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CENTERS FOR DISEASE CONTROL
NATIONAL INSTITUTE FOR OCCUPATIONAL
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ADVISORY BOARD ON RADIATION AND
WORKER HEALTH

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WORK GROUP ON FERNALD SITE PROFILE AND SEC

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FRIDAY
JANUARY 29, 2010

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The Work Group meeting convened in the Zurich Room of the Cincinnati Airport Marriott Hotel, 2395 Progress Drive, Hebron, Kentucky, at 9:30 a.m., Bradley P. Clawson, Chairman, presiding.

PRESENT:

BRADLEY P. CLAWSON, Chairman
MARK GRIFFON, Member*
ROBERT W. PRESLEY, Member*
PAUL L. ZIEMER, Member

ALSO PRESENT:

TED KATZ, Designated Federal Official
ISAF AL-NABULSI, DOE*
SANDRA BALDRIDGE, Petitioner
ROBERT BARTON, SC&A*
HANS BEHLING, SC&A*
MEL CHEW, ORAU Team*
HARRY CHMELYNSKI, SC&A*
LOU DOLL, Public
SAM GLOVER, OCAS
STUART HINNEFELD, OCAS
EMILY HOWELL, HHS
JEFFREY KOTSCH, DOL*
RICHARD LEGGETT, SC&A*
JENNY LIN, HHS
JOYCE LIPSZTEIN, SC&A*
ARJUN MAKHIJANI, SC&A
JOHN MAURO, SC&A
ROBERT MORRIS, ORAU Team*
GENE POTTER, ORAU Team*
BRYCE RICH, ORAU Team*
MARK ROLFES, OCAS
JOHN STIVER, SC&A

*Present via telephone

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Adjourn

P R O C E E D I N G S

(9:30 a.m.)

MR. KATZ: Good morning, everyone in the room and everyone on the line. This is Ted Katz. I'm the Designated Federal Official of the Advisory Board on Radiation Worker Health, and this is the Fernald Working Group.

I'm going to begin with roll call first, beginning with Board Members in the room.

CHAIRMAN CLAWSON: Brad Clawson --

MR. KATZ: Please state if you have a conflict of interest. All the agency-related people, please state that.

CHAIRMAN CLAWSON: Brad Clawson, Fernald Work Group Chair, member of the Advisory Board. No conflict.

MEMBER ZIEMER: Paul Ziemer, Board Member. No conflict.

MR. KATZ: And that's it in the room for Board Members. How about on the line?

MEMBER PRESLEY: Robert Presley. No.

MR. KATZ: Bob Presley, I'm glad you could make it.

Any others? Do we have Mark Griffon yet?

(No response.)

MR. KATZ: Or Phil Schofield?

(No response.)

MR. KATZ: Okay. Then NIOSH-ORAU team in the room.

MR. ROLFES: This is Mark Rolfes, health physicist from NIOSH. I have no conflict of interest.

MR. CHEW: Mel Chew, from ORAU Team. No conflict.

DR. GLOVER: Sam Glover, NIOSH, health physicist. No conflict.

MR. MORRIS: Robert Morris, NIOSH team. No conflict.

MR. KATZ: Robert Morris, welcome.

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

MR. KATZ: Bryce Rich.

MR. POTTER: Gene Potter, ORAU Team. No conflict.

MR. KATZ: Very good. SC&A staff in the room.

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

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

DR. MAKHIJANI: Arjun Makhijani -- have a conflict.

MR. KATZ: And then SC&A staff on the line.

DR. BEHLING: Hans Behling. No conflict.

MR. KATZ: Welcome, Hans.

DR. LIPSZTEIN: Joyce Lipsztein. No conflict.

MR. KATZ: Welcome, Joyce.

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

MR. KATZ: Welcome, Bob.

Is that it for SC&A on the line? Okay.

MR. CHMELYNSKI: Harry Chmelynski, SC&A.

MR. KATZ: Hi, Harry.

Okay. And in the room, HHS and other government agency employees or contractors.

MS. HOWELL: Emily Howell, HHS.

MS. LIN: Jenny Lin, HHS.

MR. KATZ: And on the line? HHS or government.

MR. KOTSCH: Jeff Kotsch, Labor.

MR. KATZ: Welcome, Jeff.

MS. AL-NABULSI: Isaf Al-Nabulsi, DOE.

MR. KATZ: Welcome, Isaf.

MS. AL-NABULSI: Thanks.

MR. KATZ: Okay, and then members of the public or staff of congressional offices or others in the room.

MS. BALDRIDGE: Sandra Baldridge, petitioner.

MR. KATZ: And welcome to you, Sandra.

And on the line? That's it in the room. Any members of the public or staff of congressional offices who want to identify themselves?

(No response.)

MR. KATZ: Very good. Then let me just ask. Everyone on the phone, the usual reminder. Please mute your phones. If you don't have a mute button, use *6 and *6 will take you off mute again. And please do not put the call on hold at any point. Just disconnect and dial back in if you have to leave the call for some point.

Thank you, and Brad, it's your meeting.

CHAIRMAN CLAWSON: Okay.

MEMBER GRIFFON: Hi. Ted? Ted, this is Mark Griffon.

MR. KATZ: Oh, Mark, great. I'm glad you could make it.

MEMBER GRIFFON: I'm sorry. I came on late, and I'm going to have to leave for a little while, but Brad knows about this, but I just wanted to say I will be back in a little while, as soon as I can.

MR. KATZ: That's great.

MEMBER GRIFFON: I just wanted to dial in just to say hi, and I'll talk to you in a little while.

MR. KATZ: Thanks. And, Mark, why don't you just let us know when you're cutting out and rejoining us.

MEMBER GRIFFON: I will. I'm actually going to have to cut out like pretty much right away here.

MR. KATZ: Okay.

MEMBER GRIFFON: And then I'll rejoin probably around 11, but maybe a little before 11.

MR. KATZ: Okay. Good.

MEMBER GRIFFON: All right.

MR. KATZ: Thank you.

MEMBER GRIFFON: Thanks.

CHAIRMAN CLAWSON: Okay. Well, it's been a while since Fernald Work Group has met, and on January 14th, we held, not a Work Group call, but just kind of a to-come up-to-speed on everything of where we were at on the issues, and so forth like that.

What we're going to be using today to be able to go over this is the letter that John Mauro set out clarifying on February 15th what their understanding was, and the first issue that we need to address falls into SC&A's court, and that is the uranium bioassay coworker model.

We had an OTIB-0078 that I believe you were supposed to look at and see where we were at. So I'll turn that over to you, John.

DR. MAURO: Thank you.

MR. KATZ: One thing. Just for the folks on the phone, for the benefit of people who don't have this, which is going to affect our agenda, let me just quickly --

CHAIRMAN CLAWSON: Yes, sorry.

MR. KATZ: -- just so everyone knows, the first issue the Work Group will deal with is this uranium bioassay coworker model.

The second issue is validation of the HIS -- H-I-S -- 20 Database.

The third issue will be recycled uranium.

The fourth issue will be radon breath analysis and associated reconstruction of radium-226 and thorium-230 exposures.

And the sixth issue will be the thorium-232 dose reconstruction, just to let everybody know sort of the layout of the day.

CHAIRMAN CLAWSON: Okay. Thanks.

DR. MAURO: Yes, this is John Mauro. I'm leading up the SC&A team on Fernald.

Just as background information, the six issues that were just identified, these -- behind them are large White Papers that have been issued over a two-year period. All those White Papers have been filed, PA reviewed, loaded up on the Web. They're available for anyone by topic.

What has happened subsequent to that, of course, is that we've held discussions on these, and as Brad pointed out, because of the time span over which these discussions have been held we sort of regrouped, and as a result of regrouping recently, I issued a memo which basically summarized my understanding of the status of each issue, and the action items, follow-up action items, in anticipation of this meeting so that we can move expeditiously.

So all of the technical material that we're going to be talking about is PA cleared. It's on the Web, and the new material that we'll talk about; we are going to be talking about, you know. So I think that we're in pretty good shape.

Issue No. 1 has to do with the uranium bioassay coworker model. Basically, that is a set of tables that NIOSH has assembled from a vast amount of bioassay data of uranium in urine, and using that data, the plan is to use that data to reconstruct the doses, internal doses, to workers.

And for workers that don't have any or have limited bioassay data, they built a coworker model using that data.

We reviewed that very, very

carefully. In fact, there's a large report on it, to see if, in fact, all different time periods, all different buildings, types of categories of workers are captured so you could pigeonhole a person and reconstruct his doses.

We found that a very robust report. The data were complete. We only had one concern, and that concern was that the instruction given to the dose reconstructor was to use either the median or the full distribution. So, in other words, if you have a worker that doesn't have any bioassay data for a given year or time period, you go in and use the coworker model, and you pick off the median value in the model because they give a range of values.

We did a lot of work to show that, you know, there are some categories of workers and some time periods and some buildings where the median is not claimant-favorable, where we found that you can't just automatically assign the median. You've got to be a little careful. You've got to do it on a case-by-case basis.

So our recommendation was that some modification to the language in their guidance capture that, and during our conference call last week, NIOSH pointed out that, well, they've issued OTIB-0078, which is the formalization of the coworker model that they developed, and that in that formalization, there was language to that effect.

Well, unfortunately, I have to say that I read through it, the OTIB-0078, and others within our organization. The language is not there. So I would beseech, at some point in the process when the TBD is updated or the OTIB-0078 is updated, that a little bit more language is put in because right now the language that's there is very, very limited.

A little bit more could be done to give some guidance and caution, the dose reconstructor, you know, to take a better look before you just jump and use the median.

But as far as I'm concerned, this issue has been resolved, and it's really a matter of making sure that at an appropriate time and place the guidance explicitly makes that point clear.

MR. ROLFES: Yes, sir, John. I agree that we can just simply insert a couple of lines to address consideration of the 95th percentile intakes for certain workers.

DR. MAURO: Yes, and quite frankly, that's it. So we agree in principle. It's just a matter of, you know -- in fact, even on the phone call we said we think this issue is revolved, but the work, we felt that, listen, really until the document is fixed, it really can't be closed. So we sort of put it on the shelf until that document is fixed.

And with that, we can move on unless anyone else wants to discuss it further.

CHAIRMAN CLAWSON: No, I just want to make sure that we've got a handle on what the path forward is on this, John. I understand that till we see OTIB-0078 and the changes that are being made to it to address this. Like you said, it really --

DR. MAURO: It doesn't. I did look at OTIB-0078, and it doesn't fix it.

CHAIRMAN CLAWSON: All right. So until that is done, then this item is still open.

DR. GLOVER: John, this is Sam Glover.

Could we clarify that there's SEC issues and TBD issues, and that we could --

DR. MAURO: Yes, and this is not an SEC issue. This is, in my opinion -- I hate to -- you know, as your contractor, I hate to come to those kind of bottom line conclusions, but it's self-evident. Here's a case where, I mean, the technical issues have been resolved. It's just a matter of clarification.

So I can't see anyone calling this an SEC issue.

DR. GLOVER: Because, you know, it takes some formal interpretation to fix documents.

DR. MAURO: Yes.

DR. GLOVER: It can take a long time, and I'd hate to hold up six or seven months where we've fixed documents and you go through a very elaborate process.

MEMBER ZIEMER: I wonder if you could clarify. This is Ziemer.

I wonder if you could clarify what that fix is going to look like with a couple of sentences. Are you just going to instruct the dose reconstructor to do what?

MR. ROLFES: I think that it would be appropriate for us -- right now we have a statement saying that we would use the 50th percentile intakes for a non-monitored worker to assign a uranium intake to them, and we've got those intakes listed in the Technical Information Bulletin.

What SC&A has essentially asked us to do is to incorporate some language to, basically, state that we would consider the 95th percentage for certain workers in certain time periods based upon the other facts of their case, considering, for example, recorded external doses, work time periods, and other information provided to us.

MEMBER ZIEMER: Okay. So you've agreed on what the nature of the wording will be without actually having it.

DR. MAURO: In fact, in our White Paper that stands behind this, we've pointed out which buildings, which years, and which job categories are the ones that are vulnerable. It might be worthwhile to just mention that, you know, if you are a guy who happens to fall into -- you know, I think the antennas should go up. Those are the three parameters.

And it's listed in our report. I forget the buildings and the time periods, but --

MEMBER ZIEMER: But there will be some specificity beyond some general wordsmith.

MR. ROLFES: Yes. However, we can't just, you know, because an individual was in this building and he had this job category, we can't automatically assume that they were exposed to the 95th percentile. We'd also have to get consideration for the amount of time that they spent in the building, what duties they were performing, what was operating at that time.

MEMBER ZIEMER: And those would be alerts.

MR. ROLFES: Yes.

MEMBER ZIEMER: And, number two, there could be other cases that haven't been covered by those alerts.

MR. ROLFES: Correct, but we don't want to --

MEMBER ZIEMER: But the dose reconstructor would have to --

DR. MAURO: It's important to point out that the vast majority, beginning in 1957, over 90 percent of the workers have data.

MEMBER ZIEMER: Anyway.

DR. MAURO: Anyway. So this is going to be, having to resort to the coworker model, is going to be the exception, not the rule.

Issue No. 2.

MEMBER ZIEMER: Well, one more

question though. Are you proposing that we close this or wait until we see it?

CHAIRMAN CLAWSON: No. Well, I wouldn't close it until we see what's been implemented on it. It's just we've got as far as, between us, we've got it taken care of, and it's covered, but I'd just like to be able to see that once it's implemented that it's implemented in the manner that was discussed. That's all I want to be able to see.

So does that answer your question, Paul?

MEMBER ZIEMER: Yes.

CHAIRMAN CLAWSON: Okay. On Issue No. 2, which is validation of the HIS database, the action item is actually with NIOSH and there was actually a White Paper that had been out by SC&A, and they were going to review that and get back, determine what they needed from there.

So I believe it's up to you, Mark.

MR. ROLFES: Okay. I prepared a short, little response here, and in the HIS-20 database we had considered in excess of 400,000 uranium in urine samples and looked back at SC&A's report briefly, and so it appears that there could be 8,000 to 28,000 samples that appear to be missing from HIS-20.

And the way we were able to determine that is that we have those results in hard copy and didn't have them in HIS-20. But we felt that that was not significant because, one, we have a tour for uranium intake model for unmonitored employees.

Two, with a single sample from an individual was not entered into his 20th, possible that it was an erroneous result. It's possible that it was a verified contaminated sample which wasn't reflective of the worker's intake, or it also could have been combined with another sample taken on the same day perhaps.

Furthermore, if the employee was monitored via urinalysis, given uranium's biological half-life, longer retention time in the body, the subsequent bioassay sample would likely reflect or integrate a previous exposure to uranium.

And, third, those with the highest exposure potentials at Fernald, as well as the highest lung burdens at Fernald, had the most frequent routine chest counts, and the chest counts are another source of information which can be used in dose reconstruction. The chest count results can allow us to validate or put an upper bound on an individual's previous uranium exposures to make sure that our end result is, indeed claimant-favorable.

So I guess that's our response, and essentially we found that in excess of 93 percent of the uranium urinalyses were, in fact, transcribed validly into HIS-20 and made it into HIS-20.

Furthermore, those results that did not make it into HIS-20, we didn't find that there was any kind of bias with the actual uranium urinalysis values reported. Some of the errors were misspellings of names, wrong plant, part of a Social Security number, or an extra number typed into the Social Security number.

We didn't find any kind of bias of uranium urinalysis results that were reported. We didn't see that any of the high results were removed, and we didn't see that any of the low results were, you know, removed.

So we feel that what we have is pretty good and pretty defensible.

DR. MAURO: Let me. I hear what you're saying, but I think we should back up a little bit so that everyone understands the context of this issue. The HIS-20 database is the electronic database that everything is done from, it all came from hard copy database that someone transcribed.

And you know, certainly to the credit of NIOSH, they said, listen, let's make sure that transcription was done faithfully.

And we're going to go through a sampling process. We're going to go into this vast amount of hard copy data and sample, take numbers out and see if, in fact, that number was taken out of the hard copy and put into the electronic database correctly.

And they imposed upon themselves what they call a military spec criteria of acceptability, which was, I believe, one percent. So they said, after we go through this process, if we could show that when 99 percent of the transcriptions were correct, we pass our test.

They went through and they found out that six percent failed instead of one percent failed. So you failed your test.

Now, in fact, when we looked at

this, we said, well, you know, strangely enough, we don't feel that that's a big deal because I think you've imposed upon yourself an acceptance criteria that was unnecessarily restrictive. That is, there is no reason why you have to have 99 percent faithful transcription. If it wasn't that good, there are ways to deal with it, but I think that that had to be dealt with.

Your report, I guess in effect your report, you know, comes out and says we've missed it. Now, you are giving now some reasons why you believe that it's okay to have six percent, but there's a little bit more to the story, and that has to do with shutting down the process of validation on given sets.

What I'm getting at is I think there's some more. I hear what you're saying, but I think there's some more work, mechanical work, that needs to be done to complete the record, and then after the record is completed that, yes, you've done the checks that have to be checked, then a case could be made by the kinds of transcription errors that you're observing can be managed.

Harry, are you on the line?

MR. CHMELYNSKI: Here.

DR. MAURO: Could you very nicely explain during our technical conference call, I don't know if it was 20 or 25 batches that they looked at? Some of them they sort of reined in and did not finish the completion of the checks, and I have to say when you explained it, it helps to understand the kinds of things that NIOSH might do to sort of close the book on this thing.

Could you give us a little rundown on some of the things that you felt could be done to help close the door on this one?

MR. CHMELYNSKI: Okay. First, I'd like to say that we're referring to these as transcription errors. I think that's slightly a misconception because what we found was that almost all the entries that were put into HIS-20 were correct.

The problem was there were some records that didn't make it into the database. So I'm not sure I would call that a transcription error. They're missing data.

And as NIOSH just said, there are reasons why maybe some of these are missing, and although the reviewer in the document said that there were also some, there's no reason why they were missing.

At any rate, there were some missing, and our best estimate was about six percent. And that may not be important. That's an open issue.

The procedure they used though to derive these numbers, this accuracy check, has a long history in the mil spec tradition of how it can be applied, and one of the distinguishing features it has is that it allows a reduced level of inspection that is meant for cases where you have an ongoing procedure, and everything is going pretty well in a factory sort of environment, and the shipments are going out, and every time we check them they're okay.

So at that point you're allowed to have a reduced level of inspection. However, NIOSH used this reduced level of inspection on this particular task, which I thought was inappropriate.

First off, it was a one time study, and second, they weren't doing very well on the ones they did look at.

So switching to reduced level of inspection for certain of these batches seemed inappropriate. So I don't think the study was ever completed in that sense, but those reduced inspections should have been at least upgraded to normal inspection or even 100 percent once it was found that the quality goal was not being met.

That would tie up this report. I'm not sure there's an issue here that we ought to throw out HIS-20.

DR. MAURO: I'm sorry. I didn't hear what you said, the last statement.

MR. CHMELYNSKI: There's no reason here, I think, to be suspicious of any of the numbers that are in HIS-20. Those were found to be almost 100 percent correct.

DR. MAURO: I guess would it be your recommendation that they complete the ones that they were doing or you feel that they're at a point that you feel satisfied that even though they didn't go through the complete process, you know, the reasons you described, that we could walk away and say that the HIS-20 database has been validated to our satisfaction?

MR. CHMELYNSKI: Well, I guess I would say that this report has not been published yet. It needs a lot of work to be published, and I think one of the things would be to complete the ones that had reduced inspection. There's other things it needs, too, which is sort of an overview of what the conclusion is.

MEMBER ZIEMER: Which report is he referring to, yours?

MR. ROLFES: He's referring to our analysis of the HIS-20 data.

MEMBER ZIEMER: Your analysis.

DR. MAURO: One of the things that we discussed was let's get to the point. Let's say you complete your analysis the way Harry suggested in his White Paper. You know, we have a White Paper which sort of lays all of this out, things that could be done to close the door.

I'd like to say a position that SC&A has taken, is that let's assume for a moment that you find out that six percent of the data were not transcribed.

MEMBER ZIEMER: As opposed to transcribed in --

DR. MAURO: Errors. Please excuse me. I was referring to as transcription errors as it's a hit, basically, and a hit turns out to be of a form that is, no, they didn't bring this number over. All right?

I think there were others, too. There were things like the guy's name might have been spelled wrong. So there was a variety.

But let's -- and this is what we talked about on the phone, and I think it's important to get on the record here -- is that the way we look at it is it's not uncommon for a record, an electronic record, to be incomplete. That's why we have the Whole Dose Reconstruction Center protocols. And as long as the six percent incomplete is really sort of like a randomly missed -- that's why you have a coworker model.

And the only time where if you miss a significant percent, whether it's because you left badges behind, whether you destroyed records, whether they were lost, it's when that particular set of missing records happens to be at the high end of your distribution.

Now, when that happens, you've got a problem. Then you can't build a coworker model. But there's no reason here to believe that that's the case. So I guess where I am on this is that if your formal document, finalized, addresses the issues or the recommendations that Harry pointed out and then at the end, the points that you just made in your summary become conclusionary, and maybe back off on the imposition of the one percent.

You know, I think it was admirable that you set that up, as we're going to assess it against that, but I think that was an unnecessary imposition on yourselves of a quality level that wasn't required for this kind of application, and you really can't even put up front what you really want to know, is that once you determine, you know, how much data might be missing, a judgment has to be made whether or not it's significant in terms of affecting your ability to do a coworker model and how to deal with it.

And then you will have a complete story. Right now that complete story isn't there.

MR. ROLFES: All right. I guess since we've said that the numbers that are entered into HIS-20 are good and we've got greater than 400,000 numbers in there, we've developed our coworker intake models. So for an individual that wasn't monitored for uranium, if they were in a radiation area, they would receive the 50th percentile coworker intake automatically.

You know, we can go back and look at some of the records that weren't entered into HIS-20, take a look at hard copy records, and I believe we had already done this. I think we had looked to see how much the missing results would have impacted the various intake rates over time, and from what I recall, they were, you know, a small percentage, maybe one microgram difference on a 50 microgram, you know, intake or something, you know. So it was pretty trivial.

DR. MAURO: And that's a good case at the back end. In other words, the way I look at it they're linear. You build a process. You are faithful to the process. The end result is, okay, we did check all 25 batches that became your sampling base. We didn't pull short, take advantage of the shortcut that the mil spec allows you to do under certain circumstances, but you didn't have those circumstances.

And so what Harry is saying is we really shouldn't have pulled back. Finish it up the way you began, take some work, so that it's done. At then end then you say, okay, we're coming out where we come out. Let's say it is six percent, and then you took a whole bunch of steps to say why we could live with that, and there you've got the end, story ends.

And so I'm not disagreeing with you. I'm almost looking at it as a complete record that's available to everyone to show that you have a sound documentation of the validity of the HIS-20 database. Right now it's soft because of these reasons.

MR. ROLFES: Yes, I guess my side of things is that these reasons don't appear to me to be important to dose reconstruction.

DR. MAURO: And I'll tell you the truth. I'll tend to agree with that.

MR. ROLFES: Okay.

DR. MAURO: But it would be very nice to finish the work and say that.

MR. ROLFES: Maybe we can add some text to specifically address what you're asking us to.

DR. MAURO: Yes, right.

MR. ROLFES: But if you could restate briefly what you're asking.

DR. MAURO: Harry explained it, and it is written up in our report, I mean, and I think what goes to the heart of it is the mil spec process the way I understand it is one where you enter into a sampling process for batches. Batches are coming off the assembly line, and that's a living process that goes on forever because you're making a product.

And you design -- I'm going to take one out of ten, you know, and check it. And as that process is going on and you're saying every time I do it I'm okay, I'm okay, I'm okay, then apparently you build a record that you're okay, you're okay. You could start to soften up. I'm only going to take one out of 20.

MEMBER ZIEMER: One out of 20, yes.

DR. MAURO: Yes. Well, you folks went through a few batches. You weren't meeting your one percent.

MR. ROLFES: Correct.

DR. MAURO: And you backed away. It seems to me that if you were meeting your one percent as you march through each batch, you could have backed away, but you weren't meeting your one percent. So don't back away. Finish it.

MEMBER ZIEMER: Well, I heard something different from Harry.

DR. MAURO: Okay, good.

MEMBER ZIEMER: He can speak for

himself --

DR. MAURO: Oh, yes, yes.

MEMBER ZIEMER: But this is a one time thing. It's not like you're producing product and your accuracy rate is changing. I think Harry said, number one, the transcription part, which is the mil spec was based on, was not a problem. All right? For those transcriptions you were within the one percent.

The only real issue is not everything was transcribed, but you're not producing an ongoing product. You don't have a new database coming up that you're sampling and it's a different rate.

So if I understood you, Harry, you're saying that, in a sense, for backing away this doesn't meet the way the military says they back away into a lesser sampling rate based on ongoing experience.

Did I understand that right?

MR. CHMELYNSKI: That's correct. In this whole project, I don't think it ever should have been considered. I think in a one time project, you never have that level of confidence.

By the way, the reduced sampling here means you look at 20 records, which in some cases you can get away with, but it's like I said. In this case there hasn't been a track record ever established. So 20 wasn't enough.

MEMBER ZIEMER: I don't think I like the way he described it.

MR. ROLFES: So is it a Board --

MEMBER ZIEMER: Well, I mean, you wouldn't just use 20 samples anyway to describe your accuracy rate on this database or whatever it is. I mean, you already have what you had, right?

MR. ROLFES: I'm not sure I'm following you. Much more than 20 samples were considered.

MR. CHMELYNSKI: I mean, in a particular batch. The reduced rate says that you can get away with 20 samples.

MR. MORRIS: But we did dozens of batches. This is Robert Morris.

MR. CHMELYNSKI: Yes, but there weren't any that were -- there weren't many that were within the goal though, and again, I don't think that the reduced level inspection is at all applicable in a one time situation like this.

MR. MORRIS: I'm not disagreeing. I just want to make sure you don't leave the impression that we only looked at 20 samples.

MR. CHMELYNSKI: Oh, no. I'm just saying that there were some batches where there were only 20 samples inspected. That's all I can say. Some of the batches.

MR. ROLFES: And I think that might have just been a result of the PDF size perhaps because we had compared some hard copy records. They were actually scanned into our electronic database separate from the HIS-20, and we compared the results in that PDF to the results in the electronic HIS-20 database.

Does that sound accurate, Bob or Gene?

MR. POTTER: This is Gene Potter. I actually worked on the comparison.

And let me first say that SC&A did a very thorough job, and many of their criticisms would be accepted. Others we'd have to argue with. They're both saying that our AQL was too high, which means we would have had or done bigger sample sizes then they're criticizing us for using due sampling.

So there's some inconsistencies in their work as well, in my opinion, but I think the bottom line is -- is correcting this study that we've done, is that an end product that is needed to make a decision when both SC&A

and NIOSH agree that 93 and a half percent or so in the worst case of the results are in HIS-20. Is this study an end in itself, which is what I think John Mauro is advocating?

DR. GLOVER: Is it fit for purpose?

This is Sam Glover.

DR. MAURO: In other words, what I'm hearing is does this thing need to be fixed before a judgment could be made regarding the SEC. Is this what I'm hearing? Is that the question that's being raised?

MR. ROLFES: I think that's essentially what we're asking, yes.

DR. MAURO: Is that your understanding?

MR. ROLFES: Yes. I mean, because to do additional work is going to take additional time.

DR. MAURO: I guess in mine -- this is my opinion, and I'm speaking as just an individual member of the crew around the table -- this becomes one of the rocks you're standing on. In the end, all of your dose reconstructions, all of your coworker models depend on the trust you have in the HIS-20 database.

And right now, based on what was done, there appears to be for the sampling that was done perhaps six percent error. Okay?

Now, but at the same time we know there were certain batches where the sampling wasn't complete. So that the actual percent that might be missing -- correct me if I'm wrong -- I mean, if you were to go through the thing the way Harry described, don't have any, which is 20. Let's go each batch. Go through the full treatment the way, you know, Harry suggested, and when you're done, you say, okay, this is what we have, we have for this batch this percent, for this batch this percent, for this batch this percent.

