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

convenes the

WORKING GROUP MEETING

ADVISORY BOARD ON

RADIATION AND WORKER HEALTH

The verbatim transcript of the Meeting of the Advisory Board on Radiation and Worker Health Working Group held at The Westin Hotel, Cincinnati, Ohio, on Oct. 6, 2005.

C O N T E N T S

Oct. 6, 2005

TRANSCRIPT LEGEND

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-- (inaudible)/ (unintelligible) signifies speaker failure, usually failure to use a microphone.

In the following transcript (off microphone) refers to microphone malfunction or speaker's neglect to depress "on" button.

P A R T I C I P A N T S
(In Alphabetical Order)

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BEHLING, HANS, SC&A
BEHLING, KATHY, SC&A
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ELLIOTT, LARRY, NIOSH
FITZGERALD, JOE, SC&A
GLOVER, SAM, NIOSH
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HOMOKI-TITUS, LIZ, HHS/OGC
**LIVINGSTON, REV.
MAKHIJANI, ARJUN, SC&A
**MAURO, JOHN, SC&A
**MELO, DUNSTANA, SC&A
NETON, JIM, NIOSH
**PANTILLO, DANNY, CONGRESSMAN HIGGINS
**SCHROEDER, JANE, CONGRESSWOMAN SLAUGHTER
SHIELDS, LASHAWN, NIOSH
TOOHEY, RICHARD, ORAU
WALKER, ED, BETHLEHEM STEEL

**telephonically

P R O C E E D I N G S

10:07 a.m.

MR. GRIFFON: Good morning everyone. My name is Mark Griffon with the Advisory Board on Radiation and Worker Health. I'm chairing this working group session. And I think we have Lew Wade on the phone from, and Lew, I think you wanted to make some opening remarks and welcome everyone. I'll let you start the meeting off that way, I think.

WELCOME AND OPENING COMMENTS
DR. LEW WADE, EXECUTIVE SECRETARY

DR. WADE: Thank you, just I'd like to welcome everyone and thank you for your attendance. This is a working group of the Advisory Board. At the last Board meeting, this working group was formed. It consists of Mark acting as chair, Mike, Wanda and Robert, Richard Espinosa designated as alternate.

The working group was put together to bring focus and to expedite some of the many ongoing review activities that the Board has under its purview. The thought was that this working group could do somewhat detailed work leading up to a subcommittee meeting and a full board meeting that will take place in the middle of October. The particular issues that my recollection would

be that we'll need to be focused on are the Bethlehem Steel site profile review, TBD review. We have ongoing issues relative to the Task 3 review by SC&A. That is the review of the procedures. I think there needs to be some work done on that.

We have site profiles for the Savannah River site and Y-12. Also, that it could use some discussion. But in my discussions with John Mauro of SC&A, I know that they are finalizing their Rocky Flats review. There's an issue that's come up on this, what I define as high five plutonium issue, and I think it could be worthwhile to get that issue on the table.

The Board in its wisdom suggests that we hold this working group as a public meeting, and I think that's wise. I mean, the more open we do our business the better. The public is invited. There is no public comment period that has been scheduled for the meeting, but the Board did ask, and NIOSH concurred, that we would afford an opportunity to Ed Walker who really is the focal point for those interested in the Bethlehem activity, to give Ed an opportunity not only to attend the meeting, but to participate, you know,

within reason within the discussion as the working group takes up issues at Bethlehem.
So those are the introductory comments I would have. I mean, you're under the able direction of Mark, and I look forward to a most productive meeting, again, leading up to our subcommittee and board meeting. Thank you, Mark.

MR. GRIFFON: Thanks, Lew.
The only thing I will say, Ed Walker is here. I see Ed, and we will be taking up Bethlehem Steel as the first item. And I think we, you know, we want to extend the offer that if you have comments during that discussion, then feel free to come to the mike and we'll recognize you. You made the trip all this way again, so we do appreciate you being here.
And the other thing I think we should ask is I think there are other people on the phone line. I'm not sure if that's true, but has anyone else dialed in that we should acknowledge is at the meeting?

DR. MAKHIJANI: Kathy wanted to dial in but did not know the number.

MR. FITZGERALD: Well, I just called her. The number in the Federal Register apparently wasn't

working. So I went ahead and checked, and I gave them the number and the code, so it should be fine.

MR. GRIFFON: So as people come on --

MS. SCHROEDER: Are you asking for people to identify themselves?

MR. GRIFFON: Yeah, please.

MS. SCHROEDER: I'm Jane Schroeder with Congresswoman Slaughter’s Office.

REV. LIVINGSTON: I'm Reverend Livingston.

UNIDENTIFIED: (Inaudible).

MR. GRIFFON: Wait, wait, we're going to have to start that again because we couldn't get those recorded those names.

MS. SCHROEDER: Okay, I'll try again. My name is Jane Schroeder. It's S-C-H-R-O-E-D-E-R. I'm with Congresswoman Slaughter’s Office, the 28th district.

REV. LIVINGSTON: I'm Reverend Jerome Livingston with the (inaudible) group --

MR. GRIFFON: Reverend who? I'm sorry. Excuse me, Reverend who?

REV. LIVINGSTON: Livingston.

MR. GRIFFON: Livingston?

REV. LIVINGSTON: Correct.

MR. GRIFFON: For the Bethlehem Steel group, correct?

REV. LIVINGSTON: Correct.

MR. GRIFFON: Okay, thank you.

MS. MELO: I'm Dunstana Melo. I'm with SC&A.

DR. MAURO: John Mauro, SC&A.

DR. ANIGSTEIN: Robert Anigstein, SC&A.

MS. DeMERS: Kathy DeMers, SC&A.

MR. PANTILLO: Danny Pantillo, the office of Congressman Brian Higgins.

MR. GRIFFON: Can you repeat that? I'm sorry.

MR. PANTILLO: Sure, it's Danny Pantillo. It's P-A-N-T-I-L-L-O with the office of Congressman Brian Higgins.

MR. GRIFFON: Thank you.

Anyone else?

DR. WADE: This is Lew Wade again with NIOSH. Again, this is a working group meeting, not a board meeting. I don't think we're in any danger of having a quorum with the Board present, but Mark, I'll ask you to just sort of watch for that.

MR. GRIFFON: Right, that's part of the reason I wanted to do the roll call on the phone there. And I think as Lew stated, I think we're going to

start the with the Bethlehem Steel site profile TBD review. And my desire is to do the procedures review secondly and then, as we can get to them, either the Savannah River or Y-12 site profile, in the afternoon. And I'm not sure how far we'll get along. The procedures review might be time consuming.

But to start --

DR. WADE: This is Lew Wade. I would ask that when we do get to either Savannah River or Y-12, we do Y-12 first only because we have an SEC petition pending.

MR. GRIFFON: Okay, that's fine.

BETHLEHEM STEEL SITE PROFILE

TBD REVIEW

And to start with Bethlehem Steel I think what makes more sense is probably to have SC&A do a quick overview of their recent report, the supplemental review draft, rev. two, and then maybe have, give NIOSH a chance to respond, and then have -- open up for discussion after that. So, Joe or Arjun or -- Arjun, I guess, is going to present on this.

DR. MAURO: This is John Mauro. I just wanted to say that first of all I apologize for not being able to make the meeting. I hope everyone can

hear me okay. Arjun, myself and Bob Anigstein were the coauthors of the recent review. I was hoping I'd be able to give a little overview, but Arjun, if you could please, it's probably most efficient for you to give the overview regarding the latest revision of Bethlehem Steel. So I'd like to --

DR. MAKHIJANI: Yeah, thank you, John. Yeah, originally John actually prepared the initial draft of the slides and was going to make the presentation, but since he's not here, I'm sort of pinch-hitting for him.

Let me just preface this by saying that our report was submitted about a week ago, and then the day before yesterday, and one of the principle things in the report was that there were a number of illegible data points in some very important data sheets from Bethlehem Steel. And NIOSH sent us new, NIOSH got the originals of those data sheets and sent us a spreadsheet. We would like to see the originals of that if they are available, but we do take NIOSH's word, we took NIOSH's word at it that those were properly represented and that, of course, has changed the picture somewhat. How much and in

what way we haven't had quite time to look. We've done a little bit of preliminary looking at the data.

And so I just wanted to preface that by saying that there is a new element. I will try to address the element somewhat, but of course, we haven't had really time to have a considered analysis of this new information. I just want to stress that in the beginning.

Last I was here I presented. We had a number of issues that we made in our first review, and I think a number of the issues that we raised have been resolved. I just want to go over at least we're in concurrence with NIOSH. We've looked at the NIOSH analysis technically, and we think that those issues have been addressed or resolved or where we have found that our original concerns were not -- we raised some concerns in regard -- let me just go through them.

We agree, NIOSH has said that they’re going to use the Simonds data set at least for part of the time, and we agree with that. NIOSH is making a consistent use of the 95 percentiles, and we agree with this approach. In the first review SC&A had quite strongly raised a concern as to

whether the things that were marked as breathing zone samples in the old data sheets that were taken 50 years ago or more were actually breathing zone samples comparable to modern standards.

And we've looked at NIOSH's analysis that was commissioned, and we're in agreement with NIOSH that the samples were properly taken, and they appear to be represented. There were a few data points in the Bethlehem Steel data that didn't seem quite in line, but the laboratory analysis and the representation of the data, we don't have an issue with that anymore. That was a pretty major point in the last review.

We also have said that the Simonds' data set would be appropriate for estimating resuspension which was another significant point. And SC&A commissioned an analysis of industrial setting dust loadings as to what could be breathed in routinely in order to see whether there was some kind of upper limits to routine intakes of dust that could be determined.

There were two analyses that were commissioned. They're both in the report that we sent the board. And partly by happenstance, the limit was

in milligrams per cubic meter, 30 milligrams per cubic meter. But because here we're dealing with natural uranium, by happenstance it translates into about 600 times the maximum allowable concentration. Of course, enriched uranium would be more. If it were depleted uranium, it would be less.

But for natural uranium it turns out to be roughly the same number as the 95 percentile of the Simonds' data set. And so that made us sort of very comfortable that the 95 percentile of the Simonds' data set is a very claimant favorable and robust number for routine intakes in that kind of setting and so you would not be underestimating routine intakes by applying Simonds' data to Bethlehem Steel.

We had some caveats about cobbles and incidents in transient loadings, and I'll mention that a little bit later. As I mentioned, we've taken a look at the new Bethlehem Steel data. One of our, we had said in our report that Bethlehem Steel data are inadequate for estimating doses for 1951 and 1952. Now one of our problems with the Bethlehem Steel data set was that in the early data from April 1951 there were many

illegible points. That was also the data set that contained the highest air concentrations that were sort of legible or barely legible. And we had other issues that I want to mention, but that was one of the main ones. And two days ago we got a complete set of data. So now we have essentially all of the data that were taken at Bethlehem Steel in that period including those 13 points. They did contain quite a lot of high air concentrations.

There are some issues with -- so what I'm going to say about this data is very preliminary and John and Bob and I have discussed this. And we all agreed that we really need to look at this in more detail since we have a much richer data set. One of the issues was that there was only one breathing sample for, zone sample, for the early period in the data set that we analyzed in the report that we sent the board. Now there are six which, and so the number of breathing zone samples now is not that different from Simonds -- I haven't added up all the numbers, but it, the data set doesn't look, in terms of number of points, that different.

However, we still had some issues in the

interpretation of this data set in that both for the early period and for the later period -- let me just show you. Harry Chmelynski did some statistical analysis of the data set. I should put up this slide so you can look at it.

Can we put up this slide?

Anyway, our SC&A statistician analyzed the data set, and we found that there are two -- the October 6th folder, the PowerPoint, and it's slide number four.

Well, I'll just describe it. Let me just go on. It's clear that in the early period when they were doing the lead bath experimentation, when they were heating up the uranium in a lead bath, that it was much more dusty than in the later period. This observation was also made by the AEC, Mr. Eisenbud, I believe in 1951, that the lead bath rolling process generated -- there you go. You have it there. It's above, slide number four.

-- generated much more dust and was comparable -- yeah, that one -- and it was comparable to the Simonds' no ventilation dust loads. And the red line is the data up to October 27th, 1951, and the blue line is data from October 17th, 1951, to

the end of the period for which we have data, somewhere toward the end of 1952.

And you can see that the line that goes up to zero, the Z score of zero, the average is somewhat, the median is somewhat different, but they're both below one times MAC, but the 95 percentiles are very different. In one case more that a hundred times MAC. In the other case it's, I think, about seven times MAC or close to ten, just under ten.

The processes were different, generating different amounts of dust, and one of the observations that we have now on this data set that it does appear that the early period in Bethlehem Steel should be looked at in a different way. Well, our main message to the board about Bethlehem Steel data that's different from the report that we filed is that this is, because of the missing data points that have been filled in, this is a sort of different data set that needs a look.

One of the main reservations that we have about this data set is that the breathing zone samples, both for the early period and the later period, the averages are less than the general air

samples. And that makes us kind of a little bit uncomfortable as to how they are to be interpreted as breathing zone samples. Even though we agree that the labels are probably accurate, there's no report on this whole sampling as to what was the purpose of it that's comparable to Simonds. Just for reference the Simonds' breathing zone were ten times the general air, so it's the reverse direction by about a factor of 15 to 30 depending on the period.

So the next set of observations was really about the Simonds' data set, and we did conclude that it was an internally consistent data set that could be used in a claimant favorable way. We had three categories of workers about which we presented conclusions in our report. One was that since everybody at Bethlehem Steel in that period can apply, the people who were not in routine working contact with uranium, for them the Simonds' data is clearly very claimant favorable. For workers who were not involved in high transient loading incidents, for them also this is claimant favorable, especially as it is robust from the point of view of routine

exposures from dust loading analysis.
In the worker meeting of July 1, 2004, and in subsequent interviews that SC&A did with workers, it became very clear that some issues, especially the cobbles which happen more frequently at Bethlehem Steel than at Simonds where workers had to cut up these long uranium rods probably generating fumes, and fumes could generate high transient loadings for some workers, not for all workers. You know, you wouldn't expect this to be, say, typical of somebody who's working outside or in a different area or in a crane or something like that.
But for a worker who was cutting up these uranium rods during a cobble when the uranium gets out of line and gets tangled up like spaghetti and has to be cut into small pieces, we think that has to be taken into account in some way. We weren't sure whether it was going to add anything to 553, whether increasing that above 553 would be warranted or not. But it's certainly something that should be taken into account.
So generally we agree that the use of the Simonds' data set is pretty robust and transient incidents have to be taken into account. We have

not calculated any quantitative significance of those, but recommended worker interviews.

We had a pretty big discussion over the last many months about oronasal breathing, and we agree with NIOSH's analysis that in the specific context of Bethlehem Steel, it's a small relative difference, and it's not a large uncertainty given the kind of uncertainties we're talking about. But it's not a negligible factor, and for other areas where the other uncertainties, other facilities where the other uncertainties are lower, it could be an issue. We haven't, so it could be a general issue, but it doesn't appear to be a big issue at Bethlehem Steel.

Now we still do not -- here's a sort of a point which is still outstanding -- we don't agree with the use of TIB-0009 for, Technical Information Bulletin nine, for intakes as it doesn't take into account live particle intakes. And the numbers that were calculated in the revised site profile are sort of fortuitously similar.

Bob Anigstein came up with a different model that assumes a hundred milligram ingestion every day but progressively mixed in with greater quantities of steel as time goes on after the

rolling and resulted in a considerably bigger intake, about threefold bigger than the 5.21 calculated from TIB-0009. It probably won't make a big difference to most of the doses, but the technical method is out there.

We also did some analysis of the resuspension, and Bob Anigstein came up with a model that's described in there. One of the main things that we found was that because in between rollings there was no activity at Simonds that the dust wouldn't be stirred up in that, and that factor needed to be taken into account. Again, Bob developed this different model and came up with a higher intake. Certainly, this is also something, you know, all of these things are up for discussion. We've presented this as a alternative scientific approach that could be adopted.

We found that the point that we had raised in our last review, that there were some workers like inspectors who were touching these uranium rods with bare hands and may have had extended contact, that their skin doses and near surface doses should be calculated for these types of workers. And we didn't find that issue was

addressed in the TBD.

I think that's it. Thank you.

MR. GRIFFON: Thanks Arjun.

I think NIOSH is ready to respond. I'm not sure who's going to respond, but what I might ask for the sake of the people on the phone if we can remember to identify ourself when we talk. This is Mark Griffon by the way, violating my own rule.

DR. NETON: This is Jim Neton. We've had this document for just a week now, and we've taken a fairly preliminary look it, but we do have some initial take on some of these issues. And I don't know whether it's best to go through what our opinion is on each of these findings at this point or just to start -- open the table for some discussion on what we believe to be some of the more critical points. I'll just go through, I guess, and then we can open up for general discussion, but let me just get this thing going. I sort of have just a snapshot summary of what Arjun just went through. And I think probably the most significant issue that we take exception to is bullet number one or finding number one which I've sort of paraphrased here. It says

that we can't use any of the two hundred data points from Simonds Saw and Steel 1951 and '52 to predict the air concentration datas in those time periods. That's essentially what, that's exactly what they've said. They're inadequate to be used for any, they're not informative in any way of what happened at Simonds Saw and Steel '51 and '52.

UNIDENTIFIED SPEAKER: So you're saying Simonds --

MR. GRIFFON: Simonds Saw, you mean --

DR. NETON: I'm sorry, the Bethlehem Steel. I'm sorry. So, you know, we believe that when you have a couple hundred data points something can be used to do a bounding analyses, especially in light of the fact that it's acknowledged by SC&A that these processes were substantially different. If you recall, the Simonds Saw and Steel data represented roughly, it was about 40 data points, I think, which were taken under very similar circumstances by the same organization. The Health and Safety Laboratory covered both jobs.

But the Simonds' data, if you recall, essentially was roasted uranium. I mean, this was fired in a

furnace with no salt bath, no lead bath, and generated these extremely large concentrations where we had up to a thousand MAC air. But the picture at Bethlehem Steel is substantially different.

I mean, you have a combination of lead bath operations through October, and after October 1951, they adopted the salt bath as the method of preference for heating. So there is some very good technical reasons why these air sample concentrations went down over time. But SC&A has essentially, doesn't believe that they're informative as to why these concentrations should be lower.

Secondly, so from throwing out the air sample data then SC&A has resorted to saying well, the only informative information is either the Simonds Saw and Steel data or this sort of ancillary analysis they've done that said are bounding 30 milligram per cubic meter, choking atmosphere. And that's the best one can do to estimate exposures in '51 and '52.

One of the problems we have with that, and this is all open for discussion, is that we'd be interested to hear the mechanism that is there to

sustain air concentrations of that magnitude. So sustained 30 milligrams per cubic meter for ten hours a day requires some sort of a process that's going to generate that type of airborne, and we're hard pressed to come up with that mechanism. If you recall, the air samples were taken, and this is one of their criticisms, they're short duration air samples. Well, in essence, what they are is short duration because the process was short duration.

On August 26th and 27th, that first rolling, they rolled 72 billets. On average a billet takes about three minutes to go through the production mill. Remember, at Bethlehem Steel there was an 18 stand rolling mill. They only used stands 13 through 18 because these were finished rollings. These were like one-and-a-half inch or so diameter rods. It took about three minutes to go through so in my mind if you have 70 billets about three minutes a piece, there's 210 minutes of a process that will generate a large airborne that we've got captured in these air samples. That's about what? Three hours. A little over three hours, maybe four hours tops out of 20 hours, yet SC&A's analysis suggests that this was

going to be 30 milligrams per cubic meter for their additional 17 hours of rolling. It just does not seem plausible to us that that's the, a value that should be used for bounding at this facility. We have much more to say on these issues, but we'll start there.

The profile does not address short-term episodic air concentrations. This is related to the cobble situation where you do have looping and bending of the rods as they go through. Most of the analysis is based on the fact that there was an interview with a worker who indicated that the rods were cut with a torch. Although that may be true, we're having trouble understanding how that could happen.

Cutting uranium with a torch is, doesn't seem to be a good idea to us. It's a pyrophoric metal, particularly in the light that they've speculated that the concentrations in air could be as high as 300 milligrams per cubic meter. If you've got open torch cutting with 300 milligrams uranium in air, a small particulate size, it would suggest that this would be a fairly combustible atmosphere for uranium. So, you know, it may be that it happened, but we have done some searches.

We have no evidence that uranium, we could not find any evidence of uranium torch cutting that actually occurred, and, you know, I'd like to ask SC&A if they could substantiate that a little better. It just does not seem to be reasonable to us. It may be that steel rods were cut with a torch and such, but, and in fact, we've looked at a number of samples.

And where there were cobbles in an instance, there's an indication that the process was stopped, the rod, it was opened and the rod was removed. There was one instance where they had a cobble. They actually took it out of the process, put it back in the salt bath, reheated it, and reran it.

So the whole issue of cobbles and creating potentially six thousand MAC air, I think needs to be addressed a little better. I mean, it is based on an interview of a worker, but at this point, we have no substantiating evidence that that actually happened.

The other issues become more minor as we go down. Oronasal breathing, SC&A has acknowledged that it has a relatively small effect. I think they took issue with our use of the word negligible, which

I think would be a six percent. We calculated at most it would be a six percent increase in the MAC. We accept that criticism. I mean, it is, it's not a significant issue, I think, given the uncertainties of the air sample data here.

The injection and resuspension models, I mean these were purported to be new models. Really, they are essentially the same as what we've had just with different starting points in my mind.

The one thing I have, we have a problem with is the resuspension model, I believe, started with the resuspension being equal to the air concentration during rolling.

If you have to accept the fact, it's kind of hidden in there, but you're reading through and all of a sudden, whoa, let's assume that the resuspended air was equivalent, was all due, that the rolling operation was all due to resuspension, and that just does not make sense to us to start with that high of a concentration.

In fact, they're left with a chronic resuspension of about, I think it's 13 or 14 MAC air every day for four years which seems to be implausible to us for a facility of this nature.

Ingestion model, I think we're still struggling

with SC&A's guidance that 100 milligrams per day is an acceptable amount for ingestion to start with. This is not going to make a very huge difference in the end result. Our model, in fact, was pretty much the same where you just didn't, there's a diminution in the amount per day. It's just what's your starting point. External exposure model, I think this is an issue, and we've had similar discussions in the past where we developed a model that exposes a person for, you know, I think the model currently says that one foot for six hours and one meter for four hours or something to that effect, trying to sort of get a time-weighted average of what the exposures may have been. That does not preclude the fact that a person could have been handling the metal at any given time.

We're not saying that didn't happen. So we think the model is probably, is claimant favorable. We just need to go in and demonstrate that, you know, handling on occasion is not going to increase those values substantially. You know, there are issues here. Much of the time the uranium was being handled there's crowbars and gloves as workers have indicated. It's hot, it's

1100 degrees Fahrenheit while it's being rolled. It takes awhile to cool down.

So the amount of manual handling, and one has to remember also, these are one-and-a-half inch diameter rods. We were assuming that these were planer exposure geometries of about 230 millirem per hour beta on surface which is representative of an infinite plane source of uranium which these were not. So, you know, we can go back and look at that, but I think we've got some bounding estimates that are fairly claimant favorable.

I think that's enough to get the ball rolling, so I'll stop here and I guess we'll open up for discussion.

DR. MAURO: This is John Mauro. I would like to just comment on a couple of points you made to sort of kick this off if that's okay.

DR. NETON: Sure.

DR. MAURO: Regarding –- and again this is to get the ball rolling. Regarding the data we do not have any intention of saying that the data are useless if that's what, you know. If we did, we should not have. I think that there are data, now that we've gotten the additional data, there's no doubt, it was very much a part of our

considerations in our evaluation. So I, if we left that impression that the data is useless, we should not have. That's the first point I wanted to make, the data certainly is very much part of our analysis.

DR. NETON: John, I'd just like, the finding actually says the Bethlehem Steel dust data indicate they are inadequate for use in dose reconstruction. I'm not sure how else I would characterize that.

