UNITED STATES OF AMERICA

CENTERS FOR DISEASE CONTROL

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NATIONAL INSTITUTE FOR OCCUPATIONAL
SAFETY AND HEALTH

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ADVISORY BOARD ON RADIATION AND
WORKER HEALTH

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92nd MEETING

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WEDNESDAY
JULY 17, 2013

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The meeting convened at 8:30 a.m., Mountain Daylight Time, in the Shilo Inn, 780 Lindsay Boulevard, Idaho Falls, Idaho, James M. Melius, Chairman, presiding.

PRESENT:

JAMES M. MELIUS, Chairman
HENRY ANDERSON, Member
JOSIE BEACH, Member
BRADLEY P. CLAWSON, Member
R. WILLIAM FIELD, Member
MARK GRIFFON, Member
DAVID KOTELCHUCK, Member
JAMES E. LOCKEY, Member
WANDA I. MUNN, Member
JOHN W. POSTON, SR., Member
DAVID B. RICHARDSON, Member*
GENEVIEVE S. ROESSLER, Member
PHILLIP SCHOFIELD, Member
LORETTA R. VALERIO, Member
PAUL L. ZIEMER, Member*
TED KATZ, Designated Federal Official

REGISTERED AND/OR PUBLIC COMMENT PARTICIPANTS

ADAMS, NANCY, NIOSH Contractor
BALDRIDGE, SANDRA*
BARRIE, TERRIE
BUCHANAN, RON, SC&A*
BURGOS, ZAIDA, NIOSH
CRAWFORD, FRANK (CHRIS), DOL
FITZGERALD, JOE, SC&A
HINNEFELD, STU, DCAS
LEWIS, GREG, DOE
LIN, JENNY, HHS
KINMAN, JOSH, DCAS Contractor
MAKHIJANI, ARJUN, SC&A
MAURO, JOHN, SC&A*
MARSCHKE, STEVE, SC&A*
NETON, JIM, DCAS
RAY, SARAH*
RUTHERFORD, LAVON, DCAS
STIVER, JOHN, SC&A
THURBER, BILL, SC&A*

A-G-E-N-D-A

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

8:32 a.m.

CHAIRMAN MELIUS: Good morning, everybody. Welcome and let me turn it over to Ted for the initial formalities.

MR. KATZ: Thank you. Good morning and welcome to Day Two. No public comment session today, but we have a number of SEC sessions.

The materials for today are posted on the NIOSH website under the Board section, under Meetings, today's date. So if you want, people in the public on the line, you can follow along by going to the website and pulling up those presentations there. Or you can also follow along by Live Meeting. And the agenda of the meeting is also posted there on their website. On the agenda is the information for joining the Live Meeting and that will allow you to see the presentations as they're delivered throughout the day today.

Let me go to roll call for Board

Members. And there's only really one item with any conflicts, so I will only speak to conflict where it occurs.

(Roll call.)

MR. KATZ: And then, so, for conflicts, the only conflict is Dr. Lockey has a conflict for Fernald. So he will be recused during that session.

MEMBER VALERIO: Excuse me, Ted? I'm conflicted out of Pantex.

MR. KATZ: Oh, I'm sorry. Pantex, I forgot. Sorry, right. Pantex. Thank you, Loretta, for noting that. So she'll recuse herself from that session. And I think that takes care of it. That does take care of it, right?

CHAIRMAN MELIUS: Yes. We've asked Dr. Lockey to leave town for his recusal. For Fernald.

(Laughter.)

CHAIRMAN MELIUS: Our first item of business today is Baker Brothers. And I

believe Paul Ziemer is going to make a presentation. And I think, Stu, are you working the slides? Or is this happening? Somebody, please.

MR. HINNEFELD: I can certainly do that.

CHAIRMAN MELIUS: Okay.

MEMBER ZIEMER: Okay, well, I have my own slides here, so I'll assume -- are you all seeing them there in the room?

CHAIRMAN MELIUS: Yes.

MEMBER ZIEMER: Okay, so I'll go ahead and start then.

So, I do want to thank SC&A for actually preparing the slides for me, and particularly Bill Thurber. And Bill sent me an early version of these and I did approve them.

And yesterday I discovered -- is Bill on the line, by the way?

MR. KATZ: Yes.

MEMBER ZIEMER: Yes, okay. Bill,

I discovered a single flaw in the slides yesterday. And that fatal flaw is on Slide 1. And that is the spelling of my name. So, anyway, those of you who have it on your flash drives you can correct that spelling. It's always I before E, especially after Z, is the rule.

(Laughter.)

MEMBER ZIEMER: So let's go on and get into the technical content here.

CHAIRMAN MELIUS: We will note that in our review of the contractor.

(Laughter.)

MEMBER ZIEMER: Right. There's a brief summary of the petition history. I don't need to go through all the dates here. You have them before you. But it does begin with the receipt of the petition.

It's an 83.13 petition that was received in June of 2012 and then qualified in July. The Evaluation Report was approved in November 2012. And the Class, NIOSH proposed

the SEC Class for the operating period which was `43 and `44. That was proposed in December of 2012. And the Board recommended that SEC Class in the January meeting, January 31, 2013. That was for the operational period.

Action wasn't taken on the residual period at that time, and the TBD 6000 Work Group was asked to review the residual period and make a recommendation. So following the January action, the Work Group did do that work. We actually completed that work in March but we were awaiting this meeting to make the formal recommendation.

The next slide shows a little bit of the background, just to remind you of Baker Brothers, located in Toledo, Ohio. They are an AWE, Atomic Weapons Employer. Again, the operational period is `43 and `44. The residual radiation period, `45 through `94, and 1996. There was remediation done by the Department of Energy in 1995.

Also, just to remind you of the operational actions there in `43 and `44, DuPont and the University of Chicago had subcontracted Baker Brothers to do machine rods and slugs, uranium rods and slugs for use in the reactors at Oak Ridge and at Hanford.

The approach that was proposed by NIOSH for the residual period was similar to what's done in other facilities, and that is to take the value of the residual, I'll call it the residual radiation, basically based on airborne and surface contamination at the beginning of the residual period. The starting airborne concentration that was proposed is the value you see here. It's the geometric mean. It's based on the maximum daily weighted air concentrations for operators at the facility of 5,480 disintegrations per minute. And that's per cubic meter. My particular slide says M3. It should be M exponent 3 and meters cubed. And then assuming 30 days deposition and a

settling velocity of 7.5 times 10 to the minus fourth meters per second.

So if you take that air concentration and you multiply it by the settling velocity, which is the 7.5 times 10 to the minus fourth meters per second and the number of seconds in 30 days, you end up with a surface concentration of -- well, they don't give it as an exponent here. It's 10,653,120 dpm per square meter of surface. And then one assumes exponential decay following the OTIB-70 recommendation during the residual period.

And following this approach NIOSH, concluded the doses could be reconstructed. And this was what the Work Group is looking at with the assistance of SC&A.

One of the issues that was of concern was the fact that there had been uranium fires during the operational period. You notice on the next slide, or on the same slide here, the possibility that uranium fires could cause elevated surface concentrations so

that the TBD 6000 approach might have been inappropriate, particularly if no cleanup occurred at the end of the operational period.

And there was some concern that the value that NIOSH was using might have been inappropriate if the fires had somehow impacted adversely on that value.

So what SC&A did in evaluating this was to consider some data that's available in the publication by Adley and others that shows that -- it actually gives values for uranium machining fires. And I think some of that Adley data, I think, was distributed to the Board. I'm not certain if those SC&A White Paper memos of -- there was a memo of April 2013 and another in February. I'm not sure if those were distributed to the Board.

But basically what it showed was that the air concentrations that derived from those fires were actually lower than that 5,480 dpm per cubic meter that had been used

by NIOSH. So the NIOSH value was conservative even considering the possibility of uranium fires.

So, the bottom line of all this was that uranium fires, although they did cause elevated values, that they were within the envelope of the 5,480. And the actual values are shown in this next slide from the Adley report. They ranged, for the uranium fires, from 182 to 2,340 dpm per cubic meter.

So the bottom line was that the doses during the residual period could be reconstructed with sufficient accuracy as proposed by NIOSH. And SC&A recommended that no change to the SEC was required, that the residual period could be reconstructed.

The Work Group agreed with that and voted to recommend to the full Board that an SEC Class not be extended through the residual period for Baker Brothers. So that is the recommendation out of the Work Group.

I think, if there are questions I

can certainly try to answer them. Bill Thurber is available. Is Tom Jones available also from NIOSH, who did the work on this for NIOSH?

MR. HINNEFELD: I don't think Tom is going to be on the phone, but Jim, I think, is prepared to address anything that we might have.

MEMBER ZIEMER: Okay. So Mr. Chairman, that is our recommendation.

CHAIRMAN MELIUS: Okay. Do I hear any questions for Paul? Or Bill or Jim on this? I think it's pretty straightforward. Yes, Jim Lockey.

MEMBER LOCKEY: Paul, this is just for my edification. What's the physics behind a lower exposure level during a fire, during a machining process? Just so I understand it. It doesn't make sense to me but it's probably because of my lack of knowledge.

MEMBER ZIEMER: Well, I don't know if I can say that there's -- what the physics

are. I think we just have empirical information. We have values from uranium fires and we have values from the machining processes. I don't know if Bill or Tom can answer that in a more definitive way. It's just what information we have. Basically empirical.

CHAIRMAN MELIUS: Anybody else want to comment on that?

DR. MAURO: Bill, are you on the line? This is John Mauro. I was sort of partnering up with Bill to help out with this. If Bill is there, you probably best could take a shot at this. Otherwise I think I can help a little. Can everyone hear me okay?

CHAIRMAN MELIUS: Yes, John.

DR. MAURO: I'll be brief.

(Laughter.)

DR. MAURO: That 5,000 number is based on what's called centerless grinding. So when you look at all of the data that's out there from AWE facilities, NIOSH picked the

geometric mean for probably the worst type of machining operation you could pick.

And the reality is it's a very high number, especially as applied to Baker Brothers where they were not doing centerless grinding, they were doing other types of machining operations.

In any event, the nature of that number, which is an empirical number measured for centerless grinding, when you then go into the literature and say, okay, let's look at all the data that's out there that we could capture at these various facilities where there were some types of fires, briquette burning, different kinds of activities to see what the data looks like there.

When we look at that data we find out none of the concentrations measured as reported in Adley and Harris and Kingsley, where there was known some burning going on, that none of those measured values were as high as that 5,000 number that was measured

for centerless grinding.

So, I mean, that's the information we have. And as you pointed out, Paul, that is purely an empirical comparison that seems to support that 5,000 number as a pretty good number, even under these conditions where there might have been some fires.

CHAIRMAN MELIUS: Does that help, Jim?

MEMBER LOCKEY: That helps very much, thank you.

CHAIRMAN MELIUS: Okay. Any other questions? Yes, Brad.

MEMBER CLAWSON: How much data do they -- do they have air sampling data for this? Or just nothing? It seems like to me that they're using everybody else's information.

DR. NETON: This is Jim Neton. Yes, it's surrogate data based on TBD 6000, which has been pretty thoroughly vetted through the Working Group that these values

are a compendium of values that have been observed in somewhat controlled situations.

We're very confident that these are representative of the operations. As John Mauro pointed out, we picked the highest of those grinding-type operations to use as a bounding value.

MEMBER CLAWSON: And I understand TBD 6000. I was just wondering if they had any data at all from Baker Brothers, any air sampling data at all that --

DR. NETON: Oh, yes, there's production. Production values.

MEMBER CLAWSON: Production.

DR. NETON: That's an SEC already though. We've added it.

MEMBER CLAWSON: Right. It's years after that.

DR. NETON: Right.

MEMBER CLAWSON: I was trying to clarify that. Thank you.

CHAIRMAN MELIUS: Any other

questions or comments? My understanding is this is a formal recommendation from the Work Group that we, you know, accept the NIOSH conclusion that this should not be added, this residual period should not be added to the SEC. So if there are no further questions I think we should have our vote.

MR. KATZ: Dr. Anderson?

MEMBER ANDERSON: Yes.

MR. KATZ: Ms. Beach?

MEMBER BEACH: Yes.

MR. KATZ: Mr. Clawson?

MEMBER CLAWSON: Yes.

MR. KATZ: Dr. Field?

MEMBER FIELD: Yes.

MR. KATZ: Mr. Griffon?

MEMBER GRIFFON: Yes.

MR. KATZ: Dr. Kotelchuck?

MEMBER KOTELCHUCK: Yes.

MR. KATZ: Dr. Lemen wasn't present. Dr. Lemen, are you with us now?

(No response.)

Okay, I'll collect his vote after the meeting. Dr. Lockey?

MEMBER LOCKEY: Yes.

MR. KATZ: Dr. Melius?

CHAIRMAN MELIUS: Yes.

MR. KATZ: Ms. Munn?

MEMBER MUNN: Yes.

MR. KATZ: Dr. Poston?

MEMBER POSTON: Yes.

MR. KATZ: Dr. Richardson?

MEMBER RICHARDSON: Yes.

MR. KATZ: Dr. Roessler?

MEMBER ROESSLER: Yes.

MR. KATZ: Mr. Schofield?

MEMBER SCHOFIELD: Yes.

MR. KATZ: Ms. Valerio?

MEMBER VALERIO: Yes.

MR. KATZ: And Dr. Ziemer.

MEMBER ZIEMER: Yes.

MR. KATZ: Okay. And it's unanimous, one vote to collect, and the motion passes.

CHAIRMAN MELIUS: And we just happen to have a letter ready. I've been typing here very quickly while you were voting. So I will read this into the record and so forth.

The Advisory Board on Radiation Worker Health, the Board, has completed its evaluation of Special Exposure Cohort Petition 00204 concerning workers at the Baker Brothers site in Toledo, Ohio, under the statutory requirements established by the Energy Employees Occupational Illness Compensation Program Act of 2000 incorporated into 42 CFR 83.13.

The National Institute for Occupational Safety and Health, NIOSH, has recommended that individual dose reconstructions are feasible for all Atomic Weapons Employees and DOE employees, contractors and subcontractors who worked at the Baker Brothers site in Toledo, Ohio during the applicable covered residual radiation

remediation period from January 1, 1945 through December 31, 1996.

NIOSH found that it has access to adequate exposure monitoring and other information necessary to do individual dose reconstructions with sufficient accuracy for members of this group and therefore a Class covering this group should not be added to the SEC. The Board concurs with this determination.

Based on these considerations and discussion at the July 16th and 17th, 2013 Board meeting held in Idaho Falls, Idaho, the Board recommends that this Class not be added to the SEC. Enclosed is the documentation from the Board meetings where this SEC Class was discussed. The documentation includes copies of the petition, the NIOSH review thereof, and related materials. If any of these items are unavailable at this time they will follow shortly.

There's one typo in the third,

essentially fourth paragraph. It should be "dose," not "does."

I don't know if you got your email, Paul. I did email the letters, actually all three letters for today, to you, to your home email.

MEMBER ZIEMER: Yes, I did get it.

CHAIRMAN MELIUS: Okay. So, any comments? Corrections? If not, we should be set. Okay.

MEMBER ZIEMER: Did you get Jenny Lin's corrections on that one?

CHAIRMAN MELIUS: Which?

MEMBER ZIEMER: I know counsel had a couple of corrections. I don't recall if it was on this.

CHAIRMAN MELIUS: Yes. There were some corrections that were already made and I think that you may be opening up the old one with the corrections added to them.

MEMBER ZIEMER: Okay.

CHAIRMAN MELIUS: The track

changes. I said I didn't want to be embarrassed by my corrections on my draft letter so I had Zaida print it out without the track changes. But if you have any other changes, Paul, just either --

MEMBER ZIEMER: No, I'm good. I'm good.

CHAIRMAN MELIUS: Okay. Thank you. Okay, thanks, everybody.

Wanda?

MR. KATZ: While Wanda's traveling to the mic let me just check and see on the line. Do we have Steve Marschke on the line?

MR. MARSCHKE: Yes, I'm here.

MR. KATZ: Oh, great. Hi, Steve.

MR. MARSCHKE: Hi, Ted.

MEMBER MUNN: Good morning. We've put together slides for you for information concerning a couple of the longstanding TBDs -- I mean TIBs that we've had working inside the Subcommittee for a number of years.

The first of those is Technical

Information Bulletin 10. This has been a very interesting and I think extremely pertinent TIB for all concerned. It is relative to exposure geometry.

Understandably, there has been a concern, many of which have been discussed here on the Board itself, with regard to positioning of source terms as opposed to positioning of dosimeter equipment, so that there was very much concern about the underestimation of dose that could be a result of having objects interfere with or be at unexpected angles to the material that was actually being handled.

This was particularly of importance with claimants who were glove box operators. And with the dosimeters being worn on the lapel quite often it was assumed that, as you can see from the drawing here, the triangular distribution of photon would be of concern for any dose estimator.

This particular TIB provides the

correction factors that were necessary for the reconstructors who were doing best estimate work for organs that are in the lower torso.

The approach was an interesting one to most of us. They calculated the gamma flux at 30 points on the chest and at 30 points on the abdomen and determined the ratio of each of those, of the abdominal flux to the chest flux. Using the mean ratio of those calculations was selected as a correction factor for lower torso organs.

As you can see, this was an early TIB. We have looked at various aspects of this and it's been worked pretty thoroughly throughout the years. We've made a concerted effort in the last two years to try to close the outstanding questions that remain and get the revisions that were necessary on the street. NIOSH has done a good in helping us get that done.

We had nine findings that were brought to us originally by SC&A. And our

Board Review System which, for the benefit of those in the public who are not Board Members, is a closed, internal document that is used by the Board to keep track, in digital form, of the status of these various reports. So you won't be able to reach the Board's Review System by the use of the URL that is shown on the slide that we're looking at here.

Any information that is contained -- these are mere summaries in this slide. And anything that would have been contained in the BRS is already covered verbatim by the transcripts of the individual Board meetings. So our Procedures transcripts will give you any of the additional conversations that went on and the logic that was eventually developed during our Board meetings.

Six of those findings are now closed. Three of them are in abeyance. And we'll take a look at some of that history so that you can get a better feel for some of the details.

The first one was concerns by the contractor that the bulletin didn't have the transparency that they'd like to see, the radioactive sources were not specifically identified, and the dimensions and locations, thickness of the walls, some of the details that they wanted to see in order to be reassured about the development of the correction factors weren't apparent to them in reading the original bulletin. NIOSH provided an Appendix to list the details that would satisfy this need and we closed that item in 2011.

There was also a concern indicated by finding number 2 that the lower torso organs hadn't been specified. So there was an addition made, a phrase reading, "other cancers that appear in the region of those organs" -- that was the phrase -- to cover the specifics of stomach, liver, bladder, the other items below in the lower torso that were not specified in the original document. And

that was added to Section 2 of the document to make sure that cancers like sarcomas and lymphomas that might occur anywhere in the body were covered.

SC&A also questioned the design of the analysis that compared particle flux over various locations on the torso rather than actually modeling the variation of the dosimeter response from the location. And they also questioned the assumptions that were made about the glove box model.

So the Subcommittee debated these questions at considerable length and came to the conclusion that the use of the 95th percentile instead of the mean for the correction factor was going to be adequate, and we have changed that item to in abeyance.

Instead of the 30 by 30 array, it was felt that it would be better to compare the gamma flux to the individual organ that was being considered by the claimant. And to indicate what the -- compare that to the lapel

monitor.

But it was agreed instead that we would, as I said, use the 95th percentile from the 30 by 30 array as the correction factor. And that's now going to be incorporated.

SC&A questioned the use of the illustration calculations rather than the use of anthropomorphic phantom. And we talked about that, but in view of the fact that SC&A's calculated correction factor was essentially the same as the calculation using the anatomical illustration, the additional work didn't seem to be merited. So we closed the item in 2008. That's a fairly older closure.

This is one of those places where we talked about the Attila software at considerable length, and again it was the agreement of all that the use of the 95th percentile instead of the mean for the correction factor would suffice to be claimant-favorable in all respects.

The ninth item was concern that the use of Rocky Flats to validate the model was questionable since the Rocky Flats data was for glove box and non-glove box workers, and the information that was necessary for the contractor's view of what was required for radiation sources wasn't included in the original document.

