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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WORK GROUP ON FERNALD

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MONDAY
JULY 1, 2013

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The Work Group convened via teleconference at 9:00 a.m., Bradley P. Clawson, Chairman, presiding.

PRESENT:

BRADLEY P. CLAWSON, Chairman
MARK GRIFFON, Member
JAMES M. MELIUS, Member
PHILLIP SCHOFIELD, Member

ALSO PRESENT:

TED KATZ, Designated Federal Official
MATT ARNO, ORAU Team
SANDRA BALDRIDGE
ROBERT BARTON, SC&A
KATHY BEHLING, SC&A
ELIZABETH BRACKETT, ORAU Team
HARRY CHMELYNSKI, SC&A
STU HINNEFELD, DCAS
KARIN JESSEN, ORAU Team
JOSH KINMAN, DCAS contractor
JOYCE LIPSZTEIN, SC&A
JOHN MAURO, SC&A
L. MICHAEL RAFKY, HHS
MARK ROLFES, DCAS
JOHN STIVER, SC&A

A-G-E-N-D-A

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

9:00 a.m.

MR. KATZ: This is the Advisory Board on Radiation and Worker Health, Fernald Work Group. Good morning, everyone. I think I'll have -- judging by my list, we have enough to get started. Let's get started with roll call. Let's begin with the Chair.

And everyone Agency related, speak to conflict of interest, too, since we're talking about a specific DOE site. Thanks.

(Roll call.)

MR. KATZ: All right, good. That's all that we expect, so I don't think I need to circle back around here. I have to apologize. It's really my fault, but the agenda just came out this morning. It won't get posted quickly. I've sent it to Josh Kinman, though, so I think he should be able to send it to you, Sandra. And the same notice goes to everyone else in the Work Group who's getting it so late. So I apologize for

that, but last week was murder, but I should have gotten this out.

And then there are really, there are no posted new documents, I don't believe, for this, although NIOSH has a brief one-pager that I don't think has been PA cleared to be posted. That's a response to issues.

So, Brad, you may want to just, for Sandra's sake and anyone else who hasn't actually seen the agenda, you may want to just walk them briefly through it before we get rolling with it. It's your agenda, Brad. Brad, are you there?

CHAIRMAN CLAWSON: Well, I've been talking to myself for the last few minutes, so that's not --

MR. KATZ: Okay, very good.

CHAIRMAN CLAWSON: I'd like to welcome everybody here today. Sorry if my mute button was kind of reversed there.

First of all, as Ted says, we've got this agenda that came out a little bit

late. First thing is I guess we're going to introduce background and process from, well, just to introduce the background of the meeting. We'll go from there. SC&A's issue number one, start date for the proposed SEC Class.

First of all, I guess, I think the first most important thing is I'm going to turn it over to John and let him do a little bit of the background work, if that's all right, John.

MR. STIVER: Yes, that's fine, Brad. Thank you. This is John Stiver at SC&A. And the agenda, as you can see, is not terribly different than last week, or not last week, two weeks ago. On the 17th of June, we had our last Work Group meeting in Cincinnati.

There were a few things we needed to follow up on, however. As you recall, we decided that SEC was warranted for subcontractor employees up through the calendar year 1983 based on the fact that

there were exposures that were not represented by the data set that was available for the coworker model. I did not include subcontractors prior to 1986.

And if you remember, we had gone through several examples in comparison using their coworker bioassay data compared to what they would have gotten with the coworker model. In many cases, the intakes, based on the data, were quite a bit higher than they would have gotten in the 95th percentile with the coworker model.

What remains to be decided was the early start date for the SEC. And I had put in a placeholder value of 1953 based mainly on the notion that the buildings were all under construction. Buildings one through nine were under construction from 1951 to 1953 and came on the line in 1953, whereas the Pilot Plant actually came on line and began processing uranium in 1951. And the notion being that construction workers or the subcontractors and

so forth would be in those other buildings and be in a relatively pristine radiation environment compared to what might have been the case in the Pilot Plant.

However, we've been doing a little extra research on this. And, actually, one of the documents that Stu posted, if I can pull that up, it's an air sampling data. This was SRDB 003081, and this is some air sampling smear data from Plant 9 in 1953.

And there's some interesting notes on here. The first one on the very first page, this is samples that were taken on February 24, 1953, and there's a note here that says the construction men were reluctant to enter the plant because of brown dust. Reader requested a check of the premises. So the reason they did this swipe survey was because the construction workers did not want to go into the building because there was so much dust around. And there was also another annotation to the effect that there was black

oxide on the snow outside the plant and they felt probably was the result of chip burning going on at that time.

So it got us thinking that, well, you know, it's in `53, in February of `53. We had these kinds of concerns on the part of the contractors and subcontractors. At that point, they would have been the people building the other plants.

We started to take a look at the earlier reports and see what we could find, and Bob Barton came up yesterday afternoon, actually, with a quote from one of the, this is SRDB 3230 and this is a collection report from the 1952 to 1954 time period. And there's a quote here, and I'll just go ahead and read it to you. "Dr. Quigley has a difficult problem with getting management and supervisors educated in the field of good housekeeping practices, general health and safety supervision, and enforcement procedures, which will ensure proper control

of uranium contamination, with hundreds of contractor and subcontractor personnel running around `loose," in quotes, "in the work areas. However, the sooner National Lead of Ohio management and the top management of the construction contractor know the AEC requirements for health and safety in the plant, the sooner bad practices will be curtailed."

And so this was in August 7th of 1952. And so this got us thinking, well, you know, if you have this, basically, uncontrolled contractors moving around with potential for uranium exposure, then this notion of working in a relatively pristine environment is really not warranted.

And so, because it really -- we don't see any way that you could put a plausible start date anywhere beyond what would have been the SEC start date in the first place, which would have been January 1st of 1951. So we believe that since uranium was

being handled and we have this kind of situation where we've got the subs, you know, basically going wherever they want in the work areas, that that start date is warranted, as opposed to some later date.

And that's basically what I had to say about that, if anybody else wants to comment.

MR. HINNEFELD: This is, this is Stu. I will say that what Bob identified does seem to indicate there was some co-location of contractor and subcontractor people and they weren't delineated. He describes an event in 1952, and I don't know that, you know, I don't think there's any evidence to say definitively that they are, there was this claim separation between contractors and subcontractors.

When I first saw Bob's, you know, the reference that Bob highlighted, it sounded to me like this is a 1952 issue. And I believe there may have been some radiological operations moving into the production plants

in 1952. I'm not sure about that. Maybe they moved in `53. But it does sound like a `52 issue to me, and it might be important to have a time line of when, you know, when did these radiological operations really move into the production building because I think they kind of phased in over a period of time. I just don't remember right now what it is, if I've ever seen it.

On the other hand, getting back to my earlier point, I don't know that there's any particular evidence that there was a clear separation that, you know, for instance, once a building, for lack of a better term, went hot, I don't know that there's any particular evidence to say that construction workers were excluded from that building from that time forward. And so I guess I don't have a particularly strong opinion on the start date.

MR. STIVER: This is John. Actually, I did some research on when the different buildings came into production, and

Plant 6, the machine shop, was actually, materials were being sent in there to be worked on in 1952. The others, as best I can tell, came online sometime in 1953.

So we have this period of time over two years where you've got all this construction going on. As you said, there really is no clear delineation or separation of the subcontractors or construction workers from the NLO employees. And you have a source of exposure, potential, the uranium being handled and processed. In the pilot plants and the specialty machine shop, we have evidence from the SRDBs.

I believe the January 1st, 1951 is probably the most reasonable and claimant-favorable start date.

MR. BARTON: This is Bob Barton of SC&A. I agree with you there, John. I mean, I just, I don't know that we're ever going to have the sort of precision in the available documents to, you know, come up with a more

precise date to kind of start this thing at. So, you know, in my mind, the reference that you quoted earlier actually is kind of useful. I mean, most of the documents are from 1952, but you see that Catalytic Construction, which was the main construction contractor, I guess, at the time, they were intimately involved in a lot of these meetings about health and safety and about, you know, developing additional ventilation for some of these, you know, runs that they were doing.

So it's going to be very difficult and, as Stu said, there's really no evidence to say that they were, you know, subcontractors were excluded from these areas. So, in my mind, it's tough to say, to kind of cut the SEC short if, you know, if there was radioactive material on the site. There really doesn't appear to be any control to say subcontractors were not exposed to that, so, in my mind, it's going to be very difficult to, I guess, bring that suggested SEC start

date any further in the future, besides, you know, January of 1951. I mean, I don't think that documentation is ever going to give us that clear answer as to a different start date.

CHAIRMAN CLAWSON: This is Brad. I agree with you, and I think we ought to err, if we do err, is in the claimant-favorable realm. So with your suggestion, then I'd say 1951 would be the start date for that. Phil or Mark, do you have any questions on this?

MEMBER SCHOFIELD: I totally agree on that one, I mean, just given the lack of other records.

CHAIRMAN CLAWSON: Okay.

MEMBER GRIFFON: Yes, and I agree, Brad. It seems to me, I mean, trying to decide this one year, six months difference, I think, is not going to be worthwhile. So we should be claimant-favorable.

CHAIRMAN CLAWSON: Okay. I appreciate that. Okay. Well, John, with that

taken care of, this is what we'll propose to the Board when we bring it up in July. I haven't seen any meeting place yet. I guess my backyard is open, I hear.

But, anyway, we'll proceed on to item two or, I guess, item three.

MR. STIVER: Yes, this issue has been alive for quite some time. It was, I guess we could say it was dormant for about a three-year period, about November of 2010 until February of 2013.

This relates to the coworker model for thorium-232 in the pre-1968 environment, particularly from 1953 up to 1967, when the model was really based on these Daily Weighted Exposure studies that were conducted by the Health and Safety Laboratory. And we've been through a lot of discussions about the DWEs and the uncertainties associated with them and some of the limitations and so forth. And then Revision 5 of the model, the most recent one, SC&A is in general agreement that this

approach can be used to bound the thorium intakes for the workers during those periods of time.

And I'll just kind of give little recaps. I don't know if Mark has been involved or been listening to some of these meetings. He's been really busy. But the main difference between Revision 3 and Revision 5 is that Revision 3 was kind of predicated on this notion that you could, there was enough information in the workers' files to where they could be placed in certain buildings in certain years.

And so we were kind of tentatively in agreement with their approach and theory but with a caveat that they needed to demonstrate the ability to actually place workers at that level of precision. And Bob Barton did a study last fall, I believe the issue date was November 2012, that demonstrated that, indeed, that was not possible to place workers at that level of

precision.

And so Revision 4 came out. We had problems with Revision 4 because it was a lot of the same issues that we had with the very first model they had. We wrote a big, pretty comprehensive, lengthy report in 2009 highlighting the problems we had with that model.

And so Revision 5, basically, went back to the Revision 3 approach with a couple of differences, one being that, rather than trying to place workers at a particular facility in a particular building in a particular year, what they did was they took the entire set of DWEs for a given year and then take the highest of those DWEs for throughout the entire complex for a given year and then assign that to everybody with a GSD of five.

And in looking through the data, it looked like things were okay from `54 up through about 1964. Remember, in 1964 there's

the DWE study that had the Stokes Furnace operations. This was a period where casting and re-melting was going on, which is one of the dirtier jobs, actually one of the dirtiest jobs in the entire facility. I know there's a good representation of data for all the different types of tasks, and I believe it comes in at about 5 or 6 MAC for that particular, if you scale it back to the earlier time period of 70 dpm per cubic meter for MAC.

And so we were okay with that, but that still left us with this three-year period, `65 through `67. And the problem here is you've got data, you have three different plants that are processing thorium, but you only have DWEs for two of them. And that was Plant 1 and then Plant 8 in 1966. But you had work going on in Pilot Plant during that period, and there's no DWE data. And so we have a problem. Okay. Well, how are we going to ever possibly put a plausible bound on

these exposures when you're missing data from one of the plants where some of the most thorium-related activities were going on?

And this is, NIOSH was then tasked to go look at data. Mark and Stu indicated there actually were air-sampling data for those years. And, actually, they came out and they posted some spreadsheets that contained breathing zone samples and general air samples for the years 1965 and 1967.

And I asked Bob to look at this data over the weekend, which he did, in usual form. And he put together a little document called "A Preliminary Look at 1964 to 1967 NIOSH Data." So, Bob, if you'd like to tell everybody a little bit about the statistics you did and some of the findings and concerns.

CHAIRMAN CLAWSON: Hey, John, this is Brad. Now, my understanding is on this Live Meeting and so forth, we're supposed to be able to see some of these documents. Have these been posted up? Because I'm not seeing

anything popping up. Are they on there or --

MR. STIVER: You know, I didn't get a Live Meeting notification, so I assumed we weren't --

CHAIRMAN CLAWSON: Oh, well, okay. I guess I'll shut that off then. Okay. Thank you.

MR. STIVER: That information, all those spreadsheets and --

MR. HINNEFELD: This is Stu. I think I can pull up and share what's on, if I can remember how to do this. You're looking at the -- which are you looking at now? The preliminary look at `64 - `67, the sheets, the Word file that Bob posted over the weekend?

