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

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ADVISORY BOARD ON RADIATION AND
WORKER HEALTH

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FERNALD WORK GROUP

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FRIDAY
JULY 28, 2017

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The Work Group convened via teleconference at 10:00 a.m. Eastern Standard Time, Bradley P. Clawson, Chairman, presiding.

PRESENT:

BRADLEY P. CLAWSON, Chairman
PHILIP SCHOFIELD, Member
PAUL L. ZIEMER, Member

ALSO PRESENT:

TED KATZ, Designated Federal Official
NANCY ADAMS, NIOSH Contractor
MATT ARNO, ORAU Team
BOB BARTON, SC&A
HANS BEHLING, SC&A
RON BUCHANAN, SC&A
DOUG FARVER, SC&A
STU HINNEFELD, DCAS
KAREN KENT, ORAU Team
MICHAEL RAFKY, HHS
MARK ROLFES, DCAS
MUTTY SHARFI, ORAU Team
JOHN STIVER, SC&A

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10:01 a.m.

Welcome and Roll Call

MR. KATZ: So, it's time and we have our Board Members Brad Clawson, our Chair, the Fernald Work Group, and Phil Schofield and Paul Ziemer, our members.

So why don't we go on with roll call? And then we will get the meeting going.

(Roll call.)

MR. KATZ: Okay, that takes care of attendance.

Now, some preliminary. First thing please everybody -- everybody mute your phones, except when you are addressing the group. And if you don't have a mute button on your phone, press *6 to mute your phone. That way, we won't be hearing barking dogs and other things. Also, do not put this call on hold at any point. If you need to leave for a piece, hang up and dial back in. If you put it on hold, it will disrupt the call for everyone else. So, please don't do that.

And then let me just note most of the material, all the major material for this meeting, the agenda and the materials are posted on the NIOSH website under this program's portion of the website, schedule of meetings, today's date. So you can follow along with the materials there as they are being spoken about.

There is one document that -- for posting that is not -- which deals with environmental -- the environmental TBD --

(Simultaneous speaking.)

MR. KATZ: Members of the public, please, there are people speaking on this line who shouldn't be speaking at all. Please mute your phone. Press *6 if you don't have a mute button but we shouldn't be hearing a bunch of chatter. Please, I stress this, if we can't cut out the noise, we'll cut off your line and you won't be able to hear the call anymore.

Alright. With that, Mr. Chair, takes care of all of the preliminaries and, Brad, it's your meeting.

CHAIR CLAWSON: Thank you, Ted. I would like to welcome everybody to the Fernald Work Group meeting. We'll try to finish up Site Profile issues that we have lingering out there and we hope that we can get that taken care of.

So, John, since you are SC&A's or -- have you --

SC&A Review of DCAS Internal Dose TBD (ORAUT-TKBS-0017-5, Revision 03, Feed Materials Production Center - Occupational Internal Dose; Resolution of SEC Petition Issues (1984-1989); Resolution of Site Profile Issues; and Work Group Recommendation and/or Path Forward

MR. STIVER: Yes, this is John Stiver. I'll go ahead and kind of set the stage here and then we can start getting into some of our reviews.

As you all know, it's been a long time since Fernald was front and center. The last time we actually had a Work Group meeting was back in December of 2014. And some of the issues that we are going to be discussing today predate

that considerably. A lot of this work was from back around 2009 to 2012 time frame. So there is quite a lot of water under the bridge. And in addition, three classes have been added to the SEC in that period.

But there about, I believe, if I counted up right, there are still 11 items that are outstanding from the original issues matrix. And in addition to that, we had done some reviews after the last meeting from products that NIOSH had delivered. One was the thorium -- the thoron post-SEC. And there was an OTIB-78 review, which was basically the uranium coworker model and that has since been canceled and incorporated into TBD-5.

But the two big items on the schedule today are the TBD-5 Rev 3 review, which we just finished up and the TBD-4 Environmental Review that Doug Farver finished back last year in 2016.

And since Bob and Doug were pretty much the principles on those two reviews -- I have done some of it myself -- but I would like

to have Bob go ahead and lead out on the TBD-5 review.

So, Bob, take the stage, if you want.

MR. BARTON: Sure. Thanks, John. This is Bob.

The first item that we're discussing here, as John mentioned, is the occupational internal dose TBD. Specifically, it is Revision 3, as Ted mentioned, that has been posted on the website, along with our review.

What we decided to do this time around with our review, sort of for, I guess, ease of understanding, is really pull out those issues that were still I guess the word is actionable. And instead of we'll sometimes use a matrix format, which can get pretty cumbersome, especially when you have so many sort of responses and back and forth like we had at this site and we sort of set it up into more of a narrative form, which you will see in our report. And we kind of broke it out by the major areas of interest. I mean the real big ticket items here

are going to be, obviously, the uranium coworker model, which ties into not only just uranium but also recycled uranium contaminants and then, obviously, the thorium model.

The thorium model is actually discussed pretty extensively, initially, in September 2014 when we discussed and NIOSH presented what they were proposing as a method for basically the periods after the SEC period, which ends in 1978 on how they were going to reconstruct thorium doses and that also includes thoron, which is part of the thorium decay chain.

And then, as John mentioned, there is also OTIB-78, which deals with the uranium coworker model. And then as a spinoff of that, we also had several White Paper exchanges, as we outlined in our internal TBD review about recycled uranium and what the correct ratio should be for when you have a uranium intake to sort of map back to those contaminant dosimetric inserts, mainly plutonium, technetium and neptunium.

So that's where we're at. I think, for those following along, I think probably the easiest way to go about this would be to open up SC&A's review, which is posted on the website and we can sort of go by the way the issues are laid out in that report. The first issue is in Section 2.1 and this --

MR. STIVER: Hey, Bob, this is John. Are you going to put it on Skype so we can look at it?

MR. BARTON: Sure. Let me see if I can do that. You can pull them right off the website but let me see if I can throw them up here.

Bear with me folks a few minutes. Bear with me here and I'll pull these right up, as they appear on the website so we're all looking at the same thing.

MR. KATZ: Hello. While Bob is pulling this up, please, everyone else mute your phones. I can hear breathing and background noise. Press *6 to mute your phone.

Thanks.

MR. BARTON: As I was saying, we really went for sort of a narrative form so that we could really get down to what the remaining issues were or, in certain cases, what the certain specific aspects of these issues are.

And looking at the original findings matrix, it can sometimes get muddy in what has been sort of resolved already and what has not.

Okay, can everybody see my desktop on Skype? Alright, can everybody hear me?

MR. HINNEFELD: This is Stu. I hear you but I cannot get Skype to show me what I think it should show me. I'm still working on it.

MR. KATZ: Yes, it's not showing anything to me either, Bob.

MR. BARTON: Alright.

MEMBER ZIEMER: I'm not on Skype. My Skype box is open but it's not showing anything for the Fernald meeting.

MR. KATZ: Yes, it just came up, Paul. It should be showing for you, too, now.

MR. HINNEFELD: Yes, it just came up for me as well.

MR. BARTON: Okay, great.

MEMBER ZIEMER: Am I looking at something specific?

MR. KATZ: So he's showing his document. I believe it is a matrix document.

MR. BARTON: No, this is actually going to be the TBD-5 review.

MR. KATZ: Oh, the review itself. Okay. Okay.

MR. BARTON: Okay. So, hopefully, you'll see -- you should be looking at Section 2.1 of that review, entitled Exposure to Uranium-Poor Raffinate Material.

And essentially what this is --

(Simultaneous speaking.)

MR. KATZ: Folks on the phone, would you please mute your phones? You won't be able to see the pictures that we're talking about here but the documents that we're talking about here are the same documents that are on the NIOSH

website and you can get them there on Schedule of Meetings, today's date. You can look at them there, if you would like to but you won't be able to see what Bob Barton is discussing specifically because he has a separate system for showing this. It's the same document, though.

MR. BARTON: Okay, so, we're talking about, for those of you who can't see it but can pull it up off the website, we're talking about Section 2.1 of the SC&A review of the Internal TBD for Fernald. And this deals with the uranium and uranium-poor raffinate material.

The TBD covers three exposure scenarios for raffinate exposures. Those are the K-65 drum operation, which was from 1952 to 1956; there was also the processing of pitchblende ores, which occurred from 1954 to 1958; and then also the handling of yellowcake materials which up into 1961. And those three scenarios are reconstructed using either radon breath data or actually uranium urinalysis. And what you do is, in the case of radon breath data, use those radon

results to sort of back-calculate to a radium burden and then you can use that radium body burden to ratio the other contaminants of interest that you find in these raffinates. So that is for the K-65 operation.

Now, the other two use uranium urinalysis and basically, again, use a ratio technique to the contaminants in those particular source terms to calculate what the intakes were from those contaminants which weren't directly monitored before but since we have uranium and we assumed a reasonable ratio, we can back-calculate to get those intakes of other contaminants.

The issue becomes when you might be dealing with some material that is uranium- or radium-poor. And what I mean by that is the concentration of the uranium and radium are very, very low because they have essentially been stripped away from this particular waste stream.

I think what we're really talking about is the back end of Plant 2/3, when you basically strip off all the uranium product and

you are left with just the raffinate material. This becomes problematic simply because you can't really use, in that case, a ratio to any sort of uranium intake because the uranium is just simply not there. So, to apply a ratio to a uranium urinalysis result would really result in pretty unrealistically high models of the intake of all the other contaminants that would be present.

So we really discussed this mostly in December. That transcript is on the web. Discussion takes place mainly pages 128 to page 150.

Specifically on page 146, it sort of -- NIOSH had laid out not a definite framework but some ideas on how such a source term might be able to be dealt with. And really, the contaminate of concern we're talking about here in the raffinate material is thorium-230. And what had been discussed at that last meeting was that well, there might be a possible way to use daily-weighted exposure reports, essentially their profiles contaminations breathed in, in

this case, alpha contaminations, which could be used and assumed to be thorium-230 for those workers who were potentially exposed to this raffinate material that is uranium- and radium-poor. And a method could be developed to then assign those doses to thorium-230 without having to try to use uranium and radium as a surrogate for ratio.

Currently, that one is in the BRS. It is, I believe, still labeled as "in progress." So, where we have it in our report currently is that we didn't see this specific source term, that is the raffinate material that you can't really use uranium urinalysis or radium to back-calculate intake of contaminants. We didn't see that that was discussed or really dealt with.

So at this current juncture, we recommend that it really remain in progress. And I guess I would turn it over to NIOSH, if they would like to discuss where they are on that particular issue and whether they plan on developing models or what their plan is.

Because like I said, we discussed it back in December and we left it in progress at that meeting, pending treatment by NIOSH but we didn't see that that was actually dealt with in the most recent revision.

So if NIOSH would like to sort of update everyone on where that stands or where they stand on that issue.

MR. HINNEFELD: Yes, this is Stu Hinnefeld and I'll take a shot at that.

We have in fact looked back to the information that was put together really as part of the report, Report-52, where we put together a lot of -- trying to address a lot of internal dosimetry issues and a number of the things proposed in Report-52 are no longer applicable. We have moved on past those.

But when we looked at the portion, this portion on this topic, what was done in the preparation of 52 was the daily weighted average data was actually evaluated in preparation of Report-52 to say okay, was there really much

potential airborne exposure in the portions of the plant where the raffinate existed because the raffinate was essentially either a liquid or a slurry and was kept as a slurry until it was disposed of.

And so is there really a particular internal dosimetry issue there? And there doesn't seem to be.

So in the Site Profile, while the section retains something -- I think the section even talks about raffinate doses and then it talks about the K-65 material, which recall that the K-65 material, much of that came from offsite. It was processed elsewhere. I think it was process at Mallinckrodt and drummed and then brought to Fernald and the K-65 material itself was dumped into silos, 1 in particular, and I think part of the outside material also went into silo 2.

And so you actually had people there who were exposed to the K-65 raffinate material as they were dumping it into the silos. That

kind of exposure method doesn't exist for the raffinates that were generated at Fernald, which were slurry and disposed of. From K-65 they were disposed of in silo 2 and then for the later materials, usually ended up in waste pits. And so you don't have a potential exposure potential.

So from our standpoint, the materials that were important to address were the front end of Plant 2, the digestion where they dumped the materials into the digester in the refinery, where the uranium would still be there, hasn't been refined out. So, the uranium would still be present with these other contaminants that would be present either in the ore concentrates or the ores. And so that is the method that is included in the internal dosimetry Site Profile.

So, I guess that is our position on that. You know a further possibility here is that this exposure occurred within the existing SEC framework. And so we have already concluded that not all doses can be reconstructed during this time. And if there is a really a strongly-

held opinion that there had to be some sort of raffinate exposure to thorium-230, that it falls within the category of something that we would leave out of the partial dose reconstruction.

MR. BARTON: Thanks, Stu. I guess I'll address that second part and that actually came up when we were discussing this issue is whether this particular source term was covered by the SEC. Now the SEC was for thorium but not necessarily thorium-23 because --

MR. HINNEFELD: Yes, I remember that and people have pointed out in the transcript and I have pointed out that it was thorium-230. And the thorium-230 was the logic for the class. I'm not arguing that. It had to do with the inability to interpret the in vivo counting for thorium-232. However, you know I think we probably have the leeway to decide what doses are reconstructable or not within a period of an SEC class.

And in fact, the Secretary's designation just says thorium. It doesn't say

thorium-232.

MR. BARTON: Okay, so I guess it is NIOSH's position, though, that this issue essentially of this uranium/radium-poor raffinate material in the 230 is an exposure that is not reconstructable.

MR. HINNEFELD: Well, our first position is that there is no particular exposure to reconstruct because of the physical form of the material. Alternatively, if people don't believe that is sufficient evidence of that, then our alternative position would be that it would not be reconstructable.

MR. BARTON: I see. Okay and so I mean you discussed Report-52. Is this something that was planned -- I mean just so we can put this I guess to bed, would that be put into a formal response by NIOSH? I mean it seems like the first position is that, essentially, there is no exposure just because of the physical form of the material and whatnot. Does NIOSH plan to put that into a formal response so we can close it

out?

MR. HINNEFELD: That is in the document that I sent to the Work Group and to John either yesterday or the day before.

MR. BARTON: Oh, okay. So maybe we need to take a look at that and then --

MR. HINNEFELD: Yes, that document takes the form of the in-progress, the findings in the matrix in the document we're talking about now, Review of the Internal Site Profile, farther back there is a matrix of the findings. And we took the ones that were in-progress, the ones we thought we could make the most comment on and we copied those onto another piece of paper, essentially another table, and then added our response under each of the findings. And so that is the form that the document takes and I sent it to the Work Group, and Ted, and John, like I said, in the last day or two.

MR. BARTON: Okay, well then I guess the appropriate thing will be to ask the Work Group how they would like to proceed, whether

they would like us to look closer, take a look at those responses that NIOSH has provided and, perhaps, be able to close these out or I don't know if the Work Group might have had a chance to look at that and want to close out this particular issue now. I'm not sure how --

CHAIR CLAWSON: Hey, Bob, this is Brad. I haven't had a chance to look at it. I'm trying to look it up right now. But what we need to be able to do, if they are sending you a formal response, I guess what I would like from you guys is your response from it and just send it out to the Work Group and go from there.

MR. STIVER: Brad, this is John Stiver. I haven't seen this at all. I'm going through my emails and I'm not seeing anything from Stu over the last couple of days. So, this was something we haven't looked at.

MR. HINNEFELD: Okay, I apologize. Hang on a minute. Let me see what I can do here.

MEMBER ZIEMER: Yes, Brad, I think probably in any case we just need a written

comment before we formally close it, probably.

CHAIR CLAWSON: Yes, that would be a good idea, Paul. Thank you.

That will be our path forward. I can't find it on mine either, Stu. So, we'll just --

MR. HINNEFELD: I don't think I dreamed it. Hang on a second.

CHAIR CLAWSON: I know I have sent out reports and forgot to hit that send button and that was pretty impressive.

MR. HINNEFELD: Well, that's a possibility.

MR. STIVER: Well, if SC&A hasn't seen it and we haven't seen it, let's have a chance to take a look at it before we formally close it. It sounds to me like we can readily close it but we should see the final wording before we do that.

MR. HINNEFELD: Well, I apologize. I'm not seeing it in my outbox right now either.

MR. BARTON: Okay, I just guess for the official transcript, we will leave this one

in-progress for the moment and note that NIOSH's position is that there is really no exposure to this sorts of raffinate materials due to the physical form. And I'm sure there the rationale is provided in the document we're talking about now and asking that it is likely not a reconstructable dose under the SEC in any case.

So, we'll take a look at that and I think the formal thing to do is to leave this in-progress now. Is that correct?

CHAIR CLAWSON: That would be correct. This is Brad.

MR. BARTON: Okay. So with that I think we can move on to the next section which deals with recycled uranium and the contaminant radionuclides ratios associated with that source term at Fernald.

I know John Stiver did a lot of that work and there has been a ton of White Paper exchanges, which we sort of summarized in our report here on the internal TBD review.

John, do you want to discuss that a

little bit on what we found with recycled uranium?

MR. STIVER: Yes, sure. Let me try to request control here. Okay, it doesn't seem to be working. Let's see. I'm trying to scroll down here. I'm having kind of a spotty connection today so, bear with me. Anyway, here okay. There we go.

Basically, this is another thing that there has been off the books or at least kind of in abeyance for a number of years. I believe we started talking about recycled uranium back in about 2009 time frame. And this went on until about 2012. The Work Group decided to put this issue into abeyance, pending NIOSH incorporating the values that they had put into Report-52 way back in 2011 into the TBD product revision.

And so I just thought I would go through we listed kind of a history here of all the different chances since then. And it's all in the paper so I'm not going to review it -- I'm not going to read all of that.

But the endpoint we changed here was that we had determined that there were literally three time periods involved in recycled uranium and the associated contaminants, which are plutonium, neptunium-237, fission product in technetium-99 and also in americium-241.

And I believe the first period, if my memory serves, was from about the early 1950s, I think it was like '58 up to '61 and the material which we see at Fernald but, in this process that took place. So, the exposure potential during that time frame was really quite minimal. And then in 1961, Fernald started processing these materials.