DR. MAURO: Well, yeah, you're right, and I guess where we're coming down is that there may be a better way to say that. Perhaps there's a better way to say that is taking everything into consideration. And there are a number of items that we've been talking about. We came down, and it wasn't an easy decision to make, we came down on the side that it seems like bounding -- There were concerns about making sure or trying to have a one-size-fits-all for all workers. There's a degree of confidence in that we don't underestimate the exposures in any worker. Everything considered where it comes down with Simonds Saw, Simonds Saw in the house so to speak. But I would not want to leave an

impression that we felt, and in that, that we're trying to make sure that no individual is (inaudible) would underestimate. There's not a doubt that the levels of exposure were lower and certainly coming down. Now that we have new data it's clear that we actually see a trend, but that observation regarding the (unintelligible) of the data, one of our concerns, of course, was the issue of the cobble.

That was another part of the equation. How do you deal with that issue? You don't know who might have experienced these transient exposures. How do you deal with that? And as an individual we find through a dose reconstruction we're not quite sure whether or not there's a data set which will (unintelligible) certainly is now approved.

Whether or not we've got a degree of confidence that we've captured the individual that may have had some unusual exposure, because as you know, a lot of our discussion with the workers reveal that there were some practices that were made reference to only one of which, or perhaps the most important one of which in our eyes up until this point, was cobbling.

Now, you make a very good point, and I think this is an important point of discussion is that it really is not plausible for that scenario in someone using an acetylene torch-type of apparatus for cutting the cobble. This is, at least the potential for this unusually high spike, of course, is that it diminished. So I want to say that. Quite frankly, we did not discuss that. In our lineup we took it basically on face value that well, we know this cobble had to be cut up. We have some information apparently that that might have been the way in which it was done. If it turns out that's not the way it was done, then certainly this needs to be revisited.

And finally, the issue of resuspension. Now, you (unintelligible) that the three things that as you were going through the material I sort of jotted down. The, with reading your approach in doing the resuspension, you have your line, your status and your scenario. And the way I understood the scenario was it made use of a couple things, made use of dust blowing the reserve at Bethlehem Steel at a time when things were quiet. There was no activity going on and

that characterization of the --

DR. ANIGSTEIN: Excuse me, John. You meant at Simonds there was --

DR. MAURO: I apologize. I meant at Simonds. No, that was our first. You know, I'm a little uncomfortable with using air sampling data between (unintelligible) so to speak, but where there's no physical activity. So that was a concern. And the other one I noticed in the write up that you would use as a way of sort of evaluating the resuspension issue or resuspension factor (inaudible) per meter. In a working environment like this where there might be the potential for kicking up dust as well as working, et cetera, et cetera, the potential item that sticks in my mind is a (unintelligible) factor of 10 to a hundred (unintelligible). So those two areas left us with what I would say uncomfortable.

So Bob Anigstein, came up with the strategy whereby, which I would be the first to admit is probably high on the other extreme. And you want to go with Simonds' general area. Now when you go with Simonds' breathing zone on this, and Bob, certainly you can elaborate on this, but we

approached Bob and Dr. (inaudible). One (unintelligible) is that the general air samples collected at Simonds, if you assume that that dust load which is substantially higher than the breathing zone samples.

Let's assume just to put an upper bound on it that --

DR. ANIGSTEIN: Excuse me, Doctor.

DR. MAURO: Yes, go ahead.

DR. ANIGSTEIN: The sample of air needs to be a factor of ten lower.

DR. MAURO: It needs to be under study.

DR. ANIGSTEIN: The general air samples are a factor of ten lower than the breathing general air samples.

DR. MAURO: Yes, I'm sorry, of course.

And so we went with that. Now in retrospect and in thinking about this, when we came up with our example approach, and Bob, you may want to come in on this.

DR. ANIGSTEIN: Okay.

DR. MAURO: We picked the 95 percentile value for general air samples at Simonds as our starting point, then of course, (unintelligible) down from there. I think in retrospect we probably should

have worked with the average. And I'm sort of (unintelligible) some of the realizations that when you think about it, resuspension's a long-term setting and the exposed people would experience -- would probably be more appropriate given every worker considerations that if you look for a way to bound the resuspension issue at Bethlehem Steel --

Perhaps a better way we should have done is go with the Simonds' general area samples, but not use the 95th percentile as a starting point, but use the mean. That might have been no bounding, 95th percentile to my mind now that we're talking true and a little bit more could be considered to be pushing the upper bound to the point where it's over the top.

This is my reaction to, I guess, some of the major points. Bob, I don't know what's your, do you have a sense of what I just described? Or are, you know, what do you think?

DR. ANIGSTEIN: The reason, if you took history, the reason for doing the 95th percentile was simply to be consistent with use of the 95th percentile of all the samples for worker exposures. So it seems to me like to be

consistent, we should continue the 95 percentile approach that had been used. If you go with -- I don't have, however, I don't have a strong stance on either way.

If you go with the average of the general air samples, you come down by a factor of six. So the whatever the dose from the resuspension would be a factor of six lower. The reason we're using the general air samples, Jim made the observation that these would not be all due to resuspension. Of course not, but we're looking in the absence of known data, of knowledge of what the resuspension really was at Bethlehem. It's not going to be any worse than the average, than the general air samples at Simonds.

So we're looking for a limiting approach not for the best estimate. That's the basic rationale for it, and then the rest of it is pretty straightforward, just a, the dust again, it's a limiting approach to say that on the day of the roll, we don't know when the facility was cleaned, if ever. We don't know when all the steel dust was removed, so we're making the limiting assumption that just before the uranium rolling there was no dust.

So that on the day of the uranium rolling, all the dust was uranium on, let's say they roll on a Sunday. On Monday, an equal amount of steel dust is mixed in over the period of the day. And on Tuesday, another equal amount, so you'd have one-half, one-third, one-fourth, and so forth on succeeding days, of uranium. And that the airborne concentration of all dust is equal to the general air sample due to resuspension, equal to the general air sample at Simonds during the rolling only the uranium (unintelligible) goes down day by day. That's the model.

MR. GRIFFON: Can I -- this is Mark Griffon. I just wanted to, after that response I'm curious that Bob just said that using the average versus the 95th brings the dose down by a factor of six. How does that compare with what was originally presented in the TBD? I'm trying to resolve these numbers in my head here.

DR. NETON: That's a good question. Dave Allen might have -- he's trying to find out right now.

MR. GRIFFON: The other thing let me just say for a second, I mean, I think that this might be an oversimplification, but I think that we got more common ground than initially we might have

thought. I think the first bullet, I think, deserves more lengthy discussion here. But the other things, I think, like the ingestion question, the resuspension question, I think we're getting, at least, you know, even when I hear numbers that are a factor of three apart for the ingestion, that to me is, well, you know, that's not too bad, you know.

So I think maybe we could spend a little more time on that first one. I have some questions in my mind about the one point that was raised that the breathing zone samples being less than the general area samples at Bethlehem as compared to the Simonds. It seemed like a flip-flop. I wondered if you had a response to that.

DR. NETON: We have some opinions on that, too. Do you want to move on then and let -- Dave, have you found the values for resuspension?

MR. ALLEN: Well, it's a ballpark. The number we used --

DR. NETON: Microphone, Dave.

MR. ALLEN: I'm sorry. Dave Allen, and I think, I'm looking real quick. I'm trying to remember what we did and look at the TBDs, so I could be wrong here, but at one point in here we have for

the month it would be equivalent to, what we have in here for the month it would be equivalent to inhaling 11.2 MAC in one day. So I'm just saying 11.2 MAC days per month, whereas --

DR. MAURO: Divide that by (unintelligible). Right now our average comes to 1.9 in MACs per day. In other words, I'm going to the (unintelligible) units here. In other words, you only need to use off rolling days. Our resuspension approach, using the average assignment for the general air we come in with 1.9 in MAC as the concentration in the air on the off rolling days. What does your number come to? Your number's in MAC days?

MR. ALLEN: Right, my question was are you assigning them resuspension inhalations for 29 days a month?

DR. NETON: John, I think you've 12.77 MAC according to --

DR. MAURO: Yes, we've got that. That's using with the 95th percentile.

DR. NETON: Right.

DR. MAURO: I just want to say quite frankly I think that was over the top. In retrospect I would sooner say the strategy that Bob has

adopted is certainly a bounding strategy given that we lack, at least from this perspective, given that we're a little bit uncomfortable with the sensitive item of resuspension factors in the approach involving (inaudible). So we took the position that --

MR. GRIFFON: John, John, we're having trouble recording you here, so maybe if you --

DR. MAURO: I'm sorry. Is that a little better? Can you hear me okay now?

MR. GRIFFON: That's better; that's much better.

DR. MAURO: Okay, I'm up close to the speaker here.

What I'm saying is, yes, you're right, Jim. In our report we came up with 12.77 MAC per workday for resuspension. I think that --

DR. ANIGSTEIN: We made no assumption about how many days a month a worker was working, but we don't know what the work schedule was except on uranium rolling days. That number should be applied to the days on which they worked.

MR. GRIFFON: This is Bob Anigstein, right?

DR. ANIGSTEIN: Right.

MR. GRIFFON: Make sure you identify yourself, please.

DR. ANIGSTEIN: Okay, sorry.
DR. MAURO: And then to complete that, if you use the average for defining general air's data as a surrogate for an upper end of what might have been the dust loading for resuspension at Bethlehem Steel, you come up with a concentration of 1.982 MAC on those days. And I guess it would be useful to say now how does that compare to your value that you just described in terms of MAC days? Are you going to have to convert that into a concentration on a given working day?
DR. ANIGSTEIN: If I can, I have a comment to that. So (unintelligible) MAC if you assume 22 days a month, say 22 work days a month.
DR. NETON: Forty-four MAC days.
DR. ANIGSTEIN: So that would be about 40-odd MAC days as opposed to 21.7 MAC which is in your Section 3.53 of the TBD. So now we're using a factor of two.
MR. ALLEN: I think it's -- just to correct you even though I don't want to here, it's -- this is Dave Allen again. I think it's 11.2.
DR. ANIGSTEIN: No, I disagree. The exponential model with -- I was reading the second sentence of this section. The exponential model will

ultimately produce an intake equivalent to a failing 21.7 MAC for one day. So the impact for one month, for the impact period, 30 days. Okay, I stand corrected. So then we'll multiply it by a factor of four. Assuming 22 days, I don't how many days you assumed.

MR. GRIFFON: So we're talking 40 MAC days versus 11.2 MAC days.

DR. NETON: I'm certainly much more comfortable getting down into the lower MAC range. I mean, to me 13 MAC exceeded most of the air samples that were collected at Bethlehem Steel in 1951 and '52 during rolling operations. So to assign that for those two years just seemed to me to be an implausibly high value which just made no sense to us at all.

And I think, you know, I don't intend, I don't think it's the intent that we're going to work all these details out at this working group meeting, but I think it's good that we --

MR. GRIFFON: No, no, but I think this brings us a lot closer, this discussion here, I think, brings us a lot closer.

DR. MAURO: I think, Jim, I think we're getting closer at this point. I think we're on an

agreement (inaudible). I think you can see where we concur. (unintelligible) Now certainly there may still be some point of discussion on this, the differences, but I think we're coming into an area where the differences are not large. I agree with Mark that I think really the place where our time is best spent perhaps today given our agenda is perhaps to address that first issue.

MR. GRIFFON: Right, and I think maybe we should delve into that right now.

DR. NETON: Now first I forgot to apologize for sending those data a couple days ago, but in the spirit of going with exactly the truth of what we have, you know, we did make the decision to send those data points out to clarify the remaining samples. And I do have the originals with me. SC&A is free to look at them throughout the day and verify that we, indeed, have interpreted the values properly.

They're much more easily readable, although I'll admit even in some cases on these copies, they're faint. These are those blue onion-skin-type pieces of paper. It's no surprise that the scans were not good quality.

MR. GRIFFON: Jim, can I ask you before we get into this specific, your first bullet. Ed, I think Ed had some comments so we'll let Ed Walker say a few words.

MR. WALKER: I just wanted to touch on some of these points that you had before we get far beyond them in my view as a worker. Unfortunately, I was there, and some of the things that I hear, and I've been hearing, and some of my issues in far more in what I'm going to be talking about today. I would just like to address these issues that kind of you are talking to in my opinion.

Now the first is the breathing zone issue. Obviously, I'm not a scientist, and I'm not a health physicist, but I can only tell you from experience and what I felt and what I saw down at Bethlehem Steel. And going over the breathing zone samples, what struck me the most was is the location where the ten breathing zone samples, if it so be that we rolled uranium for four years, and there was only four breathing zone samples taken, and they were all taken at the shear, now how these samples can be accurate when I believe some of your findings, there may have been one or

two that wasn't, but what I found most of them were taken, the information that I got was taken by shear.

And in your findings you say that the highest concentration was at the rolling as it went through the rollers. Well, the shearer was something like 400, probably around 400 feet away from where the rollers were at Bethlehem Steel because between the rollers and between the shears was the cooling bed which we know for a fact was over 300 feet long. At the end of this cooling bed stood a control panel of metal, sheet metal, where the fella that controlled all these rods, and you probably seen the illustration that I made. He sat there and he ran those rods through getting ready to shear.

As they cooled he moved them ahead and moved them. They were constantly moving on this rolling bed. So when he got them through, they were, to start off that they were cool. They weren't 1100 degrees no more. And he was from where the worst points of contamination according to you people was, was up by the rollers when it went through the roller. That's where you'd get the most radiation. And that's documented.

I can come up with these documents, not instantly, but I've read them all, and I've gone through it. So my question is if you're taking air samples, and you aren't even close to the areas where the worst contamination was, which would include the cooling bed and the salt bath and the rollers, the three operations that should have had the highest concentration were, they were taken protected.

DR. GLOVER: Mr. Walker, I don't know what data you may have -- is this on? I'm sorry, my name's Sam Glover. For the shears we had nine breathing zone samples taken as part of the shears.

MR. WALKER: Okay, that's about what I came up with.

DR. GLOVER: And so that's, but there are salt bath data. There is --

MR. WALKER: Breathing zone samples?

DR. GLOVER: Yes, sir.

MR. WALKER: I had requested the information, and what I got, and it came from Oak Ridge. I didn't see that there so that's what I'm basing my stuff on, just what I can tell you from what I saw.

DR. NETON: We did send you yesterday the, or day before yesterday. We tried to send the completed

analysis sheet for the April 26th and 27th rolling. I don't know if you received it. We sent it to the e-mail address that we've been communicating.

MR. WALKER: No, I didn't, but --

DR. GLOVER: I apologize.

MR. WALKER: -- it's kind of short notice. I've been at this thing for three years, and a day or two before the meeting doesn't hack it for me because I struggle going through all these things so, I don't work at it every day and, you know, I need a little bit of time to look into this.

MR. GRIFFON: Just one second, Ed. Can I ask Sam, how many in those other areas, breathing zones and the salt bath area are in the --

DR. GLOVER: There are, we have, just flipping through the data, I have actually put it into another sheet, but I don't have it with me. There are two. On the very first day there's two where they transfer from the lead bath to stand number one. Actually, there's four of that. You'll notice in this, and I think part of the difference at Bethlehem Steel versus Simonds, I haven't been there, but the size of it and the accessibility of the rolls, it doesn't lend

itself to standing up near it and getting intimately in contact with this. And you could, and as far as there is definitely a difference --

MR. WALKER: Let me tell you, okay?

DR. GLOVER: Let me just --

MR. WALKER: Standing right next to it, okay? Let me tell you. These guys, and it's documented in the reports of government people that went through, it is documented that people stood there and held hand-held thermometers to see between the rollers. And if you look at the pictures that I sent, and there's four inches of dust on the floor between these rollers, and there's a couple little holes. You can see it in the picture which I found after three years of research. And those three little holes are a foot square. You would be standing in dust, and they had to be fed in to the first roller with sledge hammers.

In most cases the first rod that went in had to be fed manually hitting the rods with sledge hammers on the first one. Maybe Simonds carried them around in a circle, but at Bethlehem Steel they put them in with a sledge, and in between the rollers, they had to station men with

crowbars to open up the wedges to receive these rods.

Now I'm going to very briefly, and I don't want to take up too much of your time to tell you. There were six stands, and the reason they went to Bethlehem Steel, it's called a continuous mill, because there was no other continuous mill. A continuous mill, they had the ability by running through six stands continually, they could take it from a billet down to a rod.

And when we talk of just finish rolling at Bethlehem Steel, the information that I have in the reports that Simonds Saw got billets from Mallinckrodt and so did Bethlehem Steel. Simonds Saw didn't rough roll them only and send them to Bethlehem Steel. Bethlehem Steel took the same ones, as far as the information that I have, and they rolled them down in a continuous operation. They didn't have to carry them around which they did at Simonds.

It went through, there was two mills. One was roughing, one was finishing at Simonds. They run it through the roughing mill once. They run it through and put it through the same mill the second time. There was two roughing rollings.

They took them over. They, I believe, they straightened them as much as they could. They brought them back, and then they went through the finishing process to get that billet down to a finished rod which they done. Bethlehem Steel, you put the rod in at one end, and it comes out as a finished rod.

So I'll probably get into that later, and I don't want to get too far into this. There's another issue about that that I'll be getting into.

As far as the cobbles, there were no cobbles at Simonds Saw. You can count on that because they didn't have a continuous mill. And a cobble simply means when this rod is going through the roller, if it doesn't hit that next roller going approximately -- it varied. There's different speeds, a hundred feet per minute, two hundred feet, three hundred feet. When it hit that next stand, it shot up in the air. It shot on the floor. It went down those holes in the drain. No one knew where it went. All the information that I heard that they cut them out. I can't swear to it because I never actually personally cut one out.

But I want to tell you they wrapped around

sometimes like a vine, and it would take them -- there's one document that says it took seven hours to clear that stand. You talk about a ten-hour work period. It took seven hours before they could get the plant started again so they done the rolling apparently on Sunday. So where is the ten hours? That's 20 hours right there, and that's just one instance that we know.

As I go through the documentation and find the cobbles that happened on almost every rolling, and another problem with that cobble is if that uranium rod wasn't heated to the exact temperature when it hit that roller, and if there was a cold spot in that rod, that cold spot would stop it from going in that roller because it was harder, obviously, not soft enough to take. And that would cause a cobble and shut down the plant.

When they had a cobble, they had to shut the whole line down, but that didn't mean that there wasn't uranium laying on the cooling bed. That doesn't mean they weren't shearing it, the stuff that was being cooled. So that was continuous. So I wanted to touch on that on the cobbles. There's no way that this information that I'm

hearing is what happened at Bethlehem Steel. And as far as Simonds Saw being smaller, it's much easier to clean out this room than it is this whole hotel. As far as the resuspension on it, I've talked to site workers. I've talked to probably 50 to 75 people that actually worked there. Some were credible; some of them weren't. And I took what I felt and from what they told me. When they were telling me what went on, I could pretty much tell if they were there when the uranium was being rolled. It was very obvious you could tell because if they weren't there when uranium was being rolled, their information was different.

And the resuspension, there was doors in the side of this place that would open, and they would periodically they would open them. And they were big doors that were almost the whole side of that building. So you talk about resuspension. If that uranium dust went up there on the weekend and laid up there, and that door was opened on a Monday where they were rolling regular steel, there probably wouldn't be any more steel going up there, any more dust going up from the steel. So you're not mixing up one-and-a-half, and it

didn't mix itself like a woman makes a cake. This stuff, as it fell down, it hit the heat and it blew it back up again. If you were setting there, and there was three inches of dust from the uranium rolling on that weekend being blown up there, and a crane went across, where do we get the steel in that uranium. That would fall right down on the workers. It would hit the heat. It would go back up.

Sure, it may mix with some, but this computing just how much it would mix every day is ridiculous. Some of that stuff could stay up there a week and never be knocked down, nothing on top of it. The uranium was laying there until something happened to make it come down.

In that photograph, that picture that I drew, I took to a fella that worked, that worked on top of that catwalk on the mill. And he said that that thing was right on. That picture was right on. He says except there's so much stuff that you left out of that picture that would contribute to colling (ph.) uranium during the rest of the week. And that is all electric lines. He says you ain't even close in that picture. And I says I know it, but if I showed

all that, I couldn't show this basement area and just what it was like.

I had to leave that stuff out, the gears and stuff it took to run that rolling process, all that was underneath, even the rollers. There was motors as big as cars underneath there where that uranium could go down in and get into those motors. And you're not cleaning it out on the weekend. And if those electricians and that went and worked the rest of the week, they were involved in that uranium so it wasn't a ten-hour shift.

Honestly, not everybody delved in it. There was only certain people, but I can't tell you which ones were there. A lot of these claimants call me. They all worked at (unintelligible), and they heard that's what was being done. So that's as far as the resuspension.

The ingestion I'm not going to say too much about, but I ate my sandwiches, and I would like to take a cup of anything you have here, being that I was there. And I'm not lying about this. I'd like to take a cup and just put any kind of dust in it and ask you to drink it. When you talk how much it was, and it didn't amount to

much, I'm telling you at times there was almost an eighth of an inch of dust in the plant. If something went over it, when you take your cup like this, and you'd go like that to get it to flow off the top and you'd drink it. Was it uranium? I don't know. Was it steel? I don't know, but we were there when the uranium was there.

And a sandwich is very similar. You'd take your sandwich -- and Dave, you know this from (inaudible) -- you'd take your sandwich and tip it over and it would fall off like pepper. So what about this ingestion? I think you really should talk to some of these people and set down. From day one nobody's come up and talked to these people until July of '04. And talk to some of these people that will swear, give you any kind of sworn statement you want on the conditions at Bethlehem Steel as far as the resuspension and this inhalation and stuff.

I got one claimant that's got cancer quite bad, and when he retired, his wife told me, she says it took him two weeks before he could spit up without having black stuff in his throat, two weeks to clear his throat just when he coughed

before he could spit up clear stuff. That's the kind of stuff we were dealing with, and that's the kind of stuff that I want you people to know in a clear conscience say that we weren't exposed to whatever was there.

That's all I have to say on this. I think it covers the resuspension. If you have any questions, you know, I'll try and answer them.

DR. NETON: I just have a quick question for Mr. Walker.

You indicated you had information that suggested that billets were rolled like five-inch diameter billets at Bethlehem Steel? We'd be interested in seeing that. Are you saying that they were rolled in stands other than 13 through 18 at Bethlehem Steel?

MR. WALKER: A six stand, and I questioned the rollers, the guys that actually run the rollers, and I says could you take down a billet from three to five inches and run it down to an inch-and-a-half? And he says easily. And he says the reason they only needed six stands because that's all it would take to get that down.

DR. NETON: All right, that's different than the information I have. I'd be very interested in

seeing that because the finishing roll --

MR. WALKER: That'll give you sworn affidavits that they can do that with that.

DR. NETON: Oh, I thought you had documentation from other areas. I mean, you have affidavits that said that they rolled five-inch diameter --

MR. WALKER: Well, I don't know if they were five inch, three to five. Now I don't know what they got. There were squares, and there were rounds from the information that I have.

DR. NETON: I understand, but of all the documentation that we have from Bethlehem Steel, there is no evidence that I've seen that suggests that anything other than finished rolling was done. In fact, that was the contract with the government, but I'd be very interested to --

MR. WALKER: I can get that. I'll mark it down, and I'll get it to you, Jim.

Any other questions?

MR. GRIFFON: Not for now. Thanks, Ed.

I think we need to speak to this representativeness of the data question and that gets in the number one so...

DR. NETON: I think one of the conclusions that was, aside from the fact that the legibility

issue was raised, I think the other fact that SC&A relied on was that the breathing zone air sample data were lower than the general area data which surprised them. And in fact, we looked at that analysis, and there was a Wilcox Rank Sum analysis done on that data which we understand. But what they've done in looking at the data, have done linear interpolations on lognormal data which, if you actually do a lognormal plot of this data and fit a curve to it, you end up with almost exactly the opposite conclusions. In fact, the mean value of the B-Z data are almost the same as the -- the geometric mean value of the B-Z data are almost the same as the geometric mean of the -- I'm sorry, Dave Allen is more familiar with this.