But we, on discussion, understood that the Rocky Flats data was only used as proof of principle, that it really was not a part of the justification for the glove box factor. And that reference had been removed from the TIB and we closed that item.

Anyone have any questions with respect to what we've done with TIB-10? Yes, John.

MEMBER POSTON: It's easier to ask questions since I don't remember. When this model was used in evaluation are there specific designs of glove boxes that were used?

The reason I'm asking is, for example, at Savannah River they added additional shielding to their glove boxes. Sometimes it was quite thick. And so the only source that would be penetrating out of the glove box except through the glass were through the glove openings themselves. So that would change the model significantly, and the dose factors that a person would be exposed to. And that may be something you -- one of these guys has to answer.

MEMBER MUNN: Well, yes. Stu?

MR. HINNEFELD: There was some discussion about variability of glove box design, because there was a myriad of glove box designs.

MEMBER POSTON: Yes.

MR. HINNEFELD: The situation you describe, and most situations that we considered non-standard, would introduce that, they would introduce shielding to lower body above what was at the torso or base plate.

And which would cause our estimate of adjustment factor to be too high.

MEMBER POSTON: Right.

MR. HINNEFELD: In other words, we would adjust these doses more than they needed so we had a bounding response. And for that we decided that because of the myriad designs even at a given facility -- you know, not every design at every facility was uniform; they changed over time and so on -- we felt like it was better to stay with not making adjustments based on design and rely strictly on a geometric consideration without the consideration for the other shielding.

MEMBER POSTON: Thank you.

DR. MAURO: This is John Mauro, just to add from SC&A's perspective. This question was raised during the process and when we ran these MCNP calculations we looked at a number of different design glove boxes. Not the specific issue you just mentioned, but we did look at a variety of designs to see if

the different glove boxes that were used could change this correction factor, which I believe ended up being around a factor of 2. And we found that really the different glove boxes assumed really didn't change that. That was not a critical factor in affecting the correction factor.

But the particular question you asked, I guess I'm not quite sure of the degree to which that was part of our consideration.

MEMBER MUNN: Stu and John articulated that far better than I could.

MEMBER POSTON: Well, it's a complex situation.

MEMBER MUNN: It is.

MEMBER POSTON: And you simplified it. And I understand what you did, but it's much more complicated than what's done. In some areas, for example, again at Savannah River, the glove boxes are arranged in a U. So a person that's working in the middle glove

box is actually being irradiated by streaming through the glove ports on the two from either side. So it's a very complex geometry associated with these kinds of exposures.

MEMBER MUNN: As you may recall, we tried to take all that into consideration. We gave a great deal of attention to this particular item and it's one of the reasons why it was on the books for so long. It was complex for all concerned.

Anyone else? Yes, Brad.

MEMBER CLAWSON: Well, I guess I've got to come at it from a little bit different standpoint than John, but I agree with him on this. I guess my issue is what we found out at Hanford and PFP where they put shielding on the front of the glove box but nothing underneath, had very thin shielding which then the person is backed up against another. You're getting scatter radiation from underneath of it. And this is where their lower torso was actually receiving more

than what their upper was because glove box design was to shield them, their body, but nothing underneath it.

I saw item number 6, which was this, and it's in abeyance I guess like that. I was just wondering, I agree with John, this is pretty complex. I know you guys drew it up pretty right, but as we found out in Hanford, the shielding of it was completely -- they had to reevaluate that.

MEMBER MUNN: Jim?

DR. NETON: TIB-10 is really a generic calculation to address the situation and it's bounding for -- as it's been described. There are a myriad of facility-specific conditions that come up when you're doing dose reconstructions and those would need to be addressed on a case-by-case basis. You can't have a document like this cover all possible combinations.

For example, I remember neutron glove boxes at Mound, they had sort of this

galley-type operation. That was considered specifically in that instance. So I think TIB-10, as it is, is adequately -- the math is adequate to describe the variance in the two measurements. But if there's other special situations they would need to be incorporated outside of the realm of TIB-10.

MEMBER CLAWSON: So, Jim, if I'm understanding you, what you're doing is you're taking a generic glove box and just the basis for that. So each one of these would have to be on a case-by-case scenario.

I guess I get into a worry when I start hearing, well, we put shielding up on this one so we're going to -- because most of these glove boxes were torn out 20 years ago.

DR. NETON: I really don't think in TIB-10 the shielding actually comes into play. It's a geometric correction factor.

MEMBER CLAWSON: Okay.

DR. NETON: Assuming that the shielding is equal for both sides. If you

start putting shielding down lower, as Dr. Poston points out, it's going to be an over-correction. You're going to overestimate the person's dose to the lower torso.

MEMBER CLAWSON: So this whole one was just set up for the geometric.

DR. NETON: Only a geometric correction, ignoring any additional shielding or lack thereof. It's just you're physically, you know, the badge is physically further away from the source than your lower organs.

MEMBER POSTON: I think the important thing that you just said was it's an over-correction. So you're getting a higher estimate of dose.

DR. NETON: Yes.

MEMBER POSTON: So that's favorable.

DR. NETON: Yes.

MEMBER MUNN: We're using the 95th percentile.

Yes, Henry?

MEMBER ANDERSON: With the geometric issue like that, height becomes an issue. So in your 30 by 30 grid, what was the standard height of the individual that you -- I mean, if you had somebody who was 6'8" versus somebody who was 5'2", your badge is going to be considerably closer to your abdomen if you're short. I mean, so it's more just what was the -- did you take into account height at all in the model or not?

DR. NETON: Yes, it was discussed. I believe this was a reference man-type height, whatever that is these days. A little taller than reference man?

But if you think about what we've done, we've taken the 95th percentile. And so, in reality, it's taking the ratio of what the badge would read to probably the lowest organ. You know what I'm saying? It's further away than -- there's a distribution of the locations of the organs. Since we've taken the 95th percentile, that kind of

stretches out the range, if you will. But it's pretty impossible to adjust these things on a case-by-case basis.

MEMBER ANDERSON: No, I was just - -

DR. NETON: You raise a good point.

MEMBER ANDERSON: It is what it is, but I was just curious as to what a U is. Because also the taller you are, then when you're actually using the reading that's coming from their badge they could be considerably further away so the badge reading is going to be lower and you move their abdomen up. So it's a challenge.

DR. NETON: Well, I'm not so sure that height has as much to do with the separation between the lapel and the GI tract. You can be 7 feet tall or 4 feet tall and the delta between those two locations is not as great as the delta in the height. You know what I'm saying? If it's a geometric

correction it's not as variable as the height might imply. It was discussed. I don't recall all the --

MR. MARSCHKE: This is Steve Marschke. I agree with Jim. Basically, we did discuss using different heights. And I think what we -- again, what we ended on is because there's 30 points covering the chest, the gamma flux was calculated at 30 points covering the chest. That could kind of be interpreted as being at different heights on the chest, or individuals with different heights.

And we felt that by going -- again, as Jim said, going with the 95th percentile, you're selecting a difference between the chest dose flux point and the abdomen flux point which is relatively close together.

But the model that was calculated -- because there are 900 combinations there, it includes also individuals who have greater

distance between the chest and the abdomen.

MEMBER ANDERSON: Again, it appears to me that you use the mean of the differences between the 30 points, not the maximum point and the lower point to maximize it.

MR. MARSCHKE: That's what they started out using, the mean of the ratio. But then we decided on the 95th percentile.

MEMBER ANDERSON: Ninety-fifth percentile of what though?

MEMBER MUNN: Of the distribution.

MR. MARSCHKE: Basically you take all the --

MEMBER ANDERSON: The distribution of the ratio.

MR. MARSCHKE: -- points, the dose ratios, and you find out the 95th percentile of those ratios.

MEMBER MUNN: Okay. Any other questions? Yes, Phil.

MEMBER SCHOFIELD: In this model,

what if the workers say that, you know, they quite often wore lead aprons? How are you going to adjust for that?

MEMBER MUNN: Do you want to address the lead apron issue for him?

MR. HINNEFELD: The lead apron wouldn't be a question for the glove box adjustment. It would be a question, I guess, for the interpretation of the badge and was the badge worn under the apron or over the apron. So, it's essentially -- the use of a lead apron is a different question than the adjustment for a glove box.

MEMBER MUNN: Remember this is just a geometric correction factor. And that would be a question the individual dose reconstructor would be addressing in a best estimate.

Yes, Brad?

MEMBER CLAWSON: I guess I just wanted to make sure what this actually was being used for. And I think, and correct me

if I'm wrong, this is just to correct the geometric means of the dose. It plays nothing else into the shielding, the manufacturing, or anything else like this. And this is used in a best estimate?

MEMBER MUNN: It's used for best estimates to organs located in the lower torso. A geometric correction factor only.

MEMBER CLAWSON: Okay.

MEMBER MUNN: Alright? Very good. If we are finished with TIB-10, we'll talk about TIB-23.

MEMBER KOTELCHUCK: Don't we want to vote on each one separately?

MEMBER MUNN: I'm sorry?

MEMBER KOTELCHUCK: Do we want to vote on each one separately?

MEMBER MUNN: We're not voting. We're just reporting to you what we have done in the Subcommittee and what the status of these is now.

Actually, from the viewpoint of

the Subcommittee, both of these are now officially closed because when we put something in abeyance that means we have decided -- we've come to a resolution on the technical issue and what is left is an administrative activity to incorporate that decision into the documentation. So we have finished with our deliberations when something goes into abeyance.

We've had nine of the findings from TIB-10, six of which are closed and three of which are in abeyance. That is to say they're closed for us, but they're now in the hands of NIOSH to incorporate into the documents. Okay?

TIB-23 was in some ways more straightforward than TIB-10. In other ways it was a little problematical. The purpose of the TIB was to give dose reconstructors some guidance as to when to determine if it was appropriate to assign missed neutron doses where the site used the lower detection limits

over 2 method as an alternative method.

Use of the alternative method, as the TIB indicates, needs to be applied when the missed neutron central estimate exceeds 75 percent of the assigned photon dose. That is, if the recorded dosimeter dose plus the missed dose is exceeded by 75 percent then the alternative method should be used.

As you can see, as I mentioned earlier, these are very old Technical Information Bulletins. We've worked on them, on this one, for about 3 years. And we have now most of the documents that are necessary to be issued have been done and the findings are incorporated.

We had eight findings from SC&A. All eight of them have been closed for quite some time. The summaries are fairly straightforward. Most are in section 6 of the OTIB, consideration 1.

SC&A felt that the instructions were inconsistent with similar kinds of

instructions that appeared in the overall guidance document of IG-001. IG-001 indicated when the neutron missed dose central estimate exceeded 75 percent of the photon dose the neutron exposure should be evaluated to determine if it should be considered to be an unmonitored exposure.

So the question here is whether or not the formula that was being used routinely, that is LOD/2, was applicable if that estimate gave you a number that was more than 75 percent of what had been evaluated.

And in 23, Section 6 said missed neutron doses do not need to be assigned if the LOD/2 would exceed 75 percent of photon dose.

The first finding from SC&A had to do with the clarity of the definitions that were included in the procedure. And the OTIB Rev 1 was issued to accommodate that finding. We closed the item in 2008.

For the alternative method,

detailed information is required that will not be readily available to the dose reconstructor, was the second finding from SC&A. Condition 1 was eliminated in the first revision and that resolved finding 2, which has been, again, closed for years now.

Finding 3 referenced IG-001 as our basic guidance. But the guidance in OTIB-23, as I said earlier, was believed to be inconsistent. The review objective stated is the procedure consistent with all other procedures that are a part of the hierarchy. And Rev 1 corrected the inconsistencies that had been identified. Was closed.

Finding 4 questioned whether dose reconstructors were in a position or whether they even had all of the information necessary to make the subjective decisions that were potential. Section 6 Condition 1 was eliminated by the Revision 1 and that resolved that finding for us.

Finding 5 referred to finding

OTIB-23-03 for the review objective. And the second review objective, 4.2, is does the procedure adhere to the hierarchical process, which is very similar to the preceding finding, as you can see.

We indicated that the issue was closed on OTIB 3 because it referred to the -- it referred back to an earlier finding, as you can see, Finding 3, which was closed. Therefore the one closed the other.

And Item 6, the reconstruction of missed neutron doses from numerous neutron measurements and accurate time information is unrealistic.

Item 7, the regulatory recommendation for striking a balance between precision and efficiency has, they said, been ignored, they felt.

Finding 8 was the generic assumption of a neutron to photon ratio of 0.75 to 1 as a limiting value for the limit of detection over 2 isn't technically defensible

or claimant-favorable.

As we've indicated in previous findings, Section 6 Condition 1 was eliminated by the first revision. That rendered these particular findings moot because it no longer existed as an instruction. It's been closed.

Do we have any questions? I think that one was more straightforward by a long shot than OTIB-10. But -- and it has been closed for quite some time.

CHAIRMAN MELIUS: Any comments or questions?

MEMBER MUNN: If not, thank you very much.

CHAIRMAN MELIUS: Okay. We actually have an hour. Our next items involve the petitioners. So we need to stay on schedule for those. I didn't think we'd finish so quickly. So I think we take a break until 10:30. Is there any other unfinished Board work/business? I think we -- yes.

MEMBER BEACH: Are there

petitioners on for Pantex -- or, I'm sorry, Brookhaven? Could we do that early?

CHAIRMAN MELIUS: Well, the only thing if we do that early then we're talking about -- it's no petitioners issue but then we've got to break. I mean, Fernald, do you want a 3-hour lunch? It doesn't really help given the scheduling and so forth. And we already did LaVon. Hey, LaVon, you want to do your presentation again?

(Laughter.)

We can do repeats. We can't really do the petition letters if we haven't reviewed the petitions already. So I think we'll be back here at 10:30.

For those of you on the phone we're taking a break and we will reconvene at 10:30.

(Whereupon, the above-entitled matter went off the record at 9:31 a.m. and resumed at 10:32 a.m.)

CHAIRMAN MELIUS: Okay, if we can

get ready to reconvene here. The Advisory Board is reconvening and the initial item on our agenda is an update on the Pantex SEC Petition. And I believe Joe Fitzgerald is going to do the presentation.

MR. FITZGERALD: Good morning.

MR. KATZ: Let me just check on the line and see if I have our Board Members on the line. Dr. Ziemer, Richardson?

MEMBER ZIEMER: Ziemer here.

MEMBER RICHARDSON: Richardson here.

MR. KATZ: Great. And how about Dr. Lemen?

(No response.)

Okay, no Lemen. Thank you.

CHAIRMAN MELIUS: Go ahead, Joe.

MR. FITZGERALD: Yes, good morning. Joe Fitzgerald, SC&A. Brad Clawson, who's the Chair of the Pantex Work Group, asked me to go ahead and give the presentation. And he has promised, along with

the other Work Group Members, to chime in when they need to.

Okay, just a quick overview. The petition was qualified in 2007. The Evaluation Report issued in 2008. And October 20th, 2011, following a Board discussion that was in August, the Board went ahead and voted an SEC for 1958-1983.

And this was based on the inability to dose reconstruct internal exposures to uranium. And if you recall that discussion, that was focused on a particular system, the W28, which was known as one of the so-called dirtier depleted uranium systems at Pantex. And in disassembly it tended to pose a lot of contamination to the workers handling that. So, anyway, that was the basis.

NIOSH believed that it in fact could use the W28 system and the bioassay data from 1990. There was a major event, if you recall, in 1989 where a number of workers were in fact contaminated with depleted uranium

from the W28 and were subsequently bioassayed. And that bioassay set is the basis for the NIOSH proposal to dose reconstruct using that data, and use it for 1984-89.

And I'll get into this a little later, but you know, the notion was it was difficult to normalize across 30 years of W28 handling. But one, perhaps, could focus in on that last 4 or 5 years where you tended to have more standard practice in taking these weapons apart and you wouldn't have as much of that difficulty in assuming normalized practice. So that was certainly the basis or hypothesis for going forward using the 1990 data.

And in January 2012, right after the Board vote, there was a White Paper, Bihl and LaBone, which provided the dose reconstruction method for `84-`89.

And essentially the Work Group sort of put a lot of that on hold for almost a year. NIOSH wanted to go back and work with

Pantex to see if they could get additional information on the start dates for these workers in that time period to get a somewhat more accurate representation of what those exposures might have been and what the bioassays represented. So that was pretty much most of 2012.

In that process we looked at the White Paper. We did have a technical call early this year. And we did go on a site data capture back in February to Pantex to in fact look at this. And we had a Work Group meeting on June 18.

Okay, essentially these are the remaining SEC time periods that are subject to review and what the Work Group focused on. We had -- we call it the bookend years. We had `51 through `57, which were the earlier years where -- the SEC started in `58 -- the earliest years where there were any radionuclide source-terms that were handled at Pantex. In fact, you began seeing fresh

depleted uranium forms arrive at Pantex in `51, the very earliest point where the plant was open.

There wasn't any assembly going on. There wasn't really any high degree of activity. But these forms were arriving from Y-12 at Pantex. And as you'll hear in a bit, what was going on was they were mating the depleted uranium with high explosives. No assembly. And then those components were being transferred to places like Burlington where in fact systems were being put together. So at that point in time there wasn't any active assembly/disassembly of systems, but there was handling.

The `84 to `89, this is following the SEC period that the Board voted on, was this question of whether or not you could use those 1990 bioassays collected in `89 to in fact retrospectively apply for that `84 to `89 period.

We separated out `90 to `91. This

period of time, this couple of years, fell right after the big contamination event of `89. And this is where Pantex management sort of awoke to the exposure potential and the implications of handling these systems and the depleted uranium, and in fact put in a fairly robust uranium bioassay program.

That's where you started seeing numbers of actual bioassay results, starting in `90 through `92. By `92, the characterization of the workplace progressed to the point where they actually stopped doing routine bioassays and did more event-based bioassays for uranium.

So there was this time period of `90 to `91, which is the last 2 years of the petition, where we felt that certainly was different than the years before that. And in the ER, NIOSH was very clear that they felt they had enough data from these bioassays to dose reconstruct.

Thorium was another question that

the Work Group focused on for quite a while. This was a little bit of a different issue. There were some systems that did in fact have thorium associated with them. You had a very similar issue when you dismantled these systems in terms of potential thorium contamination.

The proposed approach that NIOSH had included in the ER and subsequently amended in some of these papers, this was the Ruhter paper, was to use a mass ratio based on some measurements that were taken on another system. This is the W55, another system where in fact uranium and thorium figured. And to base the potential intake of thorium to the availability of uranium. So use a mass ratio between uranium and thorium.

So as the Work Group went through this we focused on uranium and if there were any implications for thorium. In a lot of cases, we found that they were subsumed by the uranium issue since the thorium method was

based on the uranium method. So even though there was thorium systems being handled before `84, of course the SEC on uranium pretty much subsumed the thorium within that. So that's why thorium is not being addressed before `84.

Okay, `51 to `57. Our focus in that time period was frankly to see whether there were any other weapons systems that predated the W28. The W28 issue in terms of potential contamination from disassembly and surveillance activities -- and really it's the surveillance activities that bring you back to that early time period. A lot of the actual dismantlements didn't take place until the sixties, the seventies, into the eighties.

In the early days they did dismantle a certain set of systems for surveillance and maintenance upgrades. There was a fairly active cycle of systems going back to Pantex. Even if they weren't being retired they were being taken apart to upgrade and to examine, what have you, surveillance.

So you always had that going on.

So before `57 the focus was, was there any systems other than the 28 that would figure in that potential source of contamination? And we focused in on three systems that had depleted uranium, depleted uranium that was not alloyed, so it was certainly subject to contamination. And that was the Mark 6, the Mark 7 and the Mark 18.

Of those three systems, only the Mark 6 actually had any commentary. And you realize going back to the fifties, a lot of your evidence comes from workers who actually handled these systems. We actually had interview notes that indicated the Mark 6 had contamination associated with it. You know, in terms of disassembly you would see that contamination. It wasn't unequivocal but we did have that evidence.

So a lot of what we were looking for in `51 to `57: were these earlier systems taken apart? In other words, just as the 28

was taken apart for surveillance and what have you, did you have the same situation with the Mark 6?