And after a lot of discussion, we had settled on a series or a set of presumed concentrations to these constituents in parts per billion in relation to uranium. And moving down here to the tables, if I can find them, and after a lot of discussion about being claimant-favorable and some of the mechanisms that were divulged there, these materials would actually be

concentrated in certain operations. This would have been the Plant 5 metal production where the material, the tetrafluoride and magnesium shavings were put into these big what they called "bombs," basically reduction containers. And then through a thermite process, the uranium metal was produced.

And about from some of the reports I read back five years' back, I believe about half of these constituents would report into magnesium fluoride. And that material would then be recycled back in Plant 1. And so we would have this kind of a exchanging process where this material would be concentrated.

And so we felt that the actual levels coming in in the feed material might not be representative about this subclass of workers in the Plant 1 or dealing with material and also the people who work even in Plant 5, who were flooding out these --

(Simultaneous speaking.)

CHAIR CLAWSON: Somebody needs to

please mute their phones so that the rest of us can hear this conversation.

MR. KATZ: Yes, Zaida, are you on the line? Okay, I'm going to have Zaida cut that line because it keeps giving us problems. But thanks.

Go on, John.

MR. STIVER: Okay. Anyway, we had finally settled on what we felt were bounding values. These values were 100 parts per billion for plutonium, 3,500 parts per billion for neptunium, and 9,000 parts per billion for technetium-99. This was for the period 1961 to 1972, I believe.

And then beyond that, Fernald started receiving materials that were more highly enriched -- well not really enriched but had higher concentrations of these constituents from the gaseous diffusion plants. So, beyond that, we had a situation where we felt that that 100 parts per billion plutonium probably really might not be adequate to bound the exposures to some of

these groups, including the very same groups that we were talking about before because now they are dealing with much higher concentrations coming into the feed stock. And so we kind of have an amplification of that concentration process.

And also this other group, as well, of workers who were the down-blenders, who broke up this material that came in in I believe 16 hoppers, I believe. Actually, that was just the one that came back in 1980, which would have the highest concentration.

But they break this stuff up and -- there was much higher potentials. So, after a lot of back and forth discussions and paper exchanges and compilations, we determined that 400 parts per billion was probably would be bounding and reasonable for that time period from 1973 on.

And so I'm going to go down here a bit, if I can find -- okay, here we go. You can all see this is Table 2.1 and these were the values that were actually included --

(Simultaneous speaking.)

MR. KATZ: Hello? Excuse me, John. I apologize John. Someone else is on this line and is speaking. Please mute your phones. Your line is going to be cut, ultimately, anyway. But please mute your phone. You are interfering with the call. Press * and then 6 to mute your phone, please. Thank you.

Go on, John.

MR. STIVER: Okay but anyway, this table here people that want to -- it was taken right out of Table 18a of our Report-52 from 2011. And these are the values that were agreed upon, 100 parts per billion of uranium mass concentration addition; 3,500 for neptunium; and 9,000 for technetium-99. And those other radionuclides listed were little in concentration. It was just these big three, plutonium, neptunium, and technetium.

And then in Table 2.2, this is from Table 18b of Report-52 -- oh, this is really to concentrate with the phone noises.

MEMBER ZIEMER: John, what is the technetium-pp in Table 2?

MR. STIVER: Excuse me?

MEMBER ZIEMER: My Table 2 shows technetium-pp as a contaminant. What is that?

MR. STIVER: I'm not seeing technetium-pp in the -- oh, wait.

MEMBER ZIEMER: Table 2.

MR. STIVER: That's a typo. That should be techntium-99.

MEMBER ZIEMER: Oh, you are using the p's as 9s there. Okay.

CHAIR CLAWSON: Good catch, Paul.

MR. STIVER: So, anyway, these are the values that we did agree on in the Work Group discussions. Okay? So, we are in Report-52.

And then if we move down here and -- I'm having a hard time. Okay, here we go. Now we overshot. Okay. We have a really bad lag here. Okay, we're getting close.

It looks like my connection is not working so well. I'm trying to pull that table.

It's just above where we are here. We're getting there. Alright.

(Simultaneous speaking.)

MR. KATZ: Again, the fellow who just spoke, please mute your phone. Please press *6 and mute your phone.

MR. STIVER: Okay, here we are.

(Simultaneous speaking.)

MR. KATZ: Okay, sir, you're speaking into a line you are not supposed to be speaking into. Please mute your phone. Press *6 and mute your phone or hang up and leave this call.

MR. STIVER: Okay, let me try again here. This is from Table 5-10 of the TBD-5 Revision 3, which we just reviewed. And these are the contaminant levels that we looked for. And as you can see from '61 to '72, these are not the levels that were agreed upon. Plutonium was a factor of ten lower, ten parts per billion; neptunium is at 400, which is 3,500; and technetium at 6,000 versus 9,000 that we had agreed on.

And from '73 to the present, these are the correct values here. So, that part is fine.

And so we were just a little perplexed by this because these discretions went on for several years and there was a lot of back and forth and a lot of the Board's resources were expended in this process. And so we are just kind of wondering why these values came in the way they did and if this is maybe just an oversight or NIOSH had some reason for changing this up. So maybe Stu, if you could kind of jump in now.

MR. HINNEFELD: Yes, I can provide our background on that.

MR. STIVER: Okay.

MR. HINNEFELD: We have looked back over at 52 again, and Report-52 is where this 100 parts per billion number appears.

And what you read in 52, it says I think 52 was where we adopted the 400 parts per billion for '73 and forward. And it says that we'll retain 100 for the earlier years because

that is the way dose reconstructions are being done now is essentially what it says at the time that Report-52 was published.

So when you look at the actual data that is available for recycled uranium prior to the receipt of the gaseous diffusion plant, mainly the Paducah materials, there is really very little transuranic contamination there.

Consistently, the plutonium is below ten parts per billion, usually well below ten parts per billion. So there's really not much there to speak of.

John's point is correct that there were certain activities where the contaminants would preferentially go to a byproduct of reactions and the metal production step is certainly one of those. And the byproducts tended to move more toward the mag fluoride than toward the uranium and was in fact treated so that the uranium that was contaminating the mag fluoride was leached out so it could be recycled.

So that is true. So you have that

particular material, where you would have a higher contaminant ratio than you would have had in the feeds that came in.

But the thing to recall here, though, is that that particular strain, you know leaching uranium out of mag fluoride is a very small contributor to any given year's production.

And into the 1980s, at least, our position is that workers at Fernald were essentially chronically exposed to uranium in a variety of forms. And so you have this blended exposure and that we're pretty confident that ten parts per billion would bound the exposure up until the materials started arriving from the feed plant. You know the materials that arrived from Paducah really in 1973 were things called Paducah scrap and that was higher contaminant values than what had typically been seen before then but it was nearly as high as the real high numbers that arrived in the '80s in what's called the feed plant ash.

So we have taken those numbers, I mean

so from '73 on and adopted the 400 that we've kept. But the earlier values we've developed from the data that are available before -- from '61 to '72.

So explaining the difference between the hundred and the ten from '61 to '72 really has more to do with the reason the 100 was in Report-52, which was well, that is the way dose reconstructions were being done at the time. And so we're not going to change that.

Today, we've done a lot as a result of the discussions over the past two years to change the way dose reconstructions are being done. So the whole process is going to be reworked. And so since we're reworking the whole process anyway, we thought it might be time to take a more realistic look at what the numbers probably were before 1973.

MR. STIVER: Okay, well thanks for the explanation. I guess we get back to the paper that we produced back in 2011, the second White Paper. I think we have made some pretty

compelling arguments.

First of all, I think that the data that are available are limited in scope. They don't really -- this came out of the DOE from 2000 and we actually have one of our associates who was heavily involved in preparing that report and they did a great job and they did it at nine months. But they never really followed up and run these things to ground and found other data. Some of the data we did find, I think there was one particular batch of the uranium tetrahydrate nitric tetrahydrate in from Hanford that had valuable or quite bit of that ten parts per billion specifications. And I think they are in the range of 20 to 30.

And we also went to considerable effort to show that ten parts per billion is generally really not uniformly enforced since about 1985 or so. So we have this kind of a situation where we are not really sure that the database that we quoted are complete and really representative of what was actually experienced.

In addition to that, we found a lot of reports that have indicated that the people who were, you know that subgroup you were talking about, there are a small number of guys for sure but, again, we don't know who they are just like we didn't know who the downblenders were that form the basis of that 400 parts per billion agreement.

And so the selections in some of the reports from the time period is guys using broom handles with steel wool to scrub out these crucibles for the casking by the thousands; guys that have gotten their heads down into the reduction pot to scrape out the mag fluoride. It was a really dirty operation. And there is not necessarily respiratory protection being worn at the time.

Stu, you make a good point that this material isn't exclusively what is being used. I mean it's going to be what's inside of the contaminant, the non-toxic variety -- you are going to knock out some of those with a tremendous

amount of stuff. We're saying maybe the concentration was slightly off.

There is a larger concern with the amount of spots but you know just I would just as soon combine this. You know we don't know that ten parts per billion is really capturing or that the data is really representative. I mean there those really dirty jobs that workers were involved in in the subset who have much larger exposures than say some of the others. They did all the recycling and they just washed up. Well you know, there is really no way to tell at this point.

So based on those arguments, we really felt that the 100 would be bounding, you know certainly it is going to be bounding. So we didn't think the ten would necessarily get it there.

And so at this point, I mean I'm really not feeling comfortable in buying off on these new values without maybe going back and taking a look at the review that you did earlier

and maybe having another call or exchange or something. I know nobody wants to open this back up again. So I just, after seeing those, I just am feeling not comfortable in the tracking of it.

MR. HINNEFELD: Okay. This is Stu again. I would like to propose this now. I think from our side, we should put together something that says, that describes in detail the data we looked at because I think we're all looking at the same data. I think we're all looking at the Ohio Field Office Recycled Uranium Report --

MR. STIVER: Yes.

MR. HINNEFELD: -- because there is a pretty large compendium of data in there.

And so we should go look at the data that led us to our conclusions that this should be ten and in light of your earlier report, and provide a more full write-up of that because the document I put together, which maybe I didn't send, I'm in agreement about the work I guess now, the document that we put together that has some responses on it does not go into detail about

that.

So it sounds like if we feel like there is sufficient data to support the position we've taken, that we should prepare that and get that to you guys and to the Work Group. And so at the time, then that we can have a -- and then it gives you guys the opportunity also to look back at the data that led you to your opinions. And so then we can have a more meaningful discussion at another time.

MR. STIVER: Yes, I would agree to that. I think that probably would be a good way to proceed. You know it's hard to bring up the time period we're talking about is assumed to be within the SEC. So you know we'd be looking at non-presumptive cancers and then the for less than 250-day employees. So that may not, at this point be that big of an exposure that we think people would include. But however, the fact that your plutonium dose would be going down by a factor of ten, that is something that does kind of concern us.

So yes, I think maybe give us just a little bit of time to go back and delve back into it because the data and the reasons behind the decisions that we've came to and for you to provide a more detailed description of your position would be a good way to go.

MR. HINNEFELD: Okay, we'll proceed with preparing something then and we will get it to everybody and then the Work Group can decide if they want to discuss that in a meeting or if we can do it in a technical call.

MEMBER ZIEMER: Okay, Brad, just for the record, I think that's a good idea since as it currently stands it seems to be we are not in agreement on this. We need to understand more fully NIOSH's rationale here.

CHAIR CLAWSON: I understand, Paul. I agree with that. And you're good with that, too, Phil?

MEMBER SCHOFIELD: Yes, I am.

CHAIR CLAWSON: Okay. So, we'll do that and once we get those papers --

MEMBER ZIEMER: This will remain in-progress, then?

CHAIR CLAWSON: Yes, it will.

MEMBER ZIEMER: What's the timetable on that?

CHAIR CLAWSON: I guess that one's for you, kind of. What would we be looking at for a timetable?

MR. HINNEFELD: Well, I have to insert this into our project schedule and get with our contractor and find out when the resources will be available to work on this.

CHAIR CLAWSON: Okay.

MR. HINNEFELD: It is my intent, though, that having gotten to this point, that we try to maintain this momentum and get this -- to move everything over quickly and while we are thinking about it, and get this wrapped up.

So, it's my intent to put some urgency on this.

CHAIR CLAWSON: Okay, I understand.

MR. STIVER: Okay there was just one

other aspect of the RUs that I wanted to bring out there. And this is in our report that's Table 2-5 on page 14. And this is taken from the 533 and 534 that show changes of the intakes of plutonium-239. And we noticed that from '73 on, assuming these intakes at 400 parts per billion, we noticed that the concentration, the becquerels per gram of uranium is decreasing during these three time periods. I think we can agree that in 1975 it is 1170; '76 to '85 it's 1160; and then '86-on it's 1150. So it's not a big decrease but we are just kind of curious as to what the basis for that reduction was.

MR. HINNEFELD: This is Stu and I'm going to have to look back at this. We have a pretty extensive spreadsheet that was put together to explain how we arrived at our americium-241 numbers and consequently, the plutonium numbers as well, the various plutonium isotopes.

So, I'm going to have to go back and look at this exactly.

It has to do with the aging of the plutonium and the adjustment from the relative abundance of isotopes. But I don't -- you wouldn't think that 239 would decay even by a percent in --

MR. STIVER: In that short time period.

MR. HINNEFELD: Right. So, I'll have to go back and look at exactly how that was done to see why that number came out that way.

And recall that we will have to think about this very carefully because really the data we are relying on is all like 1985 urine data. That is when they did all the analyses of those transuranics and determined how much transuranics was in there.

So in realty, between the time this material was first generated or you know I guess, quote, generated by probably Hanford PUREX, where they were taking out plutonium and recycling the uranium, that would be sort of the creation date. And then kind of the relevant abundance of the

plutonium alpha emitters might change a little bit over time and, in fact, we do, in our spreadsheet, show like 10-year, 20-year, and 30-year weapons-grade plutonium, you know the components of it. And of course the 241 changes during that time and that allows for the americium to grown in.

So I'll have to go back and look at this. I'm having a little trouble explaining why this happened and I didn't even notice it until I saw it in your report.

MR. STIVER: Yes, okay. Yes, I thought it was probably something along those lines that we just didn't have any technical details.

MR. HINNEFELD: Yes, I apologize. I apparently dreamed that I sent that because I can't find in my outbox that I sent that email I thought I sent with two files attached. One was our written responses to each of the findings and the second was the spreadsheet that supports the americium. But I apparently thought I had --

intended to send it but I didn't.

And this may be, this declining 239 may be an artifact of how we generated the numbers and, of course, the plutonium isotopes.

MR. STIVER: Okay, I look forward to seeing that.

The next part I guess dealt with, as far as recycled uranium was Finding 10, which is the inclusion of americium-241. And I had Doug Farver look into that.

Doug, if you would like to say a few words about what you found and the basis for that.

MR. FARVER: Okay, the basis for Finding 10 on the americium-241?

MR. STIVER: Yes.

MR. FARVER: It was pretty much stated there in the italics from Section 5.3.3.4 of the TBD that the assumption is that the RU came from Hanford. It was a six percent weapons-grade in '61. And then it was aged from thereon down, 10-, 20-, and 30-year aged intervals.

And I went back and looked through

Hanford document and that is consistent with the Hanford documentation. So that's included in the TBD per the findings. That was the reason for recommending closure to that one. I thought it was very reasonable and consistent with the Hanford TBD.

So, any questions?

MR. STIVER: Alright, I guess we can go ahead and recommend closing Finding 10 out then, since everybody is in agreement with the method.

MR. FARVER: Right and apparently that was part of the SEC-P 3 finding.

MR. STIVER: Yes, there was a little cross-pollination there between the SEC findings that were subsequently moved over to Site Profile. And they are kind of interrelated anyway. So yes, there is some cross-communication there.

MR. HINNEFELD: Yes, this is Stu. I just want to make clear that -- my understand, make sure my understanding is clear. When this

finding is identified as SEC-P 3, that means it came up, it surfaced in the review of the SEC Evaluation Report. Now it is a Site Profile issue. Is that right?

MR. STIVER: Yes, I was just retained for this historic --

MR. HINNEFELD: Oh, yes, I understand retaining it. I just want to make sure we all are in agreement that it is now a Site Profile issue.

MR. STIVER: Yes, that's correct. It is no longer an SEC issue.

MR. HINNEFELD: Alright.

CHAIR CLAWSON: That being said, I guess I would recommend closing it. Paul?

MEMBER ZIEMER: Yes, I agree, it should be closed.

MEMBER SCHOFIELD: I agree with that.

MR. STIVER: Okay. Well, Bob, you want to move on to thorium and thoron then?

MR. BARTON: Sure. Well, there is a couple of quick items. I'm just going to go in

order.

The next section is the Section 2.3, which is just below the discussion of recycled uranium, it is titled Ingestion Intakes from Thorium. This really is one of the findings from way back in the original TBD that was reviewed, which was the original TBD back in 2004. And back then, it was proposed to use air concentration data to reconstruct thorium doses.

Now, obviously, a lot has happened since then. We looked at two SECs. So really, and this is originally Finding 15 related to ingestion intakes specifically for thorium. Now, this finding really only relates to the use of breathing zone data and also to the period in which we're using a fraction of the derived air concentration. So those two periods are essentially 1990 through 1994 for use of derived air concentration and then 1995 through 2006, when breathing zone samples are used.

And this one is pretty easy. There is a TIB, TIB-9 out there that instructs on how to

assign ingestion intakes when using such data to reconstruct internal doses. TIB-9 is referenced correctly in the TBD. In fact, if you look at our -- we have a quote here under Section 2.3 that calls out the sections where it talks about estimating the ingestion intakes using TIB-9. TIB-9 has been thoroughly vetted in Procedures. So I mean this was really just a question of it was in abeyance because we agreed with the method and we just had to see that language and instruction added to the TBD.

So like I said, this one is pretty easy. That language was included. So we suggest changing this finding, which is currently in abeyance, I believe, to be closed.

CHAIR CLAWSON: Sorry, I was talking on mute. From I understand, you have evaluated this and SC&A's recommendation is closed. Correct?

MR. BARTON: That's correct.

CHAIR CLAWSON: I agree. Any discussion from the other Work Group members?

MEMBER ZIEMER: I agree. Just for clarification, is the old Finding 29?