MR. ALLEN: The, instead of linear interpolation if you do the lognormal type of distribution plot that we've done on everything else and that SC&A says would be appropriate for this data, the G-A's for that same time period, the geometric mean comes out to be about 13- and-a-half DPM per cubic meter which is about a tenth of what the average is. The average is very high on the G-A's because of one outlier. And that one outlier

was also one of the two points used for the linear interpolation to get the high 95th percentile.

So if you use all of that in a typical lognormal plot, you end up with a geometric mean of about 13-and-a-half DPM per cubic meter and a 95th percentile of about, I've got 468 DPM per cubic meter for about 6.7 MAC. So between the G-A's and the B-Z's, the 95th comes out to be fairly similar but the geometric mean is about, well, the general area is about a fifth that of the B-Z's.

DR. ANIGSTEIN: This is Bob Anigstein. I'd like to speak to that. The only, actually, the data that we just got for the first day of rolling, April 26th, 27th, 1951. The one time that you had comparable data was the October -- let me be sure it's right here. Sorry, the January 26th, 1952 rolling is the greater set of B-Z's data non-zero were significant numbers. There were nine values on that day, on those two days, oh, it was one day, with three data sheets. I believe it was one day. And then at the same time so we have, you had about 20 -- I'm flipping on my spreadsheet now, so bear with me for a second.

We've had 12, we had 92 data points and 12 G-A data points so it was a comparable number. The arithmetic mean of the non-zero value or the B-Z is 116 DPM per cubic meter. The arithmetic mean for the G-A is 147, but the G-A is higher, but thus, you can't compare because for most days there were no B-Z, so you can't compare all the B-Zs to all the G-As. It's comparing apples and oranges. In the 95th percentile done by the methodology, the non-parametric methodology, while simply taking the (unintelligible) and seeing where the 95th is, not in some lognormal distribution for which there is really no valid basis. You end up with the one for that day is G-A ends up being 1055 DPM a cubic meter. And the B-Z ends up being 368.

MR. ALLEN: Yeah, I realize. The numbers I quoted are from the exact same data you're talking about. I have nine B-Zs, and I have 12 G-As from that one day. But taking the arithmetic, taking the just straight average means if you have one outlier, you blow that whole average out the door, and that's exactly what happened in this. So you're basing your entire argument here on one air sample instead of

taking a distribution of all twelve.

DR. MAKHIJANI: Are you talking about the same -- this is Arjun. Are you talking about the 1952 data set or are you talking about --

DR. ANIGSTEIN: January of 1952. (Unintelligible). I'll give you that point. We still find that we had assignments where we had all the samples were indicated. They were either G-A or B-Z, and the B-Z (inaudible), which is the whole idea of taking the B-Z. So we took a question whether there was enough B-Z data and something similar. I did a quick look at the April 26th, 27th, 1951, and a similar conclusion that might make a difference, but a similar conclusion. The G-A is higher than the B-Z. And we just question, that one big reason of questioning the Bethlehem data. And if the data, we don't question that the data was, the samples were taken correctly and properly analyzed by AEC. We did find a few data points, there were five data points which were miscalculated by AEC, but you know, that's five out of 199. Because the numbers they gave for the DPM and for the CPM, and the flow rate are not consistent with the DPM, again, that's a very minor point. We

concede that these were good samples, but the question is they were samples of what, and are they really samples of the workers' exposure?

DR. MAKHIJANI: I just want to return to the point I made during my presentation is I think the way this data, this is a complex set of data now, especially as the new points have been added. But almost the highest points in the early data set, in April 1951, are all general air samples. The highest, I think the highest four, are all general air samples. And they are much, much higher.

So if you take a, without doing any statistical analysis, if you just take a look at the data, it seems very odd that none of the high, the highest data point in general air is 29,000 and odd DPM per cubic meter. And the highest breathing zone is like 2,000 or 5,000. It's many, many times off, and there are many, there are several data points in general air that are more than 10,000 DPM per cubic meter.

And that doesn't, I mean, it raises a question about how you use this data, and how you interpret it, specially as you got another set of data that was taken with the explicit purpose of

estimating worker exposures and the time averaging was done and so on. And there you can see that the breathing zone samples are significantly higher. They're an order of magnitude higher than the general air, and that makes sense because if you're working, if you're taking a sample at the work location, you should expect a higher concentration unless you've got some resuspension problem. I don't want to go back there. I think --

MR. GRIFFON: Let NIOSH respond.

DR. NETON: I think, and what I see they didn't really look at very closely was the difference in the processes that there were occurring there. If one looks at the, that the whole purported purpose, and we have is the documentation, of the Bethlehem Steel process, as Mr. Walker correctly indicated, was to use a rolling mill, a finishing mill, or a continuous rolling mill so that they could quickly process this without having to go around and keep refeeding it. And that's exactly correct.

But one of the other main objectives of this process was to evaluate the health protection afforded by the use of salt baths, lead versus

salt as a heating mechanism. There were also some production benefits for using a bath. You got a better, more uniform temperature and such, but it was also well established that the coating provided by either the lead or the salt clearly minimized or reduced the air concentrations in the plant. And that's exactly what's happened at Bethlehem Steel.

So what you see in the early rollings, and I think the first -- Sam, correct me if I'm wrong, the first four rollings that we had data for? The first three rollings were a combination of lead and salt bath rollings. I mean, you can see in the documentation. These are lead bath; these are salt bath. And if you plot the lead versus the salt, this is what you get. Clearly, clearly, two different air concentration sets of data.

Now what we've done is combine those into one set which ends up giving you more, gives you a higher value. It's going to be assigned across all the years, you know, rather than break them apart. We're certainly willing to discuss the merit of breaking these into two different sets because clearly, the salt bath, the lower curve there,

the red triangles, is substantially lower. In fact, there's very low concentrations when the salt baths are being rolled. So you need to look at that.

To give you a little better perspective on this, this is a graph --

MR. GRIFFON: Jim, Mark Griffon. You don't have an overlay with Simonds on that same graph, do you?

DR. NETON: Yeah, I do as a matter of fact.

MR. GRIFFON: I'm just curious how that --

DR. NETON: That's with the Simonds, so their --

MR. GRIFFON: And Simonds is no salt baths at all, no lead or salt baths, right?

DR. NETON: That's correct. That's what I would call the roasted uranium technique, and the Simonds are the squares.

Now those, and Eisenbud was correct. He said that the lead salt bath was similar to what they saw at Simonds with no ventilation, I mean, within the realm of reasonableness, these curves are way up here. The salt bath treatment clearly added a protective effect. Now what also happened here was that since these things were encrusted and not roasted uranium -- I'll use

that term -- it appears to us at least at the stand, the distribution of the air concentrations by stand was somewhat different.

You got, I think the highest concentration -- Sam, help me out here -- at Simonds was typically at stand one. I mean, there was only one stand, but the first pass through the furnace. Taking it out of the furnace was a 70,000, 1,000 MAC sample. But when you put that through the first pass, you had this no protective coating so the concentrations were fairly high on that first pass through. At Bethlehem -- I think I have some data by stand. These are just the data, there's the data by stand, and actually, that --

DR. GLOVER: Then show it by date, by salt versus lead.

DR. NETON: This is the one that shows by salt versus lead. You can see that that sort of shows you what we saw in the other graph, that the lead bath is higher than the salt bath.

MR. GRIFFON: These are breathing zone samples or all samples?

DR. NETON: These are all samples.

The other thing is I don't know, I think we seem to be going down a path that the only usable data

we're ever going to be able to come to a conclusion on is breathing zone air sample data. And I'm not convinced that that's necessarily the case here. I mean, we need to go back and look at what the G-A samples were trying to do. These were not what you would call G-A samples where you went and sampled the cafeteria or some non-working environment. These were actually positioned at the rollers themselves. Now, they weren't a B-Z taken over a guy's shoulder like they did in those days, but they had samples, you know, during vertical rolls, horizontal rolls and that sort of thing. I'm certain we don't have exactly where they were, but they were in the vicinity of those rollings, not far away, and if the same positioning occurred at Simonds, they're very close.

MR. GRIFFON: Do you have any indication of what the difference would be between general area versus some that are labeled production samples or processing samples?

DR. NETON: The process sample was --

MR. GRIFFON: My impression was those were the ones right in the process of interest or whatever.

DR. NETON: Right, in fact, we have process samples which was included in our distributions although my recollection of the process samples were not all that much higher than the general areas samples. There's actually a similar spread, the process and general area. So the process samples were taken in areas where it was not normally considered that a worker would be positioned.

MR. GRIFFON: Yeah.

DR. NETON: Yeah, so they were not in the way. But I think these G-A samples were positioned at the stands, and if you look at the G-A samples that were taken at Simonds, I mean, there are some better descriptions of the positioning in relation to the stands. And these were not, you know, far removed from those stands.

MR. GRIFFON: I guess I'm not hearing that, you know, you can't use the general area air samples. What I'm hearing is it seems like there's a peculiarity between Bethlehem versus Simonds where at Bethlehem you had the B-Zs running lower avoiding all the lognormal versus linear.

DR NETON: Right, right.

MR. GRIFFON: The geometric mean versus average,

I mean, they tended to be lower as opposed to Simonds which the ratio flipped, and it seems a little peculiar especially if you're saying, you're saying that that early coating, I mean, you might even want to look at stand-by-stand ratios if you're saying the coating was protective on the first cycle through.

MR. ALLEN: That's the point, Mark, is the idea that the B-Zs were lower than the G-As was represented that way in the SC&A review, but that same data set it's just not true. It's only an artifact if you take an average because of one outlier that's high. If you actually take a geometric mean of all data points, the G-As are about a factor of five lower than the B-Zs.

MR. GRIFFON: And is that consistent with Simonds then? What was the -- it's in the ballpark?

DR. MAKHIJANI: It would be for the -- I didn't do the original analysis. I haven't looked at the data, but just taking Dave at his word and that is the case, that would apply to January 26th, 27th. I think if you look at the April 1951 data, you would have a very clear issue where the general air samples are much bigger.

And Mark, I agree with you. You know, we're not

saying that you can't use the general air samples because obviously in the Simonds distribution you've got both a breathing zone and general air samples that are part of the distribution. And because if you only use the breathing zone sample, you wind up in a different place.
So it's not a, it's a question of, it would be really useful to have some description of what was the purpose of this whole program comparable to what we have very clearly for Simonds because I find it quite hard to interpret the data in retrospect given the complexities and how long ago it is and the questions that we have.

MR. GRIFFON: Go ahead, Sam.

DR. GLOVER: Real briefly, I did want to make the point that in the text of the revisions, I did try to show what the stands, how the samples compared so we took the MAC that we were assigning, that the tower and show at the stand, this is what it was at Simonds Saw so that you actually could see how they compared and also Bethlehem Steel so you could actually look at that evaluation. And so I wasn't trying to hide that or to, you know, really try to bring that out, that this is the worst location and this is

how that data compares.

DR. NETON: I think one thing to remember is these samples were taken by the same program that took the samples at Simonds Saw and Steel for them, too, and several years later. And so presumably their thoughts and methodology developed to even be better not to be worse, and you know, we don't have a --

MR. GRIFFON: We don't have a written methodology for that, right?

DR. NETON: Well, we have some later documentation put together by Al Breslin that indicated the purpose of a G-A, a P, and a breathing zone air sample and that sort of thing, and you know, we don't have -- there was certainly not a time-motion study done here like there was at Simonds. I think that's what Arjun is referring to. It certainly would be better, but to sort of assume that they covered the least exposed locations with these air samples just does not seem to me to be a reasonable conclusion.

DR. MAKHIJANI: There's no implication about that they did a good job or bad job. In fact, we've agreed that the people who were doing the

sampling and the lab work seemed fine and the designations seem okay. It's just that if you go to Simonds and the purpose of the sampling is to estimate time-rated exposures, and you've got a track record that they did that, then you can be confident that what you calculate from that has some relationship to exposures.

When you've got a set of data where you don't know -- if the purpose of the expedition to Bethlehem Steel was simply to examine the difference between the processes and not to attend to the exposures of the workers and that's why a time-motion study wasn't done, then it becomes harder to think how you might apply these data to worker exposures.

It's not that the data are bad. It's a question of having the documentation to interpret it, specially as we've got these anomalies. And I've an open mind about it, I want to assure you. It's just that I don't know how we're going to interpret this data. Maybe it's worthy of further study, or I don't exactly know how where we go from here.

DR. NETON: Right, and I think maybe in light of these new data points that are there and SC&A is

still, you know, has not had a chance to look at them, and there may be some different conclusions drawn. I don't know. I don't want to speculate, but until that happens, I guess, we -- my main and our main objection to the conclusion was that you have to go directly from there to 550 MAC air. And that's really where we have our biggest source of anxiety is that is it reasonable then to say that that is the only bounding value one can use in light of the data.

Now we have more points. I understand that, and as I mentioned earlier that one has to provide some mechanism that can sustain the generation of that type of airborne activity given that these billets were rolled in three-to-five or whatever minutes per shot, and we know how many billets were rolled per rolling. That doesn't give you a mechanism to generate 30 milligram per cubic meter air for ten, twenty hours.

MR. GRIFFON: I got the impression, maybe not so much in the documentation but from Arjun's brief presentation this morning, that that thought experiment with the 30 milligrams per cubic meter sort of supported the 95th, not that it was a sustainable amount but that it sort of supported

your --

DR. NETON: No, I think what that does is support the fact that at the 95th -- it couldn't be higher than that, let's put it that way. Was it that high? I don't think it was that high at Simonds even on a kind of sustainable basis. We're saying the 95th is what is was, and we've agreed to use that. But then to say that at Bethlehem Steel where we have a couple hundred samples, it's also reasonable to conclude that 30 milligrams per cubic meter was there doesn't seem reasonable to us given that these were bath, you know, the report itself lists all of our reasons why we believe that these values are lower. And SC&A actually agrees that there is a substantial case to be made that these values were lower.

DR. MAKHIJANI: I think, you know, that there is agreement on many of the points and that we actually explicitly said in quoting the draft revision of the site profile that in most respects it does appear that Simonds Saw was more dusty than Bethlehem Steel from the data and the evidence and the nature of the processes. We did raise a couple of points where the contrary conclusion, you know, that point in the opposite

conclusion.
But you know, we did not ignore Bethlehem Steel data or thought that they were useless. In fact, we explicitly said that one, we arrived at a -- maybe we ought to start at a point of agreement that we arrived at a conclusion that for two sets of workers that this number of 553 was very claimant favorable. And one of the factors that we used in arriving at the conclusion was that 553 is really higher than every measured sample even including the new data points which we didn't have at that time at Bethlehem Steel.
So it's not a question of having ignored the data or not looked at it. It's a question of when you, how we have read the regulation is -- and this is a point of debate obviously -- is that when you've said you're going to resolve every uncertainty to the benefit of the claimants, how do you interpret that in a situation like this where you're using a surrogate facility? And obviously, you know, there's room for interpretation, and we should look at that.
MR. GRIFFON: I'm not sure the regulation says exactly that, but I'll let Jim respond to that. Ed, I know you have a comment.

DR. MAKHIJANI: I have the quotation here from the regulation here.

DR. NETON: I think it speaks to the fact if we have too uncertainty, we will give the benefit of the doubt to the claimant. But we believe that the uncertainty using the 95th percentile and all the other claimant-favorable assumptions that have been built into these models, which is a sustained continuous air concentration at the 95th percentile, accomplishes that. I mean, I don't think there's any requirement for us to pick the highest air concentration that was ever observed in a uranium processing facility in light of the fact that we have 200 data points which is what the report says.

MR. GRIFFON: Let me, something that was on my mind on this thing from the beginning is just what you alluded to, Jim, up here, is that it seems to me that when they went to the salt baths there was quite a drop off. And yet, you chose to, I think you chose to roll all the Bethlehem Steel data into one, lumping the lead bath and the salt bath data together.

What was the rationale there? Is there enough data to separate that out? It seems from an

exposure standpoint it might make sense that those people in the '52 rollings, maybe they were all the same people anyway, but they would have probably got much lower exposures than the earlier rollings.

DR. NETON: Right. I guess the issue is how closely do you parse the data and when you have a couple hundred samples that seems to be fairly good when you start parsing that. We really didn't make that conscious decision to say we're going to parse it out at that point. I think we tried to use as much data as possible recognizing that the lead bath technology is higher. But when you roll that up into the whole distribution, you end up with a higher 95th percentile than if you segregated it. It's sort of six of one, half dozen of the other. There is some merit, I think, in going back and looking at that, and we'd be interested to explore that. Certainly, the people in '52 would receive higher --.

MR. GRIFFON: Especially now that you have some of those illegible points.

DR. NETON: I'm not sure it would actually, in a cumulative sense it would not probably make that

much difference.

MR. ALLEN: I think it might go down just a little.

DR. NETON: It might go down a little. I don't know.

MR. ALLEN: You've got a longer time frame that they did the salt.

DR. NETON: But then to get into the issue of what's fair to a worker who maybe worked there in '52 versus '53, you know, then you have to look at that.

DR. MAURO: Jim, this is John Mauro. Can you guys hear me okay?

DR. NETON: Yeah.

DR. MAURO: Hello. Jim?

MR. GRIFFON: Go ahead, John.

DR. MAURO: I'm sorry. I wasn't sure you heard me. I'd like to also, part of this, one of the reasons we came down where we came down is also a lot of feedback about it appears that there were a lot of activities, practices, scenarios going on at Bethlehem Steel as described by Mr. Walker that leaves us with a sense that perhaps the samples we were taking or that were taken did not capture some of those activities, perhaps many of

those activities.
And that also had a bearing on where we came down. And that's why the 550 or the 33 milligram on the back of the 30 milligram number gave us a degree of comfort. That is, if there were such practices, and if they were widespread, and if there was uncertainty as to who participated in those practices, we felt that we came down in a place where those people were covered. So that plays also.
And I, quite frankly, I'd like to hear a little bit more about the degree to which there may have been practices that could have generated substantially elevated levels of dust that we really did not capture in the scenarios and in the data that we've embraced as our model for these facilities.
DR. NETON: Right, I hear what you're saying, John, and I -- oh, Mark has something to say.
MR. GRIFFON: Go ahead, Jim, respond to this. I just had a little birdie in my ear. They're asking for a break.
DR. NETON: We're aware that there were other activities, but in our minds the rolling of the uranium seems to have generated the highest

concentrations, you know, in both Simonds and Bethlehem Steel. We've taken a look at the centerless grinding activities which was, by the way, a water, a wet operation. We did not believe that that produced concentrations higher than what we're looking at here. So any of the other activities that have been mentioned, it's just hard for us to envision that they go higher than the values that we've seen at some of these stations where rollings were conducted. But we'd certainly be interested in entertaining, you know. It's one thing to say it happened. It's another thing to say is it really higher than what we're assigning here. And right now I don't think we believe that.

MR. GRIFFON: Let's, if it's okay with everybody, let's take a comfort break. Take like ten minutes, and I think maybe we can try to schedule lunch around 12:30. I mean, this will just be a quick break, comfort break, for our reporter and myself, and then, you know, we'll think about lunchtime being at 12:30 if that’s okay. Thanks.

(Thereupon, a break was taken from 11:45 a.m. to 12:05 p.m. after which

the following
transpired:)

MR. GRIFFON: John Mauro, are you on the phone?

DR. MAURO: Yes, I am.

MR. GRIFFON: John, we're having a hard time, Ray's having a hard time hearing you for the transcript, and I don't know if you can maybe go on a hard line instead of the speaker phone.

DR. MAURO: Is that better? It was on the squawk box. Can you hear me any better now?

MR. GRIFFON: That seems to be much better, yeah.

DR. MAURO: I was on the squawk box, you know, it's a little easier, but I certainly will talk directly into the, you know --

MR. GRIFFON: That will be helpful, appreciate that.

Just trying to reconvene, and I think we should probably close out on the Bethlehem Steel discussion pretty soon. I think the one item, at least my sense is that the one item that is most outstanding or most difference here between SC&A and NIOSH is the first bullet item of Jim Neton's presentation which speaks to the air sampling of Simonds Saw versus Bethlehem Steel, the representativeness of it and several other

issues.

My thought is that since some of that illegible data was only recently collected and put into a spreadsheet format, SC&A has not been able to do any assessment on that. I think we probably need to defer that issue to, and I'm suggesting maybe to, hopefully finalize this at the next subcommittee meeting which will be the meeting in Oak Ridge or Knoxville, I guess, really, the next full board meeting or the subcommittee before the full board meeting later this month, yeah, October 17th, yeah.

So in the meantime though several items have been brought up, at least from what I've heard. It seems like we've come to maybe better agreement on certain items. And to the extent that some of these things were offered during the discussion, I think that SC&A and NIOSH should both come prepared to show those differences. The one example that comes to mind is the resuspension calculations. If you're going to modify anything from your existing report, SC&A, you should probably come with that to the next subcommittee meeting to explain that.

Is there anything to add to this before we close

out? I do want to ask, Ed Walker asked to say a few more words, but before Ed, I'd just ask is there any other discussion before we want to close out?

DR. WADE: Mark, this is Lew. The only thing I think I actually, the only thing I would hold out is if either of the parties find it critical in their need to talk to the other party between now and the subcommittee meeting, I think we should encourage them, although I think again, we should do that, notify all of the work group members so that they could participate, but I wouldn't want to rule out the possibility of an interaction if either side wants to take steps towards the purpose of bringing this to closure.

MR. GRIFFON: Yeah, I certainly agree with that, yes.

Arjun.

DR. MAKHIJANI: Thank you, Dr. Wade, for clarifying that, and I would propose that the way Jim and I did for the Mallinckrodt report, and what we have done for this report that we simply make all the e-mails part of any official report, and Ed would be copied on everything, I presume, so he would be able to participate and know

what's going on.

But my question, Mark, for you was is there a specific expectation of a resolution to that first bullet point about the use of the Bethlehem Steel data and how do you, where do you see that headed, in your judgment? It's not a long time, but we're now in October 17th. Do you expect to hear from us before the working group meeting with a revised report or a memorandum on that question? I guess I'm not clear on what the process is.

MR. GRIFFON: I guess my sense is that, you know, one of the issues that you outlined in that section was the illegible data. So now is that still an unresolved issue? I think they've addressed, right, they've addressed that, and then, but you haven't had time to look at that data. So given the analysis you did in that first section, how would this new data, you know. Maybe you want to re-look at that analysis and see if it changes any conclusions. That's what I would say. And in the meantime I also agree with Lew that dialogue between, you know, with NIOSH might help to come to some conclusions there.

Ken, did you have any --

MR. PRESLEY: Mark, is there any way though that before we go to the 17th meeting that we can have some type of a closure on this from SC&A and NIOSH on this one point?

DR. WADE: I think that's terribly important. To speak to your issue, Arjun, and to Mr. Presley's point, there are two ways this could work. Let's take just the one point. One is that, you know, based upon deliberation and consideration by both sides that you come to a meeting of the minds. There's a possibility that you don't come to a meeting of the minds.

I think in the first case it would be just as important to know that, understand that. If you don't come to a meeting of the minds, I think it's essential that the board can look at clearly articulated points. Obviously the board would have to make its recommendation. So I would hope that before the board meeting that we could hear from both SC&A and NIOSH on this point. Either we've agreed or we haven't agreed and here's our position on it.

MR. PRESLEY: That's what I would like to see.

DR. MAURO: Lew and other members of the working group, this is John Mauro. What comes to mind

that would be of particular importance in terms of coming to as much we can to closure has to do with the issue of unusual practices, cutting of the cobbles, other words, activities, scenarios, practices that may have taken place. A richer understanding of that because a lot of our concerns related to where, why, where we came down is this discomfort we have that the actual data that we do have did not capture some of these practices.