And what we established was, no, actually, at Pantex there was no disassembly even for surveillance until they had the gravel gerties constructed. Gravel gerties were a ruggedized -- I'm trying to think of -- it's like a domed igloo where they handled the system. So if you ever had an inadvertent explosion of high explosives with the radioactive materials present it would be contained and would collapse inwards.

And it was very clear that they weren't going to have any systems dismantled for even surveillance until those gravel gerties were in fact constructed and opened. And that didn't happen till `58. That included the Mark 6.

Now, what confused the situation, as the Work Group found out, you did have these systems onsite. The Mark 6 and other

systems were at Pantex, but they were there not for any degree of handling, they were just being stored and what have you.

So it took awhile to actually walk that through. And I think we walked it through with some confidence that, no, we didn't have the same situation with these other systems.

Now, you have burn pits, you have hydroshots. They all figured in that late fifties into the sixties. But there you did have a lot of air sampling data. And as NIOSH points out in its Evaluation Report, sufficient data that you could come up with a model bounding what the exposure of the individuals were.

And these were even measurements that were taken inside of bunkers where the operators were actually placed, in addition to actually in the open air. So it was a fairly, for the time, a fairly extensive set of air samples. We did have some issues, as you'll

hear, on the Site Profile side but they weren't SEC.

So for this one here we did not find a source term that clearly had any exposure potential. That's not to say -- you know, you have fresh uranium forms. Fresh uranium forms that are not alloyed will oxidize. So just by definition you do have some oxidized depleted uranium present. But we found no evidence that there was an exposure pathway of any non-negligible -- is that the right word -- non-negligible level that would have led to exposure.

We didn't see anything from the interviews. Basically the interviews painted a picture of these were clean components that were put together, mated with explosives. There wasn't any degree of contamination. And that's where the Work Group came out on that. Any questions on that, `51 to `57?

(No response.)

Okay, `84 to `89, we focused on

the White Paper 2012. Anyway, we focused on the Bihl and LaBone White Paper which was published in January 2012, issued in 2012. And that paper presented a set of assumed intakes derived from excretion values, sort of a family of curves that looked at the different solubility classes, presented a family of curves which provided a bounding dose for workers in that time period that worked on the W28.

All these again based on a log-normal analysis of the 1990, again going back to that single bioassay set. And that is the one set of data which is plentiful enough and reliable enough to use for this purpose. So that's why we keep going back to it.

There were five different intake timing assumptions used. We went through this in the "sufficiently accurate" discussion yesterday. Our concern on this one is not so much with the method but whether or not you get to a point where the assumptions aggregate

to a degree where the uncertainties start becoming a problem.

And that was the problem that I think we had with the application of this method for that period of time, was you did have to make a number of assumptions regarding the classes of uranium sources available for exposure, the intake time frames. And all these led to what we thought was a relatively high level of uncertainty. And that was our major, I think, one of our major concerns with the method.

And it's laid out in the paper, but it basically comes down to that, that when you get into I think that kind of analysis the large number becomes an issue.

The other issue we had was -- and I don't know if Joyce Lipsztein is on the phone, but Joyce spent a great deal of time looking at the 1990 bioassay set. And based on looking at the uranium-234, the uranium-234/-238 ratio, the presence of 235, we raised

some questions about the fact that it looked like you had a residual contamination level of high enriched uranium that was present in these bioassays.

And it wasn't explained by anything we had seen in terms of documentation and wasn't explained in the ER but it was pretty evident in the bioassay results themselves.

We went down to Pantex on the site trip and we looked at more contemporary results, bioassay results, and we found the same thing, that there was a residual level of high enriched uranium present in even those bioassays. I'm talking 2009.

We sat down and talked this over with the health physicist who happened, and this was very convenient, time frame-wise they were present at Pantex back 30 years ago as well as they were still there now. And so we kind of posed that question to him. I said, you know, can you explain this.

And, frankly, no. They speculated on a couple of possibilities but clearly there was no obvious explanation for what seemed to be a residual level of high enriched uranium that was present.

And it wasn't clear from what source. I mean, again, we kind of speculated where it might have come from. And I think they posed some possibilities it might have come in with the uranium from Y-12 because Y-12 handled both high enriched and depleted. It could have been a residual level that built up over time at Pantex. But, you know, again, just wasn't any real good idea about it. So that's kind of where it left.

But it certainly posed this sort of question of, you know, can this single data set, with the uncertainties involved, be applied for even these 4 or 5 years when you have in fact these residual levels of high enriched uranium that might have come from either other systems or been present in the

workplace? So those were kind of the issues that we raised in the Work Group and the Work Group looked at.

And essentially based on that concern, that that single bioassay set could not be sufficiently adequate to address the potential source of other uranium isotopes, other systems that might have played into what was being found in the urinalyses, that led the Work Group to recommend that, yes, for those 4 or 5 years you couldn't apply it that way.

Okay, 1990 to `91. You had a routine bioassay program in `91-`92 with 431 and 239 workers. A lot of workers, a lot of bioassays. No real issue for those two years.

We identified `90 as a transition year where you did have those bioassays that were taken in `89. They were still cranking up the routine program, so essentially it was a ramp-up year at Pantex for bioassay.

And we validated that certainly

there was adequate bioassay data to be used, that you could use in fact to apply backwards into 1990. But I think the Work Group felt that with the fact that it was a transition year it warranted a recommendation that 1990 there still was insufficient uranium bioassay data to support dose reconstruction.

So that's essentially what happened for 1990, whereas there was sufficient data for 1991. Any questions on that? Yes, Wanda?

MEMBER MUNN: Joe, I'm puzzled. Do we have no bioassays at all on the workers who were being studied following the 1989 incident prior to that incident? We don't have any prior assays from those individuals?

MR. FITZGERALD: No. You essentially had about 305 bioassay data sets were done in `89. It was late `89. And the results were available in early `90 for those workers that were on the W28 line. That's when the incident was recognized and responded

to.

MEMBER MUNN: Yes, but my question is you did not have any prior bioassay on those individuals.

MR. FITZGERALD: That's right.

MEMBER MUNN: That's interesting. Was the bioassay program intact prior to 1989?

MR. FITZGERALD: Well, it was focused on tritium. Certainly there was a tritium issue at Pantex. But depleted uranium, if you go back into the eighties -- it wasn't just Pantex -- it wasn't really seen as a major source of exposure, particularly if you were handling plutonium components, enriched uranium components and you had a lot of tritium. So depleted uranium wasn't seen as really a significant radiological issue.

That recognition didn't really come to the fore until `89, particularly with this event where you had a lot of workers that were contaminated. And that coincided I think with a recognition in about that same time

frame that from a radiological standpoint the programs needed to be a lot tighter on contamination control. ALARA and -- and that's the same time frame the Tiger Teams were making their visits.

So if you look at `89-`90 from the management perspective, where the recognition was growing that the programs had to be managed differently than they were in the past, and the fact that before that the site didn't see depleted uranium as something that posed a large radiological risk.

MEMBER MUNN: Knowing the status of radiological protection along about that time it's surprising to me that there was no uranium bioassay prior to the incident. But, I guess -- yes, I can see why they'd be focused on tritium. But that's really odd, isn't it? That's too bad. From our perspective, that's too bad. Thanks.

MEMBER CLAWSON: Joe, if I could -- something that was interesting to understand

about in Pantex too was Pantex was kind of a different breed of cat. You understand what they played with so they kind of fell outside of the realms of things. You had other sites that were involved with them.

And one of the things that we see is the 1989 to `90 was when Pantex was actually shut down because they could not comply to the 8415, the program for standardized radioactive handling, they had two RadCon personnel that were covering the entire site.

From that time frame, 1989 to 1991, they went from two RadCon to over 70. They had, their air sampling, major, major overhauls. It was interesting from the standpoint of they did not fall -- they were DoD/DOE mix, don't mess with this.

And, finally, and I give great credit to the HP that basically shut the whole plant down because they could not comply with what they were doing.

And this is when you see this change in the 1990 is after that happened and a Tiger Team actually came in. And it's like 495 pages. And one of the worst was there was no documentation of contamination levels, the air samples. The process was flawed. They did not have sufficient manpower. So this is why, pinnacle-y, 1990 is a change period for this site. But they had no bioassay. It was an event-driven, and that was even questionable in some of it of how they even picked who they did.

MR. FITZGERALD: And you're going to hear I think more about Fernald later. Fernald essentially was a depleted uranium operation and did not really control for contamination in any meaningful way until the mid-eighties. So it was an evolution that took place between the mid-eighties and late eighties, recognizing one needed to manage depleted uranium differently than certainly DoD had done in the past.

In terms of thorium, `84 to `91, again thorium was sort of a parallel issue that the Work Group looked at but did not, frankly, close out until uranium I think had been investigated thoroughly.

And the White Paper, Ruhter 2011, assumed a chronic intake of thorium pegged at two percent of the DU intake, again based on W55. And there were some assumptions that went into that analysis that I think we want to investigate further this year in order to close this particular issue out.

And a lot of it was founded on using the air sampling data from 1996 and using that data to bound exposures backwards before `91. And to do that the assumptions were that the operations were largely unchanged and that the oxidation and engineering safeguards were essentially the same.

And I think the major finding that we made was they installed a glove box

downdraft table. Now, they didn't do it necessarily for thorium alone. It was, you know, W55 which the downdraft table was installed for in late `91 was as much for the depleted uranium contamination that was coming off the W55, which wasn't quite as bad as the 28 but was pretty bad.

So essentially the conditions by which the W55 was handled drastically changed after late `91. It was essentially the measured contamination outside the glove box dropped to almost nil.

And that kind of poses some obvious issues if you're using the 1996 air sampling data to come up with these mass ratios. And that's kind of what the Work Group had focused on as to whether one could apply that method in a representative way if in fact you had that major change in conditions in `91. I think the conclusion was you really couldn't.

Now, what makes this a little

different is that the Work Group is only using a thorium basis alone for recommending `91, but it clearly overlaps from `84 through `91. But since it depends on the uranium for the ratio, the uranium recommendation basically subsumes thorium through `91.

But it definitely parallels the same time frame -- would parallel the same time frame. Any questions on thorium?

MEMBER CLAWSON: Another thing, too. One of the questions that came up was how come this downdraft table, how come was it only used on the 55? The 55 was smaller than the 28. The 28 was so large of a system that to build a downdraft table for it was almost bigger than the room that they could work in. And so this is why they built it for the 55 and not the 28.

The 28, after 1990, they implemented some safety regulations to be able to help the people with a depleted uranium problem, vacuum systems, respiratory systems.

Many of these things were put into place because they could not build anything large enough to be able to take care of this. This is for -- the 55 was almost half its size and they were able to implement the standard requirements.

Because what it was was a glorified glove box with a substantial amount of air flow through it, through HEPA filters, and they actually worked through a glove box to be able to do that.

MR. FITZGERALD: Okay, that's pretty much the -- I'm sorry, Wanda?

MEMBER MUNN: I'm speaking from memory here but when I read the White Paper about this my memory was that the actual amount of thorium that was measured from the downdraft glove boxes was actually quite small.

I didn't do any calculations in my head even to try to determine whether a teaspoon or half a cup of thorium would

constitute a source term of any major significance. I guess that question remained in my mind.

Is that quantity of thorium really of such significance to the dose reconstruction process?

MR. FITZGERALD: Actually, I was curious about that. I did do that calculation. It comes out to two percent, which is what NIOSH actually recommended as the mass ratio. So, I think it was a cupful. No, it was a teaspoon, you're right. It was a teaspoon compared with a cup.

What we looked at relative to that though was the fact that -- the before and after on the glove box. Before the glove box, the workers regularly manipulated during disassembly the 55. And we talked to workers that actually did so.

And unlike the depleted uranium for the 28, which when you took the cylinder apart the uranium was -- essentially just fell

out. I mean, it was already fairly free and just basically came out.

With the thorium you really had to manipulate the parts in order to get any contamination. This was to a large degree why you have such a disparity in concentration.

But to answer your question, before the glove box operation was put in place they did have practices that were discontinued where workers regularly manipulated these parts and actually promoted some of this contamination. And they got smarter on it, and certainly what the workers told us, the supervisors really said, you know, cease that kind of activity in terms of how you're actually taking these parts, using screwdrivers --

MEMBER MUNN: Screwdrivers not permitted.

MR. FITZGERALD: Right, right. And, you know, that coupled with the fact that they recognized that they could in fact put

this whole unit in the glove box and not have to deal with all this.

But the other issue, of course, is specific activity of thorium compared to depleted uranium is significantly different. So even though you have a smaller amount of thorium in terms of the radiological significance, certainly the thorium couldn't be considered negligible or considered not something to be concerned about. So that I think was the major issue.

MEMBER MUNN: Yes. It just didn't seem to me that, given the quantity and the use of the two percent assumptions, I couldn't really understand why the thorium itself was going to be a major factor in the assigned dose.

MR. FITZGERALD: Well, again, I think the parameters that were being relied upon, the two percent, the amount that might have been available for exposure, those all postdated the tightening of the practices, the

engineering safeguards and everything that happened in `91.

So what we're saying is that if you use the air sampling data for `96 and apply a mass ratio based on that, and try to apply it for before `91, the conditions have changed dramatically enough that it wouldn't hold, the method wouldn't hold. I think the Work Group agreed on that.

MEMBER MUNN: Okay. I can see it really doesn't give you a window on how much thorium you would have gotten from the prior practices.

MR. FITZGERALD: No, there wasn't any real monitoring of breathing zones and the kind of sampling that they did in the nineties. They did some fairly decent sampling in the nineties. That's why the data I think was the basis for the method, because that's where you actually have usable data.

Before that time period you did have some measurements but they weren't

measurements. They were swipes of thorium. But, again, that couldn't translate into some kind of air sample that would be reliable enough to do a dose estimate on. So before `91 you really didn't have usable data that could be a basis for dose reconstruction. And I think that's where the Work Group came out.

MEMBER MUNN: Okay.

MEMBER CLAWSON: Wanda, also, too, after 1991, when they really started to find out that thorium really was an issue. One of the things I found interesting in discussing this was when they were telling the practices that they did it was after this more attention to detail. The workers didn't even know that these were thorium components. All they have is a number goes here and this and that.

And this is when this right to know what you were dealing with came out. The samples, and a lot of the samples you saw of thorium were actually outside of the glove box, that they wanted to make sure that they

were capturing this. Because now also even Pantex looked at what items do I have in here.

I just want to give you a rough idea of why you can't generalize it. From 1984 to 1999, fifteen different systems came through Pantex, different ones. High enriched uranium, thorium, barium, so forth, different people, different processes. All torn apart. And there was no generality in, okay, we're only going to do these. They had many systems coming through all the time.

And as time evolved also the components changed, which brought in different hazards that weren't known. And, you know, we discussed earlier the AG issue and how they couldn't really put a factor to it because these were sealed pits.

But it was also mentioned of these 15 systems that went through from `84 to `89, four of them were H, high enriched uranium systems. And this is where the question they don't know, but in talking with the workers

they say we can't, you know, we have to do certain things to them which possibly could have released it. And this is where it could come.

To this day they really don't know where some of it is coming from. And we asked them. And we see it in today's bioassay samples too, that they're still showing certain traces.

MR. FITZGERALD: Okay, but to sum it up, and this is on the last page. But before we get into the Site Profile issues, `51-`57, the first bookend to what the Board has already acted on. Either we found, as with the firing pits and the hydroshot activities, that in fact NIOSH had enough data to dose reconstruct, or we did not find an exposure potential that was sufficient to identify as being an issue for dose reconstruction. So from `51 to `57, I think the Work Group felt there were no SEC issues to report.

For `84 to 1990, I think the Work Group again found that the method being proposed for depleted uranium was not sufficient given the uncertainties as well as the presence of uranium isotopes and other sources of uranium that could not be explained and could not be located in terms of the site time frame-wise as well as location-wise. 1990 again was a transition year and that was included in that group.

And of course overlaying the whole time period from `84 to `91 was the thorium issue we just discussed. So there were two sources of SEC consideration, the uranium as well as the thorium. The only time they don't overlap is `91.

With that, there's no questions on I think where the Work Group came out on the SEC issues. I can walk you through on the Site Profile status. There's quite a history of this.

CHAIRMAN MELIUS: Bill?

MR. FITZGERALD: Go ahead.

MEMBER FIELD: I just had a quick question. I think I probably know the answer but, Brad, it was a unanimous vote to recommend these Classes, is that correct?

MEMBER CLAWSON: Yes, it is. From `84 to `91. I had some people asking questions of why the `90 to `91? The thorium actually was overlapped by the uranium. It's only `90 to `91 is thorium alone. And it was unanimous by the Work Group to bring this before the Board for their consideration.

MEMBER FIELD: And does NIOSH have anything to offer contrary opinions for these two proposed periods?

MR. HINNEFELD: No, NIOSH doesn't have a contrary opinion about the proposal. I could make a few comments that I think would make me feel more comfortable about the explanation.

With respect to the W28 work in the essentially `84 to `90 period, the uranium

work we're talking about there, our approach was predicated all along on W28 population being the highest exposed people. And I think in the course of the investigation there was sufficient evidence raised that that may not necessarily have been the case.

There were other weapons systems that were similarly dirty. W28 was the one that got the attention. You know, you go to these interviews and they said there were a lot of dirty weapons systems. W28 got the attention. It got the attention because of persistent complaints from the people working on the W28.

And so the follow-up was on people who worked on W28 and that constitutes the bioassay set. As we investigated following up, it didn't really seem all that clear that W28 people were necessarily the highest exposed.

So from our standpoint, that is the issue here, is we're not sure the highest

exposed people are included in our monitored set.

And I think we have a disagreement in terms of recent bioassay. We didn't sit and compare recent bioassay when we were down there. We were down there at the same time.

The recent bioassay data I saw had some positive U-235 results but they were associated with higher U-234 and U-238 results. And the bioassay manager said that appears to be natural uranium from their drinking water source. So that was my understanding of the recent U-235 results. It wasn't really an enriched uranium intake, it was a U-235 result that was detectable but that same sample had U-234 and U-238 in it, in the proportions that roughly you would expect.

At the kind of levels you're talking about there's so much uncertainty in the counting result that it may -- you know, you can't draw too firm a conclusion from those ratios. But that seemed to be what was

going on recently. From what I saw.

But like I said, we didn't sit side by side and compare this bioassay sample and what do you make of that. We didn't do that. So there may have been other things that I didn't see.

MEMBER MUNN: We love a mystery, you know, but that's a circumstance you can think about for a long time. Isn't it?

MR. FITZGERALD: Yes, I thought we would get a clean -- like he said, I think he thought it might be environmentally related. He also thought it might be maybe fugitive additions to the Y-12 inventory that came over. But given the levels that were there, I thought, again, I think he wasn't sure. And we didn't have time to nail it down.

MR. HINNEFELD: This wasn't what we were down there for. We weren't trying to wrap this up. It was the RadCon manager who said maybe it came from Y-12. Maybe there was residual that came from Y-12. That was the

RadCon manager.

The dosimetry guy said it looks like natural uranium from the drinking water source. It was two different people.

MR. FITZGERALD: So we didn't settle it out, but again that wasn't the central question so much as it was an issue that we were trying to resolve and couldn't resolve it at the time.

Are there any other questions on the SEC part of this? Okay. Let me just back this up a little bit.

Okay, in terms of Site Profile issues. And I'm going to go through this relatively quick. Number 1 and 2 deal with the internal dose models for uranium. Obviously that was the thrust of the SEC inquiry. So those issues were closed as we closed out, the Work Group closed out, the SEC questions on uranium.

The dose estimate approach for plutonium was a question of conservatism. A

40 DAC hour was proposed. I think there were some concerns about whether that, you know, that equated to about 100 millirem, whether that was in fact sufficient. And we ultimately had some exchanges on that in the Work Group and closed that out, primarily because plutonium, again, as far as contamination sources, just did not exist except in some very isolated incidents at Pantex to begin with. And so you just didn't have the exposure pathway.

