UNITED STATES OF AMERICA

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DEPARTMENT OF HEALTH AND HUMAN SERVICES

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CENTERS FOR DISEASE CONTROL AND PREVENTION

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NATIONAL INSTITUTE FOR OCCUPATIONAL SAFETY AND HEALTH

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FERNALD WORKGROUP

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WEDNESDAY, APRIL 22, 2009

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The workgroup convened in the Zurich Room of the Cincinnati Airport Marriot, Hebron, Kentucky, at 9:30 a.m., Bradley Clawson, Chairman, presiding.

PRESENT:

BRADLEY P. CLAWSON, Chairman

MARK GRIFFON, Member
PAUL ZIEMER, Member
ROBERT PRESLEY, Member
PHILLIP SCHOFIELD, Member

THEODORE M. KATZ, Acting Designated Federal Official

IDENTIFIED PARTICIPANTS:

JIM NETON, NIOSH ORAU
MARK ROLFES, NIOSH ORAU
ROBERT MORRIS, NIOSH ORAU

JENNIFER HOFF, NIOSH ORAU
BRYCE RICH, NIOSH ORAU
LEO FAUST, NIOSH ORAU
JOHN MAURO, SC&A
ARJUN MAKHIJANI, SC&A
LYNN ANSPAUGH, Consultant to SC&A
JOE FITZGERALD, SC&A

JOHN STIVER, SC&A
HANS BEHLING, SC&A
BOB BARTON, SC&A
HARRY CHMELYNSKI, SC&A
KATHY BEHLING, SC&A
NANCY ADAMS, Contractor to NIOSH
EMILY HOWELL, HHS

ROY LLOYD, HHS
ISAF al-NABULSI, DOE
RAY BEATTY, On Behalf of Petitioner

ALLEN CALLAWAY, Petitioner

SANDRA BALDRIDGE, Petitioner

P R O C E E D I N G S

9:38 a.m.

MR. KATZ: Good morning everyone. And welcome. This is the Fernald Working Group of the Advisory Board on Radiation Workers Health. My name is Ted Katz and I'm the Acting Designated Federal Official for the Advisory Board.

And sorry we're, you know, five or seven minutes late. We had some logistical things to deal with because we have a large presence at the meeting today.

So we're going to begin this with roll call beginning with the Board members in the room. And if the Board members would identify themselves starting with the Chair and speak to conflict of interest as well. That would be great. That goes for everybody.

CHAIR CLAWSON: Brad Clawson, Working Group Chair. Not conflicted.

MEMBER GRIFFON: Mark Griffon, Work Group Member. Not conflicted on Fernald.

MEMBER ZIEMER: Paul Ziemer, Work Group Member. Not conflicted.

MEMBER PRESLEY: Robert Presley, Work Group Member. Not conflicted.

MEMBER SCHOFIELD: Phillip Schofield, Work Group Member. Not conflicted.

MR. KATZ: Okay. And then checking on the line just to be certain we don't have any Board members, do we, on the line?

(No response.)

MR. KATZ: Okay. Then the room, the NIOSH ORAU Team please.

DR. NETON: Jim Neton, conflicted at Fernald.

MR. ROLFES: Mark Rolfes, NIOSH health physicist. No conflicts of interest.

MR. MORRIS: Robert Morris, ORAU Team. No conflict.

MS. HOFF: Jennifer Hoff, ORAU Team. No conflict.

MR. KATZ: And on the line? NIOSH

ORAU Team?

MR. RICH: Bryce Rich, ORAU Team. No conflict.

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

MR. RICH: This is Bryce Rich.

MR. KATZ: Bryce Rich.

MR. RICH: ORAU Team. No conflict.

MR. KATZ: Thank you. Welcome, Bryce.

MR. FAUST: Leo Faust, ORAU Team. No conflicts.

MR. KATZ: Any others from the NIOSH ORAU Team on the line?

(No response.)

MR. KATZ: Okay. And then in the room from SC&A?

DR. MAURO: John Mauro, SC&A. No conflict.

MR. MAKHIJANI: Arjun Makhijani. I have been declared conflicted on Fernald.

MR. KATZ: Speak up please.

MR. MAKHIJANI: I'm Arjun Makhijani. I've been declared conflicted on Fernald.

MR. ANSPAUGH: Lynn Anspaugh. I'm a consultant to SC&A. No conflict on Fernald. I have a general conflict that is having been an expert witness.

MR. FITZGERALD: Joe Fitzgerald, SC&A. No conflict.

MR. STIVER: John Stiver, SC&A. No conflict.

MR. KATZ: And on the line? Anybody from SC&A?

DR. BEHLING: Hans Behling. No conflict.

MR. KATZ: Welcome, Hans.

DR. BEHLING: Thank you.

MR. BARTON: Bob Barton, SC&A. No conflict.

MS. BALDRIDGE: Harry Chmelynski, SC&A. No conflict.

MR. KATZ: Harry Chmelynski.

Okay. And then other federal employees or contractors in the room first.

MS. HOWELL: Emily Howell, HHS.

MR. KATZ: And then on the line, any federal employees or contractors? HHS? DOE? DOL?

MR. LLOYD: Roy Lloyd, HHS. No conflict.

MR. KATZ: Welcome, Roy.

MR. LLOYD: Thank you.

DR. al-NABULSI: Isaf al-Nabulsi, DOE. No conflicts.

MR. KATZ: Okay. And then in the room, SEC petitioners or other members of the public who would like to self-identify?

MR. BEATTY: Ray Beatty, former site worker. I'm here on behalf of the petitioner.

MR. KATZ: Welcome, Ray.

MR. CALLAWAY: Allen Callaway, former worker at Fernald.

MR. KATZ: Welcome, Allen.

And on the line, do we have any members of the public who like to self-identify?

MS. BALDRIDGE: Sandra Baldridge, petitioner.

MR. KATZ: Oh, welcome, Sandra. We were wondering whether you would be here or on the line.

MS. ADAMS: Hey, Ted, it's Nancy Adams. I went to hit my mute button and disconnected you.

MR. KATZ: Sorry. But welcome, Nancy. So that's -- Nancy is a contractor to NIOSH. No conflict.

Any other members of the public or staff of the Congressional offices?

(No response.)

MR. KATZ: Okay, then, just a couple other things. For everybody who is on the line, just to remind you, I think all of you are probably familiar but mute your phone

except when you are speaking to us. And if you don't have a mute button, use star six.

Please disconnect. Don't use your hold button if you need to go away from the phone for some time because the hold button will interfere with the call.

And I would just mention for everyone here in the room since we have members of the public here to please just keep in mind Privacy Act concerns when you discuss material.

And with that, Brad, it's all yours.

CHAIR CLAWSON: Well, I'd like to welcome everybody here today. We're here for the Fernald Work Group. It has been a long time since we've met. The last time we met was 11/13, I believe -- that's '07 but it was November of last year that we met.

And in that, we had numerous issues that came up but today we're going to discussing the sampling plan that SC&A has put

forth, recycled uranium, K-65 silos. We're going to be talking a little bit about thorium and the radon breath analysis.

And we've had -- John, SC&A has sent out several papers on that. We want to make sure that everybody has those papers. And, John, you were to find out which ones were PA-cleared.

DR. MAURO: Yes, I got confirmation that the sampling plan and the RU report have been cleared.

CHAIR CLAWSON: Okay.

DR. MAURO: However, the radon contamination from the silos report has not been cleared however right now I have it with Emily who is looking over the key pages. There are four pages in there that I would -- that she's going to look at right now.

And hopefully she'll clear it. And I will be able to make copies and distribute those four pages. That's all we really need right now for the purpose of this

meeting is to go over those four pages.

Meanwhile, the report itself, the entire report, it's possible to get that cleared shortly also. But right now I'm confident that we'll have at least the key pages available for our visitors this year that would like the cleared material.

So that's the only report. We probably won't get to that report based on the order I think we're going until this afternoon. So we should be well poised to do that.

CHAIR CLAWSON: Okay. So the sampling plan, is that cleared?

MEMBER ZIEMER: That's cleared.

CHAIR CLAWSON: That's cleared. Do we have copies for the public?

DR. MAURO: No, all I did was send out electronic versions of the reports late last week --

CHAIR CLAWSON: Okay.

DR. MAURO: -- to the work group

and NIOSH. I do not have extra copies. We can have that done.

CHAIR CLAWSON: Okay.

DR. MAURO: Mine is heavily marked up. If someone has a clean one, we can get copies made.

CHAIR CLAWSON: I've got a -- probably a clean one. I'll take care of that afterwards.

MEMBER ZIEMER: Did the petitioner get copies, cleared copies?

DR. MAURO: They can.

MEMBER ZIEMER: Did Sandra --

MR. KATZ: Sandra, have you received any materials for this meeting from maybe Laurie Breyer?

MS. BALDRIDGE: Yes, I do.

MR. KATZ: Okay. Thank you.

DR. BEHLING: Excuse me, this is Hans Behling, SC&A. And I'm going to be asking John to identify those four pages in question that you say are likely to be at

least cleared by the time we discuss it.

DR. MAURO: Sure. I just handed the report and the four pages to Emily. So I don't have it in front of me. But as soon as she returns -- oh, she's here. Hold on.

Hans, the pages that I was planning on distributing to everyone -- have it cleared and distributed is page two, three, five, and ten.

DR. BEHLING: Just a quick question.

DR. MAURO: Yes?

DR. BEHLING: If those are the pages you are able to hand out to participants who are present in the room, is it possible for me to go outside of those pages? Because I was hoping to discuss a few things that are not contained on those pages.

DR. MAURO: Absolutely. We just can't hand out -- in other words we can speak about them, of course, with the guidelines not to divulge any Privacy Act materials. But

certainly you can speak to any aspect of the report that you'd like to, sure.

DR. BEHLING: Well, I can assure you there's no Privacy Act issues here in the entire report.

DR. MAURO: Yes and Emily is here to make sure that we stay within the boundaries. Okay?

CHAIR CLAWSON: And I'd also like to bring up -- everybody knows that we work from a matrix on this. And it's been kind of so long and so forth. We're just reviewing the matrix right now. So, John, if you'd like -- if we could, I'd like to start from the sampling plan and then to the recycled uranium stage contents with the matrix.

DR. MAURO: Yes.

CHAIR CLAWSON: Would that be all right?

DR. MAURO: By way of introduction, last night I read through the transcripts from the October meeting just to

make sure I got my arms around the issues. And in addition to the subjects that we are planning to discuss today, I did notice that there were a few other items that came up during that meeting.

If you'd like, I could -- I sort of made a list of the things that we are going to cover. But the other things that we talked about and sort of left open that perhaps we should not lose track of.

We could do that now or we could just put together a matrix at some future date to make sure we pick those up. You know?

CHAIR CLAWSON: I think we could start in.

DR. MAURO: We could start right away.

CHAIR CLAWSON: And in closing, we can review through that and make sure that we have captured everything and we'll be able to look into the matrix on that.

DR. MAURO: Fine.

Then with that, let's start with the sampling plan. This is a document I believe was sent out as PA-cleared, as DOE-cleared. And it's dated March 2009 on the cover page. And it's title Draft Sampling Plan for Use in Evaluating the NIOSH Internal Dosimetry Coworker Model for Fernald Workers.

A little history here. When we previously met, SC&A did come to the table with a sampling plan, draft sampling plan that was designed to evaluate the completeness of the dataset, completeness in terms of is there adequate data for the different buildings? Is there adequate data for the various categories of workers? In terms of what percent of the workers had bioassay data -- this is basically bioassay data.

During that meeting, it was decided no, no, no, we don't want to do that. We want to do something a little different. We want to do that but we want to do more because between -- because by the time we had

the meeting in October, NIOSH had issued a coworker model, a very specific coworker model on how doses, internal doses from intake of uranium would be reconstructed for those workers who had -- did not have data or had limited data.

A very important underpinning of all this is -- the general concept was that well, there was a lot of data. And for most workers, you would not need to use a coworker model. But there will be some. So the coworker model was put in place.

We were asked to develop a sampling plan that would accomplish a number -- - at that last meeting -- accomplish a number of objectives. One is completeness, adequacy, but most important, we were asked to develop a plan that would -- when you are finished doing the sampling, you could feel confident that the plan will not underestimate the doses to workers that have the potential for high-end exposures. That somehow that coworker

model did not underestimate at least some of the workers that had a higher potential for exposure. And that's what we developed.

We developed basically -- the actual sample -- the number of samples are not in the plan. What we really have here is the strategy for where we would sample, which workers we would sample, what years we would sample, what buildings we would sample. But we don't actually have the number and the names of the workers that we would actually sample in the plan.

That's something that we didn't do. We thought it was more appropriate to discuss in general whether or not this is, in fact, the sampling plan that will meet your needs.

So with that as a sort of preface, I'd like to start to walk through this. If you would look -- I'd like to first describe what the coworker model is. If you wouldn't mind opening up on your screen to page two of

the report. The first thing we did in this report is to describe the coworker model that NIOSH developed.

And by the way, Jim, if in any way I misrepresent our understanding of the coworker model, please help out.

You'll see on page two, Table 1-1, this is a look-up table that is your coworker model. Let's envision we have a worker that you wanted to reconstruct the internal dose from the inhalation of uranium but you don't have a complete dataset on bioassay data or you don't have any data on bioassay data for this worker. And you want to reconstruct his internal exposures.

You go to -- there are basically three tables. One on page two and two on page three. The first table is -- if you believe -- you first ask yourself the question okay, here we have a worker. He has a certain type of cancer. What type of uranium, F, M, or S would give the highest dose to the organ of

concern?

Let's say you determine it was a lung cancer, just for an example. That being the case, you would go to the table on page three that I -- it's Table 1-3. Basically that's the look-up table for Type S uranium.

And what it says is okay, if the worker worked from 1/1/52, start of operations, to 12/31/53, you would assume that he would have a distribution. You would assume his intake rates for uranium Type S was 8,197 micrograms per day with a geometric standard deviation of 3.44.

So it becomes just a look-up table. And for that worker, you know how many years he worked there. You would assign those intake distributions to that worker. And you would run it and get your dose to the organ of concern.

And now the question becomes -- and these are the additional side pieces which we are going to talk about a little more

later, is in addition, it is assuming that those micrograms per day ingested were at two percent enriched uranium. And what is being assumed is across the board, everyone is going to be assumed to have two percent enriched uranium.

We looked very carefully at that assumption to convince ourselves that that, in fact, is a reasonable if not bounding approach and this was discussed at the last meeting. And the answer was yes.

Even though there were some workers that might have had six, seven, eight, ten percent enriched uranium that they worked with, it was generally for a relatively small period of time.

So by assuming it was two percent for his entire work history, that blends out, so to speak, and the outcome is legally to be a conservative assumption. So we are comfortable with the two percent default assumption embedded in this process.

There's also the question, and we're going to get this in much greater detail, on recycled uranium. The key to the coworker model was to say okay, once you know the activity or amount of uranium that was inhaled, using the coworker model or using the worker's actual data, you assume a certain mix of plutonium-239, neptunium, technetium, and other fission products as being the material that goes along with the uranium as a default intake.

This is the so-called recycled uranium issue. We do have some concerns with that. So unlike the two percent enrichment where we're comfortable, we do have some important concerns regarding recycled uranium. That's the subject of a separate report that we're going to go to after we finish this report. And we'll get into some detail.

Okay. Now everyone has a pretty good sense of this coworker model. Now the question becomes --

DR. NETON: There's just one point of clarification that I think will come important later. If you notice, there is a minimum GSB of three in these columns, those are not calculated GSBs. That is the minimum GSB that we would assign to a distribution that was measured acknowledging the fact that at a minimum, there is a GSB of three associated with the biological variability of the models and such.

So that's important because then that rises to the 84th percentile when the comparison is done by SC&A later.

DR. MAURO: Okay. Good.

MEMBER ZIEMER: So it is only three if there's not information to show that it's higher than that.

DR. NETON: If the GSB, for instance, came out 1.6, we would automatically at a minimum have a GSB of three which will kind of increase the 84th percentile of distribution. So I think there have been some

mismatched comparisons later on. But --

DR. MAURO: Okay. You're right. There is that.

All right. Let's go on. Now you say to yourself, okay, so now we have default intake rates. The way those default look-up table intake rates were obtained, if you go to page four, you'll see a table called Table 2-1.

What this presents here is an excerpt of a four-page table that is in the coworker model that says this is the data that was used in terms of excretion rates. That is micrograms per day of uranium excreted in urine by year. In fact, it's actually by quarter.

The only place where they've rolled up information is in the '52 and '53 time period where there wasn't enough data to parse it by quarter. But beginning in '54, there was sufficient data to sort by quarter.

This table goes on, I believe,

into the '90s. I'm not sure but we can look it up but it goes on for quite -- in other words, you have quarterly data that goes on.

And what we basically have is the excretion rate in micrograms per day at the 50th percentile and the 84th percentile, on a log-normal distribution that was determined -- that was measured --

MR. ROLFES: John?

DR. MAURO: Yes?

MR. ROLFES: The data do go through 2006.

DR. MAURO: 2006, thank you for correcting me.

So I would first offer an observation that this is quite a bit of data, okay? So what you have is a dataset. We're going to get into a little bit more detail on how much data this is because right now we're looking at a mean, median, and a standard deviation or a geometric standard -- 84th percentile. But, of course, that reflects a

number of individual samples of urine.

So what we did was say okay, let's take our face value, this long table that goes on for several pages, let's see if using this we can match the intake rates that are on those tables we showed you before. And we did.

So given that this is a correct representation, a complete, accurate representation of the distribution of excretion rates, we confirmed that the numbers that are being used as the coworker model are, in fact, compatible and consistent with the excretion rate. So a minor point but, you know, we did that check.

Now we're going to move on and get to what's the heart of the matter. Let's jump off to page eight.

And one of the things that this report does is, besides being the foundation upon which we could build a sampling plan, it is also very informative in terms of getting

a feel for the amount of data that's out there and its granularity so that each individual around the table can make a judgment for themselves whether or not this is a lot of data that looks like it's rich and with a great deal of granularity or there are places where, perhaps, it is weak.

Attachment A, page eight, this is the beginning of where SC&A started to go into the HIS-20 database and started to sort information. Now if you recall when we looked at the data on page four -- I'll get to that Table 2-1 -- it basically gave you by quarter for each year.

Whoa, we said to ourselves, hold the presses. Where could there be hidden problems? And one of the things we said to ourselves is a hidden problem could be that well, listen, if I'm looking at a particular year and I'm rolling up all the bioassay data for hundreds of workers, maybe thousands of bioassay samples, and I'm giving you the mean

and the standard deviation for that year, I effectively have captured the full distribution of bioassay samples observed in that year. And it crosses all work categories and it crosses all buildings.

So the first concern that we said was what happens if within that array of data, there might be a group of workers that have a particular job function or a building in that year that had a particular operations going on, if I was to pull that group out separately, which it hasn't been done in your coworker model, is it possible I'll find that the 50th percentile and 95th percentile or the upper bound values are a lot different than this so-called aggregate value?

If that's the case, we've got a problem. So one of the first things we started -- you know, that's how we started to think about the problem. That is assigning an aggregate 50th percentile and 84th percentile for a given year to all workers, all work

categories, all buildings, you know, in theory there could be a problem if there's some group of workers that consistently had a higher-end exposure in that year or maybe many years.

DR. NETON: And that is assuming that that work category had no bioassay data --

DR. MAURO: Correct. Now I would want -- and that's -- but I want to get you into the way we are thinking about the problem. And this is a recurring theme in all of the work we do. And that is -- the recurring theme is granularity.

Whenever you have a group of data for a given year or a given facility and you have a mean and you have a standard deviation on the data, you know, where things are sort of pooled, and if it turns out there is a significant fraction of workers that really don't have data or have adequate data, you have to ask yourself for the place where we do have data and we do build a distribution from

that data, will we pick off some parameters for that distribution?

Is it possible that there is a group of workers that were unmonitored and that fall at the high-end of that distribution and we're going to underestimate their dose?

Now I would be the first to agree that in this site, and you'll see as we get through this, once you get past the first couple of years, we're talking about over 90 percent of the workers that were working there have bioassay data. So the need to use the coworker model is the exception to the rule.

That is the vast majority of claimants will -- their dose reconstructions for internal exposure for an inhalation, an ingestion of uranium is going to be done using their data.

And the question we're asking ourselves now is well, for those individuals that we may have to resort to the coworker model, how robust is that coworker model? And

what kind of sampling plan can we implement to convince ourselves that there are not going to be groups of workers that we are going to underestimate.

All right. Now --

MR. MORRIS: Can I ask -- I have a question --

DR. MAURO: Sure.

MR. MORRIS: -- at this point. The concept you are proposing then is that there is -- we've got population data and you are subdividing the population into subpopulations --

DR. MAURO: Yes.

MR. MORRIS: -- and say how representative is that.

DR. MAURO: Yes.

MR. MORRIS: How small can a subpopulation go before it becomes an individual.

DR. MAURO: We're going to talk about that.

MR. MORRIS: Okay.

DR. MAURO: Good question.

MR. MORRIS: And I think that it really points to the big picture is that, you know, you, by definition, can find subpopulations that are above me.

DR. MAURO: Well, you're going to see what we propose as a way of testing how robust and favorable this particular coworker model is. And around the table we can judge whether or not that is a fair test.

And in the end, we're going to actually suggest a test. Okay, what is it we're going to do to -- what do we suggest we do to convince ourselves that yes, this looks pretty good -- or no, it may not be.

We will discuss the test. We don't know what the results are going to be. But we're going to discuss whether we think that is a fair test.

DR. NETON: I'd like to make one observation for what it is worth and I'm going

to hold off on this one. I'll just throw this on the table as you discuss the plan.

If, by definition, we have bioassay data for more than 90 percent of the claimants or 90 percent of the workers, it probably holds true for the claimants. I think Mark told me it is 92, 93 percent of the cases have bioassay data. Then it seems to me that this sampling plan is looking for the proverbial needle in the haystack.

Where is that one group that could have been missed when, in fact, it would seem to be more efficient to go look at the 50 people that don't have bioassay data, identify their work categories, and then go back and start looking and saying are those classes of workers really the ones that had potentials for large exposures to which if we would apply this coworker model, we'd be underestimating their dose.

You're looking at potentially 400,000 records here. And we've got a

thousand claimants at Fernald roughly. And let's say 95 percent have bioassay. There are 50 that probably have zero bioassay data in that ball park.

And so that why would one look at 400,000 records to find the ones that --

DR. MAURO: Well, remember --

MEMBER GRIFFON: Instead of hypothetical categories, look at real categories.

DR. MAURO: Let me give you this, in a given quarter, the question is how many people are we talking about? We're talking about two, three, 4,000 workers who have unique social security numbers. And what we're saying is in 1952 and '53, 90 percent of those, on that order -- in 1952, 90 percent had no bioassay sample. So there's something -- '52 looks a little weak.

In '53, 58 percent had no bioassay data out of 2,400. But eventually -- let me show you how I'm looking at this -- eventually

once you reach 1957, 95 to 98 percent of the workers have some bioassay data. At least one if not more.

So right off the bat I would say you just described a different strategy. And we're talking about on the order of anywhere from 3,000 to 4,000 workers. Now let's say it turns out two percent of 4,000 workers or three percent of 4,000 workers have no bioassay data. You're saying that we can go in and take a look at a sample from those and see whether or not there is reason to believe that based on their work history, they may be people who could have had a high -- could have been exposed.

Or is there evidence that no, these are workers that very little potential for exposure. We did not propose that. That is --

DR. NETON: One more point of clarification, too, is you have to look at how we apply these coworker models or how we apply

bioassay data in general. If a worker had no bioassay data until 1957, we would not apply, more than likely -- I can't think of a case of how we would do that -- this coworker model would fill in '52 to '56. We would calculate some chronic exposure intake that could have occurred and resulted in that bioassay value in 1957.

So the mere fact that there are a small fraction of workers monitored in '52 to '56 does not prevent us from doing bioassay data for workers who were still on in '57 and moving forward.

DR. MAURO: Exactly. Very good point. So you have to -- so you're saying -- let's say we have -- we're in 1957, we -- by the way, all these workers are workers that were there starting in the '70s. All right, so you're saying we have a worker that was there beginning from '52 working right through 1970. And we start to have plenty of data for him let's say starting in '57.

And now you say well, we have to fill in the earlier years. You would fill in those earlier years based on a best fit?

DR. NETON: Yes.

DR. MAURO: As opposed to going to the coworker model. When would you use the coworker model?

DR. NETON: The coworker model has zero data, essentially zero data for anyone.

DR. MAURO: Any worker -- there's a very good chance that there's no workers that never had any bioassay --

MR. ROLFES: Let's plug in some numbers, you're saying 3 to 4,000 workers at Fernald. I'll give you, you know, some comparison to the number of claims that we've received at NIOSH for dose reconstruction.

We've received 1,040 claims versus the, you know, larger population at the total Fernald site.

Before you had mentioned some lung cancer cases. That was the -- you know, that

was what you had cited in your report.

DR. MAURO: As an example.

MR. ROLFES: As an example, correct. So what I did is went and looked to see the number of lung cancer claims that we had received for dose reconstruction that were less than 50 percent probability of causation.

Then what is did is went and looked at their job categories and the amount of data that they had. I found roughly 16 claims that had less than 50 percent probability of causation and looked through the job categories in the data that we've received. There were approximately eight claims that did not have any data or did not have any internal dose reconstruction information in there that we could use.

So if you look at the actual job categories, there's a variety of categories. And let's see -- if you take a look, some of these people have very low latency periods so there's not very much time in between the

first exposure and the date of diagnosis.

So essentially for some of those people that have less than five years, for example, for a solid tumor, five years of latency, no matter what uranium intake we assign -- so I don't foresee this being a large population of claims.

DR. MAURO: Neither do I.

MEMBER GRIFFON: Can I step back? Can I go back one step further? And this is, I think, why I thought and I'm trying to catch up with all the matrices but this is why we decided to question -- go down the path of questioning data completeness and validity more so than the coworker model.

This is like deja vu all over again. But that's the problem with having these meetings so far apart. I mean this is very much like the Rocky Flats situation. You know the coworker model was not used for many claims, right?

So we ended up looking at the

actual -- a fraction of the claimant's data and saying okay --

MR. KATZ: Can we hold? Can we hold? We've lost the line. I don't know when we lost it.

(Whereupon, the foregoing matter went off the record at 10:13 a.m. and resumed at 10:14 a.m.)

MR. KATZ: Hello, this is Ted Katz with the Advisory Board on Radiation Worker Health. We lost the line. It was disconnected briefly.

But can someone on the line just tell me how long have we lost the line for?

MR. RICH: It's been about ten minutes.

MR. KATZ: Ten minutes, okay. We're on the same issue. There's been a lot of interesting discussion but it would be very heard to recap it because it has been on a lot of different points.

We're sorry about that. It's just

a physical problem here in the room.

MEMBER GRIFFON: But anyway, to finish my point, you know, the reason we went to data completeness there in looking at the data, the completeness of each claim in the file, you know, we looked at it and said okay, is there enough data there to reconstruct dose?

And this is to Jim's issue, maybe they didn't have many singles but they had enough to do a chronic exposure and bound their dose. It was also for the external side. And I know this was somewhere in that transcript.

But, you know, so then somehow we -- I don't know if we lost this whole data completeness side and validity. I know that at some point NIOSH did look at HIS-20 compared to raw data. And they gave a report on that.

But I don't know that we ever looked at this completeness of the individual

records. So we know that we're not going to rely on coworker models very much.

The question is is there enough data in there because part of the reason this -- at least for me, a part of the reason this comes up is that this question of in 1970, I think, the database itself only has people that were still working there in 1970 or something. So we want to make sure in their hard copy records that everything is there or nothing is there to reconstruct their doses. And we sample a fraction of individuals.

DR. NETON: I'm not sure where that 1970 date came from.

MEMBER GRIFFON: Oh, okay.

DR. NETON: We need to look into that. I was talking to Mark about that this morning. I mean I was there when this company was put on line. And I was reasonably certain we had everybody transfer over from the various legacy computer systems. So we need to look into that. I'm a little bit confused

by --

MR. MORRIS: That sounds like a different site to me actually.

DR. NETON: I don't -- we made a very concerted effort to consolidate all of the legacy databases.

MEMBER GRIFFON: That may have been true at Rocky Flats actually now that I think about it, yes.

DR. NETON: We will look into it.

MEMBER GRIFFON: At any rate, still the issue that I have stands with the question of, you know, validating the -- or data completeness and validation rather than -- I mean this sort of tests the coworker model and I'm not dropping this issue but, you know, I'm sort of stepping back to say how did we eliminate those other two.

DR. MAURO: Well, at the last meeting, we did have a sampling plan which was designed to make a statement about completeness.

That is the outcome of that last proposed sampling plan would have been we're 95 percent confident that at least 50 percent of the workers in this group have bioassay data with a sampling plan that had that as its end result.

That is we could say with some level of confidence what percent of the workers had at least a certain number of bioassay samples. It was a completeness statement. It was designed around the necessity of completeness.

During the course of our workgroup meeting, we went on for most of the meeting -- I read the transcript last night -- saying that well, you know, now that there is a coworker model, we're still interested in completeness but we're even more interested in making sure that the coworker model is claimant-favorable, bounding. Is there a way to sample the coworker -- is there a way to sample the data to convince us that the

coworker model is robust?

So the attention shifted away from completeness -- and this is the language that is in the transcript. So we went back to the drawing board and came up with this which I think --

MEMBER GRIFFON: Well, I think we're talking past each other a little bit still. I mean I'm not talking about completeness of the electronic database. I'm talking about completeness of the individual files for workers.

And I thought in our last meeting that we had an action to propose an approach to sample groups -- so we did talk about targeting the jobs with higher potential for exposure.

DR. MAURO: We had that.

MEMBER GRIFFON: Yes.

DR. MAURO: But we didn't go into the hard copy. Everything that we did was electronic.

MEMBER GRIFFON: Right. Right.

DR. MAURO: Everything we were working with was the electronic database. We did not do any things like we did on NTS where we went into handwritten records or hard copy scanned records and go into that original data.

And when we discussed this matter at the last meeting, there was some discussion about was the data, the hard copy of scanned data faithfully transcribed from the original set into the HIS-20 database.

And there was a report prepared that's on the record that NIOSH presented that I do not believe we reviewed that was quite extensive showing that it was faithfully transcribed.

MEMBER GRIFFON: Yes, and that's NIOSH's report, right, right.

MR. MAKHIJANI: I'm looking at the completeness plan that we sent to the working group before the last working group meeting

dated October 6th and the design of that working plan -- well, let me just read it -- in general we wish to determine if workers at Fernald were monitored during specified time periods and with what frequency.

The main metric to be used is the frequency of actual monitoring for the subpopulation of workers compared to the plan frequency, once a week, once a month, or once a year according to job title.

That was the design of the plan that you brought from which then there was a new instruction given to go back and design a new plan.

DR. MAURO: That's in here. In other words, in effect, we didn't implement that plan but as we go through this, you can decide for yourself whether or not to a large extent that question has been answered. So it's not going to take that long.

MEMBER ZIEMER: Could I ask one other clarification question, though, John?

DR. MAURO: Yes, sir.

MEMBER ZIEMER: On the column where you give the workers with no samples, as I understand it, you are only talking about for that year.

DR. MAURO: Yes.

MEMBER ZIEMER: For example --

DR. MAURO: Yes.

MEMBER ZIEMER: -- that worker might have gotten picked up --

DR. MAURO: Yes.

MEMBER ZIEMER: -- in the subsequent year --

DR. MAURO: Yes. And that's the point Jim was making.

MEMBER ZIEMER: That's the same point then, okay.

DR. MAURO: Yes.

MEMBER ZIEMER: So the idea that, for example, in '53 that 59 percent of the workers have no bioassay, that doesn't mean that 59 percent of the workers have no

bioassay in their record. Only for that --

DR. MAURO: Absolutely correct.

DR. NETON: In fact, we know in the claimant population, 90 percent-plus of the claimants have some bioassay data.

MEMBER ZIEMER: Right. Right.

DR. MAURO: My -- I am trying to --

MEMBER ZIEMER: So this is really -- it's something workers with no samples for that year.

DR. MAURO: Absolutely. And that's why the table is structured this way.

MEMBER ZIEMER: Yes. I understand.

DR. MAURO: That's what it means.

Now I think it is important to point out that this table demonstrates that at least by year -- I realize this is rolled up -- rolled up in this data are all the different buildings and all the different job categories -- but from the point of view as a function of

time, the percent of workers -- a large number of workers that had bioassay data is enormous.

I would say that after looking at data sets for quite some time now, five years, they don't come any better than this. I'm sorry I have to say that. This is complete in terms of the percentage of workers that have bioassay data.

Now you may have questions regarding assumptions on recycled uranium. But when you look at these data, except for 1952 and '53, once you start moving into the late '50s, the percent of workers that have at least one, and a very large percentage have more than four, samples per year is large.

So -- and you folks, of course, make your own judgments on whether that is large enough. But what the purpose of this table is -- to show, at least by year, there is a lot of bioassay data. It's all in milligrams per liter.

So that's the only message I

wanted to leave regarding Attachment A. And we have other important attachments --

CHAIR CLAWSON: John, I just need a clarification on one thing.

On this paper here at the end of this, you've got maximum number of samples per year, per worker, per year, and somebody got 229?

DR. MAURO: Yes, I circled that.

Bob Barton, are you on the line?

MR. BARTON: Yes, sir, right here.

DR. MAURO: Could you help me out a bit? Do you have Attachment A in front of you?

MR. BARTON: Yes, I do.

DR. MAURO: The far right-hand column called maximum number of samples per worker per year, am I correct in assuming -- right now I'm on page eight -- when I see 229, does that mean that there is a worker who in that year had 229 bioassay samples collected?

MR. BARTON: Yes.

DR. MAURO: Thank you.

MR. MORRIS: Can I follow up on that?

DR. MAURO: Yes.

MR. MORRIS: If that person was in one of your subgroups, you would probably identify that person as having a significant intake during the year. That's the only reason to sample that often.

DR. MAURO: I just wanted to make sure on that one.

MEMBER ZIEMER: That's virtually every working day.

DR. MAURO: Yes.

DR. NETON: I have another point I'd like to bring up about the coworker -- the coworker model -- is that we make no overt attempt to strip out all the incident samples that are in there, which tends to bias the upper end on the high side, because unless it is something really obvious like, you know, three milligrams per liter where it is just

physically impossible, they are left intact.

So all those samples are -- and we are assuming that those are chronic exposures because of the chronic exposure model.

MR. MORRIS: Now had that person been in the subgroup that you have picked as an analysis category, there is no doubt that person would have biased your subgroup.

DR. NETON: Yes, I suspect there is a pain curve that shows up later here. It was probably an incident. Those are all from one guy.