And then there's a process you go through to convince yourself the way you described that that does not affect my ability to come up with a sound coworker model for the following reasons, and you run the test you ran.

We're going to run some cases and see what happens. So, in other words, when I see -- you've got to bring it to closure, and until you bring it to closure, you're sort of leaving yourself in a funny plight. You're saying, we really never completely finished and documented our HIS-20 database validation, and it's really hard to move on from there with that sort of leaning in the wings.

I don't know. That's how I look at it. I know I feel a lot more comfortable locking that up, knowing that you're standing on a rock, put to bed, everybody agrees, and then after that, everything -- for example, you know, when we said we're okay with Issue No. 1 except for the words, but Issue No. 1 really is presuming that the HIS-20 database is everything's okay.

MR. ROLFES: Right. The two are tied together.

DR. MAURO: Do you see what I mean? So you put yourself in a funny place. I don't know if you want to be there. If you could put this one to bed, I don't know how much of an effort it is, but if you could put this one to bed, I think it will give the Board a lot more confidence that we're standing on a rock.

DR. MAKHIJANI: This is Arjun.

Can I ask Harry a question?

Harry, is there a way to characterize six percent that are missing compared to the 94 percent that are there? Are they kind of random missing or systematic missing? Do we know?

MR. CHMELYNSKI: No bias was found. We'll put it that way. Systematic? I guess you could say in some cases there were groups of them left out, but generally they were associated with one person maybe. So maybe he was left that for a reason, maybe not.

I think it's very speculative trying to figure out why they weren't put in the database. There were some reasons offered earlier, but --

MR. ROLFES: Right. There could be reasons behind those. It could be a contaminated sample. It could be, you know, some other explanation. To go back and try to identify each of those cases is actually something that we would do during a dose reconstruction, but to do it as a whole for the entire database is going to take a lot of additional time, and it's something in my opinion that really isn't warranted because we know that the data that are in the database from which the coworker intake models were derived, the numbers were essentially 100 percent accurate.

And it's the numbers that are important for us in a dose reconstruction.

DR. MAKHIJANI: Well, the point of my question was that if the answer is that it's clear that it's random, then you don't have to worry. But if, as Harry said, it's not clear, then I think, you know, you have got something to settle.

DR. MAURO: I think it has to be said. You see, one of the things that I encountered, and I always have problems. I'll read a report. It's sort of you're always left a little fuzzy. In other words, I like at the end of the report to say, listen. Okay. We found six percent error, and I would like to see where it would say, and do you know something? If this six percent error turned out to be a systematic bias that was at the upper end of the tail, we've got a problem. So we're going to run a series of tests to convince ourselves that, no, it was random and it could not bias the coworker model, not the guy that's doing your dose. I'm saying the coworker model because you have a coworker model that says for this year and this building, here's the 95th percentile, you know, and we're going to use that 95th percentile value for any guy that we're missing data for. Okay? You say you're going to do that.

Well, we're really hanging our hat on 95th percentile. We know it's good. Now, if there's any reason to believe we can't trust that 95th percentile because of some kind of bias in the transcription, you've got a problem.

So I look at it very simple. So if you could make a case that says, no, there's no reason why that 95th percentile and prove whatever process you go through is completely unaffected by this. It's unaffected. So, therefore, our coworker model is robust notwithstanding we may have missed six percent of the data.

And it's just plain language. It's just simple thinking, but it wasn't there. I mean, it took us a lot of thinking and figuring and a lot of work, you know, to bring this baby home, but I think we understand where you are and we think we understand how it needed to be fixed.

MR. MORRIS: Bob Morris.

It seems to me that Harry just went on the record saying that there is no inherent bias obvious in the data that we have.

DR. MAURO: Right.

MR. MORRIS: And I think that's the challenge that you just gave us, and Harry just gave us the answer.

MEMBER ZIEMER: Yes, I was going to ask the same question. Did SC&A run some tests?

In other words, Harry, your statement is based on what? Was that intuitive or did you guys run some tests on the missing data to assure yourselves that there wasn't any obvious bias?

Because in a sense, if you say there's no bias, that's another way of saying it's random.

DR. MAURO: Right. Oh, yes.

Harry, what I heard you say, you have a sense that you could not really see any systematic bias.

MR. CHMELYNSKI: Well, I'm basing that on the work that NIOSH did when they did the report, which was they looked at the data that found -- was not in HIS-20, and they said that when you add that data in, it makes very little difference in the estimates.

MR. ROLFES: That's correct.

MR. CHMELYNSKI: That says that those numbers weren't that much different to me.

On the other hand, the real issue isn't the numbers that we know didn't make it into the database. It's all the other numbers that we know didn't make it in, but we don't have them yet. We haven't found them.

DR. MAURO: Those are the ones where they cut short. They weren't the 20 samples.

MR. CHMELYNSKI: No, not the 20 samples.

DR. MAURO: Okay.

MR. CHMELYNSKI: I'm just saying that is six percent of them are missing, that means six percent of 400,000 are missing, which is 24,000 records. We don't know which 24,000 they are. We only know a handful of those 24 that we happened to see while we were doing this study.

We don't know all the records that are missing is all I'm saying. We just know the ones that we found when we did this study of a very small number of the records.

MR. MORRIS: Isn't it true that unless we do 100 percent sampling we'll never know them all?

MR. CHMELYNSKI: Yes, that's right. I'm just saying that we will never know why they're all missing, but the ones we did look at don't seem to be a problem is what I'm saying.

MR. ROLFES: Right. The numbers that we would use for, you know, dosimetry calculations, internal dosimetry calculations we found to be for the data that are there, the 93.5 percent of the data that is in HIS-20, we found that the numbers are good, and so that's the important thing. That's what I'm trying to make sure that we don't misrepresent the data if an individual, for example, only had one urine sample and that was the sample that didn't make it into HIS-20, we would apply the coworker intakes, and that really essentially would solve the problem.

DR. MAKHIJANI: So you're using HIS-20 for a dose reconstruction.

MR. ROLFES: The HIS-20 database is where the uranium urinalyses were extracted from in order to calculate the uranium in urine coworker study, intake --

DR. MAKHIJANI: Yes, I understood that. The individual dose reconstruction, you're starting with the HIS-20 data. That's your primary source for --

MR. ROLFES: We actually received individual exposure information from the Department of Energy, and it is from the HIS-20 database, and there is also some hard copy records that are associated with those as well.

MS. BALDRIDGE: Can I interrupt?

MR. ROLFES: Please.

MS. BALDRIDGE: This is Sandra.

You know, I don't have a problem with the HIS-20 database. My problem is you don't know when those samplings were done, whether they were done at low exposure times or high exposure times, and to assume that they were all done at high exposure times is ridiculous, especially when you mention only one sampling.

You have one sampling for a year. What kind of representation is that as exposure? It's not. I just think that based on the documents in the petition, there is too much question about the timing when these tests were done, what they reflected, what they chose to ignore, how they chose to schedule the test, and based on my own knowledge of things that were missed in looking at this data, I mean, it just -- I still question how valid uranium urinalysis data is in determining the level of exposure.

MR. ROLFES: Do you have any questions that I might be able to help explain or anything?

MS. BALDRIDGE: No. You know, going through this process and finding out, you know, we'll go -- exposures, you know, the 19 people that were exposed in Pilot Plant back in 1951. Well, and then you see how the data is looked at by the examiners, the dose reconstructors, and then you have someone who comes along with the same conditions that resulted from these exposures whose records do not reflect any urinalysis done, do not even reflect that they were part of the examination because the exposure wasn't recognized in their part of the plant and was just restricted to 19 men, and then it's not even picked up because that person's records show that they worked in a different plant than where they could have been exposed.

But they couldn't have worked anyplace else because that's the only plant that was operating at the time they were working.

MR. ROLFES: So if you're expressing concern about certain workers working in a plant that weren't monitored for uranium exposures, in those cases what we have done is looked at the urine concentrations from the people that were monitored, and we're using the monitored workers' uranium urinalysis values. We're using the median value. We've calculated, you know, We plugged in all roughly 400,000 samples and come up with a distribution.

So if an individual was not monitored for uranium, we would take the 50th percentile of that distribution and assign an unmonitored uranium intake to that worker, and --

MS. BALDRIDGE: But if you didn't pick up the fact that they were a worker in that location at that time, that assignment wouldn't be made.

MR. ROLFES: No, it would. If an individual was believed to have been exposed to radiation and was never monitored for uranium via urinalysis, we would assign uranium intake, bottom line.

MEMBER ZIEMER: Just one other comment on the sampling issue. I think there's almost no case where you do 100 percent sampling on anything.

MR. ROLFES: No.

MEMBER ZIEMER: I mean, obviously you never know. For example, if you want to know what percent of the people in the U.S. support some position of the President, there's no way to sample that 100 percent. That's unreasonable.

You know, if you sample ten people, what does that tell you? So statisticians have ways of doing that where they can get an unbiased picture.

I guess on these missing ones, isn't the question sort of you have looked at some of the missing ones --

MR. ROLFES: Correct.

MEMBER ZIEMER: -- and it seems that the question being raised is how representative are those of the rest of the missing ones.

Now, actually for a statistician, that's not that difficult of a problem. Unfortunately, I'm not a statistician.

(Laughter.)

MEMBER ZIEMER: So I don't have the solution to that, but it's a variation of pulling the white and the red balls out of a hat or out of a bag to determine what the relative numbers of each are, and obviously unless you do it 100 percent you don't get the exact answer, but you can come really close if you model it right.

So I guess I'm trying to understand. I think as I understand it, the transcription per se from hard data to the database is not an issue. It's the missing stuff, and the missing stuff that you've looked at appears to be unbiased.

And it appears to me the issue that's been raised is how representative is that, and is there a way of answering that question?

For some reason did you select only the -- you know, there's a bunch of really high ones here, but for some reason, all you got was this little distribution, which I guess someone could argue that could happen. In fact, if stuff like that didn't happen, no one would ever go to the casinos.

(Laughter.)

MEMBER ZIEMER: Because in the casinos you know that --

DR. MAURO: You lose.

MEMBER ZIEMER: -- on average, but there's always a few people that are in the cluster, and so do we have a weird cluster here?

And the only way you -- you've got to play enough. If I play enough at the casino, I'm going to lose, and I don't have to play forever. It's just long enough to --

DR. MAURO: Yes, and I would argue it's random, and you've got a guy that worked at the plant ten years in different locations. The chances that he just happened to be the guy that we missed his data in the transcription every time and he happened to just be at that place at that time --

MEMBER ZIEMER: Every time.

DR. MAURO: -- that's not going to happen.

MEMBER ZIEMER: Right.

DR. MAURO: And so I agree with you.

MR. ROLFES: In that case, we would apply coworker intake.

MEMBER ZIEMER: So I'm looking for an argument of what do you do with missing data then.

DR. MAURO: Well, you did something. What you did I think may be the solution, but I'm not sure. I put something. I think --

MEMBER ZIEMER: You're going the same direction.

DR. MAURO: Yes, let me tell you what I'm thinking. I'm saying, okay, I've got a guy. All right? He worked at the Pilot Plant in 1950. Okay? And in that plant -- this is all made up. You know, we can get the real numbers -- but let's say you have 1,000 urine samples that were collected that year in the Pilot Plant, and along comes this guy. All right, and you're going to reconstruct his dose, and you go into his records.

Now, here's my first question. You go into his records and you have the HIS-20 database and you go pull it. You go pull his numbers out, but if you see he's missing a couple of months, do you go to his hard copy data to get the rest of it, or do you just work with what his -- in other words, you don't go back to the hard copy.

MR. ROLFES: No, we do actually.

DR. MAURO: You do?

MR. ROLFES: Yes. For each dose reconstruction that we work on for Fernald and other sites, we didn't base our dose reconstruction method solely on the HIS-20 data.

For example, when we complete a dose reconstruction, we actually receive an individual DOE response file for each individual claimant, and much of that data does come from HIS-20, but there are also some hard copy records that come to us.

DR. MAURO: That solves half our problem. No problem, and who cares? You've got the transcription. For a guy that has a complete record --

MR. ROLFES: You have the record.

DR. MAURO: -- you've got the record. That's the end of the story.

Okay. Now, same guy, same guy, except in this case, you go back to his records and he has no records. So in other words, he has missing data, and everybody has got a hole. You've got him this month or this quarter and this quarter. We're missing some numbers, and we go back into his hard copy data. They're not there, okay, for some reason.

Okay. Now you've got to hang your hat entirely on your coworker model, and what do you do in your coworker models is, well, I know that for this period the mean -- I'm making stuff up -- the mean is five Becquerels per liter and the magnitude is 12. Okay? You've got that.

Now, what are we saying? We're saying that in the transcription process, some data was missing. It might have been this guy's data or, well, no, it can't be because you went back and you checked it.

I'm thinking it out. I'm thinking it through. So what you're left with is a situation where if you did have the data, no problem. It's when you don't have the data and you're depending on this distribution for this guy.

Now, the fact that six percent of these 1,000 samples, what we're really saying now is best we can tell there's -- this shouldn't be 1,000. This should be 1,060.

MR. ROLFES: Right.

DR. MAURO: That's what it should be.

MR. ROLFES: Right.

DR. MAURO: But it's not. It's 1,000. Now, in this distribution, whatever, you know, they're changed, because we're missing 60 numbers.

MR. ROLFES: Right.

DR. MAURO: And I'm going to argue that if those 60 numbers are random --

MR. ROLFES: Unbiased.

DR. MAURO: -- unbiased, nothing changes and everything is fine.

MR. ROLFES: Right, right.

DR. MAURO: Now, what I'm hearing is is there a way to convince yourself that those 60 numbers really can't -- unless someone deliberately left them out, it would be a concerted effort to deliberately leave out those 60 numbers that happened to be the worst ones.

Now, is there anything that you do to convince yourself that's not what happened?

MR. CHMELYNSKI: John.

DR. MAURO: Yes.

MR. CHMELYNSKI: Yes. We did a simulation.

DR. MAURO: Good. Go ahead.

MR. CHMELYNSKI: And what we looked at was, well, let's say six percent of the data was missing, but it does come from the same distribution that the other data comes from.

DR. MAURO: Okay.

MR. CHMELYNSKI: That rules out the systematic bias issue. Okay? I'm just saying that without the systematic bias of the missing data, let's assume that they just come from the same distribution. We just haven't seen them.

And then we ask the question: well, how much would the 95th percentile estimate be affected? We did a simulation that said that -- let me make sure I get the right answer here -- if the sample size is 100 or more, then the 95th percentile you see will be between the 90th and the 98th true percentile 95 percent of the time.

(Laughter.)

MEMBER ZIEMER: If you follow that --

DR. MAURO: It means you're okay.

MR. CHMELYNSKI: Well, it says you can be plus or minus three percentiles --

DR. MAURO: You're okay.

MR. CHMELYNSKI: -- if you have 100 or more.

Now, if you're down to where you only have 25, then it could be somewhere between the 82nd and the 99th, which means there's a lot more uncertainty. So really it boils down to what is the sample size that you build your coworker model on.

DR. MAKHIJANI: And, Harry, you know, while the difference would be 95 and 98, it doesn't look large when you say it as percentiles. The difference in the actual values could be very large, right?

MR. CHMELYNSKI: It could be, and with the log-normal and with a small sample size, you can get some pretty high numbers in there that you haven't seen.

DR. MAKHIJANI: Because you're in the tail of the distribution. So when you go from 95th percentile to 98th percentile, you're going to wind up with significant errors in your dose assignment. It's not three percent.

DR. MAURO: But remember what we're talking about is we're filling in blanks, a month here, a month there, a year here, a year there, for a guy that may have worked there for ten years. This is not going to happen every time. You see, that's my problem.

If it was a one time deal, in other words, but for you to miss it, in other words, you -- do you see where I'm going? I don't --

DR. MAKHIJANI: No, but I don't agree with your characterization of the situation. You know, I'm just coming at it was a neutral party, just kind of looking at it from a statistical point of view. I haven't been involved too much in this discussion.

First of all, Harry's numbers assume that the missing data are random. So part of the same distribution, right, Harry?

MR. CHMELYNSKI: Right.

DR. MAKHIJANI: And then you come up with this result. But you marry that now with what Harry said earlier, is that sometimes you've got a person whose data are -- it's worse if someone's data are missing and we don't know why they're missing, but there are many data points for somebody that is not there.

Now, if you add, you know, even a modest bias to this missing problem, then, you know, this question of sample size, the question of difference in percentiles could become important.

And I think that it may be useful to determine if the missing data are random or not random.

DR. MAURO: I agree with that 100 percent. How you do that I don't know.

MEMBER ZIEMER: Let me ask one other question on this in terms of simulating. Suppose you had 1,000 samples. It should really be 1,060, and make the assumption that all of those 60 had values equivalent to, say, the 95th percentile of the original distribution. They're all high.

What does it do to a -- what does

a modified distribution look like? Is that the kind of simulation that was done?

DR. MAURO: No.

MR. CHMELYNSKI: No. The simulation I did did not look at a biased sample, which that would be.

MEMBER ZIEMER: No, I'm saying what's the worst case that could happen if everything was -- I don't know if that's what you want to do. I'm just trying to think about --

DR. MAURO: It would go like something -- you know, it would lose this piece, you know. That's what would happen, and if you're interested in this number here, right? I mean, you go --

MEMBER ZIEMER: Well, I'm not sure. You have a different number.

DR. MAURO: Yes, the magnitude would change. If only the high -- if six percent of the numbers were gone and you had only the highest numbers --

MEMBER ZIEMER: You have a different number.

DR. MAURO: -- you've got a different 95th percentile.

MEMBER ZIEMER: Yes, but you also have a larger number of samples. So that sort of dilutes it anyway.

DR. MAURO: You have a really large number, and it depends on the number of samples.

MEMBER ZIEMER: I don't think six percent is going to change it that much, but that's intuitive. That's intuitive. Someone would have to try that. I don't know. We're all kind of speculating here about what the effect would be, and I'm wondering if somebody needs to give some thought to is there a way to convince ourselves that without sampling 100 percent of the universe, what's the nature of the missing stuff.

DR. MAURO: I've got an idea. I mean, it's one of my comments. In other words, let's say you know we didn't transcribe these out of the I don't know how many thousands there were. Okay? Let's say there are thousands of numbers that were not transcribed. Okay?

Now you go in and you say, do you know what? I'm going to go grab a couple hundred of those, the ones that weren't transcribed, and see what their distribution is.

And for the ones that were not transcribed, if their distribution looks exactly like the distribution for the ones that were transcribed, aren't we done?

Do you see what I'm getting at?

MEMBER ZIEMER: Well, but somebody is going to make the argument, yes, but you only did 100, 200 or a few hundred out of 20,000, and statistically, somebody has got to ask how do you do that sampling that you just described.

DR. MAURO: That's what Harry does for us. He tells us those things.

Harry, what I just said, is that a way of getting at this thing? In other words, go in there and pull the ones that weren't transcribed, some number -- I don't know how many -- where at the end you could say I'm 95 percent confident this is an unbiased -- the things that were left out does not represent a bias leave-out.

MR. CHMELYNSKI: Well --

DR. MAURO: They look exactly like the ones that were transcribed.

MR. CHMELYNSKI: I think, again, it gets back to this issue that NIOSH has already looked at the ones we know are not transcribed, which is a fairly small amount of records really, and they don't seem to make a big difference if you put them in there or not.

MR. ROLFES: Yes.

MR. CHMELYNSKI: But what we're dealing with are the ones that we haven't found yet that haven't been transcribed, and to find those is not easy. And so I'm not suggesting we do that.

DR. MAURO: Well, why would you want to do that? I mean --

MR. CHMELYNSKI: Well, to do what you want to do.

DR. MAURO: You know, I guess I thought that they -- my sense was that they entered into a process where they made it say, we're going to do this. I'm going to go through this process, step, step, step, step. And at the end we will have checked.

But you cut the process short. At a certain point in the process you decided I'm not going to do the full sampling that I originally designed. I'm going to pull back and go to 20 samples as opposed to whatever the samples were.

And it seems that you pulled back when you shouldn't have. Now, if you go back and finish it, okay, the way you originally planned to do it, now you know what percent is missing. You followed your own rules. I know now that, yes, there is -- right now the indication is six percent. When you finish the process, you'll say going through the process, yes, the number is this percent for that batch, this percent for this batch, this percent for this batch. I think you broke them up in batches.

MR. CHMELYNSKI: John, one of the things I might throw in here is that in the batches we looked at that had reduced inspection, we actually did better than anywhere else. They've got a 99 percent score on those so far. It may turn out it's less than six percent are missing when you're done.

DR. MAURO: Well, I mean, that's an important finding. I mean, that would be --

MR. MORRIS: That's why we reduced the number of samples. That's why the -- we didn't go in with the intention of taking only 20 samples. We went in and intentionally following the process.

Now, I understand in retrospect we say, oh, maybe we should never have set up a reduced sampling rule once we started getting good matching of our expectations, and we'll take that criticism.

But the reality is those data sets that did match well are the ones we didn't sample heavily because they did match well.

MR. CHMELYNSKI: But as far as you went they did match. I have to admit that, yes.

DR. MAURO: So there may be good rationale to cut short, I mean.

MS. BALDRIDGE: Can I comment again? It has been about three years ago I asked if they ever compared the data with the documents when there was no high exposure at specific locations. Was that comparison ever done?

MR. ROLFES: As far as --

MS. BALDRIDGE: You've got people working in certain plants. There are documents in the petition that state the MAC at this time was this level, was that level, excessive high, this far above. Were any of those people's urinalysis data compared to known exposure levels based on the plant's documents to see if those recorded levels were done at those times or even reflected the level of exposure that was on record?

MR. ROLFES: The short answer is, I guess, yes and no. And what Fernald did early on in the very beginning of operations, they had people from the Health and Safety

Laboratory come down and do assessments of air concentrations and the work environment, and that actually applies. Those reports, the daily weighted exposure reports, that were assembled from the years of 1952, I believe, through about 1965, and those reports actually did analyze the air concentrations at various operations both at breathing zone basically, a steelworker's breathing zone area next to his mouth or nose. The air concentrations were analyzed there and also in the general area of the work being performed.

They also considered the amount of time that the worker was exposed at these various concentrations.

MS. BALDRIDGE: Does the uranium urinalysis data reflect that exposure for those people known to be in those locations at that time? I mean, were they even tested then or was there sampling from another time when there was a lower level or one that was not recorded?

MR. ROLFES: They were --

MR. MORRIS: Mark, can I interject?

MR. ROLFES: Yes, please.

MR. MORRIS: There were published papers at conferences where that topic was presented, and if I recall correctly, there was only a modest correlation between air sampling, air concentration in the facility and worker exposures by urinalysis, but the contemporary scientists studied the question and presented reports, at least one major report and maybe two that I'm remembering.

I'll try to find the references for you.

MS. BALDRIDGE: If there isn't a direct correlation, then it makes any of the data that you do have questionable as far as whether it really reflects exposures that people actually had or whether they only reflect exposures that people have at the time that they were tested.

MR. ROLFES: Well, the thing about uranium urinalyses, if an individual is monitored, any previous exposures to uranium would actually be integrated in the results of the urinalysis that was collected. So when you have a urine sample, for example, that was collected, say, a year after routinely working for previous years, if you -- I'll draw a little diagram here.

MS. BALDRIDGE: That was a cumulative.

MR. ROLFES: Yes. Physically you had exposures that occurred back here, you know, from time zero to time 1,000, we'll say, say, 1,000 days of chronic exposure. To take a sample, you know, following those 1,000 days of exposure out here sometime, if you're exposed at this level, you could have a urinalysis sample that can actually be used to reconstruct the historical exposure, you know, several days, several years, several weeks, several years back.

So typically if there was a high result here, that would have prompted another urinalysis to be collected. So when you have an additional sample, when you have an additional sample, it allows you to basically develop a more accurate excretion rate and get a better picture of what happened here.

DR. GLOVER: I think from the beginning they recognized that it's -- that's why we always use the most favorable solubility classes. They did air sampling. It has all of the size. Some of these things aren't inhalable, process materials. So directly relating non-size restricted uranium samples to bioassay results is fraught with peril, a much larger, much larger number than what you would have got if you actually -- what did the guy get in his body?

DR. MAURO: Well, you're going to have that on air samples, you know.

MR. ROLFES: Those are reflective of uptake into the body rather than air concentrations in the plant because the other things, in comparing something like that, you would have to look at the amount of time that the individual was exposed at that air concentration; also whether he was wearing respiratory equipment. You'd have to take a look at particle size distributions because some of the samples had non-respirable particles. So those would not be inhaled or uptake. They wouldn't have gotten into the lungs and into the blood stream essentially.

So really that's why we rely on the bioassay samples, the uranium urinalyses, because they're most reflective of the actual worker exposures.

DR. GLOVER: John, I wanted to mention one thing. So from the beginning of the report, the team did evaluate the missing data that they saw and whether it was within the scope. So they say if we had 70 missing values, they said they had this reduced rate, and the original, I always go back to what was the purpose, and we say it was verification of the completeness and accuracy of the data.

We didn't necessarily say it was for HIS-20 applications or what our specific thing was. Is it the Board's feeling that we may need to, for the purposes of HIS-20, discuss what we did, and if we need to extend that, is that what you're really trying to say?

Or do we need to put it in the context of the HIS-20 and then also our dose reconstruction?

We may not have made it clear what our test protocol was. Is there some clarification that you're asking for?

We had an original purpose.

DR. MAURO: Let me give you my comments and way of looking at it. You go through a process to convince yourself you've got a transcribed database that you can depend on. You come out of the back end of that process saying right now it looks like overall you might have missed six percent of the numbers.

Now, I think you said it's based on a process where in the middle of the process you sort of backed off a little bit on the sampling, perhaps for good reason. The way I look at it is it would be nice if you didn't do that, you stuck to your process and you finished your sampling because instead of six, you may come out with three or you may come out with nine.

In other words, we don't know if you did it according to your rules where you come out. What I just heard, if you actually went through and inspect the time and money, what I just heard is you may actually come out at a lower number as opposed to a higher number because your judgment to hold back was a good one, but it's not self-evident.

So I said to myself, all right, if I want to convince myself, I go through this process. I finish up, and I find out what the real number is. Let's say the real number is six percent. Okay. Six percent.

Then I go in and say is that a problem. I say, all right, let me go see if it's a problem. Let me go pick one of these time periods and where it says I've got 1,000, I go fix the 1,000 and I go find the ones I had missed. All right? In the HIS-20 database, and now I've got the 1,060. Now I've got my complete set, you know, if I didn't forget. Originally I didn't forget to do that, to transcribe.

Now I say, okay, so I've got 60 estimates of Becquerels per liter that are not in here, and I say I'm going to put them in here. In other words, in other words, here we have, I guess -- in other words, where do they fall? Those concentrations, those 60 are sort of one over here, one over here, one over here, one over here. And I make a plot of the 60.

I say, well, we put the 60 in here, and it looks like this. I don't know. It looks just like this, only the numbers are lower, you know. It's exactly the same thing. It just looks the same.

It means that the 60 that I'm missing are just like the rest of them, but if all of a sudden I say, holy mackerel, all 60 are over here, you know? Okay. I mean, it shows you that there was some kind of built-in bias, that the ones that were being transcribed were deliberately not transcribed because they were hot. If I see that, I get really upset because then it's not random anymore. Something strange is going on here.

Why in the heck would the 60 that happened to be missing all be in the upper in of the tail? I don't think you would find that.

MR. ROLFES: We didn't.

(Laughter.)

MR. ROLFES: This is exactly what --

DR. MAURO: Well, they were done. This problem is solved.

This is me talking, all right? Not SC&A but me. As far as I'm concerned, I'm a biologist. I look at something like that and it tells me we have nothing more to talk about. You did that.

DR. MAKHIJANI: Actually we should rely on it.

DR. MAURO: I look at this, and you tell me if that's what you get. It's done.

MR. ROLFES: That's exactly what we have, John.