MR. STIVER: Yes.

MR. HINNEFELD: Okay. Do the people on Live Meeting see this now?

MR. KATZ: Yes. Yes, it's up, Stu.

MR. HINNEFELD: Okay. It's Table 1, log-normal Fit of NIOSH Data, `64 to `67,

right?

MR. BARTON: Yes, Stu, this is Bob Barton. I think that's what we're trying to look at right now. As John said, we kind of took a preliminary look at the spreadsheets that you had posted and, you know, we fit these data points to a log-normal distribution, you know, calculated the 95th percentile, just to really get an idea of, you know, the kind of magnitudes we're talking about here and how that kind of compares to the other thorium plants during that `64 to `67 period.

So as you can see here, all the way to the right there, we have, essentially, the intake rates that would be derived from these breathing zone samples, as you had laid out in sort of the Word document commentary that NIOSH had sent out. You know, we're only going to consider the breathing zone samples, not the general air, and only those that were specifically delineated as thorium.

So these values are kind of here for comparison. And if we're looking at, you know, `65 and `67, which is really what we're talking about here, at that 95th percentile, the intake rates, as you can see, in `65, it's about 1.5 nanocurie per day. In `67, it's 3.2.

These are actually bounded by Plant 1 in the currently-proposed methodology. And, really, what the whole issue here was, we didn't have this data to really look at last time, so we're kind of asking ourselves the question, you know, could the exposure potential be decidedly different in the Pilot Plant to where, you know, we can't bound the doses from thorium during those years?

What this table really demonstrates, in my mind, is that we're really in the same ballpark. In fact, as I just said, at the 95th percentile, the Plant 1 derived intakes will bound what we're seeing from this data set from the Pilot Plant.

So, in my mind, we have this sort of benchmark of comparison to say, okay, well, you know, we do have these samples in the Pilot Plant, you know, we're only going to look at the breathing zone, so those are going to be, in general, a dirtier sample. And, you know, we pick up the 95th percentile and clear that to the 95th percentile at the other plants, specifically Plant 1. And, you know, we're in the same ballpark here.

So we have that benchmark of comparison now to say that, no, it doesn't look like, to us, that the intake potential in the Pilot Plant was decidedly different than the other thorium operations going on. So, in my mind, that sort of takes care of the SEC angle. And then now we're kind of wading into Site Profile territory, at least that's my opinion on it. I don't know if anyone has any comments on that.

MS. BALDRIDGE: This is Sandra. I have a question. It was mentioned that the

Revision 5 was using the DWEs assigned to everybody from 1954 through 1964 for thorium; is that correct?

MR. BARTON: Sandra, this is Bob Barton. Actually, we're looking at using Daily Weighted Exposures in the air-sampling data to assign thorium doses from 1953 up through 1967, and that would be for everybody.

MS. BALDRIDGE: Okay.

MR. BARTON: That could be potentially exposed.

MS. BALDRIDGE: Okay. Does that include Plant 6?

MR. HINNEFELD: Yes, this is Stu Hinnefeld. Our expectation is that we would assign the highest DWE for a given year to everybody who's potentially exposed because people could move about the plant and, you know, from individual dose reconstruction, we can't necessarily put people in a specific, with reliability that they were here, you know, an entire year. You know, we know their

main assignment may have been some place, but our expectation is that we will assign the highest DWE for a given year to everybody that's potentially exposed in that year.

MS. BALDRIDGE: But my question is were the DWEs available for Plant 6?

MR. HINNEFELD: I believe --

MS. BALDRIDGE: For thorium.

MR. HINNEFELD: I believe there were some years when -- well, for the years, I believe, when the Plant 6 worked on thorium, I believe they are available.

MS. BALDRIDGE: Okay, thank you.

CHAIRMAN CLAWSON: Bob, this is Brad. So what you're telling me is that we don't have an SEC issue for the early `64 to `67 time period then, according to what SC&A is proposing?

MR. BARTON: Yes, Brad, this is Bob Barton. That is certainly my opinion. Like I said, at the last meeting, really the question was we have no way to compare what

the intake potential might have been in the Pilot Plant because we didn't have Daily Weighted Exposures for that year. We only had, you know, we had breathing zone samples and we had general air samples, but they weren't really delineated by the time spent on individual tasks, so we really didn't have that benchmark.

Now, what NIOSH has done is they've gone and compiled what available data there is, breathing zone and air sampling. So we have this sort of basis to actually compare the intake potential. And then, in my mind, it really becomes just a question of, you know, how do we take that data and implement it to make sure that it's claimant-favorable? But I think that the data exists that we can be reasonably sure that we can bound the intake potential in that Pilot Plant. And, obviously, we have some Daily Weighted Exposures for the other plants during that time frame.

So we have this method to reasonably say to ourselves, you know, we don't have some, you know, strange occurrences happening in the Pilot Plant that might not be reflective of the rest of the plant. You know, there wasn't, based on these air sampling data that we see, there's not some crazy exposure potential going on there that was not reflective of the Daily Weighted Exposures that we do have.

So given that we have this method of comparison now, in my mind, we can sort of bound the problem.

MR. STIVER: This is John Stiver. I would say one more thing about that. Bob, I'm just going to expand. You know, the data for the Pilot Plant in `65 and `67 are unweighted air samples. So if everything else was equal, if you were to take a DWE with the same data where you actually had weighting by the different tasks, you would expect it to be a bit lower than what you would get from the

unweighted data. And earlier we're comparing the numbers that Bob has told you, 1.52 nanocuries per day for 1965 for the Pilot Plant compares with 1.9 nanocuries per day for Plant 1, and they're both at the 95th percentile, the DWE being a GSD of five. So that would be the 95th generated from that.

So it's pretty, pretty good weight of evidence argument that the Plant 1 data, which is really more representative of actual worker exposures to begin with, is not only preferable but it's certainly bounding in this case. And that leaves the year of 1966 for the Pilot Plant.

The fact that, based on the thorium time line that's been developed, that there were no big differences in activities taking place in the Pilot Plant between `65 and `67, you would not expect a big spike, the potential for something like that happening. It would be pretty low, unless there was a big accident of some type, which would probably be

documented. So we're pretty confident that -- and we also have the data for Plant 8 in 1966, which is higher than either by a factor of seven, than either Plant 1 or the surrounding data or the adjacent data for Pilot Plant.

So I think all the things combined gives us a pretty good confidence. We're feeling pretty confident that we could move ahead for those years using the data that are available.

CHAIRMAN CLAWSON: Well, I guess maybe this is a question for Stu then is how -- or Mark. You said that people could have possibly been exposed. I guess that brings to my question of how are you going to separate these people out?

MR. HINNEFELD: Well, it's not clear that we will. I think, I mean, that's a Site Profile issue, I think, if there are people to be separated out. It would be, I think, a fairly rare occurrence to say that people are not potentially exposed. I mean,

there could be cooks, for instance, or we might have enough information to say that someone worked in the administration building their entire career.

But if you're talking about security officers or all the maintenance people, of course, there are not very many people that you can have much confidence in excluding.

CHAIRMAN CLAWSON: Okay. Well, I just, I realize this is a Site Profile issue, but I kind of feel like I have an obligation to make sure we know kind of how we're going to be able to do this because it makes it very difficult if this isn't an SEC issue but we can't really put it to the people that need to be able to put it there.

MR. HINNEFELD: I think we can make some assumptions and, you know, I think we can make some decisions and not miss anyone inappropriately. I mean, you have to have some, you have to have a decent level of

evidence to move them out of the exposed group.

CHAIRMAN CLAWSON: Okay. Now, in the -- John, this is probably a question for you. Now, you're saying that this information is going to be able to cover all of the plants and it gives us a good representation of all of the plants where they have thorium going on, correct?

MR. STIVER: Yes. The only, I guess the only issue we had was during that four-year period or, actually, a three-year period where there was no DWE data for the Pilot Plant. And so now we have the air-sampling data, which you can then compare back to the other two buildings where we do have the DWE data. And so that, basically, was the gist of the discussion.

CHAIRMAN CLAWSON: Okay. And we've looked at this sort of validity of -- this information has come into us. We've checked this, where it's came from and this is

correct?

MR. STIVER: Yes, we got the raw data back in 2009.

CHAIRMAN CLAWSON: Okay.

MR. STIVER: And what we did was go back and recreate the DWEs from the actual data and was able to recreate all of them.

MEMBER GRIFFON: Brad, can I ask a question. This is Mark.

CHAIRMAN CLAWSON: Yes, sure.

MEMBER GRIFFON: For John, I'm just curious, I mean I appreciate the idea of comparisons. I've promoted that from the beginning. But I'm wondering would the Pilot Plant compared to all the other plants where you had DWE data, what was the -- or maybe it's a NIOSH question -- but what was the hypothesis? In other words, were you assuming that the Pilot Plant would have been the worst exposures, or were you assuming they'd be similar, or what was the hypothesis going in, I guess?

MR. HINNEFELD: Well, this is Stu, and I'll see what I can respond here. I think the hypothesis going in was that the work, in general, would be controlled across any of the plants in largely the same way. And so you would expect some similarity of exposures, unless there was something unusual going on in production. So I guess that's, that's what our opinion would have been going in.

MEMBER GRIFFON: So differences of scale wouldn't have contributed to any expected differences anyway in exposure you don't think.

MR. HINNEFELD: Well, I mean, if you talk about scale, meaning that a Pilot Plant would have a lower production number than a larger plant, when you're interested in exposure and you're dealing with the highest exposure, you know, if the guy is busy, it doesn't matter if the Pilot Plant makes a fifth of what a production plant makes or a tenth of what the production plant works

because if the workers in the Pilot Plant are engaged regularly with the production, operation, whatever it happens to be, their exposure will be similar probably to somebody engaged in that same activity in one of the production plants.

I think the scale issue might have to do more with how much time did the Pilot Plant spend on a particular operation because, as a Pilot Plant, things tended to start and stop. But without complete information about that, we're just going with the assumption that it was a year-long operation.

MEMBER GRIFFON: And then maybe just a question. I haven't stayed up with all the revisions, I must admit, so I apologize. But can someone just refresh my memory on the actual DWE data itself? I mean, for a given year, for a given building, what kind of sampling period are you talking about? I'm trying to remember, you know, did they sample every month and compile all this data, or how

did they, how was it done for a given year for a given building?

MR. HINNEFELD: I wonder if someone else is more familiar with the sampling regime. I know there were periodic DWE reports written during many of the years of Fernald's operation, and I don't recall right now the sampling regime, if anybody remembers that or not.

The expectation, though, I think the application, you know, we're relying in our approach somewhat on the Davis and Strom paper that talked about when you have these DWEs, if you could apply a GSD of five, you should have a bounding result. And I think a part of that is the recognition that DWEs were not collected constantly, and you're going to have variations, so you place this pretty big GSD on your results in order to have some confidence and bounding.

I'm thinking, I'm trying to remember a good place to look and see those

values that were seen. Is that what you're interested, in what the values were, the DWE values for the various buildings over the years?

MEMBER GRIFFON: I'm wondering if it was, like did they do one data sampling that represented the whole year, or I'm trying to remember. You know, and I can certainly see if the plant was hit with the IH crew one day where work practices might, you know, vary when they see all these IH's show up to do sampling for a day, and you wonder if that's representative. Now, if it was done a lot over the course of a year or -- that's what I'm wondering is how much sampling per task.

One recollection I have is that there was some large variability in the test sampling. In fact, I remember values of like, I forget what the units were but, you know, activity units in the thousands and then the same task getting valued in the tens and then the average being, you know, like, say, 4,000

and zero, the average is 2,000. You know, that doesn't give you a lot of reassurance in the truth for that task.

So I'm just wondering how much sampling, again, how often it was sampled per building per year, and do we have confidence in that? I think SC&A has examined this, but I'm refreshing my memory more than anything.

MR. STIVER: Yes, this is John. I'm trying to refresh my memory. I did this about five years ago. I know what you mean. There are, just looking at some of the data for 1955 right now in Plant 9, and there are large variations. Some of the samples, like, say, there's three samples here. The high is about a factor of three over the low, and there's another 15-minute time period for eight samples and the variation is 125,000 versus the low of 5,000.

And so there is that concern. That was one of the reasons that Davis and Strom did the uncertainty analysis on the six

facilities in the late `40s to early `50s, and I believe their analysis showed a GSD range from about four to six, I believe, if I'm not mistaken, and so recommended a GSD of five for both situations, which would be bounding. And, you know, they also had indicated that, if you didn't have DWEs, you could use the unweighted samples which would typically give you a much higher value.

I could go back into the source data and take a look at the sampling frequency and maybe put together a summary of that, if that's what you would like to see.

MEMBER GRIFFON: I mean, I think that might be good for an explanation to the full Board when we discuss this issue, you know, just to give the whole background of this issue. I don't know if that's possible in a couple of weeks.