And of particular importance that Jim brought up that we had not looked at was the possible implausibility that these cobbles were not cut with a torch of some type because that would then reduce the likelihood of substantially high levels of fumes which are very small particle size and could be fairly high in concentrations. One of their areas that I think a dialogue over the next two days might be especially productive is to come a little closer together on our understanding regarding that particular matter.

MS. DeMERS: This is Kathy DeMers. Can I ask Ed a question?

MR. GRIFFON: Yeah, go ahead, Kathy.

MS. DeMERS: Ed, we know that you guys weren't

aware you were rolling uranium. Was cutting of cobbles common for when it occurred with steel?

MR. GRIFFON: We're getting a mike for Ed right now.

MS. DeMERS: Did you guys hear me?

MR. WALKER: Yes, I've got -- is it on? Kathy, would you repeat the question, please?

MS. DeMERS: We know that you guys were not aware that you were working with uranium, okay?

MR. WALKER: Correct.

MS. DeMERS: Now when you had a problem with steel, what did you do to get them out of the rollers?

MR. WALKER: We burned them out. That was the only possible way you could get a steel cobble out of, steel out of the cobble, is to burn them out.

MR. GRIFFON: As opposed to this torch cutting that they --

MR. WALKER: Well, burning out would be torch cutting. That would be the same.

MR. GRIFFON: Okay, that's what I thought. So it's the same thing.

MR. WALKER: And I never heard any of the experts ever say that they done anything. I will check.

I'll go back and bring up this issue to them and check and see. But as far as I know they had to be burned out. There was no other way to get them out. You couldn't pull them out. They had to be burned out. It's possible they could have been cut with what we call today a chop saw, but I don't think so.

MS. DeMERS: Well, Ed, as far as you guys were concerned, uranium was steel.

MR. WALKER: Right.

MS. DeMERS: Okay.

MR. WALKER: Is that all?

MS. DeMERS: Yes.

MR. GRIFFON: Arjun is --

DR. MAKHIJANI: Yeah, I have a request of Ed. You know we're going to correspond, SC&A will correspond with NIOSH about this forthwith. I think starting tomorrow. You're going to be copied on everything. This is a point that was brought up by workers and people who really know. I've never worked in a steel or uranium rolling mill, right? I think it's extremely important for the most knowledgeable people in your community to participate in this so we can have the best judgment possible.

Because as Jim has said, uranium, and we all know that uranium is pyrophoric. It catches fire very easily under these kind of conditions. And this is a kind of very important point for the integrity of the process, and we don't have documentation. If you have any documentation from the period, statements of workers, at least I would very much like to have the benefit of looking at them. And urgently, because as you can see, we're asked to come up with an opinion very rapidly. So I would request that.

MR. WALKER: To the site experts that actually worked down there, there's quite a few of them. And I only talk to the ones that I feel, and that's all I have done. If I feel they're not telling me the truth or I catch them in something that isn't right such as the fella that worked in the straightener. I talked to him, and I got an interview. And I believe Kathy has that or I'll send it to her where he loaded into the straightener with his bare hands.

He had a partner and today his hands are crippled and he's got severe headaches and his front lobal (sic) brain cell. He's about 76 years old, but he actually picked up the steel by hand off of

the, after the shear, and carried it and put it into the straightener. And as he put it in, he commented how when he put it in and these rollers were rolling to pull this into the straightener, how it would flash and light up the whole plant and blind him 'cause obviously he had no protection. And truthfully, I says is that your arthritis in your hand. He said no, I don't know what it is. The doctor's don't know. And his hands look like they've been run over by a bulldozer.

And I says, Richard, I says what went on back by the cooling rack and what went back on back by the salt bath? And he says, Ed, I don't know. I went to work there. My job is at the straightener. I done my job for eight, sixteen hours a day, he said, and that was it. I didn't walk around and look to see what was going on. So I have no information other than what happened at that straightener at that time that I worked on.

So that's how I'm getting my information. I worked there as a bricklayer. At times I was there, and there was times I wasn't there. There was a lot of the people that I talked to worked

there much longer obviously in the plant as rollers that worked on it all the time. And that's where I get my information from. And I tell them if you don't know, tell me 'cause I cannot go down and talk to these people and find out that some of the information you gave me is false. I says I'm not going to be questioned on it. If you don't know, I'd much rather you tell me right now, Ed, and this is just like what Richard done. So that's how I get my information and I'll look as long as I can.

MR. GRIFFON: Thanks, Ed. Let me -- I'll give you the mike in a second, too. I know you had some other thoughts to share.

Let me just ask in terms of schedule, I'm hearing Bob and Lew Wade, and it would be nice to have some kind of more conclusive process before the next board meeting. On the other hand I'm looking and saying, you know, it's the 6th now, and we've got till the 17th. All I would say is that maybe if there are any amendments to the SC&A report or any, it'd be nice to have them at that Friday before, which is the 14th. Yeah, at least in draft form, something that we can consider before we get there on the 17th.

DR. MAURO: Yeah, Mark, this is John Mauro. Given that the main body of our report is only 27 pages, everything we've been talking about right now, there might be some effect on some of the appendices, but I'm going to go out on a limb a little bit, and I believe between now and let's say a week from tomorrow, you know, we're going to be busy putting our slide presentations together for the meeting on the 17th. But I think it's plausible for us to issue another revision because I think it will affect only some of the main body of the 27 pages that makes up our main report. So I'll go out on a limb a little bit, and we're going to do our best to give you a revised report that will reflect not only this conversation, but also the dialogue that we engage NIOSH in over the coming weeks. It'll be ambitious, but I think it's important that we try to do that.

DR. MAKHIJANI: Can I have a conversation with our project manager in public, please?

DR. MAURO: Certainly, go ahead, Arjun.

DR. MAKHIJANI: John, I think the approach suggested to kind of present a memorandum of revisions rather than a revised report will avoid

the logistics of typesetting and revising, and we can work to present any new or revised conclusions maybe in a table with text or something like that and then give a revised report after the board for the record rather than provide, I think the logistics of a revised report may take a couple of days that would be more useful in actually doing the work.

DR. MAURO: Okay, Arjun, fine, but I think it's critical that we come to closure as much, as best we can on many of these issues as we can. If they take the form of a memorandum or such like, that's fine. Maybe that's the best way to go, just let's get the final report out after the meeting. Quite frankly, the Monday morning meeting on the 17th, more may emerge so perhaps that is a wise choice, so let's wait and see a little bit.

MR. GRIFFON: Okay, we're in agreement here, John. Wanda has some comments.

MS. MUNN: John, this is Wanda. Thank you very much for making the effort to try to get some additional data settled between now and the upcoming meeting. The fact that you have only six findings and that most of them probably

balance on a single technical point gives me hope that perhaps you can do that.

One of the other things that has not been discussed here in depth, and I don't know whether it was your intention to take this up again after lunch or not, but there's no question in my mind, I keep hearing the issue of oronasal breathing. And that's just about been beaten to death in one form or another here, but I see no solution or agreement between NIOSH and SC&A on that point as yet.

It seems to me that we're going to have to face this and face it preferably with Bethlehem Steel with the understanding that it may vary somewhat at the other sites, but it would certainly be of great comfort to several members of the board if we could put that to some degree of rest.

MR. GRIFFON: I thought I heard agreement. That's the one I had okay next to, but maybe Jim.

DR. MAURO: Arjun, if you want to respond on it, either way.

MR. GRIFFON: Jim Neton first then --

DR. NETON: Well, you guys can go first if you'd like, but I thought there was substantial agreement there that for this particular site

profile, oronasal breathing would not make a substantial difference. The issue was whether it was negligible or not, and we agree it wasn't. Possibly not negligible. There was, I think, a six percent adjustment that may or may not be required, and we have not really addressed that issue yet. But I don't think that's a show stopper to any extent.

DR. MAURO: I'm going to be very frank. I think it's a non-issue on Bethlehem Steel in the margin. I think, however, a more fundamental issue regarding whether or not it should be a matter of policy to take into consideration the fact that some workers -- now we're not talking Bethlehem Steel anymore, just in general -- are mouth breathers and certainly we have some data that says there could be 15 to 20 percent of the population. This becomes almost a -- and I mentioned this at one of our meetings awhile ago. So yes, the issue is still on the table from a generic point of view. I think it's off the table for all intents and purposes of Bethlehem Steel.

MS. MUNN: I understand that, but my concern was the larger policy issue because if we have to go

through the kind of exercise we've been through on the last two sites at every one that we come to, it would certainly be far more beneficial and far more expeditious if we could come to some general agreement about how we're going to address that issue. It would resolve problems for all of us I think.

DR. NETON: I agree, Wanda, and I think if we get into it, that same comment occurs in the, I think it's either the Y-12 or the Savannah River review or both. So it's an issue that we're going to have to deal with. It's coming up in the next set of reviews. We'd like to put this issue to bed as well.

DR. MAKHIJANI: May I make a suggestion that since it is a generic issue as John has said, then maybe it ought to be addressed in that context rather than it coming up. And maybe there's a range of uncertainties within which it's important to take it into account. And you know, when the other uncertainties are big, then it becomes a non-issue or a small issue that can be regarded as not important. And so some general guidance maybe can be developed inside NIOSH since they did do an analysis that we

agreed with on the question. So we don't think there's a kind of a technical difference anymore. It's a question of how do you go from here to a general guidance which is a policy issue --

MR. GRIFFON: And how is it applied programmatically, yeah.

DR. NETON: This becomes a -- I think we tried to address this to a certain extent at the last board meeting with the Bethlehem Steel comment. And the fact is the difference in breathing depositions among people is greater than the difference in the oronasal breathing deposition parameters. And in some sense one has to argue then if we correct for oronasal breathing, then do we correct for differential breathing rates among different sized people, different sexes, you know, all those sort of issues come into play at that point. And then one creates a quagmire of corrections that may never, we may never end, but these are the issues, the policy issues, that need to be addressed, and it's a fairly broad issue.

DR. MAURO: Yes, in fact, Jim, I think you hit the nail on the head. We managed to sort of avoid this issue with Bethlehem Steel because we

had a way to avoid it. It's not important here. I think though that the line of attack on this issue of the type that you just described, mainly reference man and the definition of reference man and the inherent variability in all of the parameters that make up the respiratory tract model. Your, as I understand it is well, this is just one more of those parameters.

And I think that coming at this issue from that perspective and whether or not we should look at the oronasal breathing as something different and separate from all the other parameters that make up the genetics of retention, deposition and clearance for the respiratory tract model, that really becomes the question.

I agree with you that if this is just one more of many parameters that represent a definition of reference man, then we, you know, why are we taking on it? However, there's reason to believe that ICRP provides for this type of treatment or to separately look at oral breathing. You know, that may be, then it becomes a matter of interpretation, whether we should be trying to apply this as a claimant favorable approach.

By the way, I don't know if Kathy Behling is

there today?

DR. NETON: Yes.

DR. MAURO: Kathy, is oronasal breathing on the Task 3 internal dosimetry generic issues?

DR. BEHLING: No. It is at the Savannah River site as Jim had already mentioned.

DR. MAURO: I didn't know if you heard the question.

DR. NETON: Yes. It's not -- Hans is going to go to the microphone, John.

DR. BEHLING: John, it's not in the Task 3 report, but it is on the table for discussion if we get to the Savannah River site today.

DR. MAURO: Okay. Now the reason I raise the question is I agree with Arjun that this is a generic issue, and it could be addressed offline or on a case-by-case basis. It would be much more satisfying as Wanda indicated if we could address it generically and put it to bed once and for all. I just thought it might be one of the ones we'd be looking at on Task 3, but apparently not.

MR. GRIFFON: I think we're all in agreement on that, John.

Larry Elliott has a comment.

MR. ELLIOTT: This is Larry Elliott and speaking from a program policy perspective here today. That's what you're talking about.

Jim Neton has clearly put on the record our thinking and our position, our rationale on this issue, and it is a generic issue in our mind. We stand on that, and that's the policy that's being applied right now; what you heard from Jim two, three meetings ago, I believe, on this issue. That's where we stand. If we need to, you know, refresh your memories of that, we can do that maybe at the next meeting or do whatever and walk through that. But that's essentially where we stand, and that's what the policy is. That's what's being applied.

MR. GRIFFON: Yeah, we might need a refresher on that.

DR. NETON: Yeah, I think we could prepare to do that maybe at the next meeting or whenever people want. But someone made the comment about ICRP not addressing it. I just want to clarify. ICRP actually chose to ignore oronasal breathing in their models because of the exact reason that I just mentioned. The difference between, among people is greater than the difference in oronasal

breathing differences.
Also, it gets into the issue of do we default and make a correction universally on one side or do we incorporate this into the distribution at which point I'd argue that the GSD of three on internal dose calculations already includes that. But I won't say anymore on that. We can pick that up at another time.
MR. GRIFFON: Yeah, and I think we've all agreed that this is a general issue, and we need to address it. But for Bethlehem Steel I think it's kind of, there's agreement here on this issue. So I think I wanted to try to close out Bethlehem Steel discussion now. If there's anything else -- I know Ed has some things.
I'm sorry to make you wait so long.
Ed has some final things he wants to share with us.
MR. WALKER: That's all right, Mark, I'm retired. Well, when I started out, I was under the impression and I was told that if we have any information, we should present it to NIOSH, and they'll give it consideration. There's been many issues that I have that I've tried to get through, and I have not been able to get through.

There's many issues, and a lot of it just requires a simple answer. If a group of three or four would have come up and sat down and says, Ed, this is where it's at. But when I don't get any answer...

(Telephone line interference occurred.)

What was that? So I'm concerned about that, and one of the things is this weighing the range letter that I've had for quite awhile, and we've discussed, I think, before. But it kind of upset me as I said. I feel my job for working with this group is to find information and present it to the people so they get a fair treatment on it. And there seemed to be a black hole in '49 to '51. The range letter was sent to the assistant manager, Mr. Anderson, of the Environmental Control.

I don't know if you're all familiar with this, but the Bethlehem Steel Quality, Environmental Quality Control. Now it says many things, but one of the things that kind of caught my eye was beginning in approximately 1949 it was determined that then current production rolling of uranium billets to rods left much to be desired in the present reduction in the mill pass schedules.

And this regular production was not being performed at Bethlehem Steel but through a contract with them. It was determined that a suitable blooming mill -- they determined this. Eddie Walker didn't determine this, and this is what they say in '76 -- and a suitable continuous mill -- which is a ten inch mill, which we all know -- existed for the necessary development and work to indentify (sic) required pass schedules. Okay, it mentions that mill. There's a lot of mills at Bethlehem Steel. There's a 12 inch mill, 13 inch mill, strip mill, you name it. But they specifically said the blooming mill. The blooming mill reduces the size of billets, or in most cases, ingots which is the first step of a billet, okay? So they said the blooming mill was there. So why would they say it if they didn't, why did they say the ten inch mill. Why didn't they just say the ten inch mill if they didn't mean it? Okay.

To the best of our ability we have established that Bethlehem Steel was given a contract for the necessary development for pass schedules. For the development of pass schedules, given a contract -- well, I read that. And work

accomplishing in the period -- in the period -- between '49 and '51 -- to '51. It doesn't say including '51 which we know they done experimental rollings. But this is their wording that it was experimental work that was being done between '49 to '51. It says a lot of other things but unimportant.

It goes on to say the extent to which air samples or surface sample contamination readings were taken is not known -- is not known, okay? It goes on to say these records long since have been destroyed. We do not believe there are any remaining records of the archives at Bethlehem Steel Lackawanna plant. It is quite probable that in 1949 to 1951 time periods of the technical information developed was a classified nature and for this reason was returned to the AEC.

Okay, classified nature and the technical development. They did not walk into that plant in 1951 and say tomorrow you're going to roll uranium rods. They had to set up. They had to experiment with it. One of the gentlemen said they done it in another plant. I've never seen anything about that. But it does state there --

I didn't say it -- that there was mills used in one area. Here it does say, it mentions mills being used.

So as I look at it as a worker, there was more than one mill being used. I take this information. I confront it. It's thrown out. Well, that doesn't mean nothing. That's a letter of somebody that lost their memory. Could be, but they used parts of it in their dose reconstruction.

This is what upsets me. I found a document that said what happened. It didn't say rollings happened. It said developmental experiments were being done at Bethlehem Steel. How much contamination did they get with the salt baths, the lead bath that they were dealing with then and probably more lead baths and uranium. When they had to figure out running it through the pass schedules, the heat temperatures and that because it had to be heated evenly, how much experimental work that they say was done, was done at Bethlehem Steel from '49 to '51 that we're not given any credit for? Because Jim will say that we are allowing you for a rolling once a month.

That doesn't mean much. We don't know what they done. We don't know what their exposure was. We've taken exposure samples from Simonds Saw to say what we done later on, but we don't know what happened at Bethlehem Steel at that time. We don't know what exposure they had and how often they had it. So if you get a document, and I present it to NIOSH, and they completely disregard it, I don't think it's fair to us.

I want to go with the lost uranium a little bit. I won't take up much longer. I know you're all hungry including myself, so I would just like to talk about the lost uranium which the document says four to six pounds of uranium was lost in the rolling process at Bethlehem Steel on every billet rolled. People will say well, they take care of that when they say they picked it up and they cleaned up the site.

No. Because if they picked it up and they weighed it, it would be accountable. This is unaccountable, was lost at the site. I have people, when this uranium come off the rollers, and a lot of it that was washed was put into what they call a scaling pit. I have a man that worked in the scaling pit. He told me when they

loaded these scaling pits, they cleaned them out once a month so that uranium wasn't cleaned out every day.

They cleaned them out once a month, sometimes twice a month. They put it in a railroad car. And what the man told me was I noticed, he says, there was no air hoses on the railroad car and this scaling was dumped that come out, not, it was mixed with steel, granted that. But it was dumped on and it was with no air hoses means it could not go off the plant site because you can't take a railroad car of any sort without brakes and air hoses had the brakes. So it was dumped on the site but there's no residual.

And that's what he told me. I didn't pump this into him. He says the first thing I noticed there was no air hoses on that car. So when you talk about residual contamination, there's a good source of it. And we don't know how often that was done, but at least for two years we know for sure.

That's about all I got to say, but I hope that I can stay in touch, and I really appreciate giving me this opportunity to talk. But there are quite a few other issues that when I send in, I would

like somebody to respond. And as I said, a lot of these questions may be answered very simple and easy. And I can understand. I'm not a hard person to understand or reason with. So thank you again, and hopefully I'll see you in Tennessee. Thanks again.

MR. GRIFFON: Thanks.

Larry's going to give us a response.

MR. ELLIOTT: I appreciate Mr. Walker's comments, and I must take exception though, Mr. Walker, that if you look on our website, we have responded to inquiries you sent to us, the information you shared with us. There are documents there that show what you sent and our reaction to your input, and we're going to stand by that. If there are points that you want to discuss further that you think you haven't got full reaction to, we should talk about that. We should continue the dialogue. I don't want to shut the dialogue down, but I just want to make sure that people understand for the record that we have been responsive and we have been working together on this. It's not that we have been ignoring the input that you've been giving us.

MR. GRIFFON: Thank you.

Any further thoughts on Bethlehem? Otherwise, I think we'll break for lunch. Ed has --

MR. WALKER: And again, I'll make this real short because I'm getting hungrier, too. But what really upsets me is our technical-based document came out in 2003, '03. It wasn't until 16 months after that date that we've been denied, that our claimants have been denied, that anyone talked to anybody from Bethlehem Steel site experts. And it was my understanding that the dose reconstruction is taken from the technical-based document which is taken from the site profile. Our site profile was done 16 months after our technical-based document. And when I questioned, a couple months after that I questioned, I says where is our site document? Where did you get the information? I was told Simonds Saw. And I says could you send me Simonds Saw site profile being that you used their documentation to assess our contamination site. And he says it is not done.

So this is what upsets me about what went on. I can't see how anybody can do an assessment and not go there or not talk to anybody who was there. If there was car accident out in

California, I could not set in Eden, New York, and say the two cars hit at 50 miles an hour. This guy was at fault. I would have to go and talk. And this was not done. Clearly, I want to know that this was not done till 15 months after our claimants had been denied, and I think that's a gross injustice to Bethlehem Steel.

MR. ELLIOTT: Larry Elliott, and I appreciate your frustration, and it is a complex and a very difficult program to understand. It's a difficult program to manage as you might suspect. In trying to handle a large caseload of claims, yes, we made some decisions early on as to when to draw the line and say we had enough data to provide a reasonable estimate of dose to determine the compensability of a case.

We did that in the early days, as you say, with Bethlehem Steel. We had experts at the table that understood the data and understood what it was like to work in a steel mill. Had we been to the steel mill at Bethlehem's site? No, sir, I've told you we hadn't. We didn't. We did get input through our interview process. We did take action later on, as you know, maybe too late, but we were doing a variety of things, as you know,

to get a meeting set up where we come in and said here's what we're working with. What do you think of it? What can we do to make it better? We did do that, and you gave us input, and we addressed that input. This is a process. You're part of that process, and we're very proud of the work you're doing in that process. And I just, you know, I wish we could do better for you. We're trying our level best, but the, you know, I think it's remarkable that for Bethlehem Steel, we have paid out over 45 percent of the cases there through dose reconstruction.

We think it is a very sound scientific product that we're using to do that. We've given the benefit of the doubt as appropriate to the claimants. And if there are ways that we can improve upon that, that's why I'm standing here. That's why I want to be here.

MR. GRIFFON: Okay, last comment, Dick Toohey, wants to make a comment and then we're going to --

DR. TOOHEY: Dick Toohey, ORAU. I just want to clarify for the record this issue on TBDs and site profiles. TBD is a technical basis document and it's just one document category we use in our

document control system. For an atomic weapon employer site there is only one document, the technical basis document. That is the site profile. That's it.

So there's only one document and rev zero of the Bethlehem Steel TBD which came out in 2003, as you say, that was it. It has been revised since then as more data became available. For a DOE site, which in general were larger and more complex, the site profile consists of a set of TBDs, each looking at a different aspect of exposure. But usually an AWE only did one thing so we can cover it in one document.

Secondly, in terms of using the Simonds Saw and Steel data, we thought that was the most complete set of air sampling data at a site that had rolled uranium. Obviously, we had nowhere near that much data from Bethlehem Steel. So we thought given the lack of whatever large, reasonable, adequate amount of data from Bethlehem Steel, the best way to develop an exposure model for these workers would be to use actual air monitoring data from Simonds Saw and Steel, and that's what we did.

And we looked at and analyzed that data, and we

saw no need to complete the Simonds Saw and Steel TBD before using that data. And the reason we went to Bethlehem Steel first, our order of doing site profiles was basically by the number of claimants from the site. And as you know, Bethlehem Steel was by and large had the largest number of claimants of any of the atomic weapon employer sites so that's where we started.

MR. GRIFFON: Thanks for that clarification. I, myself, have been a little loose with the language of a site profile versus TBD, so we might have created some of that confusion ourselves.

With that I think we'll close out on Bethlehem Steel and have lunch. So 1:45, we've got a busy schedule, so we'll start up at 1:45. Thanks.

(Thereupon, a lunch break was taken and the meeting reconvened at 2:00 p.m. after which the following transpired:)

TASK 3 PROCEDURES REVIEW

MR. GRIFFON: Let's reconvene. I think we're going to take up the procedures review, the Task 3 Procedures Review. And probably it looks like

the focus is going to be on the external dose issues because that appears to be what SC&A has completed the matrix elements and NIOSH has just now provided some draft responses for us. So we at least have, I think that's where we'll have the most fruitful discussions or dialogue.

And I did want to raise one thing though that was a little point of confusion coming in here for me was that I actually thought this was the priority of this work group and apparently this got de-prioritized as the time went between the last Advisory Board meeting and this work group meeting. And I'm not sure who did that so I just, you know, if, I mean, this may have been done in conjunction with Lew talking with our chair, but I don't know that the Advisory Board was in the loop.

And that's one concern I have going forward. There's nothing we can do about it now, but you know, I think this is a pretty important priority, and if it was to be lowered in the scheme of priorities, and I know everybody was loaded down with work between these last two meetings, but if it was going to be lowered, maybe someone should have let the Advisory Board

know about it.