DR. MAURO: See, one of the problems with the program that's -- with the sampling plan is -- let's say we go in and say okay, we want to test this. The coworker model is claiming him. And we happen to pick this guy as being -- well, we're going to go in and pick a guy, and we have data on him. And we reconstruct his dose.

And we say, how does that dose stack up against the coworker model? And we

know what is going to happen -- exactly, he's going to come in much higher. That's one of the fundamental weaknesses in the sampling plan.

That is, the people that we pick -- you're going to see -- we're going to get to a point in this process where we'll say, well, who are we going to pick to determine whether or not this coworker model is claimant-favorable and can be used as, you know -- and we're going to talk about that.

And the point you make is very well taken. You could very well walk away after the sampling plan. We randomly sampled. And we're going to show you how we think you could randomly sample to see if there are any surprises.

You may very well come out with a positive -- a result that says the coworker model would underestimate this person's dose by a factor of two or three or four if it was used. But then you would say well, wait a

minute, we have -- we wouldn't use the coworker model.

DR. NETON: Exactly. That's a circular logic there.

DR. MAURO: What do we do?

DR. NETON: The model is wrong because it doesn't account for the people who have bioassay data.

DR. MAURO: I'm going to let the work group, you know, make these judgments. We went through a -- you have to understand, we went through a process saying let's create a compendium of data. So understand what we're looking at. And you now go -- how many bioassay samples do we have by quarter?

Let's move on. I think you understand. I fully understand what you're saying and I want to completely -- I want to make it very clear, you know, what the strengths and limitations are on the thing that we are just talking about.

But right now all I'm doing is

communicating factual information. I'm not drawing any conclusions. I'm trying not to.

You will see, if you move on --

MR. ROLFES: John?

DR. MAURO: Yes.

MR. ROLFES: Also to make another comment about the years 1952 and '53, you pointed out workers with no samples during that year and that year only.

DR. MAURO: Right.

MR. ROLFES: Keep in mind also that there is a lot of construction activities ongoing. And not all the plants are operating at this time. So there are a lot of employees that are building new buildings, not working in radiologically-controlled areas. So there is a reason that many of them aren't sampled as well.

DR. MAURO: What happens is -- when we get past those tables and go to page 16 -- and in fact that's your roll-up by time -- here's the numbers of samples -- here's the

number of workers, here's the number of bioassay samples by quarter, and then the workers by quarter, and what the percent of workers that have at least one, two, three, four, or more than four bioassay samples in that particular time period.

And the story that emerges from this is that almost -- over 90 percent of the workers have at least one, and 25 percent or more have more than four bioassay samples each quarter -- I'm sorry -- each year. Not each quarter, each year.

Starting with page 17, is a -- and I don't want to spend a lot of time on these graphs because they basically tell the same story that I just did, but in a graphical way.

So you could look at it and quickly get a picture of -- one that's especially useful, just to get a quick snapshot, is go to page 18. There is a graph. And it's got a blue color line and a red color line. And this is the number of -- we're

comparing the number of unique social security numbers, which is the blue line, against the number of -- the people that have bioassay samples.

And you can see up through 1980, just about everybody has at least some bioassay samples. They track each other. This confirms the statements that you folks have been making.

Now, you do see a deviation -- as you go past 1985 -- where the number of workers on site versus the number of workers with bioassay samples, it looks like about 50 percent. Now in my opinion, that means -- okay, half the workers, for some reason, were not bioassayed in those years, but half were.

The question becomes, is it possible some of the workers that were not bioassayed could have been workers that had higher exposures than the workers that weren't bioassayed? This is a question someone could reasonably ask.

DR. NETON: I can answer that question. Starting in 1989, only workers who had the potential to see 100-millirem exposures were required to be monitored per the change in the regulations. So they were very well vetted and considered to be on the bioassay program or not.

And people who worked on what was called the clean side were certainly not monitored. People who worked -- were frequently in the process area -- let's say I have the potential to receive 100 millirems -- and that was based on an analysis of their --

DR. MAURO: So a policy change occurred.

DR. NETON: It was a regulatory change.

DR. MAURO: A regulatory change.

DR. NETON: 54(a)(35), 54(a)(11) was issued.

MEMBER GRIFFON: How that was implemented is a question at several sites.

DR. NETON: I know exactly how it was implemented because that's when I started working there.

DR. MAURO: Okay. And before that -- you can see before that, before 1980, it looked like the policy was, everybody gets a bioassay sample.

DR. NETON: There were no controls. I mean out back, no controls. The areas were not cordoned off, the radiological areas, as well as they were after the change in the regulations when you had posted regulatory areas, restricted areas.

MR. ROLFES: Also keep in mind, John, that -- the SEC class that we evaluated was for the years of 1951 through 1989. So if we're having an SEC discussion, really what happens after '89 is, you know, for a site profile -- it's technically a site profile issue. So I want to point that out.

DR. MAURO: We haven't gotten there.

I'm not going to -- it goes on for several pages of graphs. The recurring theme is, a lot of people have bioassay samples.

Let's move on to -- we've got two more points to make and then we're going to be ready to discuss this.

Let's go to page 23. It's an important page. This is where we start to talk about whether or not it makes sense to do any sampling. And taking into consideration the things we've discussed.

On page 23, what we say is okay, if there is any -- I'd like you to -- put your finger also on page 31. So open up to page 23 but also put your finger -- sorry.

PARTICIPANT: This is a test, right? Dexterity?

DR. MAURO: Let's just stick with 23 right now. Stay with me. On page 23, what we did is say listen, if there's any weakness in your coworker model, it has to do with -- we know that you've rolled up all different

workers and we know you've rolled up all the different job categories.

And what you didn't look it, are there groups -- the question is are there groups of workers that have bioassay -- have intakes of uranium that are substantially higher than the intakes that would be represented by a quartile, notwithstanding the fact that they probably don't exist because you are claiming that 90 percent -- and it's true -- 90 percent of the workers.

I'm going to leave -- I want to put that aside for a minute. I'm looking at this as a purist, saying -- listen, how do we find out if there are groups of workers that either had job functions or worked in buildings at given periods of time where they may very well be different than your coworker model. Their data shows they are different than the numbers you've picked.

This table starting on page 23 tries to answer that question. Let me tell

you what you're looking at. In that table, you'll see -- the very upper left-hand corner, it says 1953 and it says Building No. 1. So this is the first time we're looking at a little more granularity.

We were able to go into the database -- and we have the folks on the line that did the heavy lifting and they could give you a little bit more of how this was done -- but we were able to go in and start sorting on the data in a way where we could say, oh, no, we could actually go in and pull from the database the bioassay records for workers that worked in Building No. 1 in 1953, et cetera, Building 2, Building 3, '54, '55, '56.

And we could stop to ask ourselves the question -- and we could look at their data and say, is there anything about the parameters that characterize the worker population in that strata that says it might be different than the overall coworker model.

The number 181 is simply the ratio

of the doses to the workers in that strata --

DR. NETON: Intakes or doses?

DR. MAURO: This is excretion. Okay.

DR. NETON: Excretion or intake?

DR. MAURO: Samples, sorry, yes, it's samples. It's bioassay samples.

DR. NETON: So it's the 50th percentile of what?

DR. MAURO: Of the --

DR. NETON: Excretion?

DR. MAURO: Picocuries per day in urine. Bob, do I have that right?

MR. BARTON: I'm sorry, John. Can you repeat the question?

DR. MAURO: Yes. A new question was asked, and I think I have the answer but I'd like you to confirm.

In Attachment B, page 23, we have numbers -- it says, for example, 181 -- do you see that one in the upper left-hand corner -- the very first number that is shaded?

MR. BARTON: Yes.

DR. MAURO: Okay. That's a ratio of -- that is an expression of the excretion rate of uranium in that group of workers for that -- Building 1, 1953 -- the median for that group versus the median or the 50th percentile for the excretion rate in the coworker model.

MR. BARTON: I believe that's correct, John. I really think that Harry Chmelynski took the lead in compiling this.

DR. MAURO: We're going to move on, but somewhere along the line, he needs to confirm that as a fact -- not intake but excretion. I guess that is the question.

MR. CHMELYNSKI: Yes, these are excretion rates, John. This is Harry Chmelynski.

DR. MAURO: Thank you. Okay, got you. So, okay, what we're saying is the 50 percent -- it turns out -- let's put that -- 1953, Building One -- what we're saying here

is 32 urine samples were collected. See that thing in parentheses below the 181? And there were 13 workers.

So we're saying okay, well, we could pull data on 13 workers. We know there were 32 urine samples taken in that year from workers in that building. And it turns out the median excretion rate in the urine for those workers was 1.8 times higher than the excretion rate associated with your coworker model.

So we started to say, you know, are there places -- are there buildings and years -- where that subgroup had excretion rates, the medians, which are substantially higher than the ones in the coworker model? And the answer is, well, here are some. And we use substantially a factor of 1.5.

So any place where that ratio -- the number in that table is more than 150, we colored it. So you can start to get a feel where okay, it looks like in this building in

this year things were -- exposures were somewhat higher -- excretion rates were somewhat higher than what the coworker model would capture.

Stay with me. I'm not drawing any conclusions. Just giving a factual piece of information.

Paul?

MEMBER ZIEMER: Is it 181? Or 1.81?

DR. MAURO: It's 181 percent.

MEMBER ZIEMER: 181 percent, okay. I got you.

DR. MAURO: Harry, why did you do that?

(Laughter.)

MR. CHMELYNSKI: I hate decimal numbers.

DR. MAURO: It's 1.81, okay.

MEMBER ZIEMER: Got you.

DR. MAURO: All right. Now, all right, so what do we have here? It goes on

for several tables. All right --

DR. NETON: I had a question on that.

DR. MAURO: Okay.

DR. NETON: When you had quarterly data, '53 had only annual data. When you get down to the years where you had quarterly information, how did you compare the quarterly values to your annual values?

DR. MAURO: Harry, you rolled those up. Harry, please?

MR. CHMELYNSKI: Yes, this is compared to an average of the quarterlies in Table 2-1 of our report, which --

DR. NETON: So you took an average of the quarterly values and compared it to the median value of all --

DR. MAURO: The median -- yes, the average -- you've got median values and I guess you took that --

MR. CHMELYNSKI: Yes, the average median --

DR. MAURO: The average median.

MR. CHMELYNSKI: -- in the denominator.

DR. NETON: I'm not sure why that's a good comparison but --

DR. MAURO: Well, that's what we did. The point is to understand what we did. You know, we took the average of the medians when they are quarterly and compared it to the --

DR. NETON: Well, why wouldn't it be a better comparison to compare the quarterlies?

DR. MAURO: Well, we don't have quarterlies. We're not at that level of resolution here. In other words, when we grouped them by building, we could not go to quarterly. There just wasn't enough data.

And so we had to work --

DR. NETON: So you compared the average of the medians against the median of all the values?

DR. MAURO: As an indicator -- granted that there might be better ways of doing it --

DR. NETON: And I'm not sure how that works. Okay.

DR. MAURO: Think of it like this. This is an index of all their buildings and time periods where there is some indication that perhaps -- at least in those time periods in those buildings -- the excretion rates for the workers might be somewhat higher than what your coworker model would assign to them. That's all it is. An indicator.

DR. NETON: Yes, that's not surprising.

MR. ROLFES: Once again, we have to also keep in mind that there could be additional data in that individual's file for the next year or for the next quarter --

DR. MAURO: Right, yes.

MR. ROLFES: -- which would have to be considered.

DR. MAURO: We're getting there. We're getting there. One thing to keep in mind is that the threshold of comparison was set at 1.5, 150. You know, any threshold that you set like that is going to have some element or arbitrariness but, you know, it's a fairly high threshold. It wasn't like ten percent or 20 percent more.

So I think it will give you an approximate idea of where or which class there might be some issues in terms of comparing it to the median, rather than as some kind of absolute indications of a big problem.

It's designed to map out which class you might pay attention to, in terms of your coworker model, not being claimant-favorable.

DR. NETON: Okay. It's no great earth-shaking surprise that this heterogeneous population of workers, based on where Plant One was -- a uranium refinery. So you'd expect higher samples.

DR. MAURO: You see what we're doing is, we're collecting information and sorting them in a way that allows everyone to get a bird's eye view of what do we have. And let it speak to us. And let it tell us whether or not there is anything that is surprising? Is there a need to go further from here? Are we done? Or is there some sampling, some different kinds of things we could do?

But a lot -- in other words, there is a lot of information here that could start to lead you down a path of -- where do we go from here. We're not done, okay.

MR. MORRIS: Can I -- are you going to clarify for us -- what would randomness itself have done? Has there been 100 percent uniformity? No differences in any plant? We would have still gotten some --

DR. MAURO: You would expect half of them to be higher and half of them to be lower.

MR. MORRIS: Right.

DR. MAURO: No doubt. The idea being, though, are there any places where -- if there is any place where you are -- say, hmm, it looks like, for example, in 1956 in Plant No. 2, the median excretion rate was 2.5 times higher than what it would have been assigned to those workers in that --

MR. MORRIS: And is that statistically surprising? That's my question. How would you even judge if that would surprise you or not?

DR. MAURO: Well, I'm not making a judgment. I'm not trying to make a statistical statement at this point in the process. All I'm trying to do is start to identify pointers that might lead us in a direction that could be helpful to us in the end.

MR. MAKHIJANI: Let me give some perspective on what this paper is about, you know, in light of the kind of comment. This

paper is not the end result of having analyzed this coworker model according to a sampling plan.

These were simply exercises to present some idea of job types and plant placements of workers, to provide the working group with a framework for a sampling plan that we would carry out and what you might expect at the end of it.

So this isn't to be judged as some kind of conclusion that SC&A made about the validity of the coworker model or whether you can or cannot do those things.

It's simply a response to the working group's direction -- or at least what we understood to be the working group's direction -- as to whether they wanted to go there and have an analysis of this step.

DR. MAURO: Just to keep that in mind. So that's the purpose of this paper.

MR. ROLFES: Another clarification I just want to point out as well. Our

coworker model does not selectively choose what plant the individual worked in. We consider all data for that given year.

For example, for 1956, Plants 1, 3, 4, 5, 6, 7, 8, and 9 were all lower than the 50th percentile -- the excretion rates were all lower than the 50th percentile.

The only one that exceeded it was Plant 2. Our coworker model uses all plants. So we have much more data that indicate lower than 50th percentile excretion rates.

DR. MAURO: And in this table -- I mean that's what is useful about Attachment B. It shows you which years and what plants were less than 100.

DR. NETON: Let John finish. I mean, I think he's got a good point. Go ahead, John.

DR. MAURO: Okay. Now, one more time. Go to page 25. The last question we asked ourselves, you know, by now, what did we do? We started to get a sense for how

different it was in different buildings, as compared to the coworker model, which was a roll-up across buildings.

And we see that yes, it looks like in some years in some buildings the excretion rates, at least for that year and that building, might have been a factor of two higher, on that order.

And I'm not going to draw a conclusion but my inclination is --I'm not all that surprised, you know, given that year and that building, it's a factor two high. It's not a factor of 100 higher. It's a factor of two higher.

And here's where judgments comes in. You know that's one of the things I want to show you.

We did one more thing that was important. Go to page 25. It turns out we were able to go into the HIS-20 database and sample by job title. It turns out there are a lot of job titles.

But what we were able to do, you'll see on page 25, we were able to sort on the job titles. We have 26 job titles here where we have been able to pull data. And, for example, the millman, I'm not quite sure what a millman does --

DR. NETON: A mill operator?

DR. MAURO: -- a millman. Then there's a chem helper. The number one -- what we found out is that while we were able to get 133 samples -- and this crosses all buildings and it crosses all years -- remember we were not able to get a high level of resolution here, so we did what we could with the data that was there.

And we said well, if we go in and sample millmen in the database, we were able to get 133 samples. And we found out what the microgram per day excretion rate is: 110. So we now know, or at least we have an indicator of which categories of workers had the highest potential for exposure. And we're looking at

it in order, from high to low.

And that -- the work category called millman -- it turns out that excretion rate is well above, you know, any of the -- I think just about all of the default excretion rates, in terms of micrograms per day. I think there may be one number that's higher -- a few numbers. In other words, that's up here.

In other words, this 84th percentile -- if you look at the 84th percentile for the millman, then you look at the 84th percentile in your coworker data set or excretion rate, you find that that's pretty -- that's up there.

A good way to do it is to go back to the page that gives you, you know, the excretion rate upon which your coworker is based -- model is based. And we discuss it. The text talks about it.

And the one tab that is -- sort of up there. It's higher than most of the

excretion rates that you report at the 84th percentile in the different quarters, okay?

DR. NETON: Now again, you got to keep in mind that 84th percentile excretion rate has a default minimum of a GSD of 3.

DR. MAURO: Right.

DR. NETON: So if you calculate some GSD that's less than 3 and imputed at the 84th percentile, you're going to be low, from what we would use.

MR. MAKHIJANI: Actually the problem that John is describing with the reverse effect. That there are samples that are higher than your artificially high 84th percentile.

DR. MAURO: Right. So what do we have? I mean, we're done. What do we have? What we have here is, we've identified time periods and buildings and job categories where the excretion rates for those groups of workers were somewhat higher. In some cases a factor of two, maybe a factor of three

higher, than the corresponding time period in your coworker model. All right?

If we're going to design -- now here's where we get to the nub of the matter -- would it be productive to go in and say okay, let's randomly sample from the category called millman, a trend where we just go in and randomly pick workers, millman, chemical helper, painter.

Let's randomly go in and go back to the earlier tables where we had -- the ones with the shaded areas which showed which years -- let's randomly go in and pick some of those workers in whatever those years were that had more than a factor of two and randomly look at some of those.

Grab those workers. Let's reconstruct their doses using their data, using their data, and see what we come up with. Okay?

Now, what's going to happen when we're done? Some of them are going to be a

little bit higher and some of them are going to be a little bit lower than your coworker model would assign to them. You would expect that.

DR. NETON: Five percent of the time.

DR. MAURO: Yes.

DR. NETON: Well, randomly five percent of the people would be higher, right?

DR. MAURO: So now let's say it turns out that when you do that -- when you do that you find that your coworker -- this is the thought problem -- let's say it turns out in a large number of cases when we sample from those subpopulations, we come up with intake rates or doses -- let's say doses, lifetime doses, you know, his working life -- which are substantially higher, factors of three, four, five times higher than would have been assigned to that worker if it turns out he wasn't bioassayed.

But he was, of course. But if he

wasn't. Now what do we do with that information? Does that mean your coworker model is not protective enough? In other words, biased by using the full distribution.

If this guy turned out to be a person that didn't have any data and you were to use the coworker model on him, you would underestimate his dose by this factor.

Now, you could argue and say, but no, he does have the data, and we wouldn't do that. Then the question becomes, well, is it possible there might be some millmen -- and is it possible there might be some workers -- that worked in that time period that don't have bioassay data, where you would have to do this.

And in those cases, you would underestimate that person's dose. This is where -- this is the question that I put before the work group -- whether or not it is worth going through that exercise.

I can't see -- now the only other

thing we can do, other than that kind of sampling plan and see what it tells us when we're done, is the kind of thing you just described. You know, when you're done, you know it's really not going to tell you very much.

What you're saying we should do is, no, let's go find those workers that have no data. And let's see what kind of job they had. Is it possible that some of them worked in this building, too, in that year -- or some of the millmen and we don't have any bioassay data. That might be a more informative piece of work.

DR. NETON: Certainly a lot more efficient.

DR. MAURO: And a lot more efficient. So what I'm trying to do is the best I can to present to the work group options. Where would you like to go from here, given this information?

I think everyone understands what

was done and what we have.

DR. NETON: I just want to say a couple things before the work group deliberates is -- I can guarantee you that you can go and find dose reconstructions to be done for millmen that have high bioassays that are much higher than this because we have their data. I think that that's probably true that we have most of the data.

This is not one of these examples that SC&A likes to point to, I think, of cohort badging or cohort sampling. I think they really did sample the people with the highest potentials for exposures throughout the plant. I think there is a lot of good evidence.

Given that, did they miss anybody? We don't think they really did. So then, like you said, you go back and look at the five or seven percent of the people that have zero bioassay data and try to tie those job titles with --

DR. MAURO: Job categories.

DR. NETON: -- or time periods or whatever and see, if NIOSH reconstructed those doses with the application of the coworker model as we proposed, it potentially underestimates exposure.

DR. MAURO: That would be a judgment call. Because you'd have to look -- he worked in that building and he had his job category, right off the bat, you would -- see, I would say that you'd have no choice but to use the coworker model. And the evidence is, for that category and in that time period, that's going to underestimate -- you know, that's not going to be a good model.

DR. NETON: Right. But what I'm saying is without knowledge that that has actually happened, you know, there's a lot of extra work going on here to pull out and parse out mill operators and chemical operators and say yes, those had higher exposures than the 50th percentile of distribution.

And I'd say yes, we know. We acknowledge that. I mean that's a given in this model. And then using the 50th percentile, you have to look at the people to which we applied the coworker models. This is will come up in that 50th percentile discussion that we have yet to have, this technical call.

Which class of workers do we apply the 50th percentile with the full distribution, not just the 50th percentile? And those workers are picked for that distribution based on a review of the characteristics of their exposures.

Oftentimes there are people -- who may have been clerks who had visited the area, walked around and did some inventories. There may have been security guards who did some night walk around. That sort of thing.

I would be amazed if we would take a chemical operator who worked six years at Fernald in a very active timeframe and give

him a 50th percentile.

DR. MAURO: Right.

DR. NETON: I can't believe we would do that.

DR. MAURO: This is what I was told --

DR. NETON: And it is quite possible --

DR. MAURO: -- was the answer. To me, if I was sitting on the other side of the table, I would say if I do find some workers that have no bioassay data but they are millworkers, or they worked in this year in that building -- where I know that something is different there than my coworker model -- I sure as heck wouldn't give them the full distribution. I may give them the 95th percentile.

DR. NETON: Exactly. And I think we do that in a judicious characterization there. But the issue is, you know, it's possible -- I mean we believe that the highest

exposed workers were monitored. But we vow it is possible that records could get lost. I mean it's possible we could get a record from a guy that says chemical operator, never been monitored.

DR. MAURO: Well, that would certainly raise a flag in our reconstruction.

DR. NETON: I'm sorry, Mark, I cut you off.

MEMBER GRIFFON: Oh, no, I was just going to ask can I -- can we -- I mean I think that that makes a little more sense actually. But the question I have is -- and I think Mark alluded to this -- how many claims to you have --

DR. MAURO: Right.

MEMBER GRIFFON: -- with no data. And then if you know that, you must be able to pull those out.

MR. ROLFES: Right, yes, you could certainly do an easy query enough. Just enter NIOSH OCAS claims tracking system --

MEMBER GRIFFON: And it shows those --

MR. ROLFES: -- which I did.

MEMBER GRIFFON: Oh, okay.

MR. ROLFES: Because John had cited the lung cancers, I queried by cancer type and whether or not the claim was above or below 50 percent probability of causation.

By doing that search, I got 16 claims that had the lung cancer case that was less than 50 percent probability of causation in dose reconstruction.

Furthermore, I went through and looked at job categories and whether or not there were bioassay or any monitoring data. I also looked at the data diagnosis. because the latency can play a large part, as we discussed.

In looking at that, there's potentially eight individuals that had less than 50 percentile -- or less than 50 percent probability of causation that had a lung

cancer where a coworker intake model could apply.

And if you look at some of the job categories and employment durations, some of the individuals were on-site for days, a month. If you look at the job categories, there are absolutely no chemical operators, no millmen --

MEMBER GRIFFON: I guess that was my -- that sort of gets to my question. But I'm asking all cases here. But is that -- it seems like that is cumbersome. You had to go to the raw records, right, and look? Or do you -- you can't really query NOCTS, can you?

MR. ROLFES: Well, what you would have to do --

MEMBER GRIFFON: To find out which claimants have no bioassay data, you have to go through them one by one, right?

MR. ROLFES: What you would have to do is query NOCTS for the cases that hit your requirements. If you're looking for, you

know, for example, lung cancer cases --

MEMBER GRIFFON: No, I'm looking for all cases.

MR. ROLFES: Okay. All cases, we have --

MEMBER GRIFFON: All claims where they have no bioassay.

MR. ROLFES: -- we have 1,040 claims total for Fernald. Of those 1,040, we've completed 958 dose reconstructions already. So we've completed greater than 90 percent of the dose reconstructions.

Of those dose reconstructions completed, 40.4 percent have had a probability of causation greater than 50 percent. So we're quickly limiting the number of -- we've got about 571 claims that have less than 50 percent probability of causation. And we've got 16 that are active in dose reconstruction right now.

So if you were going to query NOCTS, you would really only want to query say

571 -- say 600 claims that have less than 50 percent probability of causation.

MEMBER ZIEMER: Can you query for whether or not they had bioassay data?

MEMBER GRIFFON: That's what I was asking.

MR. ROLFES: In NOCTS, what you would have to do is query those 600 cases and then go through them one by one as I did with these --

DR. NETON: I think that might be able to be automated more than that, because I know for every SEC evaluation report, we always provide a table of the number of workers with bioassay. And I don't think we go and hand-count those. I think there is a way.

MR. ROLFES: Right. It could be possible for ORAU --

MEMBER GRIFFON: Because I don't disagree with Jim's point. If we can find those claims, then you look at the job types

in there. And then you go back to this kind of system that John is talking about.

MR. ROLFES: It might be possible because --

MEMBER GRIFFON: If you see a millman in there, then it raises a question. If you see these other jobs, then we have to make an assessment on if your coworker model --

DR. NETON: And it is quite possible that in some of those cases, we wouldn't even use coworker model. We could use the efficiency process and if it's not a lung cancer -- and it's, say, a prostate or something -- we could use some very large, overestimated dose that is not even required to get into the coworker arena.

MEMBER GRIFFON: I'm just asking just to figure out over the history sort of, who didn't they bioassay? Who didn't have bioassay? Because I don't care about POC at all in this. I just want to know who didn't

have records? Who had records? And then what types of jobs are in those ones that didn't have records?

DR. NETON: Yes, I agree.

MEMBER GRIFFON: And then we can say all right if there's no -- and I expect you are right, Jim, there's no chem operators, there's no, you know -- they did have -- yes, they have them -- and if we find that out, I'd like to see a list of like what job types fall under that category of didn't have any records over their whole course of their being at Fernald.

MR. ROLFES: That may be something that is already created. Our dose reconstructors at ORAU -- for every claim that they receive -- they do take all of the data that is received from the Department of Energy, both internal and exposure information, and populate that into a spreadsheet for each individual claim.

I don't know if it has, you know,

the individual's job title because I'd have to take a look at that. But it may be possible for them to quickly -- they may already have something. I don't know.

DR. MAURO: Well, I mean right now, Harry, when you sorted on millmen and you went in, you know, I guess every one that you sorted, by definition, the ones that you were sorting, did that mean that they had to have bioassay data? Or are there some millmen that had no bioassay data?

Is there any way -- in other words, when you went into HIS-20, does the fact that you could sort on -- or wherever -- where you went in -- I know you worked with multiple data sets. Is it possible for you to go in to see -- are there any millmen that have no bioassay data? Is that something that is trackable?

MR. CHMELYNSKI: As far as I know, what you are asking is concerning people who are not in HIS-20.

DR. MAURO: Well, I guess that is my question.

MR. CHMELYNSKI: Yes, they wouldn't be in HIS-20.

DR. MAURO: They wouldn't be there. That's why I asked the question. They wouldn't be there, okay. Thank you.

DR. NETON: I think we could go back and look at the database in some way automated -- in an automated fashion and pull out --

MEMBER GRIFFON: You mean the NOCTS database?

DR. NETON: The NOCTS database. And it actually may be outside of NOCTS.

My recollection is that ORAU is coding all the bioassay data. There is a reason. We asked for them to do that early on for future reference because we're developing this huge amount of exposure information. And I was concerned we would lose all that data. So I believe it has been coded into

spreadsheets as Mark suggested.

MEMBER GRIFFON: I do recall seeing that for individual claim data.

DR. NETON: It might not be that difficult to pull out the cases that don't have bioassay. And if it is -- as we suspect or believe -- it's a few in number, let's say 1,000 cases, if it's 15, maybe 100, it wouldn't be that onerous to go back and look at those one by one and pull out the job titles.

I have some concern about job titles because -- as we've seen at other sites -- they don't always correlate in stepwise fashion with what the person is doing. Oftentimes, human resources is lax in changing things.

But it would certainly give us an idea.

DR. MAURO: Well, there are lots. They're not just here.

DR. NETON: And they are not

uniform either.

DR. MAURO: We know, for example, in 1957, 2.4 percent of the 4,000 workers did not have any bioassay data. So it doesn't --

MEMBER ZIEMER: But only for that year.

DR. MAURO: Exactly, only for that year.

DR. NETON: And that's another part of the issue. But, again, I would also question in some ways -- were all the workers who were listed as working in Plant 1 really working Plant 1 in that year -- because we know that human resources can kind of lag behind. And if it is a matter of the supervisor saying, this guy is on loan over at Plant 5 -- I'm not saying it's wrong. I'm just saying that there is some opportunities for disconnects there.

MS. BALDRIDGE: I have a question.

MR. KATZ: Hello. Who is this speaking? Sandra?

MS. BALDRIDGE: Yes. You know most workers, you're talking about the bioassay samples, but that only demonstrated a brief window. If they were -- had four pieces of data for the year, that's only four brief windows out of, you know, an entire period of time.

Were there any correlation made as to whether those samples represent the exposures during the high or low emission periods based on the MAC levels that are presented in the historical plant documents?

DR. NETON: Okay, Bonnie? Is it Bonnie?

MR. KATZ: Sandra.

DR. NETON: Okay, Sandra. I'm thinking of my other working group. Sandra, this is Jim Neton. I think we might have talked about this before.

The way we use bioassay data is if a person had a sample today that has X amount of uranium in it, we would actually do a

calculation to determine what's the maximum amount they could have had since their last sample and still be excreting that amount in their urine today.

And we would assume that that exposure occurred during the entire duration between the last sample and the current sample. In other words, it's kind of a bounding estimate that we would use as a chronic exposure estimate.

MS. BALDRIDGE: But there are periods of time between those samples that could have occurred with these high MACs --

DR. NETON: Right.

MS. BALDRIDGE: -- if they were not -- if their sample was not given at the appropriate time --

DR. NETON: Well, the uranium --

MS. BALDRIDGE: -- based on the exposure.

DR. NETON: -- the uranium has the property of being excreted over a long period

of time. And we know how that excretion behaves. And we can model that and do a very reasonable prediction of what that intake -- what the maximum intake could have been in a person only excreting a certain amount on the day they were sampled.

MS. BALDRIDGE: And we get back to the excretion --

DR. NETON: Right.

MS. BALDRIDGE: -- issue --

DR. NETON: Yes.

MS. BALDRIDGE: -- which I've brought up before. You know if you don't know who had renal damage, you can't know that their excretion rate was 100 percent.

DR. NETON: Right. At the levels we're discussing here, at least on the model that we're talking about, these were not sufficiently high to cause renal damage at least in our opinion.

MS. BALDRIDGE: But all the workers who possibly had renal damage have not

been identified to know whose records represent the 100 percent excretion and whose records potentially show lesser levels of excretion.

MR. ROLFES: I think we did discuss this, Sandra. This is Mark. And I believe we did discuss that. And I believe Hans Behling had prepared a white paper and cited a few references as well.

And I believe we did discuss that in pretty much detail. And I think we came to resolution on that issue.

DR. MAURO: Yes. And I read the transcripts last night. We spent quite a bit of time reviewing the literature on that, reviewing autopsy data. And the outcome of that was that this issue has been put to bed. That it is not going to affect the ability to reconstruct these doses.

CHAIR CLAWSON: I've got a question, Jim, you're saying that the uranium stays in your body and is excreted. How long

is safe?

DR. NETON: Well, it depends on -- if you inhale it, it depends on how soluble it is in your lung. And the way we work it is we would pick the most claimant-favorable solubility class.

For example, if it is in your lung and we're trying to irradiate the lung, we're going to assume it stayed there for a very long time to radiate the lung and give you the most dose.

If it is a systemic organ like a kidney or a liver, we often times would assume that it would just leave the lung, concentrate in the kidney, and deliver that dose. So the amount of time it stays is dependent upon the type of material.

CHAIR CLAWSON: Well, if you had it in '57, if you had a urine sample in '57, a small amount of uranium, would you still see it in '58 if you hadn't had any bioassay?

DR. NETON: Well, there's a --

maybe. It might be below the detection limit. And that's another concept that we use.

We would take the detection limit of the system and say well, we don't know what it was. It could be below that but we'll assume that it is equal to the detection limit. Or half the detection limit, I've forgotten how we exactly modeled it. But we'll acknowledge that you can't see zero.

And so we'll say well, we don't know what it was but it certainly --

DR. MAURO: Wasn't more than this.

DR. NETON: -- it is not more than this value, this bounding value that we would use based on the detection limit sampling technique that was used.

There's a pretty sort of standard health physics type of calculations. There's nothing exotic that NIOSH has invented here. This is a --

MR. ROLFES: Even for a sample that's collected, you know, this is a little

elaborate -- even a sample that's collected say 50 years after an intake potentially occurred, I mean this is pushing it but if you have an intake -- you know, back in 1950 and you have a bioassay sample that's collected out here in year 2000, for example, it's pushing it and it's going to be highly uncertain but this can be indicative of an exposure that was incurred 50 years ago.

And what we would do, we would interpret this result -- and you can get a huge intake, you know, going back here -- the more data you have, the better you are able to refine that.

MEMBER GRIFFON: Would you actually do that?

DR. NETON: It would be more of a chronic --

MEMBER GRIFFON: A chronic, right, yes. I'm not sure that you would always -- if you have them one day apart, would you tend to --

DR. NETON: I think if it was a chemical operator, we would.

MEMBER GRIFFON: You would? Yes?

DR. NETON: It it was a chemical operator, we would probably do that --

MEMBER GRIFFON: Because in that case, you're going to be over your coworker model, a lot over your coworker model.

DR. NETON: Right. But see if it was a chemical operator or a mill operator, we would do that. If it were a secretary and there was a determination bioassay sample, the only sample we had, we either would use a coworker or maybe even the ambient environmental depending on how we could bracket their work environment.

MEMBER GRIFFON: So it depends.

MR. ROLFES: You would have to consider the facts in each individual claim, on a case-by-case basis.

MEMBER GRIFFON: Can we take a break?

MEMBER ZIEMER: I was just going to say I don't think that your results here are surprising there, John, I think it is what you would expect in terms of comparing it with coworker model and you've identified some areas where possibly there could be gaps, although maybe unlikely.

But it seems to me that what NIOSH has suggested makes sense. Due to the small number of un-sampled people, to go back and characterize that.