MR. STIVER: Well, we didn't look at any every one of the -- I believe we were charged to look at about three or four years,

three different plants, if I'm not mistaken, and all of them. And we did that as sort of to build a weight of evidence argument that the data were, indeed, representative and, you know, sufficiently abundant that we'd have some confidence in applying it across the entire year.

Obviously, even within a given worker, I mean, from one day to the next you can have really big variability. I mean, just look at some of the samples we've seen. So it's definitely a legitimate concern, but we could certainly go through and pull together some summary statistics for the full Board meeting.

MEMBER GRIFFON: Yes, I think that would be -- so, I mean, no one on the call remembers, like, was it one sampling campaign per year per building or was it multiple, or no one can --

MR. STIVER: It was not consistent from one building to the next.

MR. HINNEFELD: Mark, this is Stu. I'm pretty sure it wasn't one sampling campaign a year per building during the years when they were generating the DWEs. And I'm trying to recall now whether they -- they issued, like, these periodic reports. I was thinking it might be quarterly or, in some cases, maybe even monthly, but that sounds pretty frequent. But there were, I think, a series of samplings done over the course of the year in order to characterize a particular plant's, you know, exposure potential that they considered. There was an exposure study or exposure assessment.

MR. ROLFES: Stu, this is Mark, and I'm looking at a spreadsheet titled "DWE Raw Data." It may contain Privacy Act information from back in February of 2009, I believe. And just looking at the first few results in here, the first year that is listed here is 1955. There's earlier data in 1953, but the year column wasn't sorted by year,

apparently, so it's not --

MR. STIVER: Yes, this is John. I'm looking at the same data set, Mark. I think what you have to do is go look at the individual air-sampling reports and see the frequency, you know, the source of all this data that was pulled together.

MR. ROLFES: For example, in 1955, it looks like there's about, I don't know, almost 100 results from Plant 4. Then we've got --

MR. HINNEFELD: Hey, Mark, while we're on Plant 5 or Plant 4 in 1955, what kind of dates go along with those samples?

MR. ROLFES: The dates aren't in these spreadsheets.

MR. HINNEFELD: So they're not in the spreadsheet. Okay.

MR. BARTON: This is Bob Barton. I mean, I'm looking at one of the source Daily Weighted Exposure reports. This is for Plant 9 in 1955, and, actually, in the title it says

"exposure study of Plant 9 personnel to airborne radioactive dust, May 17th to October 31st, 1955." Now, how many times a particular worker at a job site was sampled during that period, I'm not sure that the reports go into that kind of detail. But, clearly, based on that title, the study, you know, was set up for a number of months.

MEMBER GRIFFON: Okay. That's helpful. And a summary of statistics maybe for the Board meeting would be useful, just to give everyone a background on how this was put together. Thank you.

MS. BALDRIDGE: This is Sandra. In the SEC petition, there was a document which was based, which was used in the court case where a gentleman was subpoenaed who was doing the air sampling as far as the procedures that he was instructed to use and the corruption in that procedure. Now, the data, is it being taken as it was reported without regard of the possibility that the

corruption that was suggested in court? Has that been discounted?

You know, when there was a study done checking the data, what the result was that the data had been transferred correctly, but there was no study to determine whether the data itself was correct, especially with the air sampling.

MR. STIVER: Sandra, this is John Stiver. I might be able to clarify this a bit. I believe what you're referring to is the off-site exposure, and there was some question about the emissions rates and the sampling of the stacks and that sort of thing, whether it was done correctly. That was part of the lawsuit that was brought against the facility.

But we're looking at a completely different sampling regiment. The DWE data were collected by the Health and Safety Laboratory and used a very consistent approach over time.

MS. BALDRIDGE: I actually believe --

MR. STIVER: I think it's a different type of sampling that you're talking about here, if I --

MS. BALDRIDGE: I believe there was reference to both types. One was based on zeros that had been entered that apply to the stack emissions, but there was also a subpoena regarding the actual proceeding of going into the plant and how close they had to be to a given area, the direction that they were supposed to turn the instrumentation, and returning with a result. If it was not acceptable, then the fellow was sent back out to re-do it until an acceptable result was received and, thereby, recorded.

So there is a distinction between the in-plant air measurements and the stack. And both those documents are in the petition.

CHAIRMAN CLAWSON: Sandra, this is Brad. This was given in an affidavit and it's

--

MS. BALDRIDGE: Correct.

CHAIRMAN CLAWSON: -- case. Do you remember what years he was talking there? Was it in the later years or --

MS. BALDRIDGE: Let me see if I can pull up those papers. I'll go mute and get back with you.

CHAIRMAN CLAWSON: Thank you.

DR. MAURO: Brad, this is John Mauro. I recall this subject coming up many years ago when I was leading the Fernald effort, and there was considerable discussion of the matter. Unfortunately, I don't recall the full development of it, but there is a record on this subject from previous meetings that go well back.

CHAIRMAN CLAWSON: Yes, if I remember right, John, though, part of our thing was we hadn't solved the SEC issue and that this would kind of become a moot point if the SEC wasn't in. And I'm looking at the ten

years that we already put in, and I was wondering if it was within that time period. I know that we spent a considerable amount of time trying to assure that the data that we did have was representative of what was actually there. But that is right, this always brought a question into the back when we, you know, I believe the term was, well, it's hard to prove a negative and so forth without any more data than that.

But I just want to make sure that we're addressing the issues that were in the SEC, and this was one of the first questions that came up with the data and how could we accept some of this when the person that was taking it actually was the one that was saying it was bogus. And I know what you're saying, John. I'm trying to go through my mind as we went through there, but I think we kind of put that a little bit on the back burner because we wanted to wade in deeper into these issues and be able to -- and this is where the HIS-20

database came in, the transferring bit, and all this. I just want to make sure that we've addressed this, John.

DR. MAURO: You know, I agree. And I only bring it up because I think, if we go back and search the transcripts, we probably will find where that matter has been discussed and the degree to which it remained an area that required further investigation or whether, for some reason, we felt that it was adequately addressed. I do not recall the outcome.

CHAIRMAN CLAWSON: I personally, it was one of these that we could not, we could prove it per the documentation that this person put, but there was questions that, you know, it was just a disgruntled employee or whatever. But still we have this affidavit being used in a legal case questioning this air-sampling data. And I just, you know, we've knocked it around, I know, for a couple of years with you, John, but we decided that

it would proceed forward and we would take the data as it was and see if we could address the issue and problem later on. You know, if something really stood out in question of the data, abnormalities and so forth, if we could, you know, prove that all this -- this was always at Fernald and this was what has made it one of the problems is that there's too much conflicting information and we do have so much information there that it's hard sometimes to understand it all.

But that was my feeling before we kind of landed on it. We were going to proceed with it and try to look at the data and see if we could prove it or disprove it one way or another. But there's always been this lingering question in this affidavit. This was, I believe, part of the SEC that came into this. The person that was actually taking the air-sampling data said that, you know, what I'd have to do is turn my back into the wind and, once I got a reading that was

good, then we were fine.

That brings into question a lot of samples, and this is the problem. I know that, in so many cases, we'd all like to deal with the data, but then we go into the integrity of the data. This one is a hard one because all the data looked pretty good on it. And I know that John Stiver has spent hours and hours trying to prove that, you know, the integrity of the data and so forth.

But I just want to make sure this is, in my opinion, Fernald is coming to an end. I just want to make sure if we walk away from this that, myself, personally, that we've given it the best that we can and we've turned over every rock and everything that we can that, when we walk away from this, that it's been a good job that we've done. So that's the only reason I bring this up.

MR. BARTON: Brad, this is Bob Barton. And you may remember a few years back the Work Group had tasked us with coming up

with strategies to kind of try to get our head around this issue of potential data falsification. That wasn't geared, necessarily, directly at the air-sampling data. We were really kind of looking at the uranium urinalysis data to see if that kind of held up, and we came up with a number of strategies, one of them which was to compare urinalysis results for some of these high-exposed workers in the DWE reports to see if maybe their bioassay results kind of mirrored the fact that they were in these high-dust environments.

And, you know, we did some sort of legwork to kind of figure the feasibility of that and really the feasibility of being able to come to a conclusion on it. And there was some very fruitful Work Group discussion at the time about it, and we kind of all came out on the same page that, you know, any strategy that we tried to adopt has really, you know, put the data to the question to see if it held

up. All strategies were really fraught with peril and you really, there was no assurance you'd ever come out with any sort of reasonable conclusion. I mean, there's just so much variability in urinalysis and body burdens and all these sorts of uncertainty regarding the data set that, you know, like you said, it's just, it's very hard to sort of prove the negative and say, well, here's the smoking gun and, clearly, this data does not hold up.

But we did, in my mind, perform some due diligence with regard to data falsification a couple of years ago. It's just a very difficult thing to ever prove analytically.

MR. STIVER: This is John Stiver. I can weigh in a little bit on that, too. Yes, I remember there was discussions and, basically, determined that, you know, you can't prove a negative. And there are many sources of variation, but one of the things we

started looking at was, well, is there some kind of systematic bias? I mean, do you see some kind of a suppression of results that might correlate with some pre-determined level, like, say, for example, one MAC, whatever the maximum permissible concentration was at the time.

And so I would just draw everybody's attention to Plant 9 in 1955, which was one of the things we're going to talk about here from kind of a more Site Profile issue, and here there are, I believe, the top five DWEs range from about 230 up to 685 MAC. And these also correlate with some health and safety reports that identified the fact that there was just, it was kind of a crash program in 1955 and there was a real problem with dust loading up to like half a gram per cubic meter, and those are the kind of numbers you actually see when you take the MAC data and back-calculate to a dust load, assuming thorium at 2.2 ten to the minus seven

curies per gram. So it's very low specific activity.

And so you come up with a situation where, even if you were to take that high MAC and try to apply it to a group of workers, that corresponds to a dust loading of about 100 milligrams per cubic meter, which is right at the tolerance limit for human physiology.

So that was one of the issues that we were struggling with last week is, well, what do you do? I mean, even in the DWE reports, they say that during the high dust-loading operations people wore respiratory protection. And, you know, if you a apply protection factor of ten or a hundred and so forth to those high-dust short-duration exposures, you wind up with an air concentration that's quite a bit lower than that.

So in my mind, I mean, if there was some kind of systematic falsification

going on, you just wouldn't see that kind, you'd see some kind of a disconnect where you have a report saying that the dust levels were impermissibly high and needed to be changed, yet you come up with these results that were kind of suspiciously low. And here you have a situation where the results are pretty much consistent with the stated contamination levels that were observed.

Once again, I mean, you're never going to prove whether this is, you know, there might have been some falsification, you know. Maybe there was. I don't know how you'd ever tell.

CHAIRMAN CLAWSON: Go ahead, John. I'm sorry.

MR. STIVER: That's really all I had to say about that.

CHAIRMAN CLAWSON: All right. And I understand what you're saying, and I know that we have spent an awful lot of time on this. I just want to -- and, you know, now

that you're jogging my mind and, Bob, I appreciate your comment because I remember that we spent some time digging into this, that we could prove it or not, and we really never just come up with anything clear.

I just want to make sure, you know, that, as we bring this to the Board, that we make sure that we've covered everything we can. And I understand what you're saying, John. We've seen some astronomical samples. But like you say, it's borderline right at the max where people can even breathe in it anymore. And I understand that. It just comes back to one of my things that Fernald was a pretty nasty place.

So I just want to be able to make sure, and Sandra is one of them that has been battling with this. And I want to make sure that we make sure that she understands what we have looked at all of this and that, you know, this is one of them that's been hard because we could not really prove it or disprove it

either. But we have to go off what the data that we had.

MS. BALDRIDGE: This is Sandra. I found the document. On the online petition, it's identified as SEC IS 9362, and it's dash 161 is the way it's numbered. It's the trial affidavit. At the bottom, it is PE 747. The fellow worked at Fernald from September 14th, 1953 until March 28th, 1971. His responsibility was to conduct surveys, do sampling, air dust sampling, toxic gas, ventilation, measuring the dust collectors.

Just some of the things that I have noted was I used a homemade sampler. Paper was not protected on the front. For example, that it was possible to lose some of the dust if you were bumped. You could also lose dust from transferring the filter paper into an envelope. When I did air dust surveys, I could get a higher reading if I stood in the direction that the dust was blowing from the employee that I was sampling.

Conversely, I could get a lower reading if I stood in the opposite direction from the way the dust was blowing.

I recorded depending on how dirty the operation was. I stood on one side, the reading might be zero, while on the other side it might be 50 times higher than the maximum allowable concentration.

To sample, the sample should be taken in the direction that the dust is blowing if the employee is subjected to the dust. When I got air dust survey results and they were above the MAC, I was told by my supervisors that the results were in error and I was told to go back and resample. That's just an example.

CHAIRMAN CLAWSON: Thank you, Sandra. I appreciate that. And, you know, sitting here listening to this, this has almost been seven years, I remember going through this quite a bit because one of the questions that came up was that, you know,

nowadays, when we're doing a lot of this sampling, we've got a rated capacity air flow that really makes it able to be, you know, calibrated and also be able to justify a lot that's going in there. I know, in the earlier years, a lot of this really wasn't done. I remember bringing that up.