CHAIR CLAWSON: It's Finding 15, actually.

MR. STIVER: No, this was the old Finding 15. 29 relates to the K-65 radon exposure. That will be -- Hans is going to talk about that next.

MEMBER ZIEMER: Okay. Yes, I agree to close.

CHAIR CLAWSON: Okay, Phil?

MEMBER SCHOFIELD: Yes, I agree with that, then. I think we have pretty much beat that to death.

CHAIR CLAWSON: Okay, sounds good.

Okay, back to you Bob or John.

MR. BARTON: Well, the next section, if we are going in order to our review, is really the Finding 29. This is the K-65 radon exposure. And I guess just to set the table a little bit, this was discussed last at the September meeting in 2014. The discussion really starts around

page 240 and this sort of ties into, at least a little bit, into that raffinate discussion we had earlier. This is the dumping of the drums of raffinate material into the K-65 silos. And I think what happened is, if you look at that discussion back in September, there was some confusion about whether external dose rates were going to be used as a controlling factor. But really what it turned out was that radon breath data was going to be used as it was shown in Report-52.

I know Hans, if you are on the line, I know you were extensively involved in that discussion back in September and also the original issue. And I believe this is the dumping of those drums, not necessarily the sort of I guess ambient radon emanations from the silo that were extensively discussed in the following meeting in December.

So, I don't know if you want to sort of take the mike here, Hans.

DR. BEHLING: Okay, can you hear me?

Can you hear me?

CHAIR CLAWSON: Yes, we can.

DR. BEHLING: Oh, okay. As John just said, this goes back quite a ways. Initially, Finding 29 cited an issue here, where the original exposure to radon was based on two simple samples that were taken in '63 and that was considered inappropriate in the sense where that was a limited amount of data. But moreover, the issue was also compounded by the fact that NIOSH used external exposure dose limits to basically bracket for the internal exposure associated with radon exposure that were the result of people working with the transfer of 13,000 drums into Silos 1 and 2.

And my initial finding centered around the confining factor of using external exposure, which was based on an assumed exposure to workers who were engaged in that particular activity, whose exposure was estimated, on average, to be 312 millirems per week. And as a result of that exposure and this is assumed to be an empirical

exposure measurement, but then the model was used to assume that, at the time, this was in the '50s, that the regulatory limits for exposure were to be five rems and that was further reduced to four rems, based on admin limit that was lower than the regulatory limit. And now it is upon that basis it was assumed that internal exposure, as well as the radon exposure, would be limited by that external dose limits, which was not the case.

When we looked at the data, we realized that in that time period the actual regulatory limit was 12 to 15 rem, depending on which of the years we were talking about. And therefore, this was the concern.

As a result of that, obviously, NIOSH had revised their approach to dealing with it by using actual radon breath analysis. And on the basis of more than 600 breath analyses data points, they were able to identify a total of I believe 449 breath analyses and then combine that with -- this was reported in Report-52 and also

the conversion of breath analysis into body burdens of radium-226, as was defined in OTIB-25. And there, Tom is to incorporate that into the Revision 3 of the TBD. We agreed that that would be the appropriate approach to doing this and resolving it.

So as a result of those revisions, and we looked at the Revision 3 of Section 5 of the TBD, and concluded that the data that was identified in Report-52 in OTIB-25 accommodate our concerns with regard to the radon exposure, the internal exposure to radium-226. And SC&A, basically, has come to the conclusion that we would recommend the change from abeyance to a closed of that particular finding.

CHAIR CLAWSON: I understand you are recommending closure of this. Anymore discussion from NIOSH or are they good with that?

MR. HINNEFELD: Yes, we have nothing to offer.

CHAIR CLAWSON: Okay, so Paul or Phil, can we close this?

MEMBER ZIEMER: I'm in agreement with that. I'm comfortable closing this one.

MEMBER SCHOFIELD: Yes, I agree with that, too. I mean there is a lot of data on that particular point.

CHAIR CLAWSON: We've knocked this one around quite a bit.

Anyway, okay, so we're going to close this one. Back to you, John.

MR. BARTON: This is Bob. I think I can take it from here. Next, again, if we are going in order, it is Section 2.5. And now we are sort of getting into the big ticket items, the uranium and the thorium and thoron.

The first one is going to be unmonitored doses to uranium, which would actually be uranium coworker model. And the coworker model was really developed in TIB-78, which the most recent revision was 3. However since, that document has essentially been merged with the TBD so there really isn't a TIB-78 anymore but all that information now really found

in the TBD.

So we had originally reviewed Revision 3 of TIB-78 and we had come up with two findings and six observations. The first finding and also Observation 3 is kind of tied into the first finding was how you deal with values that are negative or zero.

Now, the current methods on how you construct coworker models would be you take those negative values and you center them to zero. And it makes a lot of sense if you think about how we do these coworker calculations now with the time-weighted, one person-one sample approach, if you were to take a negative value at face value and apply the time-weighted OPOS calculation, you essentially would be including negative exposure into your coworker model, which is obviously not appropriate.

So current guidance has those values that are negative to be centered at zero.

NIOSH's response to this finding was well, that is correct but that method kind of

post-dates when this coworker model was calculated. So obviously, the newer calculated values are correctly at zero. My only comment here is that when OTIB-78 was merged into the TBD, it still included some language that indicated that negative and zero values were actually going to be centered at the lowest positive observed value in a given year. To be specific, the language reads: "For years with uncensored data, values less than or equal to zero were treated as being centered at the lowest positive value in that year for TWOPOS implementation." Now, that language could very well be just an artifact and should have been removed when TIB-78 was merged into the TBD.

I assume a lot of these coworker calculations are done using programming scripts, which I likely adopt those Report-53 methods, which would take those negative values and put them at zero. But I just -- because that language was in there, I wanted to sort of get confirmation from NIOSH that when you see -- when evaluating

the data which is contained in HIS-20, the HIS-20 database, that those negative values were correctly treated as zero and that the language that I just quoted, I assume, is an artifact. Am I on the right track there, Stu?

MR. HINNEFELD: Well, this is Stu. Anybody from ORAU can correct me if I am wrong but I believe that it is not really an artifact, that the old centering method was retained. I think the reason for that is that it's not a real easy process. You're right, it is done by a script that you have to be relatively careful in the application of your data treatment. So you likely have to go back to the original uncensored data set and apply some new approach, you know your new censoring approach to that and then some careful checking. And so it's a fairly difficult and careful thing to do. I think most of the people are aware that our data handling people are relatively busy because we have large coworker work going on at Savannah River and Idaho.

And as was discussed previously, there are several in abeyance items that relate to this kind of thing about the coworker model. There is, everybody I believe agrees that the quote error is slight and it will be in a claimant-favorable direction.

So, at this time, I don't think we did the work in order to change that censoring level in order to -- we didn't do it so that we could come out more quickly and be ready more quickly.

So if anyone at ORAU wants to tell me I'm wrong, I would be glad to listen to that. But that is my understanding of the situation.

MR. ARNO: This is Matt Arno. That's correct. And one thing to also keep in mind is that if someone is sampled only annually, either censoring method gives you the same answer. So, there truly is only a slight variation.

MR. BARTON: This really dealt with how those negatives were dealt with. As Stu said, if that is not an artifact, then we are taking those values that were zero and negative and

assigning a positive result. Obviously, that is going to be claimant-favorable but it sounds like this would be sort of an item that is in the queue to be changed in any upcoming revision.

Do I have that correct?

MR. HINNEFELD: Sorry, I was on mute. We feel like in abeyance is the correct designation for this. You know we're not arguing the finding because the current guidance does say censor to zero. But based on just the availability of resources to fix this and the minor impact it would have in a favorable direction, we are really to proceed with dose reconstructions the way it is, until such time as we have resources available to make that slight modification.

MR. BARTON: This is Bob. I certainly don't have any issue with that. I agree that the issue is in abeyance. I was asking whether this would eventually be updated and it sounds like it will, as resources allow. And as the coworker model currently stands and how it treats those

zeros and negatives, it is claimant-favorable until such time as it would be updated for the current guidance.

So, I agree with you, Stu. I think that this is an item that can be put in abeyance because we agree on the method of how it should be calculated. And again, at such time as there are resources able to take care of that, it will be taken care of. So at least that is what I'm hearing.

CHAIR CLAWSON: Okay, so my understanding is that we are just going to put this into abeyance and that when time and resources become available, and if it comes up, that we will change this. Is that correct?

MR. HINNEFELD: That's right, Brad.

MR. STIVER: That's right, Brad.

MR. HINNEFELD: And I want to make sure that it is clear to the Work Group that our intention is to proceed with dose reconstructions, once we can achieve closure -- assuming we achieve closure on any remaining open

items after today's meeting and wrap those up. If we wrap those up before we get a chance to revise the internal to get rid of -- or revise the coworker in order to get rid of these in abeyance items, we intend to proceed with dose reconstructions with the coworker model in its current form. Because like I said, it's a minor effect and it is a positive in a claimant-favorable direction.

So we intend to go ahead with dose reconstruction, even if we haven't cleared these in abeyance findings.

CHAIR CLAWSON: I understand, Stu.

MR. HINNEFELD: Okay.

CHAIR CLAWSON: Other Board Members, any questions?

MEMBER ZIEMER: That's a reasonable approach. Just for my understanding, Stu, what would be the effect, typically, on a dose reconstruction? Would it be moving a decimal point?

MR. HINNEFELD: Oh, I think it would

be hard to find. As I think about this, you are talking about we're building a coworker model so we're taking all of the monitored individuals in a given year and we're building the distribution of those, essentially of people because each person has a time-weighted statistic for their result.

MEMBER ZIEMER: Right.

MR. HINNEFELD: So, you'd be taking the low end of the distribution because these are censored. You know these are people who either don't have a detectable amount, these are the low end of the distribution --

MEMBER ZIEMER: Right.

MR. HINNEFELD: And you'd be changing it very slightly by censoring at a different value.

And the distribution which is important for intake is the 50 percent amount, in most cases. And so you'd be having -- you know you are messing around with the bottom end of the distribution of where you are not -- like the

mid-point or 95th percentile is what is used for dose reconstruction.

MEMBER ZIEMER: Then I'm just wondering if it is even worth changing.

MR. HINNEFELD: Well, it's a question of having resources available and there are many other higher priority activities at the time.

MEMBER SCHOFIELD: When you're talking about the resources, I mean, are we talking about a large amount of time for a rather minor adjustment?

MR. HINNEFELD: We'll, we're talking about time for a minor adjustment. I don't know if we are talking -- it's not an inconsequential amount of time but I don't really know how much time it would involve but it is a really minor adjustment. So this is not going to be really high on our hit list.

MEMBER ZIEMER: Well, I'm sort of asking if it is worth even adjusting at all. I mean if it ends up changing somebody's dose or their probability of causation by a fraction of

a decimal point, we don't have that kind of accuracy anyway.

MR. HINNEFELD: Right.

MEMBER ZIEMER: I mean we show these values to two decimal points which is, in my mind, two decimal points too many.

MR. HINNEFELD: Yes, that's another discussion.

MEMBER ZIEMER: Yes, that's another discussion. But anyway, okay, at this point it should remain in abeyance. I'm good on that. I just wondered how much difference it is even going to ever make.

MR. HINNEFELD: Right.

MEMBER ZIEMER: Okay.

CHAIR CLAWSON: Are you good with it, Phil?

MEMBER SCHOFIELD: I'm good with that.

CHAIR CLAWSON: Okay. Back to you, Bob.

MR. BARTON: Okay, so Finding 1 will remain in abeyance and, as pointed out, it is not

going to have a great amount of consequence in the way the coworker model is constructed and the way it treats those values as claimant-favorable anyway. So, that's in abeyance.

And we can move on to the second finding related to uranium coworker model. And this had to do with what we observed and we called them paired bioassay measurements. And what we saw was that for an individual worker, occasionally, they would have two bioassay entries on the same date. And the two entries were most often off by an order or two orders of magnitude. That is, you would see an individual worker in the HIS-20 database who had one results that would say one microgram per liter and on that exact same day, a result that was 100 micrograms per liter.

And so we brought that to NIOSH's attention and they provided a response to that. They did some extensive work looking into that phenomenon. I attempted to summarize the findings of their investigation but it is

probably going to be more informative to the Work Group to have NIOSH explain it about what they did about the issue, what they found, and how that is being dealt with, if they would like to take the floor now. Otherwise, I can kind of muddle my way through what they did and what the resolution of that was.

I'll let NIOSH explain their investigation and what they found first.

MR. HINNEFELD: Boy, I don't know. Is somebody from ORAU, is that fresh in any of your minds? I guess not.

MR. ARNO: This is Matt Arno.

MR. HINNEFELD: Go ahead, Matt.

MR. ARNO: I guess I probably know the most about it but it is not fresh in my mind. We did discover that there was an issue and commonly things were off by a factor of a hundred, with occasional instances of it being off by a factor of ten or a thousand.

Our conclusion was that, generally speaking, the higher result was the correct

result in almost all instances. So we went through the data set and excluded results that were part of these order of magnitude different pairs to only use the higher results and then reran the coworker study.

MR. BARTON: So, in essence, what was found is that when you see those two pairs, evidence points to the fact that the higher result was the correct one and the low result was likely, when it was imported into HIS-20 --

MR. ARNO: Yes, and associated, I would cover that if the result had two decimal places in HIS-20, it was incorrectly imported, regardless of whether it was a paired result that was an order magnitude or more higher. We adjust to those as part of our period that there was a vetted import area and then for the rest of them, we just used the higher result when there was a pair.

MR. BARTON: Right, that was sort of a second part that wasn't actually part of SC&A's original finding but that's --

MR. ARNO: No, that was our observation, as we investigated your finding.

MR. BARTON: Right. And you know SC&A has reviewed what NIOSH did to investigate the matter. We agree with their approach. Certainly, when you have those two questionable pairs and given the evidence, we are not just picking the higher value because it is the higher value; the evidence suggests that that is the correct value.

So, SC&A agrees with the investigation that NIOSH performed and agrees with the conclusions. And so certainly we would entertain any questions but, ultimately, we recommend that Finding 2 related to the uranium coworker model to be closed.

CHAIR CLAWSON: I understand. I concur with that.

Paul or Phil?

MEMBER ZIEMER: Yes. Sorry, I was on mute. After listening to you, I agree with that.

CHAIR CLAWSON: Phil?

MEMBER SCHOFIELD: Yes, this is Phil. I agree with that.

CHAIR CLAWSON: Well, that one is closed. Next one?

MR. BARTON: Okay, so those were the two findings associated with the uranium coworker model. We also had six observations, which I think we can go through fairly quickly.

The first observation was that, for certain years, in particular the late '80s and early '90s, we at SC&A just couldn't recreate the time-weighted OPOS values and resulting calculations for certain years.

Now, we made this an observation, or I made it an observation, because I felt that I was probably missing something and using the wrong method.

So what happened is we exchanged some calculation files with NIOSH, tried to resolve why our numbers weren't matching for just those particular years. The other years we were able to match up very well. And it turns out that we

were using Procedure-95, which was titled "Generating Summary Statistics for Coworker Bioassay Data." But because the years in question had such a high number of censored results, the Procedure-95 method for doing the calculation was not appropriate and that region of squares method that had been developed in Report-53 for just such occasions when you have data sets that have a very high number of censored results was the appropriate way to do the calculation.

And NIOSH's response, once they saw our calculation file, they were able to use our OPOS results, along with the Report-53 method. And then everything sort of matched up very well. So, the original -- again, it's an observation. We couldn't match the values, as it turns out, because we were using a different procedure.

As I said, NIOSH looked at our calculation file and said, well, you have maybe the right OPOS statistics but not the correct interpretation of it when you go to fit it to

distributions and what not.

So we understand what the source of the discrepancy was for those years. Again, we're talking about late '80s and early '90s, when the data that we see is very heavily censored. We accept NIOSH's explanation and we recommend that Observation 1 be closed.

CHAIR CLAWSON: Okay, this is Brad. I understand. I agree, we'll close it. Any discussion from other Board Members?

MEMBER ZIEMER: No. This is Ziemer. I agree also.

MEMBER SCHOFIELD: This is Phil, I agree also.

CHAIR CLAWSON: Okay.

MR. BARTON: Very good. Moving on to Observation 2. This is really, this is a true observation because this is one of the very first coworker models that you used the time-weighted one person-one statistic approach to calculating the coworker distributions.

We noted that the magnitude did not

actually change much at your median level but that the variability did decrease significantly by using that time-weighted one person-one statistic, as opposed to what's referred to as the pooled approach, where you essentially fit distributions for all the data as-is, rather than generating a value for each individual monitored worker in a given year.

So that's just an observation. I mean, it can be closed as an observation. It was to be expected that you probably wouldn't see that much of a change in magnitude at that 50th percentile level, but we felt it was worth noting simply because this was one of the first coworker models that was calculated using the time-weighted approach.

CHAIR CLAWSON: Okay, so noted and we'll proceed on.

MR. BARTON: Okay. As I said, Observation 3 was sort of tied in with Finding 1 in that the HIS-20 itself, the database itself, didn't always deal with sort of the low numbers

down near the detection limit. What I mean is sometimes you will see in the comment column that the sample was noted as below the detection limit and the result was listed as zero. Sometimes it will say below the detection limit and the result was noted as the detection limit. And sometimes it was some value in between.

So that's the database inconsistently dealing with those lower end numbers. And, again, it's tied in with Finding 1, which we put in abeyance, and to deal with those values that are below the detection limit, zero, and negative.

So I think that is sort of tied at the hip with Finding 1. So I would say that that one, like Finding 1, would probably be in abeyance. Or we can close it and just simply leave Finding 1 open, however the Work Group would like to move forward with that.

CHAIR CLAWSON: I think where it's tied up with Finding 1 and it's in abeyance right now, I don't -- well, I don't know. I don't see

any real benefit of that. We've already agreed to that.

So, I guess, other Board Members, if you've got any --

MEMBER ZIEMER: Well, it sort of doesn't matter what you do with it because you are going to handle it through the finding. But maybe we could just go ahead and keep it open since it ties together, or keep it in abeyance.

CHAIR CLAWSON: Okay, I agree with you on that, Paul. And I'm sure you agree with us, too, Phil. And we'll just proceed on.

