten. And I went back and looked at the transcript to see if there were any SC&A action items which were expected of me and I didn't see any.

CHAIR MUNN: All right. That's fine. Lori?

MS. MARION-MOSS: Yes, Wanda. I

do believe the only thing that's been updated since that time is the findings that I sent out to the Committee the other day. And I do believe Steve Ostrow had updated the BRS with some responses to OTIB-54 findings since our last meeting.

DR. OSTROW: This is Steve. That's correct. I had updated that. Right after the meeting I updated the BRS on OTIB-54.

CHAIR MUNN: So can we take a look at OTIB-54 and that update to make sure that it meets the expectations of the Board Members? Here we are.

DR. OSTROW: Wanda, this is Steve again. The updating I did was on April 16th to all the open findings and I said the exact same thing in each one. This finding in OTIB-0054 Rev 1 also applies to Revision 2.

CHAIR MUNN: Yes.

DR. OSTROW: Because subsequently

to my making the comments on Rev 1, NIOSH released Rev 2. And I reviewed Rev 2 and didn't see any material change between Rev 2 and Rev 1.

CHAIR MUNN: Excellent.

DR. OSTROW: So I just basically said that all of the findings apply to Rev 2.

CHAIR MUNN: Good. Any thoughts from anyone else? If not, thank you for that Dr. Ostrow. And --

MS. MARION-MOSS: Wanda?

CHAIR MUNN: Yes?

MS. MARION-MOSS: Lori. I would just like to point out to the Board, Subcommittee Members, that the BRS looks a little different since the last time --

CHAIR MUNN: Yes, it certainly does. It was a surprise.

(Simultaneous speaking)

MS. MARION-MOSS: The major change

here that we have is actually the display on the screen and the option of being able to change your font size. And that's basically the biggest change.

CHAIR MUNN: That's very convenient though, and I certainly approve. I don't know whether anyone else has had an opportunity to play with it a little bit. I pulled it up for the first time day before yesterday, I guess.

So I looked at it a little bit yesterday and looked at the report sheets to see what our figures were looking like, so, but was very pleased with the look of the current index. It seems to be easy to work with.

If anyone else has a comment in that regard, please join in.

MEMBER BEACH: Yes, Wanda, this is Josie. I also was playing with the BRS the last couple of days, and I really like the changes

that were made. So thank you.

CHAIR MUNN: Good, yes. True. And if we have any other comments, I'm assuming that Josie, you and Paul both have seen Lori's email and the changes that she listed there.

MEMBER ZIEMER: Yes.

MEMBER BEACH: Yes.

CHAIR MUNN: All right. No question about any of that. If not, then thank everyone who was involved in that and let's move on to our first item other than the BRS.

MS. K. BEHLING: Wanda?

CHAIR MUNN: Yes?

MS. K. BEHLING: This is Kathy Behling. I was just going to ask a question. Because I was attempting, or I was going to attempt to put the findings in from our review of the DCAS PERs that we're going to be discussing today, 42, 43, and 45.

And I'm not sure that I have

authorization to do that because, and if I do I don't know how to do it. I assume that perhaps NIOSH needs to enter those PERs for us first and then I would be able to put the findings in because I don't know how to actually enter a new document.

CHAIR MUNN: Oh. And we don't have those three PERs in yet?

MS. K. BEHLING: I didn't see them. Maybe they are. Did I miss them?

MR. MARSCHKE: Actually, they're in the unassigned queue. I guess we've got to figure out how to take them from the unassigned queue. I mean you can look and see on the screen now, this is the unassigned queue. And there's 42, 43 --

CHAIR MUNN: 43 and 45.

MR. MARSCHKE: -- and 45 are there. Now we have to basically take them from the unassigned queue and bring them over into the,

assign them, I guess, to the Subcommittee, and I'm not sure how you do that.

CHAIR MUNN: Yes, that sounds reasonable. Lori, is this an activity that we've gone through before behind the scenes and that the rest of us are aware of?

MS. MARION-MOSS: Yes, but Steve should be able to do that. If you will, Steve, click on PER-0042 and let's see what happens.

You clicked on it?

MR. MARSCHKE: I clicked on it. Do you want it expanded?

MS. MARION-MOSS: Yes.

MR. MARSCHKE: Okay, Assign to Work Group. Okay, basically what I should do --

MS. MARION-MOSS: First, select our committee.

MR. MARSCHKE: Subcommittee, and then that's it. Pretty simple.

MS. MARION-MOSS: That's,

hopefully.

MR. MARSCHKE: Are you sure you would like to assign one --

(Simultaneous speaking.)

MR. MARSCHKE: Okay, and then basically we go down to, 45 is here.

DR. MAURO: We've come a long way, baby.

CHAIR MUNN: We certainly have. That's marvelous.

MR. MARSCHKE: And 43 is here. That's the last one, right? Kathy?

MS. MARION-MOSS: Yes, it is. Yes.

MS. K. BEHLING: 42, 43, 45.

CHAIR MUNN: 45.

MR. MARSCHKE: Okay, so now they, okay, let me get this one done.

CHAIR MUNN: Steve and Lori have now accomplished digital magic for us. And

Kathy, hopefully the next time you attempt this, maybe even yet today, you may be able to do what you wanted to do.

MS. K. BEHLING: Very good. Thank you.

CHAIR MUNN: You bet. That's wonderful. Ah yes, this is moving the way it should. That's excellent. Thank you so much.

MR. MARSCHKE: PERs, right?

CHAIR MUNN: PERs.

MS. MARION-MOSS: Steve, you're in the wrong committee. You're in the PR Subcommittee.

MR. MARSCHKE: Oh, I'm in the Construction and they don't have any PERs.

CHAIR MUNN: Which is a good thing.

MR. MARSCHKE: Evaluation for PERs.

CHAIR MUNN: Here we are.

MR. MARSCHKE: 42, 45 and 43.

CHAIR MUNN: Do you see them? I don't see them on my screen yet, but yes. Yes, there they are. There's 43.

MR. MARSCHKE: We've already got them so we can basically --

CHAIR MUNN: 43 and 44?

MR. MARSCHKE: Yes.

CHAIR MUNN: Yes. 42 for some reason is out of --

(Simultaneous speaking)

CHAIR MUNN: But that's all right. We don't care. We know where it is.

DR. MAURO: Hey Wanda, this is John. Quick question. How far have we gone on the interconnectedness between the Subcommittee and Work Groups and et cetera, et cetera? You know, the grand dream that started about five or six years ago, are we moving in on that?

CHAIR MUNN: I think my experience

has been it just depends almost entirely on the Chair of the individual Work Group or the Subcommittee. If the Chair is active about this, then it's moving forward slowly but surely. For Chairs that are not particularly enthusiastic or don't feel comfortable with the process --

DR. MAURO: But the wherewithal exists that is within the framework --

CHAIR MUNN: Yes.

DR. MAURO: -- like we're looking at right now can accept that if so desired by the Chair?

CHAIR MUNN: That is my understanding and that's been my experience. Perhaps someone else has a different experience? If so, please let us know.

MR. HINNEFELD: Well, this is Stu at NIOSH. And we intend going forward when we get, like we start a new site fresh, to put

findings in the BRS. And particularly, I know we did that with the Pacific Proving Ground findings from the Site Profile Review of Pacific Proving Ground.

And then I believe, well, it's my intention that we hope to do the same with some of the more recent Site Profile reviews that we're now working on responses to. But before we get down this pathway very long I'd like to get those documents.

And I'm thinking of putting things like, I think there's a W.R. Grace, a NUMEC, maybe Ames. I forget. And so our hope is to enter those in the appropriate Subcommittee or Working Group or with its own Working Group if one's established for those. And so then just kind of move people on to this.

Now in reality, we recognize that findings and responses will probably still have to be shared as they traditionally have been for

certain people's comfort level, but we think using the BRS will make it easier to collect the record of the discussion that's done at those meetings.

CHAIR MUNN: So perhaps a more accurate response to your question, John, is that in any case NIOSH will use this as the real honest-to-goodness basis of where we are and for their own tracking whether or not the individual Work Groups participate in it.

MR. KATZ: Yes, this is Ted. NIOSH and SC&A both will actually do that, enter their findings and so on. So yes, as Stu was saying, so for everything new, for all new work. So something like SRS that's been going on a long time, the SEC, we're not going to change horses midstream. For everything new we'll work this way.

MR. STIVER: Wonderful. Yes, this is John Stiver. I had a question for Stu.

Back to over a year ago, we had the occasion to talk about maybe adjusting the BRS a bit where it could handle the Dose Reconstruction Subcommittee findings as well. And I was wondering, where are we on that? I don't quite remember where that stands.

MR. HINNEFELD: I don't go to that Subcommittee meeting anymore very much, so I'm not really tuned in to what's proceeding there. I would think that at any time, I think the best place to start might be with a new set of findings when there's a new set of dose reconstruction reviews done.

MR. STIVER: Yes, that makes sense. I think I recall we were going to go ahead and finish out the 13 sets using the matrices and then to migrate over after that.

MR. HINNEFELD: Yes, I think the intention was that it could be done, and I think we roughed out some sort of business logic --

MR. STIVER: Right, right, right.

MR. HINNEFELD: -- on terms of what would be the document reviewed and, you know, those kinds of things. And so as far as I know we could probably start using it at any time, but I would think we would want to start with a set of reviews that the discussion has not yet started on.

MR. KATZ: Yes, Stu, I think if we could get, if your folks could start loading on, I guess, Set 14, 15, because those ones you're starting to review and respond to, I think, even though the Subcommittee hasn't gotten to them yet.

CHAIR MUNN: Yes, I think that's true. And the Subcommittee Chair has been trying very hard to close out all of the last remaining issues from the older sets so that we can start with a reasonably fresh slate with the new group.

MS. MARION-MOSS: This is Lori. Steve, if you could, to kind of address John Stiver's question, if you can on the string there go to the DR Subcommittee Work Group filter.

As a result of that meeting, John, efforts were made to put in some of the matrix for the DR Subcommittee so that, I believe it was Doug Farver, I believe, that attended that meeting that had some questions.

We went ahead and put in some sets for the DR Subcommittee, and we wanted to give the Committee an opportunity to take a look at how that information was entered to see if there was any adjustments that needed to be made to fit the committee's needs.

And so far that's the last I've heard of it, so what you see here now on the screen is what was done as a result of that meeting.

MR. MARSCHKE: I'm seeing for the eighth set there. But I'll go talk to Doug offline. I'm spending a lot of time on this right now with reviewing an upcoming DR Subcommittee meeting that might be a topic of discussion there.

MR. KATZ: Yes. Well, and I'm just thinking, there won't be a lot of time in that Subcommittee meeting for talking about these other matters either.

But just if you guys, John and Doug, if you look at it and it's making sense to you, I think that's probably enough to say to the folks, Stu's folks, that it's okay, why don't we upload, you know, 14 and 15 findings.

MR. MARSCHKE: Okay, I'll touch base with Doug on it.

MR. KATZ: Yes, thank you.

MR. MARSCHKE: All right.

CHAIR MUNN: All right, thank you

very much. I think we should all be pleased. Thanks very much to those of you who are working behind the scenes on the BRS. It is looking very good and I think you should all put a feather in your caps. We're making progress. Thank you.

Now moving on to our first item, we were going to have some information from NIOSH continuing our discussion about the contaminant cleaning and localized skin exposure.

Stu?

MR. HINNEFELD: Yes, I think Jim's going to talk about that.

CHAIR MUNN: Oh good.

DR. NETON: Yes, this is Jim, Wanda.

CHAIR MUNN: Yes, good. Glad you're here, Jim. Thank you.

DR. NETON: Thank you. This issue

is related to overarching Concern Number 9, overarching Issue Number 9, I believe it's Concern Number 1.

And that is that we had agreed in principle with SC&A about adding skin contamination to certain workers under certain circumstances, but the concern was raised about the efficacy of washing off a skin contamination. That is, it lasts just eight hours and it was showered off, or did it persist for, you know, a 24-hour time period to some degree?

And it was our opinion, based on just observations from working at uranium facilities, that uranium is particularly readily cleaned off with regular showering, soap and water. And that was certainly Stu's opinion having spent years working at Fernald, and we sort of almost thought it was sort of common industry knowledge.

Well, SC&A didn't necessarily disagree, but they said you need to provide some backup evidence to support that position. So I've spent some time looking through the literature trying to find some documentation that would support that position.

I've looked through a number of documents including NCRP 161 which is Management of Persons Contaminated with Radionuclides. There is a DOE guidance for good practice at uranium facilities and there's some WHO guidance out there, none of which specifically talk about uranium contamination.

Well, the DOE guidance for good practices does, but all of them start with the suggestion that the contamination should be started to be cleaned with soap and water. That's always the recommended practice, and then you follow up with more aggressive treatment modalities later.

And none of them make the case that the uranium was particularly recalcitrant to cleaning, but none of them also said that it was readily cleansed.

I dug a little deeper and I discovered a DTRA report that was produced actually by SENES, DTRA Report TR-09-16, which talks about radiation doses to skin from dermal contamination as specifically written for handling the DTRA cases, which of course would be directly relevant to fallout deposition.

There's a very lengthy treatment of skin contamination on these cases and how they deal with it but what caught my eye was a paper that was referenced from 1958, where there was actually an experiment done with what they called simulated fallout.

And they actually created a mixture of soil with a known particle size distribution and labeled it with lanthanum-140, which is not

uranium but it is radioactive, and applied it to the skin of various test subjects and evaluated the efficacy of removal with various treatment regimes.

It turns out that for that particular experiment almost 95.8 percent was removed with a single washing with just regular soap and water. So that's not uranium, but it is indicative that the material can be readily cleansed with soap and water. I didn't think that was going to, you know, that was suggestive evidence but it wasn't uranium.

I searched further and just recently, actually, found a paper that was done in 1959 written by some folks from Los Alamos where they published this in the American Industrial Hygiene Association Journal, and the title of that document, "Surface Contamination Control with Uranium Operations."

It's amazing to me that they actually did this. Los Alamos evaluated some uranium rolling operations in the late '50s where they actually used, if you recall, the salt bath method which is an extremely messy form of rolling uranium. Or not extremely messy, but it's pretty messy, where you dip the uranium in a salt bath, heat it up and then roll it, and it creates a lot of scaling and particulate contamination.

Well, the point of the article is really how you address contamination control in the facility, and they did a lot of surveys and evaluation of anti-Cs and such. But one aspect of the study, they actually monitored personnel before they left the area for contamination, both their clothing and their bodies.

And where they did find contamination they allowed the personnel to wash with soap and water and then surveyed them

again. And conclusion of their study, that little study, was washing with soap or detergent, it says, usually removes any personal contamination with skin.

In other words, they didn't find any real difficult contamination to remove at least in this experiment, which I believe is fairly relevant because it's at an actual uranium rolling operation.

So the point of all this discussion is I'm getting close to writing up something that would sort of end up being a weight of the evidence approach. I don't have any direct data though. I was really hoping to find some personal contaminations where they were, you know, before and after using soap and water, which is typically what's used, and I was not able to find any of those. So that's where I'm at on this finding. I intend to write this up as a brief White Paper to

document what I just discussed with the Subcommittee. That's it.

CHAIR MUNN: Thank you, Jim. We'll look forward to seeing that. Does SC&A have any comment?

DR. OSTROW: Yes, this is Steve Ostrow. Just a brief comment. I just got finished reading a bunch of site interviews at Idaho, INL. We just conducted lots of site interviews there with former workers.

And a number of them mentioned for skin contamination, especially in the earlier days, they just used sort of a crude bleach solution to take off the contamination. But I didn't really see any record of, you know, before and after readings. But that would seem to be the common method they were using to decontaminate, you know, things like hands that became contaminated.

DR. NETON: Right. That would

have been for what was going on at Idaho, and I hear what you're saying. This particular finding though, I believe we were focused more on the early years of uranium processing operations --

DR. OSTROW: Right.

DR. NETON: -- where they rolled uranium, and, you know, these people had no monitors. There was no contamination surveys. No way to really tell if people contaminated.

And, you know, the idea was, well, if they were contaminated and took a shower did it clean it off or not, or should we assign this for 250 days and sort of 24 hours a day kind of thing.

DR. OSTROW: Right.

DR. NETON: And again it's been our experience that uranium's fairly readily washed off with soap and water. The Los Alamos study seems to indicate that. So I'm not sure

where to go from here other than sort of provide the weight of the evidence that we have.

DR. OSTROW: Okay, thank you.

DR. NETON: I think we should look at it.

CHAIR MUNN: Probably wise. My guess would be that the Idaho workers like in most complexes, certainly in later years, were receiving hand and foot monitoring frequently throughout the day when they were in contaminated areas.

And I don't know that it was a common practice to record quantitatively what was happening on site as they came out, but they were usually monitored very carefully, I think. We'll look forward to that paper.

One comment that I neglected to make before we started was the fact that those of you who may have taken a look at the transcript from last time may be aware of the fact that we seem

to have dropped our conversational tones, our pace, at some juncture fairly frequently during our last meeting so that the transcriptionist had a hard time picking up our full comments.

It's something that's easy to forget because we get so conversational on these sessions. It is wise to remind ourselves from time to time, we do need to be very careful in our articulation as we are speaking if we want to rely on the transcripts later for accurate information about what was actually said and what we discussed.

Having said that we are on to PER-0031, a carryover from last time. Do we have a report from NIOSH?

MR. HINNEFELD: Yes, this is Stu. To be honest, I've been trying to figure out exactly what instructions we've given to our contractor on that. And because the issue, I think, in front of us, this is the Y-12 PER, and

that PER was written because of some other changes, some changes were made to the Site Profile.

But in reviewing the PER, SC&A pointed out that hey, you know, this Site Profile says that you're going to do thorium internal dose assessment based on in vivo monitoring after some year, and they used essentially the same in vivo monitoring technology that Fernald did.

They reported results in milligrams the way Fernald did, and at Fernald we determined that you couldn't really interpret those readings. So how does that, you know, what effect does that have on Y-12?

And so the effort has to be put into this so it really becomes a Y-12 Site Profile issue that we intend to pursue. And I'm just trying to figure out now, you know, how far along or have we even gone very far down this path at all

in terms of determining what might be possible.

We do know that they have a lot of air monitoring data from Y-12, a quite a bit of it which we think is thorium air monitoring data for the period in question. And so there might be techniques other than in vivo if in fact we decide the in vivo isn't appropriate. There may be a way to determine the in vivo monitoring is appropriate and maybe you can interpret those results. So I don't think we're very far down that path but we intend to go down that path.

So, but with respect to the actual PER-0031, you know, it would be okay for the Subcommittee and SC&A to finish reviewing PER-0031 with respect to seeing was this PER done correctly, meaning were the changes that were incorporated into the Y-12 Site Profile, were those adequately considered when, were cases adequately reconsidered as a result of

those changes?

So I mean it would be feasible if the Subcommittee wanted to proceed with PER-0031, you know, either worry about these findings later or transfer them to Y-12 Work Group and finish the PER-0031 work essentially, and, you know, if that were what you wanted to do.

Alternative thought is depending on how this works out, there could be another PER for Y-12 and maybe we'll just take a look at claims at that point.

CHAIR MUNN: Now you've confused me, Stu.

MR. HINNEFELD: Well, the PER-0031 was written for a specific purpose. I haven't prepared myself very well or I'd know what that was. And that, you know, PER-0031, the reason it was written had nothing to do with in vivo monitoring. It was some other change that was made.

CHAIR MUNN: Do we have any knowledge of what that change was?

MR. HINNEFELD: Yes, I'm looking for it. Here, hang on just a minute.

DR. MAURO: Wanda, this is John. I might be able to help a little bit out with regard to what, we've encountered the circumstance in the past. That is, everything we do is a living process, whether it's a Site Profile Review or it's a PER review.

And what we have now on the record is a PER review based on certain activities that took place and changes to the Site Profile, the procedures that took place up to that point in time, and of course SC&A then reviewed the PER with respect to that.

As life goes on, we always find that maybe things change again. In the past it's been our position that we just keep grinding. That is, yes, we have a PER that's been

reviewed. We have comments.

The fact that there's a new round of possible PERs that may emerge that we'll deal with that when that happens, but let's grind through and put to bed the ones that we have before us now. That's how we've handled it before. That doesn't mean we have to continue in that mode.

CHAIR MUNN: Thank you, John.

MR. HINNEFELD: Yes, this is Stu. Wanda, this is Stu. And I guess I spoke naively a while ago. This PER was actually performed because there was, you know, the technical documentation was changed to change the equilibrium ratio for a couple thorium isotopes, thorium 228 to 232, and that was changed from 100 percent to 80 percent assumption, and so that would raise certain doses. And because of that we did the PER, so it was an in vivo PER.

But the additional question of can this even be interpreted at all, that's really a Site Profile question rather than a PER question.

DR. BUCHANAN: Yes, this is Ron Buchanan with SC&A, and that's correct. PER-0031 was issued because of the change in ratio of equilibrium, and I'm the one that did the review and the finding on that. And so Stu is right that that is, the end result was that we found that this then actually decreased the dose if you applied it strictly the way the PER stated it and the TBD stated it.

And so we questioned, you know, whether we could even use this data. And so Stu went back and said, okay, we're going to need to look at this and see if we can, because it was a problem at Fernald and it's the same model as Fernald used.