CHAIRMAN CLAWSON: Where is that information?

MR. ROLFES: In our analysis of the HIS-20 database. We actually had considered the missing results and looked at the impact in the distribution essentially.

Gene, Gene?

MR. POTTER: I'm sorry, Mike. I was taking myself off mute there.

MR. ROLFES: No problem. I guess if you could maybe elaborate a little bit on what I've been referring to or what we've been discussing.

MR. POTTER: Yes.

MR. ROLFES: Could you pull up the specific portion of our analysis of the HIS-20 data that were missing?

We looked back to see what bias might be, you know, to look to see if any of the missing data from HIS-20 had any bias to it, and what we have found, I pulled up -- I've got a statement, but I know we did some additional work on this.

We had looked at this, and we found that the missing data didn't have any significant changes to the coworker study for Fernald if we would include the missing data from HIS-20. So the bottom line was that the missing data that were not in HIS-20 didn't impact the coworker intake.

MR. POTTER: Yes, that was the batches that did not meet -- let me just briefly summarize -- the batches that did not meet our preselected AQL, we looked at what the effect of the missing data versus the original data that was in there.

Now, SC&A criticized this because they said something to the effect that we didn't consider the total effect of all of the missing data. We just did it batch by batch essentially and showing there was no difference that way. Certainly that is an opinion, but from the limited work we did, it did not seem to have any effect or any major effect, I should say, whether the missing data was included or not included in the rather small batch-wise thing that we did.

MR. ROLFES: Thank you, Gene.

DR. MAURO: The way in which you convince yourself everything is okay, you put the 60 back there, okay, and then you make the plot. The 60, you really can't see anything. In other words, we're putting a small amount back in. All right? And it almost disappears. It's diluted.

MR. KATZ: John, do you want to just speak up.

DR. MAURO: The 60, if you do what I understand you did, is let me put this and see if my distribution changes. I say to myself, okay, that's a good thing to do, and you find out my 95th percentile doesn't move even though I put the 60 in.

But you know, I guess there's one more thing I would do. I would say because the 60 is going to be hidden, it's sort of like diluted in this 1,000, but if I went in and I pulled the 60 and I said let me see where the 60 are, you know, because if it turns out they tend to be over here, you still may not see them because they are diluted in the 1,000. You know, like we were talking about before, it's hard to see. I don't know how this blind is going to change, and that may not be important, but I --

MEMBER ZIEMER: Well, if you still can't see them, John, even if they're clumped --

DR. MAURO: If they're all -- if every one of those 60 fall above the 95th percentile, wouldn't that make you very nervous?

MEMBER ZIEMER: Not if you still can't see them in the total distribution it wouldn't. In fact, that's the question I was asking.

MR. ROLFES: Right. The same number, for example, 50, 60 results.

MEMBER ZIEMER: You can simulate the answer to that, but I guess the only final thing I would kind of ask is there is no reason to think that the sampling to that point -- obviously you've done a small fraction of the total. You can make the argument that it still is a random sample of the distribution, and then the only question that comes to a statistician -- and this would be Harry's -- is there any reason to think that that sampling so far would itself be biased in some very unusual way?

I mean, if your sampling is such that you were only getting certain kinds of results from the rest of the distribution, then you might make that argument, but it's hard for me to see that the missing ones, unless what you describe, John, whether there was some intentionality on the missing ones where someone said we're going to intentionally not record high ones, but if that were the case, then it should be showing up even here.

MR. ROLFES: Right, right.

MEMBER ZIEMER: If there was a biased intention on ones you leave out, then that should show up in the sampling.

MR. CHMELYNSKI: Yes, and I think the NIOSH report, original study, did address this issue, and I think maybe I should read some of their conclusions here.

On one of their files that had missing data they conclude that there were 36 missing results. Eleven were above the 50th percentile; four were equal to the 50th percentile; and 21 were below the 50th percentile.

Now, that's one file out of five that had missing data.

Another one, the statement is made that these files had eight missing results in the two files combined. I'm sorry. Eight missing results were spread around the 50th percentile, although one was above the 84th percentile.

So I don't know the actual numbers that we're missing, but there are these types of statements made in the report about the missing data, which imply that they don't seem to have a bias.

DR. GLOVER: Mark, I have one question. In the extensive sampling reports, at Hanford we actually would state these are statistics. Here is the number of high samples. Here's the number of low samples. This was the worst case sample during different periods.

I'm pretty sure that they would have done something similar at Fernald. We're not actually sampling an unknown population. We probably have some statistics that are probably subscribed in some monthly reports. Is that the case?

MR. ROLFES: The nighest exposures at Fernald were routinely followed, and people with the highest exposures, if a urine sample was collected at above 40 micrograms per liter, a subsequent sample was collected and they wanted to track that employee's internal exposure or uranium burden to ensure that it came back down to a safer level.

So those types of things were routinely followed. Now, in addition to the urine sampling program, those individuals with highest internal exposures at Fernald were counted in the whole body counter. So it opens up another data source that could be used once again to fill in any perceived missing data gaps.

CHAIRMAN CLAWSON: Now, we're using uranium bioassay to completely reconstruct everybody's dose. We're not using air sampling data; is that correct? So we're only using uranium, and everybody at Fernald got a urine sample?

MEMBER ZIEMER: I don't think everybody, no.

MR. ROLFES: Not everybody. Not everyone was sampled. Approximately 93 percent of the population was sampled, and for the people that weren't sampled, we have the coworker intake model.

CHAIRMAN CLAWSON: Okay. So for the people that didn't have urine, because --

MEMBER ZIEMER: You're going to assign it.

CHAIRMAN CLAWSON: You're going to assign it to them because that was one of the things that came into this, was people that weren't supposed to be getting uranium and all of a sudden they did a spot sample and they did have it. And I was just wondering because really we don't know what they were into or anything else.

The one that comes up is the people who were issuing the clothing and so forth like that. They weren't monitored for numerous years, and then they came up showing positive, and now we're going to go back. We're going to go back and do what for them because we have no idea. They could have been fairly high.

MR. ROLFES: True, true, that is possible.

MEMBER ZIEMER: You can still do a maximum.

MR. ROLFES: Right.

MEMBER ZIEMER: If you weren't to do the whole worker, you can assign. You can say what's the biggest dose they had to have way back there at Fernald.

MR. ROLFES: Exactly. In fact, in that sort of scenario, you know, if there were a couple of years of unmonitored exposure perhaps, subsequent urine samples ---

DR. MAURO: And that's exactly the reason we had a problem with Issue No. 1. They were automatically going to assign the median to everybody without thinking about, well, wait a minute. Is that being claimant-favorable for every one?

MEMBER ZIEMER: For everyone.

DR. MAURO: Right. Now I'm hearing that, no, you're going to think about this and say, well, for this guy it seems to me maybe we'd better assign the 84th percentile.

So we really bounced back. So, yes, we know that between '52 and '57, only about four or five percent, a small percent, not four; I forget the -- less than 50 percent were monitored. After '57, not over 90 percent were monitored.

So there are a bunch of people in the early years and in the later years they weren't monitored, and we also know that some of those people worked at time periods and in buildings and job categories that we know from looking at the data put them up at the high end.

So if you happened to do -- now, it's rare. It's rare, but we found them, and on that basis -- and that's the only reason we brought up Issue No. 1 -- on that basis all we're saying is be careful when you use your coworker model. So I think that problem is solved, and they're ready to do that.

Where we are now is that can we depend on those distributions that you have for every quarter for every building by year, an incredible amount of data. And I've got to tell you what I just heard, if you guys put to the test every segment the way you just said you did, we took the 60 and we took a look where they were. I just heard from -- and I should have known this -- but I just heard from Harry when he looked at the ones that were missing, you know, half of them were below 50 and half of them had one over here, one over there.

You know, it's almost like I don't need a sophisticated statistical analysis. It just screams at you obviously that they weren't all from the high end. You know, if Harry came back and said that the 30 that were missing were all in the upper 95th percentile, I would say shut the shop down. There's something went on that people should be in jail, you know. That's what I would say. You're going to lock somebody up, you know. Really. You know, you can't just be missing -- you have the 60 that are missing. They're all the high guys? I'm sorry. I just get excited.

But when I hear what Harry said, everything is okay.

CHAIRMAN CLAWSON: Okay. My concern is we're using this bioassay data. That is the whole thing that we're using to be able to do the coworker data with, right?

MEMBER ZIEMER: As opposed to?

CHAIRMAN CLAWSON: Well, see, early on if you remember right, they had all of this air sampling data and stuff like that, and that kind of came to be flawed a little bit and so forth like that. So you know, the more information that we have out there, the more checks and balances we have.

We have one check which is a HIS database. Now, the one question that I have is that NIOSH says that they have checked these other ones that weren't found, and I think 30 percent were below 50 or whatever else, like that.

MR. ROLFES: Right. It's basically what John has drawn up here. Red is that they followed the same distribution.

CHAIRMAN CLAWSON: Right.

MR. ROLFES: Some were higher, some were lower, and some were right on the mark. So they didn't significantly impact the distribution that we did use.

CHAIRMAN CLAWSON: Well, this is just a simple question. If you guys checked all that, then why didn't it get put into the report? Why isn't it in the HIS database if you guys checked all of this?

MR. ROLFES: I don't know. I really don't know.

CHAIRMAN CLAWSON: Because it seems to me it would have stopped a lot of this back-and-forth if it was put in the database.

MR. ROLFES: Sure, sure. There could be reasons behind it. It could have been, for example, an employee that wasn't employed by Fernald or something. It could have been a contaminated sample. It could have been a lost sample. It could have been, you know, if you take a 24-hour urine sample, sometimes they'll collect, you know, multiple samples on a day, and then it could be that they just piled them all into one 24-hour sample.

Those are some of the reasons. I'm just speculating about what they might be, but it could have been a repeat error. It could have been, you know, somebody accidentally typed it in twice.

MR. KATZ: I think Brad was asking -- you're thinking about that they've actually done this checking. Why not put it in the HIS?

And I think the answer to that question, Brad, is they've only done a sampling. So if you were to actually go through all of the records and fill in the gaps in HIS, that would be a phenomenally large work load for OCAS to do.

CHAIRMAN CLAWSON: Yes, because what I was hearing was, yes, we checked all of these and they're all right because they sit right between here.

But what you're saying is that they have just sampled portions of these, and they've come to find that we're right in the 50-50 error that you were showing on these things.

MR. ROLFES: Yes, right.

CHAIRMAN CLAWSON: Okay. Well, that's --

MR. ROLFES: In the amount that we sampled we didn't find any bias that they were either like the high results weren't entered into HIS database and also no indication that the low results weren't entered into HIS-20. So we found that the stuff that didn't make it into HIS-20 was right in line with the information that is HIS-20.

CHAIRMAN CLAWSON: So what we were trying to prove, that there was no bias in what they did. Okay.

MR. ROLFES: And that's what we found.

CHAIRMAN CLAWSON: Well, now I've got the rousing question: what are we doing with Issue No. 2?

Because I started out with an awful lot of notes, and I finally gave up because it seemed like you kind of made a lot of changes on this. Because I'm going to be quite honest here, the thing that bothers me is at many sites I know that we end up doing this, but we have one set here. We're using this data, and we need to make sure this is solid. You know, as John puts it, this is the rock that we're standing on and it is being put out there because, as we've found at other sites, kind of built these models, and as we've just seen a little while ago, they fell down, and I want to make sure that this database is correct, that it's going to cover the people correctly.

I'm still questioning what are we doing with Issue No. 2, John.

DR. MAURO: I'll give you my recommendation. It would be to finish the test the way it was originally intended, find out what the real percentage is, whether it's three, six or nine, because right now we'd get a six but it may turn out to be better than that. That's good news, right? That means the database is -- when you finish it.

And when you're done with that, you tell that story in a way that everybody can understand. The ones that were missing, we went back and tested it, and we did it and just sort it. I would say not do it in the aggregate, but break it down the way you broke it down, mainly perhaps by building, maybe particular years, enough of a grab from each segment, year, building, that you could show that the ones that were missing from the sample fall -- some fall over here, some fall over there, and show the story that, no, they don't all fall in the high end.

And then if you could supplement your report with that, this story is over and you've got the rock. And that's what I would recommend.

MEMBER ZIEMER: What does that entail? Can you spell out?

DR. MAURO: That's not going to work?

MEMBER ZIEMER: Well, spell out for us. I mean, a lot of times you enter into a sampling program to avoid doing what you just described. I mean at the front end you don't know what you're going to -- I'm going to start drawing balls out of the bag. I don't know what's going to be there. But at some point when I give enough of a picture I can say it's time to stop.

And I'm not -- I don't know, Fred, that point or not. You know, here's the original project. Are we here? Are we here? So I'd like to hear a little bit about what's entailed and at what point are we confident that it's ready to stop?

DR. MAURO: Let me modify. I have another alternative strategy. See, basically I said why don't you finish, do it the way you did now. If you can make a case that you cut back at this point in the process and why, because you're starting to see that --

MEMBER ZIEMER: It was just the same thing over and over.

DR. MAURO: And you could set your rationale. Then you don't have to. I mean, I'm just trying to find a way you can -- we've got to get on the record the logic behind the process so that when you come to the end of the process and come to your conclusions, it's clean.

So I guess I would say that really there are two alternative strategies in my mind: one, complete the thing or, two, if you don't think it's necessary to complete the thing, make a real hard case why it's okay that you pulled back when you pulled back.

MR. ROLFES: And I thought that our paper had already done this by, you know, because of our reduced sampling we reduced our sampling because of the good integrity of the data. The data was all valid there. The numbers were good. We found nearly 100 percent in our sample.

DR. MAURO: Well, the reason we thought it wasn't is you were shooting for one percent and you saw six percent, and that was a little disturbing to you.

MR. ROLFES: Right.

DR. MAURO: And to pull back, that's incongruous.

MR. ROLFES: But the six percent wasn't necessarily a number that would go into this intake model. Those are separate errors.

MEMBER ZIEMER: It wasn't a transcription error.

MR. ROLFES: It could have been spelling. It could have been a spelling error. It could have been a Social Security number error. It wasn't the urine sample result value that was erred. Those were all good. Those were 100 percent accurate and transcribed.

DR. MAURO: Harry.

DR. GLOVER: Hey, John. May I say one thing? One of the things I did want to, the bioassay, there's a series of multi-reports, and it says for October 1968 there were 288 urinalysis samples taken for uranium. It's not an unknown number, and so we have some ability to bound the annual year, how many should be there so that we can estimate statistics of, you know, what's missing percentage-wise. So these aren't just -- I mean, we went in unknown. We actually tested it, Mark, blindly. Right? We took the hard copy sheets, but there are statistics that are generated that we could test this against.

DR. MAKHIJANI: But that hasn't been done as yet.

DR. GLOVER: I'm not privileged.

MR. ROLFES: There's no reason to do it when the numbers aren't bad. One hundred percent of the numbers are reported and transcribed faithfully, there's no reason to go back and look at something that isn't important to a dose reconstruction.

If an individual's name is misspelled, that's not an issue for generating a number. I mean --

DR. MAKHIJANI: I didn't think we

were arguing about the transcription because I think we're all agreed that the transcription is fine. What we're arguing about, the numbers that are not there.

MR. ROLFES: Correct.

DR. MAKHIJANI: Now, I don't remember because it has been a while since I looked at the Fernald data whether the HIS-20 database at Fernald was compiled the same way as at other sites; that whoever was employed in the mid-'70s -- still, you know, they may have got started in the '50s -- was included in the database, but those people who had stopped their employment in the earlier time, you know, were no longer employed by the time the electronic transcription started were not in the HIS data.

MR. ROLFES: Okay. I've got you.

DR. MAKHIJANI: Is that true of Fernald? I don't remember.

MR. ROLFES: Well, it could be. I

don't know. However, if you take a look at the coworker intakes that are generated from the data that we do have in HIS-20, the actual intakes for the earlier years are a couple of orders of magnitude higher than the later years. So that, you know, I mean, if there are, in fact, less data, the workers' internal dose would actually be increased in the earlier years.

DR. MAKHIJANI: Well, I don't think that's an automatic conclusion. If you had --

MR. ROLFES: Well, however -- okay. Go ahead.

DR. MAKHIJANI: I'm not saying that you're wrong. I'm just saying if there was somebody who was 35 or 40 years old who started in the '50s and left in the end of the '60s, they would have experienced intakes that were universally admitted that they were at the high end. Intake in the '50s was very high compared to -- and to some extent '60s -- compared to the '70s.

So if there's a bias in the people who are not in the HIS-20 database -- and I don't know if this is true because I don't remember how the Fernald HIS-20 database was compiled -- I think that was true of the Savannah River HPAREH database. That's why we had to go back to the other one to check.

I don't know what the effect of that would be and whether you've taken that into account or not, and whether there's some kind of system to the data that are missing and whether they're missing from the higher period of exposure.

MR. ROLFES: And we did look at the data that weren't entered into HIS-20 and found that there was no bias in the results.

DR. MAURO: In the early years.

MR. ROLFES: For all years that we sampled.

DR. MAURO: Well, because the '52 to '57, the percentage of people that had bioassay sample was relatively low.

MR. ROLFES: We took samples from each decade: '50s, '60s, '70s, '80s I think is what we have done.

DR. MAURO: So you picked up the '60s.

MR. ROLFES: Yes.

DR. MAURO: Okay. I'm sorry.

DR. MAKHIJANI: For a total of 60 in all?

MR. ROLFES: For a total of 60.

DR. MAKHIJANI: You examined 60? I'm confused.

MR. ROLFES: I'm not sure what you're asking.

DR. MAKHIJANI: The 60 numbers that you examined that were missing --

MR. ROLFES: That's the number --

(Simultaneous speakers.)

MR. ROLFES: So we did sample each decade, data from each decade, from the '50s, the '60s, '70s, '80s, and found no indication that any of the data that was not in HIS-20 had a bias to it. We found that --

DR. MAKHIJANI: For those years. Okay.

MR. ROLFES: Correct. We have no reason to believe that the other missing data would have any significant impact on the coworker intakes.

DR. MAKHIJANI: Right. Okay.

CHAIRMAN CLAWSON: That brings me back to the break time question.

MR. ROLFES: No break until we get this thing down. What are we doing with No. 2?

MEMBER GRIFFON: This is Mark Griffon.

I just want to say I just got on, like, two minutes ago.

CHAIRMAN CLAWSON: Okay.

MR. KATZ: Mark, thank you.

CHAIRMAN CLAWSON: Part of my subscription -- I mean, I understand there were some transcription errors, but there's 100 percent of the data or so forth like that. What are we looking for on this, John?

Because what happened was, we were sampling to a certain point and all of a sudden changed our sampling plan and continued to sample on. Is that --

MR. POTTER: This is Gene Potter.

Let me interject that I think it's a mischaracterization that we changed the plan. The reduced sampling scheme is a part of the mil spec. SC&A is arguing that you can't treat these batches like they're widgets coming down a production line, which may be a valid point.

But what I did was look at the history of similar files from a similar era, and that caused me to assume that the reduced sampling protocol was appropriate. An arguable point.

MR. KATZ: Can you explain that a little more because I think you didn't say enough for people that sink their teeth into that, what you mean by the history.

MR. POTTER: Okay. Our sampling has been characterized as somehow flawed because we went to a reduced sampling plan. In other words, you pick an AQL of one percent. You look at your batch size and that tells you how big your sample size should be. You just pick these numbers off a table, and there's different levels of inspection, too.

And I'm saying that rather than not following our own plan, I think that's a mischaracterization. How I would characterize it is I looked at these batches of samples from a similar era of a similar data type and said, okay, the quality looks good on these.

I'm going to a reduced sampling for these batches, and unless they start failing, we're going to stay with that. If they start failing, then we'll go back to a normal sampling number, and it basically just changes the number of samples you're pulling from a batch.

MR. KATZ: But just the thing I was asking is when you say the quality looked good, can you explain that?

CHAIRMAN CLAWSON: Yes, because I'm having a hard time understanding this six percent. You guys were shooting for a one percent error, and we were showing --

MR. POTTER: No, I think that is a bit mischaracterized, too. You pick an AQL basically out of thin air. This is something you pick beforehand, and the fact that it doesn't turn out to be the case is not a defect in your plan as it is being characterized here. It merely sets the sample size you're going to draw from your batch.

And the fact that we picked an AQL of one percent and this has been characterized as some kind of a failure, what we saw was 20 out of 25 batches did, in fact, meet that AQL.

Now, some didn't, and we did some other things, such as looking at whether the missing results were biased and so forth that we've already discussed for those batches that failed, and that's all detailed in our draft report.

Obviously SC&A has a difference of opinion as to whether these batches can be treated in this way. However, you know, I don't think it should be characterized that we didn't follow our own plan.

CHAIRMAN CLAWSON: Let me ask you this. You said that they were all being good. So that's telling me that you had a quality level that you were striving to obtain. What was that quality level?

MR. POTTER: One percent errors.

MEMBER ZIEMER: One percent transcription error.

MR. POTTER: Now, that's a preselected thing. You can't do statistics by changing your plan after you've, you know, started.

MR. ROLFES: Right.

CHAIRMAN CLAWSON: Well, I understand, and that's kind of what was throwing me off a little bit here because --

DR. MAURO: Me, too.

CHAIRMAN CLAWSON: -- the one percent and then we were ending up with six percent. That's telling me that it wasn't making it to that.

I guess that's kind of my understanding, is what John is requesting. If you guys saw that everything was fine to --

MR. POTTER: What the one percent did was set our sample size, and we went and we found what we found. Now, SC&A, in their paper, their finding number ten says there may be as many as 28,000 uranium urine results missing from the HIS-20 database. Actually they calculated that number in three different ways so that there's a range of 8,000 to 28,000. So it's two to six percent depending on how you want to calculate it. The 28 percent is calculated very conservatively, which still amounts to 93 and a half percent of the data being in HIS-20.

So I think the question is, John Mauro has put forth his position that this study ought to be corrected. It's an end in itself, and we don't have enough confidence to make a decision here. That's certainly one possibility.

We can do that. It kicks the decision farther down the road. That's the negative effect.

DR. MAURO: But I guess I'm still

a little bit disoriented for the same reason that we just heard from Brad. That is, you pick a number, one percent. Good. So you need a point of departure, but the one percent does have a role to play because it helps you steer the ship, and as you're moving through the process you start to notice that, batch number one that you're -- I think you grabbed them in batches, and you took a look. This one looks like it's coming in at six percent. Oh, all right.

Batch number two, this one is coming in at four percent. Oh, but then somewhere along the line, whatever the number of samples, percent or whatever they are that you are pulling for sampling, you decided we could cut back.

Now, something happened that told you that you could cut back. I thought the reason you could cut back is because we were meeting our one percent, but you say, no, that's not the reason you cut back. You cut back for some other reason.

MR. POTTER: No, that's true. Those samples that were -- those batches that were subject to a reduced sampling protocol were meeting the criteria and they were the same era and same type of record.

MR. CHMELYNSKI: I guess I have to interject here that there were only 23 files. Six of them had reduced inspection, which leaves 17 files. Out of those 17, a very large percentage did make one percent.

So where is the history that we're talking about?

MR. POTTER: I'm going from memory. We would have to go into the spreadsheets to see that type of deal.

MR. CHMELYNSKI: Yes. I don't have the chronological ordering of which files were done when. So I have to admit that I'm lacking some information here, but when I see that out of the 17 files that were not given reduced inspection, that three of them where they gave 100 percent inspection to because they were that bad that they needed 100 percent inspection, and out of the other 14, your success rate averaged 98 percent, which wasn't the one percent goal. It was close, but didn't make it.

So I still don't see any reason where there's a history that establishes a pattern that's good enough to reduce inspection.

MR. POTTER: We would have to look at the detail on the spreadsheets to answer that, and I don't think we want to probably digress to that at this point.

DR. MAURO: Well, I go back to my original recommendation, and what I'm hearing doesn't change my -- I say you have one of two paths. One is to make a case why the reduced inspection is okay, and therefore you don't have to go back and do some more, and the other is, no, you've got to go back. It appears that the rationale for the reduced inspection may be a little soft and go back and finish the inspection the way you started. This is your call and which way you want to go, notwithstanding whichever way you go down, whichever path you go down you come to a place at the end that says, okay, each patch here's the percent that's missing. It may turn out -- wherever it comes out it comes out.

Then you have to say, okay, you do the thing that's on the blackboard that you can't see, which says, well, the ones that are missing, we went back and took a look at them, and you get a sense where did they fall within the distribution. And that may have already been done, but now you will do it.

Now, if it turns out that you can make a case that you don't have to do anymore, that you have good rationale for cutting back, and you are finished, and you do know the percent missing in each group, and you have already put that test that the ones that were missing do, in fact represent a random number within each batch, you're done, finished. Whether you have to document that, whether it's already in there, you know.

But I think the one thing that -- or if you feel that, no, we do have to go back because we're soft in the rationale for cutting back. You finish it up. You come out of the back end of that, wherever you come out. You run this bias assessment of the nature you just described, again, or supplement it and say, "Here is where we come out, and here is why we think it's unbiased."

So, I mean, I think that's the only way to come out of this thing.

CHAIRMAN CLAWSON: Well, and then it comes back to something that -- we're putting all of our eggs in one basket on this program to be able to do dose reconstruction, bottom line, and I want to be able to walk out of here and feel good about it because I understand everybody's standpoint, but I agree with John. We've got two paths forward, and I guess that comes down to NIOSH's decision of what they would like to be able to do to put this to bed.

Because I understand the point on both sides. I just want to make sure that when we walk away from this we walk away that this database is correct in what it needs.

MR. ROLFES: The important thing are the numbers, and we found the numbers to be good. That's the bottom line. The numbers that we used to generate this intake distribution we found are 100 percent accurate, and we found that the data that wasn't transcribed was unbiased. The high results were not selectively removed, and the low results were not selectively removed.

DR. MAURO: And one more thing. You have to be able to show that the pull-back, when you did pull back, had good basis for pulling back. In other words, when you did not do the full sampling, the rationale for why you did that and why that was a reasonable decision. Maybe that's the one piece that at a minimum you've got to give us that.

MR. ROLFES: I think we have documented that, that we had pulled back because of the good agreement in data, and it was greater than 99 percent agreement, I believe.

DR. MAURO: But I heard it was six percent.

MR. POTTER: When we pulled back. Gene --

MEMBER ZIEMER: Where there are pull-backs in different batches and some were and some weren't, in other words, for what, different decades or whatever it was? It sounded like a lot met the criteria, and you could justify pull-back, and some didn't and so they didn't pull back.

DR. MAURO: If that's the case, then you're okay.

MR. ROLFES: Exactly.

MEMBER ZIEMER: From what I heard described, it sound like what we -- and maybe I had misunderstood this -- but it sounded like they actually did what the plan called for.

MR. ROLFES: Yes.

MEMBER ZIEMER: And perhaps all that needs to be done is to document, and maybe it is documented and we've overlooked that, document, number one, and number two, confirm what's been described here about that distribution. Maybe it's already there in words, and I don't have the document before me here.

CHAIRMAN CLAWSON: Sam has got the document.

MEMBER ZIEMER: I think Sandra had some additional comments.

MS. BALDRIDGE: I have a question. When you talk about this being the rock, am I misunderstanding that are you planning to base the whole argument for your ability to go through, construct, on the fact that you have uranium urinalysis records?

MR. KATZ: That's just one issue.

DR. MAKHIJANI: No, this is just for uranium records.

DR. MAURO: No, I agree. This is more than that because the uranium bioassay issue tells you whether you could reconstruct uranium intake. However, the intake of the missed uranium missed all that because they're assuming that two percent of what you've inhaled -- I'm sorry.

The uranium that's in there, given

that it's correct, we're going to use that as a stepping stone to predict what the intake of enriched uranium is. We're going to use that as a stepping stone for what the recycled uranium intake.

So everything, everything from an internal dosage reconstruction sits on this rock. Yes, you're right. That's my issue.

MR. STIVER: Except thorium.