MEMBER SCHOFIELD: Yeah, I mean, we'll just keep it there.

CHAIR CLAWSON: Okay, sounds good.

Go ahead, Bob.

MR. BARTON: It's really just a bookkeeping thing. So, I guess we'll hold it in abeyance for now. Okay.

(Simultaneous speaking.)

CHAIR CLAWSON: Well, I still don't want to lose it, either, that's part of my thing.

MR. BARTON: Okay, very good. Observations 4 and 5 both involve comments that are in the HIS-20 database associated with individual entries. In some of those cases, the column will contain an actual numerical -- the comment contains a numerical result; whereas, the result listed in the actual analysis column is the censoring level.

So, that was the subject of the Observation 4. We said, well, how are these comments possibly -- how could they possibly be incorporated into the calculation of coworker intakes? In the case of where you have a numerical result in the comments column, would that value be more appropriate to use than the censored value, which appears in the results column?

And then Observation 5 was similar. In the comments column sometimes samples would be labeled as pre-employment, re-employment, or actually labeled as invalid. And certainly those three types of samples are not included in a

coworker model because they're not really representative of intakes that occurred at the site.

Obviously, if it's pre-employment, re-employment, that would be reflective of any intakes that occurred prior to working at Fernald. And then if a sample is labeled as invalid, then that probably shouldn't be included.

Now, in response, NIOSH stated that in future revisions of the coworker model that these types of comments would be taken into account.

When we looked at the TBD, which has OTIB-78 merged into it, it wasn't clear to us that at least those pre-employment or invalid samples had been taken out. And the reason I say this is that the total number of samples reported for any given year appear to be identical for the version of the report that we reviewed and also the version that appears in the TBD.

So, I guess we had asked if NIOSH had gone through and removed those samples which are

inappropriate for coworker modeling, and also if, in this most recent revision, those other comments where you have numerical results in the comments but a censored value in the results, if there had been any adjustments made.

Based on the earlier discussion for Finding 1, perhaps that was not done at this juncture, but I'll turn it over to Stu and his team if they would like to comment on that.

MR. HINNEFELD: Well, I don't recall. I don't recall if this was done or not. Matt or anybody on the phone, do you recall if these were taken out in this version?

MR. ARNO: This is Matt Arno. This, I guess, observation was merged with the other two or other three issues there that were considered low priority and these have not been addressed.

MR. HINNEFELD: Okay. This, again, would be a relatively small population, maybe at the bottom end of the distribution. So we don't feel like it's enough -- there wouldn't be enough

data to really significantly skew the 50th or 95th percentile results. So, we considered it a low priority item that would be taken care of in the future.

CHAIR CLAWSON: Okay. Well, I guess that's where we're going to leave it. So what would you suggest be the best, just leave it in abeyance, then?

MR. HINNEFELD: Yeah, I think abeyance would be the right category.

CHAIR CLAWSON: Okay, any discussion? By the way, this is Brad. Any discussion from other Board Members?

MEMBER ZIEMER: I'm good with that. That should be both Observations 4 and 5, right? Isn't that what we're talking about or just 4?

MR. HINNEFELD: Yes, I believe we're talking about both 4 and 5.

MEMBER ZIEMER: Yeah, 4 and 5. Yeah, I'm good with that.

CHAIR CLAWSON: Okay, Phil?

MEMBER SCHOFIELD: I'm good with that,

also.

CHAIR CLAWSON: Okay, we'll leave this in abeyance then and proceed on.

MR. BARTON: Okay, this brings us to the last observation, Observation 6, related to the uranium coworker modeling. And this is related to what we saw in the HIS-20, where individual urinalysis results would often include a designation of the intake mode, either labeled as inhalation or ingestion, and also would sometimes include the solubility type, you know, days, weeks, years, that sort of thing.

And we asked, you know, is that information -- can that be used to more accurately calculate coworker intakes? And NIOSH's response is that we really don't know the source of that information and how we've calculated it, how they determined the solubility type, how they determined that the intake was an ingestion versus inhalation.

And so it's pretty standard practice in the program that when you don't know that

information, or that information is potentially unreliable, you calculate the intakes assuming each of the solubility types and you use the most claimant-favorable in each individual case.

And so that's fine. It was something we wanted to point out, again, as an observation. There is some indication that the site had included the mode of intake, ingestion or inhalation, and the solubility type. But if we can't establish the credibility of that information, then we agree with NIOSH that it probably should not be used to make any adjustments or do any calculations based on the solubility type reported or the mode of inhalation. So we agree with NIOSH on that one and we feel that that can be closed.

CHAIR CLAWSON: Okay, I understand. I agree with you that it be closed.

Any other Board Members?

MEMBER ZIEMER: Right, they're going to end up using most claimant-favorable approach. So I'm good with closing that.

MEMBER SCHOFIELD: Sounds like the reasonable thing to do.

CHAIR CLAWSON: Okay, I agree, we'll close that one. Next?

MR. BARTON: Okay, so that is it for uranium. If it's amenable to the Work Group, we can move on to thorium, the coworker modeling -- .

PARTICIPANT: Hello?

CHAIR CLAWSON: Hello. Somebody wanted to make sure that we're on the line?

Okay, continue on. Sorry.

MR. BARTON: Okay. We can move on to thorium, which is not only thorium-232 assignment, but also intakes of thoron, which is similar to radon except it is part of the thorium chain instead of uranium; and also the issue of potential unsupported radium doses. And that is Section 2.6 in SC&A's review.

So we had seven total findings relate to thorium, thoron, and unsupported radium. Findings 1 through 3 relate to -- well, let me

backtrack a little bit.

When we're talking about assignment of unmonitored thorium doses, what we're doing is we're using in vivo results for lead and actinium, basically the daughter product of thorium, and using those in vivo counts to back-calculate an intake of thorium-232. Now, the technical aspects of that have been thoroughly vetted at numerous meetings. We include assumptions such as the triple separation, corrections for bias, and that sort of thing.

So, what was left, and NIOSH had produced a report back in 2014 that dealt with basically all dose reconstruction topics related to thorium. And that was first discussed in September, and then SC&A issued a report, which we discussed in December. We came to pretty good agreement. We did have two findings that were discussed.

Findings 1 through 3 relate to the first period we're looking at, which is 1979 to 1989. This was where the mobile in vivo counter

was used and we're using those daughter products of lead and actinium to reconstruct thorium.

And the three findings for that period were pretty much all related to implementation; not so much what the calculated values are but who is going to be assigned thorium intakes and what level.

Finding 1 discussed that most of the activities are going to be related to re-drumming and repackaging, but we did find evidence that there might have been some small-scale handling and maybe a bit of processing. And the big problem is, at this site and many other sites, we can't really identify who those workers were.

This is very similar to Finding 3 in which we noted that, when going through claimant files, and, in particular, the CATI reports, that there were a wide variety of job titles that could have been exposed. And, again, we don't really know who those thorium workers are. So, you know, how do we really go about assigning those doses?

This was discussed pretty

extensively. The original NIOSH White Paper on thorium had given some illustrative examples of what types of jobs would be assigned the thorium intake. And we discussed that, and we kind of came to the place where you really have to assume that the thorium exposure occurred, unless you really have convincing evidence that it just simply wasn't possible. And that convincing evidence would essentially be your administrative workers had never really entered these radiological areas and so would not come in contact with any thorium. And so it really wouldn't be appropriate to assign coworker doses to those people. But, I mean, if you were in a position where you could have entered those radiological areas, it would be appropriate to assign that dose.

And in the TBD, that language is pretty much in there. And I'll read this sentence. This is from Section 5.5.2.3.1, for the record. It says: "Thorium coworker doses are assigned to all monitored workers, unless there

is a reason why they should be excluded; for example, a secretary or administrative individual who worked only in non-radiological areas."

And that's pretty much exactly the language we had talked about during December. We think that's the right way to go. And that was really the purpose for Findings 1 and 3, saying, on the question of who you do assign thorium exposures to, it's pretty much anyone that could be considered a radiological worker, and the assignment of coworker doses is only withheld if you had that type of job title where you just never really entered radiological areas and so it's inappropriate to be assigning any unmonitored thorium exposure.

So, that's Findings 1 and 3, which we recommend, based on that language in the TBD, that those be closed.

CHAIR CLAWSON: I understand and I agree with that. Any other Board Members?

MEMBER ZIEMER: I also agree with that.

MEMBER SCHOFIELD: I have no disagreement with that.

MR. BARTON: Okay, Finding 2 gets a little trickier. As I said, the first three findings are all related to who gets assigned intakes and then at what level. Who gets assigned intakes, we just talked about, is essentially anyone who can be considered a radiological worker.

Now, at what level do we assign these intakes? Normally, with coworker modeling, the way the program is sort of developed is, you know, you develop a distribution, you have a median and a geometric standard deviation, and then you also have the 95th percentile that can be applied as a constant for highly-exposed workers.

Finding 2 pointed out that the monitoring program that we're using for this earlier period, the 1979 to 1989, is really a uranium monitoring period. They used the in vivo counter to monitor for uranium exposure, but as sort of a byproduct, they also listed out these

lead and actinium results.

And so we felt that since, A, you don't know who was actually exposed to the thorium materials, and since the program was actually geared towards uranium and not focused on workers necessarily who were handling thorium, we felt that it might be appropriate and representative to apply the 95th percentile as a constant. Again, simply because you have so many samples that are geared towards these uranium operations that it might, I guess, for lack of a better term, dilute the sample of workers who actually handled thorium. Which, you know, if we had just a sample of in vivo records for thorium workers, then that could be considered representative. But since we really don't have that, we felt that using the upper end of the distribution and applying that as a constant might be appropriate here.

Now, the question of at what level you assign coworker intakes obviously changes from site to site, and it is a matter of professional

judgment, I guess. But we wanted to bring that to the Work Group's attention for discussion again.

And I'm going to find the exact language that was in here. Okay. Here's where the TBD addresses the 95th percentile. It says: "The 95th percentile intake rate, with a constant distribution, is assigned to those with the highest potential for exposure. Workers with a baseline thorium fecal sample are included in this group, as well as subcontractors from IT Corporation working during 1988 and 1989. All others are assigned the 50th percentile intake rate within a log-normal distribution."

So there is the mechanism in there. It says -- it sort of appears that you had to have the fecal sample to be included in that 95th percentile group, or have worked for IT Corporation. I don't know if those two designations are simply illustrative or if those are the two groups that would get the 95th percentile.

As I said, SC&A felt that, given the fact that the monitoring data we're using is not targeting thorium work per se, but it is likely capturing it, and coupled with the fact that we don't know who the thorium workers are, that, in this case, for Fernald, it might be appropriate to assign that 95th percentile to all radiological workers.

And we wanted to bring that up for discussion, because obviously this is a matter of judgment, not necessarily technically right or wrong, but more a matter of policy.

MR. HINNEFELD: Yeah, this is Stu. I'll explain a little bit the basis behind our judgment here. And the fecal sample and IT Corp is not -- those are not considered examples. Those are considered the people who would be getting the 95th percentile.

The reason for that, our basis for this reasoning, is that when you look at the '79 to '89 period, they're really two pieces there. There is '79 through about 1987, when the thorium

was sitting in warehouses and occasionally there would be a short-term repackaging effort. There were some seriously degraded drums. They might do a repackaging. That would be a fairly short duration job by these people.

And, occasionally, there were some job orders for thorium during that period, at least the first half of that period, but they generally appear to be providing 50 kilograms of thorium to GE, or a couple hundred kilograms of a particular thorium compound to so-and-so, which would have been retrievable. Some of this thorium was good, quality compound. Some of it was junk. Some of it was good, quality thorium.

So, that would be the retrieval of those materials from the warehouse, probably the Plant 1 warehouse, and shipping it to those materials. So you don't really have a regularly -- and in either case, it is a short duration task. I mean, just like overpacking, it's a short-duration cask.

So you don't have anyone who is

regularly exposed to thorium. Our general guidance on coworker models is -- for a coworker model, if someone that is not monitored and is occasionally exposed, they get the 50th percentile. If someone is not monitored but it looks like they are probably regularly exposed, they get the 95th.

So, in our view, from this period of time, from here on top of the '79 through roughly '87, no one was regularly exposed to thorium. And so the 50th percentile is appropriate.

From that, and then, starting about '88-'89, there were some things that were being done to disposition thorium. The IT project was the removal of the thorium from the bins and silos in Plant 8. And then I think the '89 would have been Westinghouse also engaged in some more serious either repackaging or packaging of this. And so they were asking for fecal samples on these thorium projects.

So, those are the people who are likely to have more than just an occasional

periodic thorium exposure. So that's why we chose what we did, and that's why we think that what we've chosen here is appropriate.

MR. BARTON: Well, let me kind of respond to -- comment to that. As a general guidance, if you're occasionally exposed, you get the distribution. And if you're routinely exposed and not monitored, you get the 95th percentile, which I can agree with. But your coworker model is based on data for workers who were all occasionally exposed. There is no routine exposures. So since the coworker model itself, the distribution, is based on occasional exposure, I'm not sure it's the same thing.

I agree that, when you have a situation of a coworker model based on workers who were routinely exposed, then that sort of framework definitely holds. But if your coworker model is based on workers who are already only occasionally exposed, I'm not sure it applies, necessarily, here.

Again, it's certainly a matter of

policy, not necessarily a technical issue. And we wanted to bring it up. I don't know if I can offer anything else besides that.

MR. HINNEFELD: Well, I can only offer that I understand, I understand your point, but the coworker model gives an annual intake to everyone who is included in, you know, who gets the coworker approach. And it may be the thorium monitoring data is not very high, there may be missed dose in every year, but they get an annual intake.

And so, to me, I don't see how that doesn't bound the thorium exposures of the workers, given the very minimal activity of thorium that was going on during those years.

MR. BARTON: I mean, like I said, again, the coworker model is not geared towards those thorium workers, but you would expect that, even though they are occasionally exposed, if exposures did take place during that occasional work, they would be associated with the people who work with thorium and they'd be in the higher

end of the distribution.

Now, if everybody who was exposed at that higher end was monitored and we could prove that, then, you know, I would say case closed. But if you had workers who were possibly not monitored who did that occasional work, I think you would want to use -- to be representative, you'd want to assign them the coworker doses to the same workers who were doing the activities they were involved in.

I guess, to my mind, I would think those would be in the upper end of the distributions, even though it is probably quite low, near a missed dose.

And, again, I brought this up as a matter of discussion to the Work Group, just as a matter of policy. We feel that 95th percentile may be appropriate here. It's clear that NIOSH believes that what's currently in place is bounding. I'm not sure I can offer up anything else by way of technical arguments one way or the other. I can certainly answer any questions the

Work Group has or if they'd like to discuss how they would like to move forward.

MR. HINNEFELD: Yeah, I'd like to make one more comment about the monitored populations. And, granted, the vivo monitoring, at this time, '79 through '89, essentially all the work on the plant was uranium and the thorium was sitting in warehouses.

The monitored population was the people who worked with the uranium. And those same people would be the same who would do this occasional task with thorium. So, in all likelihood, the thorium workers are monitored, despite the fact that the thought process wasn't "we need to monitor these people because they are going into the thorium warehouse and getting this stuff." They were monitored because most of the time they were working with uranium.

So I think the people who actually did the work with the thorium are probably included. They have their own monitoring data. And so they won't be getting the coworker data.

MR. BARTON: But you're not arguing that everyone who was monitored, or everyone who should have been monitored was monitored, certainly.

MR. HINNEFELD: Well, I'm not making that argument or we wouldn't have a coworker model.

MR. BARTON: Right. And I agree that the same people who did the uranium work would have likely done the thorium work. But all those records are mixed in with people who only did the uranium work. And to my mind, the people who actually did the thorium work would be in the upper tail of the distribution, which includes both, we'll call them the uranium-thorium workers and the uranium-only workers. And I guess that was our thinking.

MEMBER ZIEMER: This is Ziemer. Stu, can you remind me again, what's the database for the coworker model? What went into that?

MR. HINNEFELD: Okay, for the mobile in vivo counter, all the counts are available.

So, the database for '79 through '89 for thorium is based on the mobile in vivo counting data.

MEMBER ZIEMER: So it's not based on unmonitored workers.

MR. HINNEFELD: No, it's the monitored workers.

MEMBER ZIEMER: Yeah, I thought John was saying that the coworker model was based on unmonitored workers.

MR. HINNEFELD: Well, it's applied to unmonitored workers.

MEMBER ZIEMER: Yeah, I know, but it's not based on them. So the distribution is a high distribution to start with. It's a monitored worker distribution, right?

MR. HINNEFELD: Well, Bob's point was that the -- well, yeah, it is the monitored distribution.

MEMBER ZIEMER: Right. So that an occasional worker, short-term mechanical worker on that thorium work that you described, in short-term, I expect the 50 percent level finding

that would be appropriate.

MR. BARTON: Well, Dr. Ziemer, let me clarify, though. When we say it's a distribution based on monitored workers, it's a distribution based on workers monitored primarily for uranium. Now, that will include some workers who also did the thorium work. But lately, by and large, those are people who did uranium work. So there are going to be thorium workers in there. And by the nature of the mobile counter, in addition to the uranium results, it spits out the thorium data that we can use to reconstruct. That's only going to be some fraction of the total distribution that we're looking at, because, again, these are mostly uranium workers, uranium-only workers, with some fraction that are uranium workers who also did thorium work.

So, if we were talking strictly about uranium here, then, yes, you have a monitored worker population and occasional work with uranium would absolutely be appropriate to use the full distribution 50th percentile.

But the point being that, even though you have thorium workers within this monitored uranium population, those people who did thorium work are more than likely going to be in the upper tail of the coworker doses calculated from that full distribution, which, again, is slightly diluted to some extent. We really don't know who were the workers that never did any of that overpacking activity.

MR. HINNEFELD: Well, Bob, in order to take your position you would have to say that there were workers who did work on the thorium, who were exposed to a similar degree as the people who worked on the thorium, but never got in the in vivo counter for some reason.

MR. BARTON: That's correct. I mean, that's why we have the coworker model, isn't it?

MR. STIVER: Stu, this is John. Maybe --

MR. HINNEFELD: We have the coworker model because there are a lot of people who we wouldn't exclude from thorium dose because we're

applying it to such a broad population, that there will be people who don't have an in vivo count because we are applying it to such a broad population because we don't really know who's in and out.