Basically, I looked and seen I did

carry through with a PER then to see if it was done correctly, on the other hand, and I found that the dose -- I'd have to go back. I didn't look for stuff this morning, all the doses I looked at, all the cases.

But the cases I looked at is that it was not applied uniformly. This 80 to 100 change was not applied uniformly then in the new DR. So when we ran into the fact that this was maybe not the way to do dose reconstructions or it needed some attention, I think that kind of stalled that progress of saying okay, even though we had this revision in the TBD it's not being applied uniformly, and so that's where it kind of stands at this point.

MR. KATZ: So this is Ted. I think what I would suggest, Stu, since we don't have an active Y-12 Work Group, I realize there was one once upon a time --

CHAIR MUNN: Yes, but --

MR. KATZ: Yes, exactly. But since we don't have one active now, I mean I don't see any reason why we can't just move forward this under the PER process.

CHAIR MUNN: It seems most expedient to me, especially in light of the current status of Work Groups for Y-12. I would hate to see this languish any longer on the vine, and it seems to me that we are the logical venue at this point to address it.

MR. KATZ: Right. If we had a Work Group we'd shift it over to them, but since we don't it just seems like it'll be fine. You have good representation on this Work Group. I think you can grind through the issue with the Subcommittee.

CHAIR MUNN: I certainly agree with that interpretation.

Paul, any feedback from you and Josie?

MEMBER ZIEMER: Well, (telephonic interference).

CHAIR MUNN: You're breaking up for me, Paul. I don't know if you are for others.

MEMBER ZIEMER: I was on speaker and maybe it, I'm getting an echo. I hear myself. In any event --

CHAIR MUNN: That's much better, thank you.

MEMBER ZIEMER: -- I just wanted to ask a question. This doesn't apply just to Y-12 though does it?

MR. HINNEFELD: Yes, this is Stu. The question, well, it would apply where -- anyplace in vivo results are reported in milligrams rather than in the activity of the various isotopes that are being --

MEMBER ZIEMER: Right.

MR. HINNEFELD: -- added. So the question arose at Fernald and was actually the

basis for an SEC Class up through roughly '78 for about a ten-year period when the in vivo, when they used the mobile counter. Now Y-12 didn't use the mobile counter but they used the same type of system and the same reporting.

So anyplace where in vivo counting was reported in units of milligrams rather than activity of the radionuclides being counted, you're going to face this question, is this data interpretable.

But I don't know of any place other than Y-12 and Fernald where there was thorium exposure that was measured by in vivo. Certainly the mobile counter went to the gaseous diffusion plants, but I don't believe there was thorium exposure at those plants.

CHAIR MUNN: Well, I'm beginning to remember a little bit about the original review of this PER now. And that's helpful to know that it wasn't just Y-12, because it helps us

to see that it's not truly site specific.

MR. HINNEFELD: Yes, I don't think I've encountered this reporting at anyplace, you know, for thorium exposure other than at Y-12 and Fernald.

CHAIR MUNN: Well, in any case, does that answer your question, Paul?

MEMBER ZIEMER: Yes. I think we could just proceed within the committee here.

CHAIR MUNN: Yes.

MEMBER BEACH: So Wanda, this is Josie.

CHAIR MUNN: Yes.

MEMBER BEACH: I have a comment and question. So I agree that we should proceed with this finding, but my question is kind of an overall. Y-12 has a couple other OTIBs that have been reworked and reissued recently. I think the last one I looked at was 0064.

And if we take on this one issue,

what happens to these other TBDs that have been updated that haven't been looked at? I guess this is probably a Ted question.

MR. KATZ: Right. Well, at some point, Josie, I mean we'll assign other, I mean, you know, TBDs are getting updated for different sites all the time.

MEMBER BEACH: Right.

MR. KATZ: So at some point we'll have more assignments to review new versions or new TBDs. But that has to be done first and SC&A then would then have to do its work first.

MEMBER BEACH: Right. Okay.

CHAIR MUNN: All right, any other questions, Josie?

MEMBER BEACH: No.

CHAIR MUNN: Okay, it sounds to me as though we have a general consensus that we need to address the PER-0031 issues here in the Procedures Subcommittee. Not having heard any

comment to the contrary, we will proceed with that, I think, appropriately.

This means from my perspective that we will anticipate feedback from NIOSH on each of the findings that we have next time. Is that appropriate, Stu? I hate to make that statement without a commitment from you.

MR. HINNEFELD: Well, we don't have feedback on all these findings. I can't promise next time.

CHAIR MUNN: Okay, we'll carry it over and question it next time.

MR. HINNEFELD: Yes, right.

CHAIR MUNN: All right, fine. And then let's move on to the summary clarification of the IG-001 Finding.

NIOSH?

MR. HINNEFELD: Yes, this is us again, Stu, one more time. I don't know, Steve, can you maybe pull this up? We'll maybe

look at what the finding is? I don't know if that's going to matter or not though.

The issue here, just to refresh everyone's memory, is that IG-001 has a paragraph and has a section that's kind of disjointedly written but the information's in there.

And it has this, the section refers to dose correction factors as a function of the geometry whether it's AP rotational or isotropic geometry, et cetera. For most organs, AP geometry is the most favorable and so it's appropriate to default to AP.

There are, I think, four target organs, blood surface, blood bone marrow, bone surface, bone marrow, I think it's lung and esophagus for which AP is not more favorable than say rotational which seems like that could be a likely one in some kinds of jobs, or isotropic which seems like that might be a

little less likely.

And so there's a statement in there that says that since, for these four target organs, one of these other geometries is more favorable you should default to those unless you have a reason to use AP. If you think a person's job would be such that they were probably exposed in AP geometry most often, then you can use AP.

And so from that I think it's a logical conclusion that says that if you choose, you know, since there's a default that's, say, rotational and if you choose to use AP in your dose reconstruction you should explain that you chose to use that and why. And that's what's being done now, but it wasn't done for a while.

And so the question in front of us is, there are two questions in front of us. First of all, do we think the wording in IG-001

is okay as it is, and the answer to that question in my opinion is yes. It pretty much says what we want it to say.

And the second question is are we going to go back and do a PER or a PER-like look at cases that were done previously before we overtly started making dose reconstructors say yes, this should be an AP case and here's why, because that wasn't done originally.

Should we go back and look at those cases that were done beforehand and say is AP in fact the appropriate one to select there if the dose reconstructor used AP.

So we do think we do have that look-back work to do, and it would be our preference though to do that in conjunction with a PER that we know is coming having to do with the new ICRP, I think it's 116, recommendations for dose conversion factors.

That document's been out for a

little while. We've done some comparisons of what would be done, you know, having the ICRP, dose conversion factors compared to the previously published ones that we're using now. There will be quite a number of changes. Some organs will go up, some organs will go down.

So it looks like we're going to have to do a relatively large PER to incorporate the new dose conversion factors from ICRP 116. And so it would be our view that the best time to take care of this PER-like activity from this IG-001 statement would be when we have to do that whole wrap-up anyway or that whole look from ICRP 116.

So that's kind of where we are on our position on this. I don't think I have entered that into the BRS yet, but I think we can probably do that.

CHAIR MUNN: Yes.

MEMBER ZIEMER: This is Ziemer. I

thought what happened last time were that NIOSH indicated that they didn't feel there was any need to revise it, just that they would have a response to the IG --

CHAIR MUNN: That was my expectation. I thought we were going to have --

MEMBER ZIEMER: But he just described it. I thought we had actually put it to bed and were going to do what Stu just described.

CHAIR MUNN: Yes. What I had expected was a paragraph clarifying the wording and essentially saying what Stu just said. I think if memory serves, I can't remember for sure, I think we expected last time that we would have a small written paper so that we could just reference that in the BRS.

But is that your understanding, Josie?

MEMBER BEACH: Yes, absolutely. NIOSH had the action just to add wording to indicate the process.

CHAIR MUNN: I thought we were just going to have a summary, just a paragraph summary.

MR. HINNEFELD: Well, I apologize for not getting that done before the meeting, but I'll get it done shortly after, today.

CHAIR MUNN: Okay, good. If you do that then I think, certainly my understanding is we don't have any firm knowledge of when we're going to have to address the ICRP 116 issues and it would be much nicer if we could get this one off the books and look forward to what's coming down the pike when it comes down the pike instead of holding this in abeyance.

Good. If you'll get us a very brief summary, it doesn't have to say much more than what you just said, I think that will satisfy

this Subcommittee and we'll look for that next time.

MR. HINNEFELD: All right, thank you.

CHAIR MUNN: Okay. And don't go away, we still have you on deck for the Weibull distribution criteria. We were going to hear something about that that would hopefully close that for us this time.

MR. HINNEFELD: Yes, I'm going to volley that one over to Jim also.

CHAIR MUNN: Okay.

DR. NETON: Okay. Thanks, Stu. This is not really a finding anywhere from the Subcommittee, and I forgot exactly how it came up. But I believe it just arose in discussions during the last Procedures Subcommittee meeting.

CHAIR MUNN: Yes, before that actually. We addressed it during the last --

(Simultaneous speaking)

DR. NETON: Sort of surprised to see us using the Weibull distribution, and then we had a general conversation about when did we start using it, why are we using it and how do we decide which distribution --

CHAIR MUNN: Yes, that's correct, Jim. The question as I recall was raised, I believe, at our February meeting, and then there was some discussion about it in our April meeting. At that time, I believe that we committed to providing a brief written criteria so that SC&A could sort of mull that over.

DR. NETON: And we've done that. The Subcommittee should have received, I think from Lori, a paper that was written by Daniel Stancescu our staff statistician. It's titled "Fitting Distributions to Dose Data."

CHAIR MUNN: Yes.

DR. NETON: And it's a short paper

that tries to address the issues I just mentioned that, you know, how do we determine what's the best fit for a distribution that we're going to use? Why do we use that criteria?

And it goes through, I think it's a nice little write-up about the different types of statistical tests that are out there and why we've chose to go with these more modern approaches that evaluate the information criteria.

CHAIR MUNN: It is a good paper, and specifically --

DR. NETON: Actually, you know, it discusses why we use, I believe it's Akaike Information Criteria as our test statistics. So that's covered.

And then it goes on to specifically talk about the Weibull distribution and how we came to use it during the development of the CLL

Probability Model. A lot of those distributions that we used from the literature were Weibull distributions so we were sort of forced into it.

But once a distribution was available, and it is now one of the eight distribution types that can be selected as an IREP input, it's out there and it has been used in some other instances other than CLL fitting.

And primarily it's used in the fitting of external dosimetry data. For example, when you have a badge result that has an uncertainty associated with it that's normally distributed, like plus or minus 20 percent, and then I always fold in some dose conversion factor, for example, DCF for conversion of that badge reading to the dose to the liver.

And those distributions, the dose conversion factors tend to be distributions

that are triangular in nature and have some unique shapes to them.

Well, when you use Monte Carlo techniques and you fold in a normal distribution with the triangular, you end up with a hyper-distribution that doesn't often fit the normal or log-normal distribution very well.

And we've found from experience that the Weibull distribution accommodates those fits better. It tends to be a little bit of a chameleon. You can go through and read the paper, but there are three factors that can be used to modify the Weibull distribution, the shape, scale and location. And as indicated in Figure 2 of Daniel's paper, it shows you the range and types of distributions that could be generated. And when the Weibull is used and compared to a log-normal, then we would use that AIC criterion to determine which

is the best fit based on just a numerical value that is generated using that technique, using the AIC test.

So it's there in a nutshell. I know it came out fairly late. Even though it was written in May, I didn't get it distributed until fairly recently.

I will say that the version that was distributed to the Subcommittee says that it may have Privacy Act information. Of course there is none in there. I just didn't get a chance to have it formally cleared before it got distributed. It has since been reviewed and that footnote has now been modified to say that it's been cleared for review for Privacy Act issues and it's cleared for distribution.

That version is out there on the website under the Board's meeting, today's meeting. It went out there yesterday morning, I believe, or this morning. I forgot. But

it's out there, I checked this morning.

So that's a brief summary of what's there. I know people haven't had a chance to read it, but I think it covers the issues that we were asked to describe fairly well.

CHAIR MUNN: It does indeed, and thank you very much for that Jim. And I have certainly had an opportunity to read it, and I suspect that our other Board Members have as well since we did get it time to peruse it and it's well written.

Thanks to Dr. Stancescu for having compiled this, because even the lay statistician can follow it quite well and it's appreciated.

Any questions or comments from SC&A or from any of the Board Members with respect to the fitting distributions to dose data?

DR. FARVER: Wanda, this is Doug Farver.

CHAIR MUNN: Yes, Doug.

DR. FARVER: I just have a question, and I really have no questions about the paper itself. It was an explanation of the distribution and that's fine. Mine is more practical.

On the IREP table you have a list of Parameters 1, 2, and Parameters 3, and that is how they determine a dose, or one of those or more of those values determine the dose. For example, for a normal distribution the dose is under Parameter 1. For triangular, the dose is under Parameter 2. Now for the Weibull it appears that it's the sum of Parameter 2 and 3. Is that true?

DR. NETON: I'm not sure what you're asking, Doug. If you select Weibull, there's always three boxes to fill in. And what you fill in will be determined based on what distribution you selected.

DR. FARVER: Right. I mean when you look at the IREP table that you, the final one that goes into the IREP program.

DR. NETON: Okay.

DR. FARVER: Okay. At the very right hand side it lists a Parameter 1, a Parameter 2 and a Parameter 3.

DR. NETON: Right.

DR. FARVER: When there's a normal distribution Parameter 1 has the dose value.

DR. NETON: And Parameter 2 should have the uncertainty.

DR. FARVER: Correct. And when there’s a triangular distribution Parameter 2 has the dose value. Now for the Weibull distribution it looks like the dose is the sum of Parameter 2 and Parameter 3. That's what’s used to total the dose. Is that true?

DR. NETON: Well, Daniel's on the phone. Daniel, can you help me out there? I'm

not sure.

MR. STANCESCU: Yes, this is Daniel Stancescu from NIOSH. So the three parameters that you see is equal parameters for the Weibull distribution in IREP. The first one corresponds to the shape. The second one corresponds to the scale, and the third one corresponds to the location.

If the Weibull distribution, unlike the normal and log-normal distribution, it has a different formula to compute the mean. The mean of the Weibull distribution is a function of the shape, scale and location. So it's not as easily calculated as for the normal.

So for the normal, the mean is the first parameter. For the log-normal, you know, the geometric mean is the median. For the Weibull you can compute the mean. There is a formula which involves these three parameters. So we don't have a parameter for

the mean of the Weibull distribution.

DR. FARVER: No, I understand. I'm asking you how you determine the dose. Which parameter?

MR. STANCESCU: You mean which of these parameter is the most significant corresponding to the mean of the distribution?

DR. FARVER: No, which value equals the dose for a given year? Like 1963, photon dose, external. There's got to be a dose value in that IREP table.

MR. SIEBERT: I'm sorry, let me interrupt. This is Scott Siebert from ORAU Team. I think what Doug is referring more to is not necessarily the dose because the dose is actually the full distribution.

But Doug, you're actually referring to the dose as we refer to it in a dose reconstruction report, correct?

DR. FARVER: Yes.

MR. SIEBERT: Okay, and I think that's the basis of the confusion on the question. When it comes to the report, as Daniel said, there is no simple way to calculate the mean for each of the years so that we could report the mean for the dose for each of those years.

So you are correct. What we have done as a reporting consideration is to add Parameter 2 and Parameter 3 which gets us in the same general location as the mean. It's not exact, if I recall correctly, but it's in the same general location as where you would have the mean.

That's what we were trying to create for a reporting scheme, rather than having someone have to use all three parameters for the Weibull distribution, calculate the actual mean which you could never directly get from the IREP sheet. Whereas, the approximation of

Parameter 2 and Parameter 3 got us into the right location so you're using that as the reported dose for the reports.

DR. FARVER: Okay, but it's the sum of 2 and 3?

MR. SIEBERT: Yes, that is correct.

MR. STANCESCU: Okay, and if I can mention here, the reason. So it is the sum of Parameters 2 and 3 which is the scale and the location and it can be proved theoretically that the sum of the scale and location for a Weibull distribution is equal to the 63rd percentile of the distribution.

So it's kind of close to the mean value, somewhat so, and that was an easy way to report a value that is kind of representative for the distribution.

DR. FARVER: Okay. And Parameter 3 can be both positive or negative?

MR. STANCESCU: Yes, that's

correct.

DR. MAURO: This is John Mauro. Again from a practical standpoint, so if I'm filling out the IREP input table and they're all line items, and let's say I'm doing the external dose to some organs, and somehow to get to that external dose the Weibull distribution was used because it's a combination of, let's say, some film badge data combined with some triangular which might be log-normal and some other data, like the dose conversion factor, which is triangular, and the two of course are multiplied together and then the outcome is some distribution. Now that's a Weibull distribution as I understand that is the selection now because it's an improvement on the fit.

Now in the input to IREP I will put in the word "Weibull" as the distribution that applies. I haven't done this but I'm assuming

this is the case. Then to the right of that there will be columns that will open up that might be Parameter 1, 2 and 3 as described earlier.

Now somehow those parameters have, there's some very specific instructions what you put in under what would be called Parameter 1, 2 and 3. There's some number that has to go in there.

And unlike what we used to do where we would put in the geometric mean in the first one, or we'd put in the arithmetic mean and then next to that we would put in the geometric standard deviation if we're dealing with log-normal, here you're saying that what we actually put into the boxes to the right of where we say Weibull, there's some other things we put in.

Is that all straightforward now? I mean in other words are the instructions on how

to load the input to IREP straightforward so that we do it right and we don't put the wrong numbers in those boxes?

MR. STANCESCU: I think it is. If you go to the IREP website and if you try to enter the doses, if you click on the menu it's going to show you the order of the parameters for each of the distributions. So, for example, for triangular the order is the minimum more than the maximum.

DR. MAURO: Right. And we understand that so, you know, we've learned to do that.

MR. STANCESCU: Yes, and for the Weibull it showed that the first one is going to be the shape, the second is the scale, and the third one is the location.

DR. MAURO: Bingo. That's what I was looking for. Thank you.

MR. STANCESCU: Yes.

DR. MAURO: Good, you've answered my question.

MR. STANCESCU: Okay.

DR. FARVER: Okay, this is Doug again. I've just got a couple more just, and the reason I have these questions is we were having difficulty matching the NIOSH number, and so we were scratching our heads a little.

For CLL cancers, Parameter 3 seems to be much larger when compared to Parameter 2. And I've only seen positive numbers for that. Is that just coincidence or --

DR. NETON: This is Jim. I can't exactly speak from experience in looking at those parameters recently, but yes, I would assume it's just the way the --

DR. FARVER: Okay.

DR. NETON: The way the fit works out for the CLL cases. They tend to be a little bit different because of the various

distributions that, you know, there's a lot of distributions that are combined to get that final product.

DR. FARVER: Okay. But the Parameter 3 seems to be more significant. Because if we excluded Parameter 3 and just added up Parameter 2, much farther off from the NIOSH value. So it seems more significant. I don't know if that's true or not.

DR. NETON: Well, in what's the third parameter's location?

DR. FARVER: Yes.

DR. NETON: Yes. Well, I'm assuming the location, you know, the tripping of that curve on the X-axis is significantly different for CLL cases.

And remember, CLL cases are very different than the ones you would see for external dose calculation. I mean you're just folding in a triangular and a normal. In CLL

you're folding in a lot of distributions.

So the shape of, that's one thing about the Weibull is it can have a lot of different shapes and locations on the curve. So I think that's all you're seeing is an artifact of the CLL model itself.

DR. FARVER: Okay. One final question. Scott, is there any instructions given to the dose reconstructors along these lines about so they can check their work?

MR. SIEBERT: There's does not need to be because Weibull is only used in best estimate claims where the tool creates the fit and the tool directly imports that information into IREP.

The dose reconstructor never had the option to pick a Weibull distribution because it's not a general distribution to use unless it's coming out of a best estimate Monte Carlo calculation.

DR. FARVER: Okay, and we'll never go in and sum up his doses just to make sure his numbers match?

MR. SIEBERT: Oh, you're talking about for summing the dose. We have tools that do that for us so the dose reconstructor doesn't have to go by hand and do the addition.

DR. FARVER: Okay. We've run into situations before where there have been doses omitted and it'd be better if they went and checked the final doses. That's a suggestion. But I understand what you're saying.

Okay, my main concern was that Parameter 2 and Parameter 3 are totaled up to give the dose. So that's good. Thank you.

CHAIR MUNN: Any other questions or comments?

DR. CHMELYNSKI: Yes, this is Harry Chmelynski from SC&A. From what I had just heard in a discussion, the Weibull is only being

used internally to model results of simulation programs, and generally when you do that you don't have non-detects.

I was wondering, is it also used anywhere to fit data where you have non-detects?

DR. NETON: This is Jim. I don't know offhand if we've done that. Does anyone, Scott or Daniel?

MR. STANCESCU: I'm not sure how it's used in practice. I know that ORAU is using the VOS Tool and I think the Weibull can feed the data with the sensor values. But I'm not sure if this was used in practice. If somebody is familiar with the VOS Tool maybe might know this answer.