DR. WADE: This is Lew, Mark. It wasn't lowered by any conscious effort. I mean, I, it was my understanding coming out of the Advisory Board that we were to look at a number of issues. We did not receive SC&A's matrix on internal dose until, I guess it was yesterday. And you know, I think that was just the press of business on their part. So I don't think it was a conscious on anybody's part to de-prioritize this.

MR. GRIFFON: Well, I think we've spent a lot of time at the last meeting saying can SC&A complete the matrix by this work group meeting? And can NIOSH give responses back by then? There was some hemming and hawing, but everybody agreed to the date, and then we come in and we're not there. So I, just, you know, and I don't, look, we just have to proceed with what we've got, but I think in the future if there's going to be, you know, there's limited resources, everybody's got limited time.

If there's some issue that arises, I think somebody has to bring the board into the circle, and you know, we'll work through it. And maybe we would have come to the same conclusion, but

you know, at least bring us in the loop I think. That's my opinion anyway. I'm not sure I speak for the entire board here. But given that I think we need to start on the matrix, and my sense is, if this is a good way to do this, I think this is where we need, a nice informal discussion going finding by finding down the matrix.

Some of these I think we'll pass by fairly quickly, given NIOSH's response. Some of them are going to be a little more technical in nature, and we'll have to have a discussion explanation by SC&A and an explanation of the response. So I think the best way might be just to start at the beginning of your matrix and work down and have SC&A start the --.

MS. BEHLING: This is Kathy Behling of SC&A, and if I could just make a few brief opening comments about Task 3 because it has been quite some time since we published this report and actually, it was well over a year that we started working on this project. And so therefore, just to re-familiarize everyone with what the Task 3 project was all about, and what we did.

First of all, the participants were Hans Behling

and myself, Joyce Lipsztein, Arjun Makhijani, Kathy DeMers and Steve Ostrow. And we, as I said, started this over a year ago, and we were initially given 33 procedures that were selected, I guess, by the board and NIOSH that represented the primary procedures used at the time for the dose reconstruction process. I want to note that since then, obviously, there have been a lot of additional TBDs so this isn't complete at this point in time by any means.

When we evaluated this, we looked at this and evaluated from assessing seven objectives that focused on timeliness, efficiency, completeness, consistency, claimant favorability and the procedures and the methodologicability to account for uncertainty. And then lastly to try to balance this adequate science against efficiency and to determine if the procedures did that.

So in light of that you will see in our matrix that was developed from the findings of the Task 3 report that there are a lot of issues that are not technical in nature. I guess the matrix that we will be using today to, does try to follow the Task 3 report pretty much page by page.

And the other point I wanted to make is when we

started this project, this is really one of the first tasks of the, Task 3 was one of the first tasks we did, the procedure reviews. And it preceded us actually looking at the dose reconstruction audits. And so I guess Hans has mentioned this before, but at the time it was almost as if we went into the showroom and looked at the car and kicked the tires and looked at specifications.

And since then we've obviously had the opportunity to do some, a lot of dose reconstruction reviews; and therefore, we have actually taken these procedures for their test drive. And so as we go through this matrix there may be things that at the time seemed more significant than they possibly are now and maybe the other way around also. But I just wanted to point that out and remind everyone of the process.

I was going to suggest that we would discuss only those items that possibly we had some differences of opinion on, but it appears that you prefer to go through them one by one, and we're prepared to do that.

MR. GRIFFON: I think, I mean, just to step

through them so people can follow along, and the ones that there's no difference of opinion, let's dispose of them quickly, you know.

MS. BEHLING: And one last item, this is the first that we've, in fact, as we were sitting here is the first that I saw NIOSH's response. So as we discuss these topics, we haven't even had a chance to read through these.

DR. BEHLING: Yes, and just -- this is Hans Behling, just to put things into perspective, it's part of the expanded review process. And as of today we have not had any direct dialogue with NIOSH over the Task 3 report in spite of the fact that the report was issued back in January of this year. So this is really the first time we've had the opportunity to discuss the technical merits of some of the issues that we've raised. And it's basically we're starting at ground zero with this discussion of an expanded review. And it's possible that we may have to go beyond today, but it's also possible that we may be able to resolve these things in an informal fashion hereafter.

I guess we'll do it in sequence. We will start out, for those who have the matrix in front of

you, the first procedure, and it's really the dominant procedure that defines really the foundation for all dose reconstruction for external dosimetry and that is the OCAS implementation guide 0-0-1.

And as Kathy already mentioned, there are some issues that go outside the scope of technical issues as we'd mentioned or as Kathy mentioned, we had with the board's approval defined seven different criteria that we wanted to assess these procedures, and some of them were really addressing the issue of process efficiency, and do these procedures meet that objective.

We've heard all kinds of comments made by people over the last couple years at various board hearings that what's taking you so long. And we considered that an important issue. We need to resolve these dose reconstructions as quickly as possible for the obvious reasons. And so many of these issues will be somewhat subjective in nature, and there may not be a necessary resolution. It's just a comment, a criticism that we make without the expectation that we anticipate a revision in these procedures. It's just a comment, and I want everyone to understand

that.
So the first comment that we have, or issues that we identified here is the format, the structure of the document. And quite honestly, I looked at this and as a potential person who might be asked to do a dose reconstruction, and I found some of the information provided as somewhat fragmented, difficult to follow, and perhaps excessive in terms of what information would be really needed. So the first comment that you see here are deficiencies with procedural layout, the fragmented structure of the procedure, and in some instances what we consider, or I consider, excessive information that served really no purpose.
And I don't want to go unnecessarily into detail, but for instance, when we talk about as an example the issue of occupational medical exposure. There are a certain number of pages dedicated in the implementation guide that almost reads like a primer on Health Physics 101 on x-ray and so forth that really in the end should have been something that every person who's part of the dose reconstruction process has already had, fully understands, and certainly serves no

purpose because we're not going to sit there and reconstruct an organ dose based on our understanding of milliamps and milliseconds and distances and chest wall thickness and those kinds of things.

And in the end we will use always the default parameter values used in the derivation of organ doses as defined in the various documents whether it's the implementation guide, the TBDs or in TIB-0006. So again, this is just an example where I felt you have to go through an awful lot of information to get to the point where you understand what is really being offered to you as a way of procedural guidance. And so that's issue one, and as I said, it falls into the category of a subjective criticism that may or may not require any resolution at all.

MR. HINNEFELD: Okay, from our standpoint, just I want to make one comment here that will maybe help us out later on. We have a generic response here that says that we don't disagree with the comment at all, but that any particular revision we would consider a relatively low priority revision to rearrange this structure. And so this first response is a sort of a generic

response that you'll see several times down the page when we encounter a comment that we think that's in that category. Now if we've misapplied that one, you know, we don't understand that the nature of the comment or the finding at some point, we want to make sure that I point that out today because this response will occur several times down the matrix.

DR. BEHLING: For instance if, for those who may have read the actual Task 3 report, when I said fragmented, for instance, to go through the issue of photons, you have to go through a whole series of steps that says first we discuss dose recorded dose. And we have to talk about dose recorded dose for photons, electrons and neutrons. And then we go from missed dose. This photon, and this was clearly identified as an improvement in PROC-006 where you aggregated them, and it was nicely done there.

MR. GRIFFON: Can I make a suggestion? Where we have general agreement, unless you're disputing that, maybe we can give a very quick description of the finding so that we can get to the meatier ones.

DR. BEHLING: Okay, and I think I accept your

comments and at this point I don't think we need to discuss issue number one. Let's go to the second one and that is guidance for deriving film and TLD dosimeter uncertainty neutron dose from source term and occupational medical doses and x-ray machine levels.

Skip the second one, but talk about uncertainty because at this point as Kathy pointed out already, when we reviewed these procedures, we had no real understanding of how some of the issues that we identified would translate into the actual dose reconstruction process.

And at this point in time I will say this. We've had now 60 audits of dose reconstruction reports, and some of these things have turned out to be exactly what I would have predicted. With regard to uncertainty with film dosimetry, the implementation guide gives you a lot of formulas and they give you methods by which you can calculate it if you understand what the densitometer values were and so forth. And in some cases early on when films were essentially issued to people 52 times a year on a weekly basis, the recommendation to do uncertainty would have resulted in an expenditure of time that was

phenomenal.
And clearly, now that I've had a chance to look at 60 audits, not one person has ever made an attempt to do this, and it's clearly understandable. And so my recommendation is, while it's very nice to explain what the nature of uncertainty is, but clearly no one is really capable of doing it in a practicable way. And so my recommendation here is while the uncertainty was described adequately on a technical level, it really lacks the ability to be used in a functional way.
And my recommendation is to assign at 30, 40 percent value that people can use when they come up with a dose, a recorded dose, and saying well, what do I put in under parameter two? And in most instances, as I said, the people have, either they've ignored it entirely and avoided the need for an uncertainty which is obviously deficiency and certainly not claimant favorable, or they've gone to the opposite direction by using the maximized approach by saying we'll multiply all recorded doses by a factor of two and that covers my need to deal with uncertainty. Now again, that might have been even appropriate

for the instances where a maximized dose is the objective of the dose reconstruction, but when the day comes where we have to deal with best estimate, there you are, in fact, at this point no longer in a position to make use of maximized approach of multiplying the recorded dose by two, where you're now faced with having to assign an uncertainty. And I would recommend we resolve this issue by perhaps identifying a reasonable percent value as an uncertainty value for film or TLD and exempt the dose reconstructor from having to go through this tedious process.

MR. GRIFFON: It's a little more extensive than what's here.

MR. HINNEFELD: Right, now our response really, response to the parts other than the uncertainty part of the comment, and I think I just neglected to include our uncertainty response in it. If you look at finding number IG-004, we said that we feel like the IG, the implementation guide's uncertainty description should support what is being done, as you say, in the dose reconstructions you look at. Let's have a discussion that supports that uncertainty if it's not been prepared yet. We're suggesting we'll

revise the IG, take out this uncertainty language and insert a basis for what is being done in dose reconstruction.

DR. BEHLING: And I also wanted to say it's a discussion that we could probably spend the balance of this afternoon on just by itself. The uncertainty as I found out was really not a complete uncertainty that did not address, for instance, a radiological uncertainty or environmental uncertainty. It seems that it was mostly based, or the formula that were given were essentially dealing with the laboratory uncertainties.

MR. HINNEFELD: Right, you're jumping ahead. That comes up later.

MS. BEHLING: The other thing that we also recognize is the workbooks, the workbooks that are being developed, the best estimate-type workbooks, do take into account the uncertainty on the dosimetry, the attempt to do that with Monte Carlo and crystal ball runs. However, based on this particular document, that's where the comment of uncertainty comes in.

MR. GRIFFON: I think the other thing I see repeated and for OCAS-IG-001, and also that I

think we should keep in mind is that it's a general guidance document, right?

MR. HINNEFELD: Yes.

MR. GRIFFON: So there is, I see that reflected in your response several times that, yeah.

MR. HINNEFELD: There's language in that document that would lead you to conclude that, so a dose reconstructor is supposed to be looking at this when he does dose reconstructions. But that's really not the intent. The intent is this is general guidance for performing dose reconstructions, and so, we're not really trying to write a step-by-step procedure in IG-001.

DR. NETON: Right, I can address that a little better. I was largely responsible for working with the person who drafted this document early on. And that's exactly the intent was to lay out the framework for general concepts and what issues need to be addressed. And then the subsequent procedures that are based on them would flow from them and be more general and use the efficiency process and that sort of thing. But at some point I thought we felt the need to at least address these higher tier issues some place. And it's an implementation guide, very

similar to what you see in either DOE or NRC facilities where the implementation guides themselves, they're not really useful to a person in the field trying to do a job. It's a policy, not a policy, but it's a technical basis almost.

MS. BEHLING: And I believe actually when we started to review this, I think we agree with that we understand that was the intent. However, when we started doing the dose reconstructions, I don't think there's a dose reconstruction that we've looked at that they don't reference the implementation guide. They seem to use the implementation guide quite a bit.

MR. GRIFFON: So what do we say about the second finding? Is there agreement there with NIOSH's response?

MR. HINNEFELD: In our response remember, we also have the uncertainty edit that should be part of this response.

MR. GRIFFON: In part number four.

MR. HINNEFELD: In part number four, that also relates to the --

MS. BEHLING: I said this is the first time we've had a chance to read the responses.

DR. BEHLING: Are we prepared to go to the third

item?

MS. BEHLING: They want to know if we're in agreement.

MR. GRIFFON: Is there agreement on number two?

DR. BEHLING: Yes.

MR. GRIFFON: Okay, then go to the third item.

DR. BEHLING: Again, this is a relatively easy one to resolve. The issue here is one of the implementation guide provides inadequate guidance for classifying a case as potentially less than or greater than 50 percent POC and should identify the role of the Task 2 personnel. When I first looked at, and I looked at, obviously, the regulations and they clearly spelled out that there'll be different tiers of dose reconstruction based on efficiency, the need for efficiency and so forth. And that the question I had in reading the implementation guide, it doesn't really offer you the opportunity to say how will we differentiate a maximized dose that is likely to be less than 50 percent from one where best estimates apply and the implementation guide didn't address it. Obviously, the procedure number six clearly identifies that in a series of appendices. So

again, this is an issue that I identified because the implementation guide was the very first document I reviewed. And had I had the benefit of seeing everything up front, I probably would have deleted that as an issue. So it's a question of learning things as you go along, and I don't think this really requires any --

MR. GRIFFON: There's agreement there, okay. Number four.

DR. BEHLING: Number four, again, we're going back to TLD uncertainty, and I just took a couple issues here. For instance, in the TLD uncertainty it defines an equation for mu sub n and mu sub e, or something that should be obtained from your local DOE, DOELAP-accredited health physicist. And I found that, first of all, two problems with that. It's not something that you should call somebody who was involved in DOELAP accreditation to get the answer to. And second, if you're talking about DOELAP accreditation for a TLD that was done in the early 1990s, how is that applied to a TLD system that was used in the '70s, and it's questionable. So that's really where the issue is here. One of discrepancy in terms of time and the availability

of data that should be made available in the procedures so that people wouldn't have to necessarily consult somebody within a DOE complex in arriving at variables that are necessary for defining uncertainty. So it goes to the same issue we addressed earlier.

MR. GRIFFON: In your response, NIOSH's response, it seems like this is more than simply a matter of modifying language.

MR. HINNEFELD: Well now, in this case I believe it is. I mean, there's, the discussion that is in the implementation guide, you know, the offending passages, don't really provide direction on how to accomplish anything. And so from our standpoint, you know, we have a pretty standard approach to uncertainty on these measurements, and that's used typically in each of the site profiles or in one way or another will describe what the uncertainty approach should be on measured doses, and we have a basis for that.

And so the IG should write that basis not this various other conversation about potential ways to do uncertainty. And so to my way of thinking this is really a language change, but the IG

should describe the basis for what we're doing in uncertainty rather than these other passages about uncertainty that don't, that really don't tell us what to do for what we're doing.

MR. GRIFFON: Do you agree with that response?

DR. BEHLING: Yes.

MS. BEHLING: Number five of the implementation guide indicates that what we're just suggesting here that the LOD values, I believe the implementation guide had suggested a ten milligram in the early, well, '56 through '60 as an LOD value. And based on other technical basis documents and other references that we looked at, it just appeared to us that that was a low value. And I guess in retrospect, as I said, when we started doing the dose reconstruction reports we realized that they often do use the TIB 008 and the TIB 0010 which does recommend the 40 millirem. But it just seemed that there was a little bit of a discrepancy with the implementation guide in the fact that these early years were just such lower LOD values such as ten millirem as opposed to 40 as a minimum.

DR. BEHLING: And just for clarification, Table 2.1 of the implementation guide provides LOD

values for '56 through 1960 that went from 30 millirem to ten millirem. And I have a difficult time being part of, or having been part of the dosimetry program at various locations that as early as 1960 you would find an LOD value for a film at ten millirem. And again, it's inconsistent with TBD values that identify site-specific values. And for that time period usually you end up with 40 millirem as the standard LOD value that is used throughout the complex. So it's just a statement here.
As I said most people when they go to LOD values from this dose, whether it's LOD over two or just N times LOD, they usually cite site-specific information which makes this particular statement in the implementation guide a question we should --

MR. HINNEFELD: Right, I think from both finding number five and finding number 6, I believe their, the intent of the passages in the IG are to provide illustrative, you know, an illustration of a concept as opposed to a recommended value for LOD. So it's the, you know, in one part it illustrates this is the effect of a lower limit of detection or a less

frequent badge exchange on the missed dose number. And so I think these are both sort of examples or illustrations, but without the intent that the actual LOD values are recommended values to be used.

MR. GRIFFON: And I think to me it does sort of read that way. I mean, it's a Rocky Flats, you know. It actually says in one paragraph before the table, at least for the one on page 31, so you know.

MS. MUNN: This is one that I'd like it if you really emphasized, illustrate, in your response there, Stu. I think as I read it if the word illustrate jumps out at me then it's very clear to me, and I think even to any casual reader it would be clear that you're not, even though you say later, not recommending. A little emphasis on illustrate would be appreciated.

MR. HINNEFELD: Okay, in the language of the IG?

MS. MUNN: Yeah.

DR. BEHLING: The next one is just a statement that --

MR. GRIFFON: You agree with that five, right?

DR. BEHLING: Right, right.

The next is just a statement that refers to LOD

values given in the implementation guide as being one and the same whether it's for photons and for deep dose photons as well as for the shallow dose which may be low energy photons or betas. And then I guess for anyone who's familiar with the dosimetry system that's not necessarily the case. We usually assume that the degree of sensitivity for shallow dose is not quite up to that level of the deep dose and that's really a comment. But I think you address, your response is acceptable as it stands.

MR. GRIFFON: Okay.

DR. BEHLING: The next one is finding number seven and it deals with the NTA film dosimeters, the limitations as defined by their response. And again, the implementation guide like so many of the other TBDs identify 500 keV as a threshold for being able to detect neutrons and producing a track that is observable under a light microscope.

And I looked at the early documents including information that's contained in some of the classic textbooks like Hine and Brownell, and they identify something that is considerably higher, 800 maybe to 1,000 keV which would be a

threshold that would expand the area of uncertainty for various neutron spectra associated with reactor sites. And so that's the reason I brought it up here. And it's also an issue that needs to be somehow other explained. Even if we accept a certain threshold, that doesn't mean that once you cross that threshold that your ability to observe an exposure is the same. There's a steep dose response gradient between 500, even if it is, in fact, something that on occasion you may be able to observe a track that is a result of a 500 keV neutron. The likelihood of seeing that in the terms of dose response and put it on equal footing for tracks that may be generated by neutrons between one and two MeV is considerably different. And that's really not always clearly stated. In fact, the casual observer would read that if you exceed 500 then everything is on an equal level. It is not. And I think in so many of the TBDs, including in the Savannah River site, you do show the steep dose response gradient that's above the threshold level and that let's you know that you're really operating in the dark.

And of course, the whole issue also centers

around the use of NTA film. At least for many of the facilities, NTA film has been viewed with skeptical, with a skeptical perception of how accurate is it, and therefore, completely ignored in favor of neutron-photon ratios. And so again, the question comes in why even bother for those facilities where NTA film has been used but acknowledged as unreliable, and therefore, the surrogate methodology involving neutron-photon ratio was adopted.

And I have no comments beside that other than in so many of the TBDs the issue is resolved by saying we're not even going to use it. We're going to use something that is more, obviously more practical.

MR. HINNEFELD: I don't disagree with anything you said.

MR. GRIFFON: So the only clarification I want is on your response it says interpretation of the NTA film dosimetry results probably require site-specific evaluation. I mean, is that, what is happening? It's all individual site-specific?

MR. HINNEFELD: Well, the TBDs will generally describe like Hans was saying, some of the TBDs say NTA film was used before this date because of

the nature of the spectrum they were exposed to. We're not even going to worry about it because we don't think it was effective at all, and so we're just going to use neutron to photon ratios for that period of time, so --

MR. GRIFFON: Maybe the guide should give that clarification, too, that --

MR. HINNEFELD: I think we could make an edit to IG-001 to make it more clear that we don't necessarily think 500 is a magic threshold and once you hit 500 everything's hunky dory. I mean, we could say that. I would make that kind of a low priority edit because we're behaving, by writing site-specific TBDs that address it for that site the way that we think we should behave anyway, I think, is what I believe I'm hearing. And so I would make that edit IG-001 relatively low priority because we seem to be, you know, it's going --

MR. GRIFFON: Probably a low priority, but it would be nice if it was consistent with the practices down through the chain. Yes, I agree. You agree?

DR. BEHLING: (no audible response)

MR. GRIFFON: Okay, next.

DR. BEHLING: The next one is number eight, and a summary of this issue here is methods for a reconstruction of neutron doses from survey data or source term data do not appear practical, achievable and defensible. In a sense what I'm really saying is that Appendix B has a huge citation of neutrons' fluence and their conversion, and quite honestly, we all know that when we go into a facility based on time and space, time and space, the dose rate from neutrons is highly variable.

And the option of even calculating an exposure for a person who may have worked there for years without knowing where he was, when he was there, how long he was there from neutron fluence, while it has some theoretical merit in discussing, has no practical value in dose reconstruction. And I would venture to say at this point we will never see an instance where somebody's going to be or where neutron dose reconstruction will take place with regard to a neutron fluence assessment or going through the motions as defined in Appendix B.

Now the exception to that might be a person who works in a calibration laboratory where he has a

mono or a fixed source that produces neutrons of a certain energy spectrum, and we can at least, you know, under the most extreme conditions if the person wasn't monitored, reconstruct it using a bounding value. That would be the exception I would take. But for a person working in a reactor facility I would say the use of that whole process would be an ambitious process to say the very least.

MR. HINNEFELD: I agree. I'm proposing we change the wording to more accurately describe what we do.

MS. MUNN: I would point out -- this is Wanda -- that we very likely may have such an instance as Hans referred to with respect to laboratories and sources and a variety of folks. We have an SEC that probably is coming up, it will be almost precisely what you were talking about.

DR. BEHLING: As I've said, I'm not exempting all conditions from the use of that methodology but at least for a large category of workers in and around reactors whether it's at Hanford or at Savannah River. I would say this is a very ambitious approach to doing dose reconstruction, and I don't believe for a moment people would

actually choose to use that methodology.
If everyone agrees, we can go to the next issue number nine, and I guess the center of that particular statement is that at most facilities neutron exposures were generally less than 20 percent of photon exposures. Now we do know that that's not necessarily the case, but again, it is not likely that this particular statement will be used for a dose reconstruction. It was, therefore, a statement that is generically a statement that may or may not necessarily be true.
But for real dose construction people would, in fact, go to the TBD that defines the particular facility and look at the various locations in defining what the neutron-to-photon dose ratios are as are clearly defined in all the TBDs along with the ICRP correction factors, et cetera. So again, it's just a statement that I'm not sure it serves a purpose here in giving people the illusion that neutrons are always less than 20 percent of photon doses.
MR. GRIFFON: And there's agreement.
MS. MUNN: Can we just simply take out 20 percent?

DR. BEHLING: Yeah, yeah.
Yeah, the next one is issue number 10, and it refers to Appendix B DCFs for bone surface and red marrow. And again, if I look at some of the earlier work, especially for low energy photons where the photoelectric interaction dominates, the fact is when you have, whether it's an AP geometry exposure, the instant photons when they go through the skin and adipose tissue and the muscle tissue and finally strike the bone, there is a much increased probability of an interaction by means of the photoelectric effect which raises at the interface the actual dose of the bone surface by a considerable margin, up to, at the point of transition between soft tissue and mineralized bone you will see a steep gradient in terms of the actual dose, which for certain types of bone cancers would apply here in terms of the DCF.
In fact, I brought with me some of the actual -- and I cited Hine and Brownell for that. And I brought the original document as photocopied from Hine and Brownell with me. You will see instances where the actual bone surface dose far exceeds the entrance dose. And so that was not

brought out if I look at the DCF in Appendix B for the low energy photons, the DCF does not reflect a much enhanced dose that involves the photoelectric interaction at lower energy photons. Again, this is something that you may want to look at or correct.