I think it is a leap of faith to go from that point to say that there were people who were involved in these thorium overpacks or retrieving the thorium out of the warehouses for shipment that never got an in vivo count.

MR. STIVER: Stu, this is John. Can I ask you a question here?

MR. HINNEFELD: Yes.

MR. STIVER: The baseline fecal sampling, was that only for those workers who were intended to be doing the repacking, and that would not apply during the '79 to '89 timeframe?

MR. HINNEFELD: It would not apply during the early part. I don't think you'll find any of the fecal samples during part of that '79 to '89. You'll find some, I think, toward the end. I think.

MR. STIVER: I guess my concern regarding that criterion is that are we sure that everybody who was doing the thorium activity, the thorium work, I guess, on a regular basis would in fact have a baseline fecal sample? We're kind of assuming that's 100 percent reliable and we're not going to miss anybody.

And so the problem we've had with Fernald is you never really know who's doing what activity. It looks like that's why we're using that hook to get back to, you know, who to give the 95th percentile. I'm just wondering how reliable all that data is in terms of capturing everybody who would have been in that high potential exposure group.

MR. HINNEFELD: Well, there was a qualification process for working on the thorium overpacking when they got started, toward the end of this period. People had to be qualified to go do it, and part of that was I think was the baseline fecal. I think there is documentation to that effect that in the records of some of the

stuff we looked at.

MR. STIVER: Okay, but in the '79 to '89 timeframe, what I'm hearing, then, is that everybody would essentially be getting the mean and not the 95th percentile because there was no --

MR. HINNEFELD: Well, everybody would be getting, yeah, the full distribution of --

MR. STIVER: Yeah, right.

MR. HINNEFELD: Except for the regularly exposed people during the last couple of years that we can identify based on fecal sampling or IT affiliation.

MR. STIVER: Oh, okay.

MEMBER SCHOFIELD: This is Phil. Is somebody list you guys ran across by any chance that has job codes for those who worked with the uranium?

MR. HINNEFELD: You mean job titles or --

MEMBER SCHOFIELD: Yeah, job title or maybe a code of some type that identifies people

strictly as uranium workers.

MR. HINNEFELD: No, I don't think so.

CHAIR CLAWSON: Phil, this is Brad. I believe that they were all considered uranium workers. The thing is, is that when these jobs would come in, they would just take the uranium workers and go out and do the thorium.

MR. HINNEFELD: Yeah, for the early part of this period, '79 to say about '87, I think they would just take people, if they were getting stuff out of the warehouse to ship, that they would just take people to the warehouse and ship it. There wouldn't necessarily be a qualification for that.

Or if there were people who were -- since there was a small group of drums they felt we really need to repackage these, then they would go for a couple of days, or whatever the duration was, and repackage a few drums. They would just take people and do that. There wasn't a qualification step for that.

Toward the end of the period, when

they started getting serious about thorium remediation, retrieval, and disposal, you know, getting it off the site, then they started talking about thorium project, thorium -- in fact we sort of described this project, I think, in the Site Profile, or certainly in our thorium document. At that point, then, people had to be qualified to work on those projects. And so there was a specific activity to say, okay, these people are ready to go work on the thorium project, and that was where the fecal samples came from.

MEMBER SCHOFIELD: And we're basing that on their samples that we've got, correct, Stu?

MR. HINNEFELD: Yes. Yes.

CHAIR CLAWSON: But if I remember right -- this is Brad, I'm sorry. If I remember right, a lot of this sampling and stuff, at the very beginning of it, we found documentation that actually a lot of the workers weren't being sampled. It was the supervisor to show them that

this was working and so forth.

And I'm just wondering how -- I understanding what your point is. We're giving 50 percent of this to uranium workers and the 95 percentile will go to the actual thorium workers, but I'm really wondering if we really have a good handle of who was actually the thorium workers. We do have some that have been sampled, but I do not know that we are capturing them all until, like you say, the last couple of years, possibly, when we were in the heavy process of removing the thorium from Fernald.

MR. HINNEFELD: Well, my point is that that was the only time when there was a really consistent significant thorium exposure, which was when they actually started getting into the removal of the thorium to get it off Fernald, just the last couple of years of this '79 to '89 period. And so for that reason --

MR. STIVER: Hey, Stu, this is John again. You know, during that ten-year period from '79 to '89, or at least the eight years or

whatever before they started doing the repacking, you did mention, and I had read a lot about this, due to the corrosive nature of the thorium product that there would be a lot re-drumming and so forth going on. I guess it was part of the maintenance activities.

And I don't know, it kind of seems to me that there might be the potential for a significant intake to people who were working during that time, even though it would have been short-term. So I'm trying to reconcile whether the full distribution of the 95th percentile would really be appropriate. There's a possibility we could be missing people.

CHAIR CLAWSON: John, are you talking about -- excuse me, Stu, I'm sorry. You're talking about out there on the train cars and stuff, the re-drumming of the thorium?

MR. STIVER: No, Brad, not the re-drumming, that was the later period where they actually did have qualifications and baseline fecals and so forth. I am talking about that

kind of interim period.

(Simultaneous speaking.)

CHAIR CLAWSON: When I say re-drumming --

MR. STIVER: -- there had to be drumming and so forth going on just as part of the routine stewardship, for lack of a better term.

CHAIR CLAWSON: So you're talking about while it was sitting there and it was corroding away.

MR. STIVER: Yeah, this would be the time that Stu is indicating that they would assign the full distribution because those people weren't documented as potentially highly exposed.

I'm just wondering if some of those sporadic short-term activities could still result in significant exposure in a situation that you would think the 95th percentile would probably be more appropriate.

MR. HINNEFELD: Well, my view is that, no, there is not the opportunity for the kind of

exposure you would expect that would be so high that we might consider these people regularly or highly exposed.

My view is that, throughout the maintenance period, that sort of just, you know, keeping the inventory intact period, the exposures were few and far between.

MR. STIVER: Yeah, I understand that. I'm just thinking, for the guy who might have been in one of those short-term exposures, could he have gotten such an intake over a relatively short period of time that would still result in a high dose? In which case you might miss him, you know, by using the full distribution as opposed to the 95th percentile. I'm just trying to think --

MR. HINNEFELD: Yeah, I understand what you're saying. I just don't envision it happening. I mean, the level of thorium activity -- I mean, I was there during this period and the level of thorium activity was like almost nonexistent.

MR. STIVER: Since you were there, I mean, do you remember how often the material had to be repackaged and so forth just to keep it --

MR. HINNEFELD: I don't remember -- well, I don't really, personally, specifically, ever remember it happening. I know we have seen -- we have at least one reference we refer to, I think it's in our post-SEC thorium paper, of a report that was written about a particular overpacking job. You know, I haven't chased that down or tried to chase that reference down. But my understanding is it was a matter of a few days. So, it's just not -- and it was a special enough event that the coverage, it was covered by the industrial hygiene radiation department and report was written about it.

MR. STIVER: Yeah, I'm just thinking, we kind of worked on this under the 250-day issue, and I know Hans had been involved in that a number of years ago where I think we had demonstrated that one snoot-full in a certain instance can still be enough to result in a pretty high dose.

So, granted, those would be pretty small -- pretty unlikely events. but if you can't categorically show that those didn't happen, not to get you to prove a negative, but would be it possible and likely enough that that could have happened? And in such a situation, maybe the full distribution would not really cover that particular worker.

I know there's a lot of conjecture going on there.

MR. HINNEFELD: Yeah, well, you're also conjecturing the big sudden intake from one of those overpackings and also that it happened to occur to a person who wasn't monitored. Because if a person was monitored, you've got it in his in vivo results.

MR. STIVER: Yeah, but for the guy who wasn't monitored who would be assigned the coworker dose, is it really such a remote possibility that it is not even worth considering?

MR. HINNEFELD: Well, see, the in vivo

monitoring schedule for in vivo, for the mobile counter, since it wasn't there all the time, it was based on your occupation. And so the production people were monitored, what, annually maybe, and sometimes more often if they had a detectable burden. And the people like maintenance and transportation, and probably some in health and safety, were monitored less frequently, like every two or three years, something like that. But there was probably frequency for monitoring all these people and they did what they could to try to get everybody through there. The mobile counter visited I think twice a year, usually. And so in that amount of time, you had the schedule them and try to get through everybody on their schedule.

So, to me, you're postulating a lot. You are theorizing a lot, that, A, there was this big exposure to somebody and that person, through one of these short-term projects, and that person also happened to be somebody who just got missed and never got counted in the in vivo schedule.

MR. STIVER: Yeah --

(Simultaneous speaking.)

MR. HINNEFELD: So there's a lot of speculating going on to get you to the point where you feel like there should be a 95th percentile assignment on a coworker.

MR. STIVER: You know, I was reading through the transcripts the last couple of days and this issue came up about claustrophobes and people who just didn't want to get monitored.

MR. HINNEFELD: Right, the guy is assumed be a claustrophobe. So that is speculation, then, that a guy involving a short-term repackaging was claustrophobic and wouldn't get in the monitor.

MR. STIVER: Yeah, you are taking very small probabilities and multiplying them together. That's pretty unlikely. I just wanted to put it out there.

MR. BARTON: This is Bob. I don't want to beat this thing to death here. So let me just try to posit it maybe another way.

What we're really talking about is representativeness here. Now, thorium activities were intermittent, occasional, however you want to describe them, but there was some potential there for thorium exposure. Otherwise, we wouldn't be trying to reconstruct the thorium doses.

Now, when you look at this distribution, and those thorium workers -- some of those thorium workers are clearly going to be in there. And if they were, even though exposures were intermittent, it may have been low level, those people are probably going to be best represented in that upper tail of the distribution.

Now, what I'm hearing is, though, that NIOSH's position is that if you were involved in those activities which were intermittent, which involve workers who would be in those upper tail of the distribution, they all have monitoring records. In other words, there aren't any workers who were directly involved with those

thorium activities who went unmonitored, which is a pretty big assumption in itself. That's why we came up with the coworker model, is that we felt that there were workers who could have been involved in these activities who maybe didn't get monitored, or were only monitored for part of their employment.

So if it's NIOSH's position that really all the people who were involved in these intermittent thorium activities have records and those records cover their entire employment, well, then I almost question whether a coworker model is necessary, if that is, in fact, true, that we don't have unmonitored workers who could have potentially been side-by-side with those workers who occasionally handled thorium; which would be represented best by that upper tail of the distribution, given that this distribution contains a lot of workers who never touched thorium.

MR. STIVER: Yeah, Bob, you're right. It really gets to a policy decision, then. When

there is a reasonable degree of confidence whether an exposure did or didn't happen? And then how are you going to layer on some claimant-favorability to do that?

I think we pretty much have gone as far as possible from a technical standpoint, at least in my opinion.

MR. HINNEFELD: I'm just struggling with the thought. You know, what we have here is, does the TBD -- maybe ORAU can help me out. Does the TBD go so far as to list these are, say, the daily intakes for these years based on the coworker model?

(Pause.)

MR. HINNEFELD: Maybe they can't help me out.

MR. STIVER: Hang on a second. I missed that. It cut out.

MR. HINNEFELD: I was wondering if someone from ORAU could help me out.

MR. STIVER: Oh, okay. I'm sorry, I thought you were talking to me. Sorry.

MR. HINNEFELD: No, I'm wondering if the Site Profile -- or maybe an SC&A person who's read it, who's read it recently -- does the TBD focus get so specific as to lay out these are the, you know at the 50th percentile, this is the daily intake of thorium that the coworker would be assigned?

MS. KENT: Stu, this is Karen. We just have them specified from '79 to '89. They are not specific to each year.

MR. HINNEFELD: Right, but what's it say from '79 to '89?

MS. KENT: Well, we have thorium intakes of type M and S for 50th percentile and then 95th.

MR. HINNEFELD: So they are both in there?

MS. KENT: Yes.

MR. HINNEFELD: Because we were going to use the 95th for people who have fecal samples.

MS. KENT: Correct.

MR. HINNEFELD: Okay. Well, what is

the 50th?

MS. KENT: Well, they're specified for thorium-228 and -232. So, for example, the 50th percentile for type M thorium-228 is 6.71 picocuries per day and thorium-232 is 35.3 picocuries per day.

MR. HINNEFELD: Okay. So, we have a daily intake in this. You say those are the 50th percentile?

MS. KENT: Yes.

MR. HINNEFELD: Okay, so, 35 picocuries per day. You know, to me, thinking of the thorium activities at the time, I don't understand how that's not bounding for everybody, at least through '87 when you start getting into the actual work on it, you know, when they are actually starting to retrieve and get rid of the thorium.

It is just hard for me to say in good faith that, you know thinking of all -- you know, we built this model because we don't want to leave anybody out, because there could be people who

were not monitored who wandered into the thorium warehouse or somehow got involved in that. So we've got this coworker model, but it's not sufficient because we want now to consider people as regularly exposed and more highly exposed when, in fact, I know that no one was regularly exposed.

MR. BARTON: Again, this is Bob, I think it's just the issue of what part of this distribution is actually representative of --

MR. HINNEFELD: Yeah, I understand all that. I understand all that. I just -- I mean, we're dealing -- really, we're dealing with crumbs on a really favorable dose reconstruction.

MEMBER ZIEMER: Yeah, this is Ziemer. Let me add an additional thought. The coworker model is really a kind of summation of two distributions, that there's the thorium distribution from those thorium activities that basically are already monitored, plus the uranium distribution from those uranium workers.

So it's hard to imagine someone who is

not monitored and therefore works only in that occasional thorium repackaging that would exceed or be up there with the upper end of the individuals who have both the thorium plus the uranium monitoring. You almost have to say the person with unusually high intake -- the likelihood is that most of the others were uranium workers who were in there and they're part of the distribution.

So it's hard to think of someone being in the upper tail of distribution as an occasional worker who only got that exposure compared to the distribution itself. So I think the 50th percentile is very, very generous, actually.

CHAIR CLAWSON: This is Brad. Stu, help me with something. And by the way, you guys have been able to give me a headache this early in the morning thinking about this.

So, if you are a uranium worker, according to the process here, if you are a uranium worker, just a uranium worker, are they

going to get this 50 percentile for the thorium?

MR. HINNEFELD: Well, no one --

CHAIR CLAWSON: Because we're talking a coworker model here. I mean, we only would use a coworker model if we didn't have data. Correct?

MR. HINNEFELD: Right. I'm trying to answer your question. No one is clearly identified as just a thorium or just a uranium worker until you get to, say, '88 or '89, when they started qualifying people to work on the thorium packaging jobs. So, at that point, there are people who you could say are only uranium workers if they haven't been qualified to work on the thorium job. So, up until then, people were just radiation workers.

CHAIR CLAWSON: Okay. Now, as a radiation worker, if you have data -- what I'm trying to figure out is what this coworker model, the people that would get this 50th percentile for this thorium, of who they would be, if they are going to be the whole radiation worker group or if it is people that were just not monitored.

MR. HINNEFELD: It would be people who were not monitored but likely could have been in the process area.

CHAIR CLAWSON: Okay.

MR. HINNEFELD: You know, you can't exclude them from the process area, and for some reason we don't have an in vivo monitoring for them.

CHAIR CLAWSON: Okay. Anyway, so here's what I'm looking at. We have got all these radiation workers out there that we've got different work for, and they are not going to get any of this thorium because we have data for that.

MR. HINNEFELD: If they have in vivo counting, we would use their in vivo data.

CHAIR CLAWSON: Okay, but if they did not have any in vivo, they would get the thorium, this 50 percent?

MR. HINNEFELD: They would get this 50 percent coworker, yes.

CHAIR CLAWSON: Okay. You know, basically, then, Paul and Phil, this is one of

these that we're not -- I understand both sides of what they are saying. And I understand what SC&A is saying as this one person out here, but I do have to agree, on this one, on NIOSH's side, that during this time period, from everything that I've pulled out this, the thorium work was very low.

I guess what I would put out to the other Board Members -- and Bob, you have done a marvelous job of representing this and stuff, but I see this as being very generous, to be honest with you. And I think that it would incorporate anybody into it.

If I'm wrong, let me know. I've been wrong before. But myself, I think, by using this 50 percentile, I think we would cover everybody in there until we get to the later years where they were actually designating people as thorium workers, and then they would be going to the 95 percentile.

So I guess I'm agreeing with NIOSH on this and I'd like you two to weigh in on it.

MEMBER ZIEMER: Well, I just indicated I thought the 50th percentile is generous, considering the basis for this and the unlikelihood that someone would be up there with the regular uranium and thorium workers who are at the upper end of the distribution.

CHAIR CLAWSON: Phil?

MEMBER SCHOFIELD: No, I agree. It's generous and it pretty well is going to cover everybody. I mean, there is that chance you've got an outlier somewhere that it doesn't reach out for, but that's something that there just isn't any data showing that.

CHAIR CLAWSON: And ultimately it comes down to us. So I would put out to the Work Group that we accept NIOSH's position on this.

MEMBER SCHOFIELD: I second that.

MEMBER ZIEMER: Yeah, and Stu, it's important that this application be in line with the other applications for which we use the 50th percentile. And it seems to me that it is. You're talking about a worker who is not

regularly doing radiation work and someone who's doing something that's almost a short-term, casual, not very different from someone who walks in is what it sounds like.

MR. HINNEFELD: In my view, from the start, is that people who are occasionally exposed, in a normal coworker setting, people who are occasionally exposed, get the 50th percentile of the full distribution.

MEMBER ZIEMER: Yeah, I'm saying that's the way you do it at other sites. So, there is some consistency with that.

MR. HINNEFELD: Yeah, right. That's my view.

MEMBER ZIEMER: Which means there could also be the extreme example, such as John described, that you would miss, but it's highly, highly unlikely.

MR. HINNEFELD: There could be examples, I guess, where this would miss something in some fashion, but, again, I could never identify it. I don't think we could ever

identify what that would be. And I don't think that -- and like I said earlier, these Fernald dose reconstructions are going to be really quite claimant-favorable. And we're dealing with the edges, the crumbs here, of a really favorable dose reconstruction. I think we are easily bounding everyone's dose with this approach.