MR. SIEBERT: Well, this is Scott. My understanding is the fact that we are only using this for the combination of different distributions that are already set, and in

combining those distributions we're not using the Weibull, as far as I understand, to fit any raw data which would include detects and non-detects.

It would be multiplying a triangular distribution by a log-normal distribution by a normal distribution by those weird distributions that are in CLL, not actually fitting back to the raw data. Is that your concern?

DR. CHMELYNSKI: Okay, that answers my question then. If we're not going to have non-detects it's not a problem.

DR. NETON: Okay. Yes, because I can't think of any coworker model we've ever developed that's anything other than a normal distribution, if that's what you're getting at maybe. I mean a log-normal distribution. Okay.

CHAIR MUNN: Any other thoughts or

comments?

MEMBER ZIEMER: Hi, this is Ziemer. I sort of followed the logic here, and, you know, the statistics are beyond my capacity. But I just want to ask what action do we need to take as a Subcommittee, if any?

CHAIR MUNN: I didn't believe that we had any action. I thought we were attempting to satisfy SC&A's curiosity about why the Weibull distribution was used and how it is used.

And it was my understanding that if this discussion met the criteria of the original questioners that we had no further work to do.

MEMBER ZIEMER: Okay.

CHAIR MUNN: Am I incorrect in that, SC&A?

I don't hear any.

MR. KATZ: This is Ted. I'm not

SC&A, but that is correct. This was raised by SC&A for reasons that have been talked about, and if there is no further issue then you're finished with it.

CHAIR MUNN: Yes, that was my expectation. Is that correct for you, Paul?

MEMBER ZIEMER: Yes. I didn't know that we had to take any specific action. I guess we have to confirm that SC&A is comfortable with this.

CHAIR MUNN: Yes, and that's --

MEMBER ZIEMER: I think as I understand the discussion, it appears that SC&A is okay on this. Is that correct? I don't want to presume it.

CHAIR MUNN: I don't know, but I don't think we can presume it. Is there anyone on the call from the original questioning group that has any further concern, or can we assume that your concerns have been adequately

addressed?

DR. FARVER: This is Doug, Wanda.

CHAIR MUNN: Yes, Doug.

DR. FARVER: They addressed my concerns about, you know, what parameters were used, so now we can move on with that. I'm a little concerned that the distribution is not mentioned anywhere in their technical documents or in their TBDs.

And I don't know if that'll cause conflicts later on such as they might have it written in their TBD where it says you'll use a normal distribution and add in, you know, geometric -- for deviation. I'm not sure if there will conflicts with that later on.

But I'm okay, since I believe we brought up the original question about what's it doing here, why is it being used and how is it being used was the big thing. So I understand it from a dose reconstructor's point

of view.

CHAIR MUNN: Thank you, Doug. My question to NIOSH then is do we have any internal documents, workbooks, anything of that sort that help address the concern here?

DR. NETON: This is Jim. I don't know what internal workbooks it would be. I mean there are eight separate distributions that are listed in the IREP input line for dose inputs.

There's triangular, log uniform, uniform, constant, I mean they're all there. I think when we do use a particular distribution it is discussed and documented as such, such as in the CLL model. So I don't know where else we would document use of it other than when we do use it. You know what I'm saying?

CHAIR MUNN: Yes, we certainly don't. But is that comfortable for you, Doug?

I'm not hearing a response. Are

you muted, Doug?

MEMBER ZIEMER: While Doug is trying to get off mute, this is Ziemer again. Will the White Paper itself, that is, the putting distributions to dose data, will that be attached to any of our documents in the database?

CHAIR MUNN: You know, I'm not sure exactly what we could attach it to --

MEMBER ZIEMER: Well, that's why I'm asking.

CHAIR MUNN: -- Paul.

MEMBER ZIEMER: It didn't arise in connection with a particular finding then.

CHAIR MUNN: No, it did not. It was a general question from the contractor, and I wouldn't know where we would logically do that. It is a part of the permanent record now, having been posted online so that it's easily referenceable by anyone who has a concern.

And from my perspective, being a part of the public record should be adequate for it since it is not connected to a specific finding that we were addressing in Committee.

Does anyone have feelings otherwise with regard to this very nicely done paper?

DR. BUCHANAN: This is Ron Buchanan. Where can we find this paper?

CHAIR MUNN: You can find it --

DR. BUCHANAN: I'm not looking at my screen now, but where is it located?

CHAIR MUNN: It's online under today's agenda in the Office of Compensation Analysis website.

DR. BUCHANAN: Okay, thank you.

CHAIR MUNN: Yes.

MEMBER ZIEMER: Well, that being the case, I think we've satisfied SC&A's concern and the document is available. I don't know that we need an action, but I think that

we have closed the item in a sense.

CHAIR MUNN: We have from my perspective. Josie, do you agree?

MEMBER BEACH: Yes, Wanda, I do agree with that.

CHAIR MUNN: Very good. Let's consider that item closed and we'll move on to OTIB-83, the findings report. That was a carryover that NIOSH was going to have for us today.

DR. NETON: Okay, this is Jim.

MR. HINNEFELD: This is Stu, and once again I think I'm going to defer to Jim on this discussion.

CHAIR MUNN: Thank you, Stu.

DR. NETON: Yes, this is Jim. I've got this one. Well, I've had quite a bit of time to reflect on this, and I went back and reread both documents again, you know, the 83 and the SC&A review.

And at this point, NIOSH does agree with the comments made by SC&A that the target audience is not well defined. There's a discussion of this Type L plutonium. Just for background reference, the review had to do with how we would treat plutonium-238 exposures, internal exposures, essentially on a complex-wide basis which is implied for this TIB.

SC&A raised a number of concerns about who exactly this would be applied to and also questioned the general applicability of the so-called Type L model that was developed since it was only developed on selected cases, I believe five cases of exposure at the Mound facility.

We do agree that this needs to be fleshed out better. Unfortunately it's going to take a while because we are going to go back and review the other Mound cases for possible

incorporation into the model.

We used five. I believe there's at least around 40 different cases that could have been evaluated. It turns out the five we thought were the best and, you know, for expediency purposes we used the five figuring they were representative, but at this point we agree that we need to go back and demonstrate that to some extent.

So we're going to back, rebuild the model based on the cases at Mound. But as importantly, I think we need to also demonstrate at least documentation-wise that the types of exposure, the Type L exposures are fairly standard anywhere plutonium-238 is handled. That to me is not clearly defined in this document.

It's understood like plutonium-238 behaved differently, at least is conjectured because of a high specific activity, but if it's

not clear to me, although it seems to be, some people think it's general knowledge that it's not maybe that Type L is specific to the type of microspheres that were being produced at Mound.

I'm not sure, but we need to go back and we'll add a section of this document that defines the scope and specifically under which exposure conditions we can expect this Type L material to be present. And if it's universally potentially present, no matter what type of plutonium-238's there we will make sure that's demonstrated and documented.

So we've got quite a bit of work to do to shore up some of the pieces of this, so we're going to put this on our program planning schedule and work through the issues. But at this point we do accept SC&A's critique of the document itself.

CHAIR MUNN: How many findings do

we actually have?

DR. NETON: Well, I believe there are nine findings, but in my opinion many of them tend to be related to the same issues.

MEMBER BEACH: I thought there were 14.

DR. NETON: Right, 14. There's a lot of findings, but a lot of them have to do with the representativeness of the Type L model and who this model is going to be applied to. And those are the two major issues.

MEMBER BEACH: Yes, four key items in the findings.

MR. MARSCHKE: I agree. This is Steve Marschke with SC&A. I agree with Jim that's basically there's a lot of findings but there's a lot of duplication in some of the findings. And so I think they do collapse down to maybe four or five.

CHAIR MUNN: Is it possible that we

could request SC&A to take a look at that and see if we could eliminate some of the extraneous findings? If we can combine them into a single one it would be expedient, I think, from our point of view, and I think it would be helpful for NIOSH.

Am I speaking out of turn, Jim?

DR. NETON: Oh no, absolutely. I mean it would be a lot easier for us. I think the two issues I summarized do cover most of them. I'll not say all. There may be a couple other ones. But yes, that would be extremely helpful to us.

MR. MARSCHKE: Yes, I'll work with Joyce. Joyce was the primary reviewer on this, and I can work with Joyce and try and get, you know, combine some of these together.

CHAIR MUNN: Okay.

DR. NETON: And we'll put this on the schedule and I should be able to report, you

know, where we are on this once we get on the schedule and move forward.

CHAIR MUNN: Good. Could we make the request that next time SC&A will bring us suggestions for combining the findings and NIOSH will provide us with their anticipated rough schedule for addressing them?

DR. NETON: Sure.

MR. KATZ: Yes, and this is Ted. If I could just suggest, SC&A, feel free to chat with Jim in doing that so that you can sort of get to the endpoint before the Subcommittee meeting.

DR. H. BEHLING: Yes, Ted. We will do that.

MR. KATZ: Thanks a lot.

CHAIR MUNN: Okay, that's great. What is your desire? This would be a good opportunity, I think it looks like a good point in the agenda to break for lunch. But I don't

want to do that if you would prefer to keep going for another half hour.

How do you folks who are on the east coast feel about that? Would you prefer to go to lunch now or to keep moving through the agenda?

MEMBER ZIEMER: This is Ziemer. I wouldn't mind keeping moving a little bit, but I'm flexible.

CHAIR MUNN: All right. NIOSH, do you want to stick with it?

MR. HINNEFELD: It doesn't particularly matter to me. I can always eat lunch or I can --

CHAIR MUNN: Okay, then let's go ahead and address OTIB-34 then because I think some of the PER findings may take us longer this afternoon. So let's take a look at OTIB-34. I believe that's SC&A's Finding 4 and Rev 1, Finding 7 and 8.

DR. H. BEHLING: Okay, that's mine. This is Hans Behling. And I just want to play catch up with some of the people who may not recall that Finding 4, really, was the finding that was identified by SC&A in our previous review of OTIB-34 Rev 00.

And so it was really not an integral part of the more recent review of the revised OTIB-34 among the findings that I identified there, but I'll go over it.

In our original review of OTIB-34 Rev 0, the finding was as follows, and I'll just read it. The assumed and predicted intake fits versus the values in the first approximately five years are much less, from about 3,800 days to 7,200 days. The model fit is much higher, indicating that the percentile used for deriving the intake should be greater. This in turn would be more claimant favorable.

And you'll see what this refers to

in a second. In response to that finding, the original finding in our 2007 review, NIOSH responded and it's unclear what is meant by this comment. In the fit to the early data there are eight results above the line of fit and eight points equally below, which would seem to indicate an adequate fit.

And if I can draw your attention to what's on the screen, you will see at this point the exact statement and how they fit into the picture.

When you look at Figure A-28, you will see the blue dots and those indicate the actual integral measurements at the 50 percentile value for data up to around 6,000 and some odd days after 1968.

And if you count the blue dots, you will see the blue dots on the left hand side, there will be eight dots above the best fit line up to the point of about 3,600 days, and then

there are eight dots below the line beyond that. And that was the issue here. And I looked at the information, and I guess I agree with NIOSH that this is the fit that was established by IMBA. And it's obviously that you're not following a clear cut pattern of the actual empirical data points that define that fit, but if that's the best fit that was established then there's very little one can do.

In the end, it's a fit that involves a large number of people who were monitored over this period of time. And if that's the best fit, then I'm not sure I know what one can do to say it shouldn't be used as such.

Now I will say this. In looking at this whole issue, I have a certain question in terms of how IMBA can be used to actually do this. And I'm only throwing this out without necessarily saying I'm right or wrong and I'm

looking for answers.

It is my understanding that IMBA really is not intended to be used in this kind of capacity. In the end, IMBA, as we normally view IMBA, is used to take bioassay data for a given one individual over a period of time, and then on the various assumptions that says this person was subjected to chronic intake throughout this whole period, let's say it's a 10- or 20-year period and this is what we best assume was the intake that caused -- daily intake, using a chronic assumption exposure intake -- this is the best results that we can take in terms of saying what is the daily intake, from which we then estimate our dose to a particular organ in question.

Now how IMBA can be used to do the same thing in behalf of an entire large group of people that are part in this case from '68 to '84, is something that I'm not able to

comprehend. Because as is clear, when we have a chronic intake for a radionuclide that persists in a body long, for long periods of time, there is successive accumulation of radioactivity in that organ so that the intake that occurred five years before the next particular assessment will contribute and continue to contribute.

And when you have a dynamic population where people come in and out of that population at will, which you don't know, I'm not sure how you can use IMBA.

After all, IMBA is really a sophisticated model that implements the International Commission on Radiological Protection, various models involving the respiratory tract, the GI tract, tissue dosimetry and all the other biokinetic and bioassay models that are adopted into IMBA.

How that applies to a group of

individuals is something that I'm not able to understand, and how IMBA can be used to actually establish a best fit for an entire worker group over a period of time.

And so I'm throwing this question out. So my question transcends, actually, the issue that was identified in this particular finding, and I'm hoping that somebody can answer my question.

MS. K. BEHLING: And just for clarification, OTIB-34 is the internal dosimetry coworker data for X-10, just to remind everyone.

CHAIR MUNN: Thank you, Kathy. Thank you, Hans. NIOSH, respond to Hans' question?

MR. HINNEFELD: Well, I don't know that --

DR. NETON: This is Jim. I wasn't prepared to talk about this.

MR. HINNEFELD: Maybe Jim can make some comments.

DR. NETON: Is that Stu?

MR. HINNEFELD: Jim, yes, that was me. I was talking, but I don't have anything particular to say here. Maybe you can comment on it?

DR. NETON: No, again I wasn't prepared. This is a fairly general overarching type discussion to your point, but I think what you need to look and just think about or a person needs to think about is, is the 50th percentile of the distribution of the monitored workers representative of what the exposures were in that facility?

So we're modeling, that fit goes through the 50th percentile of the bioassay points, the 50th percentile of the bioassay on a year-by-year or whatever selected period basis.

And if you can convince yourself that that's representative of what the workforce was exposed to, then I don't see, there's nothing special about IMBA or a single person versus the 50th percentile distribution being used.

DR. H. BEHLING: Well, what it comes down to, I believe, is the following. You establish the 50th percentile value which represents each of those blue dots using IMBA. In other words, you probably took --

DR. NETON: IMBA did not establish that 50th percentile dot. That is the 50th percentile of the measured data.

DR. H. BEHLING: The measured data.

DR. NETON: Bioassay data. And then IMBA decides what type of intake is needed to make those dots exist like that. The y-axis is excretion in dpm per day. That is actually the urinary excretion of the worker.

DR. H. BEHLING: Yes, I understand that. I understand that.

DR. NETON: IMBA doesn't come into play in that at all.

DR. H. BEHLING: Well, to me it sounds much like a very primitive fit line that you would be able to establish using such as a least square or something else, and would have very little to do with IMBA.

DR. NETON: Well, no, but IMBA, you have to estimate what the chronic intake was in order to get those bioassay data points, right?

DR. H. BEHLING: I understand that.

DR. NETON: How much does a person have to inhale every day of every work day in order to see this straight line excretion function? Now what happens though, in the beginning of the excretion period it has to start at zero necessarily because there is no intake, right? So that means --

DR. H. BEHLING: Yes.

DR. NETON: -- you ramp up a little bit and then go through. But on average it gives you the right value.

DR. H. BEHLING: Well, I fully understand what you're saying, but I will disagree with it because when I look at the curve, in other words at Time Zero or close to zero whenever that first blue dot appears, there was a fairly high excretion rate that corresponds to an intake value that doesn't match the fit line. And I'm just not --

DR. NETON: But then you also have to think about what happens, we tend to model periods of time that have a similar pattern, like you see the blue dots on the screen there. Those seem to be a similar excretion constant pattern.

It will drop over different periods of time, so then the next model period will

start from zero again and move up, but it doesn't account for any of the residual excretion that is still occurring from that previously modeled period.

So in essence, what happens is if a person worked in both model periods, they would receive the first model period intake, the second model period intake, but they would also continue to receive any inhalation that's residual from the first period that's not even included in there. It tends to overestimate the doses quite a bit for intakes.

DR. H. BEHLING: The real question is that for other radionuclides those kinds, the distribution seems to have fragmented into multiple time periods. In this case for americium we only have two periods. One that goes from '68 to '84, and the second one for the shorter period, basically from '85 to '88.

And why wouldn't you choose to have

perhaps a more, a higher number of time periods to segregate that distribution between '68 and '84, because you did that for plutonium?

And I don't know, it seems so arbitrary to essentially establish a fit for the '68 to '84 in the case of americium and have more discrete periods for which an intake was developed before that same period. You could have easily --

DR. NETON: Well, if you look at the data that are on the screen though, it does appear that it was a fairly constant, chronic excretion going on there.

So you can, you know, if the excretion rates are fairly constant over time you're going to fit that as long as you can, because that's what you're trying to model is a chronic scenario. You wouldn't want to break it up into multiple periods because it doesn't make any sense.

DR. MAURO: This is John. I always try to get things down to something that I can simply understand. You've got a dpm per day y-axis with a blue dot, the very first blue dot for this americium which is 0.12 dpm per day.

DR. NETON: Right.

DR. MAURO: That's an excretion rate for a single person or for the average number?

DR. NETON: It's the 50th percentile results for the population in that --

DR. MAURO: Okay, so you've got a population of workers where you have collected excretion data, and the 50th percentile geometric mean is that number, that 0.12.

Then you have taken, I guess, at another time period which may be the same population of workers or a mix of some new and some old and so forth, and you have another number. And so

in effect what we're looking at is the geometric mean of the excretion rate or of americium in a group of workers at different time periods in sequence.

Now to go back to Hans' question. Now if you're saying to him, okay, I have some data on different time periods and what people are seeing, and I would agree just from a common sense point of view it looks like, gee, you've got all these groups of people over this time period which covers a number of years, apparently, they're all around, you know, anywhere from about 0.09 to 0.12 dpm per day.

So as far as I'm concerned, what this says is that each, and notwithstanding whether you run IMBA or whatever you have, it's almost like let's walk away from that for a second. All you're really saying here is that the concentration or the excretion rate of americium in each cluster of people on that day

when that sample was collected from those people, they're all the same. I mean to me the difference between, well, within a factor of 2.

So as far as I'm concerned given the uncertainty in all these things we talk about, what you're really saying here is every time we grabbed a group of people over this time period, which covers many years, those group of people always had the same excretion rate. And that transcends IMBA. All that says is that's what we see. We're not seeing anything's changing.

Now, so in my mind, I guess maybe I go back to Hans' concern, and maybe I'm referring to it correctly is, where does IMBA come in? All you're really saying is, look, hey, we've got all these people, we've got all this data. A lot of data over a lot of years. I don't know how many people are represented in each dot but it could be a lot of people. And they always have the same excretion rate.

And to me a difference of between 0.09 and 0.15, to me as a biologist that means nothing to me. That means there's no difference. Maybe there are those who see that as important. I don't.

So now I know that over that time period, everybody that you were looking at had this chronic excretion rate that was always around, you know, 0.1, 0.12 dpm per day. And then the next question is, okay, what type of chronic intake rate by all these people over all this time would give you that excretion rate? And you're done. And that of course is where IMBA would come in.

DR. NETON: And that's what we've done. If you go back down to the graph at the bottom there, the chart.

DR. MAURO: Yes.

DR. NETON: Go back. Could you go down to the table?

DR. MAURO: Yes.

DR. NETON: There's a table, and it said between 1968 and '84 a person would have to have inhaled 6.673 picocuries per day?

MR. MARSCHKE: Disintegrations per minute per day.

DR. NETON: Dpm per day. They have to have inhaled that much per day in order to get that curve.

DR. MAURO: Right, and not even a curve.

DR. NETON: That's the intake per period --

(Simultaneous speaking)

DR. NETON: What's the printed --

DR. MAURO: Yes. I have to say in the simplest terms, what you're simply saying, listen, all these people are more or less being exposed to the same concentration, having the same intake over all this time period, which is

very surprising. So it's a very stable, chronic situation, and you could very well back-calculate out what the intake rate was.

Now of course --

DR. NETON: That black curve there is the IMBA curve that was fit for those data points for a chronic exposure and that's the best fit for the data that said what chronic exposure would give you those data points, and that's what we came up with.

DR. MAURO: As if it was a single person that had this data?

DR. NETON: No, the 50th percentile person had the data. All these data points are for the 50th percentile person.

MR. MARSCHKE: It's a single 50th percentile. It's a single person. I mean IMBA runs on a single person. So it's really a single person who has the 50th percentile excretions.

DR. NETON: Each point is the 50th percentile of each year. So it's a 50th percentile excretion value of the distribution for each year.

DR. MAURO: Got it. But as I said before --

DR. NETON: Then you fit a curve to that and say what would you have to inhale every day in order to maintain that type of excretion pattern if you were the 50th percentile worker?

DR. MAURO: Now why wouldn't you do something much simpler and simply say, listen, I have all of these data points, it looks to me that, you know, for any group of people, the excretion rate, the highest we see for a given group was 0.15, and you say to yourself, well, that certainly would be an upper bound of what an excretion rate would be for all these people over all this time.