Thorium, as we just discussed, was closed out this very last Work Group meeting along with the SEC. Metal tritides, that was more of a question of it was certainly handled as a component at Pantex. Did we have any idea or did NIOSH have any idea of what was handled in terms of the particular tritide and was there any information on it. And there was a number of exchanges, but what it came down to, it was a variety of tritides handled but there was no evidence at all that there

was any exposure. These were all sealed sources at Pantex. Unlike other locations, like Mound, these were sealed sources being assembled. So you didn't have that exposure pathway.

Number 6, external dosimetry data. There's a footnote there. We did have a White Paper that was generated and there was an exchange about the same time that this whole SEC inquiry that Stu was referring to in terms of the W28 workers, we put that data completeness issue somewhat on the back burner as we tried to resolve the SEC question of the W28 workers.

And there are some loose ends. This is one of them. And it really gets down to what adjustment factors ought to be used in terms of the actual dosimetry calculations. Nothing that would be of SEC consequence, but certainly issues that need to, from a Site Profile, standpoint be resolved in the Work Group at some point. So again that's what

that footnote means in number 6.

On number 7, neutron to photon ratio not bounding. That has a history that was actually married up to Mound to some extent. We had concerns over neutron/photon ratios. And the NIOSH position evolved from reliance on this ratio as an approach to MCNP and some other more advanced techniques.

And that I think has evolved even further, which from a neutron dosimetry standpoint we're still trying to clarify as an end game as to what approach might be used. And, again, from a Site Profile standpoint be used to estimate neutron doses in concert with other sites as well as at Pantex. But there really isn't an issue other than trying to get that closed out as far as what method is the final method that would be applied at Pantex. And there's a series of questions that we've exchanged. So that's another issue that would be resolved in Work Group.

Completeness of exposure sources.

In the Site Profile, not so much the SEC, some questions were raised about how you're going to handle offsite exposures, people that went to NTS, Nevada Test Site. There were some questions on the burn sites and also the ability to actually obtain information on the early exposures in the fifties.

And again that was all included in this data adequacy and completeness piece White Paper that was submitted. And that's still I think something the Work Group is going to resolve now that the SEC issues have been vetted.

Number 9 is incidents. This is a common, I think, finding in a lot of Site Profile reviews where I think the opinion is that more complete treatment of incidents, events, whatnot, ought to be included in some of these Site Profiles. Pantex was an early Site Profile and had some treatment but from our standpoint not as complete a treatment as could be done.

And, again, I think we ended up including that in the analysis in the White Paper. And there was some acknowledgment that certainly the TBD could be expanded. It hasn't been yet but that's certainly an issue.

Okay, consideration given to the firing sites. We already covered that in this last phase of the SEC. We went back and did more review and certainly recommended that that be closed at the last Work Group meeting on number 10.

Number 11, we closed that out -- the Work Group closed that out some time ago based on a White Paper that presented in terms of this question of the most exposed worker being batched from the external dosimetry standpoint.

The rest of these issues are petitioner issues. We've included these in the Site Profile reviews in the past. Number 12, in terms of accuracy of plant exposure data, was based on a 1980 Tiger Team finding.

And we resolved that in the Work Group from the standpoint of information being presented that in fact the most exposed individuals were captured in terms of the exposure. Now, that's not the internal side, but certainly outside the internal dose standpoint it was.

The same thing for too few monitors -- too few workers monitored. We included that in the White Paper. But that again focused on the early years as to whether the numbers of workers were sufficient for analysis.

Okay, number 14, and this is the end of the list, whether the records were sufficiently complete for subcontractors, temporary employees and short-term employees. Anyway, that was closed out based on a response by NIOSH to the Work Group that in fact all these Classes of workers were adequately covered. So the Work Group was satisfied that the subcontractors and temps and those workers did in fact have complete

records.

The petitioners in the petition raised questions on tritium leaks. And we did address that in the White Paper on data completeness and recognizes a lot of tritium data. So it was more what kinds of -- what was the approach to estimating some of the earlier tritium doses when you didn't have as many workers being monitored. And did you have the ability, given the lower number of workers being monitored, to in fact do an adequate dose reconstruction? Those were the kind of questions. But it wasn't SEC quality, more of what kind of factors are you using to make those estimates?

Number 16, badge placement. This is -- I think Wanda was talking about glove boxes. What we're talking about here is workers that had pits in their laps. And sort of this geometry was sort of a unique geometry for Pantex. And so I think, the review that we had, we presented that question as OTIB-10

wouldn't necessarily apply although we were open to hearing more about it.

I think the Work Group wanted to have more substantiation on how you would deal with a geometry where in the early days a lot of the workers actually handled these pits that closely and how -- whether or not the dosimetry would have from an external standpoint read properly or not. So I think it's sort of a takeoff from the discussion from this morning.

MEMBER MUNN: It is but there's certainly an enormous difference between handling something out of a glove box and handling it in the glove box.

MR. FITZGERALD: Right.

MEMBER MUNN: It's quite a different thing.

MR. FITZGERALD: That's why it's an open issue. Because again we felt OTIB-10 -- this is going back a couple of years ago -- would not necessarily apply unless it were

revised. So, anyway, that's an open issue.

And, finally, number 17 was sort of a bit of a catch-all. A lot of programmatic questions were raised in the Site Profile review, some of it from the Tiger Team reports about how bioassay programs were managed and those kinds of questions.

But they were all subsumed in earlier technical issues, addressed in earlier technical issues, whether it was internal or external. So, essentially, 17 was one of the very first ones the Work Group closed out as being sort of subsumed elsewhere in the list of Site Profile issues. That's just included to be complete.

And, again, in terms of a summary, maybe Brad could take this one. But the Work Group recommends full review by the Board on uranium and thorium from `84 through `90, and thorium for `91. And, again, acceptance of NIOSH's ability to dose reconstruct for `51 through `57.

And that would essentially close out all the time periods under SEC consideration, leaving us with Site Profile issues to tie up, essentially, for Pantex. And after five years I'm sort of satisfied to be able to say that finally.

Any questions on anything?

CHAIRMAN MELIUS: Yes, I have a question. It's in some ways more administrative but it relates here. Essentially, you're recommending to close out the SEC petition entirely. I mean, recommending moving forward an SEC on some and not on the `51-`57 period. And I'm a little puzzled trying to understand that when you've got a number of petitioner issues still open and a number of Site Profile issues open. And make sure that everyone is confident that those can be addressed.

For example, the statement here on `51-`57 is, well, you have enough air samples. Well, have you actually done -- has someone

demonstrated that those are actually adequate? Because essentially we are closing out this petition.

And, again, not that there's not ways of addressing that. But I'm just hesitating. And I hesitated when I wrote up the letter to really do that unless we were really certain that the Site Profile issues, or remaining petitioner issues, weren't going to affect that period and affect the SEC.

MR. FITZGERALD: Well, I certainly agree there's a couple of issues. I think tritium is one where there's a lot of data. But there's implications for smaller numbers of workers going back in time. But they don't go quite that far back because they didn't do any assembly or disassembly after `57.

We kind of looked at what operations were happening in `51 through `57 and essentially there was no assembly/disassembly. There was essentially depleted uranium being mated with high

explosives.

So the source term that we could identify was depleted uranium, essentially, in that time frame. And the question was, was there an exposure potential from the depleted uranium? We could not document one nor could we get any feedback from any of the workers that worked in that time period that there was in fact any real concern or any evidence that you were getting contamination.

Now, what -- you know, we certainly knew and understood that if you have unclad depleted uranium you by definition have oxidation that starts relatively soon. So the tough question was at what point would you have enough oxidation before the mating of the parts that you would be concerned about it?

And, frankly, talking to the workers that handled it, they did not pick up any contamination that they could measure. So it was kind of hard to hypothesize did you have enough that was beyond negligible? We

didn't establish that at all.

But the other source term questions you're raising, we couldn't establish those source terms figuring in the pre-assembly/pre-disassembly time period. That's why we focused on was there any reason you would have a radiological source term that posed an exposure potential, other than the burn pits and the hydroshots? And we could not establish any. And we did talk to workers that actually started at the plant from `51 forward. So is there any possibility? I don't -- there might be a possibility. But we didn't find any documentation on that.

CHAIRMAN MELIUS: Okay. I just couldn't tell from all the documentation. I just wanted to make sure.

Any other Board Member questions? If not, I would like to give the petitioners -- well, first of all, Board Members on the phone, Paul or David Richardson?

MEMBER ZIEMER: This is Ziemer.

My questions have all been answered. Thank you.

CHAIRMAN MELIUS: Thank you, Paul. David?

MEMBER RICHARDSON: It's been a really good conversation. I don't have any questions. Thank you.

CHAIRMAN MELIUS: Thanks. And then are any of the petitioners on the phone and wish to speak?

MS. RAY: This is Sarah and I'm on the phone.

CHAIRMAN MELIUS: Hi. Okay, go ahead.

MS. RAY: But I really don't have anything to say. I just hope that it will be approved for our workers. Thank you for your effort.

CHAIRMAN MELIUS: Well, thank you, Sarah. And any of the other petitioners on the phone?

(No response.)

Okay, thank you. So we have, essentially, a motion from the Work Group.

MEMBER CLAWSON: Right. And I'd like to give the opportunity to any other Work Group Members if they wanted to say anything on this. We're good?

What the Work Group has recommended is that January 1, 1984 to 1991, December 31, that Pantex be granted an SEC based on the depleted uranium and thorium and the inability to reconstruct dose. And this was a unanimous decision by the entire Work Group.

That's what's before the Board. I hope you understand the 1951 to 1957, that's being -- I'm looking at that's being left open because as you -- they still need to determine the ability to be able to -- if the source terms and so forth. That's not in -- this is not in the -- it's just open.

MS. RAY: And this would include all workers?

MEMBER CLAWSON: Yes.

MS. RAY: Am I correct?

CHAIRMAN MELIUS: Yes, it is all workers.

Okay, my concerns I think have been addressed by Joe. And so if the Work Group is recommending the `51 -- so I suggest we do it in separate motions just to make it less confusing --

MEMBER CLAWSON: Correct.

CHAIRMAN MELIUS: -- to people. But if everyone is satisfied in SC&A, the Work Group -- again, I didn't -- I was not -- I haven't read the transcripts. I wasn't part of the Work Group meeting so I can't say.

Let's do the first motion first, which is to add for those years all employees. And, Ted, do you want to?

MR. KATZ: Sure. Dr. Anderson?

MEMBER ANDERSON: Yes.

MR. KATZ: Ms. Beach?

MEMBER BEACH: Yes.

MR. KATZ: Mr. Clawson?

MEMBER CLAWSON: Yes.

MR. KATZ: Dr. Field?

MEMBER FIELD: Yes.

MR. KATZ: Mr. Griffon?

MEMBER GRIFFON: Yes.

MR. KATZ: Dr. Kotelchuck?

MEMBER KOTELCHUCK: Yes.

MR. KATZ: Dr. Lemen is absent, I believe, still. I'll collect his vote after. Dr. Lockey?

MEMBER LOCKEY: Yes.

MR. KATZ: Dr. Melius?

CHAIRMAN MELIUS: Yes.

MR. KATZ: Ms. Munn?

MEMBER MUNN: Yes.

MR. KATZ: Dr. Poston?

MEMBER POSTON: Yes.

MR. KATZ: Dr. Richardson? David?

MEMBER RICHARDSON: Yes.

MR. KATZ: Thank you. Dr. Roessler?

MEMBER ROESSLER: Yes.

MR. KATZ: Mr. Schofield?

MEMBER SCHOFIELD: Yes.

MR. KATZ: Dr. Ziemer?

MEMBER ZIEMER: Yes.

MR. KATZ: And Ms. Valerio is recused from this session. So it's unanimous with one vote to collect from Dr. Lemen. The motion passes.

CHAIRMAN MELIUS: Thank you. Okay, now let's go back to the `51 to `57 time period. And my recollection was that -- the question I asked was did any of the sort of outstanding Site Profile or outstanding SEC petition issues that Joe included in his list, did any of those apply to or would affect the `51-`57 period? And I thought the answer was that they did not. So if that -- and if the Work Group is comfortable in making that recommendation now -- I mean, I'm comfortable with it personally.

MEMBER CLAWSON: Yes. Joe did a

great job at it. But what I want the Board to understand is that there wasn't -- nothing ever happened until 1958. And that was the issue. But, you know, there's the question of the depleted uranium. But this was clean depleted uranium coming in and anybody that we've talked to, we couldn't see anything. So if you need a motion for the 1951 to 1957 or --

CHAIRMAN MELIUS: We already have one from the Work Group.

MEMBER CLAWSON: Okay.

CHAIRMAN MELIUS: Jenny, do you have a --

MS. LIN: No, just to confirm that there is actually a motion from the Work Group.

CHAIRMAN MELIUS: Yes, yes. Okay. So we have a motion. Let's do the vote.

MR. KATZ: To not add a Class. Okay. Dr. Anderson?

MEMBER ANDERSON: Yes.

MR. KATZ: Ms. Beach?

MEMBER BEACH: Yes.

MR. KATZ: Mr. Clawson?

MEMBER CLAWSON: Yes.

MR. KATZ: Dr. Field? Dr. Field?

MEMBER FIELD: Yes.

MR. KATZ: Mr. Griffon?

MEMBER GRIFFON: Yes.

MR. KATZ: Dr. Kotelchuck?

MEMBER KOTELCHUCK: Yes.

MR. KATZ: Dr. Lemen, I'll have to collect his vote. Dr. Lockey?

MEMBER LOCKEY: Yes.

MR. KATZ: Dr. Melius?

CHAIRMAN MELIUS: Yes.

MR. KATZ: Ms. Munn?

MEMBER MUNN: Yes.

MR. KATZ: Dr. Poston?

MEMBER POSTON: Yes.

MR. KATZ: Dr. Richardson?

MEMBER RICHARDSON: Yes.

MR. KATZ: Dr. Roessler?

MEMBER ROESSLER: Yes.

MR. KATZ: Mr. Schofield?

MEMBER SCHOFIELD: Yes.

MR. KATZ: And Valerio is recused. Dr. Ziemer?

MEMBER ZIEMER: Yes.

MR. KATZ: And it's unanimous. The motion passes.

CHAIRMAN MELIUS: Okay. So I have a letter ready for the addition but not for the `51-`57 period. I will do a separate letter. I'll circulate it to the Board before sending it forward. It will be pretty straightforward.

But for the `84-`91 period I'll read the letter as follows. The Advisory Board on Radiation Worker Health, the Board, has evaluated a Special Exposure Cohort Petition 0068 concerning workers at the Pantex Plant in Amarillo, Texas, under the statutory requirements established by the Energy Employees Occupational Illness Compensation

Program Act of 2000 incorporated in 42 CFR Section 83.13.

The Board respectfully recommends that SEC status be accorded to all employees of the Department of Energy, its predecessor agencies and their contractors and subcontractors who worked at the Pantex Plant in Amarillo, Texas, from January 1st, 1984 through December 31st, 1991 for a number of work days aggregating at least 250 work days occurring either solely under this employment or in combination with work days within the parameters established for one or more other Classes of employees included in the Special Exposure Cohort.

This recommendation is based on the following factors. Workers at this facility during the time period in question were involved in operations related to nuclear weapons production. The Board's review of available monitoring as well as available process and source term information for this

facility found that NIOSH lacks the sufficient information allowing to estimate with sufficient accuracy the internal doses from potential exposure and uranium related to the disassembly of weapons systems during the time period from 1984 through 1990, and to thorium in 1991, which employees working at this facility may have been subjected.

The Board also determined that health may have been endangered for these Pantex Plant employees during the time period in question.

Based on these considerations and discussion at the July 16th and 17th, 2013 Board meeting in Idaho Falls, Idaho, the Board recommends that this Class be added to the SEC. Enclosed is the documentation from the Board meeting where this SEC Class was discussed. The documentation includes copies of the petition, the NIOSH review thereof and related materials. If any of these items are unavailable at this time they will follow

shortly.

Any comments? Questions? So that will go forward. And as I said, I will circulate the other letter.

And I would like to thank Brad and the Work Group for all their hard work on this. I'd also like to thank NIOSH for their efforts. And also, I don't know if Greg is still here, but the Department of Energy for all their assistance. This has not been an easy site to evaluate. I also really thank the people at the facility. We got a lot of cooperation, a lot of assistance there over time. And frankly I personally was skeptical we'd get this far as well as we have in here.

I'd also like to thank the petitioners, both for all their efforts in putting this forward and also for the patience with the time it took to get here. But they did continue to advocate, continue to work hard for this, and hopefully this will benefit many of the workers at that facility who

worked for many years and as a result have developed cancer. But I'm glad that we were able to get through this and do it as well and as clearly as we could. So, thank you.

But, Brad, you've been patient.

MEMBER CLAWSON: Jim, I wanted to second everything you said. I'd personally really like to thank the DOE, NIOSH. This was a very, very complicated site. And we got a tour so that we could better understand that, that was very well put together and it really helped us understand how the process was.

But it was also an honor to be able to meet a lot of these people that all of this work that we're doing, all these other sites, this is where it all came together at the very end. And what a wonderful group of people. And Sarah Ray, we couldn't have done it without you. We're very thankful.

CHAIRMAN MELIUS: And I just recognized, I forgot to recognize Joe Fitzgerald who also did a lot of effort here.

So, thank you, Joe, and SC&A.

So, anyway, thank you. That completes our work here for the morning.

MR. HINNEFELD: I think that was Sarah wanting to say something.

CHAIRMAN MELIUS: Oh, okay, I'm sorry. I didn't hear you.

MS. RAY: I was just going to second what you said, or third it or whatever. But thank you so much for your efforts and for continuing on with this and looking at the broad picture. We so appreciate it. And it will mean so much to so many workers. Thank you so much.

CHAIRMAN MELIUS: Well, thank you.

So, we're early. We can break for lunch. We will break. I believe we're scheduled to come back at 1:30. We'll take up Brookhaven.

I believe, I don't know if it's actually -- it's not a petition issue but there may be people on the line. But I don't

think it makes sense to try to come back any earlier. So plan on 1:30 here.

(Whereupon, the foregoing matter went off the record at 11:41 a.m. and went back on the record at 1:33 p.m.)

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

1:33 p.m.

CHAIRMAN MELIUS: Okay, it's 1:30. We are reconvening the meeting of the Advisory Board on Radiation Worker Health. And do we have any announcements?

MR. KATZ: Just to check and see, Dr. Ziemer, Dr. Richardson, are you on the line?

MEMBER ZIEMER: Paul Ziemer on the line.

MR. KATZ: Good to hear you.

MEMBER RICHARDSON: David Richardson on the line.

MR. KATZ: Super. Thank you. And Dr. Lemen, are you on the line?

(No response.)

MR. KATZ: Okay.

MEMBER BEACH: Is Ron Buchanan on the line?

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

MEMBER BEACH: Oh, hi, Ron. Thank you.

CHAIRMAN MELIUS: Okay. We'll start with Josie Beach, Chair of the Brookhaven National Laboratory Work Group. And we're going to do Site Profile issues.

MEMBER BEACH: Okay. This should be fairly brief. As you know, in March we finished up Brookhaven SEC issues. The first slide just indicates again who the Work Group Members are.

So, just a bit of background. We do have an SEC for January 1, 1947 through -- all the way through 1993. It was based on lack of adequate internal dose records.

There was 13 issues for the SEC and we also had 13 Site Profile issues. Remember last month in March we voted out -- we finished the SEC based on the sampling that NIOSH was able to show the Work Group that they could accomplish the sampling on those five sample cases that SC&A came up with.

So, the remaining items. After the meeting in March SC&A completed the matrix for the SEC, which was sent to you. That came out just after the meeting, so you have that to show the work that was done and just to readdress all those issues.

The other one for the Site Profile issues came out in May. I'm sure you have that available also. And Ron is on the line. He is going to go through all those Site Profile issues for you.

One thing I do want to point out before I turn it over to Ron is on page 4 of your handout, under item 13, if you look at the last sentence -- and I guess I can go forward here -- there is a couple of corrections.

Item 1 should be -- or items 2 and 3, it should actually read items 1 and 2. And then the closure should be item 3. So instead of 2, 3 and 1, it should be 1, 2 and then 3. Just a quick adjustment so Ron doesn't have to

deal with that.