And if there are, for example, mill workers, and it's hard to imagine that they would work there for years and have no bioassay but, as you say, maybe records would get lost, but even if you had a case like that, you would handle it differently, would you not anyway?

DR. NETON: Yes, I would, definitely.

MEMBER ZIEMER: But in any event, I think it is probably worth looking at the

dataset from that point of view. It seems to be more efficient --

DR. MAURO: Yes,

MEMBER ZIEMER: -- to go back and characterize it and say are there really gaps there.

DR. MAURO: I wish I'd thought of that, yes.

MEMBER ZIEMER: Well, and this is helpful to point out that the possibility exists. And in a different situation, might have been very different. But this is a pretty robust dataset to start with.

DR. NETON: If you recall, there's a TIB, and I can't remember the number, way back when that we tried to delineate the type of job categories where the exposure may have been more administrative, almost non, intermittent, and then regular. And I'm pretty sure in that regular exposure category would be chemical operators, mill operators, that sort of thing.

So that would tip off the dose reconstructor to say well, this guy is in a higher exposure group. And to give him the 50th percentile and the full distribution would not not make very good sense.

But nonetheless, I think we'll be more than happy to go back and pull out --

MEMBER GRIFFON: That's what I was going to say. I was going to suggest a break and come back with an action. But I'll just throw it out. I was going to talk to you on the sideline and see what makes sense.

But I mean my idea from this would be for NIOSH to have an action of finding -- and I wasn't sure, like John, maybe initially I wasn't sure if it was too onerous to go back and find the cases with no data.

But if it is, you know, Jim seems to think that it can be done so --

DR. NETON: Yes, Jim did it to us again.

(Laughter.)

MEMBER GRIFFON: So NIOSH can find the cases with no bioassay data, the claims with no bioassay data across the Board. I'm not saying less than 50, higher -- you know, regardless of POC. I would say look at all the claims and see who has no bioassay data.

Even if you used an efficiency method on it, I don't think that matters for right now.

DR. NETON: Let's try to quantify --

MEMBER GRIFFON: Yes, we want to look and see the analysis. And then to the extent you can, determine jobs and buildings, question mark. I had a question on the building thing because of what you were saying. But what you can find out from that, yes.

MR. ROLFES: I don't believe that data would typically be entered into a spreadsheet. And, you know, as I mentioned before, we wouldn't selectively assign intakes

based on the plant. It would be an entire year, we would consider all plants, all intakes.

MEMBER GRIFFON: No, I understand that. But for what we're looking at, we might want to look at that if it was available. I'm not sure it would be.

DR. NETON: And, you know, this may be thinking down the line a bit but once we identify those and get some rudimentary job category information, we might be able to match that against the HIS-20 information because obviously SC&A was successful in pulling out -- well, we pulled out buildings -- and SSNs.

So, you know, there might be some ability to cross match these claims.

MEMBER GRIFFON: Mark, the reason I raised that is just what you -- and I think it is pretty unlikely. But if you go through this and you find 50 people with no data, and they all worked in Plant 2, you just said

earlier that Plant 2 tended to be higher, you know. So that would be sort of telling. I mean that would be a concern.

MR. ROLFES: Another interesting thing, since we're mentioning Plant 2 and it appears that there are some years that there are higher excretion rates in Plant 2, keep in mind that many of the employees in Plant 2 also worked in 3 because they were, in fact, one plant -- two separate sides of the same plant essentially, the same building.

MEMBER GRIFFON: But then I would do -- the follow-up action would be for SC&A to evaluate those people against the coworker model. In other words, is the coworker approach bounding? And there's some -- I think there's some -- well, I mean I think it depends on what you find with jobs and stuff how that analysis is going to go.

But some assessment of that outcome, I guess, you know, so if you see, you know, I think this gets a bit subjective maybe

but because you are going to have jobs, and you are going to have to say likely based on our knowledge of the site, these -- the coworker model would be bounding. That's a little subjective maybe. But I'm not sure how that analysis goes.

But I think the first step is to get this -- I think that makes more sense to me anyway. I don't know what other members --

MR. MAKHIJANI: One thing that we might want to hear from Bob or Harry, to my memory -- I didn't do the pulling of the data, Bob and Harry did -- but I think the plant data are only available through 1961.

Bob? Harry? Bob?

MR. BARTON: Yes, Arjun, this is Bob Barton. The plant data -- it seemed to be a practice to label the bioassay sample with plant number up until about 2/1961. The problem with, you know, searching NOCTS is to get, you know, a subset of claims with no bioassay data, we have no idea what plant they

worked in because they don't have any bioassay data. So it is kind of a Catch-22.

MEMBER GRIFFON: Okay. So we may not be able to get a plant, yes, yes. But at least we can get the jobs.

MEMBER ZIEMER: And that table only went through '69 anyway.

DR. MAURO: Yes, that's all we can do.

DR. NETON: Well, and remember, we have the CATI -- you know, if it's true, there's a small number of samples on the CATI and we know which buildings did you work in and we go through and develop an exposure -- not exposure but a history, job history.

I don't know if I'm signing up NIOSH for way too much work.

MEMBER GRIFFON: It's probably the case. If it's a small number, then it might be --

MR. ROLFES: There's plenty of actions that we've already fulfilled. And I

believe we've responded with all the things that we've been previously tasked to do, you know all of the things that have been asked of NIOSH to investigate and evaluate.

I believe we've fulfilled all those requirements. We've even, you know, even within the past month, I believe, we've done a pretty good job in keeping up with all the new white papers that have been sent over by SC&A as well.

I don't believe we've issued formal responses on all of them but we have prepared responses for those. And are prepared to discuss those.

I do want to mention once again that this evaluation report has been with the Board since October 25th of 2006. So we're in -- out past two years now.

CHAIR CLAWSON: Gee, that's new news. We understand that, you know, it's real difficult -- you know it's interesting. I sit here and I listen to -- we can do a lot of

bounding numbers over here and we can twist them around here. We can do that.

But one thing, Mark, I want you always to remember is you've got to look at what the outside people -- the claimants that are looking at this. And a lot of them are under-educated, just like me. And that is that we are getting the best product that we can out to them.

NIOSH has done a wonderful job. I think they really work hard at taking care of our issues and so forth like that. And I'm the first one to apologize about the two-year time frame. But it's something that we're trying to get best products.

MEMBER GRIFFON: Yes, we want to get it right.

MR. ROLFES: I completely agree. I just wanted to point that out because I do, in fact, speak with people and explain this, you know. What's going on? What's the new issue that's coming up?

And I do honestly speak with people and have to inform people of what the current things that are being discussed, you know. Questions have come up from claimants. Why are they discussing this again? Didn't they resolve that at the previous meeting?

So, you know, I'm trying to be honest with all the claimants that I speak with. And I want to make sure that we're doing our best job that we can to get them a timely answer.

So, if we could take a ten-minute break?

MR. MORRIS: What will be on the agenda when we reconvene?

CHAIR CLAWSON: Recycled uranium.

MEMBER GRIFFON: No, no. I don't know if we want to skip over -- while we're on this topic, I would propose we talk about the data completeness and validity. And just see where we stand.

I know that NIOSH gave a report.

It seems to be all wrapped together. Let's, if we can -- can we finish that conversation? And then move on to the recycled -- that is what I would propose.

CHAIR CLAWSON: Yes, we've got to finish this one up. But the next thing that is going to come up is recycled uranium after we get this finished.

MR. KATZ: Okay. So everyone on the telephone, we're going to mute the phone for ten minutes. It's about 20 past 11. So at about 11:30, we'll get back going again.

(Whereupon, the foregoing matter went off the record at 11:20 a.m. and resumed at 11:38 a.m.)

MR. KATZ: This is the Advisory Board of Radiation Worker Health. It is the Fernald Working Group. And we have been on a short break. And we are reconvening now.

CHAIR CLAWSON: We appreciate John's report and Jim's and Mark's comments.

We need to come to closure on

this. And before we can do that, Mark's got some issues he wanted to go over. So I'll turn it over to you.

MEMBER GRIFFON: Well, I guess on that topic, I mean my proposal for the actions, that's what I would go with, I guess -- do we have agreement on the action that NIOSH is going to follow up on -- identify the cases with no bioassay data?

CHAIR CLAWSON: On the NOCTS system?

MEMBER GRIFFON: Yes.

CHAIR CLAWSON: Okay.

MEMBER GRIFFON: Yes, go back to that. And then, you know, the follow up would be for SC&A to look at those -- most likely we're going to have job information, probably not building information, but whatever we have and --

MEMBER ZIEMER: I thought NIOSH is going to follow up on this. Who is going to follow up?

MEMBER GRIFFON: NIOSH is going to follow up. And then subsequent to that they are going to produce what I would expect is sort of this listing --

MEMBER ZIEMER: Oh, okay.

MEMBER GRIFFON: -- and hopefully not that big a number of people and what their jobs were. And then SC&A is got to then look at that and make some assessment of whether the coworker model would be a bounding approach for those workers. That's the next step.

And then maybe, you know -- I'm not sure what we're going to get so there may be some subjectiveness to that assessment. But anyway, that's the sort of the two-step process in my mind anyway.

DR. MAURO: Just to clarify that a little bit more.

MEMBER GRIFFON: Yes.

DR. MAURO: Let's say we do find some categories of workers, millmen, that have

no bioassay data which brings us to the end of the story. If you don't find any categories of workers that fall in those categories that I had listed, those 26, let's say they all have bioassay data, is that the end of the story? Basically we couldn't find any? I mean that may be the outcome of your investigation. I don't know.

DR. NETON: Well, I think it is incumbent upon us maybe to discuss how we would -- how the application of the coworker model would bound the categories that we're looking at.

DR. MAURO: Okay.

DR. NETON: Yes.

DR. MAURO: Because it could be kind of lengthy but, you know, yes.

DR. NETON: Is the coworker model appropriate for the people who were using it? I mean that's the bottom line.

MEMBER GRIFFON: That's the bottom line. And then SC&A can review that report

and that product.

MEMBER ZIEMER: Because you could have future claims, I suppose.

DR. NETON: Yes, exactly.

DR. MAURO: As an SEC issue, okay, if you do run across a person that had a job category that could be a concern and there's no bioassay data, would the solution be pick it off and use the 95th percentile or some other parameter? In other words, it becomes a -- what I'm getting at is do we have tractable route? If we do run into that, is it tractable?

And if it is, is it an SEC issue? I mean I know I'm pushing everyone but taking this to its logical conclusion, even if you do run into some cases where gee, this guy didn't have any bioassay data and he had a pretty serious job, what does that do to your ability to reconstruct doses?

MR. ROLFES: Let's also consider how is identifying a case where we have a

claim that we've completed a dose reconstruction for that had a probability of causation of greater than 50 percent, how would identifying whether or not that case had bioassay data, you know, be of benefit to us? Or to that claim?

MEMBER GRIFFON: Well, we're looking at this as a sample that's theoretically representative of the overall population of potential claimants. I know that's the way I'm looking at it.

DR. NETON: I could see that logic.

MR. ROLFES: Okay. I'm just trying to, you know, make sure that we're doing the appropriate work rather than doing a large effort if we don't need to fully do that.

MEMBER GRIFFON: We don't want that.

MR. ROLFES: I mean I don't want to waste, you know, time if it's not going to

be helpful, you know.

DR. NETON: I think the answer to John's question, though, I think is given that we have somewhere in the vicinity of 400,000 uranium measurements on workers over a very long period of time, I believe that there is something we can do for any worker who doesn't have bioassay data.

I mean there's enough monitoring data for enough subpopulations out there that NIOSH could develop an approach regardless of what was missed.

MEMBER GRIFFON: But I think the other thing, from my standpoint anyway, I won't speak for the work group, but, you know, if you look -- you find say 50 cases and you find jobs that I would expect to have some monitoring data, then it raises the question of the completeness of the -- you know.

So, you know, likely -- I mean -- I think, John, what you are likely to find is, you know, maybe NIOSH will come back and say

we found these 50 people and most of them, by job types, we believe they are fully covered by the 50th percentile. There were these two that seemed to have jobs in the chemical operations areas, something like that. We don't know how they got missed over the years. But we would assign the 95th to them. That would be their proposal.

And to me, that would probably be, I would come back and say that's reasonable, you know. If they came back with 50 out of 50 that ended up in the high category, I'd say wait a second. Something is wrong here.

Why were all these people missed over the years? You've got so many samples. Why were all these people missed?

MR. ROLFES: Another clarification that I would like to ask is that the number of workers that we have, the 10,040, many of those claimants are also outside of the current SEC period that was evaluated.

So if we're concerned about a

special exposure cohort perspective versus a dose reconstruction perspective, do we want to include the population of employees that worked that site from 1990 through 2007, you know, 2008? Do we only want to consider this as an SEC issue?

MEMBER GRIFFON: That's a valid point. I mean yes.

MR. ROLFES: I mean I don't want to do something, you know --

MEMBER GRIFFON: Right, you're right, after '89, some people were legitimately taken off. So, you know, things changed again.

MR. ROLFES: I don't want to, you know, do a large analysis so that isn't going to be helpful for answering the question that we've been asked to, you know, to --

MEMBER GRIFFON: If the petition only went up through '89, then yes.

MR. MAKHIJANI: We -- Bob and Harry, correct me if I'm wrong -- I think we

only looked until 1989 because of the SEC limitation. And I think these particular job -- Harry, do these particular job categories only go to '89 because after '89, the jobs were different anyway. The decommissioning and all that. You wouldn't have chemical operator -- you wouldn't have all these jobs.

MEMBER GRIFFON: Ray has that, yes.

MR. BARTON: If I could just add a little clarification to job title, you're right. They did change tremendously. However, in the remediation years, they did recreate the chemical operations folks under this HAZWOPER, you know, titles.

But like the maintenance functions basically stayed the same. And, you know, remediating the buildings and tear-down and what have you. But chemical operations did change immensely but they did bring them back.

MEMBER GRIFFON: Yes, I mean my opinion would be we should stop this at '89 if

that's easy to do. I mean obviously if -- well, John, I think if you add people that started before '89 and worked through --

DR. MAURO: You would catch them.

MEMBER GRIFFON: -- you're going to catch them anyway.

MR. MAKHIJANI: If there are no samples up to '89, then they would be -- well, that's why there are no samples.

DR. MAURO: But then that might be a problem.

MR. ROLFES: Keep in mind, though, if we have bioassay data for that individual in 1990, that would be sufficient in my mind --

MEMBER GRIFFON: Well, that's what I was saying -- that's what I was trying to grapple with. So you might end up -- yes --

MR. ROLFES: I'm just making sure we put these things on the table so that we do what we're being asked to do and making sure that we're, you know, doing it as efficiently

as possible.

MEMBER GRIFFON: Yes, I guess our focus would be the SEC period obviously. But if you -- how you present it for each person, you might want to think through that.

DR. NETON: Yes, we will think about it and make we do it in a rational manner.

CHAIR CLAWSON: I guess I'm looking at what kind of --

MEMBER GRIFFON: That was the action, I think, right?

CHAIR CLAWSON: Up to '89 but --

DR. NETON: At a minimum '89. We may actually do a little more if it looks like --

CHAIR CLAWSON: Eliminate carryover.

DR. NETON: -- carryover. But certainly the SEC period we will evaluate. It really comes down to can we reconstruct their dose. And if there is something in 1990

that's useful, we won't cut it short.

MR. ROLFES: Right. There could be people that are beginning employment in '89, you know, may have worked, you know, a few months in training, et cetera, prior to going in for decontamination.

DR. NETON: Okay. That would be a good idea. I just want to mention to John, this is a good start on the technical call that we're going to have on this 50th percentile issue. And these are exactly the kind of --

DR. MAURO: The conversion issue that I intend to --

DR. NETON: This is OTIB.

DR. MAURO: The OTIB where we use the 50th percentile, full distribution. That's part of the procedures working group.

DR. NETON: Yes, and it is a very similar issue. And a good start for that conversion.

MEMBER GRIFFON: Now we have

technical calls in the day of our group meetings.

CHAIR CLAWSON: So we're clear on what the --

MEMBER GRIFFON: Yes. The action for that one, yes.

CHAIR CLAWSON: Okay.

DR. NETON: I can't give you a completion date right now.

CHAIR CLAWSON: I do have one question. Does this sampling plan coming in and so forth like, you guys already came up with the coworker data, the coworker model?

DR. NETON: That was developed in 2007.

CHAIR CLAWSON: Okay. I just wanted to make sure. Okay. It just seemed like all of a sudden I'm trying to stay on focus of where this -- how the sampling plan evolved.

DR. NETON: The coworker model surfaced and then --

CHAIR CLAWSON: Okay.

MEMBER GRIFFON: Well, the other items I had, just to continue from before break, was the question on the validity of the data. And this goes back to the -- and, you know, this has been raised by the petition but, I mean, it's actually part of our Advisory Board procedure now to consider the validity of data.

So when you are developing coworker models, you're using HIS-20 data. For years, since there are some new faces around the table, for years workers at the DOE facilities have been concerned that, you know, this database stuff, we don't trust it. We don't believe it.

So I've seen, as part of my mission on the Board from year one, you know, to sort of go back and test that. And ask NIOSH to test that. And SC&A to review that.

And this means going back to raw data -- you know, as primary data as you can

find. A lot of times it is uranalysis logbooks, whatever. And I know that we have a report from NIOSH on that for the HIS-20.

DR. MAURO: Correct.

MEMBER GRIFFON: I don't think we ever tasked -- and I was talking to John on the way in here but I don't know that we specifically tasked SC&A with reviewing that. And, you know, I know we discussed it at the last work group meeting.

But I don't think we ever tasked them and said look through the details of that and give us a report back as to whether you, you know -- so, Mark, just to understand, I was looking at -- and it's actually -- it's on the O: Drive, the millspec report is on there.

And actually I think in each tab in the Excel spreadsheet there's a reference ID that gives the document, the logbook, or the urine cards, or whatever they were. I think -- I looked at it quickly just here.

So I think everything should be

there that SC&A would need to look through it, right?

MR. ROLFES: I'm taking a look.

MEMBER GRIFFON: I don't think the log -- I don't think the urine logs were posted but I think you referenced them so they can find them in the --

MR. ROLFES: Oh, if it's not there, we can find ours --

MEMBER GRIFFON: Yes. But I mean I think --

MR. ROLFES: -- and get it there.

MEMBER GRIFFON: -- you can find them through the cite research database.

MR. ROLFES: I believe those were, in fact, put out on the O: Drive. But it's been more than a year that they've been out there.

MEMBER GRIFFON: At any rate, they are either well -- I know they are well referenced because I just looked at them -- or they're on the O: Drive under the A/B document

review section is where I'm talking about, yes.

MR. ROLFES: Correct.

MEMBER GRIFFON: So I mean my -- I think that we need to task SC&A with reviewing that report and close that out. You know we haven't -- I thought we did but at any rate, John, you haven't done it yet.

DR. MAURO: No, we haven't done it, either way.

MEMBER GRIFFON: So either way, I think we need to task that if people are in agreement with that.

MR. MORRIS: Another detail you may want to know about is the issue that the coworker study that we've just discussed is now in the process of being turned into an OTIB. So the substance will not change. It will just be a format to make it a formal document.

MEMBER GRIFFON: Okay.

MR. MORRIS: And I think you've

already invested your review time there. So it may be -- may or may not be worth trying to assign that. But it won't be long before that comes out as a formal document.

MEMBER GRIFFON: Okay.

CHAIR CLAWSON: Which white paper was this one?

MR. MORRIS: The recycled -- no, excuse me -- the Coworker Study for Uranium Urine, the topic of the morning.

MEMBER GRIFFON: So that would go back to sort of our last action as the coworker review and the coworker model but if it is going to be official now, yes, it's the same thing, the same model.

MEMBER ZIEMER: I'd like to ask for clarity, John, when your group does this, you review the report. But what do you do in terms of validation? Are you going back and subsampling?

DR. MAURO: Yes. What we would do is we'd go into the hard copy, you know,

scanned data that is the source material for HIS-20. And basically what I'm hearing is were the data captured faithfully? And going from whatever the scanned hard copy logbooks, whatever form they were, faithfully transcribed.

MEMBER ZIEMER: Yes, I understand that. I understand that. I'm asking, in a sense, to what extent -- you're obviously not going to do 100 percent sampling. And do you guys develop the protocol or do you have an established protocol for how you do that?

DR. MAURO: The normal procedure would be I talk to Harry and say Harry, here's the arena. And we need to submit a statistical statement regarding the transcription.

MEMBER ZIEMER: Right. I'm trying to get a feel for the extent of the task here. What would be a comparable -- this is a really robust database to start with.

DR. MAURO: Yes.

MEMBER ZIEMER: And I don't have even a gut feel for what makes sense on at what point you say I've sampled enough or does -- Harry, do you have a kind of statistician's guideline that you use a priori? Obviously we don't want this to be an exercise that fills the time available to do the job or whatever it may be.

MR. CHMELYNSKI: The wrong way is to come up with a sample size.

MR. KATZ: Harry, can you just start over again? Thanks.

MR. CHMELYNSKI: I'm sorry. There are ways to come up with a sample size for validation. I'd have to look more into it. My guess is we're talking about maybe 100 cases. That's just off the top of my head.

MEMBER GRIFFON: Well, they're look at -- you're looking at data points in the database, right?

DR. MAURO: Yes, I was thinking in terms of actual bioassay samples. A case

being a person could include hundreds of bioassay samples.

MR. CHMELYNSKI: Right.

DR. MAURO: I was thinking more along the lines of some kind of cross-section, a nested sampling by time and maybe by -- I guess by building you already have. In other words, we had the HIS-20 data sorted out by year and by building. And by job category.

MR. CHMELYNSKI: Right. For a small time window we have that.

DR. MAURO: Well, up through '61, correct. So we'd have to somehow develop a sampling plan that I guess could make a statistical statement at the end, you know.

Let's say you, just for the sake of argument, you randomly select 100 bioassay samples, some kind of stratified sample. And all together there are a 100 samples.

And then we go in and we say okay and we make a table. Here's what's in the hard copy. And right next to it, here's the

number in milligrams per liter that's in the HIS-20 database.

And let's say we find five of them are wrong. Or one of them wrong. Or none of them wrong. You know quite frankly I'm not sure --

MEMBER ZIEMER: Well, there's two parts of it. One is how much do you sample to start with? And number two, what do you do with the results?

DR. MAURO: Right.

MEMBER ZIEMER: And I think a priori it would be useful -- and not to sort of say well, we'll kind of figure this out as we go -- and have a firm plan, you know, we're going to sample a 100 samples or a 1,000 or whatever it is.

DR. MAURO: Right.

MEMBER ZIEMER: And we're going to have some criteria, whatever they are. Now it may be that once you get into these, you know we thought this made sense but as we look at

it, it's different.

And from my point of view, I think for tasking, we need to know what kind of commitment this is in resources because we've got so many things going on now. And we've got to prioritize some things.

And I would like to see if we could do it. If Harry can develop -- now, you know, we don't want a big effort on a sampling plan but what is it you are going to do.

DR. MAURO: Yes, we don't want to do that again.

MEMBER ZIEMER: What it is is a one-pager. You know here's the plan.

DR. MAURO: Harry, we need a one-pager by tomorrow. Can you do it?

MEMBER GRIFFON: Yes, I mean I don't disagree. I was trying to keep it moving.

MEMBER ZIEMER: No, no, I know he has to come back.

MEMBER GRIFFON: I agree.

MEMBER ZIEMER: The reason I'm suggesting that that be done, that we bounce that off -- I would say bounce it off of Mark, as a minimum, and share it with the group.

MEMBER GRIFFON: Yes.

MEMBER ZIEMER: And I would like you to take a look at it. We should all look at it and Ted have the availability of the cost information. And maybe we can have this done within the week.

And then say proceed then, you know.

MEMBER GRIFFON: Right.

DR. MAURO: Yes.

MEMBER ZIEMER: I don't know what we're talking about here.

MEMBER GRIFFON: Yes, I agree.

MEMBER ZIEMER: Is this a 100 dollar exercise or a 100,000 dollar exercise? Or is it somewhere in between?

DR. MAURO: I don't see that --

MEMBER ZIEMER: Or do you have the

49.95 special this week?

(Laughter.)

DR. MAURO: To me everything is easy. This sounds easy. But I hate to do that to Harry if it's not. Harry, you know --

MEMBER ZIEMER: The statisticians can make it more complex.

DR. MAURO: Yes, right.

Can you come up with something?

MR. CHMELYNSKI: I think you're asking a very standard question. And that there are many, for example, DoD acceptance sampling plans that would work.

MEMBER ZIEMER: Let's have some rationale.

MR. ROLFES: I think that's what NIOSH used.

MEMBER ZIEMER: Yes, you did. The problem is with DoD acceptance plans, they are probably the equivalent to the cost of a toilet seat for the Department of Defense. And so --

MEMBER SCHOFIELD: That was 645 dollars.

(Laughter.)

MEMBER ZIEMER: And that's per sample. But if that's agreeable, it's just to sort of put some specificity on your suggestion.

MEMBER GRIFFON: Oh, yes, that's fine.

MR. MORRIS: It may be that your action will just be to look at what we did and accept it because we used the DoD acceptance sampling plan.

MR. ROLFES: I think we explained how it was done and then presented the data.

DR. MAURO: I think the example is on the web.

MR. ROLFES: Correct.

MEMBER ZIEMER: And so maybe they don't have to do that. I don't know. See, that's --

MR. MORRIS: We may not need to

resample the data and recreate the data collection drill.

MEMBER ZIEMER: But they may want to sample your data. I don't know.

MEMBER GRIFFON: Well, the other thing I want to know --

MEMBER ZIEMER: I don't know what it is they are doing.

MEMBER GRIFFON: Just a couple of questions on what you produced. I want to make sure I have the most current version. It looks to me like -- I didn't count all the logbooks but there is a number of them -- 20, 25, more than that probably.

MR. MORRIS: It's been so long I don't know the details to answer that.

MEMBER GRIFFON: Yes. But at any rate, my question was more the -- I think one thing that SC&A might consider when they look at this closer is what are the years covered because I see a lot of them in the '50s and into the '60s. I think I saw one in 1970 --

I'm just glancing at it quickly. But, you know, I only saw one in the '70s. So, you know, it's just a question of whether we're covering all time frames.

MR. MORRIS: The recollection, I believe, you looked at it previously back in 2007 to look a population from each decade. I believe that's what we had, in fact, done.

MEMBER GRIFFON: Yes. We did talk about that, yes. And there might just not have been as many books available for some years as others or some decades, you know, but -- because, yes, like I said, it seems to me just glancing at this, it looks like a lot in the '50s, but thin in the '70s. And I don't see any in the '80s yet. But anyway.

CHAIR CLAWSON: So --

DR. MAURO: My marching orders right now it sounds like let's first take a look at what you folks have put up on the O: Drive related to the sampling that you did, which is a millspec sample. And remember it

had a lot of nuance to it. In other words, you looked at it in a lot of different cuts.

We could do -- we could certainly read that and see what you did. And I guess, perhaps, using our judgment just check to see if we come to the same place you did regarding the percent of hits. I remember you reported it as well, we got this many spelling errors. I remember you actually caught spelling errors.

And in the end, the hits were mostly editorial more than substantive. I remember the discussion -- I read it last night. We could check that work or we can not even look at it and just do our own. I mean --

MEMBER ZIEMER: No, I think we're asking you to check --

DR. MAURO: To check their work.

MEMBER ZIEMER: -- work and --

DR. MAURO: And that's what we'll do.

MEMBER ZIEMER: -- and then if you decide that that's sufficient, I think that's the end of it.

DR. MAURO: Well, then there's no need for a plan. Then simply --

MEMBER ZIEMER: No, if you decide that you don't have to go back and sample anything --

DR. MAURO: Yes, we'll look at their work, see what they did, and see if it seems to hold up. There will be a judgment made by our statistician if this looks like a reasonable sample, and we checked --

MEMBER ZIEMER: No, I don't think we're asking you to resample.

DR. MAURO: Okay, good, good. That makes it straightforward. And we can actually start right now because we know what we have to do.

MR. ROLFES: Here -- I'll take a second. I did locate the files that I was referring to. There is a document out on the

Advisory Board Review folder. It's dated March 10th, 2008. And the title is Comparison of the FMPC Hard Copy Bioassay Records to the HIS-20 Database.

And I'll just read the executive summary for the record here:

"Since data extracted from the Canberra HIS-20 database was used in the uranium bioassay coworker study for the feed materials production center at Fernald, the verification for the completeness and accuracy of the data in HIS-20 was desired.

An acceptance sampling plan was developed using statistical method known as sampling by attributes. Hard copy records were acquired independently using data capture trips by members of OCAS and the ORAU team. They consist mainly of analytical data sheets, urine request cards, and an annual urinalysis summary report.

"For this study, 33 electronic files scanned from hard copy bioassay results

were examined. There were eight files which were primarily subcontractor or gross alpha beta results. These files were eliminated since they would not effect the coworker study of FMPC employees for the uranium coworker study.

"Twenty of the remaining 25 files met the criteria selected. Five files did not meet the criteria but were unlikely to result in any significant changes to the coworker study if the data missing from HIS-20 were to be included. Overall, 90 percent of the data was matched with only a few files accounting for the majority of the results that were not located in HIS-20."

MEMBER ZIEMER: What was the name of that file again? Comparison of --

MR. ROLFES: The title was Comparison of FMPC Hard Copy Bioassay Records to the HIS-20 Database Dated March 10th, 2008.

MEMBER GRIFFON: Do you have -- that was the title. Is that the file name

also?

MR. ROLFES: That's the title of the document. The file name, however, is fernaldhis20draftfinalanalysisversion2.

MEMBER GRIFFON: There it is, okay.

MR. ROLFES: And it was added on 3/10/2008, just the review file.

MEMBER GRIFFON: Thanks.

MR. ROLFES: There are also supporting files right next to it in there. I'm pulling it up. There's a couple of Excel spreadsheets in here.

MEMBER GRIFFON: And then the urinalysis logbooks available on the O: Drive?

MR. ROLFES: I believe those are in here. Let me see if I can find --

MEMBER GRIFFON: I'm just asking if they're -- if you sampled from the available ones on the O: Drive? Or if you only posted the ones that you used for the study on the O: Drive?

MR. ROLFES: No. Well, any data that we collect would be in the site research database.

MEMBER GRIFFON: Right, right.

MR. ROLFES: I don't know if we duplicated it in the O: Drive as well.

MEMBER GRIFFON: I don't think you did. But that's fine. You've got the references, yes. So there could be more. I'll have to look at the way you sampled but there could be more logbooks.

You didn't sample 100 percent of the logs. I think you went --

MR. ROLFES: No, I think we discussed in that executive summary the quantity of the files that we sampled.

MEMBER GRIFFON: Right, okay.

MR. ROLFES: And from looking at within the Advisory Board's review folder under Fernald, I'm looking at Document No. 4076 FMPC Uranium Urinalysis Program -- no, nope, that wouldn't be it.

I thought maybe we had some of the raw files right there but that's not the correct one. If you want to continue, I thought I'd have the time to open --

MEMBER GRIFFON: Yes, I guess what I'm asking is in that executive summary, Mark, it says for this study 33 electronic files scanned hard copy bioassay results were examined. Are there more files on the O: Drive in the site research database than 33?

There are other files? Okay. So if we wanted to --

MR. ROLFES: Yes, they are available in one place or the other.

MEMBER GRIFFON: Right. And you selected those by your methodology?

MR. ROLFES: Yes, correct. All the data that we captured has been added to the site research database so it is available either there or on the O: Drive.

MEMBER GRIFFON: Okay. So I think that's a pretty clear task, right, John?

DR. MAURO: Yes.

MEMBER GRIFFON: We'll start with that.

DR. MAURO: My guess is Harry will be getting in touch with you to make sure that we're looking at the right data.

MR. ROLFES: Okay.

DR. MAURO: Harry, are you still on the line?

MR. CHMELYNSKI: Yes, I'm here.

DR. MAURO: Great. I guess we've got an action item that I think we are going to be looking to you for. I don't know if you heard everything --

MR. CHMELYNSKI: Yes.

DR. MAURO: -- or have written it down but certainly feel free to call Mark Rolfes to make sure you are looking at the right material. And then when we get back together, we'll regroup and we'll discuss this.

MR. CHMELYNSKI: Okay.

DR. MAURO: Thank you.

MEMBER GRIFFON: So the last item I had was the -- going back to this completeness question. And this -- I mean -- and this also is a question on time, Paul, I mean I think -- but we did this with Rocky Flats.

It was the question of okay, you are clearly in this site similar to Rocky Flats. You're dealing mostly with individual data. If they have enough data to do their own reconstruction, NIOSH has made that determination.

The thing that we'd asked at Rocky Flats was look at a sampling of those -- the claim records and make a judgment on whether the records are sufficient to reconstruct -- are they complete enough in other words?

And I think in the -- what we found in the Rocky Flats review was that there were some inconsistencies. But overall, there were no systemic -- there were no systemic

trends or no problem systemically. So, you know, we judged that overall the records of the claimants would have been complete.

And I guess here is where you look at the comparison of okay, we have a chem operator -- and this goes back to -- I don't know where that 1970 thing came from but if you have a chem operator who only worked in the '50s and '60s and you see, you know, that they should have been on yearly urinalysis but they weren't, they have like, you know, two samples in ten years, that would be brought forward.

Now one thing like that alone I don't think is going to make a problem, at least in my opinion, but if we start to see a trend, the systemic problem of a lot of things are missing in these claimants' files, then that's where we would have a question about the completeness being sufficient for dose reconstruction.

So this is getting away from the

coworker model and looking at, you know, are the individual claimants' files good enough to do an adequate job.

And this goes back to some of the petitioners' concerns, too, because they've all -- we've had many questions about whether they felt their records were complete, were they all there, were they -- you know, so this is part of the reason we've been addressing these at the previous SEC evaluations.

DR. MAURO: A question for you, Mark. Right now in our data, it consistently shows starting in about 1956 approximately 20 percent of the workers have more than four bioassay samples per year. In other words, so I don't know if that goes toward what you're saying.

In other words, we know that, you know, that means some have less.

MEMBER GRIFFON: Right.

DR. MAURO: Now I guess what would be done? That is let's say we go -- I'm not

quite sure what you would do to check what you're saying. The fact that we know, I mean -- we could say that right now. That consistently, you know, 20 to 30 percent of the workers have more than four bioassay samples per year.

MEMBER ZIEMER: But I don't think that answers that per se because what would be an adequate number of bioassay samples is very dependent on where you are working and what you're doing. Or in the case of the accident where it looks like they were sampling every day --

DR. MAURO: Right.

MEMBER GRIFFON: Well and I don't that was an accident.

MEMBER ZIEMER: No, no, whatever it was.

MEMBER GRIFFON: Yes.