We know that most of them had less

than that. All of them had less than that. And why wouldn't you simply say, given that I have a person that has been chronically exposed over all this time and he continuously had a constant excretion rate of 0.15 picocuries, I'm sorry, dpm per day, then ask myself the question and run IMBA, what would my chronic intake rate be that I would have --

(Simultaneous speaking)

DR. NETON: What you're saying, change all of those points to 0.15. All the blue dots become 0.15?

DR. MAURO: Yes. I mean I'm not saying you should do that.

DR. NETON: You're ignoring the data. I mean why bother? It doesn't make any sense to me to pick the highest value --

DR. MAURO: I just picked that to say, if anybody wanted to say, listen, how bad could it have been --

(Simultaneous speaking)

DR. NETON: But the point is John we have the data. These are the data. They fit a fairly straight line across, which indicates a chronic exposure pattern and we fit a chronic exposure model, the best fit that IMBA can produce, and got that result.

DR. MAURO: You know what it is? I think it may be, the thing that's confusing me, and it's just probably my own lack of knowledge, is, you know, why wouldn't you simply take all those data points, come up with the geometric means and standard deviations, say okay, here is the, for this population of workers over all this time, they're all, you know, this represents what the excretion rate has been, chronically, as a reasonable geometric mean or upper bound, say --

DR. NETON: You don't have to fit IMBA to it to come up with an intake.

DR. MAURO: Then I would run IMBA after that. See, I don't understand how these numbers, like Hans brought up initially, I understand Hans question. He says why would you somehow need to use IMBA to represent what these dots mean? I guess I'm having trouble with that --

DR. NETON: You have to use IMBA to come up with an intake.

DR. MAURO: Yes --

DR. H. BEHLING: But Jim, this is what I'm constantly going to ask is, you're not using IMBA the way it was intended to be. You're looking at the 50th percentile value for each of the time frames that represent the dots and then you're trying to use an IMBA fitting for those.

I mean to me, if I look at those blue dots, you would almost, if you did a least square on those blue dots you would end up with

a value somewhere around 0.12 dpm per day excretion.

And rather than a ramping up, I don't see the ramping up here. This is what IMBA would suggest --

DR. NETON: But Hans, that's a function of the assumption of using a chronic exposure model. A chronic exposure model can start at zero at some point by definition.

DR. H. BEHLING: Of course. I understand how IMBA works for a single individual. I do not understand how it applies to a collective group of individuals.

DR. NETON: We're trying to estimate what the unmonitored workers' exposures were, not what these monitored workers were exposed to, period. Right? This has nothing to do with what the monitored workers' exposure was. Totally irrelevant.

This is trying to estimate what our

best estimate for someone who had zero monitoring that could have been potentially exposed, what they could have received. And our approach, this has been this way for over 12 years, is that the 50th percentile of the monitored worker distribution can be used to establish a chronic exposure scenario and that's what we do.

I mean I don't --

MR. STIVER: This is John Stiver. If I could jump in for a second here. I mean, you know, John and Hans, this is the classic pooled data coworker model approach that we looked at many, many different times, where NIOSH will pull together a series of 50th percentiles and that look to be kind of a homogenous representative of a given intake regime, if you will.

And it really, the IMBA is basically being used to model, as Jim said earlier, the

50th percentile individual which will then be used to assign a coworker dose. And each one of these is looked at as though it's a single intake with no previous contribution from an earlier intake regime. And, you know, we talked about this a lot and I'm pretty sure that we've come to a general agreement, I mean without regard to the OPOS application and so forth. But this type of approach to coworker modeling is okay and appropriate.

I may be missing something but it's been my general understanding.

MR. BARTON: This is Bob Barton. Could I make a comment here? I think from what I'm hearing, and Hans, correct me if I'm wrong, I think part of what his concern is is that when you fit this line here, I mean you kind of assume, basically, what this modeling is, this is your average worker if he was chronically exposed from 1968 to 1984.

Now how you select that time period as I understand it is sort of in the eye of the beholder. You kind of take a look at it and you try to group bioassay results together, but there's really no guidelines as to how you select that, as John just put it, intake regime.

I mean if took these blue dots and instead of fitting a line through all of them, let's say we just arbitrarily cut them in half, that's going to increase what intake that IMBA would model.

So I guess I would add is there any way we could, I mean are there guidelines as to how you select the actual intake regime or how you group these bioassay results together? Because this is modeling a chronically exposed worker for this large time period, but if you were to model any one of these individual data points, obviously the intake is going to be fundamentally higher. Hans, did I get

that correctly?

DR. H. BEHLING: Yes. As I said, I'm just not sure why this time period is only one large time period and a very small second time period, when I compare that to the data involved in Figures A-26 and 27, which are dose for plutonium-239 where we have, I guess, four time periods which are modeled here and the data doesn't look all that different.

(Simultaneous speaking)

DR. NETON: I can't see them. I'm sorry.

DR. MAURO: Could we scroll up?

(Off the record comments)

DR. H. BEHLING: I didn't ask for that, but in addressing this issue, which by the way as I said was not an issue that I identified, I'm only responding to --

DR. NETON: Oh, we're passed it now. Anyway the bottom line is, it is as Bob

Barton said. There is a professional judgment involved when you do any kind of an intake assessment like this, and it has to do with the patterns.

I mean you can see on A-24, there's apparently a clear reduction here in the amount of material being excreted as compared to the first time period.

DR. H. BEHLING: Yes.

DR. NETON: And that's what's done. I mean I don't know how you could do it any better than say, you know, it's, I don't know. I don't know if you can create quantitative criteria on exactly how many years' worth of data are fit.

In general you will see that there are trends in the data that seem to follow along. You'll see like a certain period where it looks like it's fairly uniform and then it'll drop. Maybe the project, some project was

terminated and the chronic exposures went way down. Well, then you would model that chronic exposure as a separate time period.

MR. STIVER: Okay, Hans, could I just jump in? This is John Stiver again. You'd mentioned earlier that like if this was a radionuclide you'd have different exposures. And, I guess, correct me if I'm wrong, I'm trying to interpret what you're getting at.

You're saying that there's a lack of consistency if you were indeed getting both of those nuclides in a chronic exposure over a given period of time then you should be using the same intake regime as opposed to having different regimes.

But, you know, if you had different types of campaigns where different materials were being used, then I think you'd have to set your intake regimes that would correspond to the exposure scenario under question of the

particular nuclide.

I kind of, is that what you were really getting at or am I off base on that?

DR. H. BEHLING: I'm not looking to make a change here in the way we assess a worker, coworker model. I realize there are limitations.

And I guess the coworker model as it's being used I would consider much more credible if that coworker model consisted of a fixed group of people that were there in exact numbers and same people not coming in and out of the workforce, so that it would, in essence, simulate an individual who was there and whose data we were assessing on an individual basis as opposed to a group of individuals.

I guess the weakness of a coworker model is that that similarity between multiple bioassays for a given individual as opposed to many bioassays for a group is kind of lost when

you realize there's a certain amount of dynamic movement in terms of the size of the work force, people coming in and out, retiring, entering the work force, where these data are not necessarily what IMBA intended to do here.

And I'll accept the fact that this is not going to change. It's the best we can do. But there are some issues here that I guess are subject to criticism when you use IMBA.

DR. NETON: I understand all the points you're making, Hans. But I like to think about it this way, is that we're not, we're trying to say what was the potential exposure for a person who wasn't monitored.

DR. H. BEHLING: Yes.

DR. NETON: And so I think we might agree, I would hope at some point, that for an unmonitored worker could have received about the 50th percentile of what the workers were experiencing on an every-year basis, right?

I mean it's not that worker, but if he were in the work force he couldn't have probably been exposed to more than what the 50th percentile worker was exposed to every year.

So it doesn't matter whether it's the same worker or not, if I am an unmonitored worker I could have been exposed to about the 50th percentile. And then we fit that curve. So it's not so much about what individual worker exposure was, but if I was not monitored and I worked in the plant, I don't believe that the worker would have received more, have had excreted more than what the 50th percentile worker excreted and been unmonitored. That's all we're trying to say here.

I think it's a little different way of looking at than saying we're trying to exactly reconstruct the dose to every single worker during that time period. We're not. We're trying to say what was the potential

exposure experience of an unmonitored worker?

And I would submit that it's unlikely that an unmonitored worker would have received more than a 50th percentile excretion value every single year of that time period. And then that begs the question what that included intake could have been and that's where IMBA comes in. So I don't know.

DR. H. BEHLING: No, I'm going to cut this short. I wasn't looking to change the way we do business and I accept that the numbers that correspond to that table. I'm talking about Table A-12, which as you already mentioned would suggest that the average daily intake rate for americium-241 is 6.673 dpm per day, and I think that's not an unreasonable approach.

I'm just trying to satisfy the initial concerns that were raised back in 2007, and I did have some questions about the use of

IMBA, but I think this is as good as we can do.

DR. NETON: I appreciate that. Thanks.

MR. BARTON: This is Bob again. I think, fundamentally, and we could all agree that the finer you parse the intake assessments the higher the actual calculated intakes would be. As I said, Figure A-28 is essentially modeling as if you were chronically exposed from '68 all the way to '84.

Now let's say if you worked for a shorter period of time and still had that average excretion rate, you know, whatever the time period it is, the actual intake would be higher.

And I don't know if there's a way around, I don't know, sort of adjusting the coworker model for workers who were there on a shorter duration but might have had that average excretion rate which would actually

represent a higher intake. Because the more bioassay results you group together the lower the actual calculated intake will be, if I'm correct.

DR. NETON: Well, the more you slice the salami the more you start talking about incident modeling, Bob.

MR. BARTON: Right.

DR. NETON: And you've agreed that, well, we have adopted a chronic exposure model here and we're not doing incident modeling at all. I mean ideally if you do it on a day-by-day basis it's essentially acute exposures.

MR. BARTON: Well, we're not talking day-by-day. We're talking about a 16-year period here. I don't know. And again, professional judgment has to come into it because you have to try to select a period to model.

I just, I don't know if it would be possible to sort of develop criteria for how you select it or if we're kind of stuck where you just have to leave it to, I guess, visual inspection. You use bioassay results and let's see what they're about to say, and so we're going to group them all together.

It's just very, you know, as I said, I mean if you broke this time period from '68 to '84 in half, your calculated intake's going to be much higher for both intake regimes. I mean we're not talking about days here, we're talking about many years.

DR. NETON: Again the whole premise of the chronic exposure, if I look at those blue points, to me it looks like people are excreting about the same amount during that entire time period indicating a chronic exposure model would be appropriate.

DR. MAURO: Jim, I'm not disputing

any of that. The only thing that I've got is this little knot in my head that said what does that black line mean? In other words you have all these blue dots --

DR. NETON: That is the IMBA fit to that data point.

DR. MAURO: Yes, I understand. And to me, as Hans pointed out, they don't have anything to do with each other. In other words the IMBA, in other words to me --

DR. NETON: Because the chronic model starts out at zero, right? You can't be chronically, you start excreting, you know, on Day Zero, Time Zero, you have zero excretion and then you start breathing six picocuries per day and that's what that shows.

As you keep breathing six picocuries per day it goes up. What it doesn't show is previous monitoring periods where it was also modeled and there's some residual

carried over into that period that's not shown here.

DR. MAURO: You know, I would agree with that if every dot was the same person. In other words, every one of those dots was the excretion rate we measured on a given person on that day and then the next, and then you have, and that's the data that came out.

DR. NETON: But John, we're not modeling people. We're modeling a distribution because we're trying to figure out what the potential missed intakes would have been from an unmonitored worker.

And all I'm saying is if they excrete the 50th percentile of all the monitored workers, I think that's a fair bounding representation of their intake during that period of time. They weren't monitored, remember? We're not talking about reconstructing monitored workers' doses. It

has nothing to do with that.

MR. STIVER: You know, this is John again. I think we may be sort of straying far afield from, you know, the --

DR. NETON: I agree.

MR. STIVER: -- maybe the Procedure Subcommittee and getting more of the overarching issues about coworker modeling in general. And maybe we ought to try and get back down to the --

DR. MAURO: Yes, let me back out of this, because I'm just looking at the graph and I'm trying to make it make sense to me why an IMBA black line is on there.

DR. NETON: John --

(Simultaneous speaking)

DR. NETON: -- for 12 years.

DR. MAURO: Clearly you're comfortable with that --

DR. NETON: I guess you just woke up

and saw them or something.

CHAIR MUNN: So is the upshot of that discussion that we can or cannot accept the response that NIOSH has given to the finding?

DR. H. BEHLING: If I can weigh in on this issue, I would say I will accept NIOSH's explanation that there are eight points above the line, eight points below the line, and on average the numbers will somehow or other do justice to the unmonitored person by assigning him the intake values that are identified in Table A-12.

I mean it's not a perfect approach to doing this, but in the absence of data that's as good as we're going to get.

CHAIR MUNN: Thank you, Hans. That's appreciated. Can we then identify Finding 4 of Rev 1 as having been discussed and agreed in Committee, and the Committee recommends closure. Is that appropriate?

DR. H. BEHLING: I think so.

CHAIR MUNN: All right.

Paul?

MEMBER ZIEMER: Yes, I'm agreed.

CHAIR MUNN: Josie?

MEMBER BEACH: I agree also.

CHAIR MUNN: Steve, can you input that finding for us?

MR. MARSCHKE: A lot more names in here than we had before.

CHAIR MUNN: We're getting very popular with this system.

DR. MAURO: Hey, Steve, my wife used to be a typist and she could do 120 words a minute, all right?

MR. MARSCHKE: I do 12.

MR. KATZ: That's good, John.

CHAIR MUNN: That was when we called it typing, now it's keyboarding. That's different, John.

DR. OSTROW: This is Steve. I can do about 50 words a minute but not all of them are correct.

CHAIR MUNN: Thank you very much.

MR. MARSCHKE: Okay, that's what I put in.

CHAIR MUNN: It's fine with me. Any problem with that from anyone? If not, that's the way it will be and we will close this finding for Rev 0. And we will go on to Rev 1, Findings 7 and 8. I believe that's a NIOSH comment?

MS. MARION-MOSS: Hi Wanda, this is Lori. For Finding Number 7, I think since the last time the Committee addressed this particular finding, I believe we have revised Document OTIB-34. We are currently on OTIB-34 Rev 3.

So to bring the Committee up to speed on how we've addressed this, I'm going to

ask Joe Guido to get the Committee an update on the proper selection of the 95th percentile.

Are you on, Joe?

MS. THOMAS: Lori, this is Elyse. Joe was on and he had another call, so let me try to get him back on to address that. It might take a few minutes. He just got off for a 1 o'clock call, but I'll see if I can get him on.

MR. HINNEFELD: Well, now this is Stu. I mean our response, our latest response just says that we've added wording to the Rev 2, which I think is still there in Rev 3 of this document that describes when to use the 95th percentile. I don't know if anybody's looked at that or not.

I mean then I look, let's see, we're on Finding 7, right?

CHAIR MUNN: Finding 7, correct, of Rev 1.

MR. HINNEFELD: Okay, expand that. Yes, our latest entry is additional guidance on assigning that 95th percentile has been added to Section 5 of the OTIB. And then what that OTIB said, I had it open. What Section 5 says is in most cases, doses for individuals who are potentially exposed routinely should be calculated from the 50th percentile intake rates by assuming the solubility type that results in the largest Probability of Causation.

GSD values have been not less than three, et cetera, et cetera. For cases in which there's justification that the individual might have had larger intakes than the 50th percentile intake, the dose reconstructors should use the 95th percentile intake rate input into IREP as a constant.

So there is, you know, there's instruction there, and I don't think that

there's a lot of hope of identifying the specific cases when, and a priori or in advance what specific conditions would put people in that situation.

I think you'd have to evaluate the claim individually to determine that hey, this person looks like maybe they shouldn't be in the 50th percentile, but rather should be in the 95th.

DR. H. BEHLING: Well, that was my concern from the very beginning. It's nice to have the option, but in the absence of defining the specific incidents when the 95th percentile applies, it is too arbitrary on the part of the dose reconstructor to make that decision.

And one of the things I've always been concerned about is the option or the nonprescriptive approach when you do dose reconstruction, which for one instance, for one dose reconstructor means I think this person

qualifies for the 95th, and the other instance for dose reconstructors is no, he's 50th percentile.

And consistency is a big issue that I always want to look at when I assess a dose reconstruction and I sort of say is it the luck of the draw that defines whether or not the person goes over the 50th percentile. And I would like to see a very, very prescriptive approach to avoid that issue out of fairness.

MR. HINNEFELD: Well, that's a valid point. The consistency question is a valid point. I still though, I think that there would almost have to be a finding, a programmatic finding that there's a category of worker at -- and this is what, X-10 -- that was heavily exposed but not monitored and in that case they should be put into this.

So I don't know that we've made a finding like that at X-10, but it's a

possibility that we could.

DR. H. BEHLING: Let me offer, for instance, an example so as to at least give some limited guidance. I would say the 95th percentile would be very appropriate for a worker who is an operator, in-plant operator that is considered to be a high risk, but for some reason or another his dose records, we know his employment period coincides with potential high exposures involving other people who were operators, but for some reason his dosimetry records are missing that we can't account for.

I would say that would be a perfect example to say we must give him the benefit of the doubt based on the time period of exposure, his job description as a high risk individual but there are no data.

I think this would be a perfect example for saying this is when you should consider 95th percentile in lieu of the 50th

percentile that might be very, very fair for the other average people for whom we have no data.

MR. HINNEFELD: Yes, that's a good point.

DR. MAURO: This is John. Let me add a little richness to this too, which is a little contrary to what Hans is saying, with all due respect.

I've reviewed mainly AWEs. I don't know how many now, and I can't think of a time when the judgment was made, there was discretion to be used by the dose reconstructor in what airborne concentration should be assumed.

And in my opinion it was always an overestimate. In other words, so my experience with AWEs is that when push came to shove and the dose reconstructor had to make a judgment, he always erred on the side of the claimant. That's AWE world.

So I mean, I'd like to speak, you know, in a positive sense here. Those judgments have been made in a prudent way, if not a very claimant favorable way, for the dose reconstructions that I've been involved in, mainly AWEs.

DR. H. BEHLING: Well, John, this is not an AWE. This is for the X-10 worker.

DR. MAURO: There you go, I mouthed it off prematurely. Okay.

(Simultaneous speaking.)

DR. MAURO: Okay, okay.

MR. BARTON: Well, actually, John, I mean I can give some sort of, an example of precedence is Simonds Saw and Steel in which the decision hasn't come out but I essentially agreed in principle that the 95th percentile is going to be applied to the plant workers, or essentially who would be assigned as a radiological worker.

And the 50th percentile of the GSD would be assigned to sort of like the ancillary workers, you know, that may have been in the plant briefly but weren't actually working inside Simonds.

So I think that's what Hans is saying is you want to take out the judgment call. Essentially, you can't be prescriptive for every situation, but that's only an example of where we say, okay, the 95th percentile is appropriate if you were an unmonitored plant worker at Simonds. And that's --

DR. NETON: I've got this task at hand right now to do the implementation guide for coworker models, and that's one of the issues that I'm wrestling with in that draft document right now.

And we talked a little bit about it at the Idaho SEC Work Group meeting, SEC Issues Work Group meeting. And, you know, I think

that's the appropriate place to put this, the guidance, and I think it'll be fleshed out over time as that document becomes closer to completion.

We've had lists before, and I know there are some documents that do mention some Classes of workers as being appropriate to have the 95th percentile. I've forgotten which ones.

MR. MARSCHKE: Jim, this is Steve Marschke. I think if you look at OTIB-20, when we were reviewing the construction trade workers we had a sentence that was added to OTIB-20 which basically identified construction trade workers, in particular pipefitters, that basically should be applied to these.

DR. NETON: So this has resurfaced periodically and --

MR. MARSCHKE: It has. And if I

recall from some of those discussions, and to paraphrase NIOSH, what I remember as being NIOSH's position was that these assignments of percentiles are not the sole, or, you know, the dose reconstructor is not the endpoint on these assignments. They get reviewed multiple times.

DR. NETON: They do. But I do agree with Hans' issue on consistency. My problem is when you start naming a couple categories of workers, which we have in the past, then people say, well, what about this Class and this Class? Because there's a lot of workers out there that probably were more heavily exposed than you would think just based on their job classification.

So I'm reluctant to have a definitive list. I do agree that someone like a chemical operator who clearly should have been monitored and his dose records were lost,

that's a no-brainer. But beyond that I'm not quite sure.

Anyway I think the place to address this and maybe to carry this on is in this implementation guide that I'm putting together, and it's certainly one of the issues that have to be addressed.

MR. HINNEFELD: Okay, so if we enter another response that says that this is being addressed in an implementation guide for use in coworker models and we expect some additional guidance to come out of that, would that kind of put this to be in abeyance or something for now?

MR. MARSCHKE: Or would it be transferred?

CHAIR MUNN: Well, I don't believe it would be transferred. I believe it would be in abeyance, if that is in fact agreeable to the rest of the parties here. Would the addition

of this information in an implementation guide be acceptable to SC&A?

DR. H. BEHLING: I would be agreeable if the current wording hasn't been changed the way Stu read them would be just slightly amended with a single example.

As I said, I'm not looking for to broaden the scope by which 95th percentile is assigned. For an unmonitored worker, I think it's reasonable to conclude what NIOSH has always stated that if you weren't monitored you were probably not among the high end exposed individuals. I agree with that.