MR. STIVER: I'd have to say that, as a corollary to that, in this particular situation you can also pretty well be fairly confident that there wasn't a lot of exposure potential during this sort of kind of quiescent period from '79 to '87 or so.

Before that, we had a situation where we knew there was exposure potential that could be fairly high and we didn't have any way to get a handle on who those workers were, which is the basis of the two SEC periods.

Here, I think this is a pretty good understanding that production work had pretty much slowed down to a crawl. It was basically stewardship until the repacking and so forth. So

I guess that seems reasonable, what you guys are proposing.

CHAIR CLAWSON: Well, I'd say this one is closed.

MR. KATZ: Right. Brad, can I -- at this point, it is 12:30, which is fine. I just want to take a poll if your preference is that you guys need a break or do you want to keep plowing on?

CHAIR CLAWSON: I guess that I'm just still waking up. So, I don't know what --

MR. KATZ: Does anyone else need any kind of break, a comfort break, or is everybody fine?

MR. HINNEFELD: Ted, I could use at least a comfort break. If we're not doing a break for lunch, I could use a comfort break.

MR. KATZ: Yeah, why don't we just take a ten-minute comfort break. You don't need to cut off your line or anything.

CHAIR CLAWSON: Okay, sounds good.

MR. KATZ: And it's about 12:30. So

at about 12:40, let's agree to be back online.

CHAIR CLAWSON: Okay, sounds good.

MR. KATZ: Okay, thanks.

(Whereupon, the above-entitled matter went off the record at 12:27 p.m. and resumed at 12:40 p.m.)

MR. KATZ: We're back at 12:40. And let me just check and make sure we have Phil. Are you back on the line, too?

MEMBER SCHOFIELD: I'm back on the line.

MR. KATZ: Oh, great. And then do we have you, Stu, back on?

MR. HINNEFELD: Yeah, I'm here.

MR. KATZ: Okay, it looks like we can get going here.

MR. BARTON: Okay, so what we had just finished up was Finding 2. We had already discussed Findings 1 and 3, and all three of those were related to sort of an earlier period, from '79 to '89.

The second period for which we had

findings related to coworker doses is the 1990 through 1994 period, in which it is proposed to use ten percent of the derived air concentration.

And we had two findings related to that period. The first was the chosen derived air concentration. NIOSH has chosen to go with, I believe it's the Type W, or weak or medium solubility type, which is more restrictive in the sense that it would hold -- adhering to that derived air concentration would hold air concentrations lower than if you picked the solubility class of Y, or years, or Type S.

And the rationale that was given was that it is standard industry practice to use the more restrictive derived air concentrations when doing monitoring activities in any given location. And I guess we don't have, as I sit here we don't have any technical argument against that value, but I guess we usually don't see standard industry practice as the only justification.

So we were asking if there was

actually site-specific information, whether that would be like a safety or planning procedure, that does say that the more restrictive Class W DAC would be used for monitoring in the planning purposes of these activities, to really justify use of that. Because if we use the less restrictive DAC, of course that's going to result in an increase in assigned intake values to the claimants. So, that was Finding 4.

And so I guess I'd pose it to NIOSH. Is there direct site evidence specific to Fernald that can let us say that, yeah, this is absolutely what was used at the site, and, therefore, it's the most appropriate scientifically to use in this context. Or if, absent such information, should we entertain possibly using the less restrictive DAC because we don't know which solubility class or which DAC they were actually using to monitor and limit exposure potentials?

MR. HINNEFELD: Yeah, this is Stu. I'm not exactly sure where to go on this one. Certainly, I would think that if you were

designing, you know, drawing up your controls and your air monitoring boundaries for a project and you had a mixture of solubilities, you would use the lower solubility class as your posting level. And I think we may have looked for site-specific information about that and didn't find it.

Somebody from ORAU, can you help me out on that one?

MS. KENT: Yes, Stu. This is Karen. We did look in the SRDB for references and the site reference, the internal dosimetry document at Fernald, in 2001, it does reference both Class W and Class Y material for thorium at the site.

So we based the ten percent of a DAC using absorption Class W because it was the most restrictive limit and it was used to limit the exposure in the workplace. But then to determine the dose, we will use the most claimant-favorable solubility. This provides the claimant-favorable approach. So both Class W and Y were assessed as solubility M and S now was present at Fernald.

MR. HINNEFELD: So but I think the

finding, though, would be the magnitude of the intake. So, certainly, once we do an intake, once we have an intake value, as a routine matter, we use the most soluble, or the most favorable solubility class in the dose reconstruction.

MS. KENT: Correct.

MR. HINNEFELD: The point of the SC&A finding would be that if in fact the site were controlling at the higher DAC value, at the Class Y DAC value, then you would have a larger intake.

That's the nature of the finding, right, Bob?

MR. BARTON: That's correct.

MR. HINNEFELD: So, I was surprised. I remember we looked for site-specific documentation about each class of what DAC was used for the posting level and we weren't able to come up with anything that said specifically "this is what we'll use."

Now, somebody refresh my memory. From '90 to '94, they weren't doing the large thorium overpack in Building 65 yet, right, during those

years? Wasn't that later?

MS. KENT: Yes, I believe that was later.

MR. HINNEFELD: Yeah. So, here, we would only have the thorium DAC. I don't know. Again, we're dealing with a favorable dose reconstruction and we are kind of at the crumbs of it. I'd hate to make a lot of changes anymore at this stage, but I don't know that this would be one to go to mat over.

I hate to really change position here on the phone but I will say that we'll reconsider and either provide additional evidence to support it or maybe propose a different -- and we'll provide a written product back to the Work Group. Otherwise, you know, there's just not a lot more to say, I don't think.

MR. SHARFI: Stu, wouldn't 5480.11 require most restrictive DAC for posting? And that would have been in place at this time.

MR. HINNEFELD: Well, like I'm saying, I'm pretty sure, as a health physicist working

there, I'm pretty sure that's what you would do. When you have either an unknown solubility or a mixture of solubilities, you would establish your posting at the lower level. I'm trying to figure out what 5480.11 requirement would exactly apply to that.

MR. SHARFI: Well, I know for Grand Junction, which we do implement 5480.11, I think that's what they did there, to use most restrictive solubility.

MR. KATZ: Who's that speaking?

MR. HINNEFELD: That's Mutty.

MR. SHARFI: Mutty Sharfi.

MR. KATZ: Oh, okay, thank you, Mutty.

MR. HINNEFELD: You know, we can -- I would like to have a consultation on our side, rather than actually take a position here today, and see what we can come up with as either other precedent or other things we can come up with besides standard issue practice to support this. Because, you know, my opinion is that, naturally, that's what we would do. If you're handling

either an unknown solubility or a mixture of solubilities, you would put up your posting boundaries based on the most restrictive of the available of the possible DACs.

But in terms of having something specifically that the site wrote, I don't know that we have anything. So we may want to try to think about it and see what we can come up with in terms of additional arguments besides standard industry practice.

CHAIR CLAWSON: Okay, so I guess this one will -- 2.6, is that where we're at, Bob?

MR. BARTON: Yeah, this is in Section 2.6.2, Reconstruction of Unmonitored Thorium Doses (1990-1994), and that was Finding 4.

So it sounds like we'll leave that one in-progress to allow NIOSH to formulate a more concrete position on whether the current DAC is appropriate or possibly the less restrictive DAC is appropriate. So it looks like -- I'm not sure what else we can do now.

CHAIR CLAWSON: Okay.

MR. BARTON: So I guess we'll leave that one in-progress for now.

CHAIR CLAWSON: That sounds good.

MR. BARTON: Okay. Moving on to Finding 5. Again, this is for that same 1990-1994 period. And, again, it is who gets the assignment of the unmonitored dose.

Now, the original White Paper on thorium coworker doses had indicated that the fecal baseline sample was sort of a prerequisite to assign unmonitored dose. Now, that language has since been removed since that material has been incorporated into the TBD.

So, I guess this is really more for clarification. We assume that, since that language was removed, that the ten percent derived air concentrations will be applied in a similar manner to the previous period in which, unless specific information exists that the person simply couldn't have been exposed, such as a secretary or other administrative personnel, that the ten percent DAC will be applied as an

unmonitored dose.

Again, unless you have that information that says, no, there is just no possible way that the person, due to the nature of their job, could have been exposed, which is how it's dealt with in the earlier period but that specific language wasn't in the section covering the ten percent DAC assignment.

So we're kind of assuming that but I wanted clarification from NIOSH if they intend to essentially assign that fraction of the derived air concentration to the radiological workers and obviously not assign any unmonitored dose if you fell into that not exposed category.

MR. HINNEFELD: Karen, can you help me out here? Karen, can you talk about our approach there, or somebody from NIOSH -- ORAU, I mean?

(Pause.)

MR. HINNEFELD: Is somebody from ORAU speaking on mute?

MS. KENT: Hello, this is Karen.

MR. HINNEFELD: Yes, Karen.

MS. KENT: I'm sorry. I was speaking on mute.

MR. HINNEFELD: Okay.

MS. KENT: But, basically, it's the same criteria as before. So, if someone is in a radiological area or if we cannot exclude them, then they would get assigned that dose.

MR. BARTON: Okay, thank you. And that really satisfied SC&A's concerns with regard to that. So, I guess we would recommend closing that finding.

CHAIR CLAWSON: I understand. Board Members, I move we close it.

MEMBER ZIEMER: Is this just 6 or --

MR. KATZ: That's Finding 5.

MEMBER ZIEMER: Or 5, rather.

MR. KATZ: Yes.

CHAIR CLAWSON: Yes.

MEMBER ZIEMER: Yes, I'm agreed with that.

MEMBER SCHOFIELD: I'm in agreement.

MR. BARTON: Okay, then we can close

that one out.

We have two items that are related to thorium, and that's the thoron exposures and unsupported radium. I'm going to start with the thoron exposures. And, actually, I have a couple of questions, looking at the internal TBD, before we kind of get started about what the findings were. Let me see, what page is that on? One moment, please.

So what I'm going to show you is the main body of the text where it describes assignment of thoron. We'll come down to this table here. And it provides thoron exposures. These are in working level months per year. And we have values beginning all the way starting in 1954 all the way up through 2006.

Now, it wasn't clear to us. We were under the impression that this was sort of enveloped by the thorium SEC. So, I guess the first question is, are we planning to assign thoron doses, which are part of that thorium decay chain, during the SEC period?

MR. HINNEFELD: Yeah, this is Stu. I'll speak to that. The methods that we are proposing for thoron kind of are the same throughout the history. You know, the values are different, based on different quantities and assumptions, but the technique is largely the same.

And so my view was that, well, look, if we have a technique that works, then it should work and it should be included. Despite the fact that thoron is part of the thorium chain, if we can reconstruct the dose, then we should. So that's why it goes back to '54.

MR. BARTON: Okay, I understand that, and you're right. The framework for those values, the 1.6 working level months per year, is pretty much based on the analysis that was done for the post-SEC period, and that was the bounding value based on that analysis.

So, again, it was just a question for clarification. Originally, I had thought we were only going to be assigning these things beginning

in 1979 but if we can assign earlier than that, then great.

MR. HINNEFELD: Yeah, we had that discussion. We actually had that discussion internally and our view was, well, if the technique works, then let's use it for the entirety.

MR. BARTON: Okay. And also I don't know who has Live Meeting up in front of them, but, again, we're looking at Table 5-1 in the TBD.

And the other thing I noticed is that, I mean, this breaks it down by year. And for 1986 to 1987, which is the only -- well, there's no assignment of any thoron dose during those years. It says it was passive storage and there was no significant dose.

That's new to us. That was not in the original White Paper concerning how the thoron doses were developed. And that White Paper got merged into Appendix B. And I will just skip ahead to that appendix. We see, in this table in

the main body of the report, no thoron dose is being assigned for '86 and '87.

And then if we go to sort of the underlying -- right here, these two last rows here, you said 1.6 and it is actually 1.5.5. But, as you can see, that was posited to be assigned all the way up to 1989.

So we have this two-year period where it appears it's being proposed that no thoron dose be assigned, which, again, is new compared to the last White Paper, which is actually now an appendix to the TBD. And we didn't see any discussion, necessarily, that justified no exposure for those two years during the 1980s.

MR. HINNEFELD: Well, I'm at a loss to explain that. ORAU, is there someone over there who can explain that?

MS. KENT: This is Karen. I think we're going to have to look into that.

MR. HINNEFELD: Yeah, okay. So that's another thing we owe then back to the Work Group, is either an explanation or something about those

two years.

MR. BARTON: Okay. Alright, so now our original finding related to thoron was sort of a generic finding. We didn't necessarily find anything technically at fault, and what we're doing here with thoron is, it's a model. We were trying to model thoron exposures because we just don't have sufficient direct monitoring data to come up with something that is maybe a little bit better than a model.

And as with any model, it depends on what sort of parameters you really plug into it. So, as we kind of go through these, and NIOSH provided explanations in the Appendix B about a number of these, there are essentially five main parameters that go into the calculations.

And as we sort of go through them, there are two factors that you should keep in mind here. One is, what are really the appropriate parameters that you want to plug into this model to bound thoron exposure? But also, the second factor is, even if you're not picking

the bounding parameter estimate that's available in each case, does sort of the collective nature of all of them together provide an upper bound on the observed measurement?

Which, my reading of NIOSH's response -- which again is contained in Appendix B of the TBD -- is that, well, you know there is some leeway here about what you can select for these various parameters; and we didn't always select the highest one but if you put them all together and compare them to what limited actual measurements we have, which are mostly in the late '80s and 1990s, it appears to provide a bounding approach as far as the working levels that workers could be exposed to.

So I want you to keep those two things in mind. One is, you know, what is the proper parameter to select? But also, collectively, when you put them all together, even if you didn't select the highest parameter in each case -- which, obviously, it's going to give you an extremely high dose that may be even implausible.

When you put them all together, what does that final value look like when you compare it to the limited values we have, measurements that we have, associated with actual thoron inhalation?

So, as I said, there are five main factors. And what we had asked in our finding, which is Finding 6 related to thoron, is that we saw the parameter values that were selected but we didn't really see the rationale for why that value was appropriate. And so what we asked NIOSH to do is to go and look at what values they selected and sort of provide that scientific justification so that we can say, yes, this model that's been created to bound thoron exposure is scientifically defensible.

In our report -- and actually it was part of a presentation we gave in December of 2014 -- we kind of laid out those five parameters. The very first one is what is the actual tonnage of thorium that's going to be assumed to be the source term for the thoron?

So, on page 96 -- and hold on, I'll

skip to that spot. Okay. So this is, again, "available thoron inventory for release" at the very top of the page here. And on that page, it says that long-term DOE storage could range from anywhere from 100 to 450 megatons at any given location. And it says -- well, not on this page but earlier in the document it says that 450 is assumed as the bounding number.

Now, in actuality, when the calculation was done 300 megaton thorium was assumed. And essentially the explanation we were given is that, well, 450 is bounding, it's the upper bound for any one location, but that the 300 better represents the more typical storage quantity.

Now, in NIOSH's response, basically what they say is, well, if we were to assume the 450, which is bounding, this would essentially increase the concentration of thoron by about 50 percent. And what they state is that that increase, if it were to be adopted, is not going to affect the conclusion. And the conclusion is,

from NIOSH -- and please stop me; I don't want to put words in your mouth -- is that the model that has been put forth based on these parameters sufficiently bounds nearly all of the actual working level measurements that we have. Which, again, are limited because they are later in the period, the late '80s and '90s. But that even if you were to make that 50 percent increase to be bounding weight, it would increase by 50 percent, no doubt about it, but even using the assumption of 300 instead of 450, we're still in a place where the thoron exposure is bounding.

Now, at this point, I don't know if we want to talk about each of these parameters one by one, or I can go through all five and then we can have a discussion about the overall effect. I guess I would ask the Work Group how they would like to proceed, or if NIOSH wants to discuss these one by one, we could do it either way.

CHAIR CLAWSON: This is Brad. I would rather go one by one so I can try to keep track of which way we're going, if that's all right.

MR. BARTON: Sure. As I said, I don't know if NIOSH wants to -- or ORAU wants to respond. I can put up their actual response to what we had come up with so that for everyone to see it.

So, here it is, SC&A Comment 1. This is about the thorium inventory, 450 megatons. This says millitons. That should be a big M.

In the intro, it had had actually quoted 2,000, but that was maybe not the amount in any one storage location. So that might not be appropriate. But as NIOSH states in their sort of technical basis for this, for storage, which is really what we're talking about in this latter period, and really the source term that produces the highest thoron exposure based on the modeling here, it could range anywhere from 100 to 450 and 300 was chosen as appropriate.

And here I'm going to scroll and you can see what NIOSH's response is. I don't know if NIOSH or ORAU would like to comment at this point.

MR. HINNEFELD: Well, I don't have much to offer other than what was in the response that's on the screen there. So, I think we're postulating here what might be present.

Now, this is talking about during the production years. Is that true? So we're talking about like '54 to roughly '78?

MR. BARTON: Well, like I said, we were operating under the assumption that we were really looking at that post-thorium period.

MR. HINNEFELD: Oh, okay.

MR. BARTON: And really the limiting working level calculated is for storage locations, basically due to sheer size of the source term and not necessarily the estimates of production, which produce a much lower working level.

MR. HINNEFELD: Yeah, I mean, there would be material storage during the production time as well. I was thinking that our approach developed like sort of building-by-building, inventories by building, volume of buildings,

emanation rates, and then concentration by building. And then we kind of chose a limiting value for specific years depending upon which building. For different time periods, different buildings were sort of limiting.

MR. BARTON: Well, these are, as you can see what's on the screen here, what we are really talking about is these last two.

MR. HINNEFELD: Okay, the last several ones there. Okay.

MR. BARTON: Yeah, the last two here. These are the working levels. Really, we are just talking about this value right here that I'm circling. I don't know that everyone can see that. It's 5.3 working levels. And this gives the assumptions that went into it.

And so here, again, you have the total source term in megatons. You have this 1E to the 7 is approximately the size of, I believe that's Building 65, I believe.

MR. HINNEFELD: It's probably Building 65.

MR. BARTON: Yeah. So it's 300 megaton thorium. You have the building size. You have the RF, which is the difference between how much thoron is able to escape the material. You have an equilibrium factor just like you would have radon, and then you have an occupancy factor.