MR. HINNEFELD: Yeah, this is one that I think we want to study and probably talk about some more after today because I'm not sure we're going to be able to reach a resolution today. ICRP 74 does talk about electronic equilibrium in those bone surface dose part at least, not necessarily in the marrow dose part, but in the bone surface dose. And they describe, you know, the electronic equilibrium and all, the nature of the comment you brought up and in the document that is the basis for the DCFs that we used.

So our belief is that ICRP probably was aware and incorporated it appropriately as they describe in their finding. But you know, we're just kind of sorting through that now, and I think we'll need to exchange some more messages about a final outcome here. You know, our comment here is that, you know, ICRP, we consider that pretty definitive.

DR. BEHLING: I agree with you. I looked at ICRP, and they do acknowledge it but don't do anything about it. And saying no, we're not going to address that as an issue.

MR. HINNEFELD: Well, in the bone surfaces they say that we're going to call it bone surface like average of the bone dose or something like that so it's covered. But they don't, they don't say anything about it in the bone marrow dose which is also, and the effect on the bone marrow is going to be a function of the size of the cavity that the bone marrow resides in.

And so the key question then, essentially, what is the average size of the cavity that bone marrow resides in because all these doses are average anyway. So what's the average size and that would be the extent of the effect. So I think we'll take a little more time to look at it, and then we'll talk to you about --

DR. BEHLING: It's a requirement that may or may not be something that is significant here. I brought it up because the early work, and I'm very familiar with it, with the Spiers' work in '49. And then again it goes back to what Stu was just mentioning, electron equilibrium.

And when you talk about a marrow cavity that's less than ten microns in diameter, the electrons generated in the bone matrix is what liberates its energy in the cavity itself. And therefore, for a very small cavity you essentially have an electron equilibrium value that is similar to that of mineralized bone as opposed to soft tissue.

If the marrow cavity is very large, it reaches again an equilibrium that is one of soft tissue and you average out the dose over a hundred, two hundred micron cavity that, in effect, becomes an average dose to soft tissue as opposed to mineralized bone. It's a moot issue. It's a small issue. I'm not sure if you want to address it, but I've raised it as an issue because I happen to know Spiers' work.

Item number 11, implementation guide does not account for additional laboratory uncertainty for film badge readings associated with exposures less than 200 millirem. And I think in looking at the National Research Council's report, they do address that as a separate issue in saying that the uncertainty is much higher for low energy photons. But on the basis of certain

considerations that NIOSH gave to this issue decided that it was not something that they were going to address. And then I just raised it because again, I'm quite familiar with the National Research Council's report, '89 report, on film badge dosimetry and the uncertainties as discussed in that report. And I just raised it here as an issue, but they addressed it as an uncertainty and NIOSH decided not to.

MR. HINNEFELD: Yeah, and this will be part of the rewrite of the uncertainty part. There are several uncertainty sections of IG-001 that are commented on and our general rewrite of our uncertainty language needs to address this as well. And there's a sentence in the response to IG-001, finding number 16, the last sentence there really I think is relevant to this comment rather than 16 when you're talking about the NAS's, NRC NAS's additional uncertainty at low energy.

So it's actually the additional uncertainty because of it's a field badge versus a laboratory badge, and it is most prominent. And these effects are more prominent. The higher uncertainty is more prominent in lower doses.

It's part of that whole, everything we have to address and rewrite the uncertainty part.

DR. BEHLING: In fact, there were a couple minor errors associated with the discussion of uncertainty where they referred to environmental uncertainty, and they ended up deferring to environmental exposure as environmental uncertainty in the context of the NRC report. Environmental uncertainty involves issues such as heat, humidity and other physical and chemical potential issues that may affect the performance of a badge. And it does not involve the term environmental dose as we define it in the implementation guide. And so it was just a mix-up of sorts.

The next one is obviously a very important one, at least from my point of view, and I believe NIOSH is going to look at this. The issue centers around the dose conversion factors as defined in Appendix B or Attachment A in PROC-006 which are identical.

I looked at those and obviously there are some problems here with defining how to convert a reading from a film dosimeter or a TLD into an organ dose. The assumption based on the

implementation guide is that we all start out with an air dose, which is not correct, in free air nevertheless. When, in fact, the readings that we're going to be starting out with are readings that involve either a film or TLD badge that's worn, meaning that it has also, is subject to attenuation by the human body and other factors that will obviously have some profound impacts in converting a dose, for instance, in a PA geometry, isotropic and rotational. And I assume, based on the comments that NIOSH submitted, that some amendments will be made. A reasonably quick and dirty one would be to resort to AP geometry as a dose conversion factor for all geometries independent of what you might think they should be.

MR. HINNEFELD: Well, and we're actually haven't been able to convince ourselves that that is the most claimant favorable in all conditions. We've done that much work to recognize it. Our first thought was let's just use AP, you know, no matter what we use, AP geometry does correction factors. And we're not entirely sure that that is claimant favorable in all conditions.

DR. BEHLING: Not always.

MR. HINNEFELD: Right, so we've got some work underway, but it's very preliminary, and I'm really not prepared to talk about it at any length except to say that, yeah, this is certainly a valid comment, and we are pursuing edits to resolve it.

DR. BEHLING: I think we can already address the issue 13 in conjunction with 12, so the two of them come together so we'll skip 13.

Item number 14 is angular sensitivity not accounted for in correcting measured film or TLD. Again, I went back to some of my own studies early on in my career. I also looked at Hine and Brownell. And clearly, film dosimeters as well as TLD are normally calibrated in a laboratory under the most ideal conditions, meaning that you have an instant beam of radiation that is at right angles to the face of the badge, and that obviously gives you the maximum response in most instances.

On the other hand reality dictates that when you look at a TLD that's worn by an individual in a radiologic environment, even if it's a single point source, he will rotate through his own axis, 360 degrees, over a period of a week, a

month or however long the badge is worn, and you realize that the exposure is not one of ideal exposure conditions. Under those circumstances when you look at, for instance, low energy photons, especially those that are heavily impacted by the 1,000 milligram filter that overlies the sensitive portion of the TLD or film, that you would potentially underestimate. And some of the underestimates are fairly substantial, especially when you approach the 90 degree or 180 degree. And so I raised it up, I raised that as an issue. I provided some data with that from Hine and Brownell that identifies the dose response in various angles which are substantially less than unity when compared to the 90 degree on-face exposure.

I did look at Fix, by the way, who is a very, very knowledgeable person and did an awful lot of the work on behalf of the Hanford site as well as other facilities. And one of the things that did bother me a little bit about him -- and I do respect his knowledge. He's a very, very intelligent person without ever having met him but reading his documents. He's clearly an expert on dosimetry. But he does make a point

that when you have a film badge that is irradiated at a 90 degree angle that the processor would instantly recognize that.

Well, that's true if you're going to be giving somebody a film badge and says you will not move from 90 degree to this one source. The truth of the matter is when you have a film or a TLD in an environment that has either multiple sources or you just walk around, you're going to have only a portion of the exposure that will impact a 90 degree, a 180 degree. Which means that this will not be recognized, and the very, very low values at those extreme angles are masked by exposures of angles other than those.

So I take exception to his comments that you can ignore the response at these extreme angles because the processors would recognize it. Well, that's kind of ludicrous because no one's going to be exposed for a period of a week, a month, or even a quarter at exactly 90 degrees which would reveal the exact angle of exposure.

And that's the only comment I have because we talked about the issue of angles sensitivity before, and I guess NIOSH questioned the value cited in Hine and Brownell as perhaps being

extreme. And I would just caution you that Fix's assessment of the 180 and 90 degree angles are not legitimate in the real world where people are going to be exposed not just at 90 or 180, but at all angles. And the ability to discern whether or not a certain portion of the exposure was received at those extreme angles is lost and would not be known to the person who's processing these films.

MR. HINNEFELD: Well, we're including this angular dependence of the badge in the product that we described in the last two.

DR. NETON: I agree with you. I think the range of error that could be made was cited at some of those extreme angles in your own review report, so you're, the magnitude of the error was asserted at those extreme angles implying that the error could be that large because a person was indeed exposed at those extreme angles. So we're just trying to respond in kind.

DR. BEHLING: Yeah, if you were to use an aggregate which would essentially be represented by a person who is on a rotational exposure geometry, it could prove to be that for at least very low energy photons which are most effective,

and clearly, as Hine and Brownell data and Fix's data show, he used 70 and 150 keV or 120 keV photons, they can be up to 30, 40 percent.

DR. NETON: That was our point. These extreme examples where you -- could be a factor of four or whatever.

DR. BEHLING: Yes, yes, no, I agree. I mean, I wouldn't use, for instance, an interdependence as cited by Hine and Brownell of 0.12 at the extreme end or edge of the low sensitivity. Of course, that would not be appropriate. But something on the order of 30, 40 percent as an average value for all angles that would be essentially representative of rotational geometry is not unreasonable as an uncertainty component.

MR. GRIFFON: Let me just ask, you said that, I mean, this, unlike some of the other ones, the earlier ones, that you said were not priority changes, this seems like it might be a higher priority. Is there any sense of how, what kind of timeline we're looking at or how many, could this affect cases that have already been, the geometry's completed for?

MR. HINNEFELD: I really hesitate to give a time frame today because like I said, we have some

very preliminary products, but to be honest with you, I haven't even read them all yet. But I'm not ready to decide --

MR. GRIFFON: But it's a higher priority.

MR. HINNEFELD: Oh, yeah, when I'm talking about things I think I consider low priority edit, that's like things I'll get to when I have time, you know, if I get to them. There are probably even three classifications you could put in here. This is really an important one. You know, resolving this issue and getting a correct answer or correct number down is an important response here. You know, there are some others that may fall in a middling category like the uncertainty where, you know, we're already behaving appropriately in uncertainty. That's kind of a middling sort of thing. But this is an important one and I think will resolve.

MR. GRIFFON: I was thinking the same thing. The uncertainty kind of fell in the middle, so this one was a higher one. Okay, just getting the sense of --

DR. NETON: I'd just like to point out that if we increase the uncertainty on the external doses, it's not likely to affect many decisions because

the uncertainty in the external doses probably is the smallest component of the uncertainty in all of these calculations even if we doubled the uncertainty or more. So it really, it has most impact when we actually change the estimate of central tendency as opposed to increasing the uncertainty bands really is not going to change much at all in the decision-making process.

MS. BEHLING: The other issue with this, with the DCFs being an important issue as you just mentioned, Mark, is the fact that as you said for most of these min-max cases we are using the AP geometry. But when we are looking at the best estimates, and we're looking at doing these using the workbooks, then a lot of DCF information does come into play. And so, as you said, it does have to be a higher priority.

DR. BEHLING: If we can go on to item 15. It deals with backscatter, and again, I can't speak on behalf of everything but, or all the TBDs, but at least on behalf of the Savannah River site for one, it's identified that on-phantom calibration started in the mid-1980s and there's a correction factor. And I believe they used something like 11 point something, three decimal points,

whatever, and I looked at the issue of the backscatter.

And it's clearly a very complex issue. It's energy dependent for sure. It's going to be a function of the scattering medium and the physical dimensions. And I provided some information that comes out of Hine and Brownell again in our report. It's on page 47, figure 2.1-6. And you see that you can receive backscatter factor for a very large person and for certain types of energies that are up to 40 percent. And again, the question is is the 11 percent a conservative, claimant favorable adjustment factor for on-phantom calibration, and that's the only reason I raised it.

MR. HINNEFELD: Well, the 11 percent was actually a combination of factors. There were a number of things that changed that year and not only the on calibration phantom. I think calibration energy changed, and so that was actually an evaluation that was done by Savannah River, I believe. In terms of maybe our doses, our recorded doses, should be adjusted by that much which we've adopted. But it was a combination of factors and not strictly, not only changing to on-phantom

calibration.
So I think -- now Jim, if I say something wrong here, help me out -- but I think before if a facility's using a free-in-air calibration, our approach would be that well, that, they're then calibrating with an exposure measure not a rem measure and so the dose conversion factor that should be used is the exposure to HP 10 dose conversion factor. I think that's generally how we would deal with on air calibration or in air calibration versus an on-phantom calibration.
DR. NETON: Right, if the phantom were present, it would backscatter into the badge. You would have a higher result.
DR. BEHLING: Yes.
DR. NETON: And so it would be under, it would be conservative to not have that included in the phantom calibration than if it were worn on another person's badge. So it would be claimant favorable.
DR. BEHLING: There is some limitations with regard to backscatter since most of the film and TLD actually had a filter on the backside as well. So your 180 degree backscatter photons, especially from low energy photons, would in

essence probably not even penetrate the backside of your film, so it's just a side issue.

I guess we can leave it at this. I just thought that the 11 percent adjustment factor for pre-1985 for in the case of Savannah may not necessarily be the most claimant favorable, but if there are data that suggests it was done on the basis of empirical measurements, I will accept that.

MR. HINNEFELD: I'm sort of speaking from memory on that, but I know there were a number of things that changed. It wasn't strictly an on, switching from free-in-air to on-phantom. That change occurred, but there were other things that changed that year as well.

DR. NETON: Right, there was a fairly systematic review done by Savannah River. We could produce that, I think, if need be.

DR. BEHLING: Item number 16, I think we've already discussed. That is the environmental uncertainty that is the result of physical and chemical factors such as heat, humidity, light, et cetera. It was not addressed in the implementation guide. On the other hand we've already discussed that in a couple of previous

issues as a part of a missing discussion and involving uncertainty that includes radiological such as the angle sensitivity or environmental uncertainty that was not discussed in the implementation guide.

Item number 17, I'm going to have to, I'm drawing a blank here. The issues as cited in the matrix, I'd have to probably go back to the actual report. It states guidance for selection of uncertainty distributions for total organ dose raises question of consistency and requires professional judgment. And I'm trying to figure out what the purpose of that was or what the basis was.

MR. GRIFFON: I guess the NIOSH response is the key here, too.

DR. BEHLING: Okay, let me take a look. I haven't looked at that yet either.

DR. MAURO: Excuse me, Hans?

DR. BEHLING: Yes, sir.

DR. MAURO: Can you hear me?

DR. BEHLING: Yes, I can.

DR. MAURO: We're all, the people on the line are not, sorry, the connection went bad, and we can't really hear you although now that I'm talking to

you I can kind of hear you. But what's gone on the last five minutes, hardly were able to hear you and you're going in and out. My guess is we're experiencing the same thing we experienced the last time only that was the focus that time on Mallinckrodt. I don't know if there's anything you can do about what we've done like on Mallinckrodt. For some reason we were fine this morning, but right now we're having a very, very difficult time hearing anything.

DR. WADE: Well, you're very clear right now, John, so...

DR. MAURO: Lew, are you hearing them well?

DR. WADE: No, I had the same problem the last five minutes.

DR. MAURO: Okay, so just to let you know, I don't know if there's anything you can do about it over there at your end or something's changed, something's happening with the communication, but I got a call also from Kathy DeMers who also is having the same problem. She called me on my cell phone. If there's anything you can do about it great, otherwise we'll just try our best to listen in.

DR. WADE: Right, yes, please proceed.

DR. BEHLING: Yeah, I try very hard, in fact, I have a tendency to shout at times, and so hopefully by being very close to the mike this is not going to, you're not going to lose the signal here.

DR. MAURO: Yeah, that's a lot better. If you could keep doing that, that would be great.

DR. BEHLING: Okay, I'm going to have to tell Kathy that too because she has a tendency to shy away from the mike here.

MS. MUNN: But we had something on the line that was really creating a problem there for awhile. I think they went away whoever they were.

DR. MAURO: No, there's actually static. I mean, now that you're -- I'm sorry to interrupt again, but as long as you're close to the mike and speaking loudly, we can hear you over whatever that static is. It's almost like a continuous noise in the background like a wind. That's there, but when you, that showed up about five minutes ago, but as long as you speak, you know, directly into the microphone, we can hear you over that.

DR. BEHLING: We're still on number 17 here, and I'm really for the first time looking at NIOSH's

response, and again, the issue is consistency in defining uncertainty distributions for total organ doses. And their response is that OCAS will revise the uncertainty language in various sections of the implementation guide so that it reflects the basis for the uncertainty approaches utilized in the program.

I have to tell you right now I'm struggling to figure out what the issues were that I raised; what caused me to raise them.

MR. HINNEFELD: I think -- if I could. I think I can help you out, Hans. This is Stu. The IG language describes the compiled distribution of whatever quantity we're talking about could be fit with any of a variety of statistical packages, et cetera, et cetera. So it's kind of wide open. What do I do?

DR. BEHLING: Stu reminded me, and it's really in, I believe, section four of the implementation guide where you deal with the different distributions that all come together and then the use of the Monte Carlo analysis that aggregates all these different distributions that are a part of the IREP input code I take it, where you sort of look at the distribution and sort of say,

okay, what is the net effect of all these different distributions in terms of the uncertainties. So I do remember now what it was, and I guess their comments are appropriate here. It scared me actually when I read it and say are we going to have to do all this? I know my limitations.

MS. BEHLING: I actually -- can you hear me? I actually believe that was the last finding associated with the implementation guide-001. And I did make the statement on here that PROC-0006, ORAUT-PROC-0006, is very similar to it, in fact, follows the implementation guide in this exactly. And so our comments to PROC-0006 are reflected in the implementation guide findings. The only thing I do want to add with regard to PROC-0006, it appears that they make revisions to PROC-0006 where they're adding addendums that seem a little bit puzzling sometimes because I believe the last addendum that was added was what you referred to as Attachment E, and it's an addendum specific to Hanford.

And at sometimes I know it's confusing to us, and I'm sure it must be confusing to the dose reconstructors to have site-specific information

as an appendix or as an attachment to a generic-type procedure. It's very useful data that's there, but sometimes I wonder how all of the dose reconstructors if they're even aware that it's there.

MR. HINNEFELD: Well, I understand the concern. The project population is fairly static, you know, a few new people come on now and then. And the people that are here have been at it for awhile, and they know, they've figured it out by now. And when new people come on there's a fairly, you know, fairly extensive training session to point out the, and then they also work under someone's tutelage clearly during their first period.

So I understand the comment, and it probably relates to a kind of a hierarchy and something is going to come up a little bit about if, if you have a TIB that says you can do an overestimating technique like this, and a procedure that says you can do an overestimating technique like that. They're not exactly the same. Is there one prevalent over the other? Are there two options? You know, some of those things.

And it kind of relates to is there a hierarchy to

these things comment. And I guess I'm not really willing to commit to saying we're going to write a hierarchy right now, but I certainly understand the comment. I think it's worth some consideration and evaluation. So just saying that now, but I think that's what you're saying on this Attachment E type of thing.

MS. BEHLING: Yes.

MR. HINNEFELD: That similar kind of item, right?

DR. BEHLING: Kathy's comment was just a summary statement with regard to procedure number six which by the way is a very, very good procedure. And I have to say that there is a level of detail here that is very constructive to doing dose reconstruction. It's very, it amplifies a lot of things that are obviously not there in the implementation guide, and for good reasons because the implementation guide was basically a foundation for expressing all the other things that are part of the dose reconstruction process. So PROC-0006 is not necessarily a facsimile. It's an expansion of the implementation guide, and it's a very, very useful and well organized, structured document. But some of the issues such as a DCF are commonplace and so they do need to

be looked at in concert with changes that may affect the implementation guide.

MR. GRIFFON: Can I offer something maybe that will help us with efficiency here? I'm looking ahead OCAS-PR-003?

DR. BEHLING: Yes.

MR. GRIFFON: The next two pages cover that and the response seems to be for every one of them that you're going to rewrite, you're going to have a new procedure to replace this one. Is that correct?

MR. HINNEFELD: Well, chances are we'll just cancel it.

MR. GRIFFON: Just cancel it, okay.

MR. HINNEFELD: This procedure was written in September of 2002, and it provides a pretty decent general description of how dose reconstruction is done. But it sort of attempts to assign responsibilities and without, there were no organizations at the time to assign responsibilities to. In the interim those organizations have been set up, better procedural guidance has been provided. So we don't really see a need for it.

MR. GRIFFON: So the only thing I would say is

maybe we don’t have to go through these item by item. But is there another procedure that it makes more sense for us to follow up on, or rather a different set of procedures that would --

DR. BEHLING: My gut feeling is that --

MR. GRIFFON: -- this isn't really replaced by one procedure.

MR. HINNEFELD: No, no, it's --

MS. BEHLING: But I believe one of our comments was the fact that we didn't quite understand why this procedure was necessary. So I think, in fact, we do recommend that we didn't understand the duplication between the other procedures and this.

MR. GRIFFON: So maybe we can just move ahead.

DR. BEHLING: Yes, yes.

MR. GRIFFON: Two pages and start with --

MS. BEHLING: I was going to recommend that.

DR. BEHLING: If I didn't state it in my review, it probably was at least implied that we can do away with the procedure.

MR. HINNEFELD: You did state it in your review.

MS. BEHLING: You did.

DR. BEHLING: I guess we will then go to ORAUT-

OTIB-0010 which is a procedure that is aimed to maximize exposures for claims that are not likely to be compensable.

MS. BEHLING: Yes, can I make a comment here also, Mark? Something we may want to consider. Many of the comments that we are going to have on TIB-0010 also apply to TIB-0008. And so again for efficiency we may be taking care of two guidance documents there.

DR. BEHLING: And the difference between TIB-0008 and 0010 is one is geared towards maximizing exposures defined by film and the other one is for TLDs, but they run at parallel path and they are both used for the purpose of deriving maximized doses from non-compensable cases. And I think here is where we clearly have the benefit of looking at particular dose reconstructions that had been done at this point in time.

And as I said, we've done 60 and one of the most frequent issues that we've had to contend with in reviewing those particular dose reconstructions involved these two procedures. They have been consistently misinterpreted. And it took me probably several weeks to understand what the intent was here, too. And I will basically

summarize. Well, maybe we should go through each one of them.

The first one is the guidance lacking for how to treat missed dosimetry data in which the number of zero readings is fewer than 12 cycles. Again, if I look at the dose reconstruction reports that I've seen to date, they will frequently ignore hard copy data from the DOE that defines the frequency of dosimetry exchanges. They will actually default to the assumed number of 12 even though they may have only been monitored on a quarterly basis. And so the assumption is when you maximize doses, if you are claimant favorable, that's all that counts.

And to a certain extent I agree with it, but it sometimes is hard to say why would you necessarily give a person 12 missed doses when in fact he was only monitored quarterly and possibly even had positive responses during at least one or all of the quarterly doses.

But again, I've heard from Dr. Neton that for efficiency purposes we just, rather than even dwell on the issue, we will just give you the benefit of the doubt by using the maximum number of dosimeter cycles and give you the full measure

of the missed dose regardless of whether it was a zero or a positive response or even if the person was monitored at a frequency that is fewer than the 12 assumed in the two TIBs.

Again, this is a subjective issue. We've had discussions about overly generous assignments of doses for the simple reason that sometimes, you know, sure you avoid the arguments, and you couldn't possibly have gotten any more than we are going to assign you. On the other hand if a person ends up with a POC of 43 or 44 percent based on these overly excessive and generous assignments, he may feel that he came so close and he's not happy about it. You're dealing with a potential problem in a sense where the person feels that he came close but not close enough. On the other hand there's the issue where excessive generosity with dose assignments may lead to a future problem if the person develops another cancer, and we say now that that cancer has a higher probability of being compensated, we're going to have to take away all these doses that we assigned to you from your previous claim of cancer and restart from scratch, and now we're going to basically use a best estimate.

And so there are trade-offs in terms of what benefits there are to being excessively claimant favorable especially when there's no need for it. If I see a DOE document that says he was only monitored quarterly, it's very generous to give him four missed doses. And if you want to be excessively generous use N times LOD instead of dividing it by two. But it's unnecessary to be that generous where you assign missed doses for cycles that he didn't even, wasn't even assigned a dosimeter for.