What I do want to say, when it is used at the discretion of the dose reconstructor that it's used properly not whimsically. And it should be probably highly restricted when the 95th percentile is used and correspond to unusual circumstances like the one I said.

An operator who's been there for many years whose coworkers are among the people who were maximally exposed but for some reason that workers' dose records have been lost, I think that's a no-brainer, and I think the kind of limited situation that the 95th percentile should be used.

CHAIR MUNN: So if we, of course when the change has occurred it comes back to us to agree that it meets the criteria anticipated.

So is it amenable for all concerned for us to indicate this particular Finding 7 for Rev 1 is in abeyance awaiting a NIOSH implementation guide which addresses the concern?

DR. H. BEHLING: Yes.

CHAIR MUNN: Very good. Steve, can you make the change for us? Just after discussion? Will be addressed in

implementation guide. Excellent. Is that wording acceptable to all involved?

DR. H. BEHLING: Yes.

CHAIR MUNN: Very good. Thank you, Steve. Appreciate it.

MEMBER ZIEMER: There's a spelling error, did you catch that? The last phrase? Yes, there you go. Okay, you've got it.

CHAIR MUNN: Now let's move on to Rev 1 Finding 8.

MR. HINNEFELD: Well, this is Stu Hinnefeld and I'll start here once again. And this had to do with is there additional evidence that this particular data column is a daily 24-hour excretion?

And there is response here, refers to a document in SRDB which is the data dictionary for the database and a description of it, and that is attached. That SRDB document is attached and it's Pages 12 and 13.

So if you scroll down to the, see, there's the data field disintegrations per minute for 24 hours, and then I think the description's on the next page. No, this is the one. This is actually the one I was thinking of.

The next to the last entry there, position 70 to 78 as a numeric value for the disintegrations per minute for 24 hours to one decimal place." And so this was, I think, the database that the data was drawn from and this is the data dictionary for that database.

And so we felt like this is sufficient evidence that it's a disintegrations per minute for 24 hours' excretion and that the data are presented, you know, essentially to one decimal place. And so you essentially have to insert that in the way you look at the data.

Now if Joe's on, he can probably say

I did it wrong. But I don't know if he's on the phone yet or not.

CHAIR MUNN: Do we have Joe yet?

Apparently not.

MS. THOMAS: This is Elyse. Yes, I think he's on that other call, but Matt Arno may be on. Matt, are you on?

MR. ARNO: Yes, I'm on.

MS. THOMAS: Okay, yes. Can you explain?

MR. ARNO: Yes, that was an accurate explanation of what the data dictionary means and how we interpreted it.

MR. HINNEFELD: So anyway, so that's we presented and that's what we put in the database as our response, so we feel like there is adequate information to give us confidence that that data is dpm per day.

CHAIR MUNN: Is that acceptable for SC&A?

DR. H. BEHLING: I guess what prompted this, and I'm not sure I can reconcile what I just saw on the screen here. But what prompted me to raise the question were data that I collected. Just sampling data that go back to 1951, where in the first column was the dpm per sample which turns out to be identical to the dpm per 24-hour period. And this is for 1951.

And I mean that raised the question, does a dpm per sample necessarily equate, unless one were to sample in the case of reference man at 1,400 mL urine sample in saying this is what we saw in that sample. If it was a fraction of the 24-hour urine excretion value then I would have to say that does not apply.

And so I'm not really sure how that applies to other years, I only took a sample. So when I raised that question and I raised the question as a conditional question, I was

basically looking at 1951 data that only identifies, and I assume the primary source of that data really represents the dpm per sample which then is presumably transferred to mean the dpm per 24 hours.

And this is what my question was, can you be reasonably sure that a sample, that the activity per sample corresponds to a 24-hour sampling of volume?

MR. HINNEFELD: Matt, do you have anything more to add?

MR. ARNO: That was the general practice at Oak Ridge National Lab was to collect a 24-hour urine sample. Per sample is per 24 hours regardless of volume that is the person's actual excretion over 24 hours.

MR. BARTON: This is Bob Barton. Just an observation. That reference that we were just looking at, it appeared as if there were two columns. One was the dpm per sample,

and then the next column over, after I get some filler space, was the dpm per day.

So it seemed like they were adding them both, both the per sample activity and then they were converting it over to dpm per day. So it looked like both values were there.

MR. ARNO: I mean pretty much the only exception to the 24-hour samples on these was tritium, so if you're looking at tritium data you'll see the difference, but for everything else pretty much per sampling per day are equal.

CHAIR MUNN: So is the feeling that the response is adequate? May we close this item?

DR. H. BEHLING: Well, I guess on the assumption that the care was taken, I can't imagine that all these urine samples were 24 hours. Normally, you know, if a person shows up to work and you take a sample, if it's only

a partial 24-hour volume just adjust it or standardize.

So if a person at the end of a shift submits a urine sample for analysis and it turns out to be, let's say, 300 mL, you would then simply take the activity and then standardize it to 1,400 mL and say okay, that would be what you would see in a 24-hour sample. I just don't know if that was done. At this point I can only assume that that care was taken.

MR. BARTON: Couldn't they possibly have been overnight samples? I mean sometimes they, I could say, you know, give you a kit to take home.

DR. H. BEHLING: Yes. I mean that's a cumbersome approach to doing urinalysis is to ask people to walk around 24 hours for a given day to collect this urine. I've done it myself. It's not very nice if you're obviously doing anything other than

staying at home.

So I always question when we talk about an activity for sample as representing a 24-hour void. To me it's suspicious, because as I said it's not a very easy thing to do.

And I know that from experience in other areas we had serious problems, because I remember in some instances where people were trying to tell us that a 400 mL sample represented a 24-hour urine void sample, it's obviously not likely we were getting the truth.

MR. HINNEFELD: I could be facetious here and say that in Oak Ridge in 1951 there was nothing to do except go home.

CHAIR MUNN: That's probably not too facetious. It's very close to reality.

DR. H. BEHLING: Well, given the uncertainty, I guess we will just have to give the benefit of the doubt to the people who were doing this that a 24-hour urine sample is also

representative by dpm per sample, and just assume that they obviously took all those variables into consideration.

CHAIR MUNN: All right, does anyone else have any further comment? If not, Steve, may we close this item? If so, Steve, will you please indicate that the issue was discussed and it was agreed to close the item at this meeting.

Is that okay with the other Board Members? Paul?

MEMBER ZIEMER: Yes, that's okay for me.

MEMBER BEACH: Yes, that's okay with me.

CHAIR MUNN: All right. Steve, can you accommodate us?

Thank you very much, Steve. If there's no further comment we will thank you very much for clearing up OTIB-34. We're going

to take our lunch break now, and due of the fact that we're running a little behind time, is there any objection to resuming on the hour?

Do you need longer than 35 minutes for your lunch?

MEMBER ZIEMER: Well, I don't.

MEMBER BEACH: I don't either.

CHAIR MUNN: All right, then let's resume at the next hour, whatever that is wherever you are, and we will see you back at that time. Thanks so much and have a nice lunch.

(Whereupon, the above-entitled matter went off the record at 1:26 p.m. and resumed at 2:03 p.m.)

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

(2:03 p.m.)

CHAIR MUNN: Our next item is our 2:30 agenda item. Status on PER-0038 case audits. SC&A?

MS. K. BEHLING: This is Kathy Behling. That is work in progress. Unfortunately I did not get that completed. It's been a busy month here. Sorry about that.

CHAIR MUNN: That's all right. That will be a carryover for next time.

MS. K. BEHLING: That was the Hooker Site Profile revisions and there were three cases that were selected for review. As I said, work is in progress.

CHAIR MUNN: Very good. We'll carry it over until next time, and we'll move on to OTIB-54, the reactor modeling report. NIOSH?

MR. HINNEFELD: Jim, that's you, isn't it?

DR. NETON: What's that, Stu?

MR. HINNEFELD: The reactor. The report.

DR. NETON: I thought ORAU was

going to handle that with, who is it, Morris or --

MS. THOMAS: Bob Burns is on. This is Elyse.

MR. BURNS: All right. Well, for the reactor modeling report, internally that's our Report 67. That has been approved by DCAS and I believe issued as an official project document. So I'm not sure as to the status of it. I don't know if I have much more to add beyond that.

DR. OSTROW: Hi Bob. This is Steve Ostrow. I haven't seen the report so it's out of ORAU whenever, but I don't think it's hit the street yet.

MR. BURNS: Okay.

MS. MARION-MOSS: Steve, this is Lori. The report has just been approved by NIOSH and it's in the process of being published to the public.

DR. OSTROW: Okay, great, so we should see it soon. Okay, the way I understand it, the reactor modeling report covers the Findings 1 through 4, so that'll be good to see. I think NIOSH also had action items from the last meeting and was supposed to respond to Findings 5, 9 and 10 also.

CHAIR MUNN: Yes, that's correct. That's our next item. So do I understand correctly that next time we will have the reactor modeling report which as pointed out covers Findings 1 through 4?

MS. MARION-MOSS: Correct.

CHAIR MUNN: Very good. It's on the schedule for carry-on next time.

DR. OSTROW: Maybe I can say something, Wanda, also. After the last meeting we had, I think which was April 16th, we had a technical call, SC&A, ORAU and NIOSH, on May 13th on Findings 5 and 10.

And just a brief summary, that SC&A recommended that we close Finding 10. And Finding 10 had one part in it where it referred to Finding 5 about release fractions, but we wanted -- NIOSH and ORAU agreed that release fractions will be handled in Finding 5 and whatever's left for Finding 10 we agreed to close it.

CHAIR MUNN: That's good information, we appreciate it. I see nothing is on my screen being shared.

DR. OSTROW: Finding 5 is still open. That's release fraction. And Finding 9 is still open. It has to do with the workbook tool.

Scott Siebert had updated the BRS on August 22nd with a comment on that, a long comment, and I don't want to speak for NIOSH and ORAU. But it seemed to me that the basic thrust was that the workbook tool works now.

We didn't check it out yet, but we had a few comments on it last time. So we would like to take a little time and just check it out to see if it does work correctly now.

CHAIR MUNN: So SC&A is going to respond to Item 9 next time?

DR. OSTROW: Yes.

CHAIR MUNN: Is that correct?

DR. OSTROW: Yes, we'll go ahead and do it. I think Ron Buchanan had looked at the workbook tool last time and we had a few comments that the workbook tool wasn't totally updated from the last revision of the OTIB.

And Scott Siebert seemed to indicate that it has been updated correctly, but we'd like to take a look at it and report back to you guys just to make sure that it works now correctly.

CHAIR MUNN: All right. So my list tells me now that we're going to see the reactor

modeling report next time, and next time we're going to see Finding 5 and SC&A will report on the remainder of Finding 9. And right now we are going --

DR. OSTROW: We'd like to close 10. We recommend that we close 10.

CHAIR MUNN: We will do that right now. We just need to find it. And we're on our way.

DR. OSTROW: Here, Finding 10 referred to a large generic one. I think John Mauro had brought that up originally that the question that we sort of believe that the OTIB is conservative, worker-friendly and all that, claimant-friendly, but we weren't sure on whether it reflected reality or whether it was too claimant-friendly or whatever.

But after some discussions with NIOSH there's nothing very specific to point to so we'd like to drop Item 10, Finding 10.

CHAIR MUNN: All right. So we're correct, that Item, actually 36 --

(Simultaneous speaking)

CHAIR MUNN: Correct?

DR. OSTROW: Right.

CHAIR MUNN: And it's been discussed by the contractor and the agency as well as the Subcommittee.

DR. OSTROW: Yes, and that was on April 13th we had that technical call. May 13th, excuse me. May 13th.

CHAIR MUNN: All right. Okay, then I see the way we have this broken out, Steve, we have multiple sections. I don't know if we have one Finding 10. We have their response there which is --

DR. OSTROW: Yes. The one from May 14th which is right in the middle of the screen. Don't move it, Steve Marschke. Keep it.

CHAIR MUNN: Yes, it's right there.

Yes, it's there. And it's recommended the issue be closed. And Paul, Josie? Any negative response to that suggestion?

MEMBER BEACH: No, I don't have any negative responses, Wanda.

CHAIR MUNN: All right. Paul?

MEMBER ZIEMER: And I agree it should be closed.

CHAIR MUNN: All right, very good. Let's just indicate on this date that the Subcommittee agrees the item is closed.

Yes, I think that's all we need.

DR. OSTROW: Looks good to me.

CHAIR MUNN: Very good. That tops off our list, and I believe that the only thing we have of OTIB-54.

DR. OSTROW: That's correct.

CHAIR MUNN: Very good. Let's move on then to Kathy's PER reviews. Kathy, you want to start us off with PER-0042?

MS. K. BEHLING: Actually, Hans is going to do the PER reviews.

CHAIR MUNN: All right, very good.

DR. H. BEHLING: In fact I'm stuck with three PERs, so you can love me or hang up.

CHAIR MUNN: Hans, we love you. We wouldn't hang up for all the tea in China. Please carry on.

DR. H. BEHLING: Well, we'll talk about that after the last PER is discussed.

CHAIR MUNN: All right, thanks.

DR. H. BEHLING: Okay, we'll start out with PER-0042 which is the Linde Ceramics Plant TBD revision. And just as a brief update, the Linde Ceramic Plant was actively producing uranium oxide as a coloring agent before it was contracted to the AEC to produce or refine uranium materials and both from domestic ores and foreign ores.

And one of the things that sort of

complicated the whole description of what took place was the fact that there were numerous periods, and also in addition to the fact that it was an AWE, there was also a component of Linde Ceramics that was considered a DOE facility known as Tonawanda Laboratory.

Anyway let me just briefly discuss some of the issues of relevance here. The PER-0042, in essence, considered changes that were made between the current revision, which is Revision 3, and all previous revisions to the TBD. And in total there were a total of five revisions following the initial.

And as part of the original Rev 0 of the TBD which SC&A reviewed, we were also party to all the changes. And one of the things that has to be kept in mind here is that this PER somehow differs from previous PERs.

The successive revisions into Rev 1 were changes that either increase, decrease or

both increase and decrease estimates of dose. But the most important changes that occurred were the decrease in potential internal exposures due to insufficient monitoring data that ultimately led to three SEC Classes that span from the period of October 1, 1942 through December 31st, 1969. So you have a very lengthy residual time period during which was covered the SEC petitions of Classes. The changes that occurred throughout this time period to the TBD involved revisions and changes that were extensively discussed and resolved in the total of 20 different conference and teleconference meetings that occurred over a five-year period.

And these meetings were conducted by the Board's Linde Ceramics Work Group but was heavily patronized by the people from NIOSH and our contractor SC&A personnel.

And I want to just emphasize a

couple statements here. During the last teleconference that occurred in June 2012, the Linde Work Group, NIOSH and SC&A agreed that all TBD issues had been resolved.

And Revision 3 of the TBD, Linde Ceramic TBD, was issued six weeks later on July 26, 2012, and was followed up by the DCAS PER-0042 on November 16th, 2012.

So the prompting of the SEC petition was really based on the successive changes to the original TBD that occurred in 2005 and culminated in the revised Rev 3 that was issued in 2013.

With regard to the Subtask 1 that we normally address in our review of the PER, SC&A has no finding pertaining to the issue of how PER-0042 came to be.

With regard to Subtask 2, which we are required to assess NIOSH's specific method for corrective action, we have to look at this

particular PER slightly differently in light of the fact that we were party to all of the issues that led up to the revision of 0.3 of the TBD.

However, we did not necessarily review TBD, the revision of TBD as Rev 3 and that is really the source for this PER. And what we intend to do here in this PER, for those who have read the review of the PER-0042, is that we went through the Revision Number 3 as if it were a new TBD because all subsequent revised dose estimates were based on that.

So I will just briefly go through each of the time periods and issues that we addressed. The first time period was the internal exposures for the period of November 1, '47 through December 31st, '53, and that is the operating time period during which it was assumed that the bioassay data for all workers involved in, exposed to uranium, radium and radon exposures were insufficient to really do

dose reconstruction.

However, and this is important because it will come back later on in a different format, but the statements are in the TBD. In spite of the, and I'll read it. And I'm not sure, for those who may be following the review of PER-0042 online, I'm on Page 12 of the writeup, the statements are as follows.

In spite of the paucity of bioassay data and the establishment of three SEC Classes, the TBD acknowledged that uranium bioassays are available for a limited number of workers for the period of '47 through 1950, and if uranium bioassay are available for a worker they should be used to reconstruct an individual dose. And the same thing applies to radium and to radon exposures.

And the point that I want to make here is that this period is covered under the SEC, and yet because of the fact that there are

a limited amount of bioassay data involving uranium, radium and radon exposures, the TBD specifically states that a partial dose reconstruction may be available and may be done for workers for whom these data are available.

And this is not an issue here. I just wanted to make that statement because later on I'm going to come back to another area that involves the residual period that may be relevant to the issues that I've just raised here.

So in context with the Subtask 2, again SC&A found no inconsistencies and no areas for partial internal dose estimates for the time period '47 through '53.

For the external exposures to Linde Ceramic period for the duration of '42 to '53, film badges were used to monitor some workers for beta dose during select time periods when the African ore was produced. And

for, I guess as a summary, for the external dose reconstruction, NIOSH simplified and consolidated a large volume of information that was introduced in Table 4-1 through 4-23 into a single table that is 4-24, for all years going from '42 to '53 by regrouping various job workers into high, medium and low exposure groups. And as a convenience to the reader, I have included Table 4-24 as Table 5 in my summary for those who may want to take a look at that.

So in essence, for the external exposure data, the various data that were introduced as Table 4-1 through 4-23 were consolidated and simplified into a single table that was identified as 4-25, and in my writeup was reproduced as Table 5. So I may make reference to that in a few minutes.

Okay, I do have in addition to two findings there was a single observation, and

what I wanted to do is to bring that to the attention of NIOSH for simply correcting the text that appears in the TBD.

And that involves, and I'll go to, to simplify it, to Page 17 of my writeup where there is a discrepancy between information that was expressed in Table 4-6 which erroneously cites the value of 26 rem per year to the hands and forearms for the loader. That's a worker category.

In fact, the correct value, 74 rem per year, is given in the fourth bullet on Page 45, so it's strictly an observation and a correction.

A second correction under Observation 1 is that the third bullet on Page 45 of the TBD incorrectly cites 221 rems per year to the hands and forearms to the Step 2 process operator, and the correct value is 158 rem per year as shown in Table 4-6.

The benefit or the good part is that these values were actually not incorporated in the final table, which I assume is really the source for the dose reconstructor to extract year by year dose to the various groups of people who are identified as such. So this observation is strictly a correction of two errors that are identified in the text of the TBD.

So let me go on now. We just finished, as I said, there no findings for the operating period with the exception of the observations that I just cited and are described on Page 17 of my writeup.

The next time period is the exposure estimates from the residual contamination after 1953. And here's where I will come back to the issue of a partial dose reconstruction that was ultimately identified as the first finding that I'll come to in a moment.

But I do want to read a following item that I think is important to understand in behalf of that first finding. In Section 6 of the Linde Ceramics TBD it starts out with the following statement.

This section develops parameters for reconstruction of doses due to internal and external exposure at the ceramics plant starting January 1, 1954, which is the beginning of the residual period.

NIOSH has determined with concurrence from the secretary of DHHS that internal doses at the Linde Ceramic Plant cannot be reconstructed with sufficient accuracy from the beginning of 1954 to the end of 1969.

If monitoring data are available for workers who are included in the SEC dose, including the SEC Class dose is to be reconstructed as appropriate based on such

data. However, such dose reconstructions are still considered partial dose reconstruction because NIOSH has determined that internal exposures during the SEC Class period 1954 through '69 cannot be bounded.

And it's important to take that into consideration with what I previously stated that during the operation period, as I mentioned earlier, internal dose reconstruction for uranium and product as well as radon could be added even though those years of the operating period were also covered by the SEC.

And here again by and large they said beginning from 1954 to the end of 1969, if data are available a partial dose reconstruction may be added. So having said that I will go and discuss what was done in terms of estimates of internal exposures.

When I looked at the data, NIOSH elected

to avoid estimating internal exposures for the period, for the 16-year period of 1954 to 1969, and did something that sort of puzzled me.

In terms of uranium, NIOSH employed the following information assumptions for dividing inhalation intakes for the residual period that no longer starts in 1954, but actually was decided to represent only the years from 1970 to 2009, and this is what they did.

They by and large said that because of the SEC, I assume because of the SEC period that extends through 1969, the residual period between '54 and '69 is skipped.

And so what they did, they said you will not get any internal exposure but we're going to do the following. We're going to take a maximum air concentration of 161 dpm per cubic meter that was observed in 1950. This is four years before the beginning of the residual

period.

And then in 1976, a survey of Building 30 showed air concentrations of 0.0422 dpm per cubic meter. And so they used in 1950 data point that actually precedes the residual contamination period that starts in 1954 and transported the 1950 data points and assigned it to 1970. And that to me makes no sense.

And then by extrapolation used the data to cover the period from 1970 to the balance of the period of, that goes all the way to, I guess, 2009.

And I have no way of accepting or understanding this in light of the fact that why would you take a 1950 data point and without modifying that data point assign that same data point to 1970 and then use the extrapolation of that data point to a 1970 data point for all years and assume that this is how you're going to assign internal exposure?