MEMBER ZIEMER: I think you are looking for patterns where people who should have been sampled were not. And I --

MEMBER GRIFFON: Or the data is not there, yes.

MEMBER ZIEMER: And I don't think you necessarily find that from these averages.

MEMBER GRIFFON: No.

MEMBER ZIEMER: In Rocky Flats case, you went through some -- you did some selective sampling of files.

MEMBER GRIFFON: Arjun was involved in this so he can describe -- for data completeness for Rocky Flats.

MEMBER ZIEMER: Yes, you sampled a number of cases and then looked at that. And you're looking for either major gaps -- for example, here I suppose you would select some millmen or whatever it is and ask that question.

But how did you answer it at Rocky Flats?

MEMBER GRIFFON: And then the other -- and we looked at externals, too. We looked at whether they, you know --

MEMBER ZIEMER: Yes, are there big gaps, right.

DR. MAKHIJANI: Well, at Rocky Flats, at the direction of the working group, we actually took a very small sample because the working group did not want an extensive --

MEMBER ZIEMER: Right. Understood.

DR. MAKHIJANI: And then what happened is --

MEMBER ZIEMER: It was a sampling.

DR. MAKHIJANI: -- yes, well, we looked at some cases but we did a very crude look. We didn't have job categories, for instance. So this turned out to be an issue eventually in the discussion and there was some criticism that we hadn't done enough sampling but -- so there was a problem and this tension that we -- how much do you do initially in limiting the effort?

And then when you are ready to vote or decide all the issues, put them to

bed, there was a controversy over whether we'd done enough. And specifically, I think, it was over the lack of enough examination of job categories or buildings. I don't remember what the issue was.

But definitely we did a rather more crude look than what we've been discussing this morning.

MEMBER GRIFFON: And we may need -- I don't know what's -- when you submitted a plan before, John, that wasn't answering this question for data completeness?

DR. MAURO: No.

MEMBER GRIFFON: It was a different data completeness sampling. So I mean I would think we would have to have a similar step here is that we need to get a sense of how big a sample you think is going to do it.

And, again, it may, you know, unfortunately, you know, we do, we've been running two years on this. You know we have

to answer some questions here. So, you know, I don't know that we want to go back in, you know, more than 1,000 claims.

I mean obviously I don't think you want to do 300 of them, you know. So, you know, what's the right population?

DR. MAKHIJANI: If I might say something? We've been also doing a sampling plan at Nevada Test Site. And just personally from a technical point of view, and Harry has been involved in that, I'm actually quite happy with what we did there.

We had sampled 20 in each of six job categories. And I think --

MEMBER GRIFFON: A similar approach might work, right?

DR. MAKHIJANI: -- we got a pretty good result. It was a fair effort. It was a small fraction of the population of workers. But I think in the end, we got something that is very reliable in my opinion.

DR. MAURO: In that case, though,

we worked with logbooks, handwritten logbooks and --

DR. MAKHIJANI: Handwritten logbooks and records. It was not a non-trivial effort.

DR. MAURO: It was a big effort, yes.

DR. MAKHIJANI: But here, I think, I'm a little bit confused because the completeness plan that we presented to you last October was along the lines of, you know, taking something -- some lessons learned from Rocky Flats and then doing a little bit more elaborate thing and -- but looking at completeness of data. Now what we're talking about is something different.

MEMBER GRIFFON: Well, I thought that was the same. I thought that's what we wanted to go back to. Now I don't know why we lost that. Maybe it was because the same was so large that we were concerned about how long it would take.

DR. MAURO: It wasn't -- it was small.

DR. MAKHIJANI: It wasn't very large. Well, there were three different files.

MEMBER GRIFFON: I mean --

MR. ROLFES: While we're searching for that, I can point out that the HIS-20 database table, this is from our HIS-20 draft analysis, version 2 that I mentioned before. It says the HIS-20_B_bioassaytable contains 435,982 records of which 431,016 are urinalysis records to below 406,145 are identified as U total with units of micrograms per liter.

Also you were asking about the references that we used, there are two tables associated with that summary report, which we have transcribed data from PDFs into these Excel spreadsheets for each individual reference ID, which we've mentioned in these two Excel spreadsheets. We've got that data

that we used and some notes associated with that.

MR. ROLFES: That's what I said, even if they're not on the document review as a reference, they're there. So we can --

MEMBER GRIFFON: Right. So they'd be easily recovered from the site research database.

MR. ROLFES: Also, it didn't escape before -- I forgot that we also did, in addition to, you know, evaluating the uranium analysis results and comparing those within HIS-20, we did also take some of the other results that were -- essentially any bioassay data that was collected and put into HIS-20.

And so there's plutonium, urinalysis results which would also be helpful for us in reconstructing someone's recycled uranium intake or potential recycled uranium intake.

So it's not just a small, simple, only uranium inter-comparison that we did in

a data comparison but essentially all the data that were collected and compiled in this database we sampled and determined whether the data was sufficient, whether the data was accurate. And so there is quite a large amount of data that was analyzed and presented in these files.

MEMBER GRIFFON: But I mean going back to the data completeness thing, I don't -- if we dropped it, it wasn't -- I didn't -- I don't know if the work group meant to but I didn't mean to.

DR. MAKHIJANI: Yes, the two options that we -- Harry, are you still on the line?

MR. CHMELYNSKI: Yes.

DR. MAKHIJANI: Do you have the October 6 plan open -- correct me if I'm saying anything wrong -- maybe you should take this over -- in Table 3 of that plan, there are two different sample sizes that were presented: 150 and 300.

MR. CHMELYNSKI: Right.

DR. MAKHIJANI: And of course you have different degrees of statistical confidence.

MEMBER GRIFFON: And I think even 300, you're talking about a third of the claims.

DR. MAKHIJANI: Yes. So there is -- 150 is 13 percent or about. Then the table had parsed out how many workers you would get in each plant and how many workers you would get in each of several job categories.

MEMBER GRIFFON: Right.

DR. MAKHIJANI: And I think, you know, just looking from the Nevada experience where we already completed this thing --

MEMBER GRIFFON: Yes.

DR. MAKHIJANI: -- we did 120 there. The number of job categories fewer in terms of what we were really looking for because we took predefined job categories. There are really far more job categories at

Fernald.

But if you look at the important job categories in terms of exposure potential, you could limit them and do something like the 150 option.

Harry, am I off base?

MR. CHMELYNSKI: I think we're in the same ballpark here. It was a different study that we did then but yes, I think about the same.

DR. MAURO: We did a lot of dose reconstruction audits for Fernald. I don't know how many we have. Maybe Kathy would look -- I don't know, Kathy, are you still on the line?

MEMBER ZIEMER: You would know something about completeness from them.

DR. MAURO: Yes. I mean I don't know how many we did but that's what we do in a dose reconstruction.

MEMBER ZIEMER: Yes, right.

DR. MAURO: You know we may

already have at least something intelligent to say about this based on the results of -- I know we must have done I don't know five, six, ten, maybe more.

DR. BEHLING: John?

DR. MAURO: Yes, Hans?

DR. BEHLING: This is Hans. Kathy is not in the office but I can get here and get back to you after lunch perhaps.

DR. MAURO: That would be great. It turns out, you know, we have a significant number of Fernald cases that we reviewed. Obviously we'd be able to say something about completeness of the data and the ability to reconstruct those, external and internal, and what the records look like for those workers.

DR. BEHLING: Specifically, what is the question so I can direct her focus on getting you the answer?

MEMBER GRIFFON: How many Fernald cases?

DR. MAURO: How many Fernald cases

did we review?

DR. BEHLING: Okay.

DR. MAURO: Yes, to date I know we reviewed about 240 cases. You know how many of those were Fernald cases?

MEMBER GRIFFON: But I can't imagine it is more than 20. And you're talking 150 here, you know, so --

DR. MAURO: But it's nice to take advantage of this.

MEMBER GRIFFON: Right, right, no, I agree.

DR. MAURO: Thank you.

MEMBER GRIFFON: It still seems high to me.

DR. NETON: It seems like you're getting back into that original issue was do we have data for the right classes of workers? And it seems to me that is very well established that we have 90 percent of the workers with a monitoring badge.

I don't know looking at the

database itself if it's going to be any more instructive. I mean --

MEMBER GRIFFON: No -- well, you mean the individual claims files?

DR. NETON: I think the claims files is where you really probably need to look.

MEMBER GRIFFON: That's what we're talking here.

DR. NETON: That's what I'm talking about. Originally the sampling plan was not claims files, was it? Or just to go back and look at how many workers -- or how many millrights were, you know, sampled.

DR. MAKHIJANI: The original plan was to look -- go to the claims files to look at --

MEMBER GRIFFON: That's what I thought. Like we did with Rocky Flats, yes.

And then we saw -- I think -- personally I thought 150, I was trying to think of a way that -- yes, can we reduce that

and still keep the statistical significance.

I mean we did have a problem, you're right. And we got criticized in Rocky for going too small. But we had to weigh this thing of, you know, how long, how much money are we going to spend on this task?

DR. MAKHIJANI: And if I recall, we did 40 or 50 workers at Rocky Flats.

MEMBER GRIFFON: I think so, yes, somewhere in that range, yes.

MEMBER ZIEMER: Well, if there was a systematic problem, you would expect it to be showing up in the claims that you monitored to start with.

DR. MAURO: Yes, that should be revealed.

MEMBER ZIEMER: So it would certainly be a starting point.

DR. MAURO: By the way, the original budget claim that was covered last time was 200 workers. So it was not a large effort to do the thing that we describe here.

It might have been 200 work hours.

DR. MAKHIJANI: And that might have been a HIS-20 examination --

DR. MAURO: It was.

DR. MAKHIJANI: -- and not a paper file --

DR. MAURO: Not a paper file.

MEMBER GRIFFON: That was my recollection. I was thinking about it as a paper record.

DR. MAKHIJANI: So maybe that's where the problem arose.

MEMBER GRIFFON: Because HIS-20, I think you're right, we already had that. So I think we have to think of a way to reduce that number of -- if we can -- I mean if, you know --

DR. MAKHIJANI: I think you talked about this with me, Paul, in terms of what it took for NTS. Ultimately when the thing got going, it was several hours, four, six hours.

DR. MAKHIJANI: So it's not

insignificant but it is not as huge as you would think. I mean the SC&A young people that did this doc are pretty good at it.

MEMBER GRIFFON: You're still 600 to 900 work hours.

DR. MAKHIJANI: Yes, it's not trivial. Yes, it's not trivial.

MEMBER ZIEMER: Mark, you described what, in a sense, was NIOSH's evaluation of the completeness of data.

MR. ROLFES: Correct.

MEMBER ZIEMER: Is that -- what you described, did you ever formalize that in any kind of a summary report?

I mean is there an equivalent report to your other -- what was the other one -- the report on the validity -- the validity report. Was there a completeness report similar to that?

MEMBER GRIFFON: I don't think we ever evaluated -- I don't think NIOSH ever evaluated -- this, the way I'm talking about

completeness here.

MR. ROLFES: Correct. What we've done or what we were tasked by the Advisory Board to do or the working group to do was to ensure that the data entered into HIS-20 was accurately entered.

MEMBER ZIEMER: Yes, that's it.

MR. ROLFES: I don't believe we've gone and sampled a population of workers to independently also verify that, you know --

MEMBER ZIEMER: No, but in a sense, in doing dose reconstructions -- and you've done a lot of those at Fernald, you have some sense of completeness of data.

MR. ROLFES: With every dose reconstruction that is completed, we do, in fact, determine whether the data are sufficient on a case-by-case basis for a dose reconstruction.

MEMBER ZIEMER: Right. And does that -- so does this show up anywhere?

MEMBER GRIFFON: You don't look at

it systemically though. You look at it on a case-by-case --

MR. ROLFES: Right. It's not done across the Board.

MEMBER GRIFFON: Yes.

MEMBER ZIEMER: Well, what -- okay, I'm trying to think about -- if you systematically were finding the data to be incomplete, would that show up somewhere in your system as a report where you would alert dose reconstructors?

DR. NETON: It would be on our Gantt chart tracking system saying we have a -- we don't have a method to move forward with these cases.

We track these all the time. Why we aren't get them out the door, there's always a technical reason identifying it. Well, we don't have sufficient bioassay data to move this forward.

MEMBER ZIEMER: Right.

MEMBER GRIFFON: Yes but that's a

little different question than I'm asking. I mean --

MEMBER ZIEMER: Well, it's part of the same question but it's sort of -- it's less formalized.

MEMBER GRIFFON: Yes.

MEMBER ZIEMER: In other words --

DR. NETON: Yes, we don't --

MEMBER ZIEMER: -- if there was a data incompleteness issue, it would show up in terms of how you were handling cases. And we're looking for some way to sort of certify that, in fact, the data are complete.

I was trying to see if there was a way we could say yes --

DR. NETON: I've always maintained and I'll say it again, I think the proof is in how we've done the dose reconstruction.

MEMBER ZIEMER: Right.

DR. NETON: We've done 900 and something dose reconstructions.

MEMBER ZIEMER: Right.

MEMBER GRIFFON: Right.

MEMBER ZIEMER: And that's why in the ones that you've sampled that -- and are those enough cases for us to satisfactorily answer the question? I guess we need to know how many cases there are.

MEMBER GRIFFON: Yes. But I don't even think -- you know when we do -- when we do dose reconstruction reviews, we're also looking at did they -- I mean basically it's a detailed review of did they follow the procedures?

So if the procedure says, you know, you have this many -- I mean I don't think anybody -- and I'm pretty sure we never looked and said okay, this worker in Fernald should have been on a quarterly but we only have an annual -- you know, it looks like they have annual data. I don't know if that would have come up in SC&A's review of cases.

DR. NETON: Yes, I'm not sure if quarterly or annual sampling makes any

difference in the way we do --

MEMBER GRIFFON: Yes, those are modeling. I agree. But it raises -- if you see a systemic problem across the Board, you wonder what happened to the data? How did -- where did this go?

If this person was supposed to be measured every -- you know what I mean? It may not -- like Mark's example, if you have one sample in 1989 but this was a chemical operator from 1950, he's probably right.

You can still use a chronic model and bound but what happened to all -- you know why is it all missing, you know? And I don't think that we're going to find it.

DR. NETON: I think you're asking a question you can't really answer. I mean if there's -- if you think you should have been monitored quarterly and there's annual samples, we don't know whether the plant just didn't follow their own procedures or the data are lost. Or does it really make a

difference?

DR. MAURO: Well, what we did have in our audits --

MEMBER GRIFFON: Well, it gives you a sense of the quality of the data that you're dealing with though. You know like if -- for the quality of the program.

I mean for me if they have protocols to sample certain work categories by month and certain ones by quarters and certain ones annually and if everything was annual in the thing, it raises some questions to me on what happened between, you know, protocol and the data we've got in HIS-20 or whatever or in the hard copy records.

DR. NETON: I don't they've got to that level of granularity. I think something along the lines of what John was talking about earlier where you can take these people with the higher exposure values, this list they had of 20-something job categories.

And say well, were those people

indeed sampled more frequently than the people in the lower categories? I mean that would -- and you have data to support that, yes or no.

And sort of draw a very bright line and say well, if you've got to have quarterly data for chemical operators and what not --

MEMBER GRIFFON: I am not saying -- I'm using these things as descriptors.

DR. NETON: Yes, yes, I understand.

MEMBER GRIFFON: I mean, you know, in Rocky Flats, we found several examples where it didn't match. But at the end of the day, we said there was no systemic, you know, sort of intentional thing going on.

It was just once in a while it didn't match. But no big deal. That's sort of the -- that's the outcome we're looking not to say, you know, not to try to answer every mismatch. You know we don't want to answer every mismatch. We want to look for trends,

I guess, is what I'm saying.

DR. MAKHIJANI: Jim, of the -- or Mark, of the 950 dose reconstructions that have been completed, typically when I've looked at dose reconstructions, there has been deficiency one way or another. And so most of them would actually not have used the detailed data.

DR. NETON: More than likely.

DR. MAKHIJANI: I don't know how many -- we couldn't have had an assessment of -- in going through your dose reconstruction of --

DR. MAURO: No, but you do know -- I think the deficiency process has been steered away from.

MEMBER GRIFFON: I don't mind looking at those.

DR. MAURO: But in every dose reconstruction we do, the first thing we do is -- were there bioassay data for this worker and were there fil badge data for this worker?

And we would capture that in the record file. So we would know for every case we reviewed. Now whether or not --

MEMBER GRIFFON: Because I know we've had findings recently where we said, you know, the individual had bioassay data and should not have used this model. And NIOSH is saying, yes, we're changing it over. We should have used this.

DR. MAURO: Right. But remember the question that is being posed though is that let's say we have 15 cases that we reviewed. They may have applied OTIB-4 or some other deficiency method to quickly clear this case.

Nevertheless, when we review it, his file, that worker's file, if he had bioassay data and he had film badge data, it would be in his file and we'd have a table of every single measurement and what the measurement was and when it was taken.

And we would be able to say okay,

out of the 20 or whatever cases that we reviewed, here's the worker and here's his record. He worked here these years and here's the bioassay samples that were collected.

MEMBER ZIEMER: You would also know his job category.

DR. MAURO: And we'd know -- well, to the extent that it was in his record.

MEMBER ZIEMER: Because you always show that in your reports.

DR. MAURO: Oh, we do when we have that recorded, yes, we do.

DR. NETON: Maybe we are doing several different things here. I mean wouldn't what Mark talked about earlier that we've already done speak to some of this? Which is if you went to the hard copy records and made sure the HIS-20 database has all the hard copy records or a nice sampling. And we'd have the original data in there.

DR. MAURO: Well, I think I'm hearing something different.

DR. NETON: We have the samples that they took on the workers.

DR. MAURO: Right.

DR. NETON: Now the second question is were the workers adequately monitored is a different issue. So I think the proof is in looking at each individual case. If we've demonstrated we have the records of the sample they took, we have what we have. We don't appear to be missing large chunks at least compared to the hard copy records.

Now you can go back another step and say they never got the hard copy records. But I don't know how far you want to regress back. So we have the data of the individual. Now it's a judgment call. Do we have sufficient data now that they took on this person to reconstruct this dose? So I think that's been done.

MEMBER GRIFFON: I think -- I don't like the -- I mean I think the sample is

too big but I think actually there is some usefulness in looking and saying -- I mean let's think -- let's drop the bioassay argument and go to the external dose size because now you can't hang you hat on a sample in 1990 anymore, right?

DR. MAURO: Correct.

MEMBER GRIFFON: So you got TLDs, the person is supposed to be on, you know, monthly TLDs. You have no data for, you know, eight years or something. Then what do you do?

Now in the dose reconstruction, I know just -- I'm not sure what they -- well, I'm not sure for Fernald what they would have done.

MR. ROLFES: I think we explained this pretty detailed in our site profile because it came up as -- when women were not monitored routinely. And we presented three different methods that we could use to assess their unmonitored dose. And I think we've,

you know, completed that.

MEMBER GRIFFON: So that was unmonitored by design, right?

DR. NETON: Right. And remember the security badges --

MEMBER GRIFFON: Go ahead.

DR. NETON: -- the security badge is part of dosimeter for many, many years at Fernald from very early on.

MEMBER GRIFFON: So we can't imagine them not --

DR. NETON: It would be hard. I mean we've been down this path before and where it split and things but you raise a good point. I mean -- well, I'm not --

MEMBER GRIFFON: The only thing that remains for me is that I don't want to get into the -- I think 150 -- just sitting here, it seems large. And I'm sure there's good statistics to back up why you chose that number but I'm trying to think of something less, you know, burdensome.

DR. NETON: Thirty seems to be a really good number. Once you get to 30, it's part of diminishing return.

MEMBER GRIFFON: Yes. And maybe we don't have to -- you know maybe the job and -- I mean I'd have to look back at the plan you submitted before but maybe we don't have to -- maybe there is a way to cull down that number and get what we need to answer, you know, because, you know, I don't know.

I mean we've got a number of factors here. And if all of them are looking good, I don't think we need to look at 150 cases for this aspect of it is what I'm kind of getting at, you know. So --

DR. MAKHIJANI: I would agree. I think in view of the very large number of bioassay samples that there are and the fact that more than 90 percent of the workers have some sample, I think going through the same exercise that we went at Nevada test site where only 35 -- in the Nevada test site, it

was a much, much bigger issue at least in my opinion because there you only got 35 percent of the workers were monitored internally, if I'm remembering the number right. It's on that order.

And so you have a qualitatively different situation. So the chance of your coming across a worker who was never measured at NTS is pretty high compared to Fernald where it is pretty low. So --

MEMBER GRIFFON: There were -- I'm trying to remember back to the Rocky Flats although sometimes I try to forget it. I have reasons why that's the case. But you're not a production facility at Nevada.

DR. MAURO: Well, I'm not saying we're good or bad. I'm just saying in terms of you're likely to find in a sample size -- anyway, it doesn't matter --

MEMBER GRIFFON: But I mean one thing -- the one thing that sort of came out and this is part of the reason for going

forward is it may -- and I would like to get that number down but I believe, and maybe I'm wrong, Jim, but some of that '69, '70 stuff at Rocky Flats showed up when we did this, you know, completeness reviews that we did.

You know we sort of found, oh, yes, look at this in '69. And then there was the question of the fire and what happens --

DR. NETON: Right. And that was my original objection to doing sort of analysis because then there was always -- they were on strike in that year and they moved production from Plant 2 to --

MEMBER GRIFFON: But at the end of the day, we got there. And we said okay, there's good reasons for this, you know, but that what the people are asking, too. You know petitioners are asking, you know.

And we -- yes, it is time consuming but we don't want to leave that hanging out there, the concern from the public is these records are, you know, are not good.

We have concerns about them. And this is -- you know, we've got to do this with rigor to make sure. And if we put it to bed, we put it bed, that's great.

But I think we've have to go there. I'm just uncomfortable with the 150.

MEMBER ZIEMER: Well, let's say you did a sampling, say it's ten, or it's 30, or 150 -- hopefully it's not --

MEMBER GRIFFON: Hopefully it's more like 30 or 50 but yes.

MEMBER ZIEMER: -- but, okay, you go in and you pull a case. What are you going to look at? The years worked? The number of bioassay samples? Number of film badge samples? And the job category and the plant.

MEMBER GRIFFON: Right.

MEMBER ZIEMER: And you could table these.

DR. MAURO: And that's done, to some extent, right now. It's already done.

MEMBER ZIEMER: For one part.

DR. MAURO: No, for the dose --

MEMBER ZIEMER: For the ones you've already done, yes. But it doesn't look to me -- that's just bean counting it looks to me like.

MEMBER GRIFFON: Yes.

MEMBER ZIEMER: So it doesn't look to me like it is a big time commitment. You're not having to calculate anything. Just -- you're just looking for some patterns here. There's nothing about the tabling.

DR. MAURO: What we're really talking about is let's make believe for a minute that what you were asking is we want to do an audit of Fernald dose reconstructions, you know, we'd like to go in -- what happens when we do that? You folks provide us with some electronic files, which is the record for this worker, which includes everything DOE provided you regarding this person.

In a very short period of time, we quickly go into their bioassay and we make a

table. And we say here we are. We count them. And we say here they are and we put the numbers in.

And that's the story. That's done on day one. Okay, this is what we have. Then we start the processes. How do they use that data? Did they follow their procedure?

But you're not asking that question. You're just sitting there saying let's -- what do we have on this person.

MEMBER GRIFFON: Right. What's their -- and is it appropriate for their job and their building and their whatever?

DR. MAURO: Yes, so I mean if you folks -- the way you always provide us with a CD, with, you know, the 23 cases that we are going to have to audit, I mean if you would provide us with a random sample of 30 Fernald cases and just say here, as if you were going to do a dose reconstruction audit, but we're not. We're just simply going to do this. I think this --

MEMBER GRIFFON: Well, I'd like to make sure -- think about the 30 because that's a big difference than your 150.

DR. MAURO: I'm saying we could do one thing. I mean I don't think that -- as long as we're not doing an analysis, did you follow you procedures, and then to match your numbers because, you know --

MEMBER GRIFFON: Think about the data and not the dose, not the dose.

DR. MAURO: I don't think this is a -- each case would go very quickly.

MEMBER GRIFFON: Yes, I think it's pretty helpful.

DR. MAURO: A few hours a case.

MR. CHMELYNSKI: John, I'm going to interject here. The previous studies --

MR. KATZ: Can you identify yourself please?

MR. CHMELYNSKI: I'm sorry. This is Harry Chmelynski.

MR. KATZ: Thanks, Harry.

MR. CHMELYNSKI: In the previous study, we were looking at a completely different question which was how many records would we have to look at in order to determine whether sampling -- to determine accurately whether sampling was done quarterly or monthly or annually over a broad number of cases.

Here we're looking at individual cases. So I don't think the 150 has anything to do with what we're doing here.

MEMBER GRIFFON: Okay. Good. Good.

MR. KATZ: Thanks, Harry.

MR. CHMELYNSKI: Okay.

MS. BEHLING: Excuse me, John, this is Kathy Behling.

DR. MAURO: Yes?

MS. BEHLING: I guess -- I don't know whether it's still relevant to your conversation but I guess you were interested in knowing how many cases we reviewed from Fernald as the first 258 cases. I quickly

looked that number up. We've looked at 15 Fernald cases.

MEMBER ZIEMER: There you go.

MS. BEHLING: Now of those 15, six were maximizing cases. They were early on or were minimized. And only five are best estimates or what they term full internal and external.

And I haven't had a chance to really go into those records or look in-depth at what we did there. But I can certainly do that if it would help.

MEMBER GRIFFON: I don't think we need it right away but yes, you might have those cases to work on. You might have those cases to work on, yes, yes.

DR. BEHLING: This is Hans Behling, also from SC&A.

Regarding the issue of the adequacy, I guess I do want to caution in context with what Kathy was saying is that for many of the bioassay data for Fernald, we have

data. But the question that we raised during the review of the TBDs is how much of that really requires default values. And, of course, NIOSH has assured us most of the default values are usually claimant-favorable such as the uncertainty regarding -- since most of the urine data was dosimetry data, that doesn't really tell you exactly the composition in terms of enrichment. It doesn't tell you the chemical nature of the uranium. And it doesn't tell you the solubility for all these other things.

So we basically have a dose reconstruction that has a core element to it such as milligrams per liter of uranium in urine. But then all the secondary factors are basically default values.

So with regard to the accuracy, well, it's a question of do we trust the default values. And that's a topic of a different discussion.

MEMBER GRIFFON: Right. Yes,

that's a different issue.

CHAIR CLAWSON: If I could interject -- my belly is talking to me -- John, what I suggest is over lunch that you kind of think about this because I don't want to kind of have a knee-jerk reaction. I want to make sure that we are getting exactly what -- so we're all on the same board because we've been kind of going around here.

Just kind of think about it a little bit. And when we come back after lunch, we'll discuss this a little bit more in detail to make sure that everybody is on Board with where we're at and what's asked of SC&A, you know, if we could.

DR. MAURO: Kathy and Hans, I'm going to give you a call during the break. I'd like to talk to you a little bit about what we can do with the data. If it is in cases you have right now and it's something that could be done expeditiously and maybe inform this process.

MEMBER GRIFFON: And maybe talk over break about the total number, too, that you think would be sufficient.

DR. BEHLING: John, so give us a call whenever.

DR. MAURO: Very good. Thank you.

CHAIR CLAWSON: We're done for lunch.

MR. KATZ: Okay. We're breaking for lunch. It's almost quarter to one. So let's see, what time would you like to -- quarter to two, we will reconvene.

Thank you everybody on the phones.

(Whereupon, the above-entitled matter went off the record at 12:43 p.m. and resumed at 1:50 p.m.)

MR. KATZ: Good afternoon. This is Ted Katz with the Advisory Board of Radiation Worker Health. It's the Fernald Working Group, and we have just returned having broken for lunch, and that's all I have

to say, but Brad you can --

CHAIRMAN CLAWSON: When we left for lunch, we were debating and questioning back and forth with SC&A on this sampling plan that we were going to do, and I've asked John to more clearly define what he'd like to do, so I'll turn that over to John and we'll go from there.

DR. MAURO: I called Dr. Behling during lunch and talked about 14 -- these 14 cases that we did. That's a good place to start. And I said you did a table on the 14 cases. This is -- we'll intend to look at them, they might be useful. This is what I explained to him over the phone, and see if everyone agrees this is the kind of thing we'd like to see.

MEMBER ZIEMER: Talk loud.

DR. MAURO: Yes. Basically, I made a little blank table that we filled in. There's the person, Person Number One, Person Number Two, all the way through the 14th

person. The next column would be his job title. What did he do, if you can get that. And usually you can.

The next column would be the number your worked, 52 to 72.

The next one is what's the total number of bioassay samples that were collected from that worker over that time period.

These are the changeouts that were collected from that worker over that time period.

Now that would be a very close snapshot picture of completeness. You know, if you see some zeroes or you -- you know what to expect. You've got a person that has a fairly comprehensive experimental program you know it's going to be monthly.

Same thing as bioassay, quarterly, you know. You want certain numbers to be in there, and it's fairly complete. Is this what you had or not? And this is my question.

MEMBER GRIFFON: No.

DR. MAURO: No.

MEMBER GRIFFON: I mean it is good -- it's good slushing criteria, you know, but it's not what the final product --

DR. MAURO: No, no, no. I'm saying with regard to the 14 cases.

MEMBER GRIFFON: I mean, it would let you -- I think you should use those as you can going forward, but, I mean, the final part I think should look like you did for Rocky, for each case.

You know, in other words that Person Number One --

DR. MAURO: Yes.

MEMBER GRIFFON: -- they might have worked 20 years. They might have four different job titles.

DR. MAURO: Okay.

MEMBER GRIFFON: So you have to look annually.

DR. MAURO: Okay, so you want --

MEMBER GRIFFON: You want to have

details.

DR. MAURO: That's why I put this in.

MEMBER GRIFFON: Yes, yes.

DR. MAURO: Right now --

MEMBER GRIFFON: Okay, overall, yes.

DR. MAURO: So in theory what you're really saying is we could blow this out, so for that person we could have a whole page per person.

MEMBER GRIFFON: Yes.

DR. MAURO: We get into each year where we get into each year. In other words, for that person what's the date of 1952, 53, 54.

MEMBER GRIFFON: Because otherwise you're not going to see trends or gaps. I mean, if you just see total number of bioassays in 30 years --

DR. MAURO: Right.

MEMBER GRIFFON: -- you know it

looks like 30 samples or 60 samples or whatever, but it looks robust, but it could be that from '70 to '75 every person there is missing data, you know.

DR. MAURO: Okay, so --

MR. MAKHIJANI: And Mark just to clarify a little bit of informal conversation we were having on this point about what you want so it's clear --

MEMBER GRIFFON: Yes.

MR. MAKHIJANI: -- to everyone. Is your want not going to be an annual thing, but you want something about the job category and the expected monitoring? Is that what you want?

MEMBER GRIFFON: Yes.

MEMBER ZIEMER: I don't think we have to have them put in expected frequency. I mean, we can make that judgment, but if you're going to have -- for example, if the person is a nomad for the first 10 years and there'll be some frequency. And you can do it

by year.

I agree, it should probably be by year --

MEMBER GRIFFON: Yes.

MEMBER ZIEMER: -- so you can see if something is missing. And if they change jobs and suddenly they're the -- you know, they're working in the front office --

MEMBER GRIFFON: And if done annual, then yes.

MEMBER ZIEMER: Yes, but -- yes, so I think there's just more detail you're talking about. But I don't think that adds much more work.

MEMBER GRIFFON: I don't think so. It would be copying it and pasting it.

MEMBER ZIEMER: You want to just break the years out a little more.

DR. MAURO: So -- a separate page for each year.

MEMBER GRIFFON: And for those 14 cases that you've done already. I mean, if

you don't have it in the spreadsheet, NIOSH does. I mean, I know because reviewing these cases --

DR. MAURO: Well, right now Kathy is putting that back table together. We will --

MEMBER GRIFFON: Right.

DR. MAURO: We will make the table you just described, which should look a lot like -- except that would be by year. In other words --

MEMBER GRIFFON: Or by reading. Really, by reading because it could be a sub year, but anyway -- yes.

DR. MAURO: Well, a person --

MS. BEHLING: Excuse me, John. This is Kathy. I'm listening in here and over the lunch hour I started putting this table together, and I'm putting it together just as Mark explained, because it didn't seem to make sense to me just to give you a total. And I've already for two of the individuals, and

it's 15 total, for two of the individuals I have already broke it down, broken it down by year and if it's a partial year I say the year behind it. I put in whether it's weekly or bi-weekly for the film badges, and then I've also broken down for the urinanalysis by year. So I'm already doing that.

DR. MAURO: Great.

MEMBER GRIFFON: So then I guess the bigger question is how many overall cases -- right, and you were saying probably 30 or 40 --

MEMBER ZIEMER: I think we can make a judgment. If we come back and say we can't reach any conclusions through this, we can always instruct --

MEMBER GRIFFON: I think 30 and if they're fairly random -- I mean, do you think we should bias them in any way?

MEMBER ZIEMER: These working cases typically are random.

MEMBER GRIFFON: Based on what we

have here.

MEMBER ZIEMER: And I would say the others ought to be randomized in some fashion.

MEMBER GRIFFON: The only thing I was thinking was we might want to make sure they're in the SEC period, you know. We have a lot of years in '89 through 2006. That might not be so useful.

And then also maybe if we want to bias it at all, make sure we cover those early years more than the later years. I don't know if that -- that's sort of a judgment call, but it seems to me there's no question about the monitoring '52 through '54.

MEMBER PRESLEY: Stay away from '52 to '54. I mean, that was a production year up there. It's when they were building buildings and facilities and stuff like that.

MEMBER GRIFFON: Well, it's a construction year, yes.

MEMBER PRESLEY: And a lot of the

stuff was not on site until after 1954.

MEMBER GRIFFON: So that may be difficult to evaluate whether they should have been monitored during that time period is what Bob's saying, I guess.

MR. MAKHIJANI: Well, we have to look at the site profile and the site history, and I think '52 was certainly a construction year.

MEMBER GRIFFON: Yes.

MR. MAKHIJANI: I'm not so sure about '54.

MR. MORRIS: There was still construction going on in '54.

MEMBER GRIFFON: Yes, it was still going on.

MEMBER PRESLEY: One of the things by breaking that out by year like that, it's going to be interesting to see is -- say you had somebody that was a 10-year worker and then in 10 years maybe he was promoted to a foreman, when he's a foreman in the same area.

So what his dose reconstruction as a worker and his dose reconstruction -- or not dose reconstruction -- but his dose would be as a foreman in the area. See if things drop there.

That was one of the things I was looking at on that table in there. You all had things about workers and you also had things about foremen, and the foremen doses were super, super low. A lot of the times the foremen are right out on the floor with the workers, so that's something that we -- it's going to be interesting to look at.