And with that, is there any questions on the SEC and where we finished up there? This really wasn't about the SEC but I wanted to just give you a quick brief overview.

Alright, Ron, if you're ready I've got your slide presentation up. Thank you, Brad. And we're ready to go with number 1.

DR. BUCHANAN: Okay. This is Ron Buchanan, SC&A.

Now, I'm on the -- I joined the meeting but all I have is the summary slide from Pantex.

MEMBER BEACH: Just a second.

MEMBER RICHARDSON: It's the same for me. This is David Richardson.

MEMBER ZIEMER: This is Ziemer. I'm seeing the same thing. I'm still seeing Pantex.

(Pause.)

MEMBER BEACH: Okay, so my

apologies. As I was getting prepared I neglected to hook up to Live Meeting.

Ron, can you see it now?

DR. BUCHANAN: Yes. Slide number 2 is coming up, yes.

MEMBER BEACH: And I just moved it to slide number 2, which is the start of your presentation.

DR. BUCHANAN: Right, slide number 3 is on now.

MEMBER BEACH: Okay, so we'll turn it over to you at this point, Ron.

DR. BUCHANAN: Okay, thank you. A little background here. We did the Site Profile issues first and there was 13 of those. And by coincidence then we got into the SEC and there was 13 of those. And so it's a little confusing but that's the way it was.

And so we addressed the SEC issues, and the Site Profile issues kind of sat on the back burner. And so once we got

the SEC issues addressed then we came back to the Site Profile issues.

And some of the same issues or parts of the issues were the same. And so some of these I'll refer to the SEC issues. And so we'll start out with the Site Profile issues number 1 and 2 because they're related. Can you go back one? There you go. Okay, that's right. Slide 3 is correct.

We have 1 and 2 there. And this was concerned with bioassay monitoring and records. And Brookhaven did not have a good centralized system to begin with, and so we questioned that. And then the SEC was granted through 1993 which took care of most of that. But then we were still concerned about `94, `95 and `96, in that area.

And so we worked with NIOSH, went back and looked at some of these bioassay records and also exposures and cases. And we closed that out under the SEC issues and found out there was sufficient data after 1993. So

that Site Profile issue 1 and 2 was satisfactorily addressed.

If there's any questions as I go through these, you can stop me. If not I'll just go onto the next one.

Site Profile issue number 3 there was the minimum detectable activity and uncertainties. We looked at that and we found that in the revised TBD -- there were several revisions to the Brookhaven National Lab TBD. And our latest one we looked at after the findings were posted.

We found that they did address the major radionuclides, expanded the time of coverage and included some other radionuclides. And we felt that item number 3 has been satisfactorily addressed by the revised TBD.

Number 4 was the characteristics of the radionuclides. And now the revised TBD did not give additional information on that. However, they did revise the TBD to instruct

the dose reconstructor to use the most claimant-favorable solubility, particle size, et cetera, that would create the largest dose to the organ of interest. And this is standard protocol for dose reconstruction, so we agree with that solution to that issue.

Number 5, to begin with, we noticed there was no internal coworker dose assigned. And this was also addressed in SEC issue number 12. And this goes back to our investigating the internal dose records. And we analyzed some cases to see if there was available data. And there was. And so we, at this time, found that there was no need for internal coworker data for BNL. And we feel that this issue has been satisfactorily resolved.

Now, number 6 is the only open issue at this time and that was the NTA film badge. As you know, NTA film has a cutoff at about half to 1 MeV. And so if you're working around moderated neutrons of lower energy then

it doesn't register it, or only registers part of the dose.

And so we brought this issue up. And as of April 30, NIOSH is to address this issue. And we will then review their recommendations when they're made available.

Number 7 is along the same lines as NTA track fading. And NTA film sets, the neutron detection film sets, it will lose its images. And so you have to do some compensation for that depending on how long it sets and what the energy of the neutrons are and such.

And so we found that this was SEC issue number 1 that had been resolved. This is where in the revised TBD they did recommend a fading factor of 1.8 before 1985. And going back over the literature for fading and some of the published information we find that this is a claimant-favorable factor and we agree with that.

After 1985 and forward, when they

used NTA film through `95 at BNL, we found that the Landauer records did show that they calibrated the film at the beginning of the exchange cycle and then read it at the end. And so this would compensate for any fading, and in fact it would be claimant-favorable and it was if the whole dose was assigned -- was acquired on the first day of exposure. And then it was read on the last day of when it was turned in. And so this would be claimant-favorable and we agree with that on issue number 7.

Issue number 8 was at BNL they had a number of neutron dosimetry systems. And they sometimes had them intermixed. Sometimes they'd use one, two, or three, or four in combination. And our question to begin with was -- and also in SEC issue number 3 -- was how was this dose recorded and which one was used?

And so what we did was investigated some of the readings. And we

found out that if there were several readings they always recorded the highest, or both of them, or all three of them sometimes in the records. And then so this highest reading could be used to assign a dose during dose reconstruction. And we had no further issue with that. That issue was addressed.

Number 9 is the potential exposures at the accelerators. Brookhaven had a lot of different neutron sources. They had accelerators of different configurations. And so this gave you a wide range of neutron energy.

And the NTA film and the other neutron detection methods have of course a certain range that they can detect neutrons efficiently. And we were concerned about there might be locations where there would be higher energy neutrons where the NTA film responds but would be a less than full response.

And so we looked over the possible

neutron sources, their energy ranges, and the percent of time a person would spend at the different locations such as the beamlines and outside shielding.

We found that BNL always used a quality factor, a biological conversion factor, a conservative value of 10. And most of the measurements around the accelerators showed a quality factor of about 5. And so there was generally over an estimate. And so if the detection system did have lack of sensitivity at higher energy then it was most likely compensated for. Plus, you would not stand in the beamline for a long period of time. So we felt that there was no additional need for adjustment factors beyond what was already being used. And so we considered this issue closed.

Issue number 10 was external coworker dose data. And at first we wanted to see if there was a need for external coworker dose data. And we brought this up to NIOSH.

And we find in the revised TBD that they did provide coworker external dose data from 1947 to 2010.

We looked at that data and found that it was derived in a claimant-favorable manner. And we evaluated some of the dose records and found that it was acceptable. And this provided for resolution of issue number 10.

Now, 11, of course, incidents and events was not thoroughly addressed in the original TBD. And so we find that in the revised TBD there was some additional revision and additional information.

And also we looked at the -- in evaluating these SEC issues on internal intake and exposures and neutrons and stuff, we did look at the potential field of exposure. And we found that probably the system that they used for dosimetry, internal and external, wouldn't satisfactorily address the issues of possible exposures. So we felt that that item

could be closed.

Now, there was one particular area that was pointed out in particular, that was the igloo area. Now, igloo area was kind of a storage area that they stored unused sources. It wasn't really a trash area, it was just a storage area that was made out of concrete blocks stacked and such.

And of course outside this gated area you could receive a higher dose than the general environmental area if you were using Section 4 of the TBD. And so we found -- we brought this up and NIOSH did address this in the revised TBD to include the igloo and the HWMF facility satisfactorily. So we felt that that issue could be closed.

That brings us to the last issue, number 13, and that had to do with the number and type of X-rays. And there was three items to this issue. And this is mainly wording of the tables so that it was less confusing and not ambiguous on when they should assign what

type of X-ray.

And so we had item 1 there, which was from Table 3-1. The wording on when they should assign certain X-rays if there wasn't records or if there was records. That was reworded in the revised TBD and it's clearer now. So we feel that item number 1 has been satisfactorily addressed. This is where the error on the slide is; item 1 is addressed.

Item 2 was the type of X-rays, whether they did the special X-rays as a condition of employment. So we brought that up. We looked at twenty cases and we found that what NIOSH recommended in the revised TBD agreed with what we found in these twenty cases. And therefore we considered that item number 2 could be closed.

Item number 3 was the wording and functionality, like when do you use what part of the tables. And Table 3-2 and Table 3-3 has been satisfactorily addressed in the revised TBD so that is clearer.

But we did find the 2013 revised TBD still did not contain any information referring to Table 3-4. 3-4 is a useful table. I believe it provides the skin, different organ doses as a function of time and such. And so it's a useful table but there was no reference to it, or where it came from, or when to use it.

And so NIOSH's response of the 30th of April of 2013 said they would -- the next TBD would add some information to make that clearer. And so that was item number 3.

And so that brings me to the summary. And so, Josie, do you want to do the summary slide now?

MEMBER BEACH: Sure, I'll take over. Thank you, Ron. Any questions for Ron on any of the Site Profile issues?

One thing I can point out is that NIOSH and SC&A are in agreement that the two issues that are left, once the new TBD is released, SC&A will take a minute to look at

them and that should completely close all the Site Profile issues for Brookhaven National Labs.

So eleven of the thirteen issues are closed. And I think, in summary, that's about all I can tell you. Any questions or comments?

(No response.)

I think we were pretty clear last meeting in March. Okay.

CHAIRMAN MELIUS: Good. And good job, LaVon, on the slides.

MEMBER BEACH: Yes. This is possessed, it's changing on its own so I'm going to leave it.

(Laughter.)

CHAIRMAN MELIUS: Okay. Thank you, Josie. And Ron, thank you also for that.

We have about 25 minutes until we need to start on Fernald. And we should wait because I believe petitioners may very well be on the line for this one. And we'll try to

stay on schedule for that.

I'm trying to think of any updates. Just a couple of sort of housekeeping items. One is that Ted and I were talking on Rocky Flats. I think plan on a 2-day meeting. It may be a day and a half. We'll try to pin that down. It obviously depends on what happens with some of the SECs that are coming. We don't have many in the way of 83.14s but there are some Work Groups out there that possibly could come through. Obviously, Rocky Flats will be a subject for that meeting.

We're not sure, Savannah River could also. So that's probably more uncertain in terms of that. So I think we can plan a day and a half at least, and possibly two days. But my guess is a day and a half. But we will try to pin that down by -- was it September 5th?

MR. KATZ: September 7th.

CHAIRMAN MELIUS: Yes, whatever.

We have a Board call so we will try to have a better idea by that time so people can plan travel, though I'm not sure we'll have permission yet on that. But at least in terms of your schedules and so forth.

On the coworker issue, I've been talking to NIOSH, SC&A and some others. And we will probably do some follow-up sort of Work Group, SEC evaluation Work Group meeting to try to spend a day in Cincinnati going over that issue and try and see if we can have some -- make some progress. I won't say resolution, but certainly some progress and then look at some of the applications or potential applications.

And so we'll probably come back to the Board at least with an update on that and the sufficient accuracy overall issue by the October meeting. And otherwise I think the main issue in terms of timing is going to be the issue of the -- where we are with some of the outstanding SECs. But we've got Pantex,

so the number of outstanding ones is dwindling. And if we can -- there's only a few outstanding ones, relatively speaking, left.

So why don't we take another short break and about -- and be back here right at 2:15 and we'll start. You don't even have to leave, you can stay.

(Whereupon, the above-entitled matter went off the record at 1:56 p.m. and resumed at 2:19 p.m.)

CHAIRMAN MELIUS: We're about to get started and make sure everybody that needed to be here is here. We are dealing with the Fernald site and the SEC petition there.

And first I want to -- Brad, do you want to say something or just let John go ahead?

MEMBER CLAWSON: John was going to start out.

CHAIRMAN MELIUS: Oh, okay.

MEMBER CLAWSON: And then we were going to go.

CHAIRMAN MELIUS: So, John Stiver, if you want to?

MR. KATZ: Just to note as John is getting ready that we're down a couple of Board Members. We're down Mark Griffon and Jim Lockey. Jim Lockey has recused himself from this session.

MR. STIVER: Good afternoon. I'm John Stiver with SC&A. And with Brad today we're going to give the Board a status update on the Fernald SEC petition review.

And we really want to break this into two components. There really are two open SEC issues at this point. They're quite different.

So the first I think we're going to go ahead and discuss to begin with which is the uranium coworker model as applied to subcontract employees prior to 1986. And the second is the thorium coworker model that uses

daily weighted exposures for 1954 to 1967.

But before we get too far into that, it's been quite some time since these, especially the second issue, was described. And we have some new Board Members on who really haven't been privy to a lot of the Fernald discussions. So for your benefit we're going to go back through, and just for the benefit of everybody, just as a review.

But this is the Work Group review. This is probably one of the most longstanding SEC petitions, if not the longest standing in the entire program.

April 2006, the SEC petition was qualified and the Class, the proposed Class, was all employees who worked in all facilities at the Feed Materials Production Center in Fernald from January 1st, 1951, through December 31st, 1989.

In November 2006, the Evaluation Report was issued and NIOSH found no part of the Class under evaluation for which it could

not estimate radiation doses with sufficient accuracy.

November 10th, shortly thereafter, SC&A released our Site Profile review and in July of 2007 our SEC Petition Evaluation Report review.

From August 2007 to July 2013, there have been a total of sixteen Work Group meetings. That's not a misprint, that is the real number, sixteen meetings.

And just for those of you interested in some of the early discussions, in May of 2011, prior to the meeting in St. Louis, Missouri, I gave a detailed summary and posted for review a whole series of documents related to the SEC issues up to that point. And the link is there in blue on this slide for those of you who are interested.

MEMBER MUNN: John?

MR. STIVER: Yes.

MEMBER MUNN: You might want to mention for the record that that link is not

available to the public. That's an internal link.

MR. STIVER: Yes, Wanda, that's a good point. That is an internal link to the internal intranet, the CDC intranet.

The status of the SEC issues. There were six original issues from our 2007 report. You'll see those that are indicated as closed with an asterisk based on Work Group recommendation. Some have been transferred to Site Profile discussions.

The two that are open, as I said, this is the first, the coworker model with uranium internal exposures, and number 6A, the DWE model for thorium intakes. And you see there's a note of conditional closure.

We had tentatively agreed in principle with that model back in 2010 with a caveat that NIOSH provide demonstration that their implementation strategy would indeed be acceptable.

And the chest count data, there is

an SEC that was granted based on the thorium chest count data that was reported in milligrams thorium during the years 1968 to 1978. And this is for all the workers at Fernald during that period of time.

This is open issue number 1. We'll go through kind of an overview, a history of it.

The concerns here regarded the completeness and adequacy, as usual, of all coworker models. And this is for the uranium bioassay data that was available for dose reconstruction that supported the internal dosimetry coworker model. This is OTIB-78. I believe Revision 2 of that model came out just this year.

The status of the issue. There's been countless White Papers exchanged, Work Group discussions from the inception of the SEC, discussions all the way through July 1 of this year.

At the July 1 meeting, which was a

teleconference, the Work Group did pass a motion to recommend to the Board that a Class of workers comprising subcontractor employees at Fernald from January 1st, 1951 through December 31st, 1983 be added to the SEC. And the next few slides will kind of flesh out the basis for that determination.

The central issues here are bulleted. We had subcontractors employed at Fernald from the beginning. In 1951, the Pilot Plant began, was up and running on kind of a -- well, it was essentially doing pilot studies and handling uranium.

From 1951 through `53, the other nine plants were being constructed. And so you have subcontractor employees there from the get-go. And you have uranium being handled from the start as well.

The subcontractors were not included in routine bioassay until 1986. And this is the year, kind of a pivotal year, when Westinghouse came in and took over the M&O

contract from National Lead of Ohio. And at that time they instituted a much more robust and site-wide health and safety program, including routine bioassay. And part of that was to include everybody, including the subcontractors.

Excuse me, my voice is a little shoddy today.

Prior to the March Work Group meeting -- oh, there was one thing I forgot to mention. One of the big issues here is the coworker model, which is very complete. And actually it was deemed adequate for the prime contractor employees from the beginning, which is really kind of unusual.

Most of the sites you find that they had an inadequate bioassay program in the early years, which is oftentimes the basis for an SEC. Well, Fernald is kind of an odd bird in that sense, in that they had a good bioassay program from the beginning. The only problem was it applied to the prime contract

employees and not the subcontractors up until 1986.

So you have this group of people, these subcontractors, who were not -- who have no data that are actually in the coworker model. And NIOSH was proposing to use that coworker model at a high percentile, say the 95th percentile, to bound the intakes of that subset of subcontractors.

Prior to the March meeting, DCAS did a data capture and located approximately 940 hard copy bioassay records. And this was for about 180 subcontractors collected over a 9-year period going from 1969 to 1985. These data were extremely limited and there weren't even enough of them available to make comparisons on the earlier years.

When we looked at that data set we did notice something kind of peculiar. There was a set of subcontractors in 1969 who had very high exposures compared to the prime contractors, but not only were they high, they

appeared to be a different type of exposure, as though they were involved in other activities that the prime contractors were not. And they had fairly high excretions starting in the month of July of 1969 and kind of tailing off through October, whereas the primes just had kind of a steady background level.

And so we thought it was worthwhile to do a kind of proof of concept comparison. And granted that these -- a lot of these workers were not claimants so you have no information available on their -- on the work history. All you have is a bioassay point and you have a date when it was taken. So the problem becomes how do you guesstimate a period of employment or a period of intake and so forth when you don't have any employment data?

So, what we did was kind of a proof of concept. What would you do if you had the bioassay data? Make your best

estimate judgments on what the period of employment would be. Find whatever kind of background data you can on that. And then compare that to what these workers would have gotten using the 95th percentile of the coworker model.

And if the 95th percentile bounded those intakes based on bioassay, that would be a pretty strong piece of evidence that the coworker model would be adequate and acceptable for these subcontractors.

In short, a total of I believe it was fifteen workers, there were nine of these non-claimants and then -- or excuse me, thirteen -- and four claimants. And these four claimants, who were drawn from this 180 workers in the earlier years, did have, in fact, employment data and they were picked because they had the highest excretion data of all the claimants that were available.

And so NIOSH went ahead and did a best estimate study. And there were two

aspects of this that we kind of questioned when we saw this study. It appeared that there was kind of an arbitrary distinction made about when the employment for the non-claimants ended. And they were assigned coworker dose far in excess of the period for which data were available.

And the same held for the claimants. In one example there was a claimant who worked from 1969 to 1974 and he only had bioassay data for 1971 for August. And yet he was given five years of coworker dose -- or intake. We didn't look at dose, we just looked at intake.

And even so, even with these kind of conditions and assumptions that favored the coworker model, it turned out that there were a good portion of these workers, in the claimants and non-claimants, for whom the coworker model was not bounding at the 95th percentile.

These are two curves or bar charts

here for type M and type S materials. The blue bars represent the actual best estimate based on bioassay and the red is the coworker assignments. You can see Worker 15 is the one that had one month of bioassay in five years of coworker data. So even so, especially when you get down to type S, you see that the coworker model is not bounding for most of those workers.

Now, the question then becomes, okay, since the coworker model is not bounding, how are we going to define a Class? This is after we've kind of agreed, as you can see on this slide, that the uranium coworker model is not bounding for the subcontractors prior to the mid-1980s. So we need to really define a Class period and look at the bookend years.

And we looked at the later years first. And NIOSH had done a comparison looking at all this data, this 939 samples, by year. And then they also had data from 1986

which did include the coworkers. I guess not the coworkers, the subcontractors.

And based on the fact that you have roughly the same number of individuals, and the number of samples per individual for `84 and `85 as compared to `86, there's pretty good evidence that you could construct a separate coworker model for the subcontractors for those two years, for `84 and `85.

Now, `83 was kind of an odd year because you started getting a ramp-up in subcontractor bioassay I believe the last 3 months of the year. And it coincided with a historical event. There was a Plant 9 dust release that made the news. Before that, Fernald was kind of off the radar scope. It wasn't really given much mind. But that really put it on center stage. And I think as a result of that there was an impetus on the part of management to really start doing a better job on doing bioassay and health and safety in general for their worker population.

So we figured that 1983 was probably a pretty good cutoff date for the endpoint. And then the question becomes what to do about the early years? Initially, we thought maybe `53 is a good year because that's the time that all the construction is done. They start -- all the other plants, other than the Pilot Plant, start receiving materials.

And one might presume that the workers in these other building construction projects would be working in, if not pristine environments, very low-level environments. And so we started looking into the SRDB to find any kind of evidence of that.