DR. MAURO: Except thorium. Thorium is a whole different story, and quite frankly, the thorium when we came into this meeting is where I thought the action was going to be.

MR. ROLFES: Thorium we need to clarify a little bit because the thorium-230 intakes will be based upon uranium intakes. The thorium-232 intakes will be based upon --

DR. MAURO: Oh, yes. Yes, I'm talking thorium-232.

MR. ROLFES: -- in vivo data.

DR. MAURO: So, yes, this is the rock we're standing on for internal dosimetry, a very large part of the internal dosimetry, and I think it's very important that we all walk away from this table believing, okay, we're sitting -- the rock is solid.

MS. BALDRIDGE: You talk about bias. We still don't know that the data that was reported wasn't biased.

MR. ROLFES: Our sampling of it did show that there was no bias, but we didn't look at all the data that was missing.

MS. BALDRIDGE: But I'm talking about the numbers that were reported. I mean documents show they had no qualms about misrepresenting themselves to meet government regulations, to meet government requirements.

My question and the point in part of filing the petition was everything is being based on data that the reliability is questionable because of the character of the people who were reporting it.

MR. ROLFES: That's a strong allegation.

DR. MAURO: You are bringing up a different point.

MS. BALDRIDGE: -- documented in the petition, their words, not mine.

DR. MAURO: I think you brought up a very fundamental question. Is the hard copy data any good?

You see, what we've really been --

MS. BALDRIDGE: Right.

DR. MAURO: -- talking about, given the hard copy data is complete and reliable and it's not, you know --

MS. BALDRIDGE: That's an assumption.

DR. MAURO: -- corrupted, that it has been transcribed to the HIS-20 database in a reliable way.

MS. BALDRIDGE: Right.

DR. MAURO: I have to be the first to admit, was that ever an issue that we engaged? Do you recall? Did we engage the hard copy data?

DR. MAKHIJANI: Hans was the original one to look at this petition. So he would have to say. My memory is a little vague because it has been a while.

CHAIRMAN CLAWSON: Well, this was one of the question from early on, and this was part of the issue with the HIS database. You know, I've said this numerous times. The data here is only as good as what was entered into it. You know, it's like a computer. If you enter garbage in, you're going to get garbage out. It may look good and it may calculate up and stuff like that, but the bottom line is that the information that went into this bioassay or assay program of -- what it was.

MEMBER ZIEMER: I'm going to

suggest we take a break, but if I could make one comment, and I don't think anyone would deny that it would be possible for someone to fudge or cook data, but to do that over decades for individuals who work there a long time, you would have to have a systematic scheme amongst many workers --

DR. MAURO: Organized crime.

MS. BALDRIDGE: Read some of the documents.

MEMBER ZIEMER: Well, I'm just making the statement that it's actually difficult to do that, I mean, in a way that would escape detection later because you would have to be able to --

CHAIRMAN CLAWSON: Paul, you're absolutely right, and this is one of the things of data integrity and stuff that we were trying to put forth. I agree.

MS. BALDRIDGE: And when inquiries were made to the quality of the data, the record keeper said, "You can't use this. It's not -- you know, it can't be used for this. It can't be used for that."

MR. ROLFES: What you're referring to --

MS. BALDRIDGE: They cast the doubt on their own data. It's not me saying it or, you know, I'm not questioning or doubting NIOSH's responsible evaluation of what they have. I'm just suggesting that based on documents in the petition, it is questionable whether the government was provided with accurate information which they have, therefore, passed on to you, which is now being used in this process.

MR. POTTER: I think what you had referred to was the concern about calculating internal dose from the uranium urinalyses, and the concern that they were not collected for --

MS. BALDRIDGE: There were three

different documents --

MR. ROLFES: You were concerned about --

MS. BALDRIDGE: -- that question the validity and the usability, not just --

MR. ROLFES: Right.

MS. BALDRIDGE: -- not just on that point, but the accuracy and the usability of the records that were kept, and this is from the record keeper.

MR. ROLFES: I think one of the issues that you're referring to would be the concern about using uranium urinalyses to calculate internal doses to various body organs.

MS. BALDRIDGE: That's one part of it.

MR. ROLFES: And at that time period, they had some pretty basic models which showed what happens to uranium after it's inhaled in your body, how it's distributed into different biological compartments, and at the time, those early models, like the ICRP-2 model, was very primitive.

And today we have those historical samples that were collected. We have much more advanced biokinetic models that allow us to very accurately understand exactly where uranium goes, how it's dissolved into lung fluid and into the blood stream and distributed throughout the body. That allows us to come up with a precise internal dose.

The way we interpret those uranium urinalyses that are collected, we assume the most claimant favorability, solubility -- excuse me. I can't talk. My mouth is a little dry here -- we assume the most claimant favorable solubility class for the target organ in dose reconstruction. So if it's a lung cancer, we'd assume the most insoluble material. If --

MS. BALDRIDGE: I see that part of it, but that wasn't the only issue --

MR. ROLFES: Okay. Well --

MS. BALDRIDGE: -- that has arisen.

MR. ROLFES: Okay.

MS. BALDRIDGE: As a result of this.

MR. ROLFES: I'd be happy to discuss the other two issues if you could point me to those.

DR. GLOVER: I think she said that they were fabricated.

MS. BALDRIDGE: In part, yes.

CHAIRMAN CLAWSON: Early on there were people --

MS. BALDRIDGE: They admitted it.

CHAIRMAN CLAWSON: The bioassay program was in question, the people that were performing it and so forth. There was question of the training of it, and then themselves made the comment that you can't -- this data can't be used for something like that.

MS. BALDRIDGE: They didn't explain why.

MR. ROLFES: That's what I was wondering, if you could possibly explain why because --

MS. BALDRIDGE: They didn't explain why in their papers. They just said, you know, referring to the document that you referenced, they said it couldn't be used. They did not explain. They did not provide an explanation for that statement in the document

MR. ROLFES: I understand. I think another one of the things that we had discussed is the concern about collecting those urine samples for chemical toxicity concerns rather than radiological assessment, and just because they were collected for one reason over the other doesn't prohibit their use from dose reconstruction.

MS. BALDRIDGE: But, you know, when there are documents that say, you know, the Department of Waiver or whatever is really pushing on this, they're asking us whether we've respond -- tell them what they want to hear. Just tell them what they want to here.

They hadn't addressed the issues, and it just shows that there was a deceptive climate at work during certain periods of the operation within this petition period of 40 years; that there were people who had no conscience about what they presented and who they presented it to.

Now, it may have been offhand. It most likely was some of the early years, but the issue remains it was in place. It has demonstrated that that was the mindset of some of the people who were handling the affairs for National Lead of Ohio. And that showed how they were responding and reacting to the government's request for accurate information.

They disregarded. They had no conscience that they had any responsibility to provide accurate information. All they had to do was give them what they were asking for.

MR. ROLFES: That's contradictory to what I've seen.

MS. BALDRIDGE: That's the way that has made them look.

MR. ROLFES: But if you could cite a specific example I would appreciate that because that --

MS. BALDRIDGE: I will see if I can locate the document.

CHAIRMAN CLAWSON: Something -- and I know that we need to go on break -- but something that was interesting to me about Fernald was Fernald was done as a heavy metals. You know, the early years it was lead. We were worried about lead. It was a heavy metals plant. It was run as a heavy metals plant, and part of the stuff that comes up, as Sandra was saying, the people that were doing the samples, they were not -- there was a question of how they were being handled and so forth.

And you're right, Mark. The earlier years they were looking for chemical analysis for other things, but they could also be used for uranium content.

But one of the questions that came up was that if the process was being done right. Now, in Sandra's comment though, the people that said that it could not be used for this, they are not going to openly admit what they had seen or anything else like that because then they could be held liable just as much as anybody else could for not performing the task as it was supposed to be done.

But we've seen in numerous sites that it was basically to keep us within this realm. Now, later on in the years a great deal of things have changed, and I think we need to look into this just a little bit because --

MEMBER GRIFFON: Brad.

CHAIRMAN CLAWSON: Yes.

MEMBER GRIFFON: Before you take a break -- I know you're ready to take a break -- can I ask a question?

I just wanted to know. Each sheet has a reference ID. Are those log books in one spot on the AB document review drive or do we have to search them in the overall database?

MR. ROLFES: I can take a look here and tell you.

MEMBER GRIFFON: I couldn't -- that's one question.

And then the other question I have was are there other log books. You know, this was a sampling of log books, I assume, and were there other log books and are they also on the O: drive?

MR. POTTER: This is Gene Potter. Perhaps I can attempt to answer both questions. The reference ID is our for the general -- in the generalized RDB. You can search them that way.

MEMBER GRIFFON: Okay.

MR. POTTER: And I believe, if memory serves me, Mark, this is all of the data that was uncovered by the various data capture trips. This is all of the hard copy that was uncovered.

MR. ROLFES: Correct.

MR. POTTER: I do not believe we eliminated anything that we had.

MEMBER GRIFFON: Okay.

MR. POTTER: And perhaps while I've got the floor, maybe a final word on reduced sampling. Let me read from our report just to reinforce my opinion, anyway, that this was not any sort of sneaky tactic and we weren't sticking to our own plan.

But it says for most of the files inspected the normal inspection level and type was used in this analysis. For some files the reduced inspection was performed based on the experience with similar files for similar time periods. Reduced inspection allows a smaller sample to be inspected, with a correspondingly smaller number of nonconforming results for the file to meet the AQL.

Reduced inspection was discontinued when one batch failed to meet the AQL in accordance with the switching rules in the standard.

I know Harry has a problem with treating these, as I said, like widgets, but that was the plan, and to the best of my knowledge, that's what we did.

MR. CHMELYNSKI: I would like to respond with a short quote from the document itself. It says when normal inspection is in effect, reduced inspection shall be instituted provided all of the following conditions are satisfied.

The first one says the preceding ten lots had been on normal inspection and all have been accepted.

I just don't see how you could have ten lots that were accepted.

MR. KATZ: And on that point maybe we could take a break.

CHAIRMAN CLAWSON: Well, yes. It's almost lunchtime. So I was thinking that we'd probably break for lunch if that would be all right.

MR. ROLFES: John, were these the two issues that you would expect to take the longest?

DR. MAURO: I don't know. I thought we would get through in about ten minutes. The big ones are coming at the back end.

CHAIRMAN CLAWSON: Would that be all right? Because if get back we're only going to be here for ten minutes and then go to lunch.

MR. KATZ: So what time do you want to reconvene for people on the phones?

CHAIRMAN CLAWSON: Ten to one, I'd say. Let's shoot for one o'clock, and then that way everybody gets right there.

MR. CHMELYNSKI: One o'clock.

MR. KATZ: Okay. Thank you, everyone on the phone.

(Whereupon, the above-entitled matter went off the record at 11:40 a.m. and resumed at 1:02 p.m.)

AFTERNOON SESSION

(1:02 p.m.)

MR. KATZ: Good afternoon. This is Ted Katz with the Advisory Board on Radiation Worker Health, Fernald Working Group, and we are reconvening following lunch.

And I'd just like to check on the phones for Board members. Mark, have you rejoined us? Mark Griffon.

MEMBER GRIFFON: Hi, Ted.

MR. KATZ: Hi, Mark.

And how about Bob Presley? Are you with us?

MEMBER PRESLEY: I'm here.

MR. KATZ: Hi, Bob.

And how about Phil Schofield? Any chance you're with us?

(No response.)

MR. KATZ: Okay. Brad, do you want to get the ball rolling?

CHAIRMAN CLAWSON: Yes. I guess

where I'd like to start is where we ended up on Issue 2 that we had. We were talking about the HIS database. Some things have come up that I think we're going to try to determine how we're going to have to go forward, but one of the questions that the petitioners brought before us, Ms. Baldridge, is the adequacy of the data, and it wasn't just with the air sampling. It was with the data that was pulled.

And I pulled up the petition as it was filed, and I think that I somewhat slipped because I didn't catch this a little bit sooner, but we should have been looking a little bit more to the data adequacy as it was put in.

We're basing everything for dose reconstruction primarily on this HIS database. We want to make sure of the information that was put in there. So we may have to at a later date address this or a path forward for it, but at this time I'd like -- Ms. Baldridge has a comment that she'd like to make real shortly, and I believe that we're going to continue on from there.

MS. BALDRIDGE: Just a reminder. This petition was filed in December of 2005, and we are now in 2010. I'm hoping this can proceed at a quicker pace in the future than it has to date. I hope most of the issues that were of concern to NIOSH have been addressed so that we can get on with some of the other issues.

MR. ROLFES: Thank you, Ms. Baldridge.

You had mentioned about the falsification of data earlier on, and I did want to pull back our Evaluation Report. You had indicated that air samples appeared to be manipulated to obtain desired readings and to give the appearance that radiation exposure levels were much lower than they actually were, and I think that was the third issue of the three that you had mentioned earlier.

We did take a look at this, and you had supplied an affidavit to us where an individual had indicated that he was collecting air samples in Plant 5, and he would take those samples back to the lab and have them run and then report them to a supervisor, and his supervisor would look at them, and if they were high results, he would tell him to go back and resample.

And I guess that could be interpreted in two ways. He could have thought that the individual wanted him to report a lower value or he wanted him to focus to see what the problem with the process was. If it was a high sample, that would typically attract attention to a concern in the work place. They'd want to address that concern and make corrections to the process to lower the air concentrations.

For Plant 5, that was the uranium plant where they were taking green salt and reducing it into metal. We wouldn't be relying upon air monitoring data for that. We would be relying upon the uranium urinalyses to reconstruct historical intakes of uranium in that plant.

So, you know, we don't know what the affiant's meaning behind that statement was, but that was the only thing that we had found. We couldn't find additional information to show that the air monitoring data was manipulated.

Our statement in the initial evaluation of this information, we said that the petitioner supplied affidavit states that their sample results were manipulated. You had also submitted a document stating that FMPC knowingly calculated effluent releases using a method which was flawed and grossly underestimated the releases.

NIOSH could not find additional information corroborating that air monitoring data was manipulated, and FMPC Technical Basis Documents do not specifically address the topic.

While it's possible that the air monitoring results were manipulated, this practice was unlikely to have routinely occurred, and since NIOSH will not be relying upon a sole air sample result made of worker's intake, but rather a distribution or compilation of multiple air dust measurements or uranium urinalyses, it's unlikely that this practice would have a significant effect on the individual's dose.

And for this specific plant, Plant 5, we would not be using the air monitoring data to reconstruct the uranium intake. It would be based upon the uranium urinalyses.

MS. BALDRIDGE: I think the point is you said that it was not routinely. It was done. Someone made the decision concerning that. We don't know that it wasn't a routine situation or that that mindset wasn't routinely initiated in other manners in the collection or presentation of data.

And you can't really assume that it was a one-time occurrence.

MR. ROLFES: We have gone back and interviewed a couple of individuals that were specifically mentioned in the affidavit, and they indicated that that was never the practice. They had always focused on concern for employees' health, and that they had focused if there was a concern with the high air sample result, that they would go back and take a look in greater vigor and do more sampling.

That was what we were told based on our interviews of subject matter experts.

CHAIRMAN CLAWSON: Well, and I understand that we could debate this quite a bit, but I'd like to read just from the petition right here.

Documents indicate that there was no monitoring for special types of ionizing radiation known presence. Monitoring was limited in frequency and limited of groups. Monitoring was inaccurate due to sampling techniques and dose limitations. Some data could not be interpreted due to deficiencies in the record keeping procedures and so forth.

Workers' assignments often changed as they were rotated to different locations in an attempt to limit exposure levels.

I think the bottom line and what Sandra has come up with, I think that we've got to look a little bit further into this, and this may fall into a NIOSH or an SC&A issue, but I think what we should do on Issue 2 for right now is to kind of think about the discussion we've had, and we're going to proceed on with it, but we might end up coming back and tasking SC&A to look into this a little bit.

I've got to talk with the other Board members because I really don't know. Bottom line is I don't know what to do on this one. There is a question of data integrity and so forth. I think that we have met that, my personal opinion.

So for this one right here, I think that we'll continue on to Item 3, but we do need to address this and request a path forward.

I have one other question now. Are construction workers going to be -- they're going to be monitored, their dose reconstruction is the same way. Are they different? Because I've seen it at different sites.

MR. ROLFES: Well, it depends on whether or not they were monitored, I guess. If they're not monitored and they were working on-site in the radiological area, you know, decommissioning the site or something perhaps, we would find the uranium intake based upon the coworker distributions that we have.

If they were monitored, then we would use their data.

CHAIRMAN CLAWSON: Okay. So if there's no data, you're going to use the site?

MR. ROLFES: If they didn't have data in their file, for example, if they didn't have uranium urinalyses in their file, what we would do is use the coworker intakes to apply to them.

DR. MAKHIJANI: Is Steve Marschke on the line?

DR. MAURO: No.

DR. MAKHIJANI: Does anybody remember if Fernald was explicitly covered in TIB-0052, internal?

DR. MAURO: I don't recall. I don't believe it was.

DR. MAKHIJANI: Now, we have sent our review of TIB-0075 in the context of the Savannah River Site, and as part of that, we looked at whether construction workers at Savannah River Site and non-construction workers were comparable or whether in some instances construction workers seemed to have higher exposure potential.

And you all already have the report. It has been finalized and sent to the Board or the Savannah River Working Group. So, Brad, you would have that, and I'd be happy to send it to you, Mark, if you don't.

But I think the prior assumption, which perhaps we were all sharing, that construction workers can be subsumed under a general coworker model would need another look.

CHAIRMAN CLAWSON: Well, and I need to explain why I brought this up, because we do have an individual that's in our room that was discussed with us, and Lou Doll, who was a former Fernald worker; he also is a Building Trades National Medical Screening Program. And the question was brought up when we were doing this of how construction workers were falling into this, and this is why I brought up this question, because I could not answer it.

So this is maybe another thing that we need to look at, into this because especially on the HIS database and also how the construction workers, you know, work into it, but to be right honest with you, I think we're going to have to sit down and figure out a path forward on this one because I think I've dropped the ball from the standpoint of what Sandra pointed out to me, that it wasn't just the air sampling data. It was all the data that was put into that. And I'll just leave it at that for right now.

But we're going to proceed on.

MEMBER GRIFFON: Can I just have one follow-up to Mark?

CHAIRMAN CLAWSON: Sure.

MEMBER GRIFFON: You said you reviewed a couple of experts, and they indicated that this wasn't a general practice, and the allegation made in the petition was not the regular practice.

Did you interview any other workers to either confirm or get a sense of whether -- because I think that's the crux of it, is we have to try to identify whether this is happening on a routine basis, and I'm not sure that going to the HP manager to ask is, you know, thorough enough --

MR. ROLFES: Right.

MEMBER GRIFFON: -- you know, to satisfy the petitioner or me, quite frankly.

MR. ROLFES: To be honest, I wouldn't think that any of the other workers would know if such a practice occurred. So to interview, for example --

MEMBER GRIFFON: No, no, they could have been involved in it. They could have seen it. They could have known of it.

MR. ROLFES: Yes. I mean, we've spoken with several other people. I don't know if we specifically asked them about these issues, but when you get into looking at all of the data, for example, if there was a concern about air monitoring data being manipulated, our first level of information that we would use for reconstructing uranium intakes would be the urinalyses, and those would be more reflective of worker intake than the air monitoring data.

So if, say, for example, you know, in some hypothetical scenario the uranium urinalyses were manipulated, once again, those with the highest internal exposure potentials were monitored for uranium exposures or internal exposures by the mobile in vivo unit.

So it would be another layer of information that would, once again, have to be manipulated to try to cover up this hypothetical manipulation of urinalyses.

So the compilation of the health physics practices appear to indicate that we have information that will allow us to bound unmonitored workers' intakes.

MS. BALDRIDGE: Wasn't the -- the in vivo didn't come in till what, 1970?

MR. ROLFES: 1968, that's correct.

MS. BALDRIDGE: And you've got from 1950 to '68 with nothing to compare. I mean, maybe from 1968 on it wasn't because there was a check system. So what about that 18 years prior to that? There was no check or balance. Everything was in-house. They didn't send things out.

MR. ROLFES: Well, they actually did send some of the urine samples out. Some of the early 1950 urinalyses that were conducted for Fernald employees were analyzed by an off-site entity, by the health and safety --

MS. BALDRIDGE: For a year or two years.

MR. ROLFES: For about two or three years; that's correct. And a lot of the work that was done in the earlier time period relied also heavily upon air monitoring data. So we don't have any indication that that air monitoring data during that time period was suspect.

DR. MAKHIJANI: Well, you know, I don't know about urinalysis data integrity because I've never looked into it. There is some discussion in the report that Hans wrote about this, that Hans was a principal author on for our review of the SEC petition in which some of the materials cited by Sandy were reviewed, and I just got back the e-mail, and the concerns about the non-usability of bioassay data for dose reconstruction were not limited to the 1950s on the part of the management. There were statements as late as 1984 that said you shouldn't use bioassay data.

MR. ROLFES: Correct. That's correct.

DR. MAKHIJANI: So it's a concern that goes throughout the period. So that's one thing, but I haven't independently examined that, and maybe it might be worth looking at.

But the thing that I would like to point out is there were some instances in which the data of record that were given to the public and that were recorded in air monitoring data, not in-plant DWE data, but stack data and scrubber data were manipulated, to the best of my understanding, like zeros were entered; measurements were not made.

And of more concern actually were

the scrubber releases from Plant 8 where an incorrect efficiencies were used throughout the period and never corrected. The problem was not corrected after it was pointed out. So that even when the matter wound up in court, the estimates that were not correct, and internal information indicated that they were known to be not correct, persisted in the public record.

And you know, some of that is cited in your petition, and so the reason I bring it up, it's the reason I would actually declare it conflicted, because I have some considerable knowledge of this, having looked at it and participated in that process.

And I think since it has been said that air monitoring data was maybe occasionally looked at this way, I just wanted to put on the record what the best of my information is.

MR. ROLFES: I'll address your two

points there, Arjun. The concern about having the ability to reconstruct internal doses from uranium in 1984, as late as 1984, was still not a concern about the uranium urinalyses data themselves, but the biological models that are used to interpret results which would basically allow you to determine organ dose for compliance purposes, and I think that was related to 10 CFR 835, where they were reporting organ doses.

DR. MAKHIJANI: It doesn't actually say that. The part that I have quoted doesn't actually say that. What it says is that amount of deposit of radionuclide is potentially in 1984; amount of deposit of radionuclide determined from lung count is recorded and can be used to calculate -- oh, sorry. Lost my page -- and can be used to calculate lung burden, and two, excretion urinalysis data are recorded, but this cannot be used for calculating internal dose. That's what it says.

MR. ROLFES: Right, and that was because of the lack of a biological model, which would specifically allow you to relate. We used updated biokinetic models, ICRP-66 and 68, to interpret in the most claimant favorable manner the actual uranium urinalyses.

So we have the data and we can plug those data into our computer program, into the integrated modules for bioassay analysis to determine a best estimate or claimant favorable estimate or an underestimate, depending upon the type of dose reconstruction work we're completing.

The models that we have today didn't exist back then. They were used in a much more archaic internal dose model.

DR. MAKHIJANI: I understand that, but that's an inference you're putting into that.

MR. HINNEFELD: This is Stu Hinnefeld. I'm the Director of OCAS and I also am conflicted at Fernald.

And that is, in fact, the reason why that statement was changed later than 1984, was, in fact, that models existed at that time, and those data could be used for dose reconstruction, and so to make the statement that they cannot be used for dose reconstruction I felt was an incorrect statement.

The fact of the matter is they were not until the rules of the order required. So internal dose calculations since about 1989 or somewhere around there, because they were not reaching it because it was not required, and the guidance from the DOE at this time had not yet adopted 1976 ICRP-26 models. It was still based on the ICRP-2 models.

So that is why that statement --

that is why that statement was there I am confident.

DR. MAKHIJANI: Okay. I don't have personal knowledge of why that statement --

MR. HINNEFELD: I don't remember much, but that one, I do have personal knowledge of.

DR. MAKHIJANI: Okay.

MEMBER ZIEMER: Well, plus 835 actually didn't kick in until about '92.

MR. HINNEFELD: 835 didn't, but that was, I believe, when you started. There were other things before 835 that required the calculation of internal doses.

MEMBER ZIEMER: But I want to make sure I understand what the point on the non-usability of bioassay data, those statements you were reading, which I guess also appear in the petition. Did that have to do -- the allegation there was not that those numbers have been falsified. Am I right on this?

MS. BALDRIDGE: It didn't clarify.

MR. HINNEFELD: No, the statement doesn't clarify.

MEMBER ZIEMER: Well, I understand that, but they are not claiming that that was the reason.

MR. HINNEFELD: No. The statement just says that it cannot be used. That was a historical statement. It appears well back, and that's I think a particular annual report, the report of data, a data report to DOE which I believe was an annual report, although I'm speaking from memory here, and my memory is not completely reliable.

But that document, that statement was carried forward in sort of the form letter that each year's data, the new data were put on, but the various boilerplate, the language remained the same, and when I saw it in whatever year it changed, I said, you know, that's not exactly true, you know, based on the ICRP-26 models which were available, although not in the guidance.

It's not really true to say these urine data can't be used to calculate ordinary internal doses, and so that statement was changed some time after 1984. I don't remember exactly when.

MR. ROLFES: Arjun, you had another point about the air scrubbers as well, and we did also specifically interview some individuals regarding this statement. We spoke with the individual who was responsible for changing out basically the filters in the scrubbers, and if you take a look at the reports, and I know you have previously, the entries for emissions were reported in some months or in some years as dashes or as zeros.

And when we discussed this with the individual responsible for conducting these analyses, he would do a visual inspection of the filter to determine whether it needed to be changed. If it did not need to be changed, it would be left in service and a dash or zero would be entered for the emissions for that month. It didn't necessarily mean that there were no emissions that month. There could have been. However, they would have been recorded by that air monitor in a subsequent month when they returned to determine whether the filter needed to be replaced.

DR. MAKHIJANI: That's why when I made my sort of intervention there I referred to the scrubbers as the more important problem which is not covered by the filters in that filters' stack. They had no way to monitor those emissions other than measuring the amount of uranium, the scrubber fluid, and measuring the scrubber efficiency. They didn't actually have filters because it was very corrosive exhaust, corrosive exhaust would heat up the filters.

And that set of data continued to be fundamentally flawed throughout, despite an internal memo that pointed out that the method of calculation was wrong, and that it gave high results when the emissions were low and low results when the emissions were high, and to the best of my understanding, it was never fixed even when the matter was in court.

MR. ROLFES: Now --

DR. MAKHIJANI: And the scrubber release is quite different from the air monitoring filters.

MR. ROLFES: That's what I wondered if you could clarify what scrubbers you're referring to or --

DR. MAKHIJANI: Plant 8 scrubbers and Plant 23 scrubbers, and the scrubbers that were more important and that were later determined by the RAC team, John Till's team to be the main source of emissions at Fernald, the uranium emission from Fernald largely, it turned out, came out from Plant 8 because at some points these scrubbers completely broke down or nearly completely broke down, and there's documentary evidence of that, while they were operating these plants.

MR. ROLFES: Okay. I guess the bottom line is how would that impact our coworker study or an individual's internal dose from uranium because of the number of people, given that 93 percent of the workers from Fernald had internal dose monitoring for uranium. Any environmental exposures from uranium would be integrated in their urinalysis result.

DR. MAKHIJANI: Well, there are two things of interest. One is from the Site Profile onward, and to my understanding to the present, you're ascribing environmental dose through the air releases, and through the recycled uranium White Paper, you have continued to use the Fernald site calculated air releases even though the Centers for Disease Control sponsored study done by John Till was available in which this problem was addressed.

So that's one issue. That does impact, as I understand your dose reconstruction model, actually does impact your dose reconstruction for environmental dose.

There are other problems with environmental dose that I've pointed out.

The second thing is when an engineer on the site says that our method of calculation for scrubber releases is, quote, inherently deceptive, unquote, which was said in a memo in 1971 at Fernald, and that same method continues to be used for another 15 years, this is an issue, in my opinion.