And your foremen didn't sit in an office for eight hours a day. Generally, he was right out in the middle of the operation going on.

MEMBER GRIFFON: Right. So that's -- I think that's the construct. Is that clear?

CHAIRMAN CLAWSON: I heard Kathy say 15.

MEMBER GRIFFON: Maybe you ought to do 15 more?

DR. MAURO: Now the question becomes with 15 more is what's the most efficient way to do that to get the next set of 15. Right now, you know, NIOSH provides us with the CDs for those 15. Would it be the most efficient way for NIOSH to provide us with another set of 15 according to certain criteria, or should we somehow just search the database.

I'm not sure how best to do this.

MEMBER GRIFFON: They've got to be finally adjudicated claims, right? We usually don't review other --

MEMBER PRESLEY: I say take zero -- you know, 10, 20, 30, 40, 50 until you get that, and if they're not in the time frame, then skip it and go on to the next zero, the next 10.

MEMBER ZIEMER: You mean in the order that they came in?

MEMBER PRESLEY: Yes.

MEMBER GRIFFON: I mean, I think -- I don't know. My feeling is that's the SC&A can sample.

MR. MAKHIJANI: Or Harry's done this a number of times, and the only thing I would suggest is that we do, as you were saying, have a somewhat of a bias for people who started in the '52 to '56 period, no matter how long they went.

And that we also have something of a check to see that we had a half a dozen or 10 workers who went through the eighties, up to '89 --

MEMBER GRIFFON: Right.

MR. MAKHIJANI: -- so we're not missing the tail end of the period, and we make sure that we have that, but then that we leave the rest to Harry. Let him --

MEMBER GRIFFON: Yes, we know you're going to keep it at 30 cases overall, so I don't think it's an issue. As long as

you describe exactly how you sample them, I think that's fine.

(Simultaneous speakers.)

MS. BEHLING: Yes, we can do it right off an octave.

MEMBER GRIFFON: Yes. I think that will work if that's okay with everyone.

MR. MAKHIJANI: Harry must be still on the line. Harry, are you on the line?

MR. CHMELYNSKI: Yes, I'm still here.

MR. MAKHIJANI: Does that sound reasonable?

MR. CHMELYNSKI: Yes, that won't be any problem to pick a small random sample. We may do some sort of rejection sampling though in order to make sure it meets the --

MEMBER ZIEMER: Yes, I would rather him do it that way. Randomize it, maybe you'll pick up 20 random numbers or something.

MEMBER GRIFFON: Right.

MEMBER ZIEMER: Your first 15 randoms, though, if you're missing a couple of criteria --

MEMBER GRIFFON: Exactly. All right, that's it on that topic, I think.

CHAIRMAN CLAWSON: No more discussion on --

MR. MAKHIJANI: Do we draw the data from the HIS-20 database, or do we have to go to the paper file?

MEMBER GRIFFON: I would suggest going to the paper file. Isn't that the bottom line for the dose reconstructors to use the hard copy record, right? I would go with the hard copy record.

CHAIRMAN CLAWSON: Ted, I guess out of clarification do I need to go through these as passed this, as done with this? That sounds good. So, John, I guess the next step we're going to go onto is RU.

DR. MAURO: Everyone should have

received the -- a report dated March 2009 titled SC&A's review of issues related to the reconstruction of doses for workers exposed to recycled uranium at Fernald, commentary on NIOSH white paper.

During the last work group meeting we were asked to review this issue, and mainly the concern was the mix of radionuclides. Right now the co-worker model approach being used for dose reconstruction includes the assumption that for every milligram of uranium that's in urine, along with that uranium comes plutonium-239, neptunium-237, technetium-99, a list of radio nuclides which are trace contributors due to recycling.

Now the -- when recycling actually started -- the assumption that's going to be made it begins at time zero, for all intents and purposes. That is, every single bioassay written -- Jim, again, correct me if I am misrepresenting anything.

My understanding is just like the

two percent enrichment assumption which is conservative as applied to the site, you're going to assume that all uranium process is recycled uranium with the mix identified on page 11 of the report that I circulated to everyone. So my starting point is page 11.

MEMBER PRESLEY: What date did that come out, John?

DR. MAURO: Pardon me?

MEMBER PRESLEY: What date?

DR. MAURO: This report is dated March 2009.

MEMBER ZIEMER: John, why don't we have a specific day on these last couple of reports?

DR. MAURO: That's on the bottom in the footer. It says March 23rd, and the cover says March.

MEMBER ZIEMER: Okay, I've got you.

DR. MAURO: I believe page 11 --

MR. STIVER: John, could you

possibly resend them. Do you have it in email form that you can send it to me?

MR. MAKHIJANI: I can send it.

MEMBER ZIEMER: And before you go to page 11 --

MEMBER PRESLEY: Arjun, put me on the distribution list, please.

MEMBER ZIEMER: I just have a question, on page 10 you talk about Table 4-3.

DR. MAURO: Yes.

MEMBER ZIEMER: Now I had trouble finding --

DR. MAURO: Okay, I can see where you are referring to.

MEMBER ZIEMER: It's the last paragraph 10. It says in Table 4-3 reproduced above.

DR. MAURO: There's obviously some mislabeling here.

MEMBER ZIEMER: Is that 3-3? But if it's 3-3 -- well, in the other table I couldn't read what -- on my copy I couldn't

read the items, so I --

DR. MAURO: How is the scanned information?

MEMBER ZIEMER: On 3-7 --

DR. MAURO: Yes.

MEMBER ZIEMER: -- it didn't show up, so I'm not sure what those columns were, so I couldn't --

DR. MAURO: Yes, you're right. I'm aware of that. I'm going to have to clarify that for you.

MEMBER ZIEMER: Okay.

MR. MAKHIJANI: I am just trying to send off the email.

MR. MORRIS: What you can read on your screen is not readable on the printer.

MEMBER ZIEMER: Well, that part, but when it refers to Table 4-3 it says that it contains data for zirconium niobium-95 for the first five months of '67.

Now if you look at Table 3-3, I thought at first that was the -- just

mislabeled. I don't see anything about zirconium niobium there.

MR. MAKHIJANI: It's called Table 10 in the text above. It's a pasted in table from that source, NIOSH 2008. And zirconium niobium, it's on page 11, and the zirconium niobium line is the second last line.

MEMBER ZIEMER: Okay, I was going back and looking above.

MR. MAKHIJANI: Yes -- no, just below that sentence. In my computer at least it's on the next page.

MEMBER ZIEMER: I got you.

MR. MAKHIJANI: For set total uranium --

MEMBER ZIEMER: All right, yes, yes, okay.

MR. RICH: John, this is Bryce Rich.

DR. MAURO: Yes.

MR. RICH: Quick question. You're going to be presenting the SC&A's

review of the white paper?

DR. MAURO: Yes.

MR. RICH: We've developed a response to your findings which is still in review. Do you want comments during the time that you're presenting these points or --

DR. MAURO: Sure.

MR. RICH: -- or do you want to wait until --

DR. MAURO: No. I mean, let's talk about it.

MR. RICH: I just wanted the board to know that they will be getting a formal response, and a lot of these points that are being made I think which you plan to discuss today, I think there's a logical response that should be discussed and would probably be better once the formal report is issued to the board.

I just wanted the board to know that there's a formal response -- is hanging in the balance here.

DR. MAURO: Well, from my perspective if you have information to address each of the 11 issues, that would be great. Let's talk about it and, of course, that would be followed up by your written response. That's fine, let's talk about it.

MR. ROLFES: Yes, Bryce, this is Mark. Please jump in with any response. I know that you and Paul have been working on this quite a bit, and I haven't had the opportunity to speak with you in detail about it. You are, in fact, working on it, so please jump in with any new information that you might have to discuss.

MR. RICH: Will do.

DR. MAURO: I guess -- basically, we have 11 findings, but they can be grouped. The first couple deal with inconsistencies -- let me step back.

Our understanding is the table that we're looking at that was used to build in effect your co-worker model, your default

set of mix of RU material was based on a couple of DOE reports that -- and we reviewed those reports. And we are finding that the data -- the reports, and not the data -- we don't have access to the data -- but our review shows that there's inconsistencies in quantities of material, amount of recycled material, where it came from.

So it looks like there are substantial differences in the historical record of the amount of materials shipped from various places, primarily Hanford, to Fernald.

Now that in and of itself is just indicative that since everything is based on the DOE records and that's the way Richard came out with your RU numbers -- the fact that there are very large discrepancies in that information led us to the point that --

MR. RICH: John, let me comment there.

DR. MAURO: Yes.

MR. RICH: It is indeed -- well,

let me -- let me step back a couple of points. The decision that DOE, or AEC made at the time to recycle uranium, that was a conscious decision and criteria were set up -- the specifications for the contaminants was determined carefully and iterated. These specifications between primarily Hanford because they were the first in the Oak Ridge complex.

There was no criteria given for making the determination of what constituted recycled uranium, and so a number of plants, and Fernald being one of them, made the judgment that once recycled uranium hit the plant then everything was counted as recycled uranium, even though they were in the very early days processing metric tons of ores and producing natural uranium that had no recycled materials at all.

And the -- consequently, the major effort that DOE went through in the most -- extending from 1985 to 2000 when the public

reports were published, they recognized almost immediately that there were some discrepancies in the mass quantities of material that was moved back and forth from the sites.

They initiated a three-year study and published another report in 2003, which clarified an issue -- and by the way that report in your report is the -- I think it's -- let me see -- well, it's the colored table on page seven, which is the Fernald receipts data, and that comes from the 2003 DOE report which clarified only the primary shipments from the primary shipping sites, which was Hanford, primarily -- Savannah River, and a little bit from West Valley, and a little bit less from the high enriched uranium processing plant at the Idaho Chemical Processing Plant.

The -- those shipping uncertainties were cleared up in that report. The max LOEL between sites has not been clarified, and so there are discrepancies. Those discrepancies have been explained and I

think clarified in the white paper, the differences in what they mean and constitute.

Just to make one additional comment, the dose reconstruction approach is based on determining a ratio of uranium to the contaminants, and it's not really based on max LOEL but on a confidence level that we know the ratios. Those ratios were very well documented at the shipping sites because they were required to by regulations.

And so I'll just make those statements at the beginning, John, so that perhaps we don't need to spend too much time on the fact that more uranium was shipped back and forth that may or may not have been recycled uranium.

MR. MAKHIJANI: Can I make a couple of comments?

DR. MAURO: Sure.

MR. MAKHIJANI: Yes, I think -- you know, some of this stuff was cleared up in the white paper from our previous comments

that were made in the review of the site profile, but some were not cleared up. And the different kinds of discrepancies that are there in the first couple of findings, one is the starting date.

Now as I read the white paper, you're performing the start -- assigning these doses in 1961, and our report shows that recycled uranium exchange between Hanford or other sites and Fernald started in '53 or '54. So that's one discrepancy. The statement in the white paper is that there were very small shipments prior to '61, so presumably inconsequential for dose.

MR. RICH: Arjun --

MR. MAKHIJANI: Yes.

MR. RICH: Is that Arjun?

MR. MAKHIJANI: Yes.

MR. RICH: Okay, let me respond to that. You're right as a matter of fact that, again, the daily 2003 report clarified that, and the table that has been reproduced

from that 2003 report is on page seven, and that indicates that they started shipping small quantities of five metric tons in '58 and --

MR. MAKHIJANI: Yes, but that is contradicted by the tables from DOE 2000 that are reproduced farther down.

MR. RICH: As I said, Arjun, the 2003 reports and particularly the shipping reports from Hanford were corrected by 2003.

MR. MAKHIJANI: No, no. No, no. It's -- hold on. The 2003 report shows absolutely no transactions before 1957. If you go down and look at page eight of our report and page nine you will see there two reports that says -- these are DOE just pasted in the table -- Hanford summary shipments to Fernald.

And you look at that it will say -- it shows July 1, 1954, to 30 of June 1955, you can't see -- read the top lines, but they're really natural uranium, enriched

uranium, and depleted uranium I think is what those three columns are up there.

You'll see 266.2 metric tons were shipped from Fernald to -- from Hanford to Fernald in fiscal year 1955, and if you look at the next table you'll see Hanford received from recycled uranium from Fernald. You'll actually see an item in fiscal year '54 of 2,735 metric tons of natural uranium of Fernald's shipments to Hanford.

So this -- these transactions must have started almost as soon as Hanford started recovering uranium from the high-level waste tanks.

MR. RICH: Arjun, shipments back and forth between Hanford and Fernald did occur prior to 1961. That's not in question.

The issue is was recycled uranium sent back to Hanford, and did Hanford send recycled uranium to Fernald?

MR. MAKHIJANI: That's what it says here.

MR. RICH: The table says recycled uranium, but that's the recycled uranium report. That does not mean that those shipments were recycled uranium, per se. And that's what I'm saying is that the DOE 2003 report corrected the definition of recycled uranium for -- primarily for the shipments from Hanford to Fernald.

Now I remind you that the UO3 is heavy stuff. A 55-gallon drum weighs about 900 pounds or so, and so the -- they did receive, but it is a consistent report in the entire Ohio report and the 2003 mass balance report that they did not put into process recycled uranium until 1961. That was validated, verified by talking with knowledgeable professionals whom we interviewed specifically to that point.

MR. MAKHIJANI: Well, I obviously wasn't there at the time. All I'm pointing out is when you look at the DOE 2000, the title of the report above the table number

says recycled uranium. It doesn't say uranium shipments. It says recycled uranium, Hanford shipments received from Fernald.

MR. RICH: Arjun, that's the title of the section.

MR. MAKHIJANI: No, no, no. It is not. Let me assure you it is not. I have the DOE report and can certainly send it to everybody.

MR. RICH: I have it right in front of me -- section three, recycled uranium, and then it starts out to talk about what they're defining as the shipments in the recycled uranium period.

And what I'm saying again is that the daily 2003 report is the one that we have accepted, and that is the one that corrected the definition of what constituted recycled uranium, based on the year '03 time and Hanford, and then went straight to Fernald.

DR. MAURO: Based on this conversation, I may have given some

misinformation. I was under the impression that the recycled uranium mix, notwithstanding the debate of when that started. I guess I was under the impression that you were universally going to assume it's all recycled uranium, but I guess I'm wrong.

Right now your co-worker model or your model -- it's not really a co-worker model is not to assign those recycled uranium until 1961. Just by way of clarification, because I may have -- I may be wrong.

MR. RICH: The recommendation, John, is that since there's sufficient evidence to indicate that they didn't process recycled uranium at Fernald. And by the way there's in our formal response we have extracted several -- specific information from the Ohio report that indicates that -- and that's a consistency that they did not process recycled uranium until 1961.

Now it would be a simple thing to extend that to the --

DR. NETON: Bryce, this is Jim Neton. I've got a couple of questions. Maybe I can shed some light on this.

You said that there was no consistent definition of recycled uranium. Could you expand a little bit on that because we ran into this problem at other facilities where they were calling recycled uranium essentially any uranium scrap to have been gathered from machining and such and then gone back, remelted and reused. That was also considered early on in the forties recycled uranium, not to be confused with recycled uranium that had originated and been irradiated in a reactor.

MR. RICH: That's correct, Jim. That's one of the problems.

DR. NETON: And that's one of the problems.

MR. RICH: But even beyond that the issue of -- once the recycled uranium from the generating site hit the plant, some of the

plants simply defined every single -- all the inventory in the plant as recycled uranium.

And in the case of Fernald they were generating natural uranium specifically from '53 to '62 period of time in thousands of metric ton quantities. And they defined all of that as recycled uranium, but it didn't, you know -- and producing uranium metal parts for Hanford from that site.

DR. NETON: It seems that we have got definitional issue here.

MR. RICH: What we've done there is, without trying to resolve this, just simply accepting the fact that there is discrepancy in the definition of recycled uranium.

We have a surety from the three-year review by DOE that the -- and they intended to extend that to the secondary shipment but didn't get that done.

But we have a fair degree of confidence because of the extensive review

later that they knew exactly what came out of the UO3 plant at Hanford and went to the other sites, and that then qualifies as recycled uranium, and that's the only uranium that inserted the contaminants that we're talking about into the system.

DR. MAURO: Then am I correct that you're not going to assume recycled uranium beginning from the very beginning of operations, even though it assumed recycled uranium?

MR. RICH: It is the recommendation of the white paper that it need not be considered prior to 1961.

DR. NETON: That is not represented.

DR. MAURO: Okay, that corrected my previous statement. Thank you.

MEMBER ZIEMER: Bryce, Paul Ziemer here.

MR. RICH: Yes.

MEMBER ZIEMER: Could you --

we're trying to pull up this report here, Mark and I -- or Mark is mainly, but what -- what's in the report that we're looking at from SC&A it's called Table 3-7. I guess you have that report; it's on page eight of the report, where it says recycled uranium did I understand you to say that that was the title of the chapter from which this table was extracted?

MR. RICH: Yes.

MEMBER ZIEMER: So there's a chapter called recycled uranium?

MR. RICH: Yes, that's section three.

MEMBER ZIEMER: And then there's some other tables and then -- and some narration, and then this table appears --

MR. RICH: Yes.

MEMBER ZIEMER: -- which is a summary of shipments, and the table title has nothing about recycled in the title of the --

MR. RICH: Well, initially --

when Hanford put out their mass balance report as part of the overall DOE effort they -- it was a recycled uranium report.

MEMBER ZIEMER: Well, I understand that. Yes, I was just trying to clarify, because I think we originally thought that the table had as part of its heading recycled uranium.

MR. RICH: And they could have intended that because of the fact that they recycled. You know --

MEMBER ZIEMER: I see what you're saying.

MR. RICH: They got, as Jim pointed out, they got --

MEMBER ZIEMER: The broad --

MR. RICH: -- natural uranium metal parts from Fernald --

MEMBER ZIEMER: Yes.

MR. RICH: -- and then they processed it and had a bunch of scrap after they'd made the fuel elements themselves, and

they sent that back.

MEMBER ZIEMER: Got you.

MR. RICH: So they recycled that. It was not recycled uranium in the sense that we --

MEMBER ZIEMER: Got you.

MR. RICH: It came out of the UO3 recycled uranium plant at Hanford. And so the consequence, there is legitimate confusion about what -- how much recycled uranium, but the 2003 cleared that up, at least how much was injected into the system. And that's based on recorded analysis, primarily plutonium but neptunium and technetium and they did make gross -- right from the very start when they started shipping from the UO3 plant, they made gross beta and gross gamma analyses and shipped it gradually to -- well, that's a topic specific on gross -- on a fixed amount of uranium samples compared to aged uranium.

MEMBER ZIEMER: Yes, thanks,

Bryce.

DR. MAURO: Well, good. It sounds like that there's a response to our concern about this confusing information.

MR. MAKHIJANI: We'll just have to look at it.

DR. MAURO: We'll have to look at it.

MR. MAKHIJANI: And I need to find the reference from which that thing was taken.

MR. RICH: Those come from section three.

DR. MAURO: And we -- by the way, we also agree that the real issue is the mix, notwithstanding --

MR. MAKHIJANI: I'm not finding it in the Ohio field office report. It might be a numbering mistake.

MEMBER ZIEMER: Is the DOE report -- is that the one out of the Ohio field office, Bryce?

MR. RICH: Yes.

MEMBER ZIEMER: DOE --

MR. RICH: No, no, it's the one on the Hanford field office.

MEMBER ZIEMER: Okay, so it's SRDB ref IB --

MR. RICH: BR 2003 according to --

MEMBER ZIEMER: The June 30, 2000, report?

MR. RICH: Yes, June -- well it's a July 5th is the date on the CRL report.

MEMBER ZIEMER: I'm actually looking at SC&A's references, so maybe they didn't cite this one.

MR. MAKHIJANI: I know that we used the same reference as the white paper, to be not confusing.

MR. RICH: I see. You're talking about the --

MEMBER ZIEMER: I was again trying to find the report that the table is

came from. I think it's the DOE report.

MR. RICH: It is the DOE --

MEMBER ZIEMER: Is it the 2003 report?

MR. RICH: Two thousand A report.

MEMBER ZIEMER: Here it is. Okay, got it. Thanks.

MR. RICH: It's the --

MEMBER ZIEMER: Review of Generation and Flow of Recycled Uranium at Hanford?

MR. RICH: Right.

MEMBER ZIEMER: Yes, good. Thanks.

MR. RICH: By the way, these are very lengthy documents, thousands of pages a piece, so --

MEMBER ZIEMER: Yes, we won't read them into the record.

MR. RICH: Thank you.

DR. MAURO: The real issue, the more direct issue is the mix, and I think --

again, looking at Table 10, page 11 of our report, the question becomes -- in that column where it says mass concentration of parts per billion uranium, we looked into that to see, okay, is the literature on which that -- those numbers are based, does it make a compelling case.

And what we found is as follows: Clearly, the 100 part per billion number -- when you look over the entire duration of when recycled uranium was being handled, that number overall is a sound number to represent -- for example, if a person were working there for an entire time period, assuming that all other -- let's say '61 on -- assuming one hundred parts per billion would probably be claimant favorable because you've demonstrated what the data in general shown that the parts per billion of plutonium is generally less than that, except there are some exceptions.

And this is where we felt we a hard time convincing ourselves there may have

been time periods and locations where people might have been exposed to higher values, and we could not discern.

There were two reasons we say that, two reasons. The first is in going into the reports that stand behind us, we were not able to get outstanding data that -- one of the inquiries we made is that --

MR. RICH: John, I can't hear you very well.

DR. MAURO: When we were doing our work on this one of the things we were hoping to look at was the original data, the data set that was used by DOE to come up with their reports. We really had to go to the original data, that really only had are the reports, the DOE reports themselves which even though they are large reports, they don't actually give you the original data upon which these numbers are based.

So that was one -- something to look for to convince ourselves that that 100

number was a well-founded number.

DR. NETON: Are you saying that there were periods of time where there were greater than 100 parts per billion plutonium at Fernald?

DR. MAURO: And there were people working on it for protracted periods of time.

MR. RICH: John, I'll make another comment at this point. The Ohio report, of course, dealt with the historical levels of these contaminants primarily plutonium, neptunium and technetium were dealt with and the analytical, the statistical analysis was dealt in Appendix F and F-1, and I think you guys have looked at that. And the -- what they did in those tables is they listed the very maximum sample that they ever got and the minimum, and then they had -- because of the fact that it was not a standard distribution -- there's wide variation to the sample in all of the process streams. They used the boot strap analysis technique.

The reason we settled on 100 parts per billion was -- of plutonium, just using that as the example, was that it covered even the maximum of most of the streams, with the exception of several streams that were identified as the -- what they call the receipt of the POOS on a plutonium over specification.

Let me go back a step just for clarification and say that in 1964 they were running short of uranium and they decided to reprocess the plain tower tail from the gaseous diffusion plants for recovery of uranium.

Fernald and others objected to that. Whitetail got some of it and they simply buried most of it and sent the rest back, but Fernald did take it with the intent of blending it into the rest of the stock. It doubled the inventory of plutonium specifically in the plant. They got -- received two shipments from '64 and another

set in the eighties.

And so the analyses reported in the Ohio report, by the way, was exhaustive and it covered the highest level of contamination in the plants.

When they brought those high level tails from -- they came in as sealed containers and then, of course, they were anxious about them and so they really used very, very careful operating techniques and blended them as soon as they could.

It turns out that there were a few barrels, a little bit of it that continued to be on site of those high level tails from the gaseous diffusion plants. I might just add too, parenthetically, that when you convert uranium to the US6 -- uranium US6 at high temperature is volatile. Plutonium is not, and it falls out. Ninety-nine percent of the plutonium falls in those flame tower tails and as a consequence plutonium goes through the gaseous diffusion plant comes back out in

parts per trillion as opposed to parts per million, and that's something to kind of remember as you get some of the enriched stock from the plutonium -- from the gaseous diffusion plants.

DR. MAURO: Well, I guess -- we talked -- the reason this is coming up is there was this tower ash --

DR. NETON: The Paducah Feed Plant ash came in and it was blended, as Bryce indicated, so that none of the production workers were exposed to the concentrations -- none of the main production -- uranium production workers were exposed to those levels of concentration.

DR. MAURO: At our last meeting --

MR. ROLFES: Most importantly for that data set, for those workers who handled that material, they all participated in a specific plutonium bioassay program, so --

DR. MAURO: No, we covered the

tower ash very well --

DR. NETON: Yes, I thought we had done that.

DR. MAURO: Not only that the workers that dealt with that were wearing respiratory protection --

MR. RICH: Yes, they were and airline a good share of the time.

DR. MAURO: And we're okay with that. That's not the issue.

MR. RICH: But what I want to say is that this Table 5 in our white paper is the recycled uranium summary by the process subgroups, and in looking down through there you see a couple of them that are fairly high, but even those are pretty well covered by the 100 parts per billion, not the highest values that you'll find in Table F-1 in the Ohio report, but it's -- but for the average process streams --

Plus there's -- as a process enriched uranium, it turns out that the

majority of the recycled uranium that came into the plant was in the form of enriched uranium. When they actually reduced it to metal in Plant Five, the magnesium fluoride sucked up the plutonium and that was one of the higher process streams. They reprocessed the magnesium fluoride and -- for the recovery of uranium because it was enriched. If it was not enriched it was below economic recovery limits and they disposed of it in the pits.

But the magnesium fluoride reprocessing was one of the process streams that showed higher levels, and that would have been run through a mill in Plant One, for example, the Titan Mill, and broken up into particles of a size that could be run through the recovery plant.

DR. MAURO: The special cases that you are making reference to, we agree with. But then we -- then we -- part of the mission we received from the last meeting was to look at this boot strap analysis.

MR. RICH: Yes.

DR. MAURO: Now -- so I'm not disagreeing with anything you're saying about these special cases, so we could -- we agree with that.

But then we looked into the boot strap issue--and boot strap means how did you take the data--how did DOE take the data to come up with the concentrations. I'd like to direct your attention to page 23 of our report. I'll give you a chance to open it up.

And what we did is we looked at the data. Harry Chmelynski might be on the line; he helped us with this. And we're finding that the data that you had followed along normal distribution, and when we derived the mean of these various groups, 1A, 1B, et cetera, you could see -- if you look at the table there are some rows that are in green. Okay, on page 23 it's -- it's Table A-1, if everyone has it in front of them.

And we're seeing a fairly large

difference between the mean that we would get versus the mean that is reported, that was derived using what we're referring to as the boot strap method.

Now in speaking to Harry about what is this boot strap, it was our -- it was my understanding that this was a way to deal with outliers, and so we see a little bit of a incongruity between the mean that we -- the ratio -- at least with 1-A we get a 5.1 times higher mean, and the same thing goes for 8, 9, and 10-A. We get a substantially higher mean than the boot strap method does, which starts to bring us --

Now maybe I got this wrong, but it appears to bring over the 100 parts per billion.

MR. RICH: Well, again, let me draw your attention to 10-A is the tower ash and decon residue.

DR. MAURO: Okay.

MR. RICH: And Group A is -- is

the enriched magnesium fluoride that I just mentioned.

DR. MAURO: Okay, so you're saying the -- this is important. Now we're getting to the bottom of this.

MR. RICH: Yes, so what I'm saying is that we were satisfied that even whatever statistical analysis you used we were pretty well covered with the 100 parts per billion.

DR. MAURO: Okay, so what I'm hearing is that the 1-A, 8, 9, 10-A, which where we're getting a mean that's higher than the boot strap mean, the reason is that when you did your boot strap the -- the -- these very special cases that are -- that were-- that you described earlier were taken out of the data because it was dealt with separately and under a very controlled circumstance so, therefore --

MR. RICH: When we established the 100 parts per billion, John --

DR. MAURO: Yes.

MR. RICH: -- we considered the fact that those streams, number one -- well in the first place when they did the statistical analysis using the boot strap mean it will come out with different analysis techniques a little bit higher, that's true. But these were processed streams that had an extraordinary amount of care when they were currently being inserted into the dilution system.

And so we -- we, frankly, were not worried about those streams because of the fact that they are well known and well controlled.

DR. MAURO: Okay, so -- so our derivation of the mean where we included all the data -- we shouldn't have done that.

DR. NETON: You can do whatever you want.

DR. MAURO: We can do whatever we want. We did that, and for good reason.

It answers my question, because quite frankly I didn't understand why we were coming in five times higher, which puts us well over the, you know, one hundred.

MR. MAKHIJANI: Well, from what we understood NIOSH did not actually do its own analysis. They used the analysis in the DOE reports which contains this boot strap mean, and that you used the numbers in Appendix F of the Ohio Field office report --

DR. NETON: That's correct.

MR. MAKHIJANI: -- directly from that. You did not look at the raw data, and you didn't do your own analysis.

DR. NETON: John actually called you about that or sent you an email about that.

MR. RICH: Yes. We looked at it and considered that, but quite frankly, you know, the majority of the contaminant levels came in less than five parts per billion, and most of it from the gaseous diffusion plant

came in under parts per trillion level, but where --

DR. MAURO: Okay, when did the first --

MR. RICH: -- we dealt with defaulting to the highest reasonable level and without really going overboard in these special streams.

MR. MAKHIJANI: When is the first document that we have where we have a measurement of trace contaminants. I mean, this Paducah thing that's on was in the seventies and eighties, and I know there were shipments, there were measurements, there were all these precautions that were taken and, you know, especially in the eighties. I think this Paducah thing was in the eighties.

MR. RICH: Right.

MR. MAKHIJANI: When is the earliest actual site measurement? Hanford ships recycled uranium. Here's the label. Here is the plutonium that was in it that's in

a document from the time.

When I looked at Appendix F I saw a lot of surrogate data, data from--assuming that this shipment --

(Simultaneous speakers.)

MR. RICH: Most of that's from a later period during the higher level period, Arjun.

MR. MAKHIJANI: So all --

MR. RICH: Pardon me?

MR. MAKHIJANI: I'm not aware of early data that's documented that says --

MR. RICH: In the early days the -- the responsibility for defining the contaminant concentrations were the responsibility of the shipping sites.

MR. MAKHIJANI: And so do we have like a Hanford document that says --

MR. RICH: Yes.

MR. MAKHIJANI: -- we're shipping X to Fernald.

MR. RICH: The 2008 report is

some documentation of the historical levels in those early times.

MR. MAKHIJANI: Well, speaking of the --

MR. RICH: Some of those are summary data.

MR. MAKHIJANI: Could we go back on the list of that 2008 report? The 2008 report is about recycled uranium that contains trace contaminants. That's what it says on page one.

MR. RICH: That's true.

MR. MAKHIJANI: And then at the start of chapter three, section three, actually recycled uranium that head appears on every single page, and at the top of page one of section three which I have here -- I just downloaded it. I couldn't find it in my computer.

Section three affirms that this chapter is about recycled uranium in the sense that we're talking about it here.

MR. RICH: Then I'll go back and say that a report issued by DOE three years later and identified as DOE 2003 corrected the -- well, the primary RU shipments.

Now you'll notice in the second sentence it says the transactions into and out of Hanford were focused on the 300-A Pugh Fabrication Complex that were used at all three plants.

MR. MAKHIJANI: The first line in chapter says, "This chapter is designed to quantitatively define the recycled uranium flows to and from Hanford. The transactions into and out of Hanford will focus on 300 area fuel fabrication complex."

But the whole thing is about recycled uranium.

MR. RICH: Initially it was so. It was corrected by the 2003 report.

DR. NETON: I mean, Bryce, is there definitive language of the 2003 report that speaks to that?

MR. RICH: Yes, the report does speak to that.

DR. NETON: I mean, if it does, as a later report, I fail to see why we wouldn't accept that. I mean, we have a 2000 report where it's been superceded and there's language in there if we can find it that says that it corrects what was possibly an error in 2000. I mean, why --

MR. RICH: Initially, when they put out in the 2000 report it was a matter of definition of what constitutes recycled uranium.

DR. NETON: I fail to see the argument there.

MEMBER GRIFFON: He didn't go back to the raw data because --

MR. RICH: No.

MEMBER GRIFFON: -- it was too difficult or --

DR. NETON: I don't know, Mark, you need to --

MR. ROLFES: Once again, I mean, it's a matter of timeliness on re-evaluating data that's already been summarized for us. The bottom line, getting into the recycled uranium issue is really very unlikely to affect a significant number of compensation decisions, if any. Bottom line, we need uranium bioassay data to reconstruct intakes and make a good balanced and professional decision on the information --

Go ahead, John.

DR. MAURO: I think that -- let's say we're dealing with 100 parts per billion versus 50 versus 200, okay --

MR. ROLFES: Right, right.

DR. MAURO: Now what happens to the dose, to some of the organs when you change that assumption. I think you have to think of that.

MR. ROLFES: It can for certain organs.

MR. MAKHIJANI: Moreover, it's

not just about plutonium and trying to --

DR. MAURO: Yes, we haven't gone there yet.

DR. NETON: Let's decide first whether or not we're going to use the fact of this 2000 report that's been superceded as evidence of what the plutonium concentrations were, or we're going to rely on the 2003 report that superceded the 2000 report. That's important to me --

MEMBER GRIFFON: Yes.

DR. NETON: -- and if SC&A opinion that the 2000 report is more accurate I'd like them to show me why the 2003 report is not.

MR. RICH: And beyond that, Jim, we have used the 2000 report from Hanford because it's a wealth of information.

DR. MAURO: That's right.

MR. RICH: My primary correction is primarily in the mass flow data, and, by the way, I'll remind you again the mass of

uranium is not at issue so much as the ratio of the material.

Now because of the -- the inventory control or the shipment control regulations, they did analyze every -- well, as a matter of fact they analyzed the product from U-plant and PUREX, and any other plant that contributed products to the UO3, which is a uranyl nitrate reduction to UO3 for shipment, and those were all analyzed prior to the point they were accepted by the UO3 plant.

If they didn't meet specifications, they sent them back to the extraction box. That was very carefully controlled.

DR. MAURO: And that's from the very beginning?

MR. RICH: That's from the very beginning, right from the time that they decided to send the first barrel out.

DR. MAURO: Which is '61 as opposed to '57 or '58?

MR. RICH: That's true.

MEMBER GRIFFON: Bryce, just a little background, wasn't there an Ohio Field office mass alance report also? I can't seem to find that one.

MR. RICH: Yes, that's the one that we're reporting as being the Fernald mass balance report.

MEMBER GRIFFON: Okay, okay.

MR. RICH: The Ohio field office report covered RMI, West Valley, a number of other sites in the Ohio Field office.

MEMBER GRIFFON: And then I'm trying to remember, but you're very familiar with these reports obviously, but I seem to remember that you said that the shipper usually in the early years especially characterized the contaminants.

MR. RICH: That's true.

MEMBER GRIFFON: I remember with this ash waste there was a big discrepancy between the Paducah numbers and the Fernald

reports.

MR. RICH: That's true.

MEMBER GRIFFON: How did you weigh -- how did you come down on those?