So that's a general comment about both the use of TIB-0008 and 0010 is that they do have a prescriptive process in which the number of dosimeter cycles are essentially told to you in the, in a table format. And as I said, I think in the end if you have real data perhaps you should use the data instead of defaulting to a value that is just a generic value in a table.

MS. BEHLING: And I think that's especially true when process efficiency is not being impacted.

DR. BEHLING: Yes, I can clearly understand where if a person was monitored 52 times in a given year and the person has to go through and says well, in this week, the 23rd week of that year,

there was a positive dose so I'm going to take not 52 number of cycles but 51. You know, it's so much easier to say well, we'll just ignore the positive ones and give you 52 for every year. And I understand the logic behind it. At least one can say for efficiency purposes the blanket assumption that every dosimeter cycle has to be a missed dose, has a lot of merit, but it's not an efficient process, efficient. But it's not process efficient to ignore the obvious when, in fact, you have real data that says, no, he was not monitored 52 times, but he was only monitored maybe 12 times or even quarterly. That's the point.

The next issue that we have here, and again, is a consistent error that we've observed here is the --

MR. GRIFFON: Let's just hear from NIOSH on that one because I see a response, but well, basically that's a maximizing approach, and that's...

MR. HINNEFELD: Yeah, as a general rule we, a number of our dose reconstructions are overestimating, you know, there's an overestimating component to it, and we tend to try to avoid really high POC numbers with an

overestimating approach just for the reason that Hans alluded to, but I don't know that we really want to say well, we're going to stop doing overestimating approaches.

And I think it's kind of -- you know, I don't know what else to say about that. I think we want to retain that as (inaudible). Sometimes it's not hard to predict in every case what's efficient for this case and what isn't so we want to retain at least some flexibility to do, you know, to use an overestimating approach even if it means assuming more bad cycles than there were or seemed to be.

DR. BEHLING: It's an issue also of consistency because I think we all know that there's a possibility that people among the claimants, and there are so many of them, may compare notes and saying why are you getting all this assigned doses and I'm not. And it's due to the fact that two people interpreted the procedures differently. One is more likely to be excessively claimant favorable than another person which leads to inconsistency and unfair treatment perception.

MS. BEHLING: I also believe that PROC-0006

indicates that if you're using overestimating assumptions and your POC goes over 30?

DR. BEHLING: Yeah.

MS. BEHLING: Thirty percent that you're supposed to re-evaluate that. It's something that we don't always draw attention to because again, we're looking at the efficiency process. And we understand why it's not being done, but it does reinforce this statement in this particular case.

DR. BEHLING: And it would certainly help because a lot of these POCs of 40 some odd percent are driven by excessive use of generous assignments that in principle would be avoided if you did, in fact, apply the 30 percent rule that says, oh my god, you know, maybe we should be still claimant favorable but not necessarily that favorable where we end up with a 40-some-odd percent POC value. Based on PROC-0006 the assumption is that if you exceed 30 percent, you should actually introduce the best estimate methodology.

And that may not be necessary because oftentimes you can achieve less than 30 percent using still claimant favorable values but adhering to the rules that are defined by DOE document that says why give the guy 12 missed doses when four will

do, et cetera, et cetera. And so you can avoid the costly time issue of a best estimate by ratcheting down the claimant favorability aspect that oftentimes ends up with these 40 percent plus POC values that really shouldn't be there.

MR. HINNEFELD: Well, how about if I take a look at the language in TIB-0010 and see if there's something we can insert there to do that. And I also need to talk with what does that do on the actual reconstruction side. You know, changing, taking away a technique that's currently in place would be perturbation on the dose reconstructors and dose reconstruction process. And so before I say well, okay, we'll go take that away, I'd just rather, kind of like to know what the impact is on the dose reconstruction process. Because I mean, fundamentally, our position still is that if the case is less than 50 percent of causation theoretically, although we won't go as high as 50 percent, but if it's a less than 50 percent case, and you've got overestimating approaches in there, then it's done. It's by efficiency method.

DR. BEHLING: Well, then PROC-0006 needs to be revised because that statement of 30 percent --

MR. HINNEFELD: And I will check on that, too.
DR. BEHLING: -- is something that conflicts with that.
The second item for TIB-0010 is the issue of LOD and again here we have just a repetitive number of errors among the dose reconstructions that we audited to date. When you use N times LOD, that is the 95th percentile and at that point you do not have a parameter value, the default value generally being 1.52 as the geometric standard deviation.
And there is a tremendous amount of confusion in both TIB-0008 and 0010 on that very issue. And so if you multiply the number of cycles by the full LOD value, you are at the 95th percentile which exempts the need for uncertainty. And people just haven't gotten that idea. And it's just a simple rewrite of those two procedures.
MS. BEHLING: It's written well in PROC-0006.
MR. HINNEFELD: Okay.
MS. BEHLING: They don't make that mistake when they use the instruction, desk reference instructions.
DR. BEHLING: Again, the next item three for TIB-0010 is basically what we discussed very early on

as our first issue. I'll just read it. The document contains too much upfront background information and does not provide the dose reconstructor with guidance for maximizing external dose until page eight.

And I think again, I'm speaking on behalf of both TIB-0008 and 0010. You go through an awful lot of information and digest information that serves really no purpose. It's really the final table that says for recorded dose, multiply times two. That's really the gist of it. Multiply times two and again, avoid the need for uncertainty. For missed dose use LOD times N. That's it. That's really what the whole procedure tells you to do. And it is something that you don't find out until you get to the bigger end on page eight, or the very last.

And again, it's not an efficient way to write a procedure. I would have liked to have seen an up front table that says this document is intended to maximize doses. Here's how you do it for a recorded dose and missed dose. And if you want to hear why we're doing it this way keep reading, but give the reconstructor the chance to use the information up front rather than force him to go

through eight pages of background information before he gets to the point where he has to really make use of some information for dose reconstruction.

MS. BEHLING: We believe also, based on your discussion here, we also covered finding number four on TIB-0003 and four.

MR. GRIFFON: Yes, yes.

DR. BEHLING: The next one I think we've also pointed out. Well, when we talk about missed dose, we usually talk about, or at least the procedures whether it's the implementation guide or individual procedures, always talk about missed doses being a recorded zero dose. But the truth is there are oftentimes recorded doses as little as one millirem when, in fact, they coincide with a period where the associated LOD value is cited as 40 millirem which means that a person might look at this under some circumstances. Well, this is a positive dose. I'll give you one millirem. When in fact the guy would have been better off having had a zero dose. In which case he would have gotten 40 millirem over two at 20, or in some cases, just 40 millirem. And so there's a need to identify

missed dose in two parameters. A missed dose is anything that is less than LOD over two or LOD depending on which methodology you use.

Again, I think that the next one, item six is something we've already discussed. The standard correction factor of two eliminates the need for uncertainty and that was already discussed.

Number seven, I'm not sure. Let me see what you responded to here. Item number seven says guidance provided in TIB-0010 differs from instructions in Section 5.0 of ORAUT-PROC-0006.

MS. BEHLING: PROC-0006 does not recommend standard correction factors, so I think we are questioning the inconsistencies.

MR. GRIFFON: Is there a discrepancy in --

DR. BEHLING: Well, no, I'm just reading now again for the first time NIOSH's response. And I guess they agree. You know, I have to read it here. But they basically state that there are some discrepancies that need to be corrected.

MR. GRIFFON: Yeah, yeah, yours --

MR. HINNEFELD: I think we'll take a look at those. It kind of fits where earlier we said we need to look at the language and procedures set against this TIB, and also TIB-0008 probably, and

see these various language inconsistent portions.

MR. GRIFFON: So you may amend your response by saying both procedures are overestimates but should be looked at for consistency, right?

MR. HINNEFELD: Yeah.

MR. GRIFFON: Go ahead.

MR. ALLEN: Well, basically these are two options for overestimating. There's no real reason why we have to have one, and only one method, for overestimating. Is that what you're trying to say in this comment?

DR. BEHLING: I'm reading; I'm sorry.

MR. GRIFFON: I guess if there's two options then the PROC and the TIB should indicate both options maybe. Is that --

DR. BEHLING: Yeah, that is an issue that is raised in, I think, the next one, too, is that sometimes I'm confused about the hierarchy of documents, which dominates. Obviously, there's always, at least in my mind, the final conclusion that a TBD dominates everything. In other words if you have a site-specific document that says this is how you do it, you ignore everything else assuming that the issue is properly treated in that document.

But beyond that when I talk about or when I see generic procedures, and I will just briefly allude to one. I believe, for instance, in case of Savannah River, there are a couple TIBs there that deal with the missed tritium dose. And one will say oh, it's okay for a missed tritium. It's five microcuries per liter that translates to 375 millirem a year. And the other one says it's really based on a one microcurie per liter that we didn't bother recording. That's 71 millirem a year. And it's up to the individual to make a decision which I really don't think should be the case.

You either decide one or the other. And for consistency purposes you should at least identify a common, like if there are multiple procedures that treat the same subject, they should at least be consistent. Preferably they shouldn't be redundant in terms of procedural content anyway because, you know, people have enough documents to confer with in doing dose reconstruction. They don't need to have multiple documents that treat the same issue. And so the issue comes into play in terms of hierarchy. Where does the person go to say this procedure takes precedence

over the other procedure in defining what the dose shall be? And it's not always clear in my mind.

MS. BEHLING: And there are definitely inconsistencies depending on who the dose reconstructor is.

MR. HINNEFELD: I think probably our response has to be let's go sort out the language. And I know it's relevant to 0008, 0010, TIBs 0008, 0010 and PROC-0006. And see if we can come to some common understanding and approach that's not a particular perturbation on the dose reconstruction process.

DR. BEHLING: So I think we've covered item seven and eight. Number nine for TIB-0010 is a standard correction factor of ten. And I would say generally speaking that the standard correction factor of ten is one that covers an awful lot of uncertainty. The other -- the standing correction -- I have two, I'm sorry -- encompasses a tremendous amount of potential errors associated with the performance of a film badge or a TLD. But I wouldn't say it's necessarily excessively claimant favorable. I think it's reasonable to assume that a factor

of two may define the 95th percentile. Because when I look at that National Research Council report of '89, and you look at the recorded or best estimate value versus the 95th percentile, it's usually a factor of two apart. So if you want to consider the 95th percentile as being excessively claimant favorable, okay, but it is one that is within bounding values of real dosimeter performance.

And so that statement was really written in here to say that the factor of two is potentially a 95th percentile value that that may apply under some circumstances when even a maximized dose is not necessarily the approach, but could even apply to, under certain circumstances, to people who are best estimates may be usable.

MR. HINNEFELD: Okay, I want to talk about this one just a bit. The National Research Council study that you refer to, if I remember from your report, was the dosimetry of -- was it atomic veterans?

DR. BEHLING: Yes, yes, up to '62, and those are films that were used --

MR. HINNEFELD: So a particular troop of soldiers would be given film badges. They'd be marched

through the same terrain. They'd essentially have the same exposure conditions. And the 50th percentile of their badges, it could easily be half or the 95th percentile could easily be twice the 50th percentile, so that relates to the variability of a single badge, you know, episode like that.

So there's that much variability in badge. I think from our standpoint that the times two factor for the standard correction factor, you recall, is applied as the multiplier of two times every recorded value. So we think that since we're doing it times every recorded value, the likelihood that a specific individual would always be the low outlier on the distribution of badge and exposures, we think that the factor of two is relatively okay in the way it's being used since it's, to his entire recorded dose as opposed to a single incident where you might say well, yeah, it might be that much. But over the course of his career and every recorded dose, if you apply that factor of two, we think we're pretty good at saying that that's an overestimate. So that would be our --

DR. BEHLING: Yeah, I agree with the idea that a

factor of two certainly brackets the potential exposure.

MR. HINNEFELD: Okay.

DR. BEHLING: But I guess the exception I took is the wording of it in saying that this is excessively, it's probably in some instances within the range of performance of the, especially early film dosimeters.

MR. HINNEFELD: Okay, we'll take a look at the wording.

DR. BEHLING: Yeah, I mean, nothing needs to be changed other than the reference to the fact that this is way outside the scope of reality. It's probably not. It's probably in many instances within the range of a dosimeter's performance, the error band that says, you know, a factor of two may define a 95^{th} percentile value.

MR. GRIFFON: You can do this last one, and then I've been asked for a five minute comfort break after.

DR. BEHLING: The next one is item number ten, the use of a default LOD value of 40 millirems should be considered a typical value as opposed to a highly conservative value. Again, I looked at the NRC report which defines that as a typical

value as you know if you look at the, I guess it's one of the appendices to that report that says 40 millirem is really a typical value for an LOD.

Again, I haven't looked at the raw data involved in the various facilities that made use of the dose film. But considering the fact that the military personnel in the Pacific or at Nevada Test Site, probably were given the same DuPont fiber 2-5-10 badges. What applies there is likely to be applicable to the energy employees. And so I consider 40 millirem perhaps as a typical value as opposed to the highly conservative value as an LOD.

MS. BEHLING: Again, it's just a wording issue.

DR. BEHLING: Just the wording.

With that I assume, Mark, you would like to take a break.

MR. GRIFFON: Yeah, why don't we take a, just a, and let's keep it to five minutes because I know a lot of people have planes. I know I have a plane to catch so let's keep it to five so we can get through the rest of these. We're making headway.

(Thereupon, a break was

taken from 3:30 to 3:37 p.m. after which the following transpired:)

MR. GRIFFON: Okay, reconvening now. Hope everyone's on the phone that needs to be on the phone.

One thing I wanted to bring up just in between, we're going from OTIB-0010 to 0008. I had a question in my mind was do either one of these documents -- and I can't remember for the life of me -- but do either one of these describe the procedure used for unmonitored periods as opposed to missed dose? Is there anything about unmonitored dose? Any guidance on that?

And I'm thinking of a situation where you might have many cycles of dose records with positive readings, you know, 100 millirem, 300 millirem, and then you have gaps in the data. Are they always treated as just, you know, LOD values or are they actually unmonitored and treated differently? Treated as an average of, you know, is there any guidance? Is that in here? Is that in other guidance?

DR. BEHLING: Well, I can answer that. I think if you looked at implementation guides they give

you various options. If for instance you have a person who has an exposure track record for a period of time and then there's a gap, and he resumes again with an exposure period, one can use interpolation between the two. And assuming that everything else being equal, one could certainly look at that gap and say, well, during this period and the period that follows we just simply linearly extrapolate during the period and assume that that's a reasonable approach. There are other approaches for dealing with unmonitored or missed periods such as using administrative dose limits as an approach.

MR. GRIFFON: I agree. Different approaches were outlined in the implementation guide. But that's a broad guidance document. I was wondering if there's any more specific --

DR. BEHLING: No.

MR. HINNEFELD: Not in 0008 or 0010.

DR. TOOHEY: No, they're not in 0008 or 0010. They tend to be more in the site profiles. Or the other option we've got now since we've completed coworker data distributions for the major sites is assigning that distribution for unmonitored gaps.

MS. BEHLING: But what we're actually seeing in the dose reconstructions, and I don't believe I've seen anything other than this, is they're just treating those gaps as if they were zero, and they feel that that's being, that's an overestimate. That's based on what we've seen on the audits.

DR. NETON: That shouldn't be the case. If it was unmonitored, truly unmonitored exposure should not be treated as the L-O, as missed dose unless there's some great justification for that.

MR. HINNEFELD: If there's reason to believe that the person was not monitored and was correctly not monitored. In other words there was a job title change that would make them look as if they moved into a job where they probably wouldn't have been monitored. If it has been done that the monitored worker missed dose would be applied as a bounding estimate for that, doing that.

MR. GRIFFON: I think there might have been other site-specific conditions, too, where blanks in the record, it was determined that those were actually red badges that were less than detectable. So I think that was a site-by-site issue. But it's not in the procedures. That's

the main issue. So we can address it another time.

DR. BEHLING: Neither 0008 or 0010 address the issue of unmonitored.

MR. GRIFFON: I'll let you continue. Didn't mean to sidetrack you.

DR. BEHLING: I think we're trying to expedite things so as to leave some time left for other discussion. So we're on TIB-0008, and again, we've already mentioned that TIB-0008 parallels 0010. So much of what we said about TIB-0010 applies to TIB-0008, and so I think we, if we agree we can skip TIB-0008 because it's essentially a parallel of TIB-0010.

That brings us to TIB-0007 and the first item here, and I think if I recall this deals with the issue of removing or eliminating environmental doses associated with badges that were stored perhaps in the areas that should have been reported as occupational. In other words there's EALER issue involving elevated ambient levels of radiation. There was a time in practice where control badges were used to subtract doses from the monitored badges.

And it turns out that perhaps the control badges

were stored in places that were not just natural background radiation, but actually occupational one. And I think if I recall, that particular procedure says we're going to continue this. This was an incomplete procedure at the time we reviewed it.

Am I correct, Stuart?

MR. HINNEFELD: Right, it was originally published with consideration of the practices at a selected number of sites with the idea that other sites would be addressed as they were evaluated. And realistically our feeling now is that when we evaluate each site, we'll write the site profile for the site. Let's just put that information in the site profile rather than keep this particular TIB around. So we're really going to consider whether we need to hang onto this one at all.

DR. BEHLING: Yeah. Again, the next issue here besides the EALER issue is one of the exposures defined usually in behalf of a deep dose. And of course we know that deep dose really may not be representative of a skin dose which is not only deep dose but perhaps a beta component or low energy photons. And so issue number three, OTIB-

0007, does not provide guidance for assessing shallow dose in cases involving skin cancer. Again, it is an issue we've raised beforehand, but it's confined to only those claims where skin cancer is the issue and environmental doses are, generally speaking, recorded as deep doses. And is there an issue here that needs to be looked at?

MR. HINNEFELD: Well, I think it folds into the desire to move this into site profiles because there'll be some sites we might have argon-41 potential exposures. Or you might have contaminated, you know, beta emitting contaminants and particular areas that are elevated to the point where it would be relevant. But that's kind of like a site-specific thing as opposed to a general approach. And so I think it's kind of a resolution to those kinds of issues fits with using the site-specific site profile or TBD information rather than a generic one and see if we can't move away from this in general.

DR. BEHLING: And I also have a question because I didn't really see any empirical data that would give me some understanding of what the magnitude

of exposure is that may have been subtracted from worn badges based on control badges being stored. Did you have a feel for whether this is really even a significant issue? I mean, were there some sites where the potential exposure that's been subtracted is a substantial part of a person's exposure?

MR. HINNEFELD: There might be an occasion where badges were stored in badge racks not far removed from a radiological area, and the one that comes to mind I believe it might be Idaho, there was some discussion. I don't know off the top of my head the final answer to, I don't remember that was discussed, but there might actually be control badges stored in badge racks that really weren't particularly isolated.

DR. NETON: Yeah, I think this all came up at the green runs at Hanford when there were some significant environmental, ambient environmental levels, you know, virtually plant wide at some point. And if you subtracted those values from the badge rack reading, my recollection is this could be in the realm of several hundred millirem. It's not significant, but it's certainly enough that would, you know, could put

somebody over the limit, I mean over the compensability limit if we weren't careful. But it's really isolated to a very select number of sites, pretty much early on.

DR. TOOHEY: Dick Toohey. I just really, it really is a site-specific issue. It was also an issue at Rocky Flats where it was alleged that badges were stored in high background areas. And as it turned out, they were.

DR. NETON: I think we put that one to bed.

DR. TOOHEY: Yeah, we did, but the point I'm trying to make is that's dealt with on a site-specific basis in the site profiles.

DR. BEHLING: Okay, so this procedure may, in fact, be scrapped? Is that what I'm hearing?

MR. HINNEFELD: Right, we're going to have to take a look and make sure if we don't hamper ourselves by doing that, but that would be our preference is not to keep this general. This was a general procedure issued early on with let's get the capability to do some dose reconstructions before all the site research is complete.

DR. BEHLING: Okay, so in that case I guess we're down to TIB-0006. I have to actually remind

myself what TIB-0006 is about. Is that the medical? Okay, that's the medical.

Again, the first item for -- yeah, this is Ron Kathren's medical occupational exposure generic document. I find it's at times used when in fact there's a TBD that has a separate section. And again, I would assume that when there is a TBD that has site-specific data that that should be used as opposed to the generic one. And yet I've routinely found people using the generic one when, in fact, there's a site-specific TBD that identifies exposure. So I guess I would recommend that the hierarchy of use of procedures favors the site-specific data as opposed to generic one.

But anyway this is obviously the one that's more generically used and the criticism or at least the statements that are offered under issue one is that the document is poorly structured and provides unnecessary background information and so forth and so forth. And it's really one of, again, giving the reader an awful lot of information to digest before going to the final tables that says here's what you use for identifying organ specific doses by a period of

time during which these x-ray machines were provided with additional filtration, et cetera. So the first thing is just nothing more than a comment about the structure and design of the particular TIB that is again one that involves an awful lot of background information that has little or no value for dose reconstruction. And as far as I'm concerned there's not much we can do at this point other than to accept the fact that if we had to do it over again perhaps we would restructure these procedures.

I've looked at the procedures in general. I think it's a well done procedure that has a lot of research behind it. The organ doses are well defined for various periods of time using obviously the state-of-the-art ICRP documents when organs were not necessarily identified, et cetera. So I believe the document stands as it is without any need for change other than the format.

MS. BEHLING: Just one additional issue on TIB-0006 is as the TBDs are being developed, one of the things we are finding is as we're comparing TIB-0006 to the TBDs, we're often seeing quite a discrepancy in some of the doses that are

reported. May be something to keep in mind as the TIBs make a comparison. Because if you use TIB-0006 as opposed to the site profile, that can often make a fairly significant difference.

DR. BEHLING: Yeah, and sometimes it's not necessarily something that I fully understand. For instance, assumptions regarding photofluorographic procedures, I think in one the total number of frames are five and they correspond to dose, especially to the issues, to organs in the primary beam, something in the order of three rem. And there are other site-specific tables, and I forget which one in particular, where the dose is fully a factor of ten less.

And again the question is why the difference? If the procedure, generally speaking, should have been somewhat common and for the same period of time should have resulted in similar doses. And a factor of ten is hard to explain. So again, consistency is an issue here specifically for photofluorographic procedures which is really item two for TIB-0006.

DR. MAURO: Say Hans? This is John Mauro. One of our -- and I know that it's not on your list,

but it may be something that's worth bringing up at this point in time is when we were doing a site-specific review of a site profile, one of the site profiles. It might have been Idaho. In the process of reviewing the medical exposure procedure in looking at TIB-0006 and how it's implemented, one of the commenters had indicated that the uncertainty that I believe has been adopted in TIB-0006 for the standard x-rays, or chest x-rays, I believe is around 30 percent. And the point that was made is that's probably realistic for, I guess, the physics, that is, the, assuming that the practitioner who's actually taking the x-ray is very, very highly qualified, well trained and always does exactly according to the rules. This person, the reviewer, the fellow that helped us with this review, his experience, his hands-on experience, was that there's a tremendous amount of variability in the actual practice and how people go about taking x-rays and their proficiency. So this would be a new item, I guess. It's not here on the list. I don't know if it's appropriate or not at this time to bring it up, but it was his feeling from his own personal

experience that the real uncertainty, the variability that might exist when a person is taking an x-ray because of the differences in the skills of the practitioner and also the physician and what he, his sort of druthers regarding the film that he's going to look at, for a given x-ray the difference is considerably greater than the 30 percent, and that was a point made by one of the fellows who participated in our review who had a lot of experience with these things.

DR. BEHLING: And I agree, John. When you look at the variability of the doses defined by the millisecond exposure duration, the KVP 7, the distance between the source and the individual, these are all variables that, and of course, the one of the most important critical ones is the issue of number of retakes which is, I'm not sure, fully addressed here in a 30 percent sigma value.

I'm sure that there were early times when a film was done perhaps twice, even a third time, which is perhaps going to be one of the major contributions to the uncertainty of assigning a particular dose to an organ. So 30 percent is perhaps not unreasonable, but it's clearly not a

bounding value.