So this information has always been in question. But we need to make sure that, as we do bring this before the Board, that they, I guess, understand the uncertainty that is in there because, in my opinion, there is no more data that we can find. There's no more, you know, the data, in my mind, is questionable just from the standpoint of how it was made. But, John, your point is very well taken that a lot of this data was right up there in the maximum that's tolerable for somebody to be able to even work in.

MR. STIVER: You know, Brad, I'd also indicate that most of the DWEs that were used in the study are significantly over one

MAC. If there was some kind of systematic falsification to suppress readings, I don't think you'd see that. But it's just another piece of evidence.

CHAIRMAN CLAWSON: I'm trying to think of how I'm going to present this to the Board to be able to look into this because I know, and, Sandra, you've been involved in most of these, that we've overturned every rock there is that we can on this, as we proceed forward on this. But I just want to make sure that we've also addressed some of the concerns that were in the SEC petition, too. And, you know, there's going to be an uncertainty on this, and I guess when the Board votes we'll have to vote on knowing that, too.

MR. ROLFES: This is Mark Rolfes. I just wanted to point out also in the affidavit that the individual had provided to us, I remember, now that I see the document here in front of me, I remember some of the

discussions that we did have, and this individual had indicated that he was sampling the jolter in Plant 5 where ventilation modification had just been made. And he basically had sampled, got a high result, and was told to resample five or six times by his supervisor. And there's nothing in here that indicates that the other data would have been dismissed or deleted or not recorded.

And also, in addition, I remember this discussion because Plant 5 was not one of the plants that was producing thorium at Fernald. So --

CHAIRMAN CLAWSON: This was more uranium data, more uranium samples, Mark?

MR. ROLFES: I'm sorry. What was that, Brad?

CHAIRMAN CLAWSON: I'm sorry. This is Brad again. This was more the uranium samples?

MR. ROLFES: Correct. This was for Plant 5 where this individual had

indicated that he had to take the sample, which came back high, and then was subsequently directed by his supervisor to resample five or six times. So this occurred in Plant 5, and it's likely, when he's talking about the jolters where they're compacting green salt into one of the reduction bombs prior to putting it into the furnace.

MS. BALDRIDGE: That information isn't in the printed affidavit.

MR. ROLFES: What information --

MS. BALDRIDGE: That would be based on whatever discussion he had with you.

MR. ROLFES: I'm sorry. What --

MS. BALDRIDGE: I said that that information is not in his sworn affidavit.

MR. ROLFES: Okay. I can, I'll read for the record here what number seven says. This is from page 170 of 367 pages that we received as part of Form B. What's your SEC petition --

MS. BALDRIDGE: I see it now.

MR. ROLFES: Okay. I can point out it basically says, "On several occasions during the term of my employment, I got air dust survey results that were above the MAC. I was told by my supervisors that the results were in error, and I was told to go back and resample. I remember one specific occasion when I was sampling the jolter in Plant 5 where ventilation modifications had just been made, and I was sent out there to sample the air. The production plant employee was working over the jolter, and the dust was coming up into his face. I obtained results that were above the MAC. I think that my results were correct the first time that I sampled because they were similar to the results that I had obtained before the modification, and the modifications were not effective. Nevertheless, my supervisors told me to go back and resample. When I resampled, the results were still above the MAC. I was sent back by my supervisors five or six times.

Finally, I stood in the opposite direction from the employee from the way that the dust was blowing, and I obtained results that were below the MAC. When I returned the results that was below the MAC to the health and safety division, it was an acceptable result." That was all.

CHAIRMAN CLAWSON: Okay. I appreciate that, Mark. Mark or Phil, do you have any questions? I'm sure that this is opening up memory lane here because, if I remember, this was also why, a lot of the reason why we were using the abundance of the urinalysis for uranium was part of these questions in here. Is there anything that, are there any questions you have, Mark or Phil?

MEMBER SCHOFIELD: No, I don't. That still is a gray area to me.

CHAIRMAN CLAWSON: I understand.

MEMBER SCHOFIELD: On those samples.

CHAIRMAN CLAWSON: Yes. Well, and I understand and it kind of clears it up, what Mark was saying. This is, what we're discussing today is really pertaining towards the thorium samples. But, you know, it just brings -- there's a lot of questions in there.

Mark, do you have any questions? Mark Griffon?

MEMBER GRIFFON: Nothing that hasn't been said, Brad. I mean, nothing that hasn't been said so far.

CHAIRMAN CLAWSON: Okay. I appreciate that. So I guess -- so SC&A is proposing to the Board that, basically, for thorium, that they feel -- and, John, I'm just paraphrasing this, so correct me if I'm wrong -- that they feel that the DWE data is correct? Is that what I'm getting, John?

MR. STIVER: Aside from any questions regarding the integrity of the data, which is, you know, something that has to be taken up by the Board, you know, what Mark

said definitely indicates that, at least on one occasion and one plant, there were attempts to get the levels down. But, you know, if you take the data at its face value and assume that it's reasonably representative, which we believe it is, and we'll go ahead and get the information on the dates of the studies that were conducted, pull all that together for the plant, but it appears that the strategy is claimant-favorable, not necessarily in the extreme but it's certainly placing a plausible, you know, some workers got these really high values in a given year. And on top of that, you have a very large uncertainty factor, which is going to be factored into the means.

And so given that and given the fact that some of the reports for the situations that there were very dusty conditions that needed to be controlled better correlate well with what we're seeing in the dust studies. To me, that indicates there

certainly was a lot of situations where there was no attempt to try to contain levels below the MAC level definitely. Some are thousands of times higher than the MAC.

So given that weight of evidence, we believe that there are enough data for the thorium production facilities for those years when this activity was taking place and that we can place bounds on the exposures to the workers. There's some issues about timing and what to do about some of the extreme values that we're going to discuss in more of a Site Profile context. But, yes, Brad, I think what you said pretty well sums it up.

MR. BARTON: Yes, John, this is Bob Barton. Just to kind of expand on what you just said, I think it's important not to lose sight of the fact that when we look at these Daily Weighted Exposure values, I mean, we're seeing workers who are in the hundreds of MAC, you know. In my mind, if there was a concerted systemic effort to artificially keep

those numbers low, I mean, I would think they'd be lower than, you know, seeing 200 and 600 MAC.

So to me, I guess, part of the weight of evidence is, you know, the proof is in the pudding. If they were going to artificially try to keep these things low and within the established bounds, then I wouldn't think you'd see workers who are evaluated at the levels of, you know, hundreds of MAC for their Daily Weighted Exposure.

So I think that's important to keep in mind. I mean, this is a very difficult issue, obviously. And as I said earlier, we did make a concerted effort to try to investigate and analytically see what our options were as far as trying to sort of vet this issue out and see if we really did have a problem. And that really kind of ran its course and we just couldn't go any farther and reasonably expect any sort of conclusions on that.

So, I mean, we are dealing with some very high values that, to me, don't indicate that they would be falsified. And I'm not sure there's much we can do, from an analysis standpoint, to take it any further and actually have a conclusion whether, you know, we can trust this data or not.

CHAIRMAN CLAWSON: Bob, this is Brad. I look back at, you know, what we went through on everything there, and I remember that. But, you know, just there's this real gray area there that just kind of sits off, and we've never been able to prove it, nor have we been able to disprove it. And I just want to make sure that other Board Members remember this, as we proceed into it.

The other question, I realize, Stu, that this is going to become a Site Profile issue. I'm just looking at how this is going to be dispersed across the plant and what people are actually going to be covered by this. But I realize that will be a Site

Profile issue. I just wondered.

MR. HINNEFELD: Well, I guess, sitting here, I wouldn't be able to say. And I'm not, it just seems like there are some, a few cases that it's pretty clear from the evidence in the file that the person spent their life in the cafeteria or in the administration --

CHAIRMAN CLAWSON: Well, yes. And I'm not --

MR. HINNEFELD: So I think the idea is to have that option available if the case is clear. And if the case isn't clear, then the person is considered exposed.

CHAIRMAN CLAWSON: Right. And I guess, I guess what I'm trying to say in a roundabout way is I always want to keep in the back of our mind that little gray area, too, when we're choosing people. But this will also become a Site Profile issue because, you know, our contractor has shown us that yes, it's feasible for them to be able to perform

this, you know. It goes out of the SEC realm and goes to the Site Profile.

So is there any more discussion on this from anyone before we proceed on? If not, John, I guess we'll proceed on to the next item.

MR. STIVER: Before we go on, just maybe get this clear, what we are tasked to do between now and the Board meeting is to go through and pull the dust reports and get a better handle on the dates and the period with which it took place.

CHAIRMAN CLAWSON: Well, and what Mark was saying has been one that has kind of weighed on me a lot of times. You know, they can pull an awful lot of samples in one day, and I would just, myself, I would like to be able to see kind of the spectrum of what, how often were these samples pulled to give us a better idea if they were just hit one or two times and taken a lot of data because, like Mark said, health and safety people coming in

and hitting a lot. And I'll be honest. We still see it today. You know, it's interesting that one month we'll have all this stuff and then won't have anything for another 11 months.

So I just want to make sure that, to be able to make sure that we've got a broad enough spectrum of air sampling data to assure that it gives us an overall view of what we're saying and not just one shot every year. Am I correct saying that, Mark? This was your question?

Anyway, that was kind of what I got from his question on that. So does that sound feasible to be able to do, John, or --

MR. STIVER: We have most of the air dust reports. We can certainly -- Bob, this is, I guess, to put you in the hot seat here, is this something you could do in the next week or so?

MR. BARTON: Yes, John, I think that's certainly feasible. And I think it's

an important thing to get together for the full Board to kind of get more information as to what this data really means and what we're looking at here. And, you know, I gave the example earlier where, in 1955, Plant 9, the study took place between May and probably through October. I mean, that's nearly six months, and I imagine that, when we go into the other source documents, we'll probably see similar time frames. But, again, we'd have to pull that together.

MR. STIVER: Yes, we looked at those five years ago. I know we already have them all filed away. It's just a matter of pulling them back out and putting together some summary statistics.

CHAIRMAN CLAWSON: Well, I think it would just be important to be able to put this up before the full Board and help them understand why the direction that we're going and how much available data there really is there. So if you could try to get it cleared

as fast as we could so that the Board would have it, I'd appreciate it.

MR. KATZ: Yes, and this is Ted. John, so if you would, since you're going to be presenting on Fernald on all these SEC elements at the Board meeting, if you could just, I mean, you can just have a slide that addresses this with a reference. And if you can make, it seems like, since this is not going to be, this is going to be aggregate data, it seems like it's easy to get it PA cleared and have a simple document, Bob, that we can also post for anyone from the public to be able to see to go with it.

MR. STIVER: Yes, I think we could have a summary table with dates, number of samples.

MR. KATZ: Right, right.

DR. MAURO: This is John. I just want to offer a perspective. You know, we recently had a Sufficient Accuracy Work Group conference call, and it was interesting

because everyone has been struggling with this concept.

I only bring this up because we are actually having a conversation right now that goes right into the bowels of what is sufficient accuracy and when do we converge and agree, yes, this looks pretty good and when we say, no, this doesn't look so good? This is exactly what we're talking about.

I only bring it up because, keeping that in mind as we move through processes like this, it's going to help the Sufficient Accuracy Work Group come to grips with a very difficult question.

MR. KATZ: Good point, John.

CHAIRMAN CLAWSON: Thank you, John. I appreciate that. And I bet you've probably got a model that would help us, right?

DR. MAURO: Not a chance.

MR. KATZ: Yes. Well, I mean, the sufficient accuracy issue will be on the

agenda for the Board meeting, so it will all be discussed, Fernald and that, in the same two days, which I think will be helpful.

CHAIRMAN CLAWSON: It really will because this is one that we have, we have faced, I'd say, probably every site almost that we've dealt with.

Okay. Is there anything else --

MS. BALDRIDGE: This is Sandra. I just wanted to emphasize that I'm particularly interested in the DWEs for Plant 6 from 1960 through `63 since the thorium processing in Plant 6 was the reason the SEC was filed. So I'm interested and especially since it had been omitted from the Site Profile before the dose reconstructions began, you know. What information had they found? What data did they get that they didn't have to verify the DWEs for Plant 6? That's just my personal concern.

MR. BARTON: I was going to say -- this is Bob Barton. You had asked

specifically about if there was Daily Weighted Exposure data for Plant 6.

MS. BALDRIDGE: Yes.

MR. BARTON: And I can tell you there are reports for Plant 6 from 1959 through 1963; and, in fact, from 1960 to 1963 the Plant 6 values are the limiting case, so that would be the values we would use to apply to everyone at the plant, it would be the Plant 6 highest exposure for those years, so if that kind of helps clarify. I know that doesn't address the potential falsification issue, but we do have those Daily Weighted Exposures for Plant 6; and, in fact, we're going to be or the proposed methodology is going to be using the Plant 6 data for those years.

MS. BALDRIDGE: Thank you.