MR. RICH: At that later time period, of course, and because of the fact that they were shipping known higher level contaminant level stuff they analyzed it at both ends, no question.

And at that period of time they did more analytical --

MEMBER GRIFFON: Well, there was a big disparity in the numbers, and I guess that's my point is -- Jim had asked me why don't we accept the 2003 numbers. Why don't we not go back to the raw data. You know, this is part of my reasoning because I looked at those reports years ago and you have these discrepancies, how do you handle them?

MR. RICH: Well, and then the characteristic of those flame tower tails that had accumulated over a number of decades, they

were not uniform in and of themselves, and as a consequence there was a -- a considerable amount of variability in the sampling technique itself, and part of those were sampled in -- it was mixed in Plant One.

DR. NETON: Right, but I thought the feed plant issue was not necessarily on the table because we recognize it was a separate stream. It was --

MEMBER GRIFFON: I guess the point I'm making is --

MR. RICH: It was indeed blended down and then analyzed again, but they analyzed the stuff that they got. They were highly concerned about it.

MR. MAKHIJANI: Well, the specific numbers that are derived in this boot strap analysis and that are in the white paper are not from the 2003 report, which doesn't contain this information.

MR. RICH: No, that's true, Arjun. The numbers are in the Ohio -- or the

Fernald report.

MR. MAKHIJANI: And those are all from the year 2000 which was part of the same series of recycled uranium analysis that was done in 2000. The later report is 92 pages and it covers a whole nuclear weapons complex and contains almost no detail.

The -- all of the detail is in the 2000 reports. Now if these 2000 reports were seriously in error to an order of magnitude --

MR. RICH: Arjun, let me remind you again the 2003 report corrected only the shipper's numbers.

MR. MAKHIJANI: Yes, but all of the concentration numbers, so we're saying that we're going to accept everything in the 2000 reports, much of which is surrogate -- which are assumed numbers from some other site because individual shipments are not characterized.

MR. RICH: Arjun, the numbers were lower as they left the plant, the shipper

-- the generating plant --

MR. MAKHIJANI: That's not a question.

MR. RICH: -- and the numbers in the early years were much lower than they were after -- until -- after the POOS material had been processed from the gaseous diffusion plant.

MR. MAKHIJANI: We've seen no early year actual data other than what's reproduced from literally some documents in these reports, which are --

MR. RICH: Arjun, admittedly we have accepted the analysis from that extensive -- the data was collected from 1985 to 2000, but it was a major effort by a large team at each of the plants in the year 2000 -- in 1999 and 2000.

And, no, I have not personally looked at all of the raw data. We -- I talked to a couple of the people, one specifically that served on the team that put that report

together at Fernald. He says as far as he knows the raw data is available. He's not sure where it is, but it probably would not have been disclosed.

MR. ROLFES: The bottom line is what -- what sort of impact will this have on a dose reconstruction. And I think that's what we need to keep in mind.

You know, we have different types of approaches for dose reconstructions. If an individual has uranium uranalysis we would use that uranalysis to calculate an intake , for example, for lung cancer.

If that claim were still under 50 percent probability of causation, we would also consider other sources, other potential intakes, for example thorium. We would apply intakes for thorium. If it was still under 50 percent we would consider other sources such as radon. If it was still under 50 percent I don't know what else we can do to put it over 50 percent. It gets to a point,

you know -- we can also take a look -- we are already accounting for recycled uranium components, the radiological contaminants that were sent in back to Fernald from the reactor sites. We're taking a look at that.

There was a requirement in the early days to maintain plutonium contamination levels under 10 parts per billion on a uranium mass basis. We've defaulted to an order of magnitude higher.

DR. MAURO: No, no, no. The 10 part per billion was what was shipped from Fernald to other sites. But Fernald was processing the material. The 100 parts per billion is -- is what we're -- is what's on the table here. In other words, is that a good default number for your recycled uranium.

The process by workers at Fernald from 1961 onward --

MR. ROLFES: Right.

DR. MAURO: -- and the reason -- well, there are a couple of reasons this issue

emerged. One is the boot strap. That was explained. In other words, when we wrote that boot strap was data. And we came up with a number that was five times higher.

So there's an answer. The answer is oh, no. When we did the boot strap we didn't include these extreme values because they were treated specially.

MR. CHMELYNSKI: John, can I interrupt a second?

DR. MAURO: Yes.

MR. CHMELYNSKI: You keep saying we did it, but we didn't. All we did was quote what DOE has in that report. It has in that report the numbers you need to fit the log normal distribution and to report the log normal results. It also has the boot strap analysis.

MR. RICH: And the data is plotted graphically as well as -- so, you know, it's a complete report.

MR. CHMELYNSKI: All we're

pointing out are some -- perhaps discrepancies or different answers that are obtained using the two methods.

DR. NETON: Right, and I think that the bottom line is still the same as John indicated though that there are reasons why we went with 100 versus using the entire set of data because of these special campaigns that were processed.

So I think that's okay. I'm hearing more fundamental distrust by SC&A of the things they feel they have some need to go back and look at the actual raw data set that exists, and, frankly, I don't know if we can find it and how much work that would be to obtain that.

MR. ROLFES: Getting back, you know -- literally there's a small population of claims that this, once again, is going to be applicable to because if we have a claim that hasn't achieved 50 percent probability of causation using intakes reconstructed from

uranium, from thorium, from radon, from medical x-rays, from external exposure -- you know, one additional thing which, you know, we're arguing over something that really is not going to be a significant -- you know, alone it is if we are solely using that as the basis for dose reconstruction; however, there are many other sources of other information where there are more, you know, more first-hand information, more likely exposures, for example, to uranium than a contaminant that a worker may not have been exposed to, and not at the level that we've assumed in our technical basis document.

We have additional sources of bioassay data to use that we could reconstruct someone's plutonium intake for -- for the POOS material, the out-of-specification material, but what I guess I'm getting to is the assumptions that we make in a dose reconstruction--off the bat when we interpret someone's urinanalysis data we assume a

constant chronic day-in, day-out exposure using that individual's bioassay data or reconstruct that uranium intake.

Then many of the other cases, for example, as we have pointed out for, you know, for 40 percent of the cases that we've completed -- excuse me, 40 percent of the dose reconstructions that we've completed for Fernald have been compensatory. Largely, those decisions are based on the individual's uranium bioassay data or the individual's monitoring data.

The cases that we have not been able to get over 50 percent probability of causation, we've thrown worst case scenarios which exceed, you know, exceed the credible amounts of uranium that could have been ingested, inhaled, critical amounts of thorium --

DR. MAURO: I understand, but, Mark, what you're really saying is that the assumption regarding 100 parts per billion of thorium is irrelevant, and, you know, it's

not.

MR. ROLFES: It's not irrelevant, but it's not going to have a large scale, huge impact on a significant number of claims. We're talking about a very, very few claims today. The entire -- the past, you know, the past several working group meetings, we're talking about a very, very small fraction of the Fernald work force that were potentially exposed to some of these what-if scenarios.

We're talking about very, very low odds of people being exposed to, you know --

DR. NETON: Yes, Mark makes a very good point. I mean, we were very conservative in our approach in being claimant favorable, but to get past this we have to decide whether there is a credible scenario that exposes workers at Fernald to greater than 100 parts per billion on a continuous basis outside of these areas that we identified as special campaigns.

That's the bottom line, and if

SC&A believes that it's well above 100 parts per billion and demonstrates that somehow we need to look into that.

DR. MAURO: We can't We can't demonstrate that.

MR. MAKHIJANI: Is that the right question? The -- I think for some of these batches, including some of the very high ones, we do have data, and I think whatever number you come up with there's some defensible number of doses that you could come up with, and it can be claimant favorable, assuming there's no supply there.

DR. NETON: Right.

MR. MAKHIJANI: There are a number of issues that that question doesn't cover. If you look at what happened in the 1950s at Hanford, which was the original site for recycled uranium, it was qualitatively different than what happened in the sixties and seventies in terms of how the recycled uranium originated.

At Hanford in the fifties, as you know, they started the U-plant operation in 1952, and that's sort of like a raffinate problem. It has -- all the plutonium had already been extracted from it. So you have -- you're processing a mixture of uranium and fission products first of all, so the whole question of whether plutonium is a key radionuclide on which to hang your hat for all the other trace contaminants is a very relevant one.

I don't think that plutonium is a key radonuclide, and that's one reason --

DR. NETON: Dosimetrically, I think it is.

MR. MAKHIJANI: What?

DR. NETON: I think dosimetrically it probably is. I mean, I looked at it --

MR. MAKHIJANI: Well, it depends on the relevant amount, say, of plutonium you have, relative to --

DR. NETON: Yes, go ahead.

MR. MAKHIJANI: In any case, you have a process difference which means that something that was part of a reprocessing operation where uranium and plutonium are being separated from each other after the fission products have gone. And in the earlier period where uranium efficient products are being separated from each other after the plutonium is gone. I mean, there are traces of everything that are left, obviously, but in the main.

So that sort of one whole set of questions that arises from that is do we have any data from the Hanford shipments of U-plant uranium and what was in it.

MR. RICH: Arjun, can I respond just briefly?

MR. MAKHIJANI: Sure.

MR. RICH: You're going to make a chemical processing -- the initial plant's business was separation, which was not a

liquid column separation. It was a -- it was a settling operation --

MR. MAKHIJANI: In-tank systems

MR. ROLFES: -- multiple processors. Then they went to a hexone system, which is a liquid-liquid column extraction system. That's the second generation system, and they were using that plant to separate both plutonium and uranium, when they decided that indeed they needed the uranium.

During the period of time from '47 to when they started in 1951, the stored the raffinates -- the uranium with the raffinates, and they refit U-plant with a third generation chemical separation which was TBT in an organic kerosene base. And that plant was PUREX, and it was the best that technology could provide and as determined by the DS for -- it cleaned up plutonium and uranium as well as could be done. That was the best technology available.

I started in '53 at the chem plant, and that was a hexone based system. They gradually changed it to PUREX. But the U-plant was the third generation uranium extraction system. They extracted the uranium in a slurry form out of the tanks. It had separated into a slurry and an aqueous stream and 72 percent of uranium was in the slurry.

The chemical processing for U-plant was the best technology that was available. It was a third generation. They blended that with the other plant, not the PUREX plant but the other plant, and the products were, again, analyzed as being acceptable to -- for feed for the UO3 plant.

There's no reason to believe that the U-plant process was incapable of providing the best separation of any of them, and so as a matter of fact I think they planned it for -- because it was good stuff and the other plant was -- the second generation plant was not so good.

So that also is a -- but, again, the -- the product for UO3 plant met specs and based in the very early days on gross beta and gross gamma for others than the plutonium.

And so I would say that even in the very earliest days they had a very good handle on the contaminant levels.

DR. MAURO: We've changed subjects, and that's good. I think that we've exhausted our discussion on 100 parts per billion, okay? We know where that is. What we've just done is say what about the other radio nuclides, because now we're saying that there are a lot of different ways in which the uranium was separated and processed.

MR. RICH: And my comments were directed directly to that.

DR. MAURO: I just wanted to make it clear that we changed subjects. And that's good, because I wanted to move to this other, which now means the neptunium, the technetium, thorium 232, ruthenium, these are the other

assumptions that are embeded.

Now I think what we've heard is that there is not a tight couple between the ratio of plutonium, neptunium, so it's not as if, you know, you would expect the relationships here to be labile.

What I mean by that is these ratios have been selected by NIOSH under the premise that it is -- represents a fairly bounding set of assumptions. We heard your arguments regarding 100, and I guess we really don't have -- I mean, I understand them now. And so it's on the table. Everybody understands the story, and I guess I don't feel there's any more I can add to it than what's already been said.

Now we're talking about these other radionuclides. Now what I just heard is that the separations process, the chemistry that we use, the columns changed over time which affected, I presume, the composition of the trace levels of various fission products

that were actually, some of these, activation products in that the eluent came off the separations.

Do you have data -- I mean, what I'm hearing is there were specifications, so the product that came out before it was shipped from Hanford -- these particular numbers that we're looking at, the 3,500 parts per billion neptunium, and let's go to ruthenium, which is 50 microcuries per pound of uranium.

Those -- those are -- are those the specifications? Are those measured values for various campaigns. In other words, you're obviously convinced that those are good numbers.

MR. RICH: Those numbers, John, are the -- based on the specifications, the maximum specifications that can be shipped for the fission product, you know the gross contaminants that would give you a gross beta or a gross gamma, you know, the strontium-90

or the cesium-137 or other longer lit fission products which would be the isotopes of most concern.

DR. MAURO: Well, we don't actually have like records of the actual measurements made.

MR. RICH: We do have after a period of time. I forget right now. I don't have the date, but they did ship from a -- a gross beta, gross gamma measurement with a -- they used a Shonka chamber to begin with, but then they switched to -- when -- again when the spectrometer became available then they shifted instead of the gross gamma to a spectrometer measurement in which they measured the specific isotopes.

DR. MAURO: You know, when we typically do a job like this, what we do is go back to the original data and we convinced ourselves, yes, it looks like we sampled from the data. We looked at some data from different campaigns, perhaps different time

periods and look at the results of the analysis of the material and say, yes, it looks like across the board these numbers are holding up.

We're really not in a position to do that. So what we're really doing is accepting our fate that yes, DOE, you know, did rigorously enforce that specification and, if that's the case, that's the case.

It's just an unusual circumstance here where we're sort of taking it on faith that those specifications were met, and we're not really in a position on behalf of the work group to go into the original data and convince ourselves, yes, it looks like that was universally the case.

MR. RICH: Some of that data is contained in the DOE 2000 and the 2000A report for Hanford Mass Balance Report, also in the Hanford Technical Basis documents.

DR. MAURO: Yes, I have nothing more to add.

MEMBER GRIFFON: I'm not sure where we take this at this point. I mean, I did -- I did pull up the Paducah report while we were sitting here and this is sort of what I had remembered the -- it's on Table 4.2-2 in the Paducah mass balance report.

And it says 1980 feed plant ash average plutonium concentrations in parts per billion and was 37 to 3,118. And these are the results from 16 hoppers analyzed by FMPC, so I guess that was sort of the Fernald analysis.

But you're saying this is that --

DR. MAURO: The special case.

MEMBER GRIFFON: -- special case that's --

MR. RICH: Yes, and that's very typical of that type of material that came from all of the gaseous diffusion plants.

MEMBER GRIFFON: Right, right.

MR. MAKHIJANI: What is the date of that?

MEMBER GRIFFON: This is the mass balance Paducah report --

MR. RICH: It's a 2000 --

MEMBER GRIFFON: 2000, yes.

MR. MAKHIJANI: The data that's sampled?

MEMBER GRIFFON: Oh, the data that's sampled? It's summarizing the 1980s, so I imagine --

MR. MAKHIJANI: You know, actually, the SC&A report said that beyond a certain date -- and I would suspect, I don't know, probably somewhere in the 70's or whenever from the time that we had these kinds of numbers based on measurements at the time, we can actually trace it that the stated ratios are probably claimant favorable for long-term workers when applied, et cetera.

The report actually says that. The questions are when you don't have that kind of information and you have lots of surrogate data, you have process differences

in how the plutonium was arising. You have differences, possible differences in ratios of the plutonium fission products, plutonium, neptunium, and so on.

If you look at the stack analysis that was done of the stack data that is in the white paper and you look at that, you see some stacks have pretty much fission products. Some stacks have, other than the plutonium, very little fission products, and this is a cumulative thing from 30 years.

MR. RICH: But, Arjun, what we've done from a philosophical standpoint is take a -- we used the data from the highest contaminated years.

MR. MAKHIJANI: So even if you look at the stack data, the analysis that's done in the white paper shows, you know, if you include the Titan Mill sample, which is after all a cumulative sample which was excluded from the white paper analysis, then you come up with a part per billion of

plutonium of more than 100 in an average, which is a cumulative average.

Now you could only come up with 14 ppb if you exclude the really high number.

MR. RICH: Now, Arjun, let me -- let me just tell you again. We included the effluent filter data primarily as an indication that, in a gross way, that the levels were not off by --

MR. MAKHIJANI: That's right.

MR. RICH: -- several orders of magnitude.

MR. MAKHIJANI: Yes, I understand.

MR. RICH: We did not use those numbers because of the fact that there is such a great deal of uncertainty associated with the finding those as being streams to which the workers are exposed.

MR. MAKHIJANI: Right, I understand that it's a kind of confirmatory exercise that you actually didn't use those

numbers.

MR. RICH: And as a consequence, Arjun, we did not feel that even the Titan mill, which was a process equipment and not a sampling equipment that -- that that number was higher, obviously higher that it invalidated the -- the other -- to make a conclusion.

MR. MAKHIJANI: Well, whether it did or not as a validation exercise or a confirmatory exercise is more iffy than what was presented in the white paper.

MR. RICH: But you see that that was, you know, one or two samples in a whole bunch taken across the plant, and if you're not going to use that to establish your ratio then, of course, this is a validation that the numbers are not too bad.

DR. NETON: Let me ask a silly question, I suppose. When Fernald was making uranium, I mean, we're assuming they would have 100 parts per billion plutonium in their

feed stock on a continuous basis.

DR. MAURO: Starting in '61.

DR. NETON: Starting in '61. But the majority of the uranium that they manufactured did not come through the recycling room; is that correct?

MR. RICH: That's true

DR. NETON: We have assessed what that ration is? I mean, in other words, you know, we're just assuming --

MR. RICH: During the maximum time that they were processing the high level feed from the tails from the gaseous diffusion plant, on occasion they did bump up against the 10 parts per million in products that they sent out.

DR. NETON: And that's sort of my point I guess is, you know, we've got an input term here we're trying to wrestle with. I mean, was it 100 parts per billion, was it more than that.

But we're also--they blended this

this stuff -- it's a small fraction of the total product being produced to begin with, so it's assumed to take these pure numbers and assume that the workers were exposed only -- essentially to recycled uranium is ludicrous.

MR. RICH: Though I'm convinced in my own mind that we're -- we've very conservative, at least by a factor of 10 for 99 percent of a worker population.

DR. NETON: It seems incredible to convince myself at least that the workers were chronically exposed to 100 per parts per billion plutonium throughout the life of the plant from '61 on.

DR. MAURO: As I opened up, remember we're always confronted with these problems and it's any aggregate. We don't have a big question. And what we really was probe, when I went with the boot strap -- the ratio of the boot strap, I said there's something here that doesn't ring true.

But I did know that there was a

special case with the tower ash, and it sounds like there were other special cases. There were a few special cases.

DR. NETON: There were a few excursions that were known in his --

DR. MAURO: Right and the reality of the situation is if all of those special cases were well in hand, then the boot strap method makes sense because you don't want to include those special cases because you used respiratory protection when they were handled.

So, I mean --

DR. NETON: When the workers were monitored for plutonium?

DR. MAURO: And they were monitored. So I guess, you know, in light of that, I mean, I have nothing more to say. It sounds like you make a pretty compelling argument for the 100 possibility.

I'll leave that up to the work group to make their own judgments. Well, we have nothing more to add.

The other has to do with the mix of fission products and whether or not that mix is -- of fission products--which is really separate because they're not linked.

Am I correct that the plutonium composition of the uranium and the other radio nuclides are not necessarily linked because of the way in which the uranium was purified by different methods at different times?

CHAIRMAN CLAWSON: That's true but we have sort of a default mixture that is developed -- the fission product contaminants were not developed as a ratio to the amount of plutonium, I don't think.

DR. NETON: I don't hear Bryce saying.

DR. MAURO: I've been thinking that, to tell you the truth.

MR. RICH: That's -- that's true.

DR. NETON: So you're incorrect. You have to have some kind of value to use. It's not -- this much plutonium there for

assuming this much fission products.

DR. MAURO: And throughout --

MR. RICH: And, again, for the inner isotopes, other than the ones that were -- yes, the transuranics, we used the maximum levels that were allowed to be shipped to the plant.

MR. MAKHIJANI: And for the -- and for the fission products?

MR. RICH: Those were the fission products.

DR. NETON: Again, you've got the question do they follow their own guidelines. I've taken the maximum value, meaning clearly there were shipments that were less than that, and we tried to bound them using whatever they could maximally allow.

MR. RICH: Most of them were less than that, but a considerable amount.

DR. NETON: Right. So we've got another level of conservative --

DR. MAURO: So what you're saying

is that it's very unlikely -- what I'm hearing is that the argument is, you know, even though our intent is to protect -- make sure that all workers when we reconstruct doses that we feel confident that we've -- have either a realistic or a bounding estimate of what their dose is, and the argument being that even though there might have been some short periods of time where you could have been high, in the long term maybe you'll request a year or more, it's unlikely that anyone's even going to approach these concentrations of dose periods.

MEMBER ZIEMER: And especially all of them all the time.

DR. MAURO: Especially all of them all the time.

MR. RICH: And the other thing to keep in mind too, the same products are probably about three orders of magnitude less in hazard level than the transuranics.

MEMBER ZIEMER: In terms of dose

per unit activity, Bryce --

MR. RICH: That's what

DR. MAURO: But of course the --

MEMBER ZIEMER: That's true for most organs, not in every case but --

DR. NETON: The orders of magnitude, you know, I've done these calculations and they contribute very little to the overall dose compared to things like plutonium.

DR. MAURO: Plutonium is the driver.

DR. NETON: It tends to be more uniformly distributed in the body --

DR. MAURO: I've got to say, I have nothing more to offer. Arjun, is there any more?

MR. MAKHIJANI: No, I think, you know, we're kind of discussing the -- in effect, we're discussing the paper that's in review in -- in ORAU NIOSH, and, you know, I have nothing more. I mean, it's really to the

working group as to where we go from here.

CHAIRMAN CLAWSON: Well, I think -- I first of all have got to see what -- see a white paper that NIOSH is sending us in response to them before we can go on.

MEMBER ZIEMER: Well, I think we've heard the points. Maybe we have to formally close it out.

DR. MAURO: Yes.

MEMBER ZIEMER: It appears that the practical impact is going to be pretty small -- of these issues. I mean, I think these are some valid issues -- whether they impact.

But what is it we need to decide with respect to recycled uranium, whether or not NIOSH has effectively --

DR. NETON: I would offer that it might be crucial to review the document that we submit. I mean, it might have some nuances in there that haven't been captured in this discussion.

CHAIRMAN CLAWSON: Well, and it might bring to light some of the confusion one way or another, because we saw this early on about the recycled uranium back and forth like that --

MEMBER GRIFFON: I don't think that there's any more actions, but I'd like to look. I'm not ready to vote and say close. I think we've -- I've got the arguments. I want to see the paper --

CHAIRMAN CLAWSON: That's fine.

MEMBER GRIFFON: -- and look at some of the background data a little more and maybe a few follow-up questions but no actions.

I mean, I still -- I'm going back to that Paducah/Fernald stuff, and it's not only the fact that there was this range reported which is very wide, but it's also that -- and I couldn't find it but I'm pretty sure that the Paducah side of the -- of the House Sample of these same things and have

very different numbers than the Fernald side.

DR. NETON: But again those -- those --

MEMBER GRIFFON: I know.

DR. NETON: -- the 10 parts per billion in process streams.

MEMBER GRIFFON: They're blended by someone, I imagine.

MR. ROLFES: Does it --

MEMBER GRIFFON: I guess in my mind -- I guess for me it also raises the question of well how solid are these other numbers that were assuming are accurate. Are they heterogeneous streams, are they -- you know, I don't know.

MR. ROLFES: It would only matter when you get bioassay data to reconstruct intakes of plutonium.

MR. MAKHIJANI: A couple of things you might consider -- I mean, looking at all the stuff and hearing what Bryce has said and what's in process, I think there are

no data from the early period that I've seen in terms of, you know, if the shipping site was responsible for, say, we're within the specifications and here are the measurements. Here's what we did. Here's what's on the barrel. It would be--presumably some documentation was generated. Undoubtedly, it was generated when there were inter-site shipments, and it really would be useful to have at least some kind of documentation.

The other thing that I think we didn't focus on. I just want to call your attention to it to see if you want to consider it and do anything about it.

If you look at the parts per billion data in the Ohio Field office report, a lot of them are surrogate data, that go into these average numbers that have been incorporated into the white paper.

Their data from other -- you know, we assume that this Paducah shipment was like this Oak Ridge, and if you look at the report

very large numbers of samples have -- the identical--9.16, 0.2, 412.77--because they have no data on those shipments.

Now I know we're looking at surrogate data in a different circumstance, but this is a real life practical example where you've got a surrogate data question that -- at least I want to point out that it is there, and it is pointed out.

MR. ROLFES: I'm not sure I follow what the numbers you were citing were, Arjun.

MR. MAKHIJANI: Well, if you look at the Ohio Field office report, Mark, in Appendix F where are a lot of these numbers are developed and the boot strap analysis was done and so on, you'll see that not every stream with their numbers has its own measurements, but it assumes that some streams of recycled uranium are like some other streams of recycled uranium for which there are data, and I'll try to pull up an example.

MEMBER ZIEMER: Streams from elsewhere?

MR. MAKHIJANI: Streams from elsewhere.

MEMBER SCHOFIELD: They're giving them generic numbers?

MR. MAKHIJANI: Not generic numbers, they're giving numbers from some known stream where it was measured.

MEMBER ZIEMER: And the surrogate data issue is one where for the number to be accepted there has to be a fair bit of similarity between the processes including the operation, the masses--the process.

MR. MAKHIJANI: And one of the points I think to consider, the DOE exercise was a mass balance exercise. It wasn't a dose reconstruction exercise. It wasn't an exercise to see something has to be claimant favorable. It was, you know, what happened and where did this recycled uranium come from. Do we have a grip on the order of magnitude of

the flow of the tranuranics.

MR. RICH: Arjun, could I just correct you on one minor point there?

MR. MAKHIJANI: Sure.

MR. RICH: The mass balance report was chartered with the objective of creating the data necessary to determine what the impact on the workers was. It was not specifically to do a dose reconstruction, I admit, but it was generated with the idea that it would provide the data to determine what the impact from a dose standpoint was on the workers.

CHAIRMAN CLAWSON: Well, I think that this is great, but I think I'd like to take just a 10-minute break right now, if that would be all right with everybody.

MEMBER ZIEMER: The action is that we'll review the NIOSH white paper.

CHAIRMAN CLAWSON: Right, we're going to review the NIOSH white paper.

DR. NETON: We need to deliver

it.

MEMBER GRIFFON: I guess we should have SC&A formally look at that white paper, so when we say we --

MR. RICH: I might just add one more thing. We do have an OTIB 53 which deals with recycled uranium in a general sense throughout the complex. That's being held up right now, but --

DR. NETON: It's in review.

CHAIRMAN CLAWSON: Okay, could we just take about a 10-minute comfort break? Would that be all right?

MR. KATZ: All right, so about a quarter of we'll start back up. I'm going to put the phone on mute, but we're not breaking the line.

(Whereupon, the above-entitled matter went off the record at 3:35 p.m. and resumed at 3:50 p.m.)

MR. KATZ: Folks on the phone, this is Ted Katz again with The Advisory Board

on Radiation and Worker Health, Fernald Workgroup, and we're just starting back up after a brief break.

CHAIRMAN CLAWSON: I guess first of all I just wanted to clarify that at the conclusion of our last conversations we were going to have SC&A review the NIOSH white paper that's coming out on the recycled uranium issue. Was there any other thing that we had, Paul, or that was it; wasn't it?

Okay, and I'll turn the -- John, we've got a couple of them here. Which one did we want to go to next?

DR. MAURO: Yes, well, we've got two, and it would be nice if we could do each within about 20 minutes to a half hour. And the two subjects we have left are -- one has to do with the radon releases from the silos. In a nutshell, we wrote a white paper that everyone should have, but it has not been PA cleared, dated November 25, 2008. Hans Behling did the work. The bottom line is

we're coming up with sources, radon emissions from the silos, that are 60,000 to 90,000 curies per year. NIOSH and their folks have recently issued a critique of our work dated February 2009 by Sam Chu, who disagrees with us and gives his reasons.

We reviewed that. We disagree with him. We think our numbers are right and NIOSH's numbers are wrong, and Hans Behling will explain why, but before we do that, I just want to let you know we also have John Stiver with us today. John is a CHP with us and joined our organization about --

MR. STIVER: About six weeks ago.

DR. MAURO: -- about six weeks ago. And John -- I asked John to look into this -- by the way, both the subjects we are going to cover were authorized by the last work group meeting, namely they asked us at that time -- from the last meeting -- Hans gave a brief description of work he did, and we were asked to make it a formal white paper,

which is exactly what this document is.

The other thing we were asked to do is to look into the Thorium-232 DWE, daily weighted exposure data, and the breathing zone data, general air sampling data that's going to be used by NIOSH to reconstruct inhalation exposures to Thorium-232. We are -- we haven't prepared a report; however, John has done a lot of work in looking at the landscape of the data, the records, what do they look like, and he has a number of talking points and handouts just to give you a briefing of the status of our investigations into that matter.

With that, I'd like to turn it over to Hans. Hans, are you on the line?

DR. BEHLING: Yes, I am. Can you hear me?

DR. MAURO: It's called an alternative assessment of radon releases from K-65 silos, an SC&A white paper. The cover page says November 2008 on it. The actual

footer, though, gives a specific date of November 25, 2008. This document of course went through DOE clearance, but it has not yet been PA cleared. It is in the process of being PA cleared.

Hans, it's all yours.

DR. BEHLING: Okay. Again, I'll just quickly go through a couple of historical issues. This really refers to -- this report reflects Finding Number 4.2-3, which was a finding that we identified as part of our review of the SEC petition, and of course, NIOSH's evaluation report.

In that petition -- in that review of our petition, we processed the assessment of the radon emissions from silos one and two, which were estimated at 5,000, 6,000 curies per year, might have been less than what we thought it should be.

And as part of our review, I concluded that perhaps as much as 60 to 90,000 curies per year might be the appropriate

value, and as a result of that finding, it was the work group who had asked SC&A to go back and support that revised estimate, and this is what this particular report is trying to do here.

Most of -- in fact, the -- the estimate of 5,000 to 6,000 curies per year for radon releases that was defined in the site profile for Fernald are really values that were derived from a 1995 report issued by John Till, the RAC Report. And it was really not NIOSH's calculation, but it was a reference to an early 1995 report by John Till that identified that particular number.

Now in going over my reassessment, I looked very carefully at the 1995 RAC Report, and I'm probably going to be quoting certain portions of that as part of this review.

One of the things that -- for those of you who are in a position to actually look at the hard copy of the report, either

hard copy or on the computer screen, I would ask you to turn to page three, which contains Table One in my report, and the title of that report is Summary of Historical Changes to the K-65 storage silos.

And again, this comes from Appendix J of the RAC 1995 Report. And there's a couple of dates that I want you to keep in mind. From the very beginning, there was construction defects in those silos, and everyone knew about it, and over a period of time they attempted to make corrections. But the major correction occurred, if you look at Table One, at the end of June of 1979 where the openings in silo domes, including the gooseneck pipes and other penetrations, were sealed with gaskets and installed to prevent radon emissions.

Additional modifications to the silos occurred in '83, '86, and another number or date that I want you to recall -- remember is the radon treatment system -- the year that

it was installed in 1987. And the purpose of that radon treatment system I will explain a little later on, but for the moment it was there to basically vent the head space in the silos from radon, and reduce the dose rates on top of the dome so that workers could work there, and an acceptable dose rate would result from having vented the head space.

And of course in 1991 there was some measurements taken from the matrix of the raffinates, and that's the thing that I'm going to talk about next. I'm going to refer you to Table Two in my report. That occurs on page seven.

And the key thing that you need to understand is the disequilibrium between Radon-226 and Lead-210. If you look at Table Two, and this is a 1991 sampling that was done, and you will see a whole series of rows that go from left to right, and in the second column you will see the zone, and the zones represent the depth of the raffinate matrix.

If you're looking at Level A, that's very near the top, if you're looking at B that's sort of in the middle, and C is towards the bottom.

But for the moment, to keep things short, if you look at the actual value of the mean for silo number one, and I highlighted or I enclosed the columns for Lead-210 and Radon-226, you will see for Lead-210 the average value, the mean value was 194,000 versus 525,000, and that gives you an equilibrium ratio of 37 percent -- or ratio of 37 percent, which clearly says that we're not in equilibrium.

The same thing for silo number two. If you look at the bottom, you will see 123,000 versus 209,000, and that is also a 38 percent level of equilibrium between those two radionuclides.

Those values are again repeated in summary fashion in table four on page six, and as well as on table five is some additional

data from 1993 which tends in part to support the earlier '91 data, with the exception that silo two has a much higher value. As you can see there, we go from 0.38 ratio to 0.72. And I'm not sure I know how to account for that difference, but clearly the two sampling data sets were somewhat different. I'm not sure that's the '95 data set which was done on the stratum level. That was done at an earlier time.

MEMBER ZIEMER: Hans, what table was that in?

DR. BEHLING: This is table four and five.

MEMBER ZIEMER: Okay, got you.

MR. STIVER: Bottom of page six.

DR. BEHLING: Okay, so as I mentioned before, the reference in the NIOSH site profile for Fernald in section 5.2.4, I'm going to read a quotation so that for people who might be on the phone who don't have access to either the hard copy or the computer

screen, I will read something that's very important.

In the site profile, NIOSH states the following. "As previously stated, the contents of the silos have not been disturbed during the storage to any large degree; however, it's been calculated that during the 1953 to 1958 period, 5,000 to 6,000 curies per year of radon were released from the silos." And they reference the 1995 RAC Report.

"Considering the expected large difference in release rates due to barometric pressure changes, release rates would average up to 15 to 20 curies per day after the addition of the silos were complete."

Anyway, what I wanted to simply emphasize here again is that these values were not NIOSH's values, but they were adopted from the 1995 RAC Report.

The model that John Till and his co-authors used was really a complex model. It was based on a diffusion kinetics of radon

to waste package to head space ventilation barometric pressure, and a lot of modeling data that had to make numerous assumptions regarding what could have been released.

And if you go further down the page, you will see some of his own concerns that he expressed in the report, but I won't for the sake of time deal with those issues. But let me go to page number eight, and near the top of the page, I have a title section from Page J-28 of Appendix J, and that's a reference to the John Till report of 1995, and I'll read that again for the benefit of people who may not have access to the report.

In that report, John Till says the following. The silo interior was sampled on November 4, 1987, prior to the operation of the Radon Treatment System -- and parentheses RTS, because I'm going to refer to RTS -- and prior to the application of the exterior formerly to the silo domes. And the RTS is a system that pumps air from the silos through

a series of calcium sulfate and charcoal beds, which removes Radon-222, enough potential daughter products of Radon-222, from the air space of the silos and reduces the direct radiation exposure rate on the silo domes. The system is used to reduce radiation exposures to personnel involved on the silos.