And the same thing was done with radon, where again because there were no radon surveyed for the years '54 to '69, NIOSH employed the radon air concentration value of 10 picocuries per liter and which corresponds to a radon exposure rate of 0.48 working level year that had been assumed for Linde workers for the years '47 through '53, and assigned that same value again to 1970.

And then using a 1981 data point again to determine what the source term depletion rate would be for those two data points and assigned these data for the entire residual period. And so I find that very puzzling.

And so in light of the fact that there was an encouragement to use any form of available data for partial dose reconstruction as was done during the operational period, if there were bioassay data for uranium and radon

exposure values that were considered incomplete but for a partial was used.

And for the residual period, even though we have data that predate, actually, in both instances the uranium and the radon exposure residual period by several years and then transport those two data points for uranium and radon to a time period that follows the SEC period that terminated in the end of 1969 and start off with that.

And so to me that makes no sense at all and it violates by and large the recommendation that defines OTIB-70. So Finding Number 1 which appears on Page 18, I state that SC&A questions NIOSH's restrictive methodology to deriving internal exposures to ceramic plant workers from residual contamination.

The availability of data that satisfied criteria cited in OTIB-70 allow for

the assignment of internal exposures to uranium and to radon inclusive of years 1954 to 1970, in spite of the fact that this time period is part of the SEC period.

And so as far as I'm concerned, it doesn't seem to agree with the statements that appear in the TBD and it does not comply with the requirements of OTIB-70.

If NIOSH would like to comment, I guess it's a good time to try to get an understanding of why that was done.

CHAIR MUNN: Thank you, Hans. I don't know whether NIOSH is prepared to comment yet, not having had an opportunity to provide a response. But if you do have comments, let's hear them.

MR. HINNEFELD: Well, this is Stu. I would just offer that, you know, we got these reports, what, a week or so ago and we've not really distilled them very much.

I don't know, Jim, if you had any reaction that you wanted to get into on this one?

DR. NETON: No, like I say, I agree with you. We just got them a week or so ago, and it's somewhat complicated because this is one of the few if only sites that I can remember that we added an SEC during the residual period. So I'll have to go back and look at that pretty carefully.

DR. H. BEHLING: Yes, I was trying to scan OTIB-70 and try to understand whether or not OTIB-70 would be exempted from the recommendations for defining exposures due to six independent protocols that they offer you and say that this will not apply during an SEC period.

But I didn't see anything like that, and in light of the language that was used throughout the TBD that says a partial dose

reconstruction may be done if the available data exists that allows you to do so, and I consider the data points that were obtained for the residual period that predate the actual start of the residual period of 1954 certainly would satisfy the criteria of OTIB-70.

DR. NETON: Well, again, it's complicated because that blanket statement that you read we include in all SECs, we say, and really it applies. I don't know if it's an individual monitoring data or not, but that's the intent, bioassay data not air sampling data. And so that's one issue there.

But again it's complicated because you have an SEC period that covers, a residual contamination period that's in the middle between a covered period and an ending residual period.

So I'll have to go back and look. And I understand what you commented on about the

OTIB-70 and extrapolation through the SEC residual period. But again I'll have to go back and look at it a little more closely.

DR. H. BEHLING: Yes. If it turns out that the SEC period precludes the use of OTIB-70 that's okay. And if that's the case, then those two starting data points that predate the SEC period by several years for both the uranium and the radon issue should as a minimum then be reduced to, I mean we're talking about a 20-year time period. You can't transport uranium data that was taken in 1950 and then say, oh that same value now applies to 1970, and then decrease it in a rapid fashion that corresponds to a subsequent data point. That makes absolutely no sense.

DR. NETON: Yes, I hear what you're saying. We just need to look at it a little more closely.

CHAIR MUNN: All right, I'm

assuming that we do not have those findings as yet posted on the BRS, or am I incorrect in that?

MS. K. BEHLING: This is Kathy, and no, I did not get a chance to do that. I apologize.

CHAIR MUNN: All right.

MR. HINNEFELD: Wanda, that's related to what we did at the start of the meeting. We only assigned them, quote, in the system at the start of the meeting and so there was no way they could have entered them until --

CHAIR MUNN: That's why I was assuming that they weren't there, Stu, not being able to check it myself right now. But our first action item is to add those two findings to the BRS, and our next action then will be to anticipate a response from NIOSH for those two findings.

Any other comments with respect to

the Linde PER?

DR. BUCHANAN: Yes, this is Ron Buchanan at SC&A.

CHAIR MUNN: Yes, Ron.

DR. BUCHANAN: Hans worked on that part. I worked on part of it too, and so I worked on Subtask 3 and 4. And so do you want me to cover that section at this time?

CHAIR MUNN: I think that's appropriate, Ron, yes.

DR. H. BEHLING: Well, can I interrupt, Ron?

CHAIR MUNN: Oh, yes.

DR. H. BEHLING: I'm not finished quite yet. I was going to turn you over in about five minutes.

DR. BUCHANAN: Oh, okay.

CHAIR MUNN: My mistake, Hans. I'm sorry. I didn't mean --

DR. H. BEHLING: No, no, no.

CHAIR MUNN: That's fine.

(Simultaneous speaking)

DR. H. BEHLING: I should have stated that up front that Subtask 4 and 5 will be covered by Ron.

CHAIR MUNN: Very good, yes, and go on from there. Are we done with your coverage of your portion of the report? I don't want to shortchange you, but if you have more to say please continue.

DR. H. BEHLING: Yes. The second, I told you there was an observation that I identified in the first finding, and I'm about to briefly discuss Finding Number 2 without going through a lot of things. But the Finding Number 2 centers around the utility tunnel exposures involving internal exposure to uranium and to, and in progeny as well as to radon exposures.

And let me just briefly, and for

those who may be following me I'm on Page 19, and you can quickly, because when I talk about numbers it's very hard to somehow mentally get an understanding, and it would certainly help if you would actually look at the writings and the description of the issues that are being discussed.

So right now I'm on Page 19 and I'm discussing utility tunnel exposures. And for internal exposures to uranium and progeny, the assumption was that for modeling the annual exposure times were identified as 1,000 hours per year for trade workers and 100 hours per year for all others. And there were other assumptions which are not relevant to the discussion.

And those estimates were based on tunnels surveyed, beta measurements on surfaces during, in the tunnels. And for the radon exposures, NIOSH derived radon

concentrations and worker exposures for two independent source terms, and this is really the key here to Finding Number 2.

The two source terms were based on radium-226 surface contamination inside the tunnel. So you have activity, contamination activities that were on the inside of the tunnels that were used by trade workers for during work times as well as transit times for other people.

The second source term for radon was from radium-226 levels in the soils that surround the underground tunnels. So there are two independent source terms, interior contamination of the tunnels themselves, and contaminated soil that surround the tunnels that just like in a house that in itself is not contaminated but radon permeates from the surrounding soil of a basement, et cetera. So those are the issues.

And quite frankly I can make it quickly an issue here that the 100 hours per year for trade workers and 100 hour per year for all others is a reasonable assumption.

But here's what happened. In the case of the radon exposures from surface contamination, those were the actual exposure time periods used, 100 hours per year or 50 percent of a full work year for trade workers and 100 hours per year, which is five percent, for all others, respectively.

When it comes to radon exposures due to contaminated soils, I'll read to you what the issues were here. It was assumed, and I'm quoting directly here.

It was assumed that trade workers and laborers worked in these tunnels doing maintenance work for eight hours per workday, and in parentheses, two months of the year, and for the other ten months a transit time of ten

minutes per workday using the tunnels to get between buildings. For all other workers, only the transit time of ten minutes per workday would be applied for year-around.

And they ended up resulting into estimates of working level months per year for trade workers and for others. And if you do a simple calculation that corresponds to the eight hours per workday for two months and the ten minutes transit time for the balance of the ten months, you only end up with 375 hours per year for the trades worker and 41.7 hours for all others.

And of course you cannot separate these two source terms. I mean if you're in the tunnel for source term number one, you're going to be exposed not just only to the radon that comes from the contamination that's inside the tunnel, you're also going to be exposed to radon that permeates the tunnel from contaminated

soil. So you cannot separate these two source terms in terms of exposure time.

So in essence, my Finding Number 2 states the following, and I'm on Page 20. The assigned radon exposure rates in Table 6-11 and 6-12 are correctly based on the identical occupancy of 50 percent and five percent which translates to 100 hours per year, and 100 hours per year for trade workers and all others, respectively, and not by the stated occupancy factors described in the text.

So once again you have a situation here where the actual numbers are correct, but the supporting time frames that support those numbers are incorrect in the text.

Again I don't expect a response, but this is strictly a technical error that says the actual numbers that appear in Table 4-24 use the consistent time frames of 1,000 hours and 100 hours, respectively, but the text is incorrect

in stating, state times for the second source term that simply do not apply to those hours.

At this point, if there's no comment from NIOSH I will turn this over to Ron Buchanan for just a quick discussion of Subtask 3 and Subtask 4.

MR. HINNEFELD: No, we don't have any comment. We can go on to Ron.

DR. BUCHANAN: Okay, thank you. This is Ron Buchanan, SC&A. And of course Subtask 3 has to do with the approach that NIOSH include the correct number of claims to reevaluate.

While in this case is unusual, I stated earlier in that there was so much change in the TBD that they reevaluated all the claims that had qualified.

Now it's kind of hard to go back and determine how many claims are actually on the drawing board in this. I think it's July of

2012 when the TBD was issued in the latest revision.

And so what I did is I went back and seen if what they said made sense or if there's any contradiction to that and what's available on the database at this time, and I found no contradiction.

And that my best estimate was that there was about 250 claims had been submitted and DRs performed in July of 2012 and that 134 of these had PoCs better than 50 percent. And so we don't need to go back and revisit those. So that leaves 116 claims that needed to be evaluated.

Now 38 of these had SEC cancers only and so those would be paid and so you would not reevaluate those. Now some of them had SEC cancers plus non-SEC covered cancers, so we want to reevaluate those because they might be paid for medical benefits under the non-SEC

cancers.

So that left 78 claims, and I tried to kind of list of those there on Page 21 of our report in decreasing numbers. And so it left 78 original dose reconstructions with PoCs less than 50 percent that need reevaluated.

They did reevaluations on all 78 cases. Seventy four of those resulted in the PoC being less than 50 percent as the original DR indicated but with the new TBD recommendations, and four of them came out with PoCs greater than 50 percent.

And two of those four had SEC covered cancers and also non-SEC covered cancers, and so they would qualify for the medical benefits if necessary there. Two of them had no SEC cancers to them, and so one of them was greater than 50 percent so that would be available for compensation.

The other one was kind of an unusual

case in that originally the original DR was less than 50 percent, when they came back and did it with a revised TBD is was greater than 50 percent and DOL sent down a letter saying that the employment period was incorrect and that they decreased the employment period so the PoC went below 50 percent on that.

And so that was our analysis of Subtask 3. We felt that NIOSH did what they needed to do. They reevaluated all the claims that had been done before July of 2012 and went down the right process, so we had no findings in that section. And we agreed other than the findings Hans said in the TBD revision.

As far as selecting the cases, we felt that that was correct. Any questions on that?

CHAIR MUNN: None here.

DR. BUCHANAN: Okay. And so that brings us to Subtask 4 on Page 23. And so, in

this section, we recommend that SC&A audit two of the cases that were below 50 percent to see that the TBD Revision 3 was correctly applied.

Now, this will mostly consist of a complete audit of the cases because there was so many changes that it was completely reworked. And so we won't just do a focused audit like we do in some PERs where we just look at internal plutonium or something. In this case we'll have to look at the whole set and determine that it was done just like we do during the audit process for other cases.

And another option, we think we'd probably recommend two of those that the Board would work with NIOSH to assign to us to audit, and then perhaps audit that one case that was below 50 and then greater than 50 then less than 50, to make sure that the final DR on that was actually less than 50 percent according to the new TBD, the Rev 3.

And so that's our conclusion on this PER.

CHAIR MUNN: Thank you so much, Ron. Any comment from any source?

MR. HINNEFELD: This is Stu. I mean, there aren't different flavors there, right, like we'd need one from Column A and one from Column B in picking these two? It's just any two?

DR. BUCHANAN: Right. Since they'll undergo a complete audit, any two. And then, of course, the third one would be that particular case.

MR. HINNEFELD: Okay. There's a new app that shows cases that were reviewed under PERs. Do you guys just want to select them yourself? If you can look on our Staff Tools page.

DR. BUCHANAN: Yes.

MR. HINNEFELD: Let's see what I

can get here. If you look on our Staff Tools page, there should be something called -- let me look for it a minute. I saw it just the other day. Program Evaluation Reports. That makes sense.

DR. BUCHANAN: Yeah.

MR. HINNEFELD: One of the buttons, like Board Review System is a button.

DR. BUCHANAN: Okay.

CHAIR MUNN: Sounds like a logical thing.

DR. BUCHANAN: And it'll have the cases that were associated with that?

MR. HINNEFELD: Click on Program Evaluation Reports, you get a list that actually looks like the Board Review System, but it's a list of the PERs that have been done.

And if you find the one you're interested in and press the select button, it will display all the cases that were

reevaluated under that. And it includes the files of the reevaluation. There's a link to those files as well.

DR. BUCHANAN: Okay. With the Work Group's approval, I will address two cases, the 71 and the one case that had the different PoCs at different times.

MS. K. BEHLING: This is Kathy Behling. Should we proceed with that or do we wait until there's been some discussion on the two findings?

CHAIR MUNN: It appears to me that you're going to need to complete the reevaluation of the audits, and under any circumstance, how do the other Board Members perceive that? Any differently?

MEMBER BEACH: No, not here, Wanda.

MEMBER ZIEMER: That wouldn't affect what they do on this, would it?

CHAIR MUNN: I wouldn't think so.

You're still going to have two cases that require -- that should have audit. It seems to me that the two could occur in parallel.

DR. H. BEHLING: Yeah, there's a significant difference between what may have been done and the two findings really involve the residual periods that at this point eliminates any consideration for exposure for the years '54 through '69. And then that could impact at least a case that's very close to being compensated.

CHAIR MUNN: So you're suggesting that we wait until we've addressed the findings?

DR. H. BEHLING: Yeah. Because we're potentially biasing in our review of cases that do not consider potential exposures from residual contamination for years '54 through 1959, if it turns out that NIOSH will concede that issue in saying we could do a

possible dose reconstruction during that period of the SEC period for some residual contamination.

MR. KATZ: This is Ted. Without speaking to that, because I wouldn't make any assumption about that, but it's going to make no difference auditing whether or not how the other two findings come out.

It's going to make no difference, because the purpose of the audit isn't to determine if the case would change, it's just to determine whether the procedures were applied correctly. And you're not going to be auditing a procedure that's in contention for that. So I think you can do the audit now. It will make no difference.

MS. K. BEHLING: This is Kathy. And, yes, that's correct. And if they were to make some change to the residual period, there will be another PER issued.

MR. KATZ: Exactly.

MS. K. BEHLING: I would assume. So, yeah, okay. I just wanted to be sure that we could go on parallel paths here. That's fine.

CHAIR MUNN: We see no reason why not. So our actions will show two findings that need to be posted and will require a NIOSH response, and the audit of two cases will proceed.

DR. BUCHANAN: Just to clarify, do you want to do just two cases rather than the third one involving the one that went up and down on the PoC?

MR. KATZ: Ron, can you just explain for everybody, why does that make that -- why should that be audited?

DR. BUCHANAN: Well, it bothered me that it was less than 50 percent, greater than 50 percent and then went back to less than 50

percent. And I just wanted to point it out. I mean, if the Work Group doesn't want that checked out that's fine. It was kind of an anomaly compared to the rest of them.

CHAIR MUNN: Well, it was. But by the same token, since the Department of Labor has reduced the amount of allowable work time, then it seems to me it's a fairly clear-cut rationale.

DR. BUCHANAN: Okay.

CHAIR MUNN: I personally don't see any compelling reason to pursue that further. Paul?

MEMBER ZIEMER: No, I agree.

CHAIR MUNN: Josie?

MEMBER BEACH: Well, I guess I'm kind of at a loss on that. I think that there's no reason why we shouldn't do a third audit if SC&A thinks that it's logical to do that.

MR. KATZ: Well, I mean, it's more

like, just because -- it's an odd circumstance but it's an explained circumstance. And I don't think you do audits by, you know, by glancing at what you've run across catches your eye. I mean, I don't think that's really good procedure.

MEMBER BEACH: Okay. Well, that's probably true.

CHAIR MUNN: Fine. I think we can do without the audit, Ron.

DR. BUCHANAN: Okay, fine. Two will be fine.

CHAIR MUNN: The two will be fine from our perspective.

All right, any further comments with respect to Linde? If not, then now let's move on to ICD-9, PER-0043.

DR. H. BEHLING: PER-0043. Yes, again I was the person who reviewed this. Just for background information, this particular

PER addressed the OTIB-5, which is the bulletin for internal dosimetry organ and external dosimetry organ IREP model selection of ICD-9 codes.

And just as a review, the original version of OTIB-5 was issued back in 2003, and since that time there were a total of nine revisions. Of the nine revisions, however, only seven made significant changes to potential dose reconstruction.

And for those who are looking at their screen, I am on Page 7 as well as now I'm going to 8 and 9. And at the bottom of Page 7 and 8, I identify what each of those revisions really did in terms of affecting dose reconstruction, and comments involving those changes to these revisions are on the bottom of Page 8.

And one of the things that I looked at was how were these changes made? And for

your benefit again here, I quoted a section of the TBD that identified how these changes came about. And again, if I'm on Page 9 here, I quote Section 2 from the PER.

And it identifies that the Department of Labor is really the key agency or group of individuals that identify what assigned ICD-9 codes apply in the dose reconstruction process, with exception of those instances where you have a medical review, which then obviously involves the contractor to the NIOSH people.

And on that assumption, SC&A concludes the following -- and I'm on the very bottom of Page 9 where I say the SC&A concludes that revisions to OTIB-5 were exclusively introduced by parties that are generally not within the scope of SC&A's review.

So we assume that these changes and additions to ICD-9 codes reflect updates and

revisions to the International Classification of Diseases and ORAU's improved understanding of corresponding internal and external target organ. Therefore, SC&A accepts these changes introduced to ORAUT-OTIB-5 and there are no findings.

Under Subtask 2, again I looked at all the revisions, and again they all basically complied with what was stated in the revisions and how they were introduced in the final tables. And so again, in behalf of Subtask 2, SC&A found no discrepancies and no findings.

For Subtask 3, which evaluates the PER stated approach for identifying the number of DRs requiring reevaluation of dose, they were by and large -- I'm on Page 13. There are four particular criteria that have to be met and potentially are used to determine whether or not a claim will have to be reviewed.

And in the case of Subtask 3, again SC&A has no findings pertaining to the identification of claims that were impacted, and there were a total of 36 claims that are subject to dose reevaluation.

For Subtask 2, of the 34 DRs that are subject to audit, SC&A recommends selection of one claim from each of the following revisions of ICD-9 codes. And there I identified those revisions, and they are Revisions Number 2 in ICD-9 code number 50; Revision 3, ICD-9 code 155.1; and from Revision 4, ICD-9 code 232, as well as 238.

So in this case, as I said, if we can select one of each of those revisions I think that would satisfy our need to evaluate PER-0043.

CHAIR MUNN: Any thoughts or comments? In this case, as the preceding one, there is no -- unlike the preceding one, we do

not have specific findings. I see nothing for NIOSH to respond to, except for the selection of claims for the audit. Is that the same view of the other Board Members?

MEMBER BEACH: Yes, Wanda. It is for me.

MEMBER ZIEMER: And for me.

CHAIR MUNN: Very good. Then all that remains for us is to identify how the choices will be made for the audit cases.

MR. MARSCHKE: Wanda?

CHAIR MUNN: Yes.

MR. MARSCHKE: This is Steve. Do you want us to enter a finding of no findings in the BRS for this one?

CHAIR MUNN: I believe that's appropriate and will keep us from being puzzled two years from now.

MR. MARSCHKE: Okay.

CHAIR MUNN: Thanks, Steve. I

will make a notation that a choice needs to be made with respect to auditing cases.

And is there any concern with respect to the recommendation as far as numbers are concerned? I find that to be, personally, quite acceptable. If anyone has any concerns, please express them now, otherwise I'll take that as assent.

Hearing none, we can proceed with selection of claims for audit. I'll ask that --

MR. HINNEFELD: This is Stu. In this case, since there are categories based on ICD-9 codes, we can probably query for these ICD-9 codes and provide the claim numbers that fall into these ICD-9 codes to SC&A, and so that they can then select from those numbers. Because if they were to go to the PER application, and in order to find the ICD-9 code they'd have to open each case to see what the

ICD-9 code was for that. So --

CHAIR MUNN: If you'll be good enough to do that, that would be helpful, Stu.

MR. HINNEFELD: And probably run a query and then just generate the list of here are the claims that have ICD-9 codes, the 150 and so forth. So we can provide that. And since it's an ICD-9 code selection that should be pretty straightforward.

CHAIR MUNN: All right.

MR. HINNEFELD: And then once we have that available we'll just send it out to SC&A and to the Work Group.

CHAIR MUNN: Thank you.