You cannot simply say that there was an occasional problem with air monitoring and that you interviewed people and this didn't occur because as Mark Griffon has said, you interviewed the person who was watching over this stuff and who could not identify, to the best of my memory, any documents on which these scrubber releases were based, which were later shown on careful, at least in my opinion, careful re-analysis to be wrong.

MR. ROLFES: We didn't interview just one person. There were three individuals that we had spoken with about this, and once again, you know, if we have emissions from the site and employees were being exposed to those emissions, it would be integrated in their uranium urinalyses that they were required to provide.

DR. MAKHIJANI: I understand that. I'm not disputing that if you have your analysis record and you're using that none of this would matter.

The two reasons to make a

statement about this at this point -- didn't say anything for quite a while -- was when you stated that there may have been an occasional problem with air monitoring, but it was not systematic, and I don't think that's entirely accurate.

And the second thing is you are still using an old source term in your recycled uranium report, and you are still saying that you're going to use that source term for environmental doses for unmonitored people.

And so there's a reason to put that on the table.

MR. ROLFES: I understand, and it could, you know, be a perception, but to my knowledge, when we complete a dose reconstruction, for example, if we have an individual that didn't participate in the urine sampling program at Fernald, we have the coworker study that we can use. To my knowledge, I don't recollect ever reviewing or completing a dose reconstruction where an individual was denied compensation based upon environmental levels.

When we have to complete a dose reconstruction, we want to make sure that we've given every benefit of the doubt to the claimant, and for the cases that don't become compensable, we use even higher intakes, TIB-0002 methods.

And so we have approaches to assign intakes which greatly would exceed any environmental emissions. So I think we're okay. The TBD which was developed back in 2003 has an environmental dose reconstruction approach. However, to my knowledge, I don't believe an environmental dose assessment has ever been the only source of exposure considered in the denial of an individual's compensation.

CHAIRMAN CLAWSON: Arjun, I think

that your point is going to come up a little bit more in the next one, but we've got to close up Issue 2 here of what are we going to proceed forward with because this is a validation of the HIS-20 database, and I'm kind of at a loss.

I think we've got to be able to check the information that was put into it. I know that the paper work was put in there, but I guess I'm --

DR. MAURO: I think what happened was it confounded a number of issues that could have been kept on separate lists, and let's unconfound them.

First of all, regarding Issue 2, I think a proposed approach was put on the table, that is, NIOSH would justify and perhaps has already justified the reason it limited its number of samples in the process of verifying the faithfulness with which the HIS-20 database was transcribed, so to speak, and the missing data.

And it's my understanding that there is an action item here. The action item is to remind the Work Group that, yes, in fact, your arguments are well articulated in your existing report of why you did that and your rationale for doing it holds up soundly scientifically.

If that rationale is not there, perhaps you will provide it to us. That would be one way of not having to do additional sampling. If it turns out that your sense is that the rationale for limiting the number of samples in, say, a group of six out of 25 groupings, cannot be really justified well for the reasons Harry explained. They have to have -- I mean, if you go back to the mil spec, I don't now if you remember.

The criteria the mil spec guidelines offers is a little bit stricter than perhaps the criteria that you imposed upon yourself for when you could cut back.

So my sense is that a compelling case needs to be made why it was okay to cut back. If you feel that perhaps that case cannot be made well from what you -- whether it has been made already or it needs to be made and it really can't be made, it seems that going back to the six sets that you cut short, you finish up the six sets, come out with a set of outcomes for all 25 sets, show where the percents come in, three, two, five, six, eight, wherever they come in in terms of the percent of samples that were not transcribed.

If and when that's done and notwithstanding whether you have to go sample some more or whether or not you're fine the way you are -- sort of like Stage 2 -- the type of argument that was made that we discussed before. It appears that the samples that we did, the ones that we looked at out of the sampling where you found that they were not transcribed, when you go back in and you go pull them and take a look at them, you find out where they fall within the distribution.

And some type of argument needs to be made whether or not it certainly appears that the numbers that were left out do not appear to be biased in any particular way.

Now, how you do that statistically, whether you simply make a graph or a table and show where they fall, oh, about half fall on this side, half fall on that side, obviously there's nothing here where there was a significant bias in terms of what was left out.

And I think that that's where I come out on what has to be done. With respect to Sandra's concern, namely, the original records that were originally the basis for everything, it sounds to me that historically the attention that was given to that subject had mostly to do with airborne emissions, and the degree to which the records regarding airborne emissions were, in fact, recorded appropriately.

The extent to which the Work Group decides that the very fact that that practice might very well have existed at that time for airborne emission, does that somehow imply that the same practices may somehow have found their way into the bioassay program?

I can't speak to that, whether or not that's it or not, but it sounds to me that -- was that a -- now, I haven't looked at the issues, but is one of the issues specifically a concern that hard copy records of bioassay data somehow may have been problematic the way the air sampling was?

DR. MAKHIJANI: You know, I think it is all very complicated. Brad, might I suggest sort of three items for kind of moving ahead. One is NIOSH has to complete the thing about the six percent.

DR. MAURO: Past that.

DR. MAKHIJANI: And whether the six percent --

DR. MAURO: That's what I just said.

DR. MAKHIJANI: So that's kind of one item, and then we don't have to discuss --

DR. MAURO: Anymore.

DR. MAKHIJANI: -- the substance of that.

The second thing is there's a data integrity issue which Sandy introduced and which you said that we need to do more work and you can tell if NIOSH is going to do something, if you want us to do something, and it obviously can't be resolved here. It's very complicated.

DR. MAURO: Right.

DR. MAKHIJANI: And the third thing is the gentleman who is the construction worker just raised this question and the question of construction workers and whether you want to look into it and what you want to look at.

And then we can kind of move on from a bioassay to a record perhaps. I don't know what. Ted knows.

MR. KATZ: My note has one other item. I mean, originally I just had two, before these other data integrity issues came up. So one was clarifying the basis for pulling back on the sampling. Either make your case or do some more to shore that up, either way.

My second bullet was to perhaps provide additional clarification regarding the test you did to determine that the data is unbiased. That was the other piece of it that you wanted.

They may have done it, and it may be that SC&A and others haven't scrutinized that closely enough, but if you look at that and you think there is more to be said about that, then now is the time to do that, right?

DR. MAURO: And that's Item 2. I mean, as far as I'm concerned, that closes the door on that.

MR. MORRIS: This is Bob Morris. Can I interrupt?

MR. KATZ: Bob, yes, of course.

MR. MORRIS: I wanted to refer you back to October 24th, 2007, transcript, more or less page 200 or 201. We discussed the intent of what we were going to do with mil spec sampling, including the point of reduced sampling frequencies when we met the prior one percent criteria, and that's all in the public record that's available. I don't think there's any mysterious thing that has gone on on this.

So we were up front before we did it and now we've done what we've done, and now we've reported it. I don't know that there's a lot more to do unless you just want us to rewrite the report for some reason.

MR. KATZ: Bob, so tell me that date of the transcript again.

MR. MORRIS: 10/24/07.

MR. KATZ: 10/24/07. Again, I was just going off of what was discussed here, but if that transcript answers the questions for why and that's satisfactory, then clearly that does the job. No one is saying that there has to be another report written. It's really OCAS looking at what's been already provided and is that fully explanational.

And then you might want to guide just as you did SC&A and the other parties to look at the material that they need to look at to be sure that they understand what you put forth to explain this, justify this. That's all I was saying.

So if it has all been already

written and said elsewhere, that's great. That's less trouble for you guys.

DR. MAURO: We're in a window though before we can move on to the third item, which is recycled uranium. I would like to hear a little guidance from the Work Group whether there's anything that anyone needs to do related to the original hard copy data integrity question that was brought up by Sandra, and, two, whether there's anything that SC&A should be doing regarding the construction worker data set for some reason might be of a different ilk than the total work data worker set.

Right now we have not taken any action on any of that. We had no intention to take any action on that, and we look to the Work Group as to what you'd like done.

MEMBER ZIEMER: Sandra?

MR. ROLFES: Sandra, did you have any concerns about construction worker monitoring in the original petition?

MS. BALDRIDGE: I had just included subcontractors. So you know, I am assuming that they would fall under that inclusion. I've spoken with some, you know, on my own that, you know, expressed the fact that there was little protection. There was little information that they were provided about what they were even working in or the danger in the environment; felt very frustrated, kind of like they were out of the -- they were subjected to the same danger and peril without any of the protection or inclusion in information.

CHAIRMAN CLAWSON: So, Sandra, in your original SEC petition, when you call out contractors, that is basically where the construction worker --

MS. BALDRIDGE: Right.

CHAIRMAN CLAWSON: -- will fall under?

MS. BALDRIDGE: Or painters or, you know, electricians or plumbers.

CHAIRMAN CLAWSON: That came into the site to work.

MS. BALDRIDGE: Brought in to work on either maintenance issues beyond what the regular workers did or for specific projects for converting them from one process to another. I think they often went into the dirt and didn't know what they were dealing with.

MR. DOLL: I know you don't have public, but I'd like to.

CHAIRMAN CLAWSON: You'd better introduce yourself.

MR. DOLL: Lou Doll, pipefitter at the plant. Started in 1983 through 2004; worked with Stuart, although I didn't know him very well.

And our first job down there was the pilot plant, and we didn't get urinalysis.

We didn't get any air monitoring.

We, like I told these guys before, we had to steal a frisker one day and damned near got fired over it just so we could justify ourselves, was there anything in there. When they came in and the painters painted the place, they painted green so that none of the material that would spill on the floors or anything was -- you could see. Okay?

So and some of the other practices that were there, the in-house people, I mean, they brought us in a lot of times because it wasn't things that the in-house people wanted to do. It fell under Davis-Bacon and that.

So they brought us in, some people, on an interim basis. They called them interim workers, to do this work, and they really didn't care, you know, because they were going to be gone in a month or two, and be that right or wrong, but that sure is the feeling that was there. Because these guys are going to be back out the door versus the people that worked for the in-house plant.

One thing that bothers me that you guys touched on, I don't want to infer anything. I think that every bit of material that you've taken, that you were given in honest, above board, you've done a good job with what you were given. Let me say that first before I make the second statement.

The second statement is my understanding is I know more about the back end than the front end, but every one of these companies that went in there, National Lead of Ohio, Westinghouse, and Fluor, were on a fee basis. It was tied to safety with the Department of Energy and a lot of other things.

As late as 2003 or 2004, just one little incident that came through was my son was working out on the -- putting in a liner and he hurt his knee, and so they put him in, and he sat in a trailer so they wouldn't get a lost time accident because it was tied to the fee base. I mean, that's the way it worked down there with safety.

MR. HINNEFELD: If you say so.

MR. DOLL: And there were other incidents that came through, and I think, you know, the feeling is that there were things covered up down there from the beginning with National Lead of Ohio through Westinghouse, through Fluor, that you know, if they didn't get the fee base, then corporate was coming back in and finding out why and somebody is going to go.

And so I think there's enough things that were found like these, you know, these different things. They had one set of dust collectors that blew apart and nothing was made of it and all of a sudden it shows up in the inquire, and now we've got the Tiger

Team down there, you know, all of these different things. Well, why wasn't that reported up front?

So I guess what that does is sow the seeds of doubt into all the data and how it was put out there. So, you know, was it widespread? I don't think so. I think it was more the individuals that were responsible for the fee to the company that set the parameters.

But there's more stories about the green salt and the hydrofluoric and all kinds of stuff down there and, you know, we could get to, but I just called two guys and they said the same thing as I did. 1983, '84, '85, '86, we didn't get urinalysis for the construction workers that worked in the power plant, and we put the system in, and the byproduct was HF, hydrofluoric, with plenty of leaks.

MR. ROLFES: So if you're

producing hydrofluoric acid, are you just producing the hydrofluoric acid or are you producing --

MR. DOLL: It was a byproduct from the enrichment process to make green salt. He brought in the hex and the catalytic chambers. I mean, he mixed it in the catalytic chamber. Okay?

MR. ROLFES: So you're working with UF6.

MR. DOLL: He brought in the hex. He heated it, put it in the catalytic chamber, introduced anhydrous ammonia. Green salt comes out the bottom. Off-gas comes out the top. Refrigerations get off-gas as HF.

MR. ROLFES: Got you. I guess what we can do is take a look back to see. You had indicated that you didn't have any urinalyses conducted in the pilot plant during that time period from '83 through 1986. What we can do is take a look back to see if the individuals that were working in that area have any urinalysis data, and if we find that, for example, no individuals ever participated in the bioassay program during that time period, that's an important point.

However, if we have indication that, you know, some of the individuals with the highest potential for exposure were, indeed, monitored, what we would do for an individual that did not have a urine sample collected from them if we had to complete a dose reconstruction for a claimant that has cancer under this program, we would use the data from those who were monitored, who likely had higher exposures to assign unmonitored intakes of uranium to those without uranium urinalyses.

MR. DOLL: To go back, there was two guys at work in there. One of them was Paul Sammons, and I'm trying to think of the other guy's name. Both of them died at a young age of lung cancer, right in the later '80s. Okay? So there's one name that you can go back to. I know Paul Sammons was in there.

And they talk about stuff getting outside. Well, he went up and cleaned the fans one day in white coveralls and came back looking like they were green coveralls. You know, and that was right where it went to the outside, using the exhaust fans on the building at the top catalytic chamber.

So there was a lot of material introduced to the outside from that -- through that building, which was the talc, which was the green salt. But I still go back to the same thing again. My biggest concern is that the material that you were given wasn't necessarily -- that there's some big holes in it -- wasn't necessarily the correct numbers or whatever you want to call it.

MR. ROLFES: Right. I guess that plays into a little bit, you know, whether a worker was monitored. That's something that we would certainly look at. If we had an individual who had been diagnosed with cancer and bio-declaimed at NIOSH or at DOL and sent to NIOSH for a dose reconstruction, we provide every individual claim the opportunity to relay this type of information to us in an interview.

And so an occurrence like that would be identified for the individual claim. When we complete the dose reconstruction, we would take a look at the information that's provided to us as part of the Department of Labor initial claim file, as well as any information provided directly to us via telephone interview or any other correspondence. Plus we would use information from our Site Profile.

If we found that that individual had no DOE monitoring records and had a potential for exposure during the operational period, we would certainly assign intakes to that individual based upon coworker data, and we want to make sure that if an individual was potentially exposed that we have assigned a claimant favorable internal dose to that person to insure that we give every benefit of the doubt to that individual in our dose reconstruction.

MR. DOLL: Well, the last thing I'll say is I understand that, but everything goes back to the same thing as to how you do your dose reconstructions based upon the numbers that you were given that were produced for you.

Now, if that material was flawed, then your dose reconstruction is flawed.

MR. HINNEFELD: In point, Mark, rather than get into the dose reconstruction process, I think the point here is, Lou's point is, I think, that the construction workers specifically in the pilot plant from

'83 till whenever that was finished -- when they were installing the 64 unit in there, right?

MR. DOLL: It's more a systematic problem across the plant over the years.

MR. HINNEFELD: Well, in terms of your personal experience.

MR. DOLL: My personal experience, well, I mean, we worked right out there with the rest of the guys on all those plants, too.

MR. HINNEFELD: I know, I know. But your personal experience was you were not monitored. You and the construction guys doing that work were not monitored.

MR. DOLL: A lot of the time.

MR. HINNEFELD: And you are saying that that is not just the only place where that happened.

MR. DOLL: Right.

MR. HINNEFELD: Okay. That's the point here, is that -- which was the point that they were trying to summarize earlier, which was construction workers, were they adequately represented and is this the appropriate database to do a coworker data study for construction.

And so that is an issue that we either -- if we have addressed, we need to pull it back out or convince them we need to address it. Isn't that where we are to get this going?

CHAIRMAN CLAWSON: Yes, I think so. I'm still trying to figure out was Sandra's the data that was put into it.

MR. HINNEFELD: Well, now, that has been captured. I think that was captured in one of the other comments that either Ted or John mentioned, was that -- and the allegation here is that you should not consider these data reliable because we have enough instances to make us think they were not careful in producing reliable data. I mean that they were willing to put down data that fit their purpose rather than what was true. So that's that statement, and that has been captured by one of the other things that John mentioned.

And you didn't say it exactly that way, but that was the issue, right, John?

DR. MAURO: Yes.

MR. HINNEFELD: Okay. So we've kind of captured these.

CHAIRMAN CLAWSON: Okay.

MR. HINNEFELD: I'm trying to move on.

CHAIRMAN CLAWSON: And I realize that, and I'm trying to, too, but I don't want to -- I don't want to miss that there's a question with the data that went into the HIS data base, the HIS-20 database.

So if that's been captured --

MR. KATZ: The question is what to do about that moving forward. What OCAS or

SC&A or both might have to do to move that issue forward. That's the question on the table.

CHAIRMAN CLAWSON: Well, that's the million dollar question. That's what I'm trying to get to as a path forward for this. My suggestion would be because NIOSH is already -- they've put forth the effort. The data is good and so forth like that. So I guess basically what I'm looking at is somehow that SC&A would be able to look into this, that there's not -- that the information that was put in was put in correct.

DR. MAKHIJANI: Well, you know, what we can do, Brad, is we can get our team together and send you a work plan on this specific question because I think it would be a little bit difficult to tease out.

MR. DOLL: Yes, it would.

DR. MAURO: To control costs and be efficient. You're talking about two new items.

MEMBER ZIEMER: Well, I'm really bothered by this. There's no allegations that the bioassay database have been falsified. Nobody is alleging that. There's insinuations that because of these other cases where there were management pressures in a report, a public report to make it look good.

And, by the way, the data are still there. I mean, you're not using what came out in the public. The reason we know that is because what's been found afterwards. You still have the information that's usable, if you were going to use it.

DR. MAKHIJANI: The thing that I was saying, the public reports and the internal report all contain scrubber information that was wrong, which was corrected later by the RAC, and that information is available.

MEMBER ZIEMER: You could correct

it and could identify what was done incorrectly.

DR. MAKHIJANI: Right.

CHAIRMAN CLAWSON: But the fact that that was done, I have a hard time even in spite of that saying, therefore, all of the bioassay data are suspect. I don't think it follows logically unless somebody can show in the same way they did here that something similar happened, and they still have that data, and we know how to correct in that first case. I mean, it was still identifiable.

DR. MAKHIJANI: Right. I mean, there were obviously, you know, a whole method of doing it with uncertainties, but yes, that data was collected.

MEMBER ZIEMER: And I still say what I said earlier today. To take a database this size and even if you had a few instances that would have virtually no impact on a coworker model. You'd have to have a giant scheme involving virtually every technician, every manager, every foreman in the place conspiring to do that. To me it's implausible. I'll just start with that.

And I don't object to SC&A taking a look at this issue, but I think it's just chasing straws just to say, well, we can't trust anything because of these other cases. I mean, this is a big, big database over decades. It's just implausible to me, and I recognize because, I mean, we ran into this, and I'll just tell you because I was responsible for the Tiger Team so. So we've seen stuff go on at every lab. Every DOE lab had stuff like this. This isn't the first place where these -- there have been places where people have manipulated the system, and I've seen it in my own institution now where people, you know -- I'm not going to wear my film badge because it's going to put me over and I'll get into trouble.

For an individual or a few individuals that occurs, and it shows up later one way or the other. You get a guy that's always right up at the limit and never over, you say something is going on here. Somebody is doing something, although management-wise that's often how you work also. You let them go to the limit and then you stop work.

But I don't want to overburden this, but you get my point. I think it's wrong to assume a priori that that whole database is suspect because of these few cases where people have done some bad things. I mean they --

MS. BALDRIDGE: But you're not going to distinguish what portion of it is --

MEMBER ZIEMER: I recognize that.

MS. BALDRIDGE: -- and what isn't.

MEMBER ZIEMER: But nonetheless, there's always a few people like that, but I also recognize at Fernald and other places, I'll bet you that 99 percent of the workers were high integrity people, all the way top to bottom. There's always a few, and you get them in every institution in all kinds of circumstances, not just radiological and health and safety issues. It has often been this way, and I know, Brad, you see this.

People want to look good, and sometimes they take the actual data and they present it in a way that makes it look good. You know, they don't actually change it, but they make it look good. And sometimes we do that in the way we write reports. We like to make ourselves look good.

But we do go back to this original data. I just add that as a word of caution. If SC&A can come up with some way to test the system, but you're trying to sort of prove a negative.

DR. MAURO: Well, I'm not advocating one way or the other, but I would like to point out the last time we were confronted with this issue was at the Nevada Test Site where the claim was made that lots and lots of workers left their badges behind. We mobilized an effort that was enormous. We interviewed an enormous number of people, and we found out a lot of data was left behind, but it did not have any effect.

We could not find; we could not come out of the back end of a very expensive process with a conclusion, a conclusive proof that, in fact, the amount of data that was left behind was such a nature that you could not build one of those graphs, and it was after spending a lot of money. You had to do it because it was the heart; it was the heart of the NTS petition, but it was no small effort.

Now, whether or not the Work Group decides this is something worth pursuing, but I can say this, based on previous experience if you go down that road it's not a small effort.

MEMBER ZIEMER: I think we need to know how you would do it and what is it going to entail.

CHAIRMAN CLAWSON: That's part of my issue, and I agree with what Paul says, but going on the other side of the fence, when you see all of these other issues that have been tweaked to make it took good.

The workers, a lot of them, have no control over their bioassay programs. This is all done behind someplace else. They have no way there, but I do know that when things get accelerated a little bit higher, this is one of the reasons why a lot of the bioassay programs, they try to get away from company having --

MEMBER ZIEMER: Right, and do independent stuff.

CHAIRMAN CLAWSON: And I know

this, and this is my difficulty as what path forward we need to go to. Maybe we need to table this one for right now and proceed on -- maybe give Arjun and even NIOSH a chance of maybe suggestions because, to tell you the truth, I don't know which way to be able to go. I don't want to go to a big expense of this, but I also want to be able to make sure the petitioners know that we have adequately looked at this data, and that we can see no signs that it was manipulated in any way.

MS. BALDRIDGE: Can I add one thing? You're talking about things that are plausible. What I find is really hard to believe, but is a fact, is that my father had two physicals. One was his last physical and one was his physical when he retired. The same physician conducted those physicals, exactly the same format. The omission was the condition of his lungs when he left. No statement on his retirement.

Within five months and X-ray showed that he had lung cancer. Now, I know that was evident before he left, and someone in their unscrupulous way or distorted thinking felt justified in withhold that information to the point that it was no longer treatable.

Now, where are ethics there? It's just another one of a multiple of pieces throughout that 40-year picture, him being there 13 years, whatever, that says what if, you know. The integrity is questionable.

Whether you can prove that or not prove it, I think it's probably an issue that will never be resolved unless, you know, documents can be found. Whether there's time and effort worth that, I can't say that it would justify the process, but the point is this is a mindset that was prevalent not just on one occasion, but periodically through a limited number of people, but it does represent a problem with truthfulness.

MEMBER GRIFFON: Brad, can I just say one thing before? I'm sure you want to get on to Item 3 pretty soon. This may be a suggestion for path forward here. I think it wouldn't be a bad idea, and I don't disagree with Paul. I think it might be a reasonable idea though to have SC&A just to keep the ball moving develop or at least outline an approach that they would use assuming the Work Group decided to go down that path to test the concern about falsifying records, you know, similar to the Nevada Test Site.

I mean, I think I want to get a sense of how they would do this with bioassay samples and, you know, sort of the extent of it if we have to go down that path.

And then the other item I had, listening to Ted, I guess I want to put a little more emphasis on that last point that Ted raised, which was the question of the discrepancies that NIOSH found. I looked in the report while some of these discussions were going on here, and I can't find any, you know, sort of concluding remarks saying that, you know, even for non-discrepancies, we see no bias to indicate that. You know, sort of the notion that they were dropping all of the high samples intentionally or something out of the HIS-20 database.

I think that at least for me if that can be clarified, I don't know that you have to rewrite the whole report, but I mean, I don't see that in the report. If it's there, if you can point me to it, that would be great, but if it's not there, it must may be an action item that would be to clarify that before the next meeting.

MR. ROLFES: It is in there Mark, and if you can give me just a second, I will point you to the specific page in our analysis.

MEMBER GRIFFON: Okay. I may have the wrong draft, too. I thought I had the most current draft, but I --

MR. ROLFES: Okay. I've got the comparison of FMPC hard copy bioassay records to the HIS-20 database, and the date on it is March 10th, 2008, and I'm going to skip all the way down to the conclusion, page seven of eight. I'll read the conclusion.

It says, for this study 33 electronic files scanned from hard copy bioassay results for FMPC were examined. There were eight files which were primary subcontractor or gross alpha-beta results. These files were eliminated since they would not affect the coworker study of FMPC employees. Twenty of the remaining 25 files met an acceptable quality level of one percent. Five files did not meet the acceptable quality level, but were unlikely to result in any significant changes to the coworker study for FMPC if the data missing from HIS-20 were to be included.

In addition to the subcontractor results and alpha-beta results, it appears that there were some issues with the early New York Operations Office data, the first two quarters of 1957, 1961 through 1963 data that may have been part of a workplace monitoring program and some data collected as a result of incidents in the 1950s. Given that there were efforts to hand enter the data when it was discovered, it is unclear as to what NIOSH was able to find even a few files that were not completely entered into HIS-20.

As mentioned previously, at least one possibility is that the data was intentionally not place into HIS-20 based on additional information not analyzed by NIOSH.

MEMBER GRIFFON: Okay. I still don't see the --

MR. ROLFES: Okay.

MEMBER GRIFFON: I mean, the closest I saw was the end of that first paragraph.

MR. ROLFES: Yes.

MEMBER GRIFFON: It's unlike that it would have had an insignificant to coworker models.

MR. ROLFES: Right. Now, in a separate analysis --

MEMBER GRIFFON: I don't know whether it is or not. I don't know. Unlikely, right.

MR. ROLFES: I think if I could have Gene chime in because I believe we had a subsequent discussion of this where we did actually look at the specific urine samples that weren't entered into the HIS-20 database. We looked at those and found that some of them were above the intakes or some of them were above the average concentration. Some were below and some were the same. So we didn't find any bias.

MEMBER GRIFFON: I guess that's what we want to look at.

MR. ROLFES: And if I could have Gene on the phone.

MEMBER GRIFFON: Sure. Yes, just list them out and share it with us. Share it graphically, I think that would solve it right there, you know.

MR. ROLFES: Okay. Gene.

MR. POTTER: Mark, you'll find that discussion in the discussion for each of the files that did not meet the AQL.

MEMBER GRIFFON: Oh, okay.

MR. POTTER: The detail is in the spreadsheet has accompanied this, which hopefully you have access to.

MEMBER GRIFFON: Okay.

MR. POTTER: But the discussion for each file that did not meet the AQL includes what the specific analysis was like, and you can see the detail on the spreadsheet.

MEMBER GRIFFON: Okay. Because I'm looking at the three that you noted that had a large number of discrepancies, referenced ID 4399, 3169, and 40322. Do you know the years on those offhand? I think one of them is '52.

I'm just wondering if there's any trend that the earlier years were more problematic. Instead of looking at this overall, were there more problems in the early years? Are these all in the '50s I guess is my question.

MR. ROLFES: Mark, this is Mark, and let me point you to the Fernald HIS-20 comparison Excel spreadsheet, and it is on the K drive for --

MEMBER GRIFFON: Yes, I was looking at it a few minutes ago and then I got logged off of this. Anyway --

MR. ROLFES: Okay. The Fernald

HIS-20 comparison Excel spreadsheet dated March 12th, 2008 has information that Gene was reporting, and what we had done when we sampled the HIS-20 database, we had selected results from the '50s, '60s, '70s, and '80s.

And if you take a look back, I'd have to take a look through each of these spreadsheets, and I don't think --

MEMBER GRIFFON: Yes, we, too, and I guess that's one of the questions I would ask, and I'm going to look at that myself. Do they all fall in the '50s or you know?

MR. POTTER: And you can see in the summary of the problematic ones what years they were from.

MS. AL-NABULSI: Okay.