In other words, you were sending workers up on top of the silos, the exterior of the silos, and the intent of the radiation -- Radon Treatment System is to vent the head space and in the process reduce the dose rate because of the fact that you're removing the radon and its daughters.

Furthermore, I'm also going to quote a couple of other statements here. Searches through the historical records of the FMC have located some results of radiation exposure rates on top of the K-65 silo domes which are summarized in Table J-19, and that table I exclude as Exhibit Number One.

And let me ask those who have a

copy of the report to turn to page 10, which is -- comes directly -- it's a verbatim replication of the table J-19 from the report that John Till issued in '95. And you will see for the sake of, again, simplicity I have identified by hour certain dates.

The top of the table involves dates. The first one is April 1964. The second one is '72. There are two of them in March '72, and then there's May '73, and a couple of other ones in May '72 and July '73.

Important to note here is the fact that these measurements were taken prior to 1979 when there was corrective measures taken to seal the dome that is a gooseneck and the manhole covers, et cetera. And important to note here are the -- is the column that contains the measurements of dose rates in milliR per hour. So you'll see on April 1964, 75 millirem per hour, and on March 1972, below that is 30 and so forth and so forth.

And on the far right side you will

see some statements with regard to the average values which defines those particular measurements. You will see, for instance, in the case of -- let's see, no, they don't on this one.

But anyway, those are the dose rate measurements. Some were as low as 30 mR per hour to as high as 90 with an average somewhere in the sixties to seventy milliR per hour. That's an important number to remember.

Now on the next -- below that series of columns you'll have dates after the ceiling silo opening, and we'll skip the majority of them until you get down to the bottom where you have two more arrows identifying two particular dates. The first one is from the fourth from the bottom up, November 1987. Again, you have a contact reading, and that contact reading is 168 to 208 milliRs per hour, and the average was 193.

On that same date they start out -- they start with the Radon Treatment System,

which I will go back in a few seconds and explain what the technical specifications are.

Oh, let me just simply refer to you to the page eight on the bottom, which explains that the RT system was operated on one silo at a time with a flow rate of a thousand cubic feet per minute and was operated until the radiation level on top of the silo dome surface contact stopped decreasing, and that usually meant several hours.

And then it goes on to say the following. "With these flow rate and operating times and an assumed removal efficiency close to 100 percent of the radon concentrations in the silo air space should have reduced to less than three percent of the initial concentration. Thus, for this analysis the exposure rate measurements made after the operation of the RTS are considered to represent the quote background exposure rate in the absence of radon daughters in the

silo air."

So let's go back to Table -- Exhibit 1 on page 10 and look at the contact reading after the RTS was in operation, and you see for November 1987 the contact reading was reduced from an average of 193 to 35.5 to 68, with an average of 55 milliR per hour.

Another attempt was to measure it below on November 1987 and, again, the baseline reading before the RTS varied between 221 to 250 MR per hour, with an average of 230. Once you activated the RTS system, that was reduced to 68.

Now you look at those particular measurements after the RTS that assumedly cleared in excess of 97 percent of the radon out of the head space, and you will come to the conclusion that pre-1980 when the -- the gooseneck and the other penetrations were still open and actively venting that the dose rates on top of the dome pre-1980 was essentially nearly identical to the dose rates

that you would experience after the activation of the RTS system, meaning that you have vented essentially all of the radon and the daughters from the head space.

And on that basis I concluded that in essence prior to the serious attempt to finally seal the domes of Silos One and Two, the ventilation rates from those domes through whatever penetration that the goosenecks, the manhole covers essentially was equivalent in efficiency in removing the radon gas as the RTS that has at least as a specification designed to clear the head space volume of air at a thousand cubic feet per minute and was operated until essentially there was no further reduction in the dose rate on top of the dome.

Now if you go to Exhibit Two, it basically depicts the numbers that I just talked to you, on page 11 you will see the exposure rate in milliR per hour and a -- you have several data points prior to 1979 -- June

of 1979, and you see that the dose rate among those -- those lower on the left hand side oscillates somewhere between 60 to maybe 75 millirem per hour, and at that very moment in time when that modification was done to Silos One and Two you see a rapid acceleration in terms of dose rate that the highest reading was close to 400 milliR per hour.

Now on that basis, I concluded that obviously the silos must have vented most of the radon that escaped from the waste package from the raffinate waste package into the head space and was vented into the environment.

Now the big question that I had to deal with is what do we do as a starting point. Obviously, as a starting point the equilibrium between Radium-226 and Radon-222 could have been anything basically as an upper limit and lower limit from zero up to 100 percent equilibrium. And for that reason, not knowing the data and not having any

information as to what the ratio between those two radionuclides are at time of emplacement, I consulted a couple of documents from the scientific literature which are supplied to you as Appendix -- let me see, as Attachment One. It's an article by Claude W. Sill, and if you had a chance to read it there were measurements taken both of mined ore, uranium ore, as well as mill tailings.

And you will see that in both in ore and mill tailings the ratio between -- if you go to page 27 of my report, you will see a column of Radium-226 and Lead-210 as ratios to the parent uranium. They're basically identical. So at least in ore you see the ratio between radium and Lead-210, essentially at unity. They're essentially at equilibrium.

Of course, one could say that doesn't count, but let's go to uranium mill tailings, and I think I summarized that actually in the report on page 13. If you looked at the tailings, and they apparently

had several different samples to choose from -- one from a single mill, the other one was a composite of 16 mills. In the single mill tailings, the ratio between Lead-210 and the Radium-226 -- there's a typo there, it's 226 -- was 90 percent. For the composite of 16 mills the ratio was 87 percent. So I wasn't really quite certain as to what to do about estimating or making assumptions of a starting point, but what I did do was to essentially assume that the disequilibrium that we saw in 1991 when there were core samples taken out of Silos One and Two, that level of disequilibrium existed at the time of emplacement, which I consider as relatively unconservative that I might have ended up with a significantly higher ventilation rate than I ended up assuming.

And I assume that that disequilibrium that existed at the time of emplacement continued throughout the entire period up to 1979, June of 1979, when the

modification took place. And on that basis I came up with my numbers which I can just summarize, but I concluded that somewhere in excess of 100,000 curies per year between Silos One and Two may have been ventilated per year between the time of emplacement and the time of the modifications in June of 1979.

So for the sake of brevity I'm not going to continue adding more of the details, but if you have the report you can certainly look at some of the additional information that I've included that would support the notion that the 5,000 curies that were initially estimated by John Till in his 1995 RAC Report may have significantly underestimated the release, which I estimate to be in excess of 100,000 for both Silos One and Two.

DR. MAURO: I'd like to add one last thing. We did review this -- the February 2000 report by Sam Chu, and basically what Sam argues is that, no, the diffusion

calculation, the transport contained error, which is a transport calculation where you know the temperature difference, and you could model diffusion.

Argues that that's a very reliable way to predict source terms. It's basically to develop reactors, but the reality is, as Hans pointed out, it's filled with lots of assumptions regarding the diffusion coefficients, crack size, delta T. There's a whole litany of assumptions you have to make.

We checked those numbers, that is that were derived originally by RAC, and we got 6,000. In other words so if you were to use the RAC or John Till approach, we would get 6,000, but we think that that's a very indirect way of trying to get a handle on the source term. We think Hans's approach, which is based on the deficit of the progeny compared to the radium, coupled with the fact that there's good evidence that the -- there

really, there was no radon and radon progeny inventory in the head space meant that the radon left, and that the real number is probably more like 60,000 curies per year, so we hold to our position.

Now I'll be the first to admit this is not an SEC issue. What we believe is that the estimate of the radon release rate and associated doses has been underestimated by a factor of 10, if not more.

DR. NETON: Well, I honestly haven't kept up with this issue probably as much as I should, and I'd like to go back and review Hans's report because it's been some time since I looked at it. But I've thought about this a little bit, and I remembered that the Fernald dose reconstruction project was very much in the public eye. In fact, it was so much in the public eye I recall that they commissioned a National Academy of Sciences review of that dose reconstruction.

So a committee of the National

Academy of Sciences convened, reviewed that dose reconstruction in 1977, and in the opinion of the committee the RAC approach was considered to be -- I forget their exact words -- the committee concludes that the methods used in the Fernald dose reconstruction project are appropriate and scientifically sound. Furthermore, they went on to say, in the opinion of the committee the RAC approach has resulted in an overestimation of doses to people exposed to radon. So here we have somewhat of a difference of opinions.

DR. MAURO: Yes, we do.

DR. NETON: And we have one expert opinion that has confirmed the RAC approach, the National Academy of Sciences review. I have to say I'd like to go back and look at Hans's analysis. I mean, I respect Hans, and I need to look at his analysis again.

DR. BEHLING: And let me just finish off. I really try to avoid models if

I can, and to me those particular data points regarding dose rates on top of the dome that's involved pre-1979 measurements and then, of course, the use of the radon treatment system on and before it is activated tell me an awful lot of information that transcends non-empirical model data that, for instance, John Till used.

And if, in fact, the radon treatment system that was venting the head space at 1,000 cubic feet per minute was operating for several hours with a ventilation rate of 1.2 ventilation volumes per hour, what does that tell you about the fact that those dose rate measurements in earlier years, pre-'79, were essentially identical to the measurements after the RTS was activated until the dose rate no longer dropped.

To me that pretty much tells me more than somebody's opinion about the RAC data, even if it involves such noble people as the National Academy of Science. All they did

was look at what we did when we looked at the RAC report, and John just finished telling you we looked at the data and said, hey, you know, if this is all you've got you may have to concur with the conclusion that it was five to six thousand curies per year. But maybe they should look at the Appendix J of the RAC report and then identify the various numbers that I identified and then determine whether or not you still feel that the RAC report has in its original form a more credible data.

DR. NETON: There also occurs to me that there was a recent analysis done by the University of Cincinnati, funded by NIOSH, by the way, that went and reconstructed the dose for all -- all workers at Fernald, I think over all -- not all time but through a certain time period, starting I think at the beginning of the entombment of the K-65 material. And my recollection was that they developed yet another diffusion model. I'm not sure how much it relied as a starting

point on the RAC data, but I'd like to go back and look at that, as well.

So there's some issues on the table here. I have some concerns about the -- the diffusion -- how deep a pile of material this way and Hans's assumption about emanation rates and uniformity of that, and all kinds of concerns like that that I think need to be really looked at in some detail.

I respect Hans. He's an excellent scientist, but I think so far it's not passing the peer review process, and I'll go back and look at it myself.

MEMBER ZIEMER: Hans, this is Ziemer. I have a question, too, maybe you can help me clarify. In going through your calculations around page 15 and so on where you started with the inventory of radium, did that come from the total inventory in the silos?

DR. BEHLING: Yes, it came basically from the curie content of Radium-

226.

MEMBER ZIEMER: Okay, so that's what I thought you had done, so it appears that you're assuming that all of the radium or all of the radon atoms generated by the decay of radium actually are vented?

DR. BEHLING: Well, not quite. As I said there is obviously the ratio of about 38 to 40 percent that remain. I'm not saying no. I did not say 100 percent, but the fact that in 1991, which is approximately 40 years after the emplacement of the raffinate waste you still only have a 40 percent ratio between Lead-210 and Radium-226.

Now Lead-210 has a half-life of 21 years and in essence if -- let's assume for a moment that the -- all of the radon remains in the waste package and decayed and gave rise to a starting point that had zero Lead-210. After 40 years, in 1991 we're talking about 40 years, you would have had two half-lives of in growth, meaning you would have had at least 75

percent.

And so you realize that radon has to have escaped. There's no question around that, and the question now is if it escaped the waste package and ended up in the head space, what happened to it? And this is where I believe the second issue comes into play with regard to the data that was reported in Appendix J.

It's clear that the radon left the waste package or the matrix of the raffinate waste. If it enters the head space, what happened to it? And if the dose rates pre-1979 and post-'79 with the RTS system are essentially identical, you almost have little or no choice but to conclude that that radon had to have escaped.

MR. MORRIS: So essentially you're saying that 97 percent of the radon entering the head space was released to the environment?

DR. BEHLING: Well, those are the

two data points that I rely on, and I believe that's the conclusion that you almost have to come to. My discussion about the Venturi effect does not to explain these numbers. It just explains the possibility by which an enhanced release rates could have occurred. When you have a dome that is basically an airplane or an asymmetrical foil, it's subject to the Venturi effect and may have created a significant vacuum in the head space that basically was the means by which it escaped, even through modest penetrations.

MR. MORRIS: Excuse me, Brad? Are you interested now in getting this summary of what Sam Chu reported in his paper in rebuttal or is that -- I don't know what you want to do.

CHAIRMAN CLAWSON: If you're good, Jim also said he'd been a while and he'd like to --

DR. NETON: I'd like to -- I mean, John characterized it as essentially

saying that it's definitely -- he bought off on the RAC assumption. I think that's what John characterized the Sam Chu report.

DR. MAURO: Oh, no. I said that if we run the model -- no, no, no. We don't accept -- we don't believe this is the way to do it. We think --

DR. NETON: No, but what I'm saying is Sam Chu evaluated Hans's approach --

DR. MAURO: Yes.

DR. NETON: -- and if you have anything of substance to offer in rebuttal to Hans's arguments.

DR. MAURO: All he said was that the diffusion model --

MR. MORRIS: Well, you know, why don't you let me represent that instead of you representing that?

DR. MAURO: Go ahead.

MR. MORRIS: Basically, Sam said, okay, we'll start with the beginning assumption of the amount of radon that reached

the head space that Hans took, but that's not the end of the story. There are barriers to the radon getting out of that head space and into the environment.

And if you think about it even for a moment you'll think oh, yes, there are barriers. There is the matrix of the waste, and then there is the dome. I mean, that's why there is a dose rate there on the top because it actually impedes the flow of the radon.

So Sam went through -- let me see if I can get to my highlighted sections here. So missing from that assessment that Hans just described is the amount of radon released to the environment from the head space -- has to consider that containment capability of the silo, the retention time of the radon in the head space, and the depletion of the radon in the head space due to radioactive decay.

The assessment really doesn't take into affect -- into account the amount of

radon released to the environment that was driven by the daily temperature differentials, the Venturi effect of prevailing wind speeds, the retention time of radon, and the depletion. Fundamentally, radon is heavier than air and consequently will tend to be in the bottom of the head space just by nature unless it is stirred up with some mechanical force that's moving it up. There were openings in the top of the dome and cracks also. There was a six-inch gooseneck pipe bend, the gaps between the manholes and the manhole covers, and so collectively you can begin to describe these as leak paths.

A leak path factor is the ratio of what's released to what's contained, and there is a computer code that the NRC uses called CONTAIN. CONTAIN 2.0 is the version that's out now. It's a generalized mass transport and thermal-hydraulics computer code, and it was developed to predict the thermal-hydraulic response inside a nuclear reactor, but it's

sufficiently versatile to take any set of pressure or temperature-driven flows and the cells which would be the components of the waste, sort of the layer cake waste, and then the head space is a cell, and then the release portion and actually do a predicted model that -- that can define, based on these mechanical and physical properties that can be measured or assumed easily, the amount of flow that could happen.

And so, you know, Sam goes ahead to show the equations and then implements the -- the calculation with the contained code. The bottom line is that the numbers really do not change very much from where we left it in the Technical Basis Document, so we're content with saying that we can validate by this modeling and the assumptions that Hans begins with -- provides us to begin with a rationale for having exactly the same position that we left in the Technical Basis Document.

MEMBER ZIEMER: So you end up in

your analysis with something which you might call a resident time of the radon in the head space?

MR. MORRIS: Yes.

MEMBER ZIEMER: Which is roughly what? Do you know what that --

MR. MORRIS: I can find it if you want --

MEMBER ZIEMER: It looks like it's got to be a couple days.

MR. MORRIS: Well, I think it's more than that. If you would let me look that number up. That's not the kind of detail I have at --

MEMBER ZIEMER: No, no. I understand, but I'm just trying to get a feel because Hans's number like -- well, roughly a 100,000 versus -- here, 30,000, is it a factor of two or three?

MR. ROLFES: Our current Technical Basis Document has 6,000 curies per year, and the white paper that we produced

actually has 660 curies being vented, so this model, the CONTAIN calculations that we presented in the white paper here have essentially another order of magnitude lower than what we have in our current approved Technical Basis Document.

MEMBER ZIEMER: Okay. So you need several effective half lives if you want to think of it that way.

MR. ROLFES: And basically these are -- these are orifice-driven flows.

MEMBER ZIEMER: Yes, I understand.

MR. ROLFES: And so, you know, you just can't instantly have everything come out.

MEMBER ZIEMER: No, no.

DR. BEHLING: I guess I have a question as to why you would explain or how you can explain the quantum leap in the reduction in dose rates following the RTS that reduces the dose rate on top of the dome to

levels that essentially are pre-'79, and you can reasonably assume that that is the result of having vented after several hours, and most of the radon daughters are short-lived radon daughters with half-lives of microseconds to up to twenty-some minutes. And if you run the RTS for a period of three hours you basically blast out all of the radon and the short-lived radon daughters which result in a massive reduction in the dose rate, and as far as I'm concerned the post-1987 RTS values are essentially similar to the pre-1979 modifications to the dome. And to me those numbers speak everything I need to know.

DR. NETON: I'm confused, Hans. You're saying that by virtue of the fact that they can pump the short-lived progeny out of the dome and reduce the dose rates, that plays into your hand?

DR. BEHLING: Well, yes. I believe if you can essentially pump and keep the radon system on indefinitely, meaning that

there is no build-up of radon in the head space and you end up with a dose rate that is the same as the dose rate before the RTS before the dome was modified --

DR. NETON: I could suggest that, you know, the emanation rate coming out of the material is pretty low, and once you pump it out of the head space you've removed the source term.

DR. BEHLING: The same thing with -- if you have natural ventilation --

DR. MAURO: You wouldn't have a deficit. You can't have it both ways.

DR. NETON: I suspect that there's a lot of plate-out of this material on the dome itself. Radon has a very large affinity for -- it's born charged. Radon progeny are born ionized to some degree. There's a charge on those particles, and, in fact, in an indoor environment the equilibrium ratio is only around, what, 30 percent because they attach to the surfaces of the material in

the area that they're born.

DR. MAURO: If the radon stayed in the dome pre-1979, why is the dose rate 30 to 60 millirem per hour? That means that it's not there. The dose rate on the top of the dome before 1979 is low. It means that you don't have this inventory sitting up there inside this dome space. The radon isn't there. And the fact that after they sealed it -- in fact, if what you're saying is true you would have expected to see 200, 250 MR per hour pre-1979 because it would be trapped in there, giving you this high dose rate, and you don't see that.

DR. NETON: Well, didn't they also put a cap on top of the silo material itself? There was a massive cover -- a bentonite clay cap on top of the silo to prevent the migration --

MEMBER ZIEMER: That was later.

DR. NETON: That was in the 1980s.

DR. MAURO: We have to talk to --

DR. NETON: There were several campaigns to put a cap on the inner material to prevent exactly what Hans is talking about, the migration of material out of the -- out of the silos.

DR. MAURO: Look at the '87. I mean, the numbers are -- I mean, it's screaming at you. When you turn on that vent, you drop right back down. After you turn on the vent you enter the head space of radon using the vent system, you're right back down to the 35 MR per hour, which is what you have before 1979.

DR. NETON: And how long did it take to build back up?

DR. MAURO: The next reading, it doesn't take long.

DR. BEHLING: Well, you can look at that if you look at, again --

DR. MAURO: The graph will tell you.

DR. NETON: Okay, we're probably not going to solve it here.

DR. MAURO: I know, but I mean -- listen, I mean, I look at this and I say the common sense argument -- this is really what we have here is Hans brought to the table a common sense argument that really directly contradicts the sophisticated transport equation calculation. The two are incompatible. The numbers we're looking at in Exhibit One and the model -- something's wrong, and quite frankly I had much sooner trust the empirical data than I would these transfer models.

MR. MORRIS: But in terms of common sense, it doesn't make common sense to assume that the silo did nothing to impede the flow of radon.

DR. MAURO: Why would you say that?

MR. MORRIS: It makes no common sense to assume that none of these hold-up

factors were in play.

DR. BEHLING: Well, let me shed some light on the issue that simulates the dome to a floor in a basement under which you may accumulate radon. You can -- and I've done this before because my house suffered. I lived in the radon prone area. If you use a toxic paint and you seal all but the most smallest of cracks, you have done nothing. The infiltration remains the same. It isn't until you introduce a ventilation, a sub-slab ventilation that you actually then do something constructive. So it doesn't take much of a perforation to vent most of the material if you have a negative pressure inside your basement compared to the pressure underneath your slab.

So I do believe that you don't need to have huge, huge gaps of cracks. A few major cracks, a gooseneck, and a few other things under the condition of a Venturi effect can essentially serve to vent the head space

fairly efficiently to the level where you see dose rates that pre-1979 are equivalent to the ventilation rates and the reduction in dose rates with the RTS system.

DR. NETON: Well, again, we need to take a look at this, but I agree with John that this is not necessarily an SEC issue. It's a novel analysis of an issue that has been reviewed by the National Academy of Sciences, which I tend to trust, but we need to look at it in light of this new concept.

DR. MAURO: You know what? That's our story. I really would like John to get a chance to -- give John a break, but I know we're in the home stretch, but you made a trip all the way, so to give us a quick --

MR. STIVER: Okay, let's go ahead. I'll try to keep it as brief as possible.

MEMBER GRIFFON: Before we --

MEMBER ZIEMER: NIOSH is going to review this.

DR. NETON: Well, we already have a review. We'd appreciate SC&A to respond to it.

DR. MAURO: No, no, no. Our response is very straightforward. We don't believe running -- is that contained air or contained --

DR. NETON: Contained.

DR. MAURO: -- a transport code that makes certain assumptions -- diffusion coefficients, average your differences is the way to come at this problem when you've got data like this. You know, what are you going to trust, and really this becomes a matter of scientific judgment. Do you trust -- you know, the barriers that you're talking about it, it's very difficult to contain radon.

MEMBER ZIEMER: Let me ask a question regardless of which number's right. How are you using -- remind me of how you're using the radon information that's vented from the silos.

MR. ROLFES: Basically, we -- the way we would reconstruct an individual's radon intakes, we're assigning default values based on the site profile.

MEMBER ZIEMER: Down wind or are they location specific?

MR. MORRIS: They're location specific. In the environmental.

MEMBER GRIFFON: In the environmental, and then, I mean, that's what I want to get back to. This part, I think, I actually agree with this that this side of it is a site profile deal. The question that I'm not sure is -- might remain an SEC question is how is dose assigned, you know?

MR. ROLFES: Exactly. I guess exactly how this affects claims, you know, we can take a look at some of the perimeter radon air monitoring data and other track-etch detector data that we have.

MEMBER ZIEMER: Are you seeing significant lung doses to people in the

environment from the radon?

MR. ROLFES: Yes, but the K-65 silos aren't necessarily the sole source. It's more people working with Q-11 in process.

MEMBER ZIEMER: I'm really asking you what is this contributing to the big picture, or is it too early to say?

DR. NETON: It's pretty small compared --

MEMBER ZIEMER: That's what I was --

DR. NETON: I mean, we've -- 90 plus percent of the lung cancers in -- respiratory track program are compensated. So there's a large dosage associated with a missed dose associated with uranium intakes, thorium intakes, thoron in the building, radon in the building. It's sort of an environmental issue where how much radon could be wafting outside from the K-65 silos is an environmental TBD issue that we would use to assign to people who were not necessarily

production-type workers.

MEMBER GRIFFON: But that's the question here, and I'm going back to the matrix, believe it or not, at a quarter of five. I mean, I was, while Hans was presenting there, I was flagging some old -- going through and looking at the old actions that we might have forgotten about, but for 4.2-1 this is that question that, Mark, I think you just alluded to is NIOSH is supposed to further evaluate the ability to reconstruct doses from raffinate specifically for workers exposed to materials from Silo Three. And then updating -- there's another one, NIOSH is updating Technical Basis Document to consider the Pinney radon study. That gets into the Q-11 stuff, I think. So this is back to the question of not only the K-65 but the Q-11 silo stuff and how are you assigning radon to a site.

DR. NETON: That's correct. That's a separate issue, but Hans's analysis

would -- that's actually contradicted in the RAC study, the Pinney Study, and other studies that we've been using.

MEMBER GRIFFON: No, I understand that, but this part of it, this dose assignment part of it to me is not necessarily just a site profile issue. I mean, how are you going to determine who was in what areas and how are you going to decide who gets what doses. That's that age-old question.

DR. NETON: I need to talk with our group here.

MEMBER GRIFFON: I'm just keeping that action on the table.

DR. NETON: Remember, though, that there is a Pinney study out there that has reconstructed a dose for all workers based on some default values --

MEMBER GRIFFON: I'm very familiar with it. I just don't want to lose the action. That's all I'm saying is that -- it sounds like we're closing it out kind of as

a site profile issue, and I'm saying for that side of it, I don't disagree.

DR. NETON: I think that the SC&A analysis that Hans has done is not a site profile issue.

MEMBER GRIFFON: It is a site profile issue, right. I agree with that, but the other side --

DR. NETON: Exactly.

MEMBER GRIFFON: That's why I want to keep it on the table. That's all. Okay, I'm just reminding us that it's out there, and I'm going to update this matrix when we leave this meeting.

And I'm going to do like I've done in the dose reconstruction subcommittee. I'm going to highlight the actions in yellow. It seems to work very well on these kinds of documents so the actions -- you can just flip through on the screen and find where we left off because there's several of them that we haven't discussed, and they're kind of getting

lost in the weeds a little bit. And I want to make sure that we close them out because, you know, the petitioner's watching us and, you know, we have to be responsive to them.

CHAIRMAN CLAWSON: We're going to lower -- before you take off real quick, we're going to lower our intellectual level way down here. I'm trying to understand something here, and I apologize for my ignorance.

But pre-1979 we were really maintaining a 50MR off the top of the silos, and after they sealed it all of a sudden we're going to 250 to -- to as high as what I see as 400.

And, Hans, correct me if I'm wrong. What -- what you're saying is -- is this is showing what could have been possibly venting out of the K-65 silo previous before sealing it?

MEMBER ZIEMER: Right.

DR. BEHLING: Yes, the truth --

CHAIRMAN CLAWSON: How much

activity is going on, so really what we're doing is when we're pumping all that head space down we're basically seeing the radiation that's being given off by the -- the actual product that's inside?

DR. BEHLING: Well, yes, you obviously have radon activity in the raffinates, and that is your -- as was stated -- let me see here -- in one of the things that I quoted.

On page -- top of nine the statement -- and this comes from, again, the RAC report: "Thus, for this analysis the exposure rate measurements made after operation of the RTS are considered to represent the background exposure rate in the absence of radon daughters in the silo air."

What basically, I was saying, we're looking at is this. If, for instance, you had a -- the RTS system operating for an indefinite period of time, not just for a few hours so that workers could go up, but based

on the fact that as the statement says they would run the RTS until there was no further reduction in the dose rate.

What you would then essentially assure yourself of is that there was no additional build up of radon in the head space, and if at that point you had a dose rate measurement of 65 or 70 milliR per hour and then realized that pre-1979 you had no RTS but it was a continuous ventilation system and the dose rate never went much above the 65 to 70 MR per hour.

So you, in essence, have to come to the realization that pre-'79 the ventilation rate was basically in a de facto RTS system.

CHAIRMAN CLAWSON: Okay, I just wanted to make sure that I understood what you were saying. I appreciate that, so basically the action item that we're going to have is that NIOSH is going to --

DR. NETON: We've looked at it.

It's been determined that this particular issue rated by SC&A is a site profile issue, so in light of the fact that this SEC evaluation's been in process for over two years now, I think we've put that on the back burner at this point.

I mean, contrary to what I said I'd still like to intellectually look at it and we'll get to it, but we've got a lot of other more pressing issues to resolve from the SEC perspective at this point than to burden to SEC review process with this.

CHAIRMAN CLAWSON: And I understand that, but like we said on the matrix here it does actually get back to the radon --

DR. NETON: There is a radon reconstruction issue that is related but not directly related to Hans's. If Hans is correct and SC&A is correct, it would be a scaling factor that could be applied to all the radon doses that we assign on the site.

The question is can we actually figure out who to assign radon to, and if we use six curies or 60 curies, it doesn't matter. It's a scaling factor.

DR. MAURO: The issue remains --

DR. NETON: The issue remains, but it's not -- it doesn't mean that we can't bound them to some degree of certainty.

CHAIRMAN CLAWSON: Okay --

MR. ROLFES: Once again, the organ of significant -- you know, the target organ essentially is the respiratory tract, and I think we, you know, reiterated once again that, you know, 90 percent or greater of the respiratory tract cancers that we've received claims for at Fernald have been compensated.

CHAIRMAN CLAWSON: Okay, I appreciate your time to be able to explain that. I'll turn the time back over to you. I'm sorry.

MR. STIVER: Okay, let me go

ahead and distribute out some of these handouts here.

I'm not able to explain the thorium time line that we put together, but we have something taken from Bob Morris' time line that we put together in 2008, which is essentially the exact same information.

So I apologize for the poor quality of the first two. We tried to explain what's going on as much as possible.

Anyway, I'll try to keep this as brief as possible without losing too much of the detail that I'd like to cover. If you take a look at that first table there that I gave you. That came out of the original version of Bob Morris' white paper on how to use the daily weighted exposure data derived from a alpha-air concentration samples that were taken before the institution of the lung counting program in 1968.

That's really the heart of the issue here is can we -- is there sufficient

data available to reconstruct thorium doses -- internal doses during the period 1954 to 1968 before the lung counting program started.

My readings have shown there is an extensive discussion of this a little over a year ago in the March 2008 working group meeting. There were action items prepared for October, and for a number of reasons it never got to the table, and so here we are over a year later just getting back to this issue, and as a result I would like to recap some of the action items and some of the discussion that took place back in March about delivering the point.

First of all, NIOSH emerged from that meeting with two action items. Both involved posting excessive data to the O drive. The first was to post spreadsheets that contained the DWE data and the latest version of the white paper describing how it could be used in a dose reconstruction for various -- selective years.

As a corollary to that the advisory board, recognizing what an enormous undertaking this was, decided that it would be better to do a sampling of that data. Basically, what they decided on was to look at all plants for 1955 through 1966 and then Plant One for 1960, with the supposition that if the data were adequate for those years and those plants then they would probably be adequate for the other years, as well.

The second item that NIOSH got was to post these 160 -- roughly 160 DWE reports that you see on that first table. All those little dots -- actually, there's 167 of them. Each one of those represents a facility and year for which these DWE reports are available.

Our review of the data that's out there on the O: drive indicate that we were able to discover 152 of these DWE reports. Selective sampling within that set of data indicated that the job exposure evaluation are

data that were in those reports were indeed what was transcribed in the spreadsheets.

The spreadsheets, and there are two of them, they contain a substantial amount of data. All this job exposure evaluation are data for various clients for different years, but not all of them.

And our action item was really to review the data and in addition to that the co-worker model, with the ultimate goal of determining whether this data was adequate for the purposes of dose reconstruction for all categories of personnel, all years, during the periods of exposure.

Now one of the first things we came up against was that in looking at the spreadsheet data, all plants are not covered for 1955 and 1966, and in addition to that we're not able to locate a set of data for Plant 96 in 1960, so what we decided to do was to shift the focus to looking at all the different facilities in years of thorium

production, or when we believe thorium production took place or inferred that it took place and get an idea of what's really out there, kind of a preliminary snapshot of the data availability as it stands as of March of 2009.

Now it's important to note that resolving these action items really get to the heart of -- the action items or the issues that were identified basically 4.3-1 through 4.3-10. All of those issues are really -- the common thread here is whether this air sampling data is adequate for dose reconstruction, with the exception of 4.3-6 which gets to post-production era and whether the lung-counting model is adequate.

But most of these other issues all relate to this particular set of data.

Now the status of the action item -- before we really get into that there's a couple of concepts and reports and things that I'd like to talk about. This whole idea of

what a DWE is, and really what this is, a DWE is just an average daily weight of exposure. It's a way of assessing the exposure potential for a particular job category at a particular facility. And the data that were recorded were in terms of alpha air concentration. These were both in terms of general air and in breathing zone, types of samples.

A whole series of anywhere from maybe one to up to 20 to 30 samples would be taken for each subtask that is defined within a particular job category. So you may have 16 different tasks for a particular job, and each of those tasks is assigned a time period within that day, so when we sum up all those times you end up with eight, eight and a half hours, basically the entire daily exposure.

For those samples that were taken for those different tasks, like I say they can range anywhere from this one sample up to 20 to 30. Some very basic statistics were provided just below the high and the average

value. And to calculate this DWE then what they did was multiply the time for the task, time for the average concentration, sum all those up, and divide by the total amount of time. And so what you then have is this kind of a generalized overall weighted average of the exposure potential for that person or for that particular job category.

And another interesting point is that in looking through just preliminary review, not an in-depth review but just looking at the sample of these DWE reports, it looks like the breathing zone data were really associated with those particular activities that had a high exposure potential over a short period of time, like going into a furnace, breaking open a mold, pouring thorium into one of these bomb retorts along with the calcium and zinc chloride to create the derivatives, anything where you can really be disturbing a lot of material, picking up a lot of dust.

The general air samples by contrast were typically in the low concentration areas like cafeteria, hallways, locker rooms, general levels of a particular facility, and so there's a mixture of these, and for each of these DWE calculations. And it's not a situation where you have a general area and breathing zone for the same particular operation or the same particular task. So there really are two different types of measurements.

The DWE typically was expressed in multiples of the MAC, maximum air concentration, which was 70 off the EPN per cubic meter for 463 and was changed to 100 thereafter. An important point to note here is something that really permeates this entire analysis is that the method, the analytical method employed here is gross health accounting. And gross health accounting doesn't give you any information whatsoever about isotopic specificity. And so what we're

forced to do then is rely on process knowledge to infer what particular operations were going. We have uranium going on this year. We have thorium going on, and if we did have thorium for however long is a particular campaign. Was it three weeks, six months, nine months, the entire year?

So at this point we're limited in our granularity to basically by years which is in turn inferred from operational knowledge of what was going on.

MR. MORRIS: John --

MR. STIVER: Yes.

MR. MORRIS: -- if I may. When we don't know that data was specific to thorium or uranium we assume that they were thorium for that year.

MR. STIVER: Yes, I was going to get to that.

And, yes, DWE reports are very interesting. I've had a chance to go through some of these. One that Bob included in his

2009 white paper happens to be for Building Nine -- for Plant Nine during 1955, which is the period of high thorium metal production. And these reports are really very striking in that the amount of material that's contained, the consistency from year to year for the different activities, they typically involved about eight sections. They're about 30 to 70 pages long. They start out with an introduction, which is just kind of a brief summary of the processes that were going on at the facility, a description of the sampling, and an analysis method that was included.