MR. HINNEFELD: And then I would assume SC&A could select the cases from those. Is that correct?

CHAIR MUNN: I'm assuming that that will be the case. Hans? Ron? Is that all right with you?

MS. K. BEHLING: This is Kathy. I'll probably be doing that, so, yes, it's fine. I can do that if you're in agreement.

CHAIR MUNN: Very good. No problem here. Any problem from anyone, speak now.

Thank you, Stu. I'll make a notation to verify if we haven't had a status. Thanks much.

And we can move on to Aliquippa Forge, PER-0045.

DR. H. BEHLING: Okay, that's me again. You're going to get tired of hearing me talk.

CHAIR MUNN: No, that's quite all right. We'll just forge on.

DR. H. BEHLING: Well, unfortunately the worst one's for last, and this one is going to be very difficult to follow, really. And I'm hoping that we can

minimize the discussion to only do most critical issues here that involves the individual findings, because they're quite difficult at times to understand without having first read the document in its entirety and understand the various issues. Because they talk about different values, different times and numerical quantities that are difficult to assess without having a full understanding of what's involved.

CHAIR MUNN: There's a lot of material here.

DR. H. BEHLING: Yes. The PER-0045 involves the Aliquippa Forge TBD revision. And the original TBD was issued back in 2004 and then was revised in 2012. And this TBD addresses the changes that occurred in the TBD revisions between those years.

For the sake of getting a few pieces of information, I provided my write-up on Page

8, specific issues and a timeline for those issues. And I want to briefly -- I won't go through all of these, but I want to point out the items identified as Number 2, 3, 13 and 14.

Number 2 identifies the time frame during which Aliquippa Forge was a production facility for the AEC, under contract, and that period extends from August 16th, '48 through February 28, 1950. The issues there was they were rolling operations of uranium.

In Item Number 3, it identifies the actual period of residual contamination, which starts March 1, 1950 through December 31st, 1987, and again from January 1, 1989 through December 31st, 1992. So those time frames are very important in understanding the issues that I will be addressing with reference to particular findings.

Item Number 13, and again these are integrated now. In August 1983, the Aliquippa

Forge site was designated for remedial action under FUSRAP. In December '87, storage activities began in Building 3 and then remedial actions were taken from October to December 1988 to enable additional restricted use for Building 3. But those dates are very important, and again I don't expect people to remember them but it's something that at least NIOSH has to look at in more careful terms.

And finally, Item 14 identifies the final remedial activities that occurred from June '93 to September 1994.

So also the items that I did not discuss I briefly discussed under SC&A's comments that by and large states that the most basic health physics practices in facility engineering designs and controls were lacking during the operational period.

And correspondingly the air concentrations during rolling operations were

orders of magnitude above the AEC-recommended preferred level of 50 micrograms per cubic meter, or the more conventional metric of 70 dpm per cubic meter.

And the last bullet -- I'll skip the third bullet. The last bullet was under FUSRAP, these are the dates that need to be recalled. A radiological survey of the Aliquippa site was conducted in 1978, and there was an interim remedial action undertaken to decontaminate the facility in 1988.

And the final site remediation occurred between June of '93 and September '94, because those are dates that will come into play in dealing with the findings.

Then we go to the next page, and under Subtask 1, identify the circumstance that necessitated the need for DCAS PER-0045. And obviously it was these changes that occurred between Revision 1 and Revision 2.

What's important to note, however, is that neither Revision 0 and Revision 1 of the Aliquippa Forge TBD had ever been previously reviewed by SC&A. So once again we're kind of starting out after the fact, or we're putting the cart before the horse in terms of this PER since we did not really review the TBDs.

The major changes we have to address under PER-0045 as a supporting document really involves the ORAUT-OTIB-70. And I will read from DCAS PER-0045. It states the following: Revision 1 of the Aliquippa Forge Technical Basis Document revised the dose estimates in a residual period starting 3/1/1950. This revision included both internal and external dose and was the result of both new data, and this I underline, a revision of OTIB-70.

Now, when I read that, it doesn't really matter and I only identified it as observation. The fact that OTIB-70 postdates

the original version of the TBD by several years, it is not something that is technically correct to assign that it was a revision of ORAUT-OTIB-70 that prompted the issue of a PER, it was the fact that when I looked at the data it shows that the original OTIB for Alquippa had made no reference to any OTIB because it didn't exist. And it was based on assumptions and methodologies which had very little in common with ORAUT-OTIB-70.

So it was not the revision that were introduced in Rev 1 of OTIB-70, but the very existence and substitution of guidance contained in Rev 1 of OTIB-70 for earlier estimates that identified, that prompted or that introduced these changes.

So, in essence, Observation 1 is that NIOSH should rephrase the role of OTIB-70 in Section 2.0 of DCAS 45. Also, Observation 2, review of records indicate that neither Rev

0 nor Rev 1 of the Aliquippa Forge TBD was ever reviewed or audited by SC&A.

Going to Subtask 2, Subtask 2 requests that SC&A assess NIOSH's specific methods for corrective action. I want to briefly go and review the basic issues that are defined in ORAUT-OTIB-70.

And one of the things that in our review showed that we had several criticisms that involved the depletion of rates that was originally identified as one percent a day. And two, the resuspension of residual contamination of 1E minus 6 per meter.

In our review, we were able to get NIOSH to rescind its one percent per day depletion rate into a much lower value. But with regards to the residual contamination of 1E minus 6 per meter, that remained unchanged with the exception of the fact that a footnote was added. And I'm going to ask you to turn to

Page 14 of the write-up, where the footnote appears that was introduced, and perhaps in light of our concerns that maybe 1E minus 6 may not always be the appropriate resuspension value that should be used.

And I read to you in the footnote, it says, in cases where the contaminated area is still involved in active operation, a site-by-site analysis of the appropriateness of the 1 times minus 6 per meter suspension should be done. And that issue also appears in Page 23 of this report later on.

The table itself identifies clearly a total of six potential options where a dose may be reconstructed based on the availability of air sampling data and surface contamination in combinations, and you see in the Table 1 that I provided, those different combinations and how they may be used.

So then we go to Page 16, which

involves estimates of residual external dose. I briefly explain how doses, external exposures would be constructed by NIOSH using residual radioactivity. And I quote from the document that says to reconstruct external exposure to residual radioactivity, the maximum reported exposure rate of 0.01 milli-R per hour was back-extrapolated using the source term depletion rate calculated from internal data.

And I put that in brackets, which were defined by 1.15 into the minus 4 per day or 0.042 per year, and assuming that workers were exposed to 2,000 hours per year. And in the load that I briefly identify with those numbers -- by the way there's a typo that needs to be corrected, but the actual value is that 0.082 rem times 5.6 gives you a dose of 0.15 rem. That is the starting point.

Finding 1 is the failure to account for previous D&D effort. What this really

involves is that they took the year 1992 as a point to extrapolate between, and as I mentioned earlier, and this is why I spend a fair amount of time identifying that on Page 9 of the report, I identified the fact that in 1988 there was a remediation or decontamination effort that was substantially going to reduce the exposure. So that when you extrapolate from 1992 and ignore the remediation effort that took place in 1999, you're going to grossly underestimate dose exposures that occurred between 1988 and 1950.

So the Finding Number 1 is, in essence, an issue where we underestimate the exposures by ignoring the decontamination effort that took place several years before the final remediation in 1992.

Finding Number 2 addresses the issue of the 1.15 times 10 to the minus 4 per day. It's also a problem because it was based

on removable contamination. And when you talk about a dose rate, the dose rate does not differentiate between removable contamination and fixed contamination.

So when you by and large take removable contamination to establish a decrease in activity, you're making a mistake, because it should not be based on the removable contamination but on the collective contaminations since dose rates do not distinguish between removable and fixed.

And so the backward extrapolation by means of the NIOSH-devised source term is incorrect because it is based on a dose rate that does not necessarily reflect the removable contamination by itself. And I elaborate on that issue at the bottom of Page 17 and 18. So that's Finding Number 2.

Let's see here. It's very difficult to talk about things that we may not be in a

position to go through, but I did want to make an issue here about the estimates of residual internal exposures.

There's numerous findings associated with it and they come in stages. The first attempt was to, in essence, identify what NIOSH did and see if we can duplicate those numbers. And then the second level of review is to say, what's the protocol that NIOSH used, was it correct? So on Page 18, I start to at least identify the methodology that NIOSH used in assessing the estimates of residual internal exposure.

And so on Page 18 I have a verbatim transcript of the information that was used. And what they did was to actually identify activity levels, contamination levels, in the furnace area which was identified at 5.9 microgram per cubic meter, which, based on the specific activity of 1.516 dpm per microgram,

translates to 8.94 dpm per cubic meter. And so that becomes your data point for 1950.

And, let's see, they then used that data point and said let this activity level, this is an air sampling data point, the 5.9 micrograms per cubic meter or the 8.94 dpm per cubic meter, is an air sampling point, and that needs to be understood.

And so in the next paragraph, I underlined it, that to calculate -- and this comes from the document itself -- to calculate internal exposure from residual activity, the analysis assumed that all buildings had an air concentration of 8.9 dpm per cubic meter in 1950.

And it says this operational air concentration was assumed to have occurred for one year with no cleanup, and an indoor deposition velocity of 0.00, the standard deposition velocity was applied to calculate

2.11 times 10 to the 5 dpm per square meter surface contamination level at the end of operations or start of the residual period.

And then they applied the standard resuspension factor that's identified in OTIB-70 of 1 times 10 to the minus 6 per meter, and applied that to the surface contamination level that would result from the deposition for one year as formerly mentioned, and then they came up with an air concentration that would result from that resuspension.

And then they also used the 1992 calculated air concentration of 0.35 dpm was based on applying the resuspension factor of 1 minus 6 per meter and so forth and so forth.

And we looked at this, and here on Table 3, which I included, on the basis of those numbers they came up with inhalation of picocuries per day for all the years between 1950 and 1992.

When I accepted this without reservation, the air data, I came up with the fact that, for 1950 and 1992, for those two years that I selected from Table 3, I came up with slightly different numbers.

And for the 1950 data, I'm on Page 20, the cited value of 0.627 picocuries per day matches NIOSH's numbers, so this number was okay. For the 1992 number, I tried to match that and I ended up with a value that was slightly lower and does not match NIOSH's value of 0.112 picocuries per day.

So in trying to simply determine whether or not the NIOSH's approach I could match the numbers, I was able to match one but not the other. And the same thing, because the daily ingestion rates were based on air concentration values, I was not able to match those numbers either, and I'm on Page 21.

So Finding Number 3 says that I was

not able to match NIOSH's number exactly, without considering whether or not their protocol was correct. So that's Finding Number 3.

But a much more critical assessment I am going to discuss involves Section 4.2.3 on Page 21 of my write-up. And that involves two elements. The first element I will discuss states the following.

In Section 5 of the TBD it states the following: After the end of AEC rolling operations, a July 1949 survey was performed and the survey indicated that the maximum air dust concentration taken during normal operation in the furnace areas was 5.9 micrograms per cubic meter, which translates to the 8.994 dpm per cubic meter that we identified above.

They also state, as I had already mentioned before, that all buildings were

assumed to have an air concentration of 8.94 dpm in 1950, and therefore they came up with the estimates that are defined in the previous table that I mentioned.

When I looked at the actual data from which this came -- and that's included in Attachment 1 of this report, which is on Page 30 and 31, if you can go there, Page 30 and 31 has Attachment 1. What you'll see on Page 31 is the furnace area value of 5.9 -- I think I blocked it out and hopefully that's easily identifiable, middle page -- the furnace area had 5.9 micrograms per cubic meter, and so it corresponds to the number that was cited.

However, right below, it says during floor sweeping of the mill area the samples showed 110 micrograms per cubic meter, this being the only sample in excess of the preferred level.

And so the first question that comes

to mind is why wasn't that sample used, because it is part of the area that was frequented. It was obtained by a floor sweeping activity, which is not outrageous, which is something you would expect in an operational setting.

And so the original value that was identified as the maximum, 5.9, is not correct, but 110 micrograms could have been used as a bounding value for air concentrations in 1950. So it turns out to be -- I guess it's Finding Number 5.

But the second, more important issue, and probably the single most important issue that I want to address here, is Finding Number 6 and how all this comes to light when you use the suspension factor as a way -- and what NIOSH did, they took an empirical air sample of 5.9 micrograms per cubic meter, which converts to 8.93 picocuries per meter, and then goes and reconfigures that internal air

concentration.

And this is -- I'm sorry, I should have said 8.94 dpm per cubic meters. That is an actual empirical air sample. And what they did, in essence, was to say, no, we're not going to accept an empirical air sample. And we will rather, then, convert it into another air sample by assuming that that air concentration will deposit on the floor for one whole year, and then use a resuspension factor of E minus 6 per meter and then come up with a revised, modeled air concentration that is 42-fold lower.

And this leaves me baffled in terms of how do you justify taking an air sample and then using a modeling approach that involves an assumed deposition velocity and assumed resuspension factor and then establish a new air concentration that is 42-fold lower than the empirical air concentration that you

started out with?

That, I think, is really the most serious issue that I have come across. And actually, on Pages 23 and 24, I actually identify the flaw in the use of the E minus 6 resuspension factor. If you were to use the higher concentration that was identified in floor sweeping in the furnace area of 110 micrograms per cubic meter, you would end up with a resuspension factor that's close to E minus 4.

And this is what we always talked about. When you have a facility where there are still residual activity, the resuspension factor of E minus 6 is probably a factor of up to two-fold too low and would support a factor of E minus 6, which I then actually came up with, and I describe that briefly on the top of Page 24.

With regard to, let's see, the next

issue comes on in the middle of Page 24, comments pertaining to NIOSH's assessment of the source term depletion rate. Here they used, let's see, 1992 survey data that showed an air concentration of 0.035 dpm per cubic meter. And they also had the 350 dpm alpha per 100 centimeters square that -- let's see, I'm trying to recall exactly what I've done here.

Yeah, this issue once again goes to the issue of the removable contamination levels. And in Finding Number 7, NIOSH's choice of the 1992 survey measurement of 350 dpm per 100 centimeters square removable alpha contamination is compromised by the fact that it postdates the interim decontamination efforts of 1988 here.

And so one should not take a point in time that does not consider a previous decontamination level. Remember that in 1988

the decontamination was fairly significant. Not as stringent as it was in the subsequent time period between 1992 and '93, but it clearly was something that has to be taken into consideration. And you cannot take a point in time that does not address the interim decontamination that took place in 1988.

Okay. And I guess the final finding raises the issue, why wasn't OTIB used in the way I would have expected it to be used? And that is, if you have, assuming for a moment that we're not even going to address the highest contamination level that was considerably higher than the 5.9 micrograms per cubic meter, but instead use 110, assuming for a moment that NIOSH were to continue to insist that that is the more appropriate value, why couldn't you have used that, the measured air concentration of 8.94 dpm per cubic meter, at the end of the rolling period and the beginning of the

residual period, and then use the source term depletion factor, as defined in OTIB-70, of 0.00067 per day and then calculate what the air concentration would have been for all years subsequent to the beginning of the residual period?

That, to me, again, is not consistent with the recommendations in OTIB-70. And that particular recommendation is the one that I would have preferred, but alternatively, among the six options that OTIB-70 permits for, you could have chosen the second and third tier option. In other words, that would have also provided a suitable means by accommodating the residual period.

So that is pretty much the end of the findings. And then considering the significance of the findings, especially the way the air concentrations were modeled, like I said, where you take an empirical air

concentration and then reconfigure it by using an assumption about deposition for a year, resuspension, and then coming up with a starting air concentration that is 42 fold lower than the empirical air concentration data that you start out with makes no sense.

And given the significance of that error, I made a recommendation that it would be premature to assess any potential reworked DRs until this issue's addressed.

CHAIR MUNN: Thank you, Hans. And given the scope and depth of the findings that you've presented, I have a tendency to agree with you with respect to the audits.

Does anyone have any comments one way or the other with respect to the recommendation relative to postponing the audits? Paul?

MEMBER ZIEMER: You're asking about postponing the audits?

CHAIR MUNN: Yes. The recommendation from the contractor is that the audits be postponed until after NIOSH has had an opportunity to review and respond to the findings.

MEMBER ZIEMER: Well, I don't object to that. Is this a case where it's going to make a big difference?

DR. H. BEHLING: The difference is, like I said, when you start out, even if you ignore the highest air concentration sample that defines the residual period that was 20 times higher than the --

MEMBER ZIEMER: Yeah, yeah. We better have -- yeah, I understand what you're saying. We better have NIOSH respond first, probably, yeah.

DR. H. BEHLING: Yeah. But even if you ignored that difference of the 20-fold difference between the sample that they

selected as the highest but accept the fact that the 5.9 microgram per cubic meter is in fact the more credible value, what they did was they converted that value, which is an air sample value, and converted by modeling it with assumptions about a deposition velocity and resuspension that results in a starting air sample concentration that is 42-fold lower. I mean, how do you --

MEMBER ZIEMER: Well, like I said, I'd like to hear whether NIOSH has any immediate response. Is there something that was overlooked or is it something that's been misunderstood or any immediate reaction to the review?

MR. HINNEFELD: This is Stu. I don't have any. I don't know if Jim has looked at it more than I have or not.

DR. NETON: I think I can make a general observation. The conversion that

we've done here has been applied at a number of sites. This is not unique to Aliquippa Forge. That is, take the end of operational period air sample data and convert it and use it to estimate the surface contamination. Because, in reality, what you're trying to do is get an estimate of the amount of air concentration due to resuspension of surface material, not to use the air data that was conducted during the operational period, which is a combination of resuspension and some source term activity that's been ongoing.

So it makes no sense to use the operational air sample data to start with. And we've done this many times. You take the operational air sample data that is a source term generator and deposit it on the ground using those default parameters that Hans mentioned to come up with the surface contamination.

MEMBER ZIEMER: Right. That was my impression, that that was not different from --

DR. NETON: It's been the practice for a number of years. There's nothing new here. Because you, really, if you start with the operational data, of course you're way overestimating what the resuspension is in the residual period because you have no -- you don't need to account for the source term that's generated in the air during that period.

As far as the floor sweeping sample goes, that's not a source term generator. That's just a resuspension generator. And I'll reverse judgment on the use of 10 to the minus 5th, 10 to the minus 6th. I think, you know, we need to talk about that later.

But as far as using a sweeping activity that is not really a source term generator to estimate the deposition back onto

the ground makes no sense at all. It's just technically not correct.

DR. H. BEHLING: But let me --

DR. NETON: -- some kind of an air sample that is representative of what was generated during the operations of the plant.

DR. H. BEHLING: Well, the value of 5.9 was documented July 28th, 1949. That's when rolling operations ceased. And so I would consider that almost the best estimate for the beginning of the residual period. And to take an air sample that truly defines the beginning of the residual period and then convert it and come up with a 42-fold reduction --

DR. NETON: Well, that's where we need to go back and look and see, is that really representative of operations or is that representative of, what I would call, the end of operations, where there was nothing ongoing? I don't know. We'd have to take a look at that.

But if it really was with operations ongoing, then it's totally valid, I think, what was done. If it's true what you just said, that there was no activity going on in the plant at all, then I would tend to agree with you. So I'm needing to look at it.

MR. BARTON: This is Bob Barton. Just to make a comment here. I hear what you're saying, Jim, about the application of a deposition velocity at other sites, but that's not always used.

I mean, we just wrapped up the Simonds discussions and they actually used operational general air sampling as the starting source term for the residual period. So it's not always the case.

DR. NETON: I understand that, Bob, but there's usually a reason behind that. There's the quality of the data, or we don't have -- it's determined to be general area data

to begin with, which would be indicative of resuspension, versus a sample that was taken in the middle of a process. So you've got to be careful what you compare.

MR. BARTON: I understand. And in this case it was general air during an actual rolling operation.

DR. NETON: Exactly. And general area air samples have been considered to be more representative of resuspension than a sample that was taken in a process, while a process was ongoing, you know, right at the process.

So we need to look at it a little closer, but there are reasons why one is used versus the other, and we've been behaving fairly consistently for a number of years in this area.

The only thing that really concerns me now is if this 5.4 sample was truly -- was operations-driven or whether it actually more

representative of a general area sample where nothing was going on. We'll take a look at it.

CHAIR MUNN: All right, very good. I have as our action items for next time to add eight findings to the BRS, and anticipated responses from NIOSH at our next meeting. Any other comments?

DR. H. BEHLING: I just want to make a comment. I understand what Jim Neton was saying regarding the modeling of an air concentration at some point in time, let it settle for a year and using resuspension, because that basically complies with one of the six methodologies that says you can start out with surface contamination and end up with an air concentration.

But in the case where you start out with an air concentration, I think you should stay with an air concentration either by using, under Table 1 that I identified on Page 14,

those are the various options. You can use an air sample during operational period and an air sampling during post operational period, or you can use an operational air sample and then apply the depletion factor of 0.00067.

And I can guarantee you, if you did that, even if you assume that the 5.9 microgram per cubic meter was an operational air sample, you would end up with a higher dose than you're getting with the model that was used here. And I would assume that the various options that are recommended in OTIB-70, and as I said, those are the options defined on Page 14 of my write-up, they're taken directly from OTIB-70, you have to use various options.