MR. KATZ: So just going back to the action item, I mean, it seems like if OCAS can focus a little bit on that question to Mark and the clarification about where the information is that puts this to bed -- you can do that as followup and you don't have to sort through it now, Mark.

MEMBER GRIFFON: Yes, that's fine. Thank you.

CHAIRMAN CLAWSON: I guess I'm at the point right now of, I guess, John, if you could just -- the other part of the database, but we don't want to -- we want to look at what it's going to --

DR. MAURO: Right. I understand. Our marching orders are very simple right now. We're to regroup amongst ourselves and give some thought about where there's a plausible way of taking a look to see whether or not there might be some problems, what it might involved and whether it can or can't be done, and we'll just report back.

So it will be a very -- it will be regrouping with our crew and say, listen. Is there any way to come at something like this?

The folks that know the database,

Bob Barton knows the database, and whether or not there's a way to get a handle on this and let you know, and if it turns out pretty quickly, great. If it turns out it's a big deal, you know, you make your call.

MR. KATZ: But what's what resources that would require --

DR. MAURO: Yes, exactly.

MR. KATZ: -- and keep in mind that you'd want to keep those --

DR. MAURO: I listened 100 percent. So that's that. Okay?

MR. KATZ: Okay.

DR. MAURO: The other half was the construction worker. Now, I'll tell you right now that that's an easy problem. All right? You see this card over here? Let's make believe these are all the workers. The same thing, the same story. These are all the workers. All right?

If there's a way to go into the

database, and maybe someone could tell me very quickly, we know who the workers are and the construction workers. We know go in and do a sort, boom, drop out all of the construction workers, and plot then on the same thing.

That plot looks like this.

CHAIRMAN CLAWSON: I realize that. I think actually NIOSH is going to -- I think this is kind of going to be an item for NIOSH of how they're going to handle the construction workers. I think that's where it comes down to. I think that will fall into NIOSH and how they're going to handle the construction workers, subcontractors, whatever, because that is part of the SEC.

So that would be --

MR. ROLFES: Not all subcontractors were construction workers. So --

CHAIRMAN CLAWSON: Right. Well, I think --

MR. ROLFES: They used to deliver ice.

CHAIRMAN CLAWSON: Right, right. Well, I think that's how that comes in there because we got in this construction worker issue before. So I think that one falls into NIOSH's work there.

MR. ROLFES: Yes. There is, you know, a large food service, you know, companies coming in and other products that aren't related to radioactive material.

CHAIRMAN CLAWSON: Right. Well, I was looking more at the rust. I know they had several contractors come in and do a lot of that stuff, and those were all construction workers and so forth like this.

MR. ROLFES: I know that we have a file specific to Rust Engineering employees.

Gene, do you happen to know if we have looked at any of the data from Rust Engineering in our analysis of the HIS-20 data?

MR. POTTER: You notice in the report we talk about specifically excluding those, and I think we've found initially that the files that we managed to capture did not appear to be in HIS-20 for the old subs. I think this was fairly early data. I'd have to refresh my memory on that, but I think you will find that at least some of the stuff we have captured is probably not in HIS-20 for subs.

MR. ROLFES: Okay. That sounds right.

CHAIRMAN CLAWSON: Okay. That answers quite a bit.

Are you guys treating the construction workers a little bit different than the normal workers?

MR. ROLFES: Let me explain a little bit once again. You know, we've got the HIS-20 database that we're using to assign intakes to unmonitored workers from Fernald just because the construction worker data wasn't entered into HIS-20. If that's the case, that doesn't mean we don't have it because when we receive -- we did do a data capture, and we have those hard copy records in our database. So because we have those and because we also receive individual specific dosimetry information from the Department of Energy, we would have that data for a claimant if they were monitored.

So we can go back to take a look to see, you know, I mean.

CHAIRMAN CLAWSON: I just want to make sure that we cover how the construction workers -- if it was going to be done the exact same way if they have the data and so forth. This has basically come down to NIOSH and how they were handed out.

MR. ROLFES: Right, right. As John Mauro had said, you know, if the construction worker data all indicate, you know, higher intakes, then something needs to be done. If we find that the intakes are roughly the same or less, then I think we're okay.

CHAIRMAN CLAWSON: Okay. That sounds good.

Well, now that we got rid of the two small issues, we're going to --

(Laughter.)

CHAIRMAN CLAWSON: -- we're going to proceed on to issue -- unless there's anymore discussion -- we're going to proceed on with Issue 3, which is recycled uranium. This is basically NIOSH will review the issues raised by SC&A in the White Paper and determine if followup investigation is needed, and so forth.

So that's to you, Mark.

MR. ROLFES: All right. I apologize. I didn't expect to get the ball back this quick.

DR. MAURO: Do you want me to set -- tee it up? I could tee it up for you a little bit.

MR. ROLFES: Well --

DR. MAURO: Tee it up?

MR. ROLFES: Well, it's a pretty complicated report, you know. We have 11 different --

DR. MAURO: Yes.

MR. ROLFES: -- and it's quite complex, and we discussed this during the phone calls, and I think what you decided during the phone call was that NIOSH would give us a formal response.

CHAIRMAN CLAWSON: Right. Okay. We'll just sit tight there.

MR. ROLFES: All right. Well, the response that I had previously issued was that OCAS does not intend to reexamine the DOE provided recycled uranium data, and I just relied upon the previously calculated bootstrap means for the plutonium concentrations in recycled uranium.

A brief review of the data show that the log-normal means and the bootstrap means both support the claimant favorability of the NIOSH default to 100 parts per billion on a uranium mass basis. This default is ten times higher than Fernald's historical administrative control for recycled uranium shipments.

The exceptions to the claimant favorable default of 100 plutonium parts per billion would be Paducah tower ash residue shipped in several T-hoppers to Fernald for which additional engineering controls did come into place. They also included some personal protective equipment, such as air line respirators, and also put individuals on plutonium bioassay programs.

Those plutonium bioassay results

for employees are in the HIS-20 database. We excluded those specifically from our analysis of the uranium data because they were separate, but the data is in the HIS-20 database, and you know, from what I recall, there were no positive plutonium results except for maybe a handful of individuals, maybe ten people, and they had done some initial studies and evaluations. I believe they had done some lung counting on those individuals as well up at Hanford.

So we didn't feel that it would be a good idea to go back because there is quite a bit of data, once again, that we didn't feel would put us in any better position.

Now, our approach for --

DR. MAKHIJANI: Just as a point of information, Mark. Is what you are reading from your White Paper?

MR. ROLFES: Yes, it's my response to the issues that had been sent out by John

Mauro.

DR. GLOVER: Here I recall there were some points that were raised like the uranium from the tanks. I think we agreed that we weren't -- well, we had not had time to add it.

DR. MAURO: Now we're getting to where we --

DR. GLOVER: All right. We had not had time to adequately move forward. That was the response. We had not. So we can't adequately -- you've got, you know, 12 or 13 different things. Some of them require input from Hanford. We have data on the tanks and what happened, but we haven't had time to really trust these as they properly should be, as we can.

DR. MAURO: I think it --

DR. MAKHIJANI: That's what I was thinking.

DR. MAURO: Yes, I want everybody

on the same page because my sense is that everyone is looking at this from a different level, and I want to get everybody on the same level.

You're 100 parts per billion and you're mixed for your reference approach. The reason we had a problem with it is we believe it probably useful for the time at which the PUREX process was being applied to recycled uranium coming out of Hanford over a certain time period, which represents a large fraction of the uranium that was recycled, a lot of it.

However, we understand that there was recycled uranium coming in Fernald that came in from lots of other different kinds of processes. This is separate from the tower ash analysis I understand the tower ash. Let's put the tower ash over here. I'm not worried about that right now.

MR. ROLFES: All right.

DR. MAURO: Now, you know, there

are processes that were other than PUREX. In fact, Hans is on the line. He identified a couple of them. He has some information, and those processes were such, the material they started with, whether it was the tanks and the processes, the chemical processes they used were substantively different than the PUREX process, and right now we have no knowledge, and we were not able to find or have an appreciation of whether or not the mix that you folks have selected as your default mix is, in fact, applicable to the recycled uranium that came out of these other processes, other than the PUREX process.

And in fact, that's my sensibility of where the problem is right now, and we need to hear back from you why you believe the mix that you selected is also applicable to uranium that was recycled from these other processes and perhaps other facilities.

DR. MAKHIJANI: John, in addition,

I just -- you know, the report that we sent you is quite complicated. For instance, in your White Paper, you used dust collector data from 1985 to argue that what you've done is claimant favorable because the average plutonium concentrations in dust collector data were less than 100 parts per billion.

However, in making your average, you omitted the highest concentration of plutonium in the dust collector data from the Titan Mill, which was 3,548 parts per billion. It's in your White Paper.

And also in the same dust collection data from 1985, the strontium-90 ratios for plutonium varied by four orders of magnitude. So the White Paper that we sent you is a fairly complex document that we didn't agree with your reason for excluding the Titan Mill data. You referred that, you know, it has something to do with raffinates, and I didn't agree that it had anything to do with raffinates.

So you know, I'm with Sam, that I think there are certain issues that are covered by saying we looked at the tower ash and so on, but there are several other issues, including the U plant thing that John raised that are in the report that to the best of my understanding NIOSH has not addressed, and we just looked at the working group and to NIOSH to say whether you're standing where you are or whether you're going to address them, and I understand from Sam you are going to address them.

CHAIRMAN CLAWSON: I guess my standpoint is that really at this time you guys aren't really -- you don't have it in a formal form to be able to reply back to us, do you?

DR. GLOVER: No, not at this time. We can't. We're not going to be able to make a response that's --

CHAIRMAN CLAWSON: Okay.

DR. GLOVER: No, we're not going to be able to do that.

MR. ROLFES: What exactly is the issue? Do you have indication that the individuals who were working with Titan Mill samples or Titan Mill waste, do you have indication that they never participated in the plutonium bioassay program that was in place? Is that the issue?

DR. MAKHIJANI: Well, you're using 100 parts per billion.

MR. ROLFES: Right.

DR. MAKHIJANI: Yes, I don't know that they participated or didn't participate. I mean, and I don't believe that you presented any information to show that they did or didn't. There wasn't a lot of plutonium, and this is just one snapshot. Dust collectors were emptied, right?

MR. ROLFES: Right. Yes, sir.

DR. MAKHIJANI: The same dust collector wasn't over there.

Now, in 1985 you have dust collector data showing a huge range of neptunium to plutonium rations, a huge range of technetium to plutonium ratios, a huge range of strontium to plutonium ratios that far exceed the choices that you have made.

So there is the question of the plutonium concentration in itself. There's the question of whether a variety of different recycle type of uranium were used, which I would argue is indicated by just this one 1985 snapshot.

Now, the DOE itself has said caution against back extrapolation of this data. You've got a fundamental data validation issue to use in an SEC context in which the DOE reports were all done as materials balance exercises.

Materials balance exercises are

macro exercises for figuring out where all of plutonium went. That's why the DOE got into this stuff. Whether that's an adequate exercise to back extrapolate -- so all I'm saying is I've reviewed what we sent you and I also, again -- and I also reviewed in part what you wrote, and as I understand it then, from my opinion there are a number of outstanding issues, and I'm happy to go over them in detail that haven't been addressed, but if NIOSH is going to address them, then simply, you know, we should wait for that time.

MR. ROLFES: Let me set the context a little bit to explain how we would assign an intake from the other radionuclides right now. Basically when we would receive a case for a dose reconstruction at NIOSH, we would first go to the DOE response files and take a look at the uranium urinalyses there.

If there were none, we would

assign the coworker intakes of uranium as the very first basic step of an internal dose reconstruction.

Now, this sort of goes back to my first response that I had previously issued, and my response for number one, the issues that we discussed today, I said the application of the 50th percentile uranium intake to an unmonitored worker will likely overestimate that unmonitored worker's actual intake, giving the simplifying assumptions applied by NIOSH during the dose reconstruction process.

Some of these assumptions specific to internal dose reconstruction include, but are not limited to the following:

The assumption that the employee was chronically exposed for an entire year.

The assumption that the employee was exposed to the uranium compound that resulted in the highest internal dose to the target organ of concern during the dose reconstruction.

The assumption that the employee was exposed to the radioactive materials via the exposure pathway that resulted in the highest internal dose to the target organ.

The claimant favorable assumptions that the uranium was enriched above what the empirical data demonstrate to us.

The calculation of internal dose to the target organ using the single uranium isotope from a mixture, such as U-234, which delivers the largest internal dose.

These are just some of the basic assumptions. So we use that information to reconstruct the uranium intake.

Now, on top of that claimant favorable uranium intake, we go a step further and we apply intakes of plutonium, neptunium and technetium, and these are based upon the 100 parts per billion of plutonium on the uranium ash basis.

Historically, plutonium concentrations in uranium sent back to the Fernald site were controlled at levels of ten parts per billion or less. Now, we feel that to apply a chronic intake over an individual's history of employment using coworker intakes or their actual uranium urinalysis data, because we're already overestimating those uranium intakes, we'll likely be overestimating the plutonium intakes and the neptunium intakes and the technetium intakes.

When it comes down to it, if you take a look at some of the fission product contaminants, like technetium and strontium, the doses imparted by those radionuclides are typically not included in a dose reconstruction because they're less than a millirem.

The plutonium and neptunium can be significant for a certain party of organs, and so maybe what we need to do in order to better address this is show the impact of how one batch of elevated plutonium contaminated materials, how that might affect a specific case, and maybe we can complete a dose reconstruction or a sample dose reconstruction to demonstrate. No?

DR. MAKHIJANI: We understand. I think we have a very clear understanding of what you're doing.

CHAIRMAN CLAWSON: I hate to get in the middle of this, but I really don't think that we can address this adequately until we have a response back. I'm sorry.

DR. GLOVER: I think we just leave it right now. In trying to come up with a bunch -- we will have a -- and we may not respond to every point. We certainly will review it and see which things seem to be the priority, but we commit to coming back with it.

DR. MAURO: It's really simple. You have a default mix that you're going to use as part of your coworker model. We have raised for a variety of reasons why that mix -- there are questions whether or not that mix is, in fact, claimant favorable.

DR. MAKHIJANI: Whether it's bounding under 42 CFR --

DR. MAURO: Right, because remember I would be --

DR. MAKHIJANI: Well, we're in 42 CFR 83.

DR. MAURO: Right. I would be the first to admit that on average if I was looking at 100,000 workers, and I was going to say I want to find out the average intake in the aggregate, the approach you take is going to be conservative.

But we're not trying to do that. We want to make sure every worker one by one by one at different times, different places, different jobs are, in fact, being -- from different organs, that their doses are being reconstructed in a way that a plausible -- decide to do it correct and reasonably bounded, sufficient accuracy.

I think that you have an obligation to put to bed the concerns regarding the mixes that you've seen in various data, the concerns that Hans has looked into. Though you're seeing the six percent or ten percent out of the PUREX process, we don't know what the mix is in terms of percent -- not percent -- parts per billion.

We don't know what they are, and these other processes that were going on concurrently and prior to the PUREX process.

DR. MAKHIJANI: Well, in fact, you know, the criterion at Hanford for UNH was 80 parts per billion in 1951. So, you know, I take Sam at his word. It's quote in here.

This other complication is in there about blending and so on. We don't know whether it was ever done. It just needs to be gotten into.

You know, I reviewed the whole report in preparing for this, and I defer to Sam. I think it's just right. I take him at his word.

CHAIRMAN CLAWSON: Okay. We're going to move on to Issue 4. NIOSH will respond to us in writing.

We're going to proceed on to Issue 4 which is radon breath analysis associated with reconstruction with Ra-226 and thorium exposure.

Who has got that one? I think NIOSH has. NIOSH has that responsibility.

MR. ROLFES: I don't know if the best way to address this would be to point you to the sample dose reconstruction that I had provided to the Advisory Board back in

February of 2006 when I presented the initial evaluation. It clearly demonstrated I believe it was internal dose reconstruction three. It was a sample dose reconstruction provided to the Advisory Board which shows how we would interpret radon breath samples to estimate a radium body burden using information from OTIB-0025.

From there we would go back. Sign-in takes other radionuclides based upon the isotopic contents of Silos 1 and 2.

DR. MAURO: Well, subsequent to the position we've taken our report made a couple of points. That is, radon breath analysis using the protocol that you folks have identified -- and Joyce is on the phone and she could certainly weigh in -- the general sensibility is that, yes, that protocol will work. That is, if you have a body burden of radium-226 and you do the radon breath analysis in accordance with the protocol you lay out in the OTIB-0025, you'll probably come up with a pretty good estimate of the body burden of radium-226.

So notwithstanding, I mean, certainly let's for a moment assume that that's fundamentally a sound approach, well accepted by the scientific community.

Now, the concern that we raise that transcends your example is that we know that it's going to be difficult; that you have radon breath data for certain people at certain times doing certain jobs, but we also know that there are people out there at different times, and the question becomes doing different jobs does the radon breath data that you have -- basically you're building a coworker model. You're saying we have radon breath data for a bunch of worker, and here they are.

Now we're saying that we know there are other workers working at other jobs at other times that you don't have radon breath data, and we need some level of assurance that the coworker model that you're building, using the data that you do have, can be appropriately applied -- and this is only radium now. I'm only talking radium now -- can be applied to these other workers. There are holes.

Now, we'd like to hear a little bit more about why the radon breath data that you do have in your data set for the workers you have can be applied to these other workers, and we identified in our report what the time periods were, what the job categories were; whether or not there's anything about those other workers that perhaps their exposures were substantively different than the ones you do have radon breath data for, and we want to hear more about that.

MR. ROLFES: All right.

DR. MAURO: Now, and the other one

is very simple. The thorium-230, there are situations that have occurred in the past where there was thorium-230 but no radium. So the use of the ratio of the radium to the thorium-230 will not always work, and we want to hear a little bit.

Now, I understand from our conversation last week that you folks are working that problem and the White Paper is on its way.

MR. ROLFES: We sent it.

DR. MAURO: Oh, you sent it out.

MR. ROLFES: We submitted it to you.

DR. MAURO: Okay. Great, great. So now the ball is in our ball park now.

MR. ROLFES: Yes.

DR. MAURO: Okay.

MR. ROLFES: Correct.

DR. MAURO: So bottom line is, okay, they responded to the thorium problem.

The ball is in our park to review it because we were given the green light during the previous meeting to do it.

We will do it.

MR. ROLFES: Okay. Let me --

DR. MAURO: The first item is still on the table.

MR. ROLFES: Okay. Let me point out first the thorium-230 White Paper that we've developed, it was put out onto the K: drive or, yes, the O: drive per you on January 20th of this year, and let's see. I don't know if you want to go through any of that information right now.

DR. MAKHIJANI: Can you tell us where it is?

MR. ROLFES: Yes, I sure can. If you go into your K: drive under all the files that are there and quick find.

DR. MAKHIJANI: This is for the document review?

MR. ROLFES: Yes, that's correct. May be documents review under Fernald.

DR. MAKHIJANI: Yes.

MR. ROLFES: And if you go to the top column that you've got name, size, type, and date modified.

DR. MAKHIJANI: It's under the main Fernald directory?

MR. ROLFES: That's correct. I guess the easiest way might be to click on the date modified column there and it should have something that pops up last week.

DR. MAKHIJANI: Yes.

MR. ROLFES: And it's dated 1/2010.

DR. MAURO: Okay. So let's look at it.

MR. ROLFES: I believe I sent it out in an e-mail as well because it was Privacy Act cleared. Let me see if I can find the e-mail, as well.

DR. GLOVER: The e-mail occurred on the 19th.

MR. ROLFES: The e-mail was sent out on the 19th. Okay. Thank you.

Okay. Now, back to the first issue. You were --

DR. MAURO: The radium-226 coworker model using radon breath data.

MR. ROLFES: Right. I think Bob Morris had put together an analysis early on. We set that analysis aside because we had OTIB-0025, which allows us to estimate radium body burden from radon breath analyses.

DR. MAURO: We're okay with that.

MR. ROLFES: I think maybe his analysis might have spoken a little bit. It might not address some of the issues that you've just indicated you had documents that in your review, but it might speak to this somewhat.

And I don't know if Bob or Mel is

-- is there anything that you might be able to relay about how we would estimate radium exposures for workers that did not have radon breath analyses?

And I know a lot of it pertains to the changes in the types of materials that came to Fernald. Rather than receiving radium ores that hadn't been milled, the later years was more involved in producing ore concentrates, and those ore concentrates didn't have the radium-226 contamination in them because it was stripped at the mill.

And so the radon exposure or -- excuse me -- the radium exposure issue is slightly different based upon the period because of the different materials being processed. It was the early materials back in 1951 through -- really through I think the drum dumping that occurred at Fernald for all of the silos or for Silos 1 and 2. All those drums, there were roughly 13,000 drums of K-65 wastes that were shipped from Mallinckrodt and dumped into the Silos 1 and 2. It was those workers who would have had exposure to the radium content in the ore, and those are the ones we have the radon breath samples for.

So we went back and interviewed some individuals to determine when the changes in the types of materials occur, and the types of processes that were in place, and I wondered if I could have Bob or Mel contribute a little bit about this discussion.

MR. RICH: Mark, this is Bryce.

MR. ROLFES: Hi, Bryce. Thank you.

MR. RICH: You mentioned Mallinckrodt, 13,000 drums, and the reason they were put into Silos 1 and 2 is because the United States didn't own them. They were owned by the Belgian Congo people, and so they were separated.

And Fernald also processed Belgian

Congo -- and all of that material came out in what was called the hot raffinate system. They were slurried, the 13,000 drums, over about a three-year period, and the workers themselves weren't limited by the external exposure. The drums themselves were in the few hundred millirem per hour background levels, and so that's why it took them as long as it did to dump the waste slurry and put them in Silos 1 and 2.

We have a database of air sampling data. Initially we thought we would assign doses on the basis of air sampling data, and then we have a significant database that Bob Morris has analyzed on radon breath analysis, and that is applied primarily to the people that worked the slurry transfer of those raffinates, which contained primarily the radium-226 and other isotopes.

The intent, I think -- and Bob can address this in more detail -- but was to take the radium body burden as determined by radon breath analysis and apply a ratio of what else was there in the raffinates. This only went on for about a three-year period of time, and then they ran out of pitchblende. So it was a relatively short period of time and a specific number of people involved.

DR. MAURO: Bryce, this is John. In our report, we identified a number of time periods and work jobs that involved exposure to radium that were not -- and when we look at those, we see some of them you have radon breath analysis where it could be used to reconstruct radium body burden, but in other cases, other time periods and other jobs you did not.

And I guess, you know, what we were hoping to see is an argument made why the ones you do have were the bounding ones so that if you used that data for reconstructing radium body burdens or some high end in the distribution of that, you'd feel a degree of confidence that you could apply that to these other workers because the other workers had a lesser potential.

We didn't see that. We haven't seen that. If that's the case, we'd sure like to see that.

MR. RICH: Let me just say, and perhaps this is not in as much detail as need be, but the operation of transferring those 13,000 drums of hot raffinates from Mallinckrodt was the highest potential intake, and so there were some others primarily associated with tending and feeding the K-65 silo material, but the rest of it was in the transfer from the modified PUREX process that processed the pitchblende ore, and the exposure associated with that was -- the exposure potential was much less because it was not a handled raffinate system.

That's very qualitative at this

point, and we can examine that in more detail, I'm sure.

DR. MAURO: Up to this meeting I have to say that we had submitted many White Papers, and where we raised some simple questions, and there really hasn't been until this meeting where I think, you know, we're starting to hear answers of the nature that we're looking for.

What I'm saying is you just responded to the specific concern we raised, and I was hoping that we can go down, actually take a look at our reports, go down and say, oh, no. No, they're wrong. This particular worker who worked in this job category at this time, his exposures were much less than these other workers, and that's the reason why we believe the radon breath data from this group is more than adequate to bound to that group.

We haven't seen that, and we need that, and that goes for just about every issue that we raise, whether it's recycled uranium or it's radon breath analysis.

MR. ROLFES: I think the simple response is that we can take a look back and see if we can, you know, provide some justification for it.

DR. MAURO: Please.

DR. GLOVER: So the ball is in our court.

DR. MAURO: Yes. I mean, that might be answered. You know, if we need --

CHAIRMAN CLAWSON: Okay. So we've got an issue with Number 4 that NIOSH is going to respond to this and get back.

DR. GLOVER: Well, I think there's two. We provide you a thorium-230 --

DR. MAURO: You already did.

DR. GLOVER: And something Mark clearly pointed out. When I first read it, and I just didn't read through it enough, that the early stuff, that radium-226, because it is mixed with the thorium-230, that's the early method, and then the thorium-230, after we start dumping it into Silo 3, then there's this --

DR. MAURO: Right.

DR. GLOVER: -- and there's kind of a closed raffinate system. So that's Part 2, and that recent paper is that second phase. And I just want to be sure that's clear.

DR. MAURO: We understand that. We understand that.

MR. ROLFES: I don't think SC&A has really had the opportunity to look at the --

DR. GLOVER: Oh, no. That's Phase 1. We have two. We own half of this and you guys have the other half.

MR. ROLFES: That's correct.

CHAIRMAN CLAWSON: Wow, okay.

DR. MAKHIJANI: Now we're moving along.

CHAIRMAN CLAWSON: Well --

(Laughter.)

MR. KATZ: Brad is blown away.

CHAIRMAN CLAWSON: Issue No. 5, radon emissions from the K-60 --l

MR. ROLFES: Before, Brad -- excuse me -- could we take a break quickly, please?

CHAIRMAN CLAWSON: We're going to take --

MR. KATZ: Take ten and start back up around three?

CHAIRMAN CLAWSON: Yes, we're going to take a break for ten minutes.

MR. ROLFES: Thank you.

(Whereupon, the above-entitled matter went off the record at 2:48 p.m. and resumed at 3:03 p.m.)

MR. KATZ: Okay. We're just reconvening. This is the Fernald Work Group, Advisory Board on Radiation and Worker Health, after a ten-minute break.

Mark, do we have you back on the phone?

Do we have anyone on the phone?

MEMBER PRESLEY: Yes, I'm here.

MR. KATZ: Okay, great. Hey, Bob. You've been very quiet today.

CHAIRMAN CLAWSON: Okay. That's Bob Presley.

Okay. We've got the last item which is the K-65 silo and the radon issue.

DR. MAURO: There's two more issues.

CHAIRMAN CLAWSON: Number 5.

DR. MAURO: Number 5.

CHAIRMAN CLAWSON: Yes, Number 5, sorry. Thorium, yes, I forgot about that one.

DR. MAURO: I do believe we can go through the radon issue very quickly. As you know, we have been in a heated debate on curies released from the sidewalls. Folks have been claiming it's 6,000 curies per year of radon, and we're saying, nope, it's more like 6,000 or higher release of radon.

And you folks know that method that was used by RAC, Risk Assessment Corporation was a good method. We did it our own way, which we think is a better method, and we come up with much higher releases.

You folks have pointed out that the National Academy of Sciences have approved the RAC method, and you sent us as a result of our conference call the other day. The material from the National Academy of Sciences that you sent out as being what the National Academy of Sciences had to say about the method.

You probably received very recently a report that I forwarded that Hans prepared where we quote what the National Academy of Sciences said, and I have to tell you it doesn't look like they approved it. In fact, it looks like they disapproved it.

So I think we're at a point where you folks have got to take a look at our model on its own merits.

MR. ROLFES: Okay. To get back to that, I did look back at the National Academy of Sciences' review, and you're right. It was very brief on the discussion of radon.

Subsequently I was looking back into other documents that I had, and I had not yet sent these out to anyone, but there's a couple that I wanted to just read some excerpts from.

The first is a radon and radon flux measurement at the Feed Materials Production Center document, which was submitted -- it was done by Mound.

Just to get down to the end conclusion of their analysis, they had basically put some charcoal canisters on top of the K-65 silos, done some analyses of the results, gave the results. Basically they had come up with a couple of statements here.