CHAIRMAN CLAWSON: Yes, thank you, Bob. I appreciate you bringing that in because, Sandra, we wanted you to understand that out of Plant 6 came some of the highest

data that they had and that they are going to be the ones that are used for this whole site. Thank you, Bob.

Okay, John. Any Board Members have any questions before we leave this one? Okay. John, I'll --

MR. KATZ: Well, wait, Brad. I mean, so you have the recommendations of SC&A and you've had your discussion, but do you need a recommendation for the Board for this piece?

CHAIRMAN CLAWSON: You're correct. I was going to try to sum that up at the end there, but I guess we ought to take care of it now. So what we're looking, I guess, John, we've already taken care of this start date issue, so this is on the DWE era from 1965 to 1967 of being an SEC issue, correct?

MR. STIVER: Actually, there was one aspect about the start date on the DWEs that we haven't gotten to yet.

CHAIRMAN CLAWSON: Okay.

MR. STIVER: And there is some thorium data in 1953. The original thorium time line started in 1954, but, as a result of some of this additional research, NIOSH found some thorium air samples, some breathing zone samples for December of 1953. And so the question then became -- this was in the, I believe in the -- let me pull this information out here. Yes, these were a series of samples that were collected in December.

And I guess there's a bit of a question in my mind. NIOSH, if you go to their little position paper here, at the bottom of page one, they're talking about the thorium exposure in 1953. And it's kind of an argument put forth here because other air samples for previous months did not identify them as being thorium, but this one set is identified. And the presumption is made then that there was no more, no thorium handled earlier in that year. And I'm just not really comfortable without some kind of

corroboration, sort of a materials manifest or receipts or something like that that would indicate when, in fact, thorium did arrive and was handled in the plant.

The fact that you just have some samples doesn't preclude the possibility that there were other samples earlier on or there was work being done for which sampling was done or maybe there was not sampling. And so just to take that one set of data and assume it's only for one month, we have a bit of a problem with that.

So I guess we'd be looking for some kind of corroboration, other than just an assumption, because the reports didn't say it was thorium. There wasn't a thorium hazard.

MR. HINNEFELD: Okay, John. This is Stu. There were other machining samplings in October and November, and so what you're saying is, well, they don't really say what they were and so it's not, so you're not comfortable with the conclusion that if it's

not specified as thorium than it's uranium; is that what you're saying?

MR. STIVER: Yes, unless there was some kind of additional evidence that they didn't have thorium on-site until that time. I know, I believe it came from Simonds Saw, if I'm not mistaken. So it might be possible to identify when that material was shipped or received. It could possibly have been being machined for those previous two months, too. So until we know, I think it's kind of premature to cap the exposure period to one month until that's --

MR. HINNEFELD: Okay. So the question here is about the duration of the exposure.

MR. STIVER: Correct.

MR. HINNEFELD: Okay.

MR. BARTON: This is Bob Barton, if I could make a comment here. We had this conversation earlier, John, and I agree. I guess I kind of pose the question do we have

evidence to suggest that thorium was not handled or processed earlier in the year? And, in fact, there is one sort of, I guess it's a letter from early January in 1953, and it said, you know, we surveyed our available facilities and we're not ready to start receiving thorium yet. Again, this is January 1953. But it also says, you know, in eight to ten weeks, when -- let's see here -- when Building 3005 is released, then we can start receiving thorium materials.

Now, when those materials might have actually showed up or when and if they were handled, I don't have any specific references to that. But I guess our concern is that we're kind of just using the fact that we have some air samples that are specifically labeled as thorium in December and we're just going to kind of assume that that reflects the fact that they didn't handle it until December when I'm not sure that's really been corroborated by any of the available

documentation.

And like I said, there is this letter from January that says, well, we can't receive yet but in, you know, two months the facility will be ready to receive it. So I think that we need to kind of do a little more work there to definitely say that it was only handled in December or, if we can't say that, we probably need to expand the exposure period.

MR. HINNEFELD: Okay. So it's a question of exposure period then, how much of the year do you apply that 95th percentile air data to.

MR. STIVER: I think it's kind of a twofold. Are the other data factored into the model and should they be; and then should the period be extended, as well, rather than just using that one set of data that's purely identified as thorium and applying it to an earlier time.

CHAIRMAN CLAWSON: Okay. That was

for what year?

MR. HINNEFELD: 1953, 5-3. Well, with respect to the air-sample sheets that we have, I don't know we're going to have any more definitive statement about it. There is -- I don't know if we're going to find anything more definitive about the nature of these other air samples.

And we, I looked at the, I looked at the monthly reports that I could find from the period; and, unfortunately, we don't have a complete set. And a lot of times the safety and health department reports will describe things that are going on of interest to them. Unfortunately, we don't have a complete set, at least that I didn't find. I found a file that had quite a lot of them, but it was not complete.

MR. STIVER: Well, I would just say -- this is John again -- just from the standpoint of claimant favorability, if there is uncertainty, we are kind of unclear about

what type of material is being machined here, I'd just make a presumption that it's a thorium and go ahead and use the whole data set. I mean --

MR. HINNEFELD: Well, yes.

MR. STIVER: -- significantly higher than the one that you're using, but there's some up around 15 MAC or so.

MR. HINNEFELD: Yes. One thing that comes to mind, and I only know this because we've been looking at it for a week, in another document, I believe it's the DWE study for Plant 9 in 1955, the one where they have the really high numbers, the writer of the DWE report makes some statement about the technology at the time didn't allow this material to be machined with coolants. And so that's contributed to the airborne in a particular job.

And in some of these samples, the July 30th samples from Pilot Plant, some of these, they are machining things with coolant

sprays on the item being machined, it sounds like. So, to me, that is evidence that they were machining uranium above and beyond the fact that there's no specification of what they were sampling for.

You know, I guess it's not definitive. I mean, you're kind of looking at scraps of information. And I think all we're going to see is scraps of information. I think, as you suggested, John, it would be a fairly simple matter to just say, well, let's take all these samples that could be thorium and let's include them in the lot and then expand the exposure time. I don't know that it matters all that much. I mean, it will matter in terms of the dose for that one year.

But the evidence on -- to me, there's evidence that the uranium was kind of an unusual, rare thing that they did, and they noted it when they were sampling for thorium. But on the other hand, that's sort of a conclusionary, that's a conclusion I reached,

and I've certainly discovered, as you go through this process we've been or I've been on for ten years, is things that you would like to be true tend to look true to you. And I don't just mean me, I mean all of us.

So I guess I think we're in the vicinity of this being a Site Profile issue, and I think maybe there's some things -- so I think we can finish up after the Board meeting, but I think you make a valid point that it was a pretty firm conclusion to reach that they only did them in December because that's the only time you have samples from. That's a pretty, that's a pretty gutsy conclusion to reach.

MR. STIVER: Well, yes, I think, given Bob's note that he found, they're possibly going to receive it fairly early in that year. But just because we are uncertain and, you know, err on the side of claimant favorability, there's certainly no indication -- you know, you did mention the cooling

issue, but there's no other indication that that other data set may not be thorium.

It comes down to, like you said, I mean, you tend to find the evidence that you're looking for without necessarily consciously doing that. But I think we're all guilty of it to some extent. It's kind of hard to stay at the objective level we need to be.

MS. JESSEN: Well, this is Karin Jessen from the ORAU team. What was the SRDB number that you provided for that memo in January of `53?

MR. STIVER: Yes, one second. Let me get that number for you. Okay. I'm showing it as SRDB 28884.

MS. JESSEN: Thank you.

CHAIRMAN CLAWSON: This is Brad. So, John, I guess my question to you is I'm understanding that this is a Site Profile issue or --

MR. STIVER: Yes, Brad. I think

that the data that they have is a reasonable data set. It's just a matter of determining the period of exposure. There's a potential for exposures earlier than just the month of December of 1953.

CHAIRMAN CLAWSON: Okay. That could be handled as a Site Profile issue. So this brings me back to the question for the Board Members on here of you have the data of 1965 to 1967 that were in question, correct, John, as an SEC issue for thorium?

MR. STIVER: Yes. Remember, there was the issue of no data for the Pilot Plant, no DWE data for the Pilot Plant. And so that, I think we have resolved that one, as well, based on the fact that the data for Plant 1, the 95th percentile of the available DWEs is higher than the 95th percentile of the unweighted air samples that were provided for the Pilot Plant.

CHAIRMAN CLAWSON: And the recommendation from SC&A to the Board is?

MR. STIVER: Our position is that the doses can be reconstructed with sufficient accuracy. We believe they can for the period 1953 through 1967.

CHAIRMAN CLAWSON: Okay. So I guess this is for the Board Members. I guess I'm asking for a vote to be able to take this to the Board that we accept SC&A's conclusion that it can be reconstructed for these dates. Phil?

MEMBER SCHOFIELD: It sounds like, to me, they've got it covered, so if they feel they can --

CHAIRMAN CLAWSON: That's correct.

MEMBER SCHOFIELD: Okay.

CHAIRMAN CLAWSON: So that's a yes, Phil?

MEMBER SCHOFIELD: That's a yes. Convoluted, but it's a yes.

CHAIRMAN CLAWSON: That's a yes to be able to accept NIOSH's ability to reconstruct dose. Mark Griffon, are you still

with us? So it comes down to me. I've got to go with the information that we've got, and I vote yes on this. We'll proceed to take this to the Board.

MR. KATZ: Okay. But, Brad, if we don't have -- Mark, are you on the line?

CHAIRMAN CLAWSON: Oh, we don't have a quorum?

MR. KATZ: If Mark is not on the line, you basically only have half your Work Group. So what you need to do here is you will not actually formally have a Work Group recommendation.

CHAIRMAN CLAWSON: Okay.

MR. KATZ: But what you'll do is just raise it, just give them the background that there were two Members, you two, present. You concurred with the SC&A finding, but there's not a Work Group recommendation. And then at the Board meeting, the other Work Group Members can, you know, make a motion and so on and carry it forward that way.

CHAIRMAN CLAWSON: Okay. Well, I thought we still had everyone that we could here, so I guess that's why I proceeded on that way. So we'll bring this up at the Board meeting then.

So, John, I guess I'll turn it back to you.

MR. STIVER: Okay. Thank you, Brad. The last thing related to the DWE data was this idea of the infeasibility of assigning the highest MAC in Plant in 1955. Remember, Plant 9 in the year was the site of thorium metal production, a very high-intensity program. There was a lot of problems with air-dust loading. It's indicated in the health and safety reports. And there's also evidence to the high DWEs for that particular year. The top one to top five ranged from 215 up to 686 MAC.

I believe we talked about this a bit last week, but the dust loading associated with 686 MAC is at the physiological tolerance

level for humans. It's about 100 milligrams per cubic meter.

And so we're faced with this conundrum you can't really plausibly assign that kind of a dust load to somebody, certainly not with a high GSD. And that is, after all, a daily weighted average. There is a lot of uncertainty in that value.

The problem we're dealing with here, though, is that the actual reports specify that respiratory protection was worn for what they call the high dust-loading jobs. Of course, now, we don't know what the cutoff is for a high dust-load job, so we could certainly take a look at the data set and see that, yes, there are a couple of different tasks within that sampling procedure that yielded very high transient dust collections. For the highest, the 686, it was a secondary welder helper. There was one, a 75-minute test that was close to, like, 900,000. The highest value was 900,000 dpm per cubic meter.

And so you're faced with this problem then. Well, what do you do? You can't really give them the standard process coworker model of assigning a high GSD to that number, and it's probably not even feasible to give that number itself an upper bound, given the fact that, not because you're trying to constrain intake of radionuclides, per se, but it's just impossible to inhale that much dust. You just couldn't get people to do the job. They would refuse to do it. And if they tried, they could be in a lot of trouble.

And so this idea of, well, what do you do? Are we going to go ahead and consider the possibility of respiratory protection or what NIOSH had proposed originally, which was to generate a set of kind of theoretical air samples based on a log-normal fit of the available data by using repeated samplings and a whole series of air samples and then pick off a 95th percentile of that.

And when they did that, I think

they came in at about 75 to around 100 MAC for that data set. But we thought, well, you know, do we really want to take good data that we know is representative of the most highly-exposed group of workers and just not use it because we think it's too high and instead replace it with a modeled value.

On the other hand, traditionally, NIOSH has not used respiratory protection for claimant favorability purposes. And so we were kind of trying to balance out these two problems here.

And one of the things that NIOSH did, they went back and looked at the highest ten DWEs for 1955, and they applied respiratory protection factors to the highest tasks within that group. And as expected, the values were knocked down considerably. Most of them came down to below, certainly below 50 MAC, just going through the worksheet right now and looking at the numbers.

However, there was one, by virtue

of a very long 386-minute task at about 18,000 dpm per cubic meter, applying a respiratory protection factor does not knock the DWE down very much at all, from 215 or so down to about 200. And so NIOSH proposed to go ahead and use that value was an upper bound.

Now, it wasn't really clear to us whether they intended to use it, they said, if you go to the NIOSH position paper under thorium exposures for 1955, the very last line of that paragraph, the value will be used in the same manner as DWE values for other years. And so if that means applying a larger GSD to a 200 MAC value, we're right back up into that range of implausibility again.