And what we found was just the opposite, that you had situations like described in SRDB 3230, as early as August of 1952, they said right there, the highlighted part, you had hundreds of contractors and subcontractor personnel running around loose in the work areas. And this is coming from

the health and safety folks themselves.

And so when you have that kind of a situation, and we have other SRDB evidence that there were -- there's a set of -- actually one of them, I believe it was Plant 9 in -- it was `52 or `53, and subcontractors, the construction workers, didn't want to go into the building because there was black oxide around. And so they had to do a swipe survey before these guys would go in.

So you had problems from the beginning. And we decided, the Work Group decided, that the best -- rather than to try to get too precise when it wasn't warranted, to just go ahead and propose the Class from January 1st of 1951 all the way through December 31st of 1983.

And that is really the end of the first -- all I have to say about the first portion of the discussion today.

MEMBER CLAWSON: From the Work Group, what I'd like to -- this came from the

Work Group. We had this one motion when we had enough people there for the subcontractors. And so what's coming from the Work Group here is, as John said, the Work Group passed a motion to recommend to the Board that the Class of workers comprise subcontractor employees at Fernald from January 1st, 1951 through December 31st, 1983 be added to the SEC Class. And this was a recommendation from the Work Group. For uranium.

CHAIRMAN MELIUS: So why don't we take questions on this issue now while it's -- because the other one is -- the other issue is a little -- is different. I don't want people to lose track of questions here. Bill, go ahead.

MEMBER FIELD: I just have a quick question. Who's in the Work Group?

MEMBER CLAWSON: What's that?

MEMBER FIELD: Who's in the Work Group?

MEMBER CLAWSON: Who's in the Work Group?

MEMBER FIELD: Yes.

MEMBER CLAWSON: I'm sorry. Let's see. We've got Paul Ziemer, Phil Schofield and Mark. Yes. Those are the ones that are on the Fernald Work Group.

CHAIRMAN MELIUS: And I actually listened in on at least this part of that Work Group call. Any questions? I just wanted to while it's fresh. Then we'll go onto the other issue, go through, and then we'll come back and take action. We also have to give time for the petitioners if they wish to speak.

MR. STIVER: Okay. Moving onto the next issue, which is a bit thornier. This is issue 6A, which is the reconstruction of internal exposures to thorium using daily weighted air concentration data.

And this is an issue that's been alive for about 5 years. And I'm just going

to give you the broad brush stroke overview before we start getting into the details.

And really this is the use of breathing zone and general air sampling data which were weighted by task for a certain set of workers a certain set of days. And this is going to -- NIOSH is proposing to use this data set to reconstruct intakes of thorium-232 to all workers at Fernald for the period 1954 through 1967.

The central issues, as always, revolve around data adequacy and completeness. Are there sufficient data there to bound the internal doses? And given that there are adequate data, is NIOSH's proposed method sufficiently robust to reconstruct doses according to the requirements of Part 83 for sufficient accuracy?

And the status, I want to jump ahead, there is a slide here that was misplaced. We should be up to 18 here. My apologies for not catching that earlier.

This is kind of a more detailed example of the five years of deliberations and different models that have been proposed and retired during that period of time. Obviously, numerous White Papers have been exchanged. There have been five revisions to the DWE coworker model during this period of time.

In March 2009 when I joined SC&A -- from SAIC, too many A's and C's in those terms -- I was tasked to look at the coworker model and the associated data for Revision 2 of the model.

And Revision 2 proposed to use the DWE data and break it out by job type, by building and by year. So you basically had three degrees of freedom. I realize that's not the proper statistical use of the term but we'll use it for our purposes.

We had about twenty findings in our review. Okay, does anybody know how to stop it from doing that? We'll see it that works.

(Pause.)

Okay. We had twenty findings regarding Revision 2. They related mainly to the modeling approach and to the lack of an uncertainty analysis. We also looked at data, and which we'll get to in a minute, that we were tasked to review. It was a small set of data. The Board felt that, based on the Revision 2 model and the criteria that were proposed, that it was not necessary to do a full-blown review of all the DWE data that were posted. Instead we were tasked to look at all the thorium buildings in the year 1955 and 1966, and then Plant 1 in 1960. We'll get to that shortly.

In October of 2010, NIOSH released Revision 3 of their coworker model. And this revision drew pretty heavily upon an uncertainty analysis that Adam Davis and Dan Strom up at PNL had produced back in 2008.

And it assigned the highest DWE for a given building in a given year basically

to all the workers in that building in that year. And based on the Davis and Strom recommendations, based on their uncertainty analysis, they gave the high DWE along with a GSD of 5.

And we agreed that, in principle, that approach was probably defensible. And recall this is back in 2010 when one size fits all models were being used routinely, as opposed to today where we have a much more sophisticated approach to coworker modeling and more of an emphasis on sufficient accuracy. So this kind of has to be looked at in the context of the time.

Finally, in November 2012, Bob Barton, one of our best analysts at SC&A, he did an analysis to just see whether it was really possible to place workers in given buildings in given years. And the results of that study were that, no, it was not possible.

So NIOSH went back to the drawing board and they came out with Revision 4 back

in February which is kind of a fallback to Revision 2. We had the same types of problems. And so at the March meeting we talked about it. They came back out with Revision 5. Maybe I'm jumping around too much up here.

Revision 5 was a lot like Revision 3. It was a one size fits all model but it dropped a degree of freedom. Instead of assigning the high DWE for each building and year, they just assigned the highest DWE for the entire site for a given year to everybody.

Okay, so it was assigned the high DWE for the entire site to all workers for each year for thorium production with no attempts to place workers in particular buildings.

And so let me get back to our original position. We're going to go through these in some more detail here. Get back to slide 11.

So let's get back to the actual

concept, as it applied to Fernald, of the daily weighted exposure. The Atomic Energy Commission's Health and Safety Laboratory had been using this approach since the 1940s. And they introduced it to Fernald in 1953. They actually did the first DWE study in 1953. And then they handed it over to the industrial hygiene and safety group at Fernald in 1954. And that group then conducted the DWE studies from that point on.

They did these air dust studies and produced reports which are summaries of the data prepared by the IH&S group. And they had estimates of the average worker exposure by job type. And they used this mainly to assess and control dust levels in the plant really to kind of improve industrial hygiene. They were not used to assess intakes, although they were kind of indirectly used to assess exposure in terms of what they called the maximum air concentration, the MAC, which at the time was 70 dpm per cubic meter. It later

shifted to 100 dpm per cubic meter, I believe, in 1960.

This concept was based on the gross alpha air activity concentration, or AAC for short. And it's really not applicable to just one nuclide. It could be applicable to recycled uranium, uranium, thorium, uranium and thorium progeny, basically any alpha emitters that were present in the workplace for which they wanted to get a handle on the potential exposures to workers.

What exactly is a DWE? It's a time-weighted alpha air concentration, job- and building-specific. There are several tasks per job. Like say you had a guy whose job was in metal production and he took the derbies out of the furnace and broke them open and cleaned them off and then transferred them down the line to be remelted.

And so for each task within his job they would give him a little breathing zone sampler. They'd follow him around and

sample in his breathing zone as best they could for each one of those tasks, for a certain period of time and take his replicates of those samples. And they'd do that throughout the day.

So there's a series of breathing zone samples that represent the tasks that typically involve the highest exposure potential. And then there's the general air samples, which are typically ambient, like say going to the cafeteria, or just the floor of the chemical room, or something along those lines which you would expect to be much more uncertain due to the concentration differences across a room.

The time to complete each task was reported. I should have put in a sample DWE report. I didn't for the sake of brevity. But what they would do then is to take -- these reports would have -- okay, here we go. They would take the high, the low and the average and report that for the different samples that

were taken for each task. They would then have the time for each task, the number of samples for the task and the number of workers who were involved in that job.

They would multiply the average air concentration per task by the time required to complete that task. They would sum all of those time-by-concentrations and then divide by the total time. So basically it's just a weighted average air concentration. And it's specific to the job. It's task-weighted.

The important thing is to keep in mind it really is an average for the workers who were monitored for the specific days in which they were monitored. And the time weighting, I can't emphasize how important that is because it really is the link between the air concentration and the exposure potential at any given time and place in the facility.

So in reality, you don't just have

one value, you have a whole distribution of DWEs. You have all the workers who weren't measured. You have all the variations that could possibly go into creating uncertainty in each one of these DWEs. And so obviously you have spatial and temporal variation.

And probably the biggest problem we had with the DWE reports in our original review was there was no uncertainty analysis provided. But as I said, in 2008, Davis and Strom came along and they reviewed -- they basically wanted to do some sort of uncertainty analysis to where these types of data could be used in dose reconstruction. And that was really the impetus behind the study. And before EEOICPA, I might add.

And what they did was they went out and they reviewed six HASL reports covering five sites. These were visited between `48 and 1955. They were looking at uranium, uranium or thorium, and radium and radon. I'm just going to kind of brush

through this. This isn't all that important.

They had 63 job titles. Each job title, one to twelve employees, for a total of 165 employees. Each job involved from one to thirteen operations characterized by one to 27 air samples for a total of 428 air samples. About 63 percent of these workers were exposed above the contemporary maximum allowable concentrations of the time.

They focused on the variability and the observations as evidenced in the air sampling data themselves. And they called out different sources of uncertainty and variability. Obviously, measurement uncertainty, mistakes in data processing and communication.

One thing that is important that is really not quantifiable in this kind of study was the representativeness of the air samples to what the workers actually breathed. All you have is what the workers were breathing at the time to the best of your

ability to measure it.

And so obviously there's variability in particle size, distributions, process variability, air sampling placement, changes in ventilation, and also self-absorption for below specific activity materials like thorium. If you've got a high dust load you may end up with a lot of self-absorption. So you may not be getting a true reading of what the actual activity was in that air sample.

They then ran Monte Carlo simulations assuming log-normal distributions to generate distributions of both discrete DWEs and log-normal fits. Obviously, the log-normal fit allows the possibility that the exposures in our interest would be larger than those actually observed.

The important part here is they determined the upper 95th percentile to GSD to be about 4, and the 99th percentile to be about 6 and 7. And they used that to support

a GSD of 5 when air concentration measurements are available, but there's no information on uncertainty, which is the situation that we find ourselves in.

A couple of other things here. They did indicate that just using the distribution of air samples without time weighting doesn't produce a DWE or a GSD that's really representative of any worker in the plant. In fact, you find that the unweighted samples obviously are going to be a lot higher than the weighted ones because most of the high air concentrations were performed over short periods of time.

And, indeed, the site-wide average concentrations were higher than the DWEs for all workers in the `60 to `63 cases. And I guess that's enough talk about Davis and Strom.

Let's start looking at the actual data availability and the buildings and years in which thorium was processed at Fernald. And

this chart was taken from Revision 3 of the coworker model. It shows thorium being processed in Plants 1, 4, 6, 8, 9 and the Pilot Plant over the years `54 through `67. Each one of those X's in the yellow highlight is a thorium plant and year.

And these are the data. They were actually requested and posted by DCAS. They posted 160 of these air dust reports for the plants that are identified in the previous slide, basically all those different plants. And they provided spreadsheets for a limited set of data that was requested by the Board which was, as I said, all plants in `55, `66 and Plant 6 in 1960.

And this curve here is just a probability plot of the data for 1955. It's just an example. This is for Plants 1, 4 and 9 for those years. A total of about 200 workers representing 88 jobs and 412 tasks.

Each one of those little blue diamonds is an individual DWE. So each one of

those is an average value that has associated with it some uncertainty. And so while it kind of resembles a probability plot, say, for bioassay, where each point would represent a one-point estimate, these are actually job averages.

So, if you were to just try to plot this and then pick off the 95th percentile and give it to everybody you might miss these top four job titles. So that 95th percentile would not even be representative of the average for those highest jobs. It would be quite a bit lower.

And so you're faced with the situation, well, how do we actually provide an upper bound? And the only plausible or reasonable way to do that is just to pick the very highest one. If you're going to bound it, you have to pick the highest DWE and then provide some uncertainty associated with that.

Now, Revision 5 of the coworker model proposes to use the limiting DWE for

each year in which thorium was produced. And this table here provides the high DWEs where available for each given year in each given plant. And you can see that DWEs are provided for all plants and years for which thorium was produced, with the exception of `54 in Plant 1 I believe -- I can't really even read my own typing here -- `55. All the years you'll see up to 1965 have DWEs for the plants that actually processed thorium.

You did not have data for the Pilot Plant in `65, `66 and `67. However, you did have it for Plant 8 and for Plant 1. We were a little concerned about this because the Pilot Plant was doing a lot of activities during this time. They were handling a lot of thorium in various chemical forms and they were doing remelting which was probably one of the dirtiest, for lack of a better term, one of the jobs that had the highest potential for dust exposure.

And in order to address that,

NIOSH went back and they found some breathing zone data for the years 1965 -- or actually `64, `65 and `67. They couldn't find any for 1966.

And so they did kind of a -- another proof of concept study where they looked at the unweighted air concentrations for the breathing zone samples only and calculated the geometric mean and standard deviations, as is usually done, and the 95th percentile, and then came up with an intake of nanocuries per day using the methods prescribed in the Revision 5.

And for `65 and `67, these 95th percentiles are actually lower than the corresponding DWEs, 95th percentile DWEs, for Plant 1. And so it kind of provides some assurance that we're not underestimating the intakes for those years in `65, `66 and `67. However, there is that is nagging concern about the year 1966.

I'd like to back up just one more

minute and talk about this slide here, because this is important. After listening to the discussions yesterday about sufficient accuracy and coworker modeling criteria I started thinking more about this data here. And there are really four things that kind of came out and stuck in my mind about it, about the modeling approach and about the data utilization that is kind of related to this sufficient accuracy.

In fact, you can kind of think of this as sort of a case study on when do you have enough sufficient accuracy to go ahead and use the model? And when is it just -- you're kind of right on that cusp of the point where you can actually say that your data is accurate enough for dose reconstruction.

The first has to do with the fact that you've got now a model, a one size fits all model, when you're only looking at one degree of freedom. You're taking a high DWE, a high value, and you're going to give it to

everybody in the plant for an entire year. So, in my mind, that requires a much more stringent data quality requirement if you're going to make that kind of assumption across the board.

The second thing is we've got a few years here where there's missing data. Now, you can say, well, you know, the Plant 1 data are probably adequate. But, again, you're going to apply this to everybody for the entire plant. And so that's still a little questionable too. I mean, you've got weight of evidence that it's probably okay but you don't have any definitive proof.

The other thing that we discussed the last couple of meetings, as you recall, we're not using bioassay data or individual monitoring data, we're using air sampling data. And so oftentimes you run up against plausibility. You wind up with intakes that are so high as to just not be plausible.

And we ran into that situation in

Plant 9 in 1955. And you can see the value that's assigned there is 215.1 and there's a couple of asterisks next to it. And if you read that little blurb there, the actual high value in 1955, all these values, came from Plant 9.

And this was a period of rapid -- of quick throughput metal production. They had a big order for thorium metal in 1955 and they were producing it as fast as they could. There are SRDB references that talk to this very issue. They report air concentration dust loads of 50,000 micrograms per cubic meter, 50 milligrams per cubic meter. And the DWE data we've looked at show that it's even considerably higher for some operations.

And so you come up to a situation where we did some research on this in support of Chapman Valve and a few other things. John Mauro had delved into it a few years back and found some good reports on what are the physiological tolerance limits for dust

loading.

And most of these reports indicate that about 100 milligrams per cubic meter is about the most anybody can stand for any length of time. And, guess what, it just so happens that 686 MAC is 98 milligrams per cubic meter. So you're putting somebody -- if you were to take that value even as a constant and assign it to everybody you're choking the entire population. It just doesn't pass muster.

And so the question became how do you deal with this? I mean, we've got real data. It's the real MAC. It's measured. We've got a dust report that goes with it. And the dust report said that respiratory protection, airline respirators, were worn for the high-dust operations. Of course, they don't tell you what a high-dust operation is.

But we started thinking about this. Well, I mean, if you were to take these DWEs, these high ones, you look through,

there's usually one or two samples, breathing zone samples, that are driving the train. They're very high transients typically over a short period of time.

And you know these guys were wearing respiratory protection during this time or they couldn't possibly breathe that atmosphere. And of course these are integrated over a period of time so you may have a short-term transient that's captured on the filter and then it drops down. But you're not going to get that fine level of refinement. And so the question becomes what do you do?

Well, NIOSH proposed one method which would be to take the DWE data and generate air concentration data from that And do Monte Carlo sampling and then generate an air concentration, an unweighted air concentration, using that. And then just not use the actual data. And that gives you a value, I think it was about 70 or 80 MAC. So

it was a pretty high value.

But we had problems with that. It was like, well, you're kind of going down the slippery slope here when you're taking actual data that's supposedly, or we were pretty certain is representative of actual exposures and because it's too high we're not going to use it; we're going to go to a model instead.

We started thinking, well, why don't you just take the actual DWE data and apply a respiratory protection factor? And that has its own problems. NIOSH traditionally has not used respiratory protection factors. Only in this case we argued that, well, you're not doing it to constrain intake, it's basically just to get to within a reasonable level which you know would have been the situation at the time to control the actual intake of dust alone and not necessarily to constrain intake or dose.

And so NIOSH came back. They went through and they applied a bunch of

respiratory protection factors, I think they used 10 and 100, to these high-dust operations. And sure enough, you drive the DWEs down from in the hundreds down to the tens and twenties.

But there was one sample here, which was the wet area helper I believe. Maybe I shouldn't talk about the different job titles. But it was one particular worker that had a one sample over about a 300-and-some minute period of time that just was not sensitive to respiratory protection. And that's where this 215 comes from.

And this kind of, while it might have been a subjective judgment, it represents a DWE that's within the realm of a reasonable intake. It translates to about 30 milligrams per cubic meter. So it's high but it's tolerable and it doesn't require the introduction of a respiratory protection factor. So that's the example of butting up against the upper limit of plausibility.

So, we have a situation where -- let me just move ahead. At the July meeting we kind of presented this. We kind of hashed it out. And I believe it was Mark Griffon who wanted to get a little more information on these. He wanted to know how long these dust studies were done. I mean, were they just one day? Did they go in there and do this on one day and then give it to everybody for an entire year, or was it over a longer period of time?

We didn't have that information handy, although it is available in the dust reports. And so we went through and compiled some statistics. We looked at the duration of the limiting plant studies.

And for four out of the fourteen it was indeterminable. You couldn't find any information on the time span. The others you see over in the far right column, the duration and days of the study range from, what, about 28 to a full year. Average about 135. And

then the center column, I believe the fourth one over from the left, the number of DWEs for the limiting plant average about 31, and the top number of job types evaluated average about 44.

So this seemed to be -- it's not a real definitive, conclusory statement, I guess, for lack of a better term, but it does show you that these weren't just one-day things. They were conducted over a period of time. The number of workers, tasks, DWEs is pretty well represented for each plant.

You know, there was one more thing I wanted to say about this slide right here. And this has to do with the tasking we had five years ago in support of Revision 2. And that was to do a full adequacy and completeness analysis.

And we did it for 1955 and 1965 -- excuse me, 1966. We added one, two, three, four, five and then Plant 6 in 1960. We had six of these building-year combinations

represented in our adequacy and completeness study. And so that is the fourth example.

So there's 20 others that aren't accounted for in our original adequacy and completeness study. And so while we felt initially that this was probably -- Revision 5 is a reasonable model, given if the Board accepts the one size fits all model under the conditions that exist, that it would probably be adequate for dose reconstruction.

There are those other aspects that relate to sufficient accuracy, however. But let's look at the bright side here first. You've got job types that are well represented. There appear to be sufficient DWEs for each year in the limiting plant to do a bounding analysis. You have a high GSD of 5 to account for uncertainties as based on a study of several different plants using DWE data. The air dust duration is available for a total of 14 years. As I said, it averaged about 135 days.

We have the proof of concept comparing the Pilot Plant air concentrations to the actual available DWEs, which appear to indicate that the DWEs are bounding.

And also in the petition there were some affidavits brought up, possible data falsification, that there are certain situations where they're trying to constrain readings to within the maximum allowable concentration.

And while there's really no way to prove or disprove that, we did note that the available data do appear to coincide with the known processes and the SRDB historic references. In fact, many of the DWEs far exceed the maximum allowable concentration.