"The annual radon release from the tanks is probably less than" -- and they're referring to the K-65 Silos 1 and 2. It says, "The annual radon release from the tanks is probably less than from the inactive mil tailing sites which reported releases of 200 curies to 11,500 curies per year."

So this is something that I think you guys should get your eyes on to take a look at, and also a separate report which I'll briefly describe as well. It's a Journal of Environmental Radioactivity paper titled Uncertainty Analysis of Exposure to Radon Release from the former Feed Materials Production Center by George Killough and Duane Schmidt. It was published in August of 1999 in the Journal of Environmental Radioactivity, 49, and it's dated 2000, pages 127 through 156.

And I just wanted to call attention. I know you don't have this report here in front of you, but I wanted to call attention to the particular table which lists the effluents from K-65, and they basically had gone back and looked at five different time periods on-site, beginning in 1951 all the way up through 1988, and they had put them into bins. Basically they looked at the radon releases from the K-65 silos, and the results here are reported in terabecquerels.

For the first period I don't have the dates right here on this table. Oh, wait. I take that back. Period No. 1 is 1952. There was a mean release of 3.9 terabecquerels, which -- well, I don't want to give the amount of curies. Anyway, I can punch that in in a second here.

Anyway, Period No. 2 was 1953 through 1958. The mean radon release was 5.19 terabecquerels.

Period 3, which included 1959 through 1979, the average release value was 5.41 terabecquerels.

For Period 4, which included 1980 through 1987, the mean release was 3.49 terabecquerels.

And for Period 5, we had 1988, and the mean release of radon from K-65 silos was 2.06 terabecquerels.

The ranges, I believe, were in between 46 curies per year if you convert terabecquerels into curies. It gives you a range of 46 Becquerels to --

MEMBER ZIEMER: Becquerels or curies?

MR. STIVER: Forty-six curies per year, wasn't it?

MR. ROLFES: Let me pull this up.

MR. BARTON: Hey, Mark, this is Mel. This says from 55 curies to 146 curies per year.

MR. ROLFES: Thank you.

So anyway, we've got another source of information which indicates lower releases from the silos.

DR. MAURO: Did they say how they got those?

MR. ROLFES: It's detailed in this report and this other report here, the two reports that I mentioned.

DR. MAURO: What is the name of the first one?

MR. ROLFES: I will repeat the report titles here. The first one --

MEMBER ZIEMER: Could you email us those after the meeting so we can --

MR. ROLFES: I certainly can. I can email the one right now if you would like, and the other one I only have a hard copy at the moment. So I would have provided them earlier. However, I found them last night.

DR. MAURO: Well, we'll look at

them. You might want to look at our report though, and the very interesting challenge will be why are we coming off with numbers based on the method we used, which seems to be first principles --

MEMBER ZIEMER: Were these modeling exercises or measurements?

MR. ROLFES: Well, the first document that I had referred to Radon and Radon Flux Measurements at the Feed Materials Production Center, Fernald, Ohio, has -- let me give you a brief description. It has -- it has radon flux measurements, and it describes the method using charcoal canisters which were four inches in diameter by one and a half inches high. Basically they had put the canisters on the domes of the silos, put caulking around it so that it had a sealed fit, and then subsequently counted the canisters after a known amount of time, exposure, with some sodium iodide crystals.

DR. MAURO: So the sample over some time period, but was it like a week or a month or --

MR. ROLFES: Yes. Actually they had done some as short as a few hours, I believe, here.

MR. STIVER: So they actually put in the perforations in the dome, or they were just kind of sitting there exposed to the actual air concentrations?

MR. ROLFES: I'll take a look here. Let's see. Placement of canisters. From recollection, I believe there were some that were placed directly onto the domes. They might not have had a perforation. However, they did selectively go at -- the cracks.

MR. STIVER: Okay. How about near the goosenecks and that type of thing? Were there any --

MR. ROLFES: Well, the gooseneck

was removed in 1979. So these measurements were conducted in roughly 1984. So they wouldn't have been around the goosenecks.

MR. STIVER: There were no measurements that would have been found from the earlier period.

MR. ROLFES: Okay.

MR. STIVER: -- we found were very similar to what was happening after the mitigation system was put in.

MR. ROLFES: Right.

MR. STIVER: Basically you had the same concentrations before and after.

MR. ROLFES: Right. Now, the other report that I referred to, Paul had asked if it was modeling, and, yes, it was modeling.

MEMBER ZIEMER: Yes, we need to understand the difference between. Because yours is a model as well.

My other question, do we have that

Academy statement? Didn't you just read that to us? The one that you said was pretty sort of ambiguous.

MR. ROLFES: Well, it's in SC&A's White Paper at the back. Let me pull that back up. I apologize here.

DR. GLOVER: Is this under the recent memo?

MR. ROLFES: Yes, yes. Sam, if you have that, if you could.

DR. GLOVER: Here's the hard copy.

(Simultaneous speakers.)

DR. GLOVER: You want the last paragraph?

MR. ROLFES: Yes. Actually I can probably pull it up. I've got it.

I can read the NAS statement here regarding Fernald, I believe, if I can get down to it fast enough.

MR. STIVER: Actually I have it right here.

MR. ROLFES: Okay. Looking at the National Academy of Sciences' review, it's got a -- I'm reading from the National Academy of Sciences' review of the RAC dose reconstruction for Fernald, and on page 17 of the PDF, it has a radon section, and I can read that if you'd like.

DR. MAURO: Is that what you're reading now?

MEMBER ZIEMER: I'm reading it right now, yes. It certainly leaves the question open.

MR. ROLFES: I'll go ahead and read that into the record.

"The importance of the radon source term associated with the K-65 silos is difficult to establish primarily because the silos have been modified several times over the years. If the head space has been adequately sampled, the silos inventory could be modeled for release, assuming no retardation by the cap which has been sealed to various degrees over the years as a worst case endpoint.

"It is reasonable to separate the calculations into daytime and nighttime dispersion because the dispersion figures would certainly differ. However, there is no justification given for the release terms of 140 curies per year continuous or 810 curies per year during the daytime only. It also might be a reasonable refinement to have transition periods in between."

So I think that's really the part that is relevant, and it basically calls into question what the release is, and so the RAC report doesn't really get us any further down the road on, you know, validating the radon releases. However, I think these two documents that I've just read into the record and referred to here would probably be best suited for the Advisory Board Working Groups.

I think they'll speak more directly to the number or curies or terabecquerels, however you'd like to report it, being released from the silos.

So I'll send these documents to you.

DR. MAURO: Please do one other thing for us. We certainly will look at those papers very carefully, and let's say we walk away from this. You read those papers and it looks pretty good, and then you take a look at our papers, at the arguments based on where the -- you know, there is this deficit of lead-210, and you say then where did the radon go.

I mean, we've got ourselves quite a dilemma here because the radon had to go somewhere because of that deficit. Now, if we could somehow reconcile whatever you have here and our analysis, I'd be -- it would make me very happy that we could somehow reconcile how one analysis, which is based on the radon deficit, our work, is coming up with some number and I will look at the other work that was done and how they came up with theirs.

We should be able to reconcile somewhere along the line. We maybe made an assumption that isn't appropriate or numbers and how they measured it here may not tell the whole story. So we've got to get -- now, this is -- the reason I bring it up is that we've never been so far apart on something, but interestingly enough, I don't believe it's an SEC issue because as far as I'm concerned, the radon deficit approach is an upper bound or close to it.

So the degree to which the Work Group wants to invest a lot of time on this is certainly your choice. I know it's something that I'm going to look at very carefully because I find it fascinating that we could be so different, but keep in mind, please, everyone on the Work Group, that you know this really is not an SEC issue because we can place an upper bound.

DR. GLOVER: Mark, the crux of it, are we assuming the Pinney model?

MR. CHEW: May I interject? This is Chew. I have looked at the SC&A analysis for the source term for radon, and there are potentially three issues that I can point out.

The generation of radon is based on the, as you say, the deficit amount of lead in the K-65 material. However, I don't think that we know enough of the process that would have depleted the radon -- the lead before it gets into the -- before it gets into the K -- becomes K-65 material, such as play-out, whatever process. I don't think that we know enough to make sure that all of the lead maintained itself throughout all of the process and ended up in the K-65 material. That's one problem.

The other area that I'd like to point out is that in the analysis all the radon that are emanated from the K-65 material is assumed to go into the environment. This is not the case. It goes into a silo which has some confinement function, and you have to take into account -- the analysis did not take into account the -- radon inside the silo as well as the decay of radon inside the silo -- so based on those two factors, there's some question about data analysis.

DR. BEHLING: I'd like to make some comments in regard to this. This is Hans Behling, and I'm the principal investigator behind the White Paper that is under discussion here.

First of all, the lead-212, the disequilibrium is clearly one that I designed as a non-conservative assumption because, after all, this material was put into place back in the '50s, and assuming that no radon escaped, you would almost at this point assume a very close to full equilibrium, which is not. The measurements that define the disequilibrium for lead-212 is much more recent.

Secondly, the issue of radon as it is being released from the waste package, as I clearly pointed out -- and this was the argument that was posed by NIOSH for all of the last three years since this discussion first erupted; the assumption was always that the radon somehow or other emanated into the head space where it was held up, and the majority, the vast majority simply decayed in the head space.

Now as I clearly pointed out in my report and also included an exhibit which involved empirical measurements that were taken before and after the sealing of the dome, and what you really have to look at closely is the effect of the radon treatments.

Before the dome was encapsulated or sealed off, you had certain contact dose rate measurements on top of the dome which on average for the years preceding 1978 were somewhere around 70 millirem per hour. Those measurements raised up to 400, in some cases, 400 millirem per hour after the dome was sealed, and that motivated the introduction or revision in the design of the silos to include what was called a radon treatment system.

That system would allow the evacuation of radon so that workers could actually go on top and not be overexposed, and that system had the capability of moving 1,000 cubic feet per minute and was operated for three hours before workers were allowed to go back up.

At that point, it was assumed that the residual amount of radon and its short-lived radioactive decay daughters would be evacuated to the point where 97 percent was removed, and if you look at the -- and I showed those in Exhibit No. 1 in my report -- the dose rate was reduced down to about 75 or so millirem, which is the equivalent of what the dose rate was before the dome was sealed.

To me that is one indication that says the radon that was contributing to the high dose rate after the dome was sealed was, in fact, essentially evacuated with nearly the same efficiency as a radon treatment system which removed 97 percent of the air including the radon in the head space.

And as far as I'm concerned, those data speak for themselves. I don't know how you can argue that issue.

DR. GLOVER: I do believe there's a number of things that we have completed though, Mark. I mean, aren't you in the process of uploading the Pinney study?

MR. ROLFES: Yes, yes, that's what I wanted to get back to, and let me -- well, you can go ahead.

DR. GLOVER: No, I just wanted -- there's a number of parts of the Pinney study. We have a Q-11 edition. They do a number of things in here. Mark is obviously the person who can speak.

I think we probably don't have a response yet. There has obviously been interactions back and forth. We've brought some new evidence to the table, some backup data. I think as a measurement guy, I like measurements better than models if we can support them, if the data supports it. So if we can get it based back on measurements, we would be better off, but we carefully want to make sure that we don't underestimate things and make sure -- maybe represent where are we getting our data from.

And maybe, Mark, you can speak to what we're doing.

MR. ROLFES: Yes. In addition to

these two reports, if you recall from the SEC Evaluation Report, in our Petition Evaluation we had indicated that we would use the Pinney study data, and what we have essentially here in the Pinney study that was conducted -- she's a professor from the University of Cincinnati. She had a contract with a different portion of NIOSH to basically do an epidemiological study to assess historical radon exposures and also cigarette smoking for Fernald workers, and it was an epi study essentially to look for an end result of lung cancer.

And so what they did, basically completed individual dose reconstructions for each individual on-site at Fernald, and it relies upon the K-65 modeled effluents and also incorporates another source of exposure which turns out to be larger exposure source. It was the Q-11 silos outside of Plant 23.

NIOSH has indicated and adopted

this approach. So what we have now is individual specific radon dose estimates in working level months by year, and I don't recall. I'd like to point you to the Pinney report, and her report was also published. I believe it was in the Journal of Environmental Radioactivity, as well.

DR. GLOVER: It's SRDB No. 41619.

MR. ROLFES: Thank you.

I know that we have -- let me see if I can pull this up.

The methods that were used to reconstruct radon exposure historically to Fernald workers, I believe incorporate the information from the earlier report I mentioned, the Journal of Environmental Radioactivity, the uncertainty analysis, and the radon.

So the Killough paper that I had referred to that I said that I would email to everyone, I believe the Pinney study incorporated that data but also added an additional source term of the Q-11 four silos. So what we have now, what we're currently doing, we've got all of that information in our site research database, an individualized radon exposure estimate over time based upon input from the employee.

Several former workers from Fernald were interviewed, and they put that individual employee in their appropriate building where they were working on certain shifts, you know, day or night because the radon concentration varied based on day or night.

Looked at meteorological data and essentially came up with a working level estimate of radon for each individual employee, and it's tied to Social Security numbers. So what we are doing right now is putting that all -- and we're going to use that information for their dose reconstruction if we need to do that.

MR. MORRIS: This is Bob Morris. Could I interject, please?

MR. ROLFES: Go ahead, Bob.

MR. MORRIS: Okay. I'd like to also point you to a memo that Dr. Pinney wrote to Ms. Baldridge on September 13th, 2006, while Ms. Baldridge was preparing information for the SEC petition apparently, and she writes to her about using the data from the RAC report which was an off-site dosimetry model and extending it, extrapolating it back toward the source term to reconstruct the doses on the site. She got assurance from the model developer, Dr. Killough or Mr. Killough, that it could be extrapolated back on-site, and then took the initial action that she describes of validating that model based on some on-site information that was available.

She took a separate set of data that had never been used before for this purpose that John Cardarelli developed during his Master's thesis in September, March to September of '91 and compared that to the predicted results that came out of this extrapolated on-site model and reported that there was good agreement and then also took the additional action of comparing or taking window glass panes, if you recall this part of the study, to study the lead, I think it's lead-210 that is a residual and embedded in the window glass as a confirmatory measurement of radon on-site.

That's what led her to the conclusion that the Q-11 silo data was worthy of including in an on-site model. But you get the impression that there are, besides the modeling that we've done in the RAC study, Dr. Pinney's work has also got a foundation under it of on-site dose measurement or on-site measurements of various kinds.

So I want to make sure that you

pay attention to that as you look at this, saying, well, the only measurement data that we have is that from the top of the silo during the radon evacuation work.

There are other on-site long-term evaluation data sets that can confirm the off-site as well.

MR. ROLFES: Right. That seems that the Pinney model has the validation of the RAC model essentially in it, and what we had previously said had been reviewed by the National Academy of Sciences, when the RAC model was reviewed by the NAS, we thought that it had spoken to the radon effluent, but it didn't very much, and now what we have here when we look back at the documentation we have, we found that the Pinney model actually relies upon the RAC model, which has been validated by Susan Pinney's model as well.

DR. BEHLING: Well, I think that's kind of circular reasoning, and I cannot accept that, and even if the window pane data has some level of support, and I've said it before, here you have the equivalent of let's assume you have a reactor facility that has released through a controlled ventilation system certain numbers of curies and you have an actual stack monitor that gives you the actual data at the point of release. To me that would obviously have a high priority in terms of credibility as opposed to in the case of the Pinney model swiping some window panes and figuring out how much lead were deposited onto the window pane. That would be the equivalent of taking an air measurement ten miles downwind and then somehow or other applying the concentration in a cubic feet of air and multiply that by chi over the Q value to come back to defining the source term.

To me you're orders of magnitude removed from accuracy that relates that measurement to a source term measurement where you have a stack monitor giving you the actual value. So I can't take that very seriously, quite honestly.

MR. MORRIS: With all due respect, please, don't characterize the window pane data as a contamination survey. Before you discount, you need to understand it, please. You need to go read that report.

DR. GLOVER: What I do want to say is that we kind of caught you guys by surprise. This is some new data. We've got some new things that you haven't -- there are some serious things in here because the Q-11 in her study is the dominant thing in the beginning years. That dominates that radon concentration on-site. It is not trivial. It is a significant dose impacter.

I think we owe Mark at least putting the data up to make it available. We're linking this stuff. We haven't given you guys a written response to what we're really doing. Okay? And we can take a look at what your stuff says and why our stuff really is -- why we feel it's the strongest weight approach, and that way we can proceed.

Does that seem reasonable? I know this is kind of --

DR. MAURO: Absolutely. I'm looking forward to looking at this. I'm especially interested in this, I guess, charcoal filter that was placed right on the cap, and it's going to sample what's coming out and come up with a -- I guess you measure the inventory of bismuth-214 when it's at equilibrium. I'm picturing how to do it.

That's going to give you an even more direct measurement of the effluent.

DR. BEHLING: John, I hate to disagree with that. The release from the silo dome was at very discrete locations, at fissures, the gooseneck, et cetera, and depending on where those canisters were placed with regard to those particular discrete release points, I have to, again, raise questions about the validity.

DR. MAURO: We're going to look at that. They could miss it.

MR. ROLFES: Let me point you back to the document, the Radon and Radon Flux Measurements at Feed Materials Production Center from 1985. It does, in fact, indicate that they had placed some of the charcoal canisters on the fissures of the silo dome. So to measure the flux, the flux data is going to be much more important for getting an understanding of how much radon is leaving the silo versus an external dose rate measurement, which can be highly variable as well based upon the measurement that's taken, you know, how the meter is placed, if it's measured in the same conditions and same geometries each time.

DR. MAURO: The one thing we

talked about, I mean, we're talking about we're going to look at each other's report, but I think it was Bryce had mentioned he has an argument that says, you know, there may not be this lead-210 deficit. In other words, the lead-210 may be there, but they missed it when they were sampling. I mean, that's what I heard.

Somehow -- in other words, we came up with our model. It's very simple. There's a whole bunch of samples that were collected of the radium concentration in the silo, and they went up and down and sideways, and they pulled samples, and they analyzed the radium-226. They analyzed it for lead-210 and the polonium-210 I think they looked at, and we saw a deficit.

Now if Bryce can make a case that hold it, that deficit isn't real. The lead-210, because of its chemistry or whatever happened to it, it went someplace. It was produced. The radon stayed in the silo. It decayed, and it turned into lead-210, and the lead-210 then went someplace and was not part of the sample. There is some process at work in the silo that's removing the lead-210 from the sample that they took. If that's happening and that's the reason for the deficit, I buy that.

DR. BEHLING: John, how do you account for the radon treatment system when it's in operation that reduces the dose rate on top of the dome?

DR. MAURO: Yes.

DR. BEHLING: Obviously, you're not removing the other radionuclides that are steadfast held in the matrix of the waste package, and if this whole issue occurred in the head space and played it out, you wouldn't get this reduction.

DR. MAURO: You're right, right, right.

DR. BEHLING: As I said, I don't see it.

DR. MAURO: You're right. That's the other half of the problem. You're absolutely right. We've got to look at this.

CHAIRMAN CLAWSON: Well, you know, the bottom line comes down to SC&A has not been able to see this, and we've got to go back a little ways because this was held up to us as the holy grail for the radon and that everything was good with it, and now we've changed our whole course to that. So we're going to have to have SC&A review what NIOSH has put out there, the Pinney report, and so forth because, you know, we're changing whole directions.

MR. ROLFES: That's a slightly different -- we've always said since our Evaluation Report that we were relying upon the Pinney data. So I did want to point that out.

CHAIRMAN CLAWSON: Well, and I want to point out to you what was told to me last time was that I would question the National Academy of Sciences of what they came to. I will be right honest with you. I read this, and I wondered where the heck it ever came up with it because they flat said that, as you read a lot of this stuff, they didn't have source terms. They didn't have anything. It didn't quantify anything.

But besides that, we've got this process. We've got this information we need to look at. We need to task SC&A to be able to review this and go forward with this, but I also would like NIOSH to really look at what SC&A has put out, too, because there is some good -- it makes a lot of sense to me, but I'm just me, but I think that's what we'll have to do with this issue.

MR. HINNEFELD: We'll do that.

CHAIRMAN CLAWSON: Good.

MS. BALDRIDGE: I have a question. I listened to the discussion about Dr. Pinney's report, and where does the radon come in from the thorium? The test that she did could have distinguished between that which came from thorium and that which came from radon -- from uranium, but she chose not to include any of the radon that was the byproduct of thorium, only that which came from uranium in her results.

So the results that she provided you do not include any of the, what is it, thoron. It doesn't include any of the thoron.

MR. ROLFES: That might be true, but if you take a look at the --

MS. BALDRIDGE: It's not it might be. That's what she said.

MR. ROLFES: If you take a look at the contents of Silos 1 and 2, a large part of that when you have thorium -- when you have thoron, thoron has a very, very quick decay time, and so it's very difficult to get that out of the matrix and evolve --

MS. BALDRIDGE: But evidently this glass etch test could distinguish between that which came from uranium and that which came from thorium, and she chose not to include the thorium byproduct, only the uranium byproduct.

MR. ROLFES: That is true, you'd have to take a look at the depth of penetration of the alpha particle in the CR-39 track detectors. It's something that's of slightly different concern really because the amount of thorium in those silos, the radium-226 effluent or -- excuse me -- the radium-226 content in Silos 1 and 2 is much greater than the amount of thorium by orders of magnitude, and so it's going to be a much smaller contributor.

Also because of the fact that the thoron will decay within the matrix pretty quickly and not -- it has a decay half-time of roughly 55 seconds or 53 seconds. So it's not going to have much opportunity to migrate very fast through a watery matrix. So that really wouldn't have been a significant source.

DR. MAKHIJANI: Could I make one suggestion in addition to what you've said, Brad? Just a suggestion for Sam in terms of what he said earlier, is when you send these materials, I'm somewhat familiar with Pinney's work from my review of the Site Profile, and this is a while back now, and because her work relates to the Q-11 silos, what Hans is talking about is really the source term from the K-65 silos, not the Q-11 silos.

And in transmitting these materials, if you could indicate to us how you're differentiating between these two source terms it would be very helpful because in these glass etch tracks, which are primarily oriented to a different set of silos, if I'm remembering right from many years ago, we want -- if you're going to sort out what Hans has put on the table, when you send us that material it would be helpful, I think, to us, to our team, in knowing how you sorted these two things out.

MS. BALDRIDGE: From my understanding, she took the window pane. She didn't interject glass. She took what was in the air and had come in contact with the glass. That's what she measured.

MR. ROLFES: She'd do a little glass etch with some acid and then put like a CR-39 cup detector onto the glass and laid it there for a predesignated amount of time and then subsequently look at the tracks from the alpha particles --

MS. BALDRIDGE: I mean, it's not like it was set off some place, that the thoron had to go through water before it got to the glass when you're talking about water matrix --

MR. ROLFES: We're talking about two separate issues, and the Pinney study does actually have the internal exposure in working level months by year for each employee from the K-65 silos and also from the Q-11. It breaks them out, separates them and shows what each contribution to internal exposure is.

And so I think it would be best for SC&A to take a look back at this data to determine, you know, if that might help to respond to their issue or concern.

MS. BALDRIDGE: My point is anything that would have gotten onto the glass. It would have been airborne. It would have been in the proximity --

MR. ROLFES: Right.

MS. BALDRIDGE: -- where it could have been inhales.

MR. ROLFES: Right. We're not disagreeing with that at all.

DR. GLOVER: We will commit to

putting this on paper, what are we doing, and, you know, comparing some of the stuff that comes from the SC&A reports. This does describe the distinction of how the Q-11 versus K-65, but we need to walk through that -- need an opportunity to review the stuff.

DR. MAKHIJANI: That's right.

CHAIRMAN CLAWSON: So I need to make sure that I'm clear on the path forward. Actually you guys just got these documents. So you need to prepare a paper for SC&A to review, correct?

DR. GLOVER: If we could just summarize, I think we don't need necessarily --

CHAIRMAN CLAWSON: Right.

DR. GLOVER: -- a tremendous document.

CHAIRMAN CLAWSON: No, it's just so they can know what your process was, how you're going through, because a lot of times we've gotten down there and that isn't what I thought.

(Simultaneous speakers.)

DR. MAKHIJANI: Let me make clear for you, Brad, why I said what I said. What we've been talking about is a source term from the K-65 silos which are in one part of the plant, and Hans put a model on the table that said the RAC source term was off by a factor of ten, and all the time we've been discussing this one thing.

Now there was another source of radon.

CHAIRMAN CLAWSON: Q-11.

DR. MAKHIJANI: These Q-11 silos and these window panes, and Dr. Pinney did this study about that, and I just want to make sure that what we're looking at is the same thing, you know, that we're talking about the same source terms. Otherwise we'll kind of -- if you're sending us a source term from the Q-

11 silos or a source term that's a convolution of the two things, then we won't be able to disentangle this problem, and that's -- should be clear about which source terms we're talking.

MR. ROLFES: Right. The study does break those out and makes it pretty clear.

DR. MAKHIJANI: Okay, and that must be the new element of it.

MR. ROLFES: Yes, the Q-11 was not previously considered. However, the Pinney study incorporates that in addition to the K-65 source term.

DR. MAKHIJANI: Obviously I haven't seen it.

CHAIRMAN CLAWSON: Okay. Well, we're clear on the path forward with this one.

Okay. One more. Thorium.

DR. MAURO: Thorium. There are two parts to it. Okay. I'll tee it up.

Actually we have one action item, and I think you folks have one action item. There is the thorium-232 exposures can be broken up into pre-'68/post-'68 time periods, the way in which NIOSH plans to reconstruct the doses of people who inhaled thorium-232.

For the pre-1968 time period, and this goes back a ways now, you folks have compiled an immense amount of DWEs, daily weighted exposure information in a big database, and that data in theory could be sorted by time or by building, by thorium campaign, and to an extent the position is to the extent that you could build a coworker model that covers all of the different increments, different time periods, different buildings, different campaigns, and with those coworker models you have breathing zone data and air sampling data and bioassay data now, and we love breathing zone data.

And from that you could construct

intake rates by time and location. Now we recognize that the data, gross alpha, but you're prepared. You say, well, listen. In this building at this time we can assume that all of the gross alpha we're looking at is thorium-232, even though it may contain some U-238. So that's a conservative assumption.

John Stiver has looked very, very closely at your work and all of the data you've provided us with, and you know, you've been sending us packages of material, and John is going to have a little presentation that describes the places where you're soft. Okay?

Now before we do that, though, just to let you know that there is the back end of the process, which is post 1968. Post 1968 it turns out, you have chest count data, okay, and you have provided -- last week I believe it came in -- a report, a White Paper. Following our conference call you have pointed out, oh, we have now a White Paper on the subject, and we've reviewed it, and we have Rich Leggett and Hans and Joyce have reviewed the data that you have provided to us on the chest counts.

It's basically a chest count where you are looking either for actinium-228, one of the progeny of thorium, or lead-212, which is one of the progeny of the thorium series, and from there your position is you could reconstruct the body burdens and intakes of thorium -- so for the first item, which John is going to cover, deals with the breathing zone data, pre-`68. Subsequent to that hopefully we'll get to our position regarding your recent transmittal, which we did have a chance to thoroughly review.

MR. ROLFES: Let me clarify a little bit, John. What we sent out last week was the thorium-230 White Paper. The thorium-232 White Paper that we're referring to, which describes the coworker intakes based on in vivo data, was sent out to the Advisory Board in March of 2008. So it was sent out approximately two years ago.

MR. STIVER: And that is the one that we've reviewed.

DR. MAURO: Well, I got that wrong.

(Simultaneous speakers.)

DR. MAURO: Okay. Then I'm lost. Okay. So I thought that was --

MR. ROLFES: The thorium-230 is the new one that we sent --

DR. MAURO: The thorium-230, that was part of Issue 4, and that was recently.

MR. ROLFES: Right. And that -- parts of Issue 4 you said you were going to review that, and we were going to look back at radium --

DR. MAURO: Right. Okay. Now, then what I misrepresented, I thought the chest count information where you estimated thorium-232 body burden, chest count post '68, I thought that was relatively new. You're saying it's not.