There were two data tables. A summary of Table One provides the average DWE for each job description at the facility and also a DWE for the entire facility.

Data Table Two contains the average air concentrations for specific operations or areas.

The discussions were very interesting too, because it really provides a

more detailed description of the processes or controls that were in place.

And then finally there's a recommendation section based on the study or what did they discover, what types of recommendations did they make in terms of controlling exposures, or what types of remediation or mitigation could be employed to reduce the concentrations to workers.

And finally we have the appendix, and the appendix is where all these job exposure evaluation reports are found, and this is what really summarizes, you know, the tasks for each of these different descriptions. It gives you line by line what the inputs were for that DWE as I described earlier, and then the initial DWEs.

Two of these that I found particularly interesting was the 1954 DWE report for Plant Nine, and that particular report was taken during a pilot study to really try to perfect the chemical processing

techniques. There were very few people employed at that time, in the first half of '54.

This particular report pertains to 19 individuals, and the personnel are named. Their actual names are there, their job descriptions. These job exposure evaluation cards for each of the different 19 personnel -- their positions are included, a description of what was going on at the time. This was just kind of a pilot study, and it's very interesting. And then you see, of course, in the second half of '54 they really start to ramp up their production, and we don't have a DWE that has been identified for that particular period of time.

However, for 1955 there's a report that has sampling data collected all the way from March through November of '55, all related to thorium production. In this case, there was 119 personnel, and the description is very enlightening too because there's

always been this issue of, well, what particular activities in Plant Nine in relation to other plants. You know, with uranium you have this concerted effort among all the different facilities. You know, you have the sampling plant grinding all the material down to a uniform size. Then you have the refinery producing the nitrate which then goes into an oscillating oxide calcite process, then to a fluoride production, and then finally into metal production.

And so there's always been this issue of what was going on at what particular plant and when. Well, this particular report shows that in Plant Nine they received the nitrate. They did the oxide production there, in Plant Nine, they sent it over to Plant Four to be converted into the tetrafluoride. It was then brought back to Plant Nine, and then the derbies were produced in the furnace -- that was zinc there -- and then they were sent off for rolling off site and then brought back

on site again for cutting into various shapes by the machine.

And so this is all contained in that particular DWE report, and this is information I feel would be very useful, and if that similar type of information is in the other reports I think we can have a very good handle on what activities were going on and when, what the exposure limits were, what the job descriptions. All this is a wealth of information that's contained in these and really, I think, help us to reconstruct these doses to a very, very precise level.

DR. MAURO: Do you know if that was thorium or uranium?

MR. STIVER: It was thorium. That was during -- thorium was going on -- 1955 was the big year of production.

We don't have a DWE report for '56; however, we do have one for '57 and it clearly states that uranium is being produced in '57. So there's a tailing off of thorium

in '56 and ramping up of uranium production in 1955.

This was just kind of a snapshot. There's lots of data we can see here.

The next I'd like to do is take a look at the -- which is this multi-colored spreadsheet table here, Table Two. And our initial approach here is to take a look at -- based on NIOSH's action item one, we're going to look at just those that were called out there, but it became pretty clear that wasn't going to wake you up.

And so this really looks like a really complicated table, but really there's -- there's only four types of data here, okay, and these all relate to the availability of the DWE reports. I've color coded it to try to make it a little bit easier to understand, but the values here -- we have in the first column years of production, and across the top the various columns we have the different plants. Basically, this was similar in

structure to Table One from the white paper, and the values that are high, they're bolded and not colored are essentially -- these are values that have been transcribed into the spreadsheets. These are the job exposure evaluation line items. These are not individual samples. These are either averages or because they are single sample it could be averages. But those are the individual task items. That's how many were -- in terms of breathing zone and general area samples.

DR. MAURO: Just a quick question, for Plant One, 1954, there's a number 16. Is that a three, I'm sorry, 1953.

MR. STIVER: Yes, sixteen breathing zone line item samples.

DR. MAURO: Is that 16 breathing zone samples?

MR. STIVER: Sixteen averages.

DR. MAURO: Averages, so the multiple breathing zone --

MR. STIVER: This is basically 16

tasks that are identified.

DR. MAURO: Sixteen tasks, okay.

MEMBER GRIFFON: And those are the average for each task?

MR. STIVER: That could contain any -- say for Plant Nine, that DWE report, there was over 400 individual samples for that particular DWE. It could be more, it could be less.

MEMBER ZIEMER: And then the 11 general areas are specific -- averages of specific areas?

MR. STIVER: Those would be just, you know, continuous air monitor --

MEMBER ZIEMER: Yes, so 11 locations?

MR. STIVER: Yes, those would be locations associated with those activities during the period like, say, going to the cafeteria or time spent in the locker room, and so forth.

I see Plant One really has the

lion's share of the available data at this point. If you get down here below the actual -- below 1969 you see there is the sum of the DWE samples by type. That is just a summation by plant of all the years.

And the next level below that shows the ratio, basically the breathing zone to general air by building, and it's kind of interesting here that you see -- whenever you have -- for the facilities that have more than about 100 samples, the breathing zone portion or proportion ranges from about five to 25 percent of the --

So what, what does that mean? It may just be that, you know, fewer breathing zone samples are really necessary in order to characterize that. It doesn't mean, like I said before, that these are two different types of measurement, one being more accurate than the other in the same type of activity. They're different activities.

And let's see. The light brown

here is -- these represent DWE reports that have not yet been transcribed, and there are still quite a few of those. We'll get into exactly how many and what they mean here in a minute.

The dark blue shading are reports that we didn't think were available but actually were transcribed or found and transcribed but don't show up in Table One.

And then this light blue really are supplemental data that we'll discuss at the end here which I felt because it did provide a lot of data related to some of the thorium facilities, I thought it might be worthwhile to include here and discuss a little later in regards to the last table.

Let's see, where were we here? There are basically four types of sub-issues, if you will, that kind of come up in reviewing this data. The first really has to do with record applicability, and this again gets connected in a time line. The DWE reports are

basically for all out there data and there is some portion of that is related to thorium. The rest is related to uranium.

Now as Bob said, when in doubt, the approach here is to high-side the dose, and the way to do that is to use the dose coefficients for Thorium-232 as opposed to Uranium-234. And I did a little calculation on my own using the ICRD database. And it's just to verify using Class M and Class S of the two different nuclides, and sure enough, for type M, the ratio of thorium to uranium, the range is from one to one up to about 560 for round surfaces. And there's a whole range in between there. And the values for Type S are very similar.

And this particular information was also in table seven of our site profile review back in November of '06, same basic data structure. So even if we're not able to get more granularity on the -- on the production time line, we can always be fairly

confident that the doses will be claimant-favorable.

Now one way we could actually get a better handle on this, which might be kind of labor-intensive, but it's worth bringing up, is that if, you know, in Table One, which was the time line of the thorium activity. Now included in that, in addition to the time of the activities, is the total production quota in metric tons for -- by year. And so we have that data. And now if we only had some information on production capacities for the various facilities and pieces of equipment. It's my view that we should have an idea of what a run time would entail, and so we have at least a way to get down below a yearly basis, maybe some fraction of a year.

Now I don't know if it's really worth doing that or if it's, you know, there would probably be quite a bit of labor involved in that. But I'm just going to put it out there as a potential way to increase

the resolution of our time line.

MR. MORRIS: John, we've learned on -- the thorium capability was usually not fully used, so they ended up campaigning thorium.

MR. STIVER: Yes, so it would be a short duration campaign.

MR. MORRIS: So because the equipment was really sized for uranium in many cases, and so the thorium was much smaller mass moving through than uranium.

MR. STIVER: So it's very, very solvent, except maybe in '55 when you have that big campaign.

MR. MORRIS: Yes, so usually the campaigns were short, and they stopped and started multiple times during a year.

MR. STIVER: Well, I kind of wondered about that because of the pilot plan. And you can see that from '64 to '79 there's always some flurry.

MR. MORRIS: But it didn't take

many days for them to do that. I think it's a good suggestion. I just don't think it's going to yield a lot of information.

MR. STIVER: Yes, it might be something that's a lot of effort for the results that might not really be that practical in the long run.

MS. BALDRIDGE: This is Sandra. I have a question.

MR. STIVER: Yes?

MR. KATZ: Go ahead, Sandra.

MS. BALDRIDGE: How do you address the fact that there's no data for Plant Six?

MR. STIVER: Actually, there is data for Plant Six --

MS. BALDRIDGE: Well you said there wasn't.

MR. STIVER: -- from '61 to '63. This is some of that data that we didn't think we had that turns out did show up. There is data for 1961 through '63, and the next table,

table three, really gets to what data is out there, what would be valuable for the assessments of thorium, but really has not been transcribed.

Now that was the kind of segue for this next idea, which is really the record availability, and as of now, only 32 of the 171 identified DWE records have actually been transcribed. Well that doesn't sound like much, but for our intents and purposes here, if you go to table three, you'll see that what we have here is a list of different facilities, the reports that have been transcribed for that particular facility, and those that are not yet transcribed for years of thorium production. And the ones that are not yet transcribed I think summed to about 12. There's only 12 more that we need to get, and so if we could -- I would say that if we're going to grade or assign some priority to a record transcription in order to get this particular analysis clarified, that would be

the data set to concentrate on.

But if we can back up again to table two. Another issue, kind of a sub-issue, is this whole idea of the completeness of the transcribed records. Now so far only the job evaluation data, those line task items have been transcribed into the spreadsheets. Now the DWE reports obviously also contain the DWEs for the jobs as well as for the entire facility. And also it's not 100 percent clear yet whether all the job evaluation data has been transcribed for a facility that are actually posted. I assume they are.

But I guess my question is do you anticipate transcribing these other DWE metrics into those particular --

MR. MORRIS: I'm not -- I want to make sure I answer exactly the question you've asked. Are you asking, are we going to go farther back to find the original air samples?

MR. STIVER: No, no, not that. But so far all that's posted are the task

items, the averages, the time for tasks, the type of samples, so forth --

MR. MORRIS: Yes.

MR. STIVER: -- but the actual DWEs aren't provided, nor is the DWE for the entire site.

MR. MORRIS: Oh, but those --

MR. STIVER: -- but I was just wondering if the --

MR. MORRIS: Well, my intent would not be --

MR. STIVER: What source data are you planning to use?

MR. MORRIS: And we'll just recalculate it. It's probably easier and more accurate for us to recalculate it with a spreadsheet than that's the original take of that.

MR. STIVER: Okay, all right. I was just kind of curious as to where that was going to go.

MR. MORRIS: I see the question.

I think we're going to stop where we are on this.

MR. STIVER: Now we talked about record availability here, and I guess the last one is really this Titan sample. There's a large amount of data that is provided to support this, but as I said, there's only about six to 25 percent is breathing zone; the rest is general air.

And the reason I brought this up is because there was considerable discussion about this whole issue at the March 2008 meeting, and then actually in the NIOSH draft response I copied out some text here. I think it bears repeating.

And then their contention here was that the uncertainties, particularly those differences in breathing zone versus general air samples, are compensated in TBD by combining the data, which increases the data spread. Basically, you've got a broader GSD. By adding more data, you're increasing the

robustness of the sample size, but also by using highly conservative assumptions for air concentrations and model input. The intake model includes the annualized thorium air concentration values calculated at the 95th percentile of the not normally distributed thorium air samples for each year. This results in a bounding estimate for intake that is biased high in favor of the claimant. Okay?

And a little later on here, it says, NIOSH emphasized the important point is there are clearly a large number of DWE records that are available to be used to reconstruct exposures in any manner deemed sufficiently conservative --

COURT REPORTER: Sir, I need you to keep your voice up.

MR. STIVER: Okay. On chronic thorium exposures for all workers. And I guess my -- this kind of gets more to the issue of the white paper.

Now one of the action items, or sub-action items in NIOSH's Action Item One, is to explain how workers will be assigned to low, medium, and high exposure potential. That's basically on the type of position they held, but I didn't see anywhere in the -- in the co-worker model where he addressed the paucity of data, as well, and how to high side to compensate for that lack of data in certain situations.

MR. MORRIS: You mean what a job description actually says?

MR. STIVER: Yes, so here you're saying that, well you know, it doesn't matter if you have a mixture. You have more general air samples that may not be use appropriate. Because we've got to high side all of our assumptions inside the 90th percentile. But you add in the Technical Basis document -- in your co-worker model, you can go to great lengths to describe how are workers going to be assigned to different categories based on

their exposure potential.

MR. MORRIS: I think that we need to understand those comments in the context of sequence. You know, the ones you just quoted are before our most recent version of the white paper, which has been informed by more information as we've gotten it. In fact, the information that you've presented this morning on job descriptions and exposures, where the mill man was the highest and a chemical worker was second highest, I remember --

MR. STIVER: Okay.

MR. MORRIS: -- we'll take that information and we'll fold it back in to helping make that decision about whether a worker is in that low, medium, or high category.

MR. STIVER: I understand how you did that. I mean, you go to great lengths to categorize all the different job descriptions, but in the situation where you have sparse data, and so you try to compensate for that by

assigning somebody to a high level, that automatically puts them into the high exposure category.

Now how does that -- I guess I didn't see there was any mechanism in that white paper to address that particular subject.

MR. MORRIS: Well, I'm not exactly following you. That's my problem right now, but we'll specifically deal with that if you can give us a real concrete example to work from, and I'll be happy to take it --

MR. STIVER: I guess maybe because this is older discussion and things have taken place since then --

MR. MORRIS: Yes.

MR. STIVER: -- some of those issues have been resolved.

MR. MORRIS: Perhaps but nonetheless I think your comment is one that if it didn't come through clear in our white

paper, we need to make it clear. And so if you can give me a concrete example I'll be glad to work with it. And we can do that offline.

MR. STIVER: Okay, we can do it offline.

MR. MORRIS: Sure.

MR. STIVER: Now let's see. Well, you know, despite all this talk about, you know, the appropriateness of general air versus breathing zone samples, I think looking at the actual DWE reports show that they really are kind of a mixture and that they're really appropriate to the particular task at hand, so the reason -- another reason I brought this up was that in looking at the site profile there was a large discussion on this, and the table presented showed this kind of a plot of breathing zones versus general air samples and how the GAs were consistently low.

And I guess that would be

appropriate, if you're taking the two different types of measurements of the same basic task.

But it looks to me like this DWE approach is pretty robust, and the data are taken for the type of samples that's really appropriate for that particular analysis. So I don't really think that's an issue here, at least as far as I've been able to tell by my review.

I guess we could go on, if nobody has any other questions about Tables Two and Three. Look at Table Four. Table Four was really a completely separate set of data that Bob Barton had located on the HIS-20 database back my second week of employment with SC&A, where we naively assumed that this was the thorium data, and this is all there was.

And so we downloaded this data, and it turns out it's -- these are not averages. We have the actual air sampling data, and what we did is we went through and

cleaned it up and calculated some general statistics, did some log-normal transformations and some percentiles and the distribution fits. And for each of those data we summarized it by a total for year as well as by each plant that's characterized per year. We've got the number. And let me back up one minute.

These are all breathing zone samples. There's also a lot of general air samples that went along with this data set. At the time we were really concentrating on the breathing zone. And the reason I included this was because it looks like there are a large number of these data that may be useful in supplementing or at least validating some of the DWE data.

Now, of course, this is contingent on being a separate data set, and I'm not quite sure whether this data was indeed some of the raw data that went into creating the DWEs in the first place.

Back in the March meeting there was an extensive discussion about these 3,000 samples of thorium data. Now this may very well be the same data set. I don't know if it is or not.

MEMBER GRIFFON: Can you tell me. I'm catching up a little here on this thorium data, looking online and this may be a question for NIOSH but you're saying the raw data -- is this -- I know you approached a bunch of things. I'm trying to go through some of them now, like I say, catching up.

This says DOE raw data may contain Privacy Act. Is that -- or DWE, I'm sorry, DWE raw data. It's an Excel spreadsheet; is that the one?

MR. STIVER: Correct.

MR. ROLFES: That would be the DWE data. We basically had our data entry team from ORAU go through each daily weight of exposure report by year, by plan --

MEMBER GRIFFON: Okay.

MR. ROLFES: -- and extract relevant --

MEMBER GRIFFON: But it's not raw data?

MR. ROLFES: Yes, it is.

MEMBER GRIFFON: It's not the sample data. It's the data from the report. So it's the averages, and this goes back to I'm having deja vu again, but it goes back to my original question. You have a radon sampling. You have a high of 64,778, a low of eight, and you have an average. And I think you're using the average for your modeling. Am I correct, or you're getting -- actually, those averages go into building a job -- for each job.

MR. ROLFES: And so there might be a very high concentration for a short period of time.

MEMBER GRIFFON: Right.

MR. ROLFES: And so that's factored into an overall --

MEMBER GRIFFON: Right, and this is a 55-minute sample, so I'm assuming it's that task, that one task or whatever, and then they get an eight-hour for whatever job that is, right?

It is interesting though to look at these highs and lows that I think anyway, because you sort of wonder what worker was getting eight while the other worker's getting 64,000 doing the same thing.

MR. MORRIS: Well, they were on different days. They were not --

MEMBER GRIFFON: Okay, so are we talking about the DWE day? I hear my days but they're supposed to be representing the same task.

MR. MORRIS: I think you're talking about air samples, aren't you?

MR. STIVER: We are talking about the raw air sampling data.

MEMBER GRIFFON: We might be -- that's what I'm trying to figure out. I don't

want to be talking apples and oranges.

MEMBER ZIEMER: This is --

MEMBER GRIFFON: This is the breathing zone?

MR. ROLFES: Correct. That would have been the raw data that was basically compiled into a single spreadsheet. That was not the raw DWE data. These are raw air samples --

MEMBER GRIFFON: The title is DWE Raw Data.

MR. ROLFES: -- which may or may not have been used in the daily weight of exposure reports, so I don't know if these were separate samples that were taken, in addition to the daily weight of exposure --

MEMBER ZIEMER: Thirty-six were the high, low, and average, so --

MEMBER GRIFFON: Right, right.

MR. STIVER: So those are probably are the DWE.

MEMBER GRIFFON: This must have

come off the job sheets. And then you sort it by task, it looks like because there's --

MR. STIVER: Yes, it's sorted by task.

MEMBER GRIFFON: Yes, but then there's year, plant and category, and this one is sample prep operations. And then it tells the operation --

MR. ROLFES: Yes, it kind of looks like --

MEMBER GRIFFON: That's the worksheet that it came from, yes.

MR. ROLFES: So it is in a daily weight of exposure spreadsheet is what you're saying, Mark? It's from the DWE?

MEMBER GRIFFON: Well, the title -- the title that you -- that it is --

MEMBER ZIEMER: If you call it DWE raw data.

MEMBER GRIFFON: DWE raw data, yes. It's in your DWE white paper folder, yes. It's in the DWE white paper folder, so

I don't know which one's which but there's three spreadsheets and a white paper.

MR. ROLFES: That's correct and, yes, that is extracted from the daily weight of exposure report.

MR. STIVER: Those are the data that you --

MR. ROLFES: Yes.

MR. STIVER: Okay, let's see, where did we leave off here? Yes, Table Four.

Now like I say I posted this with this other data set because I felt it might be useful as a supplement or also as possibly a -- another data set that may be used to invalidate or benchmark the statistics that were calculated based on the daily weighted averages using actual results for a particular facility and time.

Is Bob Barton on the line?

MR. BARTON: Yes, I am.

MR. STIVER: Bob, do you have any more insights to where that data came from or

how it was related to the DWE data?

MR. BARTON: That first set that we downloaded?

MR. STIVER: Yes, that first set that we downloaded back on, I think it was March 11?

MR. BARTON: It's how that was originally intended to be used.

MR. KATZ: Bob, your voice is breaking up. I don't know whether you're using a speaker phone or --

MR. BARTON: Can you hear me okay now?

MR. KATZ: Yes, that's better. Thanks.

MR. STIVER: Yes, that's better.

MR. BARTON: Okay, to start over again, I did not find any guidance as to how those air samples were going to be used. The original going in to try to find this data set there, and that's why we originally go in that direction.

MR. STIVER: Okay, it might be worth our while to -- to, you know, do some comparisons against the DWE data and just see, you know, whether we can kind of get a match up and see whether in light of what actually might have been the source data.

And if not it could be pretty useful as a supplement to what's already out there.

MEMBER GRIFFON: Can I just ask -- and I apologize. I had to step out and take a phone call, so I might have missed this, but -- or else we discussed it at previous meetings and I'm blanking out on it, but the -- when you say high, medium and low job categories how are you assigning doses to each one of those categories. What's the -- is it a co-worker model with all this data in it, or what's the constant?

MR. MORRIS: I don't have it open but it's 16th percentile, 50th percentile --

MEMBER GRIFFON: Sixteenth, 50th

and 84th, something like that?

MR. MORRIS: I think that's right, and one has variability and one's a fixed number.

MEMBER GRIFFON: Okay, so it's in the white paper?

MR. STIVER: Yes, it's in the white paper.

MEMBER GRIFFON: And it's based on the values populating that distribution part of the average. Are they job averages or what's populating that distribution?

MR. MORRIS: They're really facility averages.

MEMBER GRIFFON: They're facility averages.

MR. STIVER: Averages the DWE for each job description.

MR. MORRIS: The reality is, you know, we talked about it.

MEMBER GRIFFON: Each job or each facility or what?

MR. MORRIS: The white paper has probably more detail and I would put in it if I were writing it again today. I described how if you knew exactly the job description of the person and how you can match a DWE report for that facility --

MEMBER GRIFFON: Right.

MR. MORRIS: -- you don't have -- you can reduce your uncertainty side really matching it up. But the reality is that most of the time we won't have that.

MEMBER GRIFFON: Right.

MR. MORRIS: So what we would then do is say here's the DWE spread for the facility. It goes from -- a job description has got this little of exposure.

MEMBER GRIFFON: So you have this distribution for each plant, for each Plant One, Plant Two, Three, and not necessarily -- or over --

Do you have different distributions for different years or --

MR. BARTON: Yes, every year for -

-

MR. MORRIS: Every facility, every year gets its own spread.

MEMBER GRIFFON: Okay, got it.

MR. MORRIS: And just to answer, Mark looked this up for me a lung-exposure potential is a constant at the 16th percentile of the distribution. Medium is the 50th percentile of what the GSD -- based on the observed GSD for the data, and the high is 95th percentile.

MEMBER GRIFFON: Ninety-fifth, okay. And -- I think that's it for now.

MR. STIVER: Okay.

MEMBER GRIFFON: Thank you.

MR. STIVER: Okay, we haven't really gone into any analysis of the white paper in any detail but because at this point we're really trying to sort out the data -- the data granularity and veracity and applicability, and I think once we have that

information in a situation we have a clear picture of what data are available, where the gaps are, then it might be more useful to conduct a more systematic review if the advisory board feels that that's appropriate for the white paper and maybe come back with some comments on that, as well.

But I think that going forward I think the best thing to do is to probably get those DWE reports that identify reports and get those transcribed, and then we can probably from that maybe do something similar to what John did, maybe not to that level of detail in assessing the granularity and where the gaps may be.

MR. MORRIS: But I guess my thinking is that's why we just went off and did this demonstration, to show that our data were going to be good enough. And, you know, we know we can go transcribe that and apply it to the white paper. The question is is that going to be what we need to bound doses in the

SEC context.

DR. MAURO: Yes, I think that -- when you were summarizing the previous meetings that it all started to come back. It was not the original intention to load up everything.

MR. MORRIS: Correct.

DR. MAURO: It was because of the massive amount of material, we deliberately picked selected years and buildings as being good ones to represent the entire set, and if those hold up well, those years and those buildings, in terms of the ability to recharacterize --

MEMBER GRIFFON: That's right.

DR. MAURO: -- these intakes --

MEMBER GRIFFON: It's coming back.

DR. MAURO: Yes, it's coming back. We'll stop. Now is that right now are -- is the database complete with regard to those years and those buildings?

MR. STIVER: For those years and those buildings from Table Three, we're halfway there, but there's not that many more reports that need to be transcribed. I think there's like 11 or 12 of them on there.

MR. MORRIS: I was under the impression we have done all that.

MR. STIVER: Actually, the ones that were requested were for '55 -- all buildings for '55, all buildings for '66 in Plant Six for 1960?

MR. ROLFES: Correct.

MR. STIVER: And I did not see that that data was complete for those facilities. That's why we decided to take more of a generalized survey of what's actually out there.

MR. MORRIS: I see.

MR. STIVER: You can see in Table Two what's there for '55 and '66. There's some gaps that have not yet been transcribed.

DR. MAURO: You know what, just

to help you a little -- looking at Table Two the original plan was to have a complete set for which plants?

MR. STIVER: A complete set for all plants for the year 1955 and 1966. I think in '55 you don't have Plant One. You don't have Two, Three or Four --

DR. MAURO: Oh, okay.

MR. STIVER: -- or Eight or Nine. You don't have any of those.

DR. MAURO: This is very helpful the work group.

MR. STIVER: And the same for '66. You have the same basic --

DR. MAURO: Where there's ground that means that in order for us to do the things that were asked of us to do, we still need NIOSH to provide that information.

MR. STIVER: Yes, those reports are available but haven't been transcribed.

DR. MAURO: They haven't been transcribed.

MEMBER ZIEMER: So everything in brown?

MR. STIVER: Everything in brown.

DR. MAURO: In other words, all the plants in 1955, right?

MR. STIVER: All the plants in 1966, as well.

DR. MAURO: And all the plants in '96, there will be no cross in '55 and there won't be any place where I guess there is a brown with an X in it. That means this is something that exists but hasn't been transcribed.

MR. STIVER: Hasn't been transcribed, correct.

DR. MAURO: So '55 and '66, and there was one more that you said.

MR. STIVER: Well, Plant Six in 1960. That was not included either. We have '59 but we don't have '60.

DR. MAURO: So in theory if we were going to continue on the path that we

originally laid out, that information would be provided in the O drive. We would then go in and do an analysis of that data.

MR. STIVER: That was the original plan at the time. Now that still doesn't really -- there are a couple of things here.

To do that would require just as much effort as it would to get those sheets I indicated in Table Three for thorium, and by doing that with the Table Three worksheets we would then be able to have a clear picture of the thorium issue, not necessarily the uranium component but the thorium component because for the same amount of effort they could really bring this thing to a head.

DR. MAURO: A shift in plan to go --

MR. STIVER: A shift in the plan to -- rather than look at those original plants --

DR. MAURO: Yes.

MR. STIVER: -- which had not actually been done probably because for some reason other parties came along and other data was available initially. For whatever reason, those plans were not transcribed, so to go ahead and finish that out would be as much effort when we look at the numbers of plants that still need to be done as it would be to go ahead and just, you know, get the ones that we identified that pertinent to thorium.

DR. MAURO: The ones that you feel --

MR. STIVER: The ones -- yes, based on a time line.

DR. MAURO: And where would that leave you?

MEMBER GRIFFON: I thought those ones we picked originally were pertinent to thorium, but we learned more about the campaigns.

MR. STIVER: Yes, the more we learned about it, we discovered a lot more.

MR. ROLFES: John, you were mentioning that for 1955 the brown on Table Two denotes that the report exists but we've not transcribed it into a spreadsheet.

If you take a look we did send three different -- three different DWE raw data spreadsheets, and if you take a look the spreadsheet that I'm looking at has 1955 Plant One and it has DWE data. I'm not sure if we're --

MR. STIVER: Okay, I got -- we got two spreadsheets. We didn't get a third, so maybe there is a third that has more of this data available.

MR. ROLFES: There are three out there, and let me point them out to you.

MR. STIVER: I don't have access to --

MR. ROLFES: We have the DWE raw data dash Privacy Act Information, Excel file which is dated 03-24-2009. The Fernald DWE raw data granularity, 04-16-2000.

MEMBER GRIFFON: That's the one I showed you, yes.

MR. STIVER: There's one at 04-16 which is raw data by plant year.

MEMBER GRIFFON: And that was the biggest one that had the most data.

MR. STIVER: Let me go back to the actual data files here.

MEMBER GRIFFON: And then there's an FMPC.

MR. ROLFES: That was the copy of DWE for 04-16. And then there's, let's see, the third one.

MEMBER GRIFFON: FMPC, DWE --

MR. ROLFES: Correct. And the one that has the 1955 data would be the DWE raw data dash may contain Privacy Act, so there is a total of three that are available out there. They were all added on March 24, 2009, to the advisory board.

No, I take that back. That is the date that I put them on my disk. They are on

the advisory board review board.

MEMBER GRIFFON: They're on the DWE white paper.

MR. ROLFES: And also with the Microsoft Word file that describes the approach. Three Excel spreadsheets and the --

MR. STIVER: We only have two of those. The third one then only has that 1955 data.

MR. ROLFES: I think we've completed the data transcription for really more than we were tasked to.

MEMBER GRIFFON: So you think you did all those --

MR. ROLFES: I think we did.

MEMBER GRIFFON: -- and SC&A just didn't see that last -- or didn't get that last sheet.

MEMBER ZIEMER: Well, maybe they can work that out.

MR. STIVER: We can work that out.

MEMBER ZIEMER: What needs to be done on this job? What's the next step.

MR. STIVER: Well, the next step I think is really to flush out the rest of the thorium, the data that's pertinent to the -- Table Three, those particular sheets. If we can get those we can really come to where we have a clear picture of the data.

MR. MORRIS: And if I might suggest, really you should be judging all of the white paper approach, because that -- you demonstrated today that there's a robust set of data.

MR. STIVER: Oh, yes.

MR. MORRIS: And the real question now is what are we going to do with it. We made a proposal about what we're going to do with it. And somebody needs to say yes.

MR. STIVER: I'm not a hundred percent clear that it's -- that all the data's available that we need.

DR. MAURO: Do we have a need for

a group of principles -- step for this process, in other words a case and show how it would be done. One of the things that's often done is say, okay, we've got all these data. There's a white paper describing how you're going to do a dose reconstruction.

MR. STIVER: Why don't we just go ahead and take that white paper and try it.

DR. MAURO: Try one out?

MEMBER GRIFFON: We have to kind of test one. The question is do you have the information that you're laying out.

MR. STIVER: Yes, yes, at this point this is just a preliminary snapshot and it's by all means not complete, but I believe that would be certainly a logical next step would be to --

DR. MAURO: Well, there would be two different -- I mean, first of all does the work group want to -- you understand where we are now. Obviously, you have a sense of -- and it sounds like do you want us to continue

--

MEMBER GRIFFON: Yes.

DR. MAURO: -- and put a white report out. And second do we want to stick with the old plan, or do we want to go with your recommendation. Let's go with Table Three. Right now it sounds like that NIOSH has loaded up all the data -- '55, '66 -- it's there we just don't find it. And we can just continue down the road we planned.

MR. STIVER: I guess the next step really is to ascertain what's in that third spreadsheet.

MR. ROLFES: Yes, the third spreadsheet does include 1960 plant data, 1966. It's got several plants. The 1955 data has several plants.

MR. STIVER: Okay, could you take a look at the handout, Table Three, the DWE report not yet transcribed? And can you see that the third spreadsheet has these particular reports.

MR. ROLFES: DWE report not yet transcribed --

DR. MAURO: Yes.

MR. ROLFES: Okay.

MR. STIVER: I've got a plan for '54, '56 and '66.

MEMBER GRIFFON: Oh, a pilot plan?

MR. ROLFES: I have got roughly 1,500 data points in here so you're looking to see if pilot plant for --

Okay, we've got 1955, Plant Nine. Maybe it would be easier for me just to read off --

MR. STIVER: Okay, so that's one that we need right there.

MR. ROLFES: 1955, Plant Four?

MR. STIVER: Okay, that's another one that we need.

MR. ROLFES: 1953, pilot plant?

MR. STIVER: Not really pertinent.

MR. ROLFES: 1956, pilot plant?
MR. STIVER: We do have that, good. That's one we need.
MR. ROLFES: 1960, Plant Six?
MR. STIVER: Yes, yes, we need that one.
MR. ROLFES: 1966, Plant One?
MR. STIVER: Yes, we have that one.
MR. ROLFES: 1966, Plant Eight? And I don't know. I started in the middle somewhere so let me reiterate. If -- I apologize if I'm repeating myself here, but 1955, Plant One?
MR. STIVER: Got one at '55, okay.
MR. ROLFES: 1955, Plant Nine?
MR. STIVER: We've got that, yes.
MR. ROLFES: 1955, Plant Four? Have I repeated those?
MR. STIVER: You've repeated those. 1955, I think you've already gone

through.

MR. ROLFES: Okay, so that -- any other data.

MR. STIVER: Do you have anything for '54 for pilot plant in Plant One?

MR. ROLFES: Let me take a look in the other files here and check.

CHAIRMAN CLAWSON: I apologize, but I guess I'm kind of confused on a path forward. Are we going to continue on with --

MEMBER ZIEMER: Well, I would -- critique the white paper.

MR. STIVER: Yes, it looks like just from what we see right now we have more than half of what we thought was not yet transcribed here, so I think we're well on our way to be able to critique the white paper.

MEMBER GRIFFON: And the other thing, and let Brad finish us off here, but I'll send this updated matrix out to you because I can tell you there's some things hanging, like the later -- when you're using

in vivo for thorium. It's the later years.

MR. ROLFES: Yes, I think we discussed that in pretty much detail at a previous working group.

MEMBER GRIFFON: In here it says action, so I just highlighted those. If they come back and we all agree that it's closed, that's fine. I'm just going to highlight them, then the next time we meet we'll sort of check those off and get rid of them.

MR. ROLFES: Do you recall what the action might have been there?

MEMBER GRIFFON: Well, I have several pages here, but --

MR. ROLFES: I want to make sure that if there's something that we were asked to do that we completed it.

MEMBER GRIFFON: It actually says SC&A will review NIOSH white paper for the in vivo.

MR. ROLFES: Just as far as I can tell from everything that I have been

tracking, NIOSH has completed --

MEMBER GRIFFON: Yes, every action.

MR. ROLFES: -- everything that we've been asked to do.

MEMBER GRIFFON: So I'll just -- I'll highlight -- I think we just, you know, we had certain high priority ones, then we had some other ones. I just don't want to lose track of the ones that might not have been on people's radar, so I'll do that and Brad can get it out.

CHAIRMAN CLAWSON: I appreciate that, but on this thorium issue I want to get my hands on where we're going. We're proceeding ahead. As we previously stated, SC&A is going to review NIOSH's white paper --

MEMBER GRIFFON: And complete the data review.

MR. STIVER: Complete the data review.

CHAIRMAN CLAWSON: Okay, did I

leave anything out on it or --

Okay, then that should conclude us for today. Is there anything else that needs to be brought up before we leave.

MEMBER GRIFFON: We're all tired.

MR. KATZ: Thank you, everyone on the phone. The meeting is adjourned.

(Whereupon, at 5:45 p.m. the above-entitled matter concluded.)

G

Q