And that combined with the fact that this data was not used to assess intake kind of leads us to believe that we're probably adequately represented. I know Bob Barton had done a study in relation to bioassay data regarding -- saying the same

kind of thing, data falsification.

And we went through that, oh gosh, probably about 3 years ago, again at one of our meetings. And the conclusion was it was really inconclusive. There was nothing you could really do to prove this. And so the determination was made by the Work Group not to pursue that any further.

So at this point this is kind of the end game here. Regarding bullet 2, I would say that at this point I would not be comfortable recommending model 5 until at least we have a better handle on those other twenty building and year DWE combinations.

And of course I think the big issue here is the policy issue of sufficient accuracy and the more stringent requirements for coworker modeling that have evolved and developed over the last three years.

However, as Brad mentioned earlier, the Work Group does recommend action regarding reconstructability for subcontractor

employees for uranium from 1951 to 1983.

And the remaining issues, I haven't gone into the Site Profile issues. There were originally thirty-three from our 2006 report, some of which have been closed out in discussions, others which have been added to through the SEC deliberation process. And so, before pursuing those, the Site Profile issues baseline, or matrix would have to be re-baselined.

So at this point I'll answer your questions as best I can.

CHAIRMAN MELIUS: Board Members with questions? Well, I'll -- go ahead, Bill.

MEMBER FIELD: Yes, I had a question. Let me just go back to where that was. I guess it's page 19. But the double asterisk where it talks about physiological tolerance level. Where did you get the information to come up with what would be tolerable? Where did that come from?

MR. STIVER: There was a paper by

Wes Van Pelt. And I can get that to you if you like.

MEMBER FIELD: Okay.

MR. STIVER: There's also another statement that John Mauro found. John, are you on the line by any chance?

DR. MAURO: Yes, I am. I'm listening.

MR. STIVER: Do you happen to have that reference for Bill Field?

DR. MAURO: Yes, I have it on my shelf. I'd have to pull it off. It was a study done where a statement was made by --

CHAIRMAN MELIUS: That's okay, John. You don't need to fly out here with it.

MR. STIVER: You don't need to FedEx it out here.

DR. MAURO: There are three -- I think there are three pieces of information, the Thorne work, the Van Pelt work and this one paper that we cite -- that's cited where it discusses this issue that led us to sort of

converge.

They were all independent, by the way. They sort of converged around this number of when things get a little difficult for you to stay in that breathing area for an extended period of time.

MEMBER FIELD: Okay. So it's an extended period of time and it assumes no respiratory protection, even a cloth or something over your face.

DR. MAURO: Correct.

MEMBER FIELD: Okay.

CHAIRMAN MELIUS: Leave it on the same page because my comment is making sure I understand this and what is being proposed. And hopefully everybody else does also.

So what this Revision 5 model is would take the highest value for anybody in the plant and apply it to people for that year. Is that correct?

MR. HINNEFELD: This is Stu Hinnefeld from DCAS and I am obliged to remind

everyone that I am conflicted at Fernald, in that it may be perceived that I have an apparent potential loss of impartiality having worked at the site for 20 years. However, I've been authorized for participate in these discussions by HHS Office of Ethics.

So, having said that, the question again was about the use of the DWE approach. And there were a number of discussions over a long time about what's the appropriate value.

It's relatively clear that we have little success, or we can have little confidence in putting a worker in a specific plant for his extended work area. So, not being able to do that, it did not seem feasible to use a plant-specific DWE for the people who worked in that plant, just didn't know where it was.

So, since we couldn't do that, the next bounding step would be to take the maximum DWE in any given year, because these are kind of -- yearly you can understand

variation because of changes in workload. For a given year take the highest DWE, and these were measured per job. Like someone might be called a bottom helper. And take that highest DWE value and say that is the bounding thorium exposure for that year.

The uncertainty that's associated with DWEs, because it's not like a film badge that somebody wears all the time; you go out periodically and you take these samples. And there is the Strom -- or the Davis and Strom paper described the kinds of various uncertainties and arrived at the conclusion that a GSD of 5 applied to your DWE value probably is a suitable distribution. It's quite a large distribution. So that was the basis for arriving at where Revision 5 arrived.

CHAIRMAN MELIUS: And I guess my question, really more of a concern, is we're taking this single value for the facility for a given year and applying it to people

throughout the facility because we don't have information to place them for that given year. So that means it applies to people working, say, for example in Plant 8 for most of this time period where there was no thorium exposure within the plant. And, you know, it varied. John's presentation showed the different years and it's quite a wide range here. So we're having people with no exposure being given this bounding dose. And while I may have some questions about the bounding dose I think that's at least generally probably in the ballpark. You've done a lot of work on that.

But is it really plausible to apply that as a coworker model for everybody in the facility? And I personally don't think it is a very plausible approach for this particular exposure.

And I think, Stu, I think you hit the main factor in that, which is that we can't place people within the facility. We

don't know where they worked.

In addition, I mean, there are limitations in terms of where sampling was done. But at least if you could place people then sampling was generally done within buildings where thorium is used. But without that, it's one number, you know, for everybody.

And even if you look at this slide, there's some pretty significant differences between what was the highest value in two or three different plants. I mean, an order of magnitude, two orders of magnitude difference. And so you're going to have people over a very wide range being given this one single value. And I don't think that can be said to be used to support, you know, accurate dose reconstruction. I think that's just stretching it over the line.

But I also want to make sure I haven't missed something about the process or what's gone on here. Henry?

MEMBER ANDERSON: And there's absolutely no bioassay data at any time in the facility?

MR. HINNEFELD: Well, not for thorium. Thorium is a really difficult bioassay. In vivo monitoring started in `68. That's the first period and that's been dispositioned as part of a Class already because of the interpretation of the milligram reported data.

MEMBER ANDERSON: Yes, okay.

MR. HINNEFELD: Thorium is a very tough bioassay.

MEMBER ANDERSON: Yes, I know. At the other sites we've had this has been the sticker. So I'm not -- you know, what would be useful is if you had another facility that had similar measurements so you could actually get a sense of that.

I mean, it's almost like you look at the number and you say, well, that's -- you know, you have to accept that they actually

measured it correctly when they did it. And then you get these where, you know, couldn't sustain life if the people were breathing the dust. And then you say, well, we will have to adjust that, as opposed to, well, how do we know some of the -- what does the quality of the count or the measurements? I mean, are they overwhelming because of all of the total dust that you really can't use the methodology of the dust counts.

MR. HINNEFELD: Okay, are you talking about the 1955 year?

MEMBER ANDERSON: Yes. Yes.

MR. HINNEFELD: Well, that was --

MEMBER ANDERSON: I mean, the others seem to be, you know, somewhat comparable so that you have greater confidence that what they were doing is -- unless, you know, there was an upset at some time and therefore somebody went in and that's -- I mean, if you have any idea about that one --

MR. HINNEFELD: Well, 1955 --

MEMBER ANDERSON: You know, the 686 MAC. I mean --

MR. HINNEFELD: -- 1955 is, if you look at all the DWE data from 1955 it's pretty extraordinary. I mean, these numbers were enormous. And one of the SRDB references that we found was a letter written from an employee of HASL, Health and Safety Laboratory, to his boss describing a visit he had just made to Fernald and it was about thorium exposures at Fernald.

And the health and safety director at Fernald was a former colleague of these two guys' at HASL. He had previously worked at HASL. And it sounds like the person who wrote the letter said, hey, Joe, what are they doing? What the heck's going on with these thorium exposures?

And the health and safety director at Fernald had explained to him, well, we had this crash program, et cetera. And so the HASL writer is writing to his boss what he

learned in his conversation with the health and safety director at Fernald.

So this was -- I mean, the exposures at 1955 in Plant 9 really look like they were extraordinary by all the evidence we've seen. And it wasn't like there was one value at 686 and all the rest were down around 20. There were 400, there was 200. You know, there was -- it must have been something to see.

MEMBER KOTELCHUCK: I'm going back to slide 19. I'm less worried about the 215 value for one year than I am for the fact that for most of the following years we're assigning a thorium exposure to people who never worked with thorium. And I accept that we're trying to see if we can do a dose reconstruction.

But, you know, for 1958 there was one measure -- I'm sorry, not one measurement -- there is one plant for which a daily weighted exposure was there. And there are so

many plants, so many parts of the plant where people were not using thorium.

It just seems to be such a push to make assignments of something that anybody working there will say I never worked with the stuff.

Now, to be sure, this is claimant-favorable. But I would expect that people would say, look, let's just be -- workers would say let's just be fair. Those people who were exposed, let's do the best estimate that we can. Those people who weren't exposed, don't do me a favor. Just get something, quotes, "right."

And so it's, as I say, I think it just seems too much of a push to assign one number to every single person in the entire facility for each year.

MEMBER ZIEMER: This is Ziemer. Can I make a comment on that?

CHAIRMAN MELIUS: Yes, go ahead. I was going to call on you in a second. And

then Wanda also wants to make a comment.

MEMBER ZIEMER: Well, the description that we just heard is something we have over and over again in many different sites. If we can't place the people in those plants, for example, a Plant 8 worker, I think we have to say you can't really place them strictly in that plant. They may have been in Plant 1 or Plant 4, Plant 9 as well.

It's not unlike what we've had at many sites. We've had that at General Steel, we've had that at GE. I think we had that over and over again. If you can't place the people you've got to make the assumption that they had access to the other locations.

CHAIRMAN MELIUS: Yes, but then, Paul, applying a coworker model based on that and saying that's sufficient is I think where it's -- that's, in my mind personally it's stretching it. I just don't think that's what should be -- that's not sufficient accuracy. And I think based on the facts that we have.

And but I think you're right. The key fact here is the inability to place people within buildings. I think if we could place them within buildings then we'd be talking very differently about this approach and so forth.

Wanda, you've been patient. Or at least trying to be patient.

MEMBER MUNN: That's not an easy task, especially since I swore I wasn't going to complain. But now I'm going to complain.

I have had a very, very difficult time hearing some of the presenters and certainly anyone who's on the phone. There's a muddy quality to the echo and the tinny business that makes it very difficult to concentrate on the quality of what's being said, the words that are being said. And as a result I'm fearful that I'm missing a great many specific words that I need to hear.

Stu, can you remind us on this slide 19 that we're looking at what the units

are?

MR. HINNEFELD: The units on this slide are in multiples of what was called the maximum acceptable concentration, which at the time was -- and I think these have all been normalized to 70 dpm per --

MEMBER MUNN: Per square meter.

MR. HINNEFELD: -- per cubic meter. Cubic meter.

MEMBER MUNN: Yes.

MR. HINNEFELD: Seventy dpm per cubic meter.

MEMBER MUNN: Per meter. Alright.

MR. HINNEFELD: So, these are multiples of that value. So 6.1 would be somewhere around 420 dpm per cubic meter.

MEMBER MUNN: Okay. Okay, that -- I was trying to remember whether it was a flat surface but I couldn't see that it would be if it were going to be air concentration. Cubic meter.

MR. HINNEFELD: Right. No, it's

cubic meter. It's an air concentration.

MEMBER MUNN: Okay, thank you.

CHAIRMAN MELIUS: Brad?

MEMBER CLAWSON: I just wanted to clarify one thing in one of John's. The Work Group didn't have a recommendation here. We did not have a quorum at this time. This is why this is coming before the Board without a recommendation from the Work Group because we did not have a quorum.

I want people to understand what the complexity of this site is. And I'd also like to thank Stu because he has helped substantially.

One of the things about this plant is we've got urinalysis running out of our ears. We don't have anything else. They ran this as a heavy metals plant. That's it. This is what they were for.

And we've got a lot of processes that can go into this, and we've tried to go to the most claimant-favorable we can. But at

some point too also, and this is my personal opinion, we need to look at what the SEC was there for.

Plausibility falls into this. The more we look at these models and everything else the wider it gets. And I personally think that we should be using the SEC for what it was for.

That being said, I want to emphasize that the complexity of this site is just unbelievable. And it has been a challenge from all sides to be able to do this. But this is why we're coming before the Board with this, is because it comes down to what are we going to do with this?

And I will say, personally, and this is only my opinion, not the Work Group's, that we should be using the SEC for what it was for. We can put all these values out there, but to tell you the truth I would have a hard time justifying it, really.

CHAIRMAN MELIUS: Thanks, Brad.

Gen?

MEMBER ROESSLER: Since Wanda mentioned it I will also make the comment. I've heard the comments in the room but the sound coming in from the phones is really difficult. And I'm afraid I may have missed something.

Could someone who heard Paul, Paul's comments, give us the essence of what he said? I really would like to know, since he's a Member of the Work Group and participated all this time I'd like to get the essence of what he said.

CHAIRMAN MELIUS: Maybe we can -- Paul, do you want to repeat your comment?

MEMBER MUNN: In terms that you would use for a person for whom English was a second language.

CHAIRMAN MELIUS: Wanda, please don't --

(Simultaneous speaking.)

MEMBER CLAWSON: What? Paul, we

couldn't hear you. We're sorry.

CHAIRMAN MELIUS: Wanda was --

MEMBER ZIEMER: How is the sound?

CHAIRMAN MELIUS: It's better here.

MEMBER ZIEMER: I was simply pointing out, I think it was in response to David's comment, that we have this situation frequently where, although, for example, it appears that people in Plant 8 didn't get exposed most of the time, if we can't really place people in that plant, they may have been at any of the others, then we have the situation like we have at so many other facilities.

We have a situation like that at General Steel where we can't specifically place people. We have had that situation in General Electric, in many other plants where we assign the maximum dose to virtually everybody simply because we can't place them.

CHAIRMAN MELIUS: And I would just

add to that that we've taken different actions in those different facilities. And some that are issues of the Class Definition after we've made a determination one way or the other as to whether or not there's adequate data.

I think the problem here is that what's being proposed is a coworker study that's attempting to use, you know, what data is available but without enough specificity of available information to place people within different parts of the facility. So it has very, very few factors. It's just basically taking one number and applying it to everybody when we know that there's a wide range of exposures here.

I think we had, again, a similar situation, for example, with the Linde plant where we had people doing construction work and fairly good data on that, and people in other parts of that same facility who would have had to be -- with no data. And do you apply essentially people doing renovation to

people doing other kinds of work.

Here, if we were able to do -- we were taking Plant 1, it appears that for Plant 1 for most years we have adequate data, but we can't put people in Plant 1.

So I think we have to judge it on a case-by-case basis. It is something we commonly run into but I don't think there's any general rules as to how we've dealt with it, because it depends on the situation that's involved.

I would -- do you have another question, Gen? If I turn my mic on we could all hear.

What I'd like to do is give the petitioners a chance to comment if they wish to and then we'll come back.

MS. BALDRIDGE: Yes, this is Sandra.

CHAIRMAN MELIUS: Hi. We can hear you, Sandra. Go ahead.

MS. BALDRIDGE: Okay. For the

benefit of the new Board Members, I'd like to give a brief understanding of why the petition was filed.

There were two primary criteria that I felt were especially important. And one was the Plant 6 thorium processes that were not recognized in the Site Profile.

Secondly was the data integrity with a focus on the manner in which air monitoring data was obtained and recorded.

Now, for a brief history, very brief, Fernald was asked to begin stockpiling thorium in 1956. It became the official national thorium repository in 1972. So there was a lot of thorium onsite.

Some of it was in storage, and the documents show the issues that arose there, in addition to the processes that were going on in various parts of the facility.

Eight years ago I discovered this discrepancy between a historic documents and the Site Profile concerning the Plant 6

thorium processes from 1960 to 1964. To date not a single worker has had this exposure added to their dose reconstruction. That's eight years and they still have not been dosed for the thorium exposure. And that's the thorium in processing as well as the thoron exposure that these workers received.

And I think that's the most distressing part of this whole process is even though provision is made in the law, no one has availed themselves of redoing the dose reconstructions for the Plant 6 1960 to 1964 workers.

And that's basically all I have to say. And I thank you for the opportunity.

CHAIRMAN MELIUS: Thank you, Sandra. So, let's continue our discussions. I would like for, I think, purposes of maybe making this a little easier to talk about and deal with is to have us first deal with the recommendation from the Work Group. And then we'll go onto the second issue, the one we

just talked about, and deal with that separately. We can figure out how to handle it as we go along. Yes?

MEMBER FIELD: It's my understanding there wasn't a recommendation. Is that correct?

MEMBER CLAWSON: No, the construction workers. That was the recommendation from the Work Group.

CHAIRMAN MELIUS: We just again, all the recent -- so we have a recommendation which is essentially a motion from the Work Group to add that Class, which is all employees of subcontractors who work at the Feed Materials Production at Fernald from January 1, 1951 through December 31, 1983. That's subcontractors.

Now, there was discussion that I overheard, listened in on with the Work Group, as to could subcontractors be better defined. And it really I think was decided, as I understand it, that that was not possible.

Because there's a variety of different subcontractors. The vast majority are people doing construction work there. For purposes of implementing that, it requires all subcontractors. Yes, John.

MEMBER POSTON: Jim, sorry, I'm an old man. I'd like to have a complete statement of what it is we're about to do for the record so that I can understand what it is, how I should vote.

CHAIRMAN MELIUS: That's fine.

MEMBER POSTON: Okay?

CHAIRMAN MELIUS: Okay. The motion would be to add to the SEC all employees of subcontractors who worked at the Feed Materials Production Center in Fernald, Ohio, from January 1st, 1951 through December 31st, 1983 for the number of work days aggregating 250. The normal part of adding. So we're adding that Class to the SEC. That's what the motion is.

Any further discussion on that

motion? Or questions? Yes, John.

MEMBER POSTON: Was that recommendation unanimous from the Work Group?

MEMBER CLAWSON: Yes, it was.

MR. HINNEFELD: I believe Dr. Ziemer was absent from the Work Group meeting. So it was unanimous among the Members who were there.

CHAIRMAN MELIUS: Okay.

MEMBER CLAWSON: That is correct.

MEMBER POSTON: May we ask Paul to --

CHAIRMAN MELIUS: Yes, that's fair. Paul, are you on the line?

MEMBER ZIEMER: Yes, I'm on the line.

CHAIRMAN MELIUS: And do you have anything you wish to say about that?

MEMBER ZIEMER: No, I actually I had to be absent. That was the meeting on the first of July, I believe, when they actually voted on this. But I do support this addition

to the SEC.

CHAIRMAN MELIUS: Thank you, Paul, for the clarification. Any other questions from Board Members? If not, Ted, do the roll.

MR. KATZ: Dr. Anderson?

MEMBER ANDERSON: Yes.

MR. KATZ: Ms. Beach?

MEMBER BEACH: Yes.

MR. KATZ: Mr. Clawson?

MEMBER CLAWSON: Yes.

MR. KATZ: Dr. Field?

MEMBER FIELD: Yes.

MR. KATZ: Mr. Griffon is absent so I will collect his vote. Dr. Kotelchuck?

MEMBER KOTELCHUCK: Yes.

MR. KATZ: Dr. Lemen is absent. I will collect his vote. Dr. Lockey is recused. Dr. Melius?

CHAIRMAN MELIUS: Yes.

MR. KATZ: Ms. Munn?

MEMBER MUNN: Yes.

MR. KATZ: Dr. Poston?

MEMBER POSTON: Yes.

MR. KATZ: Dr. Richardson? David Richardson, are you on the line? Okay, he is absent. I'll collect his vote. Dr. Roessler?

MEMBER ROESSLER: Yes.

MR. KATZ: Mr. Schofield?

MEMBER SCHOFIELD: Yes.

MR. KATZ: Ms. Valerio?

MEMBER VALERIO: Yes.

MR. KATZ: And Dr. Ziemer? Dr. Ziemer indicated he was supporting it. Dr. Ziemer, your vote? Dr. Ziemer? You might have put yourself on mute. I know he said he supported it, but this is a formality that you really shouldn't forgo. Dr. Ziemer? Okay, that's fine. I'll record him -- if he comes back I'll get his vote when he comes back. But I'm going to record him as absent at this point.

MEMBER CLAWSON: Hold on, I think he just got on.

MR. KATZ: Dr. Ziemer?

MEMBER ZIEMER: Yes, I got dropped.