So it's these last two rows that we're essentially assigning. And as you look at the table, in the main text, that's what's put there. It says 1.6 working level months, which is essentially three months at a working level of .53.

MR. HINNEFELD: Well, I don't have anything more to offer, I guess, other than what we wrote on the quantity right now.

MR. BARTON: Okay. And as we go through these, I don't know if the Work Group wants to ask any questions or have any discussion on individual ones.

Essentially, where we're coming on this is we don't have necessarily technical

arguments against any of the chosen parameters. What we did want to point out is that there's certainly room in there if you wanted to assign higher values.

And the real proof test is when you start to look at what was calculated, which is this .53 working levels, compared to what working level measurements we do have, which are albeit a bit limited. But when you look at what was calculated, and then we can compare and I'll show the table that NIOSH provided of what measurements we do have, you can compare what was calculated versus what was measured and then sort of determine if that value, or the summation of the values that are currently being chosen, are appropriate.

So, like I said, I mean, this is directly from the technical basis. You all said that given storage location it could be anywhere from 100 to 450 and the 300 was chosen. And so it's certainly, obviously ,well within the range. It's a little over the -- well, a little under

the halfway point, I guess -- a little over the halfway point, I'm sorry.

But, again, this is a question of we're building a model here. What's appropriate? Obviously, it's probably not appropriate to pick the maximizing value in every single instance. That's not realistic. I guess it's a question of, again, what is that final value going to look like? How does it compare to what limited data we have? And is that acceptable in the end?

So unless there are questions directly related to the source term -- that is, the amount of thorium that we are talking about -- we could move on to the release fraction, which was the subject of Comment 2.

CHAIR CLAWSON: Go ahead.

MEMBER ZIEMER: Could you clarify, was SC&A comfortable with the 300 value? Or clarify what the issue was there.

MR. BARTON: Well, as we get to the end -- I guess maybe I'm being a little too cryptic. When we get to the end, I think you're

going to see that the bad actor is Building 65. That's where the higher working level measurements were shown to be, in general. And there were a number of measurements taken and I believe it was about 2.2 working levels.

MEMBER ZIEMER: Yeah, Building 65, you had a 267 picocurie per liter value, right?

MR. BARTON: I believe that value was used to calculate a potential release fraction.

MEMBER ZIEMER: Yes.

MR. BARTON: Building 65 itself has 323 -- I believe 323 megatons thorium. So it's a little higher than the 300 that was assumed. And in fact, that actually comes up a little bit later, under Comment 5, where the calculation was actually made. In Comment 5, we were talking about the equilibrium between 228 and 232 and that some measurements were shown that in Building 65 the equilibrium was higher, which is going to give you more thoron.

And they actually calculated, under Comment 5, that using the 323, which is a

measurement of the amount of thorium in Building 65, coupled with that equilibrium factor, you again get that 50 percent increase.

And I guess my comment there, I wonder if it wouldn't be --

MEMBER ZIEMER: On the 300, you were talking about the 300 tons? What was the 300 you were referring to?

MR. BARTON: Well, the 300, if you have this table up in front of you, 300 was what was --

MEMBER ZIEMER: I don't. I don't have it up. I'm looking at the NIOSH Site Profile figures.

MR. BARTON: Right. So we're on -- if you look on page 98, Table B-5. Table B-5, the last two entries there really lay out the assumptions that went into calculating the working levels, which is .53 working levels.

MEMBER ZIEMER: Right. Right, I see that.

MR. BARTON: So if we were going to

look specifically at Building 65, the amounts of material is slightly higher. Not by much, but it is higher. And so I guess, in the end game, I'm wondering if it wouldn't be more appropriate -- because what we're trying to do is develop a range of different parameters and select ones that are appropriate sort of from different areas of the site, different locations. And I'm wondering, since when we get to it you will see that the bad actor is really that Building 65, if it wouldn't make more sense to pick parameters that are really geared towards Building 65, rather than more generic site parameters.

At the end of the day, I'm not saying 300 megatons is inappropriate. It is certainly technically defensible. It could be higher, by NIOSH's own admission. And this becomes, again, a professional judgment. And then looking sort of at the end result of all these assumptions and can we live with that end result, knowing there is some leeway in there that would increase the value.

MEMBER ZIEMER: I guess NIOSH may need to -- there' some difference there, too, right? NIOSH, what is the position on that, then?

MR. HINNEFELD: Well, I think we'll have a better idea of position when we hear Bob's discussion of the parameters in combination.

MEMBER ZIEMER: Yes.

MR. BARTON: And I would point out, just specific to the amount of the source term, that in that same appendix they did say that they were going to use 450, but then in the actual calculation, it uses 300.

MEMBER ZIEMER: Three hundred, yes.

MR. BARTON: Alright, I'll quickly go through these so that we can kind of get to more fruitful discussion.

MEMBER ZIEMER: These are going to have to remain in-progress then, right?

MR. BARTON: Well, the original comment -- the original finding was that we wanted justification for why certain parameters were provided. And I think what we got is an

explanation that says that the parameters are reasonable and NIOSH is -- again, I don't want to put words in anyone's mouth -- is that, in the end, we are left with a working level assignment that is reasonable and bounding for most of the actual measurements that we have that were taken in the late '80s and '90s.

I believe that's what the conclusion, based on this finding, which again was we have all these parameters that are out there but we don't have really a justification why one's better than the other. That was the original finding.

MEMBER ZIEMER: So you're basically looking for the justification. Is that what I'm understanding?

MR. BARTON: Right. And the justification, the explanation, at least from my understanding, is none of the chosen variables are unreasonable or scientifically indefensible and the combination of them results in a working level that, when you compare it to the available

data, bounds a lot of it. Not all of it, but most of what we have. Or a lot of the working level measurements that we do have.

I believe that is NIOSH's position. Is that correct, Stu?

MR. HINNEFELD: I think that's characterized pretty well.

MR. BARTON: So, again, for the amount of material you have a range, 100 to 450. Three hundred was chosen in the actual calculation, though, earlier it said that they were going to assume 450 but that 450 was a maximizing assumption and a more realistic one was 300, which is, obviously, a professional judgment. I don't think it's necessarily based on any sort of specific analytical construct. These are the ranges that we see and that we can assume and we are going to pick one that is relatively in the middle.

For release fractions, which is the next parameter, NIOSH had put a couple of examples together, calculations of what the

release fraction would be; that is, how much thoron is actually coming out of the source term. And they found that the release fraction could be anywhere from ten-to-the-minus-three to ten-to-the-minus-four for temporary open storage or storage with compromised containers. The NIOSH analysis shows the lower of that range of ten-to- the-minus-four.

On page 94, their technical basis actually had a calculation for the release fraction for Building 65 and they came up with three-times-ten-to-the-minus-three. So that, again, is at the higher end of the release fraction. And if you have a larger release fraction, obviously, you, again, have more thoron. In this case, the lower end of the release fraction was chosen.

The next factor in here is the equilibrium factor; that is, the equilibrium of the daughters to the actual thoron. And we had found a reference that cited that that value could be between .02 and .1, but then there was

another reference that pretty unequivocally states that it is .02 and cannot exceed .04.

So, we had a range of .02 to .1, but probably the more defensible reference says it's .02 and can't be larger than .04. In this case, .02 was the number chosen. Though, actually, on page 105, they calculated the working level assuming .04 because that's the higher; it cannot exceed that value according to trusting the reference. And I think that they calculated that just to show it for a reference, but it's not used in developing the final number.

So, again, we have sort of a reference that says it's mostly .02 and can't get above .04 and .02 was chosen.

The occupancy factor is a little bit different. Now, occupancy factor just basically says how long is someone exposed at that working level. And the occupancy factor was, at least in the technical basis, was three months, up until 1989, and then one month after that.

And in the response provided in the

TBD this is not really addressed, on what the technical basis for three months and one month is. So that was still kind of hanging out there. You know, we don't know why three months was chosen. We don't know why three months was chosen for one period and one month for another period.

And then the final factor going in here is the equilibrium between thorium-228 and thorium-232. The higher the equilibrium, again, the more thoron you have.

And we found that, for Building 65, which, again, is sort of the bad factor according to the measurements we have, the equilibrium fraction should be about .95.

Now, in NIOSH's response to that, they said, well, if we use assume the Building 65 equilibrium and also assume the amount of material in Building 65, that increases the working levels by 50 percent. And once again, you know, it would increase 50 percent if we assume that, but it doesn't affect the conclusion of the appendix, which is that the currently

chosen values are still fairly high compared to what measurements we have.

So those are the five main factors that go into this calculation. Now, what does it all mean?

You put all these together and what you get is .53 working levels. Now, how does that compare to what measurements we have? And hold on a moment and I'll pull the table that summarizes it all very nicely in the TBD. There we go.

So, remember, we're talking about working levels and the currently calculated value is .5. It's .53, but around .5. We can see these are all -- these are the number of samples that we have, the location, the time. In some cases, they were able to calculate a 95th percentile.

And I'm just going to scroll down here. This is the one that caught my eye. You have measurements from 1996 of Building 65 over eight days, and you have 191 measurements. And based on those measurements, you are left with a

working level of 2.20. So, approximately a factor of four higher. And, again, this is at the median -- or the mean, geometric mean. 2.20 working level for Building 65, over 191 measurements taken over eight days, and this is compared to the final value that is currently calculated of .53.

And that led me to question whether any thought had been given to, again, try to model Building 65 specifically, if that's truly the bad actor we are talking about here. And what I did was, as I stated before about the equilibrium fraction under Comment 5, NIOSH had said, well, alright, if we assume that Building 65 equilibrium fraction and the Building 65 source term of 323 megatons versus 200 megatons, you get about a 50 percent increase.

And so, you know, your working level goes from .53 to somewhere about .8. So, that is an increase, but we're still far below what is measured here for Building 65 in 1996.

Well, let's think about we're talking

about Building 65, which is a storage location with potentially degrading containers. And as we said under the release fraction, NIOSH had calculated that could be anywhere from ten-to-the minus-three to ten-to-the-minus-four. Ten-to-the-minus-four was chosen but what happens if we had chosen the midpoint of that range, so five-times-ten-to-the-minus-four instead of one?

Well, now you're doubling that working level value from .8 to somewhere in the 1.5 to 1.6 working level range, and now we're starting to get into the same types of numbers that we are seeing in the actual measurements for Building 65 in 1996. And, again, there are 191 measurements, which is the second column here listing. And as you can see, that's a pretty good number compared to the other measurements that we have.

So, at the end here, I don't have any technical objection to any of the numbers that NIOSH has chosen. I wanted to demonstrate that there is certainly some wiggle room, and, in some cases, leads me to believe that higher parameters

could have been chosen. And even just kind of a rough estimate of Building 65, based on those ranges of parameters, you get a number that can be pretty close to what was measured.

And so I wondered if that had been entertained or how NIOSH feels about the parameters they have chosen, and how the Work Group feels about sort of the leeway in each of these parameter estimates that I just described.

MR. HINNEFELD: Bob, this is Stu. Can you scroll back up to the table before this one where we were looking at the various -- it showed various eras and time periods?

MR. BARTON: Okay. Is this the one?

MR. HINNEFELD: Yeah, that table there down toward the bottom, which is what we are really interested in.

MR. BARTON: Right.

MR. HINNEFELD: Okay, the .53 -- okay I don't -- what are the headings on the last two columns?

MR. BARTON: That's working levels and

then working level months.

MR. HINNEFELD: Okay, so working level is the concentration. I see what you're saying. Okay.

Okay, I think it would be worth looking at 65 just as a specific item, and maybe look into some of these items because -- and just to alert everyone, I think that we, on our side, may want to re-look at that ten-to-the-minus-three release fraction. My recollection was that I think there might have been an incorrect assumption in the generation of that number.

The author assumed that the 260-some picocurie per liter number was extrapolated from a daughter product measurement. And there were instruments that measured radon and thoron gas directly. And so that might actually be a spawn measurement, as opposed to something extrapolated from a daughter product measurement.

But I think that your point, Bob, about let's look at 65 specifically might be worthwhile here.

MR. BARTON: I agree with that, Stu. I think anytime you're trying build a model, I mean, as close as you can get to actual specific situations and site parameters, in this case, it seems apparent that Building 65 was, again, your bad actor. So to the extent that we could choose model parameters specific to that I think would benefit the scientific defensibility of the model. And, you know, we may get back to the same place. We might have a higher working level measurement; we might have a lower one.

MR. HINNEFELD: Right. And I think, if I'm not mistaken, 65 really didn't -- it only got full of thorium like from '72 on, was when the thorium was placed in 65. I think that was when Fernald was designated the repository for thorium and that stuff came. I think it got shipped down from Mound around 1972.

MR. BARTON: That might account for the higher equilibrium between thorium-228 and thorium-232.

MR. HINNEFELD: Yeah, I mean, that

stuff had been sitting around, I don't know. Whatever processing they had done to it, it had been done long before, because it had been sitting at Mound long before it got sent to Fernald.

MR. BARTON: And I guess the only thing I would add is, in addition to maybe looking at Building 65, the parameter of the occupancy factor, which again was one month for a certain period of time and three months during another period of time.

MR. HINNEFELD: Yeah.

MR. BARTON: You need something behind that to really kind of justify that as correct. Because once you get to the working level, now it's just a question of what portion of the year are we assuming that the workers were exposed to that working level.

MR. HINNEFELD: Yeah, I think -- I'm surprised it was left out here, but I'm thinking the justification for the really short occupancy in the latest years was, at some point, when we

got into actually remediating the thorium, all these thorium locations were sort of segmented off and you couldn't go in there unless you were on a thorium project and you were wearing a respirator when you went in there. And so I think that occupancy is actually a combination of protection factor and occupancy, but I'm not sure of that.

MR. BARTON: At this time, does the Work Group have any questions? Or it looks like the path forward might be for NIOSH to maybe take a look at Building 65 specific to thorium -- or thoron, rather, and provide justification for the occupancy times.

CHAIR CLAWSON: This is Brad. You brought up some very interesting points here. We've got to give NIOSH a chance to be able to take a look at these and see where we're at and what the best, most claimant-favorable direction is.

MEMBER ZIEMER: I think that's not a bad idea, Brad.

CHAIR CLAWSON: Yeah, so I guess we'll just take and regroup on that and give them an opportunity to have a look at it.

MR. KATZ: Right. So, Bob, this would just show in-progress.

MR. BARTON: Okay. And, you know, I have some notes here that I was kind of going through that talk about the different parameters. It might be helpful for NIOSH, I could probably forward those to them just to show what I'm seeing as far as the ranges and kind of what's said in there and what options are out there.

MR. HINNEFELD: Yeah, Bob, if you have those notes, that would be helpful if you could just send them to me and I'll send them over to the ORAU side.

MR. BARTON: Okay, great. Alright, moving on from thoron, there's only one thing left and that is unsupported radium. And what essentially this is, is when radium was stripped away, there was a potential for exposure to the unsupported radium.

Now, in the original White Paper, actually, the procedure calls for first you take these in vivo results and you evaluate the thorium. So it's the thorium-232 dose using the lead result. And then on top of that, you would take the actinium results and evaluate it as if it was radium-228.

Now, while that's certainly claimant-favorable, it's sort of double-counting because when we do our thorium dose assessment, we're assuming triple separation. So we're assuming that that in vivo result is representative of just thorium, which in the end game results in essentially a factor of five increase in the thorium results. So then taking the same sample and assuming it was all radium-228 would really be double-counting. And we kind of discussed this back in December.

Since then, that part of it has been removed and essentially what happens is, if you have an individual and see that their actinium result is a factor of 1.5 or higher than the lead

result, then the correct conclusion is that the exposure was not actually to thorium but rather to unsupported radium.

So it's sort of a threshold value. We discussed this with Joyce Lipsztein. Unfortunately, she couldn't be here today. She's visiting with her grandchildren. Well, I guess that's fortunate for her but unfortunate for us.

So, unsupported radium is dealt with for the probably rare case in which the in vivo results actually indicate that the exposure was likely to radium, unsupported radium, and not to thorium. And that is discussed on page 53 of the TBD.

And I guess the only comment I would have, and this isn't really a finding, what we had asked in the original finding was for NIOSH to investigate whether unsupported radium was possible and how to do that. And as I just described, again, there is that threshold value, where you have to have a factor of 1.5 between the two in vivo measurements to really conclude

that unsupported radium was there.

And page 54 of the TBD also states that any unsupported radium intake is not part of any coworker model and so would not be assigned, unless you had monitoring records which indicated that.

And I guess I'd just ask NIOSH to talk a little bit about that and why, because we are kind of admitting that an unsupported radium source could exist at Fernald but there's not going to be any unmonitored assignment of it, which may be reasonable but I kind of wanted to hear NIOSH's rationale on that.

MR. HINNEFELD: Well, this is Stu, and I'll give it a shot. Fundamentally, there was no -- the only unsupported radium-228 would be in a raffinate. And so while there may, in fact, be a potential for, say, a raffinate spill, or an individual may get into the raffinate at some point, it was certainly not a routine exposure at all.

If I'm not mistaken, we have looked at

all of the in vivo data and I think there might be one person who meets that criteria of having actinium result more than 1.5 times the lead-212 result. And so it just wasn't an exposure mechanism and there wasn't -- like there was no radium 228 inventory stored anywhere. It was disposed of out in the pits with all the other stuff.

And with thoron having like a, what, 55-second, roughly, half-life, you know you don't get a lot of emanation once you bury that or stick it in the pit under with a bunch of other stuff. So, with respect to the thoron question around radium-228 storage areas, well, there weren't any radium-228 storage areas.

And then with respect to why no coworker for unsupported radium-228, it just wasn't an exposure that could be experienced by hardly anybody.

MR. BARTON: I mean, then there's really no objection from me. I guess I'd just ask that any of my other members at SC&A if they

have any thoughts otherwise. I think that explanation, it really does seem like a very remote possibility. As you said, even in the records we have, that indicator that there could have been an exposure only happened for, I guess, one instance among these.

MR. HINNEFELD: My recollection was there was on in vivo count, one person. He may have been counted more than once, but there was one person who showed evidence of where the actinium-228 was more than 1.5 times the lead-212. I was a little surprised there was anybody, myself.