And so what we thought might be another way to look at this is that, because the respiratory factor, protection factor does not really impact this value, just don't even apply it. Just use the value and use it as a constant, a constant exposure. It would be about 30 percent or so of the theoretical

limit.

So it's something that's certainly plausible and certainly bounding. Certainly, there's a bounding value in terms of thorium intake.

And so it's one way to consider providing a plausible, feasible upper bound value. It's claimant-favorable and, yet, still stays within the realm of the physical reality.

So that's what I would have to say regarding Plant 9 in 1955 and certainly welcome any more discussion about this.

MR. HINNEFELD: Well, this is Stu. And I think there's a certain -- yes, I have no particular objection to what John's proposing. I think we left the last meeting with the idea that the respiratory protection approach was something we could try to see what would happen, and it turned out we found the one job that is not particularly sensitive, the DWE value is not particularly

sensitive to whether respiratory protection was used on these short-duration jobs or not because the DWE is driven by a long-duration exposure.

And so we thought that provides us an avenue here to not get into guessing at protection factors because, you know, today we could find out airline respirator protection factors for today, but those protection factors assume a number of things, like quantitative fit testing and training and no particular care of the respiratory equipment that probably wasn't used in 1955. All those things probably weren't done in 1955, so we thought that the protection factor, you know, deciding what protection factor to use was going to be problematic anyway.

Now, John, what you're proposing, as I understand it, is to say, well, we have this 215 MAC DWE that is not sensitive to radiation protection factor, and why don't we use 215 as a bounding value because it seems

to be plausible because we have a DWE at that value. But if you just use that as the bounding, maybe you don't have to get into the choking atmosphere question. You know, I think that's something, as well.

We also, you know, we originally said, well, we could consider that approach for the 686 number, as well, and just use that as a bounding value. But that, by itself, might even be a choking value. I don't remember how that came out, how those numbers came out.

MR. STIVER: Yes, this is John. The 686 would put you right at the upper limit of what's tolerable.

MR. HINNEFELD: Okay.

MR. STIVER: So that has plausibility issues in and of itself.

MR. HINNEFELD: Right. Because you're saying the person would be working there all year long.

MR. STIVER: Yes. In theory,

somebody could be exposed to a choking dust level for an entire year every day for eight and a half hours a day.

MR. HINNEFELD: Okay. Well, I think the 215 number has some merit then as a bounding value.

MR. BARTON: This is Bob Barton. Yes, this is a tough issue. We really kind of wrestled with it two weeks ago. And in my mind, really we're kind of looking at two evils here. And, you know, the first evil is that we don't want to be throwing out very good data and a very useful tool in these Daily Weighted Exposures simply because we felt that a number was too high because that's, you know, sort of a Pandora's box where if the number is too high, well, we'll kind of model something to replace it.

And, on the other hand, you know, NIOSH has always had the policy not to give any credit for respiratory protection. So that has its own sort of slippery slope aspect

to it.

So NIOSH went and they looked at, you know, all right, if we do some calculations here and apply a respiratory protection factor, what are we really looking at for a range of reasonable exposures? And it was very nice work, and, as me and John were looking at it, you know, we see this one worker who his Daily Weighted Exposure of 250 MAC was really driven by that six and a half hours spent in, I believe it was the general chemical area, you know, doing various tasks there. He spent most of his day there, and it was 1800 or 18,000, rather, dpm per meter cubed, which, you know, you can do the calculations and that comes out to, you know, less than half of what we're kind of assuming is the physiological limit of 100 milligrams per meter cubed. You know, as you get above that, I mean, you're just, you're choking. You can't breathe unless you had some sort of protection.

So that avenue, I guess we could call it, to me, almost kind of obviates those two evils because, one, we're not throwing out good data because we think it's too high, and also the underlying data in there is within the physiological limit, so we're not being implausible in using that worker's exposure potential. And also we don't have to get into sort of the Pandora's box of starting to apply respiratory protection factors when it's always been NIOSH's policy never to give credit for that.

So in my mind, I think this provides an avenue where we don't have to get into either issue of, yes, well, this guy would be choking in this job environment because the 215 MAC is well within the reasonable physiological limit. So I think we're still looking at a bounding exposure scenario where we don't have to get into, well, now we're going to apply respiratory protection or get into the slippery slope of

totaling out data and then modeling a different number because we thought it was too high.

So that's, I guess, where I come out on this.

MR. STIVER: Yes, Bob, as you say, it's kind of a tradeoff really. Somebody threw out the question, well, what would happen if you didn't have a guy that spent six hours on one task? You'd still be facing this issue. So it may be kind of fortuitous that we have this particular what area operator down to use as a bounding value. Yet, on the other hand, it is a value that is considered very high, yet it is an actual value by a particular worker on a particular day. It has uncertainty associated with it, but we all know that the only way these values could ever have been achieved is if these people were wearing respiratory protection, given the uncertainties involved.

And so I understand Brad and

NIOSH's standpoint. We don't want to change a policy that's very claimant-favorable and that's been in use for ten years. But, on the other hand, we don't want to take good data and replace it with a model simply because it's uncomfortably high.

And so I think this particular job type is adequately representative of the DWEs encountered by most people or the air concentrations they actually had to breathe during that one year of thorium metal production.

MR. BARTON: Yes, John, I agree. And just to give a little more perspective on that 686 MAC, we're kind of saying, well, 686 MAC really corresponds to the physiological limit of what a person could reasonably inhale without choking. But, I mean, really, if you think about it, that 686 MAC was the daily average, so, I mean, if you look at what's really driven by that one 75-minute job you mentioned and, you know, if you calculate it

out, that 75-minute job involved dust levels that were closer to being 700, you know, milligrams per meter cubed. Now we're getting into like 600 or 700 times the physiologic limit. I mean, so that's really in the land of implausibility there, and that's really what was the genesis of this whole issue.

MR. STIVER: Thanks for bringing that up. And that's also beyond the limit of what's going to be sustained in a cloud of respirable particles. I believe the upper limit is about 500 milligrams, and we'd be looking at applying the 95th percentile to 2 grams, which is just clearly not feasible either from the physics of cloud formation and maintenance or respiratory tolerance.

CHAIRMAN CLAWSON: Well, I remember wrestling with this, and it is kind of a slippery slope either way that we go. But if I remember, John and Bob, that when you guys applied the respiratory to this, didn't it turn out fairly close to what NIOSH's

process came out?

MR. BARTON: Yes, Brad, this is Bob Barton. We were certainly in the same ballpark when we took that 686 MAC and that one 75-minute job and we applied a couple of different protection factors. We were in the same ballpark.

But what we're really talking about now is we don't even need to go the route of the implausible 686 MAC without respiratory protection or the modeled value that NIOSH came up with. What we're talking about now is still using real data for a worker that was in a sustained environment, you know, six and a half hours out of his day, that still resulted in that Daily Weighted Exposure of 215, which is about 100 MAC higher than the modeled numbers we were talking about and certainly higher than that bounding 686 with respiratory protection factored in.

So we're kind of in a spot where we don't have to go into the realm of applying

respiratory protection, which can be, as you said, a very slippery slope, but also we don't have to fill out data and use a modeled value because we can use that worker who was in a reasonable environment. And so we could still use the Daily Weighted Exposure and be reasonably certain that we're going to be bounding the doses to workers in that year.

CHAIRMAN CLAWSON: I understand what you're saying, but this really comes back to Stu and Mark, doesn't it, what you're proposing, correct?

MR. HINNEFELD: Yes, I guess it does, Brad. And I think it makes quite a lot of sense. Like Bob was saying, two avenues that kind of, that have their own evils associated with it. One is using a protection factor when conditions for protection factors are not all being met, and the second is, you know, artificially generating distributions when you have actually measured data in front of you.

So I think it's certainly a high number, but all the information that we saw, the letters that we saw from 1955 in Plant 9 would indicate that high number is warranted.

CHAIRMAN CLAWSON: So what -- I don't want to put words into your mouth, Stu.

MR. HINNEFELD: Oh, the words in my mouth are that I think that this is a good thing to do and that we could alter our proposed approach for 1955 to use, I think it's 215 MAC as a constant for the exposed people in that year.

CHAIRMAN CLAWSON: Okay. Phil, do you have any, any questions on this? Phil? We don't hear you if you do. That being said, so, John, now we've come up this is a Site Profile issue?

MR. STIVER: Yes, this is John. Yes, it was a Site Profile issue to start with really. It's just a matter of how best to use the data that were available or, alternatively, try some modeled numbers. But

it is definitely in the realm of Site Profile issues.

CHAIRMAN CLAWSON: And NIOSH has agreed that they're going to use 215 MAC for this time period?

MR. STIVER: Yes.

CHAIRMAN CLAWSON: Okay. So I guess that concludes that one. So, John, I'll turn it back to you for -- so we've taken every, we've taken care of everything in three. I just want to make sure of this. Because we don't have other Board Members on there, we can't give a Work Group recommendation, but we can bring this up at the Board meeting for the 1965 to `67 DWE, and I'll bring that up to the Board, correct?

MR. STIVER: Brad, this is John. I just want a point of clarification here. I believe the SEC period at issue here is from `53 all the way to `67. I think we already cleared up the issue of --

CHAIRMAN CLAWSON: Right.

MR. STIVER: -- `65 through `67.

CHAIRMAN CLAWSON: It was 1957 to -- no, it was 1965 -- oh, yes, we've already done that. So it was 1953?

MR. STIVER: Yes. Remember, there's the issue of there's some potential for thorium exposures for partial year in `53.

CHAIRMAN CLAWSON: Okay.

MR. STIVER: And then there's also -- which NIOSH is researching. And then there's, traditionally, it was `54 through `67.

MR. KATZ: So, Brad, so all John is saying is that, in the presentation about recommendation about feasibility and the presentation that John would make on the technical material, he would cover the whole period from `53 to `67, even though in the last few meetings we've been focusing on these end years.

CHAIRMAN CLAWSON: All right.

MR. KATZ: Okay.

CHAIRMAN CLAWSON: Okay. That's what was throwing me off.

MR. KATZ: Yes. No, I was just saying. I understand. It's just we've covered the other turf earlier.

CHAIRMAN CLAWSON: Okay. I understand now. I thought -- okay. And then NIOSH on the strategy for 1955 Plant 9, we're going to use the, NIOSH has agreed to use the 215 MAC, correct?

MR. STIVER: Yes.

CHAIRMAN CLAWSON: Okay. And so we'll proceed on to item four, John.

MR. STIVER: Brad, if it's all right with you, could we take about a five-minute break?

CHAIRMAN CLAWSON: Yes, that would be fine. So if everybody is agreeable, we'll take a -- how about a ten-minute break?

MR. KATZ: Yes, that's fine. So we'll get started again at ten past eleven Eastern time.

CHAIRMAN CLAWSON: Okay. Thank you.

(Whereupon, the foregoing matter went off the record at 11:00 a.m. and went back on the record at 11:12 a.m.)

CHAIRMAN CLAWSON: Okay, John. Before we went to break, I believe we had taken care of issues three and were proceeding on to item four.

MR. STIVER: Right. Joyce, are you on yet?

DR. LIPSZTEIN: Yes, I am on.

MR. STIVER: Yes, item four. This is the thorium coworker model for the period 1979 to 1988 where it uses the chest count data, basically the measurements of actinium-228 and lead-212 to back-calculate lung burden or intake rates of Thorium-232 based on some assumptions regarding equilibrium.

And the issue here is that there is some values of actinium-228 that are higher

than expected, given the assumptions that are made in the model. And so there's some question as to whether, what could it really represent, whether it's an intake of unsupported radium-228 possibly, which would give rise to higher values, or possibly it could lead to other issues, like translocation of radon-220 out of the lung; therefore, you might have lead-212 values that are lower than expected.

And so Joyce and Tom were having to do changes of different approaches to working on this. And, Joyce, if you'd like to kind of step in at this point, and I think you could probably discuss it the best of all of us.

DR. LIPSZTEIN: Okay. I'm discussing --

CHAIRMAN CLAWSON: Is this the report that you sent out, did you send a report out on this? The one that I see is activity ratio, and it's got Tom LaBone's name

on it. Is this what we're talking about or --

DR. LIPSZTEIN: Yes, yes, that's the only thing that was sent out was Tom LaBone's paper.

CHAIRMAN CLAWSON: Okay. That's what I want to make sure. Thank you.

MR. STIVER: Brad, there is no SC&A formal response to that paper.

DR. LIPSZTEIN: I was, that's one of the things that I wanted to say is that we really didn't see a complete paper from NIOSH to respond. We just saw one item describing why some studies, we found a lot of actinium results that were higher than the lead results.

SC&A has several points that should be discussed by NIOSH, and we still need some answers. One of the things that we, in our formal answer that was sent in -- let me see the date. It was on November 26th, 2012. We analyzed several results from `79 to `88. And we know that, from NIOSH papers,

that thorium activities at Fernald should have stopped in `79, but we still see some results and some people that were monitored for thorium after `79. And especially we see that there were positive results, especially in 1980 and some in `81. And then the number of positive results for actinium and lead, they go down to just one in 1988.