MR. KATZ: Okay. You vote -- did he say his vote just now?

MEMBER ZIEMER: Yes. I vote yes.

MR. KATZ: He votes yes. Very good. Okay. I can't count so quickly but it's unanimous for Members present. We have more than a quorum and it passes.

CHAIRMAN MELIUS: Paul, if it had been a closer vote we would have really thought somebody would have been up to something. You're living close to Chicago there. They have strange ways of voting.

MEMBER ZIEMER: Right. Vote often, vote early.

(Laughter.)

CHAIRMAN MELIUS: Right. And suppressing the vote also. Okay.

The second issue we don't have a motion on. And I guess I would like to start off discussion. Is there any additional

clarification or anything that would be helpful to people in terms of information?

I will add that I talked to Stu briefly during one of the breaks today. Correct me if I'm wrong, Stu, but this is Revision 5. So there's been a lot of effort up to this point. But it doesn't appear that there is an alternative approach that would be available here now.

I told Stu to think about that so I want to give him the opportunity to say. And I don't know, John, if you have anything to add.

MR. HINNEFELD: Well, after a several year process of getting to this point I didn't think of anything that hasn't been thought of in those several years in the last half hour. So I don't have anything else to propose, no.

CHAIRMAN MELIUS: And John?

MR. STIVER: I would have to agree with Stu on this. I think we've come to the

endpoint on it.

CHAIRMAN MELIUS: Do Board Members have any questions for clarification? I think John Stiver's presentation sort of laid out the basic facts. There's certainly been a number of White Papers, a lot of work done on this, again, a lot of those being revisions trying to come up with a better method that would fit the situation.

Yes, Henry?

MEMBER ANDERSON: Just briefly. I mean, this is a site that's been around for a while. What is the status from the beginning of the site to now? I mean, we add it to the SEC. How much of it is currently in SEC? And components of it. Are we -- you know, where does this -- we've got the whole petition but we've sort of broken it up into parts. Do we have a good sense of --

MEMBER CLAWSON: We have an SEC from 1968 to 1978.

MEMBER ANDERSON: For the whole

facility?

MEMBER CLAWSON: For the whole facility.

MEMBER ANDERSON: Okay, that's what I wanted to hear. Yes.

CHAIRMAN MELIUS: Any other questions? Yes, John.

MEMBER POSTON: I'm thinking as a technical person here. And I have to say that I agree with Dr. Kotelchuck's remarks regarding the situation, which to me is no different than what my colleague from the great state of Idaho suggested, and that is we have an SEC. I don't see the difference between allowing everyone to be treated equally and just saying let's just do an SEC. It will save us a lot of money and a lot of time if we do it that way.

So I don't understand the difference between taking the recommendation that's on the floor and taking the recommendation that Brad made that we just go

directly to an SEC.

CHAIRMAN MELIUS: Josie?

MEMBER BEACH: Well, along those lines, I guess my question is what work can continue to resolve this issue other than what's been discussed as an SEC or NIOSH's recommendation?

CHAIRMAN MELIUS: And I think the answer we got is that there doesn't appear to be any new ideas or new approaches. And this is Revision 5 and a lot of work has gone into this. Wanda?

MEMBER MUNN: One thing that I should have asked earlier and didn't was clarification of the difference between blank spaces and N/A's on this particular chart that we're dealing with on slide 19.

MR. HINNEFELD: I'll take a shot at that and if I'm wrong John can correct me. A blank spot or a dash indicates that thorium was not used in that plant in that year. An N/A is that thorium was used in that plant in

that year but there was no DWE value generated for that plant in that year.

MEMBER MUNN: Now, that's helpful because that tells us that in Plant 8 during only one year was thorium even present. And it also tells us that in Plant 9, after this year in which the extremely high measurement was actually taken, there was no thorium.

Again, one of the thorniest problems that faces the question of whether to promote an SEC or not for any of these sites has been the gotcha of, yes, you don't know where everybody's been. And that's certainly a big gotcha here. We don't know where everybody has been.

It appears that in most situations where we would be looking at this kind of data, I believe that most people would come to the conclusion that a single year's information in a place where nothing else was being done with that particular radionuclide later was too much of an impact to expect it

to be included in the general recommendation that's being made for the entire site.

But in our case here in this Advisory Board, I don't think we have that option that we could do an academic exercise. We just don't have that option.

The question is well-posed and what else can you do? Apparently nothing. And if that's the case then we're at a dead end. It appears we will have to accept an SEC regardless of the fact that this outlier seems to be outrageous and in most cases would simply be rejected as erroneous in some way, or not applicable to the rest of the site.

CHAIRMAN MELIUS: I think the question is whether they can be bounding and plausible. And the group's -- all the work that's been done has been trying to make it bounding given the situation. But without being able to place people in the facility it is not plausible for those.

And, again, as I said before, if

we had people in the facility then and knew what building they worked in then I think we'd have an entirely different story. Because there are some gaps but I think those gaps probably could have been filled. But without that we can't do this. And I think the only feasible method was some kind of a coworker model, and that's what they've been working on. And I just don't think that's, at least, again, personal opinion, that's plausible. So if we're going to move forward we're going to need a motion.

MEMBER CLAWSON: As the Work Group Chair, I'd like to say something on this. Because I don't think that you guys really see how much work really went into this. Sixteen meetings and we have gone completely full circle on all things.

And it's not without a lot of trying and a lot of digging. And I've said this earlier, I've got to admit that NIOSH, we've worked back and forth. But I see no

other way.

And as the Work Group Chair, and this is why I wanted to make this clear, I am bringing forth before you at this time, and what I'd like to present is that we give Fernald Feed Production Plant an SEC from 1954 to 1967 for the inability to be able to reconstruct thorium dose.

MEMBER BEACH: And I'll second that.

CHAIRMAN MELIUS: Okay. We have a motion and a second. Further discussion? Stu, were you going to --

MEMBER ZIEMER: Ziemer has a question.

CHAIRMAN MELIUS: Go ahead.

MEMBER ZIEMER: I just wanted to ask NIOSH, and I didn't know if Stu or one of the others there, it wasn't clear to me if NIOSH's position is now that in fact they cannot reconstruct dose? Or does NIOSH still believe they can?

Because my understanding of the high value for `55 was that that was considered at least plausible in Plant 9. And if it's plausible for Plant 9 then it is also plausible for any others who would have entered Plant 9. Could you clarify where NIOSH stands on the general issue of both reconstruction of dose and plausibility?

MR. HINNEFELD: Well, I mean, our position has not changed since we presented Revision 5 to the Work Group, that this is a method that provides a bounding dose to the workers there.

And these AWEs are quite high but they were plausible for the person that it was measured for. I mean, these AWEs were measured for some guy working some task, or some people working some task, at the time those measurements were made. It was plausible for that person.

And so our position is we presented a plausible bounding dose for the

people who worked with thorium during these years. It's not changed since we presented it to the Work Group.

CHAIRMAN MELIUS: But the second part of Paul's question was I think that it was -- I thought you were asking if it was plausible for Plant 9 but -- I just wanted to make sure I understand this correctly. It's not possible to place people within Plant 9.

MR. HINNEFELD: That's correct.

CHAIRMAN MELIUS: Yes.

MR. HINNEFELD: And there's -- if I can maybe -- I'm going to say this to move the discussion along. I might be arguing against interest here but I don't even know what -- my interest is to be done. So I'm arguing for my interest.

Paul's point about, you know, this is a common problem at sites when we use AWEs where we use air sampling data. I think there's a substantive difference at Fernald where you have a variety of radiological

operations going on across the plant. You know, we have AWEs who do one thing, machinery, and then you have some data and you apply that. You don't know who was in the machining area and so you do that.

In this area you have people who were doing radiological work through all these plants through all these years. And they were exposed to things other than thorium.

So to David's point I think -- I'm speaking to David's point here. And so I'll make that point in kind of contradistinction between other situations where we have used air data for essentially everybody who worked there.

CHAIRMAN MELIUS: Any other -- Wanda, I'm sorry. Go ahead. And then Bill.

MEMBER MUNN: The only alternative that I have not heard anyone broach at all is the possibility of limiting the SEC to the years `54, `55 and `56, which would incorporate this totally untenable figure that

no one can deal with but would leave the latter years with data which would be much more feasible in terms of establishing an upper bound.

If you look at the remaining data, the only thing I see there that -- the highest number is 25 MAC. And that, although a very high number, would be much more reasonable in terms of possibilities and probabilities. Since --

MEMBER ZIEMER: But those high numbers only apply for that particular year anyway, don't they?

MEMBER MUNN: Yes.

MEMBER ZIEMER: That's not used for every year.

MEMBER MUNN: Yes, that year is the only one that shows that. So, my point is the other years -- I was only incorporating `54 and `56 simply because it was, as I understand it, there was thorium there but it wasn't -- but there's no report.

Am I correct in that assumption, that there was thorium there but there was no report on those two years?

MR. HINNEFELD: Are you asking about 1954 and 1956 for Plant 9?

MEMBER MUNN: Yes.

MR. HINNEFELD: That means thorium was used there but there was not a DWE study done for those years, right.

MEMBER MUNN: Yes. So, in view of the fact that the high number is in that location and during that time frame, then that would -- I don't think there's any question in anyone's mind here with respect to SECs for those years.

But what I'm saying is an SEC is not automatically falling into place on the basis of the fact that the remaining data is all outside the realm of probability for the other years that are shown on this chart. Am I incorrect?

MR. STIVER: I just want to

clarify something about Plant 9 in `54 to `56. In `54 they were just getting started to ramp up towards the end of the year for the big push in 1955. And in 1956 it was all but complete and they were just cleaning up for the most part. So there's a very small piece on either end of 1955 when thorium was being handled in Plant 9.

MEMBER MUNN: Yes.

CHAIRMAN MELIUS: Yes, but I think, again, you can't place people within the facilities at the plant, within the different plants. So you're going to then be, if I understand the proposed method, is taking the Plant 4 value of 6.4 and that applies to everybody across the facility, that if you move on later you're still having a 1961 twenty-five times the MAC that would apply to everybody working in the facility. And 9 and 10, I mean, it's for each year.

I just -- I find the whole method to be implausible. I just don't -- I think

it's hard to sort of pick and choose those years. I think, again, even say, again, taking 1961, Plant 6, 25 MAC, the next highest plant used in Plant 1 and that's less than one MAC as the highest value. So you've got a 25 times-fold difference there in the value that's used if we had information on the facility. And then you have people that weren't exposed at all in these other facilities. And I don't think that's a plausible application here.

MEMBER CLAWSON: Jim, something else comes into this. And this is why I've tried to get so much information into the full Board.

Because one of the other things too, as the petitioner said earlier, that these gaps in here and stuff, these are our best available information that thorium was not there. There's still gaps in all the information.

Another thing that comes into this

too is they became a thorium storage facility which we have nothing for at all. But what we do have the information on the thorium somewhat are at these plants.

There's another little part that plays into this that is a hard one for us to be able to deal with, and it's a signed affidavit in a lawsuit stating that the person that was taking the air samples, when he came back too high of samples, was instructed to go back and get other ones.

This is one that we can't prove. You can't put anything on it and be able to prove it, but this is a signed affidavit that the petitioner has referred to in this of the sampling.

And so what we have tried to do is to take the best information that we have and try to put this into a coworker model that we can. And as I've said, we're at the end and I've said this for the last four meetings. We can't go anyplace further.

And I come back to my question of I thought this is what the SECs were for. We've exhausted all avenues and this is where we're at.

CHAIRMAN MELIUS: David Kotelchuck?

MEMBER KOTELCHUCK: Yes. I don't want to comment on the affidavit because I don't think we can deal with that, or at least I think it's another issue. I don't think that's the central issue we need to decide now.

I agree on `54 through `56, but as I said earlier, the central issue in my opinion is that we are assigning thorium exposure to many people who we have to believe full well had absolutely no exposure, or had -- or I won't say absolutely no exposure because so many people were exposed -- but had, let's say, negligible exposure. So -- a very small exposure.

So I just think that it's just

pushing the plausibility of the model too far. And I just would feel more comfortable saying let's just add an SEC for this period, which is to say I support the motion.

CHAIRMAN MELIUS: Bill?

MEMBER FIELD: With all the uncertainty I think I favor an SEC as well. But I had the same question when I put it up before Wanda asked, is that for those three time periods -- I just want to clarify something. I thought earlier discussion someone said if it wasn't for the 215 or the higher value that a coworker model could be developed. Now, did I just hear that? I thought someone had said that.

MR. HINNEFELD: No, I don't think so.

MEMBER FIELD: Okay, then I misremembered that. So it wasn't just based on that one.

And is there anything to believe that that one value in that one year, that

whatever process that was could have occurred at the other plants during all monitored periods that weren't captured?

MR. HINNEFELD: Oh, that one -- no. I think there was no reason to believe that. Plant 9 was built to be a thorium plant. And it contained all the operations that were spread throughout the rest of the buildings were available in Plant 9. And sometimes it was even referred to as a semi-works because it had all those operations in it.

It was built to be a thorium plant at the time when the Department of Energy said thorium is the next big thing, we're going to be using this stuff a lot. And then in a couple of years they decided they weren't.

So Plant 9 stayed and it actually turned into what was called the Special Products Plant later on. But it was built for a thorium plant because they thought they were going to do a lot. They did a lot in `55 and

for various reasons, part of which was the plant was not adequately designed for the work they were doing, clearly, given the exposures they were having, it just didn't carry on from there.

CHAIRMAN MELIUS: David, do you have another question? Kotelchuck. Thank you. Any further?

MEMBER ZIEMER: This is Ziemer. I have one more question.

CHAIRMAN MELIUS: Go ahead, Paul, and then Loretta.

MEMBER ZIEMER: Yes, I want to ask John Stiver could you clarify -- I was trying to read between the lines but what was SC&A's final position relative to this issue now?

MR. STIVER: This is John. Our position is that, given the changes in the criteria for sufficient accuracy and the fact that there's still a lot of unanswered questions about the data, that we are not advocating using the model at this point.

MEMBER ZIEMER: That's another way of saying you're supporting an SEC Class. Is that correct?

MR. STIVER: I wouldn't say necessarily supporting an SEC. It's just that this is kind of out of our hands at this point. I think we've done all we can with the data we've got and looking at the different models. And there's still some very large uncertainties. And I think it becomes a policy decision at this point.

CHAIRMAN MELIUS: So Loretta first and then --

MEMBER ZIEMER: Let me ask it a different way. You do not believe this is an adequate coworker model.

(Laughter.)

MR. STIVER: I think as it stands it's probably not.

MEMBER ZIEMER: Thank you.

CHAIRMAN MELIUS: I was hoping we'd have a lawyer jump up and object at that

point. Go ahead. Loretta then Josie, I believe.

MEMBER VALERIO: My question is, for the year 1955 in Plant 9, that value was significantly higher than all the other years and locations. But after 1957 there's all the dashes showing that there was no potential exposure. So my question I guess is what about residual contamination in that plant?

MR. HINNEFELD: I guess, with the model presented, our expectation would be exposures from residual contamination that would be re-suspended from people going into there would be less than the exposures, the maximum exposure in the plants where the thorium work would be going on.

And so the residual dose exposures from Plant 9 would be less than in 1957, the 2.2 MAC, from Plant 1. And everyone is getting that value anyway. So, it would be less than the work -- than the values that were measured for the active work.

MEMBER BEACH: My question is for John, so if you want to come back up. I just wanted a clarification, because you mentioned it during your presentation, the N/A highlighted at the end of the Pilot Plant. I just couldn't quite remember what you said what the significance of those years were.

MR. STIVER: Those are years in which there's quite a bit of thorium production going on in the Pilot Plant. They were doing I believe -- trying to speak extemporaneously from my memory is never a good thing. But they were doing some recasting, remelting of thorium. They were doing gel production for storing the materials. Some chemical processing work. There were about three or four different things, different tasks that were going on in the Pilot Plant.

Whereas in Plant 1, it was basically a sampling plant. They were receiving ore, they were grinding it up,

sieving it, packaging it up to go to the refinery, for the most part.

And so in my mind it was -- there certainly appears to be a lot of potential for a thorium exposure in the Pilot Plant compared to Plant 1. And so we were concerned about that.

MEMBER BEACH: Okay. And we don't have any data for those three years?

MR. STIVER: There's just -- for `65 and `67 there was some unweighted air sampling data but that was it.

MEMBER BEACH: Okay. Thank you.

CHAIRMAN MELIUS: Loretta, another question? Okay.

Ready to move forward? I will try to clarify and read what I understand the motion to be.

So it would be all employees of the Department of Energy, its predecessor agencies and their contractors and subcontractors who worked at the Feed

Materials Production Center in Fernald, Ohio from January 1st, 1954 through December 31st, 1967, and that that Class be added to the SEC.

So if there are no further questions, Ted, do you want to do the roll call?

MR. KATZ: Sure. I'll stick with my alphabet here. Dr. Anderson?

MEMBER ANDERSON: Yes.

MR. KATZ: Ms. Beach?

MEMBER BEACH: Yes.

MR. KATZ: Mr. Clawson?

MEMBER CLAWSON: Yes.

MR. KATZ: Dr. Field?

MEMBER FIELD: Yes.

MR. KATZ: And I'll collect Mr. Griffon's vote. Dr. Kotelchuck?

MEMBER KOTELCHUCK: Yes.

MR. KATZ: I'll collect Dr. Lemen's vote. Dr. Lockey is recused. Dr. Melius?

CHAIRMAN MELIUS: Yes.

MR. KATZ: Ms. Munn?

MEMBER MUNN: Yes.

MR. KATZ: Dr. Poston?

MEMBER POSTON: Yes.

MR. KATZ: Dr. Richardson, are you back with us? I'll collect his vote. Dr. Roessler?

MEMBER ROESSLER: Yes.

MR. KATZ: Mr. Schofield?

MEMBER SCHOFIELD: Yes.

MR. KATZ: Ms. Valerio?

MEMBER VALERIO: Yes.

MR. KATZ: And Dr. Ziemer.

MEMBER ZIEMER: Yes.

MR. KATZ: It's unanimous, the motion passes.

CHAIRMAN MELIUS: I do not have a letter ready on this. I had a partial letter but we're going to have to revise it so I will have to -- I actually have to talk to counsel. It's a little complicated Class to define. But we'll work it out and I will again circulate

that.

Again, I'd like to thank SC&A and NIOSH and the Work Group and everybody involved. There's a lot of effort that's gone into this. I'm not sure what's left to do on Site Profile issues. I've forgotten that slide already from an hour ago, whenever John put it up there. But, again, it's taken a long time, it's a lot of effort and I think the work's been very thorough.

Again, I think all the Board Members appreciate everything everybody's done on this. And this was not an easy one to deal with on a lot of levels. So I again thank everybody.

I believe that finishes our business for this meeting.

MS. BALDRIDGE: This is Sandra.

CHAIRMAN MELIUS: Yes, go ahead, Sandra.

MS. BALDRIDGE: I would like to thank everyone for all their hard work, and

the Board for listening so attentively to what we have worked, it seems, hours and hours and hours, and I know my participation was just a small portion of what the Board put in and SC&A. And I'd like to express my appreciation to everybody.

CHAIRMAN MELIUS: Well, we appreciate your interest, involvement on this and meetings going back in time. I'm afraid I can't even remember all the years but it's very good.

And we appreciate your patience with this process. I know it's been frustrating at times. But, again, I think we've reached a fair conclusion on this. So, again, thank you.

Unless anybody else has something they want -- oh, Brad.

MEMBER CLAWSON: I'd like to thank Sandra. But I'd also like to bring something else too. You know, our conflict of interest is an important thing, but Stu's ability to be

able to come in there with his knowledge was a great -- it's what brought many of the things to the surface. I'd like to thank NIOSH for being able to do that.

I'd also like to thank Ted because I know it was a lot of work to be able to get the variance for that but it helped greatly. I just wanted to go on record of noting that.

CHAIRMAN MELIUS: Okay. Thank you. Anything else? If not we stand adjourned. Thank everybody and have a good trip back. And we'll see you -- hear you on the phone in September and see everybody in Denver in October.

(Whereupon, the above-entitled matter was adjourned at 4:13 p.m.)