MR. BARTON: And the original finding was sort of geared towards -- because unsupported radium had been discussed in that White Paper back in 2014, the thorium White Paper, so we brought it up. Well, you know, I guess we need to flesh that out a little bit and how that's going to be dealt. You know, it would be inappropriate, as it was laid out in that White Paper, to sort of double-count both the thorium

exposure and say there was no radium present for the thorium, but then with the same results say oh, well, the rest of it is -- it also represents unsupported radium.

And that language isn't in there. And I think we have no technical objection to what's being done there. And I don't have any objection to the explanation as to why a coworker model is probably not appropriate for this highly unlikely source term.

So if anyone has any questions or wants to comment --

CHAIR CLAWSON: Bob, this is Brad. So, I have just a clarification. So, what NIOSH and SC&A is saying is that for the radium the possibilities are very remote, and also, too, the half-life on it was how many, 30 seconds or --

MR. HINNEFELD: Well, what I was talking about, the radon-220 half-life is about 55 seconds. And so it doesn't emanate out of materials even as well as radon-222 does. You know, and so once you stick it in the waste pits,

it's not really going to be a particular source of radon or thoron emanation.

CHAIR CLAWSON: Okay.

MR. BARTON: And thoron doses would be bounded essentially by these -- well, the topics we just discussed with the model and how we're going to do thoron.

MR. HINNEFELD: Right.

MR. BARTON: So, again, it was in the original White Paper. There were some concerns that adding in the radium body burden -- we're talking about radium-228 here -- would be unrealistic, especially because that same data is being used for thorium. If you have a thoron model, you know some of the parameters might be tweaked. So we just kind of wanted to see that fleshed out a little bit. And we're satisfied with NIOSH's position on that.

MEMBER ZIEMER: Can we close this one?

MR. BARTON: That's what I would recommend, yes.

MR. STIVER: Yeah, this is Stiver. I

agree with you, Bob.

CHAIR CLAWSON: I guess that one is closed, then.

MEMBER ZIEMER: Good.

MR. BARTON: Well, that's it for me. That gets us to the end of our review of the internal TBD. Obviously, there are some action items going forward, but I don't have anything else. It took a little while to get it done but that's sort of it for me, unless anyone has any questions.

MEMBER ZIEMER: Quick question. This is Ziemer. Where did we end up in Finding 2, which we discussed for a long time but I don't recall us ever finalizing that. Where did we end up?

MR. KATZ: We did. Finding 2 we closed.

MEMBER ZIEMER: Did we close it? Okay, that's what I was asking.

MR. BARTON: Yeah, it was decided that the 50th percentile is claimant-favorable and

appropriate.

MEMBER ZIEMER: Okay, got you. Thank you.

Work Group Presentation to ABRWH for August Meeting

MR. STIVER: Okay, this is John. I guess the next thing that we were going to look at that kind of impinges on us in findings was the TBD-4 review that Doug Farver did last year.

Doug, are you on the line now, still?

MR. FARVER: I'm on the line but I'm not on Skype.

MR. STIVER: Okay, you can probably just kind of read on through and people can follow along.

MR. FARVER: Okay. This was a memo that was sent back in May of 2016 upon review of a Revision 3 to the environmental TBD.

And if we just go down to Table 1, in summary, what I did is I looked through the matrix and I found 12 findings that mentioned or referred to the environmental TBD. Of those 12,

four were closed; four also related to the TBD-5, which we just talked about; and three of them were specifically for TBD-4, the environmental TBD.

And if you go down to the bottom of page 4, the first one that was open is Finding 25. And it's "NIOSH modeling of radon dose is not claimant-favorable and does not take actual working conditions into account." And at the December 2014 Work Group meeting, it was decided that they should use the 95th percentile of the modeled dose, as they did in their Report-52.

And so I reviewed the revised TBD and they do now use the 95th percentile instead of the median dose. So we can recommend that you close Finding 25.

CHAIR CLAWSON: Okay, any discussion on that?

MR. STIVER: The only thing I was going to say is that, yeah, this is that radon emanation issue that Hans had spent a great deal of time on. And it was in abeyance, pending the

revision of the TBD, and it has been addressed. The 95th percentile is being used and so we can go ahead and close that out.

CHAIR CLAWSON: Okay, we'll close it. Any -- Phil?

MEMBER SCHOFIELD: I agree with it. Close it out.

MR. FARVER: Okay, the next one is on the top of page 5, Finding 26, which is very similar. It also has to do with radon. And it was decided at the previous meeting that the 95th percentile doses will be used. And this would close out this finding.

As I said before, the TBD has been updated to include the 95th percentile instead of the median doses. So 26 is very much like 25 and we recommend it be closed also.

CHAIR CLAWSON: Anything else?

MR. STIVER: I have nothing to say, other than -- this is Stiver -- we agree it can be closed.

MEMBER ZIEMER: I agree. Agreed.

CHAIR CLAWSON: Close it?

MEMBER SCHOFIELD: I agree with that.

MR. FARVER: Okay, the next one is Finding 27, "the TBD does not consider outdoor diffuse emissions in production areas as a source of external environmental dose."

It was discussed at the September 2014 Work Group meeting and it was agreed that the finding could be closed if NIOSH would add specific language about OTIB-17, dose on the skin.

So they included that in the updated revision in Section 4.1.2. It says, "Exposures to the skin, including localized doses from direct deposition should be reconstructed in accordance with OTIB-17."

So, since it was included, we recommend that we could close this finding.

CHAIR CLAWSON: No objection. Let's close it.

MEMBER ZIEMER: Agreed.

MEMBER SCHOFIELD: Agreed.

MR. FARVER: Okay, those were the three that were specific to OTIB-4. Other ones have been previously closed, like 28, which you see on that page.

If we go up, I will mention that, under the section in Findings 9 and 11, this has to do with the recycled uranium which was discussed in the TBD-5 review, previously. There's little misstatement in here. It says that SC&A verified that the RU constituents contained in the table contained the agreed-upon RU constituent concentrations.

Well, as we talked about earlier today, there was a little difference in agreement. So, this really doesn't impact anything, other than when we get that nailed down with the RU constituents, we just have to make sure they're consistent between the environmental and the internal dosimetry documents. But it didn't change the status for this report.

And that was the same for SEC, the last one, SEC-3 that had to do with the RU

constituents.

CHAIR CLAWSON: So are you saying we're good on this or still needs something?

MR. FARVER: Well, I guess we probably should keep this open since we're keeping the one from the internal dosimetry one open.

CHAIR CLAWSON: Okay.

MR. STIVER: Yeah, this is John. I think you're going to have the same source term, it's just your looking at it as occupational exposure versus an environmental exposure. So those assumptions and values have got to be consistent with one another for each of the two TBDs. Until we reach consensus on 9 and 11, from an occupational internal standpoint, we have to leave these open as well.

MR. KATZ: Well, are they open or are they in abeyance?

MR. STIVER: They're I think open because we thought that we had reached agreement, like I said earlier, back in 2012. And it turns out that NIOSH changed those values based on the

discussion we had today about the dose reconstruction methodology.

MR. KATZ: So they're in-progress?

MR. HINNEFELD: Yeah, they're in-progress, John.

MR. STIVER: Yeah, that's the long way of saying, yeah, they're in-progress.

MR. FARVER: And that's pretty much the gist of the environmental memo.

CHAIR CLAWSON: So, we'll just leave these open until we get that taken care of.

MR. FARVER: Correct. So the only three we would be able to close specifically for the environmental TBD would be 25, 26, and 27, which you closed.

CHAIR CLAWSON: I understand. Thank you, Doug.

MR. STIVER: Okay, there are only two other items that we haven't looked at that are still on the docket, and have been for quite some time. And these are TBD Findings 17 and 19 that relate to extremity dose calculations with

regards to primarily beta exposure.

And I'm looking at the issues matrix, on page 25 of 51, looking at TBD Issue 19. And this is a situation where you have a person that might have extremity dosimetry, either from a film badge or some badge of some sort, on the limbs or wrist. And we tried to modify the reading on that dosimeter to account for geometric changes in relation to the exposure that might be received in one location based on a dosimeter but would be in a slightly different location.

And in their response, NIOSH references DCAS-TIB-13, "Selected Geometric Exposure Scenario Considerations for External Dose Reconstruction at Uranium Facilities."

And this was kind of left dangling because that particular TIB still has an open finding and Procedures Review Subcommittee, and also it doesn't really address beta exposure. It addresses gamma exposure.

And since you were mostly in

concurrence with NIOSH on the photon adjustment factors, there's still left this method of dealing with the beta exposure. What are you getting on the open window, the film badge versus what was the skin of the extremity experiencing?

And so that is still open. Much related to that is Finding 17. And this was discussed, I believe, back in September of 2014, if I'm not mistaken. And John Mauro, at the time, had been looking into this and he wanted to look -- I guess at the time we were doing a TBD review, a part of the TBD review for Idaho National Laboratory. And as part of that study, we were looking at the NOCTS claims for skin cancer and extremity skin cancer. And I believe we discovered 52 claimants.

And John had indicated at the time that we were closer to finalizing that paper and that we'd be able to get back to that. And that was kind of -- I looked at that paper and basically it kind of makes some generalized recommendations, that we feel NIOSH needs to look

at developing a methodology to do this for some of the claims. Some of the doses that typically we thought would be higher than what would be derived and was actually done in a dose reconstruction. But there has been no development of any particular methodology to do that. And it wasn't much longer after that the SEC Evaluation Report for Idaho came along. And everything since then has been related to the SEC and all the Site Profile issues have kind of been tabled.

So, at this point, the only thing I would say is that we should probably keep those in-progress until such time as we can get back to that particular issue.

MR. KATZ: John, this is Ted. These are sounding like Procedures business, not really Fernald business.

MR. STIVER: Well, yeah, it does kind of have its origins in Fernald but it really has broader applicability. So, you know, it may be something that we consider moving over to

Procedures, or at least keep track of it in that environment.

MR. KATZ: Paul, I don't know what you think, but this sounds like it belongs with Procedures, really, to generic issues you are talking about.

MEMBER ZIEMER: Well, certainly, 17, the extremities dose one is a procedure. What was the other one? Was that 19?

MR. STIVER: Yeah, 19 had its origin and exposure to organ -- let's see, basically the geometry of the source relative to the exposed organ and dosimeter and thorium-handling in production. But, you know, it kind of evolved into more of a generic discussion. So I think those two are kind of two sides of the same issue.

MEMBER ZIEMER: Yeah. Yeah, I think it would probably be appropriate to have Procedures look at those, unless there's something very exclusively specific to Fernald that you don't have at other sites. It looks generic to me.

MR. STIVER: Yeah, like I said, it has origin in Fernald but it really is more of a generic issue.

MEMBER ZIEMER: right.

MR. KATZ: It was stimulated by Fernald issue. But so let's, if we may, why don't you, John, just as part of this closing up this process, why don't you just send a brief memo, address it to Wanda, since she's the Chair of Procedures, and identify these two findings. And then that could be taken off there. Because the Procedures Subcommittee dealt a lot with the issue of extremities and all that.

MR. STIVER: Yeah, this is something that we've seen before in different venues.

MR. KATZ: Yeah, and Jim Neton certainly sort of has most of this under his hat.

MR. STIVER: Okay.

MR. KATZ: Yeah, that would be great, and I think you could probably close it out, in terms of it being an issue for this Work Group.

MR. STIVER: Yeah, it would be good to

get those off the docket.

MR. KATZ: Brad, is that okay by you?

CHAIR CLAWSON: Yeah, that's fine. I just -- if we move it over to Procedures, how are we going to take care of this for the Fernald?

MR. KATZ: So, you can. What we've done is we can close it here, since it's a generic matter. And then however that gets resolved in Procedures, like other things that get resolved at Procedures, that would be addressed for dose reconstruction, not just in Fernald, but everywhere else where it might apply.

CHAIR CLAWSON: Okay, that's all I wanted to make sure.

MR. KATZ: Yeah, that's what the follow-up would be.

MR. STIVER: Yeah, also keep in mind that all of this is going to go into the BRS and it is much easier to kind of cross-link things there, than trying to keep track of these huge gigantic issues.

MR. KATZ: That's true. John, copy

Brad and me when you send that email just so --

MR. STIVER: Okay, will do.

MR. KATZ: Yeah, thanks.

CHAIR CLAWSON: I appreciate that.

MR. KATZ: Does that take us to the end?

MR. STIVER: Yeah, that's pretty much the end for SC&A's presentation.

MR. KATZ: So then it's time to talk about the upcoming Board meeting?

MR. STIVER: Yeah, that's pretty much it.

Petitioner Questions/Comments

MR. KATZ: Let me just check. Do we have the Fernald petitioner on the line? We don't expect her, but is she on the line?

(Pause.)

MR. KATZ: Okay, we didn't expect her, so it's not surprising.

But, okay, so to talk about the Board meeting, I'd ask John to address it and start preparing for a presentation, since that Board

meeting is coming pretty soon and this a lot of material that we've covered.

My question to the Work Group is, how do you want to handle things? You've gotten through a lot of the Site Profile review. We're completed with, I believe, unless I have misheard something, the SEC petition review for the last period of years.

I have it on the draft agenda as potentially addressing the SEC and the Site Profile, although they're not completely finished. So my thought was that we could, the Work Group could come up with a recommendation for the Fernald SEC, and it could also provide a pretty substantial update, depending on how much time all this takes, on the Site Profile work. Because, given how much work has been done on the Site Profile, it's pretty good to spend more than one meeting sort of bringing the Board up to snuff anyway.

So the fact that there are some matters still that have to be put to bed, it seems

like an awful lot of the matters have been put to bed and you could be reporting out partially on that and that would get the Board back in Fernald territory, at least.

But what's the Work Group's thinking first about the SEC and then about Site Profile matters?

CHAIR CLAWSON: You're right, we have gone through a lot of it but we need to bring the rest of the Board up to speed up of where we are at on that.

As far as the SEC, I think we're pretty well there, aren't we?

MR. KATZ: I believe we are there.

MR. STIVER: Yeah, this is John. We have, essentially, closed out all of the SEC issues. The only thing that was left hanging was the post-SEC period for thorium, and as you've heard, there really are no SEC issues there.

MR. KATZ: Now, let me just note, Brad, I mean, in past issues, I mean, I think we're all familiar at this point, where there is

major Site Profile matters that are left in black boxes, the Board hasn't really wanted to act on the SEC while those black boxes remain unresolved. But in this case, it seems to me that most of the matters that DCAS has followed up with are relatively minor, peripheral things. And I'm not sure that the Board, necessarily, would have the same concerns here.

CHAIR CLAWSON: I can't speak for the rest of the Board Members but the one that only gives me a little bit of trouble is that Building 65 that we discussed about. But, that being said, that, to me, is more of a Site Profile issue. It's not that it can't be done. It's what is the best way to be able to do it?

MEMBER ZIEMER: That's right, because we have the numbers, the issue is which ones to use.

CHAIR CLAWSON: Well, right, and also, too, if we actually single out Building 65 on this, because that one's kind of a special problem, myself, personally, I don't see a

problem proceeding on with that with the -- oh, my goodness, it's been a long day.

MR. KATZ: The recommendation.

CHAIR CLAWSON: The recommendation, yes, and take care of these other ones. I don't see anything that would really affect that. But that's just my opinion.

MR. STIVER: Yeah, Brad, I would have to second that. I mean, there's no area that we found where dose reconstruction appears to be infeasible.

CHAIR CLAWSON: Right.

MR. STIVER: It's just a matter of picking the right parameters.

MEMBER ZIEMER: So we could have a recommendation on the SEC and then a -- well, I think, probably for the update, you want to just update on the ones that we've recently reviewed, which is the ones today. And that's fairly extensive. And I think we have to recognize that you can't get into complete depth with the Board on the updates, other than to give an overall

description of the issue and what we did with it. But we don't want to spend two or three hours on the updates.

MR. KATZ: For sure. What I was thinking, Paul, I think what would be good for the Board to know is just have a brief summary of the major issues that there were.

MEMBER ZIEMER: Right. Almost a heading of the description, a few sentences on it, and what we did with it.

MR. KATZ: Yeah, and then just an indication of the remaining issues so that they understand that those are basically fairly minor and well in hand.

MEMBER ZIEMER: Right. Plus, on all of these, they can go to the main document. Now, all of these things have been put on the Board what-do-you-call-it, right?

MR. KATZ: Yeah, and all these things are available to the Board Members, but you know there's always a lot of us. You know, this Board meeting, by the way, coming up, there's a lot of

material to cover. So it would be hard for them to do a lot of background reading on this.

CHAIR CLAWSON: I would like to highlight some of it, though. We can all go onto the Board's area and be able to review any of the documents we get in there, but I would like them see what we've gone over, just the highlights of what you said and go from there.

MR. KATZ: John, is that clear enough direction for you?

MR. STIVER: Yes.

MEMBER ZIEMER: I think if you go more than half an hour you're going to lose people.

MR. STIVER: We won't go that far.

MR. KATZ: So we would have, really, in a sense, we'd have two presentations. We would have the SEC 1 and 2.

MEMBER ZIEMER: Right.

MR. KATZ: We would remind them a little bit about that. And then after we're done with all that, we would have this presentation on the --

MEMBER ZIEMER: Right.

MR. STIVER: Yeah, just lead off with the SEC and then go into Site Profiles.

MR. KATZ: Yeah.

MR. STIVER: Alright, sounds good.

MR. KATZ: Okay, well, thank you, everybody. I mean, I think we're -- right, Brad, are we ready to adjourn?

CHAIR CLAWSON: Yes, I have nothing to keep us here. But, John, you will keep me in the loop on this as we process this forward?

MR. STIVER: Oh, absolutely, yeah.

CHAIR CLAWSON: Okay. You always do. I just wanted to make sure.

MR. KATZ: Yeah, we'll have to be looking at a presentation about a week and a half before the Board meeting, at least, to get it posted.

MR. STIVER: I'll be sure to circulate it with the Work Group and NIOSH to make sure that everybody's on the same page.

MR. KATZ: Thank you, John.

MR. HINNEFELD: Thanks, John.

MEMBER ZIEMER: Okay, thank you.

Adjourn

MR. KATZ: Okay, folks on the line, have a good day.

(Whereupon, the above-entitled matter went off the record at 2:04 p.m.)