So we don't know exactly what kind of activities were done in `79 through `88, why people would have positive results if they were really, there was some workers that were exposed to thorium, or if it is reminiscent from thorium previous exposure.

We saw, for example, that, in `98, 85 percent of the results were below the MDA and 15 percent were above the MDA. And of these, we had 13 results that had both actinium and lead above the MDA and 14 results of actinium that were above the MDA, while only actinium results, 14 results, while there was only one lead result that was above the

MDA result.

Now, I've been monitoring thorium workers for a long time, and thorium is not an easy nuclide to deal with, it's very difficult. It's very difficult, and if some of you could look at the models for thorium that's used in `69 and is adopted here, that's the only one that was published.

But ICRP is going to publish occupational intake of radionuclides either in 2013 or the beginning of 2014. And this document is available on the internet if you go to the ICRP site, ICRP.org, and you see the documents for consultation. And even if it's past the consultation times, you can still download the thorium document, and you see that there are several considerations about the problem of in vivo monitoring of thorium.

One of the problems is that the daughter nuclides, they might leave the lung faster than the thorium itself. But the ICRP doesn't give any hint on what to do with this

information. Everybody has, you know, each person, each laboratory, each health physicist has to deal with this problem and know that there's going to be an error when you are monitoring for results.

And, especially, ICRP says radium and lead-12 leaves the lung in a faster way than thorium-228 and thorium-232. So that's one of the reasons that we might be finding some lead results that are below the detection limits, while actinium is above the limit of detection.

So it's, you know, I would like to see something that is sporadical, like actinium can be related to thorium-232 with such and such, you know, we can consider some errors on it. But, especially, we want to see if they really worked with thorium after `79 and who were the people that were working with thorium after `79.

We have found seven workers that had monitoring of thorium in all the years

after `79. They were monitored in `80, `81, `82, etcetera. So those people, maybe they were doing some special work with thorium that we didn't find what was done.

The other people, the majority of the people had results below the level of detection. There were a lot of people that were monitored every year, more than a hundred people every year. So we don't know if they were monitored just as routine or because they were working really with thorium.

And as the whole, you know, the majority of results is below the detection limit anyway, so what does this mean? It means that the majority of the workers didn't have measurable results either of actinium or lead, so they didn't have measured results of lead-212, and, if you put some error into these because of the fact that lead could be leaving or radium could be leaving the lung after exposure, we don't know exactly how many years since they dealt with thorium. So

that's something that we would like resolved.

MR. HINNEFELD: This is Stu Hinnefeld. I can offer something here if --

DR. LIPSZTEIN: Okay, please.

MR. HINNEFELD: -- the monitoring question. This actually extends into the period when I worked there. I was in, I started in the radiation safety department in 1983. And the in vivo monitoring in the `80s was done for uranium, and the in vivo counter just spit out a result for the thorium, the radionuclides, and it was recorded.

But the purpose of the monitoring was because of the potential for uranium exposure because thorium was in storage, but it wasn't being used with a possible exception maybe a redrumming operation on occasion, and, frequently, that just meant putting a deteriorating drum in a larger drum. So there was almost no thorium work going on until sometime after Westinghouse got there and they started making a serious effort to get it out

of there.

But the selection for monitoring was not based on working with thorium. It was based on their potential exposure really to uranium, which was material that was being processed. And then the mobile counter just spit out a sweep of results, and one of those was the actinium result and one was the lead result.

DR. LIPSZTEIN: Oh, okay. And do you know how many people were working with redrumming or --

MR. HINNEFELD: No. It would have been really intermittent and probably short term, and I certainly don't recall. You know, I'm afraid I can't be very helpful. I don't think it would be very many people at one time, and I don't suspect at any particular redrumming operation during that time went on for very long.

DR. LIPSZTEIN: Because I was thinking that that coworker model with so many

results below detection limit, I don't know even how much worthwhile it is to dig into this question of what results to use. From my experience, probably you have the same experience that I do, we always monitor actinium because we have one less problem which is radon and we don't have the problem of lead leaving the lung. We have the problem of radium leaving the lung but not lead also leaving the lung. And we wouldn't have the problem of the number of separations.

And that would account only for I think 1980 and 1981 when you have more positive results. The rest of it, it's going to be mostly below the detection level. I'd, as you say, because they were not exposed to thorium anymore. I don't know.

MR. HINNEFELD: I think that's, in most of their cases, that would have been it. And like I said, you know, I can't say definitively, talk definitively about the redrumming because, A, it was too long ago;

and, B, I might not have been in on it. I may not have known about the operations, and if I did know about them at the time I would have forgotten them by now.

But it just, I just, you know -- but I do clearly remember that there was thorium stored in several places around the plant, and those areas just, for the most part, people didn't spend any time to speak of, except for an occasional inspection now and then.

DR. LIPSZTEIN: And this wouldn't be people that were exposed to separated thorium and nothing like that because we think ended in `79, from what I understood.

MR. HINNEFELD: That's my understanding, as well. I don't remember any thorium processing during my tenure there when they were actually converting, you know, a thorium compound into some other thorium compound. I don't remember any of that when I was there.

CHAIRMAN CLAWSON: Stu, this is Brad. I thought Fernald became the nation's repository for thorium.

MR. HINNEFELD: Yes, that's why it was stored all over the plant.

CHAIRMAN CLAWSON: And you're saying that they didn't, they didn't work with it at all? I guess, and I've got to go to Hanford for this one, but, you know, we had documentation of trainloads coming into Fernald and being, you know, moved and having problems with the train cars because they have to repackage them before they could remove some of these drums. And I --

MR. HINNEFELD: Okay. What era, what era are you talking about there, Brad?

CHAIRMAN CLAWSON: I'm looking at the, I thought this was in the, I thought this was in the `85 time period. I thought this was after Westinghouse came in that they kind of become the repository for this.

MR. HINNEFELD: No, they were the

repository before that.

CHAIRMAN CLAWSON: Before that. Okay. That's -- so what you're saying is that -- I guess my question is here they've got this product there but they're not really monitoring for it.

MR. HINNEFELD: Yes, it was in storage. And like I said, I can't say that no one ever, I believe that these drums did tend to deteriorate. I mean, most of the material was stored in drums, and some of the drums tended to deteriorate. Not all of them. Some of the compounds were well suited for storage in drums, and those drums held up really well. But some of the materials that were there were drummed and stored in drums that tended to deteriorate. And I think there may have been some redrumming campaigns, although the worst of the drums were in `64 and `65, and I think that redrumming didn't occur until after Westinghouse was there and there were quite a lot of controls involved in that large

redrumming. But there may have been some smaller operations during the `80s that I just don't, didn't know about or don't remember.

I can't say for sure that no one was exposed to thorium. But, certainly, it wasn't the big actor the way uranium was in the `80s.

DR. LIPSZTEIN: And if the in vivo monitoring was geared to uranium, were these people that were working in redrumming monitored for a time?

MR. ROLFES: This is Mark Rolfes, and I just wanted to point out I do have an example here from 1985. It's dated March 2nd, 1985. This is from an air sample for thorium spreadsheet that we assembled, and it lists a person that was a truck operator and two other people that were actually doing some of the redrumming operations. And let's see. It says that the operations were conducted to the north of the thorium warehouse. Let's see. And it's got a building code of 65, so

Building 65, which would have been one of the repository buildings. And there's some notes here for the same three people on March 2nd, 1985. We've got a note that says outdoors, wind five to ten miles per hour, 380 gallon cans of thorium being redrummed north of Building 65. And there's similar notes for the next -- these are all BZ air samples and we've got some air concentration data for these individuals.

Let's see. There's approximately 20 entries for this redrumming operation in 1985. But the earlier, the next earlier set of samples that I have in this spreadsheet, and this spreadsheet may not be complete. It was just something that we had compiled, you know, from the available data that we had at the time. The next earlier miscellaneous thorium air sampling was from 1978 it looks like in this spreadsheet. I don't know if that helps you at all.

DR. LIPSZTEIN: This `85

redrumming that you were talking about, do you know if those people were in vivo monitored?

MR. ROLFES: I haven't checked their names against the list of people who were monitored. That might be something of interest. I can certainly provide those names and this data to you, if you would like to --

DR. LIPSZTEIN: Yes.

MR. BARTON: Mark, this is Bob Barton. As I recall, and I do remember the 1985 document, and we did cross-check those names, and I believe they were involved in the in vivo monitoring program. It was, I believe, you know, between six months to maybe, you know, a couple of years after that redrumming that they appear to be monitored in vivo. So they were included but not necessarily specifically monitored because they were involved in that redrumming activity.

So if that helps clear it up. But I do believe they were included in the

database that you guys compiled. It just, you know, it was pretty clear that they weren't monitored immediately after that activity, but they were included in the program as, you know, a highly-exposed worker, which is what the program was really geared to. Not necessarily thorium exposures, as Stu mentioned. But if you had high-exposure potential, then you were generally targeted for the in vivo program.

And I remember one of the conclusions of our completeness study on all this data was that we tried to take a look at, all right, if these guys had a positive sample for lead-212 or actinium, you know, how quickly were they monitored again versus, you know, the rest of the worker population. And I believe, depending on what kind of metric you wanted to look at, you know, average number of days or geometric mean, you know, they were monitored between four and five times faster if they had a positive result.

So, I mean, that was one of the findings sort of that was beneficial to the use of this data to build a coworker model.

MR. STIVER: Yes, Bob. Thanks for bringing that up. I know that was one of the problems we had in the early data set with the milligram thorium data was there was no correlation with the positive results, whereas we definitely see that in the post-`79 environment.

DR. LIPSZTEIN: The number of days between high entries -- oh, yes, because I was looking, there were about less than three percent of the workers that were monitored had a result above the MDA, and the problem is that if you take the whole population, you know, those points will be nothing in the coworker log-normal distribution. And these were the people that were working in redrumming, so I don't know if these were the people that were working in redrumming or not. Do you know that, Bob?

MR. BARTON: No, I don't have that information handy right now, Joyce.

CHAIRMAN CLAWSON: So this is Brad. I'm trying to figure where we're going here. So what, in my layman's terms, what are we trying --

MR. STIVER: Well, I guess, at this point -- correct me if I'm wrong, Joyce -- but what we're looking for from NIOSH is kind of a practical way that they could utilize the actinium results when they're, you know, higher than the lead or you've got, you know, a set of MDA lead value and you've got positive actinium, just a kind of claimant-favorable mechanism in the model to use that data. Isn't that kind of basically what we're striving for here, Joyce?

DR. LIPSZTEIN: Yes, exactly, because the years that we had actinium higher than lead is 1980 where we have 14 results of actinium higher than the lead result, actually above the limit of detection. And after `80,

actually after `82, the number of positive results is very low.

MR. STIVER: This is kind of what you'd expect from a, you know, from Stu's account of, basically, Fernald being a storage facility at that time with some limited redrumming going on. I guess the question in my mind is, for those individuals who did the redrumming, it's a big enough intake to register. It was 78. But would they possibly have, would the coworker model be representative and bounding for these people? And, you know, it certainly looks like it would be.

I mean, assuming that the monitoring was random and that you weren't missing a sub-population of these workers who were actually involved in the activity, then you could say that. I guess that's kind of what I'm looking for in all this discussion about who was monitored and when. There's certainly a lot of data there. And were the

people that had any exposure potential at all monitored? Again, this would be a short-term intake over a few days while they did the operation, like Mark was describing.

MR. HINNEFELD: Yes, this is Stu. I think the people that would be involved in redrumming would be on the in vivo monitoring schedule in some fashion. You know, the in vivo monitoring schedule was set by job title, and certain job titles fell into certain frequencies. And I think pretty much anybody who would have been involved in a redrumming operation would have fit into a job category that would have been on some in vivo monitoring frequency. Now --

MR. STIVER: That's my sense, too. It would have been the chemical operators, for the most part.

MR. HINNEFELD: I think you would have either chemical operators or people who were called laborers who would move things around sometimes. And both of those, I

believe, were on the in vivo schedule. I just don't know how often.

MR. STIVER: The fact that there's quick follow-up when there is a positive result is encouraging. It sounds like, you know, they were attuned to the possibility for exposure and when there was a result they followed up on it.

DR. LIPSZTEIN: Yes, but they were, you know, like, very few workers had positive results. And if they were exposed, then you have one interpretation of results. But if these are the results from working in previous years, the `69, the `70, before `79, then it could be also a reflection of the buildup of the daughters in the lung.

MR. STIVER: I don't think we're arguing about the validity of the data at this point. I think we've been through that.

DR. LIPSZTEIN: Something that we didn't see maybe. I don't know if it would be better for NIOSH to do some description of how

they are going to interpret this, not only a pure mathematical because we have all those sporadical interpreting lead and actinium with the, you know, the fact that the daughters leave the lung in a faster way than the thorium nuclide, and I know it's very, very difficult to get positive results and also the fact that lead depends on some assumptions about the number of separation and number of years that has passed since separation and actinium only on the number of years. And also you have if it was type S or type M thorium, some description that could be type M. So I don't know because if those are results from only redrumming, then separation took place a long time ago or some time ago before the measurements.