And I would imagine that they are given in order of priority. Highest priority meaning that you have air sampling data, which you do have. The lower tier options for reconstruction exposures would involve the

surface contamination during operational and post-operational period.

So there were multiple options here available for use and I'm not sure I go along with the methodology that was used by NIOSH.

DR. NETON: Just for your information, those are not listed in order of priority. That the operational air samplers are the best way to reconstruct inhalation in the residual period, that just doesn't make any sense. It's an option, but to take an operational air sample that is in the middle of a process of generating airborne activity and say that's representative of what's being resuspended from the ground during the residual period, it makes no sense.

I mean, if that's all you have, that's all you have. But if you've got something better, I think that you should use it. In fact, surface contamination

measurements in the post-operational period, which is the last choice, is probably the best thing to use.

DR. H. BEHLING: Well, as you read in my footnote on Page 14, I did acknowledge the following: SC&A notes that Method 1 was identified as the method of choice for bounding internal exposures from residual contamination in behalf of the Dow Chemical Company/Madison Site, and this is identified in NIOSH 2008.

DR. NETON: What, using operational air data was the method of choice?

DR. H. BEHLING: Yes.

DR. NETON: I'd have to look at that. I'll take a look at it, but that's certainly not what I would consider to be true.

CHAIR MUNN: We'll address it after NIOSH has had an opportunity to review in-depth.

DR. NETON: And it may be the method

of choice for that particular site given the data we had available. That may be true. But certainly if we had surface contamination data available that was taken during the residual period we would have used it. So I think you're misinterpreting what was said there.

CHAIR MUNN: We will address PER-0045 at our next meeting, and thank you all for your comments.

Our next item on the agenda is the prioritized list of PER recommendations. And thank you, John Stiver, for providing that to us. Would you like to address that, please?

MS. K. BEHLING: This is Kathy Behling. John, if you'd like, I can take that.

CHAIR MUNN: All right. Thank you.

MS. K. BEHLING: Okay. Yes, we sent out a memo on the 25th of August, and just I updated the three tables that are provided. In Table 1, I just moved these three PERs that

we've discussed today into that table, and I've highlighted in blue those things that have changed since our last meeting.

And I guess what you're really most interested in is Table 2, which is actually a list of four new PERs that have been issued by NIOSH. And I do give you a summary description of those four PERs.

And I will point out that three of the four, the first thing you're going to read is that no TBD actually existed for these, and however there was a template that was used and that template has changed and that's what prompted the PER.

And I think maybe at first glance you might say, well, why do we need to look at these? But my feeling is that the fact that a template did exist, it hasn't been really a formalized document that SC&A has looked at, and we go back to this consistency issue. And

so in all of the cases of four new PERs, I did suggest that we may, as a minimum, want to do a pre-review for those new PERs.

And in some of the cases, there are 50-some, almost 60 cases that were reevaluated. So that was my recommendation. In Table 2 there are four new PERs and that you may want to have us at least do a pre-review.

And then, finally, Table 3 is -- I probably won't get too formal, we won't even need this. But I just indicated on there, there are a few of the very earlier PERs that were done that we never went back and reevaluated any claims. I don't know if that's something that's even necessary at this point. And I indicated on there that you have assigned us claims for the Hooker and that work is underway.

CHAIR MUNN: Thank you, Kathy.

MS. K. BEHLING: You're welcome.

CHAIR MUNN: Does any Member of the Subcommittee have any questions for Kathy with respect to the list that you have in hand?

MEMBER ZIEMER: Well, we appreciate her analysis of these for us. Thank you, Kathy.

MS. K. BEHLING: You're welcome.

CHAIR MUNN: So the question now, what is your desire with respect to the four that have not been assigned and have been recommended? Does a pre-review seem to meet your personal feelings with regard to proper --

MEMBER ZIEMER: Well, how much difference is there in the workload, Kathy, for a pre-review versus a review?

MS. K. BEHLING: Well, actually, I was going to say I think, to some extent, we're adding a step that probably can be avoided. Because I do think that in each case, the only one that I actually would say is perhaps the

Bliss & Laughlin, just because when I look through these, TBD-6000 has been reviewed quite extensively by us. We've reviewed OTIB-4, and I know with OTIB-4 there were a lot of very conservative assumptions there. I have a hard time imagining that there would be a lot of increase in dose associated with that particular one.

But I would really almost recommend, rather than just a pre-review on at least 47, 52 and 54 to just go ahead and do a full review rather than adding the additional step.

As I said, these were done by templates which were never formalized or reviewed by SC&A, and I just think it might be a good idea to do a full review of those three as a minimum.

And I can dig a little bit closer and do a pre-review of the 50, PER-0050, because

that may be a little bit more extensive. But that would be my recommendation.

MR. KATZ: So, Kathy, let me ask you a question about the carborundum, because I had asked Stu just to give me his thoughts about how significant the changes are here in terms of how complex they are and how much likelihood there is of there being much here at the end of the day. Because we've done a lot of PER reviews at this point, and quite a number of them, really. I mean, they've been good in sort of finding concurrence that everything was done right, but in fact we didn't learn much was out of whack for the vast majority of PER reviews that you've done so far.

And carborundum, I mean, as I understand, I think, from Stu, I mean, the main thing here is the revision of depletion values in OTIB-70, which, you know, SC&A was heavily involved in, so there's not much new here.

CHAIR MUNN: No.

MR. KATZ: So I was wondering whether there's, for example, really any value in going further on that one.

MS. K. BEHLING: Yeah, you're right. As I look at this, as I said, with the Bliss & Laughlin that's true. This was more based on the OTIB-70. I was just looking at the number of claims that were affected by this. But you're probably correct. I guess probably a full review of 47 and 52 would be probably adequate.

MR. STIVER: This is John Stiver, if I can just jump in for a minute. I'd just kind of like to remind everybody that the reason we started doing these pre-reviews was to avoid kind of having to, you know, guess at the level of value in realtime during the meetings.

But you do a quick scoping review, I know Hans did the last -- correct me if I'm

wrong, but I don't think he spent more than about a day or two doing that. So, you know, let you take a quick look and you can just say right away that, you know, this is something, this is a no-never mind. The others might take a little bit more digging. So I wouldn't necessarily recommend abandoning the pre-review stuff with some of these things.

MR. KATZ: Yeah, except that, I mean, every PER doesn't need to be reviewed at all. I mean, there's still -- there doesn't have to be a sense that all PERs get reviewed. In fact, I mean, that doesn't make much sense given the experience so far to be reviewing every PER.

So, anyway, that's why I just raised the question about, similar to what Kathy said about 50, about 54. It's not looking like there's anything there of value.

CHAIR MUNN: Thank you, Ted, and

thank you, John. Any other thoughts? Any recommendations with respect to the four PERs we're looking at?

MEMBER BEACH: I think I agree with all the discussions so far. I think 47 and 52 look like likely candidates, and 50 and 54 have been reviewed based on OTIB-6000 and 70, so I'm good with that.

CHAIR MUNN: I have a tendency to agree. Paul, how do you feel about it?

MEMBER ZIEMER: Yeah, I think it makes sense. Just certainly backing just the two of them.

CHAIR MUNN: Let's move forward with PER 47 and 52.

MEMBER ZIEMER: Yeah, yeah.

CHAIR MUNN: And recommend those to SC&A as appropriate subjects for --

MEMBER ZIEMER: Are we talking about pre-reviews or reviews?

CHAIR MUNN: We're talking about reviews, I believe.

MR. KATZ: Yeah, what I would suggest is just assume it's a full review. If SC&A, John, if you guys get into it in one of those and there's not much there, then, you know, you can back off of it. But if there seems to be some pith there then carry on and just do a full review.

MR. STIVER: Okay, we'll go ahead and take those marching orders.

MEMBER BEACH: And then I have one question on 54. This is just in general. Because of the number of cases, did SC&A feel like we should do a pre-review on that or just eliminate them totally from review?

MS. K. BEHLING: Well, based on the fact that we have looked pretty extensively at OTIB-70, I guess, as indicated by Ted, perhaps we don't even need a pre-review.

Again, if you want us to do just a quick look at this, we can do that. And like Ted said, back off if we feel that there's no real need.

CHAIR MUNN: When I first looked at it, the OTIB-70 jumped out at me as being something we have vetted extensively, and for that reason, and primarily that reason, I would have a tendency to reject 54, personally.

MS. K. BEHLING: Okay. And I'm comfortable with that. I just wanted to throw it in.

MEMBER ZIEMER: Yeah, I would hold off on it and let this one --

(Simultaneous speaking.)

CHAIR MUNN: Yeah, 52 has been appropriately assigned and we can move on to Table 3 and your response to the question as to whether or not you feel that it's appropriate to be going back to these and addressing them

further.

I don't see any really driving reason to do that, personally, but if other Board Members --

MEMBER BEACH: The only one that stands out to me is PER-0029 and that's just because of the number of claims that potentially affect.

CHAIR MUNN: Paul?

MEMBER ZIEMER: Yeah, I don't have any that jump out at me, but I'd go along with 29 if Josie is concerned about it.

MEMBER BEACH: Well, I just noticed it was referred back to the Work Group, so they may make a decision on that themselves.

CHAIR MUNN: Yeah, it doesn't seem necessary for us to address this here, in my personal view. I don't want to throw a monkey wrench in that.

MEMBER BEACH: No, I'm fine with

that.

CHAIR MUNN: All right. So I think we can dispense with concerns for that. And thank you. Anything else from anyone with respect to the PER recommendations? If not, then let's take a look at some of our details from carry-overs from previous meetings.

We have status reports that we haven't had in quite some time on what's going on with one report and four PERs as listed on your agenda. Can we hear a status update on RPRT-53 and the four PERs?

MR. HINNEFELD: I think RPRT-53 might be Jim.

DR. NETON: I'm sorry, I was distracted there for a second. RPRT-53. I think most people are aware, the Board is pretty aware of where we are. NIOSH is in the process of preparing an implementation guide in draft form right now that is accepting comments from

the Board.

We had a fairly lengthy discussion about that document, about the draft version of that document, at the Idaho Working Group that was held prior to the Idaho Board Meeting and received a lot of good feedback. The transcripts have been issued and I'm combing through the transcripts trying to glean the suggestions that were made during that discussion, and there were a number of them.

We're working on that from that front, and that will address a number of the findings that were made on -- at least to put NIOSH's position on paper as to the number of findings that were made in RPRT-53.

Also I think that a lot of that has to do with the evaluation of a significant difference, how you evaluate significant difference between a strata of coworker models, and NIOSH has put out a paper on that.

But we also, I think, have made some progress on the OPOS, the issue of OPOS. There seem to be some favorable reaction, at least from my opinion, from SC&A, on the use of a backwards time-weighted average calculation. I owe a response to SC&A on comments that was made on the use of calculations that we've done using IMBA in chronic versus -- multiple acute intakes versus a single chronic acute intake, and I will provide that shortly.

But I think there was -- I sense that there was reasonable agreement on that, so we may be moving towards some agreement on how OPOS is calculated. The key difference, the key thing in my mind right now is the evaluation of what do we consider a practical or a significant difference between two strata. That's about all I have to say.

CHAIR MUNN: Thank you, Jim. Shall we continue to carry this as a request for

status report, or are we almost to the point where we can begin to address your response to the findings specifically?

DR. NETON: I would carry it right now just as a status.

CHAIR MUNN: All right. We'll continue to carry it the way we have been.

Who is prepared to give us a status on the four PERs?

MR. HINNEFELD: This is Stu and I'll give it a try. Maybe somebody else can help me out. PER-0037 is Ames. And we also have an Ames Site Profile Review that we're working on responding to, so I presume that our research and response efforts for the Ames Site Profile Review will include the -- we'll just go ahead and throw the PER-0037 findings in there. I don't know to what extent they're similar, but we'll sort that out.

I was trying to look around BRS on

PER-0037. That particular one wasn't working. Everything else was working great and that one didn't seem to bring up any findings and didn't even seem to bring up a field that looked even remotely familiar, I mean a page that looked even remotely familiar. So did anyone try to put findings in the PER-0037 from --

MR. KATZ: Stu, there's no review yet.

MR. HINNEFELD: Oh, there is no review yet?

MR. KATZ: Right. The review's waiting for the Site Profile resolution.

MR. HINNEFELD: Thank you.

DR. NETON: That makes sense. I was going to say, because we didn't roll in any PER-0037.

MR. KATZ: That's right. No, I was about to interject. But SC&A we had to hold off on the PER review until the Site Profile goes

through resolution with the Work Group, which is just awaiting for your responses.

MR. HINNEFELD: Okay, good. So then I'm not behind the eight ball on 37.

DR. NETON: I can report that, somewhat related, that we have addressed all the findings, I think there were 22 on Ames, and I've got them on my inbox for review now. So we should be able to put those out fairly shortly.

(Simultaneous speaking.)

MR. HINNEFELD: PER-0011 is K-25, I think, that is -- really the finding that is open relates to how are construction trades workers identified and how are they selected, treated. And in going through that issue we've determined that there was some confusion on which workers that construction trades workers adjustment should be applied to. And so we are in the process of doing that. In fact, I think

we may have just reissued that construction trades worker OTIB, didn't we, Jim?

MS. K. BEHLING: OTIB-52 was updated.

MR. HINNEFELD: Yeah. So I think there might be some effort associated with trying to make sure, you know, look back at cases that maybe weren't treated appropriately. The confusion came in to whether the construction trade workers adjustment should apply only to subcontractor construction trade workers or should be applied to everybody with a construction trade worker job title, including what I would call in-house employees, the prime contractor employees.

So there was some confusion about that and it wasn't being used consistently, and so we've sorted that now that it should apparently apply to all construction trade workers regardless of their employer. And so

there's going to have to be some sort of look-back associated with that change as well.

Now, once that -- I think that action in some fashion may be able to finish out -- or we might be able to finish out this PER-0011 finding by referring to whatever action's coming out of the OTIB-52 revision and associated PER.

So we still need to sort that out. I'm not really ready to say definitely how we will say it, but I think we can probably close this finding based on that that OTIB-52 work.

I know that PER-0033 is a Reduction Pilot Plant and the only finding is no finding, so I don't know what we would have to do on that. Anybody got any ideas there, if I missed something there?

CHAIR MUNN: Certainly not I. Perhaps we need to take a close look at why we're even carrying that.

DR. BUCHANAN: Yeah, this is Ron Buchanan. We discussed that at the February 13th, and I have a note that we had no findings on the two cases we audited and that it was closed at the meeting on 2/13-14.

CHAIR MUNN: Do we say that in the BRS, or is that our only --

MR. HINNEFELD: The BRS has no finding in it, so there were no findings.

CHAIR MUNN: If there were no findings then we don't need to continue carrying it, if we've said that in the BRS.

MR. HINNEFELD: Yeah, it's in there.

CHAIR MUNN: Good. All right, I think Steve's checking on that for us just to make sure. Thank you, Steve.

MS. K. BEHLING: This is Kathy Behling. I can speak to PER-0018.

CHAIR MUNN: Kathy, you're

breaking up badly.

MR. HINNEFELD: Can't hear you. No, still can't hear you.

CHAIR MUNN: Something happened to your connection. You're really bad now.

MS. K. BEHLING: Okay. Hold on just a second.

MR. KATZ: It's a bad case of laryngitis.

MR. MARSCHKE: Wanda, am I looking for 18?

CHAIR MUNN: I'm sorry about that. No, PER-0033.

MR. MARSCHKE: 33.

CHAIR MUNN: There it is. No findings.

MR. MARSCHKE: Yeah, but it's open. We should make it as closed, shouldn't we?

CHAIR MUNN: Yeah, we should. I don't think you even need to have a comment. It

just needs to be closed.

MR. KATZ: So what Kathy was saying is that she could address PER-0018.

MS. K. BEHLING: Yes, can you hear me?

MR. KATZ: No. You're still --

MS. K. BEHLING: Hold on one second. Hold on one second.

CHAIR MUNN: Now we're hearing you. We're hearing you, Kathy. Suddenly you're okay.

MS. K. BEHLING: Is that any better?

MR. KATZ: Yeah.

CHAIR MUNN: It is.

MS. K. BEHLING: I think my phone was telling on me here. PER-0018, we actually finished our subtask for review on PER-0018, and PER-0018 is the LANL Site Profile revision. And that was sent out on May 30th of this year.

I can briefly go through that if you'd like. There were five cases that we reviewed and four findings. Do you still want to do that today?

CHAIR MUNN: I don't think we need to review the findings. Do you know whether we have them in the system?

MS. K. BEHLING: We do not. I have not gotten them into the system yet but I can certainly do that.

CHAIR MUNN: Okay. I think if we had that as our action item for next time then we will pull it out of our status review and bring it to the forefront.

MS. K. BEHLING: Okay.

CHAIR MUNN: Thanks, Kathy. I'd appreciate that.

MS. K. BEHLING: All right.

CHAIR MUNN: We'll look forward to that when you have an opportunity to do it.

MS. K. BEHLING: Okay, I will do

that shortly after this meeting.

CHAIR MUNN: Thank you. No further questions here with regard to status on the outstanding items that we have listed. Does anyone have any question or do you have any additional items that need to be added to our status list?

If not, then we'll move on to the next item, which is simply to comment for those who have any concern about it and who might not have followed what transpired recently.

The PPG status will be going to the hands of that Work Group, which has now been established, and we will stop carrying this item as a concern for us until we hear back from the Work Group.

Our next meeting needs to be defined. My suggestion would be two months from today, a little bit before today. My suggestion would be Wednesday, October the 22nd

or Thursday, October the 23rd.

MR. KATZ: Those are out of the question for me and probably Stu too.

CHAIR MUNN: Okay.

MEMBER BEACH: That's out of the question. Anything after the 20th for me won't work.

CHAIR MUNN: Then can we say either the 15th or the 16th?

MR. KATZ: I'm just wondering, it's awfully soon, considering that work needs to be done before it's a worthwhile meeting. I think we might as well then -- and I know that Josie's going to be off until, when do you get back, Josie?

MEMBER BEACH: November 20th.

CHAIR MUNN: That would put us into Thanksgiving. I would suggest not postponing it another month.

MR. KATZ: Well, I don't think we

have much work to do if we give it six weeks or whatever. I mean, there will be hardly anything to address.

CHAIR MUNN: Well, Ted, I might take a little issue with that. We have all of the things that we looked at early this morning we'll take care of that next time, we know what we're doing, we just haven't done it yet.

MR. KATZ: But the next time, I mean, we'll take care of it means they have to do work to be ready. The next time, I mean, we haven't been meeting --

CHAIR MUNN: All right.

MR. KATZ: -- every two months --

CHAIR MUNN: When would we suggest?

MR. KATZ: So, well, I would suggest we do this later in November, then. Stu can see --

(Simultaneous speaking.)

MR. KATZ: -- and if there's plenty

of work to have a meeting, that's fine. I'm not seeing where all that work is.

MR. HINNEFELD: Well, it's a little hard to judge how much will be accomplished. I think there are probably some things we can enter but I don't know how much. I mean, we always have the option for having a shorter meeting. Since we're not traveling, you know, there's no imperative to have a full-day meeting or a six-hour meeting.

We will accomplish what we -- you know, we will work to accomplish it, like we always do, to align, you know, fitting it in with the other work we do on the program. So we'll try to get -- but it's hard to judge today how much exactly we'll have in there.

CHAIR MUNN: Well, someone suggest a target date to me after the 20th, after the week of the 20th, if Josie is not --

MEMBER BEACH: Actually, Wanda,

this is --

CHAIR MUNN: -- to find out where she lives again.

MEMBER BEACH: Wanda, this is Josie. I'm actually available the 18th and 19th of November, if that works.

CHAIR MUNN: I'm waiting for a suggestion from others.

MR. HINNEFELD: Well, not that it matters very much but those dates work for me.

DR. NETON: The 19th does not work for me.

MEMBER BEACH: How's the 18th?

DR. NETON: Yes, the 18th works.

CHAIR MUNN: 11:00 a.m. Eastern Time, October the 18th.

DR. NETON: Wait, in October or November?

CHAIR MUNN: November the 18th.

MR. KATZ: November the 18th is

good here. Paul, is that good for you? Do we still have Paul on the line?

MEMBER ZIEMER: Yes, I was on mute. Is that October 18th?

CHAIR MUNN: No, November 18th.

MEMBER ZIEMER: Or November rather. Hang on here. November 18th, yes, that's okay.

CHAIR MUNN: Anyone have any problem with November 18th?

MR. KATZ: I'll send a note to Dick. But we'll do that in any event because it seems like his availability -- when he's available, it can fall away pretty quickly.

MEMBER ZIEMER: Was that November 18th?

CHAIR MUNN: November 18th. Tuesday, November 18th.

MEMBER ZIEMER: Yes, that's fine for me.

CHAIR MUNN: Any other business to be addressed today? If not, thank you for your help, and we will see you, talk to you, hopefully we'll see you in Los Angeles, and if not, then we'll talk to you three months from now.

MR. KATZ: Thanks, everyone.

CHAIR MUNN: Thank you much.

MEMBER ZIEMER: Thanks, Wanda.

CHAIR MUNN: Bye-bye.

MEMBER ZIEMER: Bye-bye.

(Whereupon, the above-entitled matter went off the record at 4:13 p.m.)