MR. GRIFFON: John, John Mauro, I was just wondering if there was any reference, any publications supporting that claim that these in practice uncertainties might be greater?

DR. MAURO: I can get that information, and I don't know if it's in the published literature, but here in New Jersey we have a sort of licensing process. We keep records of all of the licensed practitioners and their performance. So I probably can get my hands on them, and this is current now, data, but I could look into this. This, the comment that was brought up wasn't brought up by me. It was brought up by a practitioner who that was his personal experience.

Let me look into this and so before we put this on the list so to speak as an item perhaps I could put out, look at this a little further and give his comment a little pedigree so to speak and forward it on. I'll forward it on to Hans, and Hans if you could work it into the system as appropriate.

DR. BEHLING: Yeah, for instance, I'm looking, sometimes when I look at the data, and I look at

the original data as supplied by the DOE, I actually look at, for instance, the individual in question in terms of his gender and his body weight. And of course we know when we talk about a reference person, we're assuming a 70 kilogram individual with a chest wall thickness of a certain fixed dimension. And of course that defines what the KVP setting is, et cetera, et cetera.

When in fact you look at a guy and you see his weight is 240 pounds, he's going to obviously receive a much higher entrance skin dose for sure because the KVP setting has to be jacked up considerably to accommodate that person's weight. So these are all variables that clearly will come into play.

The question is will the doses be significant enough to deal with issues that we're concerned with and that is the compensability of the claim. If one were to say okay, the long dose goes from early in years from 42 millirem to 52 millirem, would that necessarily, that increment, make a big difference with the likelihood that there will be instances where a person's claim will be compensated based on changes or increases, modest

increases, in medical exposures. Again, it's a judgment call here.

MR. GRIFFON: Dick's been waiting to -- sorry.

DR. TOOHEY: Yeah, I'm sorry, just a couple of general comments on that.

We actually had some serious arguments between two medical physicists while 0006 was in there. I think there were some health physicists when it comes to arguing about the physics. But I did want to mention we are revising TIB-0006 to include C-spine and T-spine doses for fluoride workers which was a gap we had. And in general, I think if we had site-specific data that we can put in the medical TBD at the site, that should take precedence for dose reconstruction.

TIB-0006 should have the tables of the dose coefficients we're going to use given the skin entrance dose exposures, and default parameters if we don't have site-specific information. So as we include the other things in there, I want to get revision of that to cover these other bases and make it clear what the purpose of that one is. Again, that was one written early on to get us going.

MR. GRIFFON: Does that address all the items for

TIB-0006, Hans, or --

DR. BEHLING: Yeah, pretty much. I guess if I do have a modest criticism is that in some instances we defined doses for a lot of years that are in units of microrem and I find that a little disturbing. I think it would be nice just to say less than one millirem rather than microR readings or microrem readings. It gives you the false impression that there's a level of accuracy that simply doesn't exist.

You know, it's like dividing ten by three. You could end up with 3.33 and an infinite number of threes, when in fact, if the value of ten is not a good, a solid number, you know, you should stop at the value of three or maybe 3.3. And I think there has been a tendency on the part of certain procedures to give the illusion of a level of accuracy that simply isn't there. But you know, that's just a comment on my part.

I think we're done at this point, Mark.

MR. GRIFFON: How about, isn't there a TIB-0007? There's two.

DR. BEHLING: Oh, I'm sorry.

MS. BEHLING: Now the next one's OCAS-TIB-0006 as opposed to ORAUT-TIB-0006, and this has to do

with Savannah River site external dosimetry records.

DR. BEHLING: Okay, the comment on, the first comment for TIB-0007 is that guidance does not specify all occupations that may involve neutron exposures.

MR. GRIFFON: No, no, no, you missed -- go back up to TIB-0006, OCAS-TIB-0006, instead of --

DR. BEHLING: Okay, oh, I'm sorry. I'm sorry. Okay, we're at OCAS-TIB-0006, and the first comment there is that guidance regarding the need to correct SRS dosimeters with aluminum filters between 1954-1981 is complex, confusing, and does not clearly indicate which dosimetry data requires refinement.

As this comment summarizes, I found it very confusing in terms of the description for dealing with the aluminum filter, and what it represents in defining a correction factor.

MR. GRIFFON: Actually, can I make a recommendation here since I'm looking at the time? The next two procedures are Savannah River-specific, and I'd propose that we're going to take that up at the subcommittee in Oak Ridge. Let's put the Savannah River site profile on

there and start with these two procedures. We can start with a matrix review from here. Is that agreeable?

MR. HINNEFELD: It's okay with me.

MR. GRIFFON: Yeah, because I want to give, Jim asked, and rightly so, to maybe have a little time to discuss Y-12 at this meeting.

One thing before we move on from here, what I wanted to ask is on the matrix now SC&A has expanded the matrix to include all the internal dose findings now in the CATI interview findings, I guess. Is there any chance that we will be able to discuss that at the subcommittee meeting in Oak Ridge on the 17th? Will that give you time to --

MR. HINNEFELD: There are five workdays.

MR. GRIFFON: Some preliminary response.

MR. HINNEFELD: There are five workdays in the meantime so --

MR. GRIFFON: So the answer is yes?

MR. HINNEFELD: I would think, I think what we could hope for is to hope that there are some simple resolutions and go through case, you know, a number of these comments and see if we have relatively, oh yeah, you're right kind of

resolutions or --

MR. GRIFFON: Well, the other thing is, the other thing I would ask is if you can focus on the ones that are at least Savannah River-specific because then we can get a site profile in the procedures that relate to Savannah River.

MR. HINNEFELD: We can focus on the Savannah River ones. Yeah, we can focus on those.

DR. NETON: Many of the Savannah River ones are related to the high five approach which is covered in the Savannah River site profile review.

MR. GRIFFON: Right, so they are one and the same.

DR. NETON: They're very one and the same actually. I believe, and also the organically bound tritium, and there were a few other issues like that.

MR. GRIFFON: I think you're right. I think we deferred most of them to the --

DR. NETON: Matter of fact most of them were deferred to the resolution within the site profile review.

MR. GRIFFON: Well, I guess I would ask to the extent you can fill in the matrix responses on

the internal and the CATI, you know, we can move ahead on this at the subcommittee on the 17th.

UNIDENTIFIED SPEAKER: Mark, could you speak into the microphone?

MR. GRIFFON: Sorry, sorry. I was just saying to the extent that NIOSH can fill out responses for the internal dose and the CATI interview sections of the matrix, that will allow us to pick this item up at the subcommittee meeting on the 17th. So we'll just understand they'll do the best they can, and we'll proceed from there.

MS. BEHLING: The only additional comment I would like to make about the matrix because we were also under lots of time constraints here, there are a few things I may want to add to the interview that was the very last one I did. I was anxious to get it out and there may be a few clarifications I want to put onto the CATI, the matrix for the CATI reports.

MR. HINNEFELD: Could you send it as a, essentially the same file name, but rev one or whatever rev you want, so we'll do the same thing?

MS. BEHLING: Yes. And they're just clarifications.

MR. HINNEFELD: And it would be acceptable to, if she's going to send these to me, who else should she send them to? I mean, the working group, the entire board?

MR. GRIFFON: The entire board I think.

MS. BEHLING: Just a few clarifications that I wanted to add.

MR. HINNEFELD: Sure.

MR. GRIFFON: Any other clarifications on this, the matrix, and what we're going to have prepared for the next meeting? Okay.

I think we're going to try to cut this off at like 4:30 because I think a bunch of people have flights or at least I do.

Y-12 SITE PROFILE REVIEW

Jim, for the Y-12 site profile review I think it makes the most sense to have Jim maybe give a preliminary discussion of --

DR. NETON: I don't know if Joe is prepared to discuss this at all and I can react or whether it's just more time efficient for me to --

MR. FITZGERALD: I think in 30 minutes I would, rather than going through a recitation, we did present this back in July although that was awhile ago. And I think in the process of

responding, you can, in the process of responding I think you can just outline the finding at the same time. That would be very efficient.

DR. NETON: That's fine.

You know, as with the Bethlehem Steel, I think there's a couple key issues here that we really need to come to grips with and then the other ones are -- not that they're not important, but they're not as much of a show stopper as a couple of the other issues.

So that was a cue for me to --

MR. GRIFFON: I think so.

MR. FITZGERALD: I was going to say if there was an hour we could have done a tag team, but I think with 30 minutes, I'll just defer to you.

DR. NETON: Actually, all I have is a slide that loosely paraphrases the findings. I know the report was very extensive. Again, we received this report I think some time in September 9th or so, and we did have a preview earlier as to what it might be. But you know, it's a couple hundred page document. It's pretty hard to react to given all we have going on, but -- and Joe can correct me if I'm off base here, but these are really in my mind the six major findings that

summarizes what was in the executive summary. There are a number of vertical issues that go along with these, but they all fall along similar lines. I'll just go through them briefly and we can set the stage here maybe for discussion at the subcommittee meeting.

Support services worker was defined. There's incomplete monitoring data for the support services workers. Those would be welders, pipe fitters, those type, the crafts-type folks. And the finding was that we did not have enough data for, to justify that these workers were not as exposed as the monitored workers, and I'll go through that a little bit later.

The second one is lack of evidence that monitored workers were maximally exposed. That's sort of a similar theme in the sense that prior to 1961, anywhere from seven to 20-something percent of the workers were monitored for external, and I think the finding was that the profile didn't do a sufficient job of defining why we believe those workers were in the maximally exposed population.

The third finding is a number of issues rolled into one related to external exposure. Primarily, this had to do with our coworker

matrix and solubility types used and the 48 hour lag period when you take a sample on a Monday when a person hasn't been exposed since Friday and what that means in terms of interpretation of the dose. It's a fairly complex technical issue that we'd like to address. I'm not sure we're going to be able to go through that today.

And then as we go down the issues become a little less problematic. Internal dose not addressed for all radionuclides. There is a few, a number of radionuclides other than uranium at the site that were in existence. For instance there's some discrepancy in our mind as to whether these nuclides move rightfully under the purview of X-10, versus the National Lab, versus Y-12, that sort of thing.

And then this neutron exposure issue not adequately addressed is of a similar vein to what we just talked about with the procedure review. That is, you know, can one indeed see neutrons of greater than, less than one rem, I mean, yeah, one rem. And there's actually a fairly complete companion report now at TIB that goes along with the site profile that addresses this issue, and has very much along the lines of the table we saw

in the procedure review, some documentation to support the fact that 500 millirem was indeed seeable although there are correction factors that need to be applied as you go.

MR. FITZGERALD: And Jim, on that one it has a sort of a companion issue where I think there was a claim that the neutrons were sufficiently hard higher energy at Y-12 so that that, in fact, wasn't as inappropriate as it might be elsewhere. And that got into the question of spectrums.

DR. NETON: What were the spectra coming off of these devices.

MR. FITZGERALD: Right, in the sense that there really wasn't really a broad sense of that.

DR. MAURO: Excuse me, Jim. This is John Mauro. I'd like to add a clarification. It sounds like there with regard to the neutron issue, there are two aspects to it that I'm hearing. One is this business of whether you can see, using NTA film, exposures of less than one MeV, (unintelligible) 500, (unintelligible) one MeV, and now I'm also hearing this issue is now regarding the energy of the neutron but also, I guess, the exposure itself if it’s less than one rem notwithstanding the energy, you might have a problem. Is that

what I heard?

DR. NETON: No, I think what we're saying is you can see, NTA film can indeed see neutrons less than one rem.

DR. MAURO: Okay, but there was a one MeV issue also, right?

DR. NETON: And I said one rem. I mean MeV. It's late in the day. My mind is fogged. I'm sorry. A rem and MeV.

DR. MAURO: I'm okay now.

DR. NETON: I'm sorry.

I think the first two issues in our mind, and one needs to keep in mind that we also currently have an SEC petition under evaluation and these are very relevant to that petition. You know, the profile reviews and petition evaluations are very interconnected obviously. And the issue of support services workers is actually the subject of the SEC petition.

We have gone through that, and I've done a review of the data available, of which are fairly extensive. I mean, I hate to keep spouting numbers, but you know, we do have monitoring data. Y-12 is one of the sites that has a fair extensive monitoring history. Believe it or not,

there were over 900,000 air samples taken at the Y-12 facility between '51 and '76, about 50,000 lung counts for uranium, 10,000 lung counts for thorium, 80,000 air samples for thorium, and I forget exactly the number of external measurements, but tens and tens of thousands.

So we have a fair monitoring history. The question though is when you get down to this time period, and we have two to 23 percent of the workers monitored, the profile review asserts that we don't really know and can't prove to a reasonable person that the workers who were monitored were the ones most likely to be exposed. This is a very similar theme that we've seen in other reviews.

We do know job category here, but the fact is if you go through and analyze by job category, it turns out that a fairly wide spectrum of workers were monitored. I mean, you will see, and my original thought was you would see chemical operators or something like that and then none of the service folks. And what happened is you see a fairly diverse spectrum of workers being monitored.

SOL has gone and interviewed support personnel

staff, the health physics staff, and they pointed to documents, procedures which we have which we will provide, that indicates that there was a very conscious effort on the part of Y-12 health physics staff at that time to monitor maximally exposed individuals, those who had the significant potential for exposure.

If you couple that, and they've gone back and done an analysis of these support personnel that were indicated that maybe were not monitored properly. It turns out that during the time period when the monitoring was conducted, about 11 percent in this early time frame prior to 1961, of support service personnel were actually monitored, not a hundred percent, but it's right in the same ballpark of the --

MR. FITZGERALD: Were these the -- certainly Bob can speak better than I can, but are these the outside maintenance or outside support people as opposed to the inside? Because it took us awhile to figure that out that there were support staff on the line programs versus what they called outside support or outside maintenance and sort of two distinct groups. And we found actually monitoring information and dose values for the

support personnel on the line but not for the so-called outside maintenance staff.

DR. NETON: Now are you talking about subcontract, like --

MR. FITZGERALD: No, no, they made a distinction between two groups of support service workers. Ones that were, in fact, assigned to a specific facility, a specific operation, and were providing dedicated support to that operation versus -- I'm not sure what the best term would be, maybe sort of a freelance, support folks that would work anywhere in the plant on a daily assignment almost. They would provide maintenance, even over at the X-10 facility. And those folks seemingly did not have the monitoring, regular monitoring, routine monitoring that the people that were assigned to the line programs did. If you were a 92-12 maintenance person, you did get monitored just as if you were a chem operator, it was a regular monitoring. But if you were an outside maintenance person, you could probably well not be monitored, and in fact, we interviewed a number of them. Interviewed workers from both groups, and clearly the one group was distinct

and different even though they were both maintenance people.

DR. NETON: Well, I don't know the answer to that question. I mean there was really just out of the database that was polled, 317 workers with job titles including, you know, pipe fitter, plumber, steam fitter were identified which was about 11 percent of the population.

MR. PRESLEY: Can I talk? Can I talk? All right, at Y-12 you had -- this is Bob Presley. At Y-12 you had what we call a prime construction contractor. Then you had Y-12's own individual construction people. The people that worked for Y-12 we had carpenters and pipe fitters and everything like that. Some of them, yes, were assigned to work full time up in the areas of 92-12 or alpha five, places like that because we had satellite shops. And those people should have had a badge and should have been monitored just like everybody else in the plant. You had a prime construction contractor at that time I believe was Rust Engineering. And those people were hired to do nothing but construction or tear down. They would go in, if we decided to build a new building, they would go in and build

that building and turn it over to Y-12. And then we would start production in that building. You have to know whether those people are Y-12 employees or whether they are Rust or M.K. Ferguson (ph) because it's very likely that what you've got is everything rolled into one. You've got so many, say 200 pipe fitters.

Well, those pipe fitters, Y-12 didn't have 200 pipe fitters. We just didn't do, you know, we didn't have that much pipe fitting because the pipe fitting on the new buildings was done before we ever took it over. So you've got two sets of people. You've got construction contractors and then you've actually got people that worked for Y-12.

MR. FITZGERALD: Well, it sounds like we might actually have three based on the worker interviews that we --

MR. PRESLEY: And yes, there were. People that went to all three plant sites.

MR. FITZGERALD: Yeah, I was going to say -- not to dwell on this, but this might be important certainly for the SEC reviews, the fact that there were the dedicated, in fact, we did have the interviews with 92-12 and alpha maintenance

people. We also had interviews with a group that called themselves the outside maintenance staff, but not, not, they weren't construction staff. They were people that were covering the entire site with different assignments. They were Y-12 employees, so --

MR. GRIFFON: Like a pool maintenance --

MR. FITZGERALD: Like pool maintenance, and those folks generally were not monitored. And that was of concern and that's something that would be useful to pin down. But literally, that was the answer we got back. And now the ones that were assigned to specific operations as Bob was pointing out clearly were, in fact, monitored. And we can go back to the early '60s and find data for those specific workers as being monitored. They were monitored just like the chem operators were so...

DR. NETON: Right, you made a very important distinction though. Right now NIOSH is not doing dose reconstructions for building trades folks that were not related to the prime contractor. We recognize that those folks were not monitored and there was special exposure circumstances, and this is complex wide where we have held up those

things. And in fact, we have a contract in place with the Center to Protect Workers' Rights to help us evaluate the potential exposure conditions for those, that special --

MR. FITZGERALD: What I'm saying though is that just, the clarification for that group that we're talking about may be whether or not they were --

DR. NETON: And I appreciate that because I'm not sure that that was really pointed out as a difference in the review.

MR. FITZGERALD: Right, and what we say here is that we couldn't nail the personnel records and figure out, you know, who was actually who in some of these cases.

DR. NETON: In fact, whoever these 372 workers are, we've compared them to the average of the monitored population, in fact, they are lower than the average population of the workers that were monitored during that period. So that's some of the data that we tried to flesh out. One more issue that we tried to address. I'm sorry, these were the five major findings, and then as we get down here there were some five, there were ten total findings. Now I'm refreshing my memory, but we won't dwell on these

right now. Let's stick with --

MR. GRIFFON: We've got about ten minutes.

DR. NETON: We've got about ten minutes here so in the interest of saving time I want to just switch over to this one slide that I have.

MR. GRIFFON: Jim, while you're switching can I ask, is any of this data, the databases you referenced, are they on the O drive? Are they accessible to us to see this data, all this monitoring data, you talked about, nine thousand samples, et cetera?

DR. NETON: I don't believe it's readily accessible. Dick Toohey is not here right now, but we can certainly try to get those out there and make them available. They may be, but I'm not sure if they are. Those tend to be more -- a lot of this data came out of CDR databases very much like Mallinckrodt data. We'd need to put it out there in special format and everything so we can work on that.

MR. GRIFFON: If that can be done, that'd be great.

DR. NETON: This slide is very confusing so I apologize for that. I didn't put it together, but it's instructive. If I just can step you

through here, this speaks to the issue of were the maximally exposed people monitored or not at the facility. So what we have here is the cut point.

After 1961 almost everybody was, external exposure was monitored. And so we have prior to '61, after '61 which is the green hash line there. The red dots on the top are the maximally exposed workers by quarter during all those monitoring periods. If you notice, prior to '61 and after '61, the maximum exposed workers continue to be about the same. There is no trend there in the data set. However, after '61 when you start monitoring everyone, you notice a trend going down which is not what you would expect if only the lesser exposed individuals were monitored where you're not capturing the maximum exposures.

So this is somewhat instructive to point out that, you know, we believe there's sort of a three-pronged approach here. You talk to the line. You talk to the health physics staff. You talk to line managers. They indicate the maximally exposed people were monitored. There's some documentation to support that fact, and then

you look at the monitoring data itself, and it also bears that out. So we feel we have a pretty good handle on this fact. And you know, we need to move forward on this, and we can engage in some --

MR. FITZGERALD: But just to confirm what you're saying is that the interviews plus the empirical information is pretty much what this would have to rest on. There still isn't anything that would be documentation procedures or --

DR. NETON: There are, well, there are a couple later reports issued like in the late '50s that when they were switching over from, I think a weekly to a quarterly program, there's some very good discussion in these documents while working with line management and discussing who should be on based on past histories of what the monitoring program is showing and that sort of thing.

You know, I'm not sure what more we can produce on this, but it certainly is all pointing toward a direction that these folks were the highest exposed individuals were monitored. This is a very significant issue though because it also carries over into the internal dose area where we've worked on a coworker analysis of the

hundreds of thousands of urine samples that were taken and intend to use those to establish bounds for the internal dose of unmonitored workers. We are proposing to use the 50th percentile, and we have been using the 50th percentile to assign unmonitored workers since we believe that the monitored workers were the maximally exposed. Now the profile review is suggesting that no, you need to use the 95th percentile to assign that to unmonitored workers. That puts you in -- if these workers really were the most exposed, the absurd situation of assigning higher doses to unmonitored workers the 95 percent of the monitored workers. And that's not very appealing to us, and I think we can discuss this when we have more time about why we believe the 50th percentile is more appropriate for these workers than assign the 95th percentile to the unmonitored workforce.

That's probably about as far as we can get into this given five minutes left, but I think it's important to keep these thoughts for the subcommittee meeting. We're going to work out these details, you know, flesh these out in more detail. And if we have time, we can engage in

some discussion just with the Bethlehem Steel profile with SC&A, and maybe, you know, some facts out there that they can react to.

MR. GRIFFON: Just one other question. Are there other new TIBs? You mentioned one new TIB that might have come after this review or was it considered during this review? I mean...

DR. NETON: Most of them were considered during this review. I think there were a couple. There's one that was, that evaluated the external exposures in 1948 and '49 that documented what was done and the pocket ion chambers and that sort of thing. And there is another one coming out that is in press, and the subject of it escapes me at the moment, but I think it's the beta dosimetry.

There's a three-part series that discusses gamma, historical gamma, historical neutron, and there's an historical beta document coming out. So those are all out there on, as they’re published on the O drive or X drive or whatever it is on your computer, and they're available for review. There's probably, I had them totaled up but there's probably eight or so documents that are supplemental to the TIB and most of them were

considered as part of this review. That's probably all we're going to be able --

MR. GRIFFON: Yeah, that's about -- okay.

MR. FITZGERALD: And I guess maybe a thought would be to pick this up that Monday before.

MR. GRIFFON: Yeah, my hope is, again tentatively, I think we need to try to close out Bethlehem Steel at the subcommittee level, continue on our procedures review, and probably take up Y-12 before Savannah River. We have a full day of subcommittee so --

MR. FITZGERALD: Well, this will help. We'll have Joyce Lipsztein who did a lot of the internal section, in Knoxville, so that will be good timing for that.

MR. PRESLEY: Before we stop, can I make a suggestion or a motion or something that the next time we have a committee meeting that we kind of meet where everybody's at a round table where we can meet as a roundtable discussion, and we don't, you know, it's nice to see everybody's face.

And the other thing that we could -- public comment's fine, but the telephone call-ins and stuff like that's been awful today. You know,

we've had trouble listening to everybody and hearing what they say. And I kind of have a problem with that. If you're here, fine, but if we have a committee meeting, we need to, they need to be here.

MR. GRIFFON: Yeah, I'm not sure we can make a motion here. It might be something we should bring up at a regular board meeting, and you know, bring up the format of these work group meetings as well as the, you know, the telephone aspect because it does make it difficult especially when we're sorting through technical documents.

Anything else? All right, I guess we'll see y'all in a few weeks, two weeks. Meeting adjourned.

(Meeting adjourned at 4:30 p.m.)

C E R T I F I C A T E OF COURT REPORTER

STATE OF GEORGIA

COUNTY OF FULTON

I, Steven Ray Green, Certified Merit Court Reporter, do hereby certify that I reported and transcribed the above and foregoing from the day of Oct. 6, 2005; and it is a true and accurate transcript of the testimony captioned herein.

I further certify that I am neither kin nor counsel to any of the parties herein, nor have any interest in the cause named herein.

WITNESS my hand and official seal this the 6th day of December, 2005.

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STEVEN RAY GREEN, CCR

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