MR. STIVER: The fact that you have positives in `80 and `81 kind of --

DR. LIPSZTEIN: Exactly.

MR. STIVER: -- that hypothesis that it was due to previous exposures during

processing.

DR. LIPSZTEIN: Yes, maybe. I don't know. I don't know how many had processing in, you know, redrumming in `80 or `81. I don't know.

MR. STIVER: In terms of what you'd like to see from NIOSH, would it just be an approach to utilizing the actinium data to generate a thorium result, as opposed to --

DR. LIPSZTEIN: Both nuclides. Why not both nuclides? You know, you get the feeling from analyzing both nuclides at the same time.

MR. STIVER: If you have two results, then you'd use it --

DR. LIPSZTEIN: Yes, you'd use the two, yes. But that's a better approach. And see which ones, if you have M for, I don't know, some operation that you know some workers were involved in, then separate those workers and see what has to be done with -- you know, you have a lot of data but very few

positive results, so what does it mean?

MR. HINNEFELD: Okay. So then, as I understand our assignment here, it may be more than just one assignment. The first assignment is to, rather than ignore the actinium-228 and rely on lead-212 solely, what could interpretations be if we used both? If we considered both the actinium-228 result and the lead-212 result, what kind of interpretations could we draw?

And then the second part -- that's one thing you wanted us to do is what kind of approach could we come up with? And then the second thing was, given the fact that if potential exposures during the time period were probably limited to these repackaging operations, is there some other way that this data set could be utilized than what we would typically do in a coworker study where the population is exposed to the stuff and routinely over the course of your data set? Something like that?

DR. LIPSZTEIN: Yes, exactly.

MR. STIVER: So, Joyce, if I could just step in here for a second, during this period, repackaging period or storage period, for lack of a better term, what you were saying was that to kind of move away from the idea of this triple separation to make a claimant-favorable intake because it's just not applicable during that period of time, so kind of start over with a new model or a new approach altogether?

DR. LIPSZTEIN: I mean, see what you can do also with actinium result because in 1980, when you have most of the positive results, you have 14 actinium results with lead below the detection limit. But, also, you have to see what's practical and what makes a difference in the coworker model. The 1980 probably makes a difference. You go to `88 where you just have one result, no, two results above detection limit, it doesn't make really a difference for the co-worker model.

MR. STIVER: Well, in those cases, Stu, this is a question for you. I mean, the typical approach is to use a chronic exposure at half the MDA. And so you're still assigning an intake --

DR. LIPSZTEIN: Oh, yes, yes, yes, but most of the results are below the MDA. The MDA is the, you know, and now you have to choose what you want to use, either lead or actinium. I don't know. Actinium, you just have to make assumptions about the time after exposure, while lead you have to make assumptions about the number of separation and actinium. I don't know. It's up to NIOSH.

But it will make a difference maybe for the -- I didn't try it. It might make a difference for the coworker model. But the positive result is going to make a difference in `88 for the coworker model. And 98 percent of the results are below detection limit.

MR. HINNEFELD: Right. Okay.

Well, I'll see what we can do. I mean, this is probably not going to be me, personally, who does this, so I'll see what we can do. You know, as a practical matter, in terms of executing dose reconstructions going forward, it's usually fruitful to have a, not necessarily simple but not to have too complex a decision tree in a dose reconstruction, to have a relatively straightforward decision tree that gives you assurance that your bounding exposures in some times.

And so, you know, I want to be careful about drawing too fine a point on -- I mean, we can do some detailed work and comparisons to make sure that we're on solid ground with the approach we choose. But I would think that, in execution of dose reconstructions, ultimately, we would want to have a fairly simple, rather than a complex, decision tree on how we're going to interpret these numbers.

DR. LIPSZTEIN: I understand that,

yes.

MR. HINNEFELD: I think we can take a try at this, and then maybe the approach will be to have some exchanges of technical information. We'll provide them to, when we have something we can provide it to the Work Group and SC&A, and then maybe some back and forth and kind of go back and forth.

We won't do anything without, you know, strictly with SC&A, but we'll give it to the Work Group and SC&A so that the exchanges can occur, you know, with the Work Group engaged in it and see if we can narrow in on what's desired here because I guess I'm struggling a little bit on what's desired. We may need a technical call at some point with the right people on the phone to sort out exactly how we want to proceed here.

MR. STIVER: Yes, Stu. This is John. I agree. It's going to have to be kind of an incremental approach, and we'll work our way towards, you know, the best solution.

DR. LIPSZTEIN: Yes, because I think the solution of thinking of radium unsupported radium exposure is even more complicated.

MR. HINNEFELD: Yes, it might be. It might be.

CHAIRMAN CLAWSON: So this is something that, this is a Site Profile issue, correct, John?

MR. STIVER: Yes, it is, Brad, a Site Profile issue.

CHAIRMAN CLAWSON: Okay. So I guess my question is, is that are we going to capture this into the matrix to make sure that we don't lose this? I guess, unfortunately, I'm really kind of focused on the upcoming Board meeting and the SEC issue, and I just wanted to make sure that I didn't, I didn't miss this as being one.

MR. STIVER: Brad, this is John again. I don't think this is something that's going to really be crucial for the discussions

at the Board meeting so much as the earlier discussions we had. I see it more as just sort of an update, maybe a slide saying where we are on this or for the Site Profile level. You know, there are a lot of other Site Profile issues, you know, from our original report. Granted, some of those have kind of changed over time and new approaches have come up. Sandra had mentioned how the thorium intakes were calculated in the original Site Profile, and so it's considerably different than what we're seeing in this Revision 5 at the DWE level.

It's a bit of a moving target, and it's changed over time and evolved. Those are issues that we're going to want to take up. Those are things that will be on the plate after the SEC deliberations.

MEMBER SCHOFIELD: Could I ask a question, Brad?

CHAIRMAN CLAWSON: Sure.

MEMBER SCHOFIELD: Are we talking

about widespread throughout the complex there, or is this kind of like, say, on Plant 9?

MR. HINNEFELD: I didn't understand the question, Phil.

CHAIRMAN CLAWSON: Maybe I could help you, Phil. You're kind of breaking up as you come in here. One of Phil's questions was are we looking at just this specific plant for this, or are we looking at this kind of being widespread throughout the workforce? To me, from what I'm understanding, I'm just seeing a few people involved in this; is that correct, Stu or John?

MR. STIVER: Well, I think that's one of the issues we had, remember, before is the inability to determine who was necessarily a chemical operator and who had the high exposure potential. And I recall that was one of the main reasons why we couldn't carve out an SEC for a subcategory for the earlier period of `68 to `78.

So this is the kind of situation

where there were certain people involved in the work, but we can't really necessarily identify them. And so it kind of has to apply to everybody, to put it in the simplest terms.

CHAIRMAN CLAWSON: So, Phil, does that answer your question?

MEMBER SCHOFIELD: Yes, it does, Brad. Thanks.

CHAIRMAN CLAWSON: Okay. Yes, Phil, when you were coming in, you were kind of breaking up a little bit there, and it was kind of hard to hear you is what the issue was. Okay. So we're going to address this. NIOSH is going to look into this a little bit and correspond between SC&A and the Work Group to kind of figure out an approach forward; is that correct, Stu?

MR. HINNEFELD: Right. We'll work on something. And like I said, I think it might take a technical call to really kind of get us aligned on where we're going here, or we may start something and hand that over and

see is this close to what you're looking for.

MR. STIVER: I would agree that, you know, before you get too far down the road that we should have some discussions back and forth between --

MR. HINNEFELD: Okay.

MR. STIVER: -- to make sure we'll all on the same page.

MR. HINNEFELD: All right.

CHAIRMAN CLAWSON: Okay. Anymore discussion on item four? If not, the next -- well, it goes four to six real quick there. I didn't miss a five, did I, John?

MR. STIVER: No, that was just my bad memory there.

CHAIRMAN CLAWSON: Okay. I think probably cut-and-paste. Now, is there any other additional SEC or Site Profile issues that we want to --

MR. STIVER: There aren't any new ones. Like I said, there are Site Profile issues that are kind of in abeyance until the

SEC is determined, after which we'll have to kind of re-baseline the matrix and start looking at the Site Profile side.

CHAIRMAN CLAWSON: Okay. And then we've got, you've got the matrix as being updated with the changes? Because I know the first matrix to where we are now is quite a bit different.

MR. STIVER: Yes, we had focused on SEC issues almost exclusively.

CHAIRMAN CLAWSON: Right. Okay.

MR. STIVER: That's why I'm saying, you know, take it from 2006 up to the present.

CHAIRMAN CLAWSON: That's good. So I guess, I guess, right now, Work Group conclusions on some things. Some of them I can, but some of them I can't because we didn't have enough people. But I wanted to run back over your items here and see what, if I've got a better understanding on this.

On item two, we were discussing

the start date of `53 or `51, and we concluded that the start date is going to be 1951, is that correct?

MR. KATZ: That's correct, Brad.

CHAIRMAN CLAWSON: Okay. And then on issue three, this was really broke out into a couple of different areas in here. And we can't take this to the Board because we didn't have enough Board Members here, but there's a, when we do bring it there it's to accept NIOSH's ability to be able to reconstruct the thorium dose for the earlier years of `53, I believe, to `67; is that correct?

MR. KATZ: That's correct, Brad. And you can speak for Phil and you, Brad.

CHAIRMAN CLAWSON: Okay.

MR. KATZ: And then you can just call for the other Work Group Members to either address it or the whole Board can just simply take it up. But then the only difference is that they'll need a motion to actually act on this because they won't be

getting an automatic motion from the Work Group.

CHAIRMAN CLAWSON: And I understand. I was wondering how I was going to kind of work that. But we'll proceed on with that one.

And then the next part of this was the strategy for bounding 1955 Plant 9 intake, and NIOSH has agreed to use the 215 MAC; is that --

MR. HINNEFELD: That's right.

CHAIRMAN CLAWSON: -- correct? Okay.

MR. HINNEFELD: Right.

CHAIRMAN CLAWSON: And then number four we just got done discussing, and NIOSH is going to get together with SC&A and discuss how to be able to use the data that we have to be able to make the model. This isn't an SEC issue, but there's a problem there, correct?

MR. HINNEFELD: Yes, we have to sort some things out on how best to interpret

the data that's there.

CHAIRMAN CLAWSON: Well, I'm going to be honest with you, too. I've got to figure out really what we were talking about there because I kind of got a little bit lost. I understand what Joyce is saying, and I'm just wondering how -- I'm with NIOSH. I'm wondering how we're going, what we're going to do and how it's going to be implemented.

So with that, you know, as we've already said, we've got these recommendations that are going to come to the full Board in January and we've got our recommendations that we'll have. But like I said, because we lost one person, I can bring it up there.

With that being said, is there anything more that needs to come before the Work Group at this time that we need to discuss before January?

MR. KATZ: No, Brad. I just want to confirm you want to handle this session the way you wanted to handle the Pantex session,

which is for John to give a comprehensive presentation and you can then follow with sort of summary recommendations from the Work Group, you know, for the recommendation that was established, to the recommendations that are established, and then also, you know, speak for Phil and you on the remaining one that the Work Group didn't complete?

CHAIRMAN CLAWSON: That is correct. That's how --

MR. KATZ: Okay. We'll go forward that way, John. So you'll be sort of laying all the groundwork.

MR. STIVER: Okay. Now, are we talking about a complete covering the waterfront, or are we just kind of more focusing on --

MR. KATZ: Well, you're going to cover the waterfront but only the SEC recommendations that we're addressing, not the waterfront -

MR. STIVER: Okay. Otherwise --

CHAIRMAN CLAWSON: I feel that, over the last many Board meetings, that we've covered the bases and back and that we've got, we've had these outlying issues there. And, you know, if you want to give a brief summary of where we're at, and then these are the issues and this is what's been represented, and I can bring up the recommendations --

MR. KATZ: Because, for John, let me just clarify. I mean, you do want to, since this is sort of closing out SEC matters for Fernald, you probably do want to just remind the rest of the Board of where we've come, you know, what has already been put to bed and your background before you get into the details of the remaining issues. Right.

CHAIRMAN CLAWSON: What was that, John? I didn't hear you.

MR. STIVER: Like I say, it will be similar to the presentation I put together for California last year.

CHAIRMAN CLAWSON: Yes, it will

just be a little bit shorter probably, and we'll go from there. Okay. With that being said, is there anything else additional that needs to come before the Work Group that we need to discuss?

MS. BALDRIDGE: This is Sandra. I'd just like to thank everybody for their efforts and all their hard work.

CHAIRMAN CLAWSON: I appreciate that, Sandra. This has been a difficult one. You know, I think I can speak for everybody, your input has been very critical in this, and you've brought a lot of things to life that has helped us proceed forward. And I'd like to tell you thank you for all the hard work that you've put in, too.

With that being said, we'll call this meeting adjourned.

(Whereupon, the foregoing matter was concluded at 11:58 a.m.)