MR. ROLFES: It was from March of 2008.

DR. MAKHIJANI: We were just not aware of it.

DR. MAURO: Now, we did have -- now, we became aware of it relatively recently. That's a better way to say it.

MR. ROLFES: Yes.

DR. MAURO: And we did have a chance to look at it. I'm not saying, you know, that we did this in-depth analysis, but you know, we put some time in on that one, and we have some observations, questions, and comments, and we'll get to that, too.

But I'd like to allow John first to tee up the issues that he has regarding the breathing zone data.

MR. STIVER: Okay. I'm not sure

the extent to which everybody is going to be able to see this. What I have today is a PowerPoint presentation of -- kind of gets into our investigations into the utility of the thorium-232 air sampling data and the White Paper that NIOSH has proposed to use as the coworker model to assess these intakes, chronic intakes of thorium-232 from possibly 1953 to 1968.

Again, in the process of doing this, we've prepared a fairly comprehensive review, and came up with 20 findings. So I've also prepared a findings resolution matrix that goes through these findings, groups them according to similar topics. They're not in numerical order, but I'd like to go through the presentation and then take a look at this resolution matrix.

And my only concern here is that it may be too detailed to show up very well on the screen, in which case I can hand out some.

Let me pull it up and then you tell me if in the back of the room if you can --

DR. MAURO: This is the PA cleared.

MR. STIVER: Yes, this is the PA cleared.

DR. MAURO: Okay, everything's PA cleared.

MR. STIVER: Is everybody able to read that or do you need -- I have hard copies. I can give everybody hard copies. It would be easier to do that.

DR. MAKHIJANI: You'll put it on the O: drive, too, right?

MR. STIVER: It is on the O: drive as well.

MR. ROLFES: In the same email as Hans's radon memo. Nancy Johnson had sent out an email with three attachments.

MR. STIVER: Okay. So in any event, I can get going on this, and actually let me back up a minute. The last time we talked, it was a technical conversation that you guys had indicated that you were preparing some formal responses, but it won't be ready at this time. So what I want to do is instead of trying to go through chapter and verse on every response, is present our findings so that the Board is aware of where we stand on this, and then when you guys come back with your responses, then we can hash out all the details on that.

Anyway, let me pull up the PowerPoint here, and basically I'd like to start. Really there were two central issues in this whole discussion here. One is really an SEC issue and the other is more of a Site Profile issue. The SEC issue is whether this DWE data, this air sampling data, is accurate and complete enough to construct internal doses -- in accordance with the requirements of 42 CFR Part 83 for accuracy and timeliness.

And basically what we have here is your typical coworker data quality requirements. I like to think of it as kind of a three dimensional array. We have enough data in terms of the time period, the various facilities, and the particular occupations of the workers.

And the second aspect of this is given that adequate data are available, are NIOSH's proposed methods sufficiently to reconstruct the doses in accordance with the requirements of Part 83?

If we can move on, since it has been a while, it's been almost a year since we wrote you this subject. We have just a really brief recap of where we stood.

Two years ago, NIOSH was given three action items. We were given one action item. NIOSH's action item was to take these air dust reports, the inhalable air dust reports, that I shall refer to as the DWE reports, of which there are approximately 160, post those onto the O: drive along with a set of spreadsheets that were a sampling from these reports that were supposed to be representative of the thorium process in the plants during the entire history from '53 to 1968, with the presumption that if this data were adequate, other time periods are probably likely adequate as well.

The White Paper then provided the methodology for reconstructing the chronic intakes, and these action items were completed about a year ago before the last meeting. Our action item was to review these, prepare a draft report, and now here's the topic of this discussion, the report being on the O: drive for those of you who are interested in looking at it. It's entitled The Use of FMPC DWE Reports for Estimation of Chronic Daily Intake Rate for thorium-232 Internal Dose Reconstruction.

Now, I really want everybody to get a good understand of what the daily rate of exposure, what it really is, what the concept was, what its limitations are, and what its advantages were.

The concept was introduced to FMPC by the AEC Health and Safety Laboratory, and surveys were conducted by HASL personnel as well. It was not conducted in-house by Fernald management, and what they really sought to do was to provide an estimate of the average worker exposures by job titles which management could then use to pinpoint where the high exposures were and what types of tasks were involved for giving rise to these high dust concentrations and thereby control and improve the working conditions in the plant.

They weren't intended really to use in constructing doses in any sense of the word. However, it did provide a standardized methodology throughout the FMPC history. This method did not change during the entire period that we're interested in.

It was based on gross alpha air concentration, and so it was applicable to all work place alpha emitters, whether it be uranium, recycled uranium, thorium, or progeny, and so this gives rise immediately to the problem of, well, how do you identify these thorium workers, and it was not necessarily straightforward business to do this because thorium production took place in short campaigns. It was a small fraction of the uranium production, and so we really have to go back to process knowledge and the subject matter expertise for personnel who were involved in that back in the time.

And based on this then an estimate can be made of the plants where product was produced, the yearly production, and rates and amounts produced.

And finally if the processes by which uranium and thorium were produced -- I should say the processes that gave rise to the airborne dust -- are sufficiently similar, then you may presume that thorium exposure that took place based on the fact that ICRP DCFs are found in thorium for all organs concerned.

If you'll excuse me a second, I'll jump ahead.

So exactly what is a DWE? It's a time-weighted alpha air concentration. It's specific to a job in a particular facility. There are several tasks that are involved per job ranging anywhere from three to more than 20. I think one I saw 22 separate tasks associated with it.

The high, the low, and the average value of air concentration per task was reported in units of dpm, disintegrations per minute, per cubic meter for each task associated with the job.

However, the data that underlie that -- building that average are not reported. The time to complete each task is reported. This is based on management's assessments doing time-motion studies, whatnot, of approximately how long each of these various tasks would occur.

The two types of samples reported, breathing zone samples, which are more indicative of job-specific exposures, and then general air samples, which are the ambient contributions from the cafeteria, the wash room, things of that sort, which are really minor contributors to dose.

This slide number 6 is an example of a job exposure evaluation card. It was taken from an actual air dust report. This is for Plant 9 in 1955. This is the period of maximum thorium metal production at FMPC, and you can see here over on the far left column, if I can get my mouse here, this is the type of sample. There are three breathing zones and, I believe, seven general air samples.

Each shift was approximately eight and a half hours long, 510 minutes, and you have the time per shift for each of these operations. For example, the very first one, dumping the thorium nitrate tetrafluoride into the dissolving tank. It took about 60 minutes to perform that. The high value is 1088, low 293, and an average of 774.

The far right column is the multiple of the time per shift by the average concentration, and so these T by C values for each particular task, or type of exposure -- these are not really tasks. I guess they're just apportioned times in these various locations -- these are all summed up and divided by the total amount of time per shift to give you this worker-specific or job-specific weighted exposure, which was either reported in the dpm per cubic meter, as I said before, or else in maximum allowable concentration, MAC, which was about 70 dpm per cubic meter at the time of --

DR. MAURO: John, just a quick question for clarification. So this is a guy who had a job that's called wet area helper.

MR. STIVER: Yes, this is for one category of worker, the wet area helper.

DR. MAURO: And this is what kinds of exposures he would experience in eight hours.

MR. STIVER: Exactly. This is the type of exposure that worker would be expected to accrue on the day that these samples were taken for the workers who were actually there. We'll get into that aspect of uncertainty.

DR. MAKHIJANI: Can you go back just a second?

MR. STIVER: Sure.

DR. MAKHIJANI: There's one thing

I want to find out. If you look at any particular task within these eight and a half hours, you'll see that typically exposure levels are highly variable. So, you know, --

MR. STIVER: -- later.

DR. MAKHIJANI: Oh, you're going to get into that.

MR. STIVER: Some are even more, even more extreme than this.

Let's see. Why don't we just back up for a second and pull up -- let's see here. Here we go. We'll go all the way back up here.

And this is an example. It's Table 1. It's from the same report, and this shows for all the different workers within the facility, there were a total of 119 employees during the time period in 32 different jobs, 292 separate tasks.

There were 640 separate air samples that were collected in this year, this particular group. A portion of about 92 different tasks. The reason that you have the difference of 273 versus 92 is a lot of general air fatalities were assigned to multiple job types. So there's quite a few replicates in the data set regarding the general air stuff.

The DWEs range from the lowest at 1.36 up to 685, a little bit more than that. So this really stresses the importance of job category. The actual air dust reports are posted on the O: drive if anybody is interested, the ones that are related to this particular study.

So in summary, all we can say about this job-specific DWE, it's really a task-related air concentration for a given alpha emitter. For the specific days on which sampling took place, the answer is workers were actually monitored.

The time-weighting element here is

critical because this is really the linchpin that ties potential worker exposure to an air concentration in this big plant with all of these processes going on, a certain area for certain profits for a certain period of time. Without that, the link between the worker and the concentration is lost.

So really in actuality what we have, as Arjun alluded to here, we have a distribution of DWEs. We don't just have this average value in this report. When you look at all of the workers and look at the job, there's really a distribution on these and it's variable both in space and time. There's a lot of variation even within a given task, but certainly among all the different tasks for inside workers, and that's going to become a critical element of this discussion here.

In summary, the DWE data set is very large. It's a very impressive data set, one of the best I've seen. It covers most facilities in years and a lot of jobs, a very large array of jobs, and as I say, it establishes the typical exposures under the working conditions on days they are performed.

Now, I'm going to briefly list some of the highlights of the NIOSH White Paper which we referred to as Morris 2009, Bob Morris' paper. As we've discussed, it seeks to use the DWE data to estimate the chronic daily intake rates for thorium-232 of thorium workers in the period prior to 1968 before the in vivo counting system was put on line.

It involves a thorium time line which was developed based on the process knowledge and subject matter expert interviews; introduce what are the best estimates of the production facilities and processes, the quantities in production, and for workers who actually have specific data in their CATI report, in my records that identify the specific jobs, I'm not going to propose that we use the DWE for a specific job. However, that's typically not the case. Usually you might have some information. The guy was a welder, but there are several different categories of welders. He could be the helper. He could be the primary one. You don't know. There's a big range of exposures for that type job.

So what they seek to do is to then take job DWEs, all of them, for an entire facility, sit them to a log-normal distribution, and then pick off different quantiles, the high, medium and low quantile, and then pull these different groups of jobs that in the field have similar exposures, whether it be at the low end for administrative and clerical versus the high end of, you know, the welders, the furnace operators, the people that are exposed on a regular basis to high air concentrations of this material.

And then for those who can't really fit into either category, and they're putting the same maintenance workers and construction workers, which we have run into a different wrinkle with that today, but that's another story. These guys would go into the medium category. It would be the full distribution, essentially a geometric mean plus or minus the standard GSD on either side of it.

Now, this is where I'd like to jump off to the findings resolution matrix, and our report identified 20 separate findings. Several of them really address different aspects of a given topic, and I've listed the four big ones here. One is this issue of bounding intakes under the requirements of Part 83.

Another really gets to the uncertainty and the representativeness of this data, the different types of exposures in thorium processes, as well as the applicability to thorium versus uranium. A lot of the data reflected uranium processes. So there's this uncertainty as to whether this can be adapted to thorium for the reasons I cited before relating to process with the same types of dust collectors.

And then another set, about four, related to the statistical integrity. We have the NIOSH facility distributions reinterpreted. I'm going to get into building a job-based DWE distribution.

And so I will now jump off to that, and the first two findings relate -- let me see if I can make this a little bit bigger here. It's not going to work. I don't know if you can read this or not, but I'll go through the highlights of it, and if you'd like, I do have copies of it we can distribute if anybody wants to really read these.

DR. MAKHIJANI: We want a copy.

MR. STIVER: Anybody want a copy? I've got them right here.

CHAIRMAN CLAWSON: I'd like one. You can hand them out as you're talking.

MR. STIVER: It's fairly detailed because it was intended for NIOSH to use this as a basis for responding.

MEMBER ZIEMER: It's not marked whether it's PA-cleared or not.

DR. MAURO: Has that been through PA clearance?

MR. STIVER: Yes, all of these have.

DR. MAURO: Is it marked on the bottom that it has been PA-cleared?

MR. STIVER: Yes.

DR. MAURO: Okay.

MEMBER ZIEMER: It doesn't show up on this one.

MR. STIVER: Emily cleared them for me. You can blame her if it's not.

MEMBER ZIEMER: My copy doesn't show either a date or a PA code. I mean, what was distributed in --

MR. STIVER: Yours would probably have been the non-PA code. Those are the ones that were sent to the Advisory Board.

MEMBER ZIEMER: Yes. Okay.

MR. STIVER: Here's one.

DR. MAURO: Do you want to keep the non-PA cleared? Because they have unredacted information.

MR. STIVER: Okay. So these first findings, first and the second, related to the variability within DWE, a given job in DWE. Finding 1 states the DWEs for a specific job descriptions listed in Table 1, which you just looked at, are realistic estimates for a given job category that may not cover some workers that worked in those jobs, and this is really the fact that you have a particular worker. You know, he's, like I say, a welder's helper, primary welder's helper, and then you go ahead and assign him that DWE for that particular job. It's going to give him the average value. It's not going to be a bounding average.

And like I said, I have this category here, the secondary helper within a particular job category. The highest one is at 185, and that's really where the -- you know, you give them that value. It seems this is the highest exposure with this whole facility, but if the average worker happens to be in that category, you're not giving him a bounding dose.

And in addition to that, as I mentioned, there's this larger variation within a job type. Say in this particular situation, you've got a principal worker who gets a 9 MAC. His helper, on the other hand, gets 685 MAC. If you have to assume these guys are pretty much joined at the hip throughout the day, there's a huge amount of variation here and you don't know whether that's related to the tasks or the variation within that data as we collected it.

Now, for this particular case, the helpers, you typically give them the dirty tasks. I cite an example here of leaning these furnace pots. This is a 75-minute operation, 3.2 times ten to the fifth dpm per cubic meter for the measurements that were taken. However, we don't know where the principal workers were. At least sometimes it goes to these levels as well. So there's an element of uncertainty. There's an element of variation as well in the data set.

Now, Morris' report, 2009, in Section 4.1, it describes a method that from that estimating a log-normal distribution for the task of air concentrations, from the average and the range, and they are then able to use that using fairly standard, statistical techniques to generate the geometric standard deviation from the geometric mean and then generate the log-normal distribution to go with each of those tasks.

So in theory, you can build the distribution for each task. What they don't do is explain how to combine those distributions into a job DWE. Now, they do, on the other hand, cite this paper Davis and Strom, said it looked into these types of weighted exposures in different facilities and reported a range of GSDs.

So they list these GSDs, a range of about 1.25 to eight, and there's no guidance provided as to how they should be used to the dose reconstructor and what conditions would you apply three versus five versus eight.

And so there's a basis for a good job DWE, a distribution model here that just has not been treated.

DR. MAURO: John, let me ask you a question --

MR. STIVER: Yes.

DR. MAURO: -- so I understand. In the example you have this 685 MAC for a secondary helper.

MR. STIVER: Yes.

DR. MAURO: So it is a guy and it's a given year and we know his job description is the secondary helper. The assumption is we will assume he's exposed to 685 MAC eight hours a day or 2,000 hours per year, and the intent would be to assign that dose to that guy, I guess, in that year.

I just want to make sure I've got the mechanics.

MR. STIVER: We're looking at the situation when we actually know the guy's job description. Okay? So if we assign them just the -- I'm trying to illustrate here -- if we assign him just the DWE -- this is good. When you don't know what the job title was, that's when you go to this facility distribution. That's another -- it's kind of a different topic than the other one.

DR. MAURO: But sorry. I guess the answer is yes or no to my question. If it's a given year that this DWE measurement was made and a given building --

MR. STIVER: That would be his average.

DR. MAURO: Yes.

MR. STIVER: That would be his dose.

DR. MAURO: And you knew that this guy was designated as a secondary helper, according to the coworker model that they've developed, they would assign to that guy 685 MAC exposure continuously for the entire year.

MR. STIVER: Right.

DR. MAURO: That's how they would do to that. Okay, and one of the points you're making, though, is that the reality is that particular MAC is based on one worker, one day. You're saying there could be some variability.

MR. STIVER: It's a weighted average. You don't know how many workers that represented above or the number of samples.

DR. MAURO: Well, in other words, it may be -- let's say it turns out there are 20 secondary helpers that work in that building in that year, all 20 will be getting the same --

MR. STIVER: We know they'll be getting that same value.

DR. MAURO: Okay.

MR. STIVER: Now what I'm saying is they do address the issue among certain --

DR. MAURO: Okay, got it.

MR. STIVER: -- okay? But it's just not a completely fleshed-out model.

Now, moving on to Finding 5, this

really kind of developed this idea of variability within the DWE, and this is kind of another angle of the same issue. In those cases where you do have a good DWE, if a person gets average exposure for a job description, you haven't addressed the variability among the workers in that particular category.

In some situations there's a huge amount of variation. In one task, presumably a lot of samples were taken in an appropriate manner, and this was the top of Column 4 here. This was in Plant 1 in 1955 and a certain category of operators here, and there was one particular task of blending and canning, and there's 36 samples taken, read these samples, and ranging from eight to 65,000 ppm per cubic meter.

Now, you can see the log-normal for those two extremes, but there's a tremendous amount of uncertainty from that distribution, and we feel that if the source data were available to reconstruct that distribution, then by all means it should be done.

And the Morris 2009 is really kind of silent on this issue of underlying data availability. We have established at least some of the source data that are available. When I first started on this project, Bob Barton pulled down some spreadsheets, and I was able to match up some of the values in the spreadsheets to the high and low values on those DWEs. So I know they were used.

What we don't know is how extensive that data set is and how retrievable it may be or whether some of this data is irretrievably lost.

DR. MAKHIJANI: Well, a little bit more on that. You've got 36 samples and what you will have, the data we have is the low, the high and the average. So we've lost 33 pieces of information.

DR. MAURO: What is the eight to 65,000?

DR. MAKHIJANI: That's the low measurement and the high measurement.

DR. MAURO: Oh, I see.

DR. MAKHIJANI: And then you have an average.

DR. MAURO: And then we get the average, but we don't get the underlying samples of --

DR. MAKHIJANI: If you have two samples or three samples, you can construct the whole data set for that because you have low, high and average, but if you have more than three samples, you have lost those extra pieces of information, more than three.

DR. MAURO: But do you have four separate estimates of the DWE for that work category?

MR. STIVER: We do not.

DR. MAURO: But it says four workers.

MR. STIVER: Oh, this is four workers that were involved in that particular --

DR. MAURO: Oh, this is the workers.

MR. STIVER: It's not four worker samples. It's four workers in that category. There's a lot of -- we get further along with some other findings that kind of get that and most of the developments of this idea. So anyway, we feel that some kind of systematic search should be conducted to try to identify availability.

MR. ROLFES: We do have some of that data. I just don't know how much it is.

MR. STIVER: So, yes, the source data would really improve that and reduce a lot of uncertainty on the whole thing. Lacking the source data, I think their approach to generating the log-normal distribution to be credible to the extent that it's complete, it's not going to be up to full capacity.

Now, the next five findings relate to different aspects of uncertainty and applicability, representativeness of this data, certain types of exposures and conditions. This first one relates to off-normal occurrences such as fires and reduction bond explosions.

I've got an example here of our Petition Evaluation Reports, page 70. This describes 1960 that background levels are at this one particular fire back on Level 1 at 2.1 MAC on 458, just in this one particular instance. So the question is, you know have limited sampling, limited number of workers. How well is this DWE data capturing these off-normal occurrences within the task context?

And we believe that some efforts

will be made to uncover these types of accounts to the extent that they exist and perhaps account for the uncertainty of the model that's related to that particular aspect.

During our technical conference, Mark had indicated that you guys had found an example of these plant items of reduction bomb explosions. So there are accounts of this type of thing out there, to the extent that they can be catalogued, it's kind of uncertain at this point.

The next findings, I was just going to say that's one of the things, too, that you know we have said that we typically consider the historical dose reconstruction. You know that it accounts for an acute intake separate from a chronic intake. This has really been almost in all cases, it's not something that makes a difference in the assigned internal ones. Some of these off-normal occurrences result in fairly high intakes.

MR. ROLFES: Well, the Plant 9 incident that you had referred to, the explosion actually was lethal to the individuals involved, and so there really wouldn't be a dose --

MR. STIVER: Well, not in those cases, but in the situations where you have really high concentrations, it may not be captured in the data.

MR. ROLFES: Yes.

MR. STIVER: And there may not be accounts of them. So there's an element of uncertainty that needs to be introduced in addition to the variability in the data set.

Now, Finding 3 relates to how well the data captured -- and often that isn't known -- the thorium process. In this case, back in 1966 during a redrumming operation, and the DWE data is just very inconsistent.

And I've cited two references here, DeFazio and Audia and Starkey and Chapman, 65 and 68, respectively. But they indicate that the redrumming operations were the most important contributor to dust loading and resulted in unacceptable levels of loading in that facility on many different occasions, and yet when you look at the DWE data, you get the very highest category, which is somebody we know is handling drums, and it was only 103 dpm per cubic meter. Now, most of our way down low are much lower than that. So there's some question as to whether that data is actually capturing the intended process that it was collected for or that it's proposed to be used for.

Another aspect of uncertainty here is the situations. We know that all the workers weren't monitored during these surveys. Sometimes one was monitored, more than one. Sometimes none of them were monitored. So there was an element of uncertainty in how well this data represents all workers.

And an example of this is Plant 4, the green salt plant in 1955, where there were 21 workers in five different categories that were given the exact same general air sample mitt, but there was no difference between any of them. So there was this appearance of more granularity than really exists in the data in certain places.

And so how well does this DWE data actually represent what these guys were doing and what they were exposed to at any point in time. So there's another element of uncertainty in this group.

Finding 6, this relates to how well the uranium data can be translated into thorium exposure potential, and in addition to that, how well some known thorium exposure DWE data actually relate to thorium processing in a given plant in a given year.

In the first case, when you're looking at uranium data and trying to apply the thorium operations, it's Plant 6 in 1960, one of the biggest sets of data we've got in the entire batch. They're data from the rolling mill, from the machines area, from the inspection area, and the dust room for it is nice. This is one of the few ones that actually has a blueprint of the layout of the entire plant. So you can see where all of those different things were going on there.

And the thorium operation was going on there in 1960. There was this oxidation of thorium residues. There's all of these pyrophoric residues that get analyzed to fires all over the facility. So what have we got to do? We've got to oxidize this stuff. We've got to burn it, get it into stable form. I believe it was like 80 metric tons that were oxidized in terms of processing in Plant

6, and so what they did is they used one of the furnaces in Plant 6 to do this oxidation.

But we have data for Plant 6 from the rolling room, which we know that is where the furnace is. There's two furnaces in the rolling area. They treated the units, heated them up from the rolling machines, and there's also a slug furnace, and so you say that, well, the best data are probably going to be for the furnace loaders and heaters.

Now, the guys are working the furnace, but then the question is the residues generate the same kind of dust and plume loads as treating, you know, ingots of metal. Now, you're probably going to generate a lot of big flakes of metal coming off these ingots and larger particles, whereas, in the oxidation process you probably have respirable small particles. So, you know, how well is that data really representative of what's going on?

The example I've got from the

limited thorium production would be Plant 9 in 1954. Now, there were two days where samples were collected in May. There was only 19 workers that were involved at that time as opposed to 119 the next year. We know that the whole process got ramped up in 1954 to where, by the end of the year, they were at full force. Whereas, in the beginning they were just getting things underway.

And so the air dust report says there were now blending and reduction operations going on during the time these data were collected, but only the remelting and working the thorium, and that's the only data that was available for that year.

So is this really representative of what was going on during the month of highest exposure potential at that time? Or do you have to take next year's data and back-extrapolate, do some sort of surrogate data application to assess the exposures?

Finally, the last finding related to uncertainty would be this idea of exposure outside of the task context, basically fugitive emissions.

Now, you might say that, well, fugitive emissions, to the extent that they exist, are going to be captured by the general air samples because you're spreading the stuff all over the place. There's a higher amount being produced, and in general, our samples, it's not going to be an issue.

There are a couple of examples here. This is from 1970, which is really outside our range of interest, but it kind of illustrates the point. One was this bad boron mill. It was just a really bad source of dust. It was leaking all over the place. They put buckets under to try to catch the dust. It sprayed dust throughout the annex.

Another is these trays of calcined thorium tetrafluoride and calcium fluoride.

What they do is they took the trays and they stack them by the door of the facility to cool them off. Well, the wind comes along and blows the stuff back in all over the place, and people don't know that it doesn't also blow it off? It was a severe source of environmental exposure, too.

But the problem I have with, you know, assuming that the general air samples will catch this is, you know, how about the re-suspension? This stuff comes in and settles down in there, and so a general air sample is, you know, a meter above the ground or wherever it is may not be capturing the potential there. In other words, when the wind blows in or whatever, a forklift comes by and kicks the stuff in the air, you know, you have a higher exposure potential that may not be captured, and so that's another source of uncertainty.

DR. MAURO: But the general air

samples are going on continuously.

MR. STIVER: Right.

DR. MAURO: So it's an integrated. In other words --

MR. STIVER: Yes.

DR. MAURO: -- the general air sample, wherever it happens to be located is just going on continuously and you pull another one.

MR. STIVER: This is where our samples are located. They're really capturing what --

DR. MAURO: Except your concern is that if they're not in the right place.

MR. STIVER: Yes, if they're not in the right place you may not capture them.

DR. MAKHIJANI: And besides that, John, the problem here is that these are like episodic light exposures.

MR. STIVER: Exactly.

DR. MAKHIJANI: They have dust

loading all the time, but you will have dust loading leaking. Actually in that memo, I think it actually says outside air. So you have fugitive emissions that are creating environmental concentrations that are pretty high, according to their own description as late as 1970.

MR. STIVER: Yes. That really gets to --

MEMBER ZIEMER: I wasn't going to ask about this, but just a general question because I have to hit the road. I do have an appointment in Indianapolis at 6:30. So I've got to make that.

MR. STIVER: I'll try to get through this.

MEMBER ZIEMER: Well, I'm going to leave.

MR. STIVER: Oh.

(Laughter.)

MEMBER ZIEMER: I'm going to leave

in three minutes, and I can read the rest of these, but I assume that we're going to get a NIOSH response.

MR. ROLFES: Yes, we've got all of this information.

MEMBER ZIEMER: And we've just all got to read it as well.

MR. STIVER: Yes, we left it in the room for you guys to --

MR. ROLFES: Yes, right now we had hoped to have our responses prepared in time for this meeting and haven't had the opportunity to complete them yet. I know we've received all of this information and we're considering it right now first off. I don't know if we need to go through all of it here on the record, but I don't think we have time. I think it will be appropriate to do it next time around.

MEMBER ZIEMER: I think it would be better to have your response in writing before. It is more helpful.

CHAIRMAN CLAWSON: So we know where we're at.

MEMBER ZIEMER: And I assume that's the path forward on this, and I just wanted to get that on the record, but I am going to have to leave.

CHAIRMAN CLAWSON: Yes, that's fine. Thank you. I appreciate your coming.

Okay. Go ahead and continue.

MR. STIVER: Moving along here, Finding 8 relates to there are certain years in plants for which the time line has no report, and so there are not too many gaps for most of the plants and years in the thorium time line. There are reports available. Probably the one example here, at least as of March in 2009 is last year.

The pilot plant, there are four years of missing data. Now, Mark also indicated that you guys have transcribed a lot more of this DWE data. It just hasn't been posted yet.

MR. ROLFES: Correct.

MR. STIVER: So it may very well include the pilot plant. If it does not, there is limited data for the pilot plant on either side of that gap. There is also some of this underlying air sampling. So it might be a good test case for this, you know, applying a process whereby you can apply surrogate data, you know, within a plant or from another plant with similar processes going on. You know, if that is the case, that might provide a good pilot study as a good testing source.

I might also indicate that our report was in error in a couple of spots. We listed Plant 6 in 59 and Plant 8 in 56 as being pertinent but not having data, and it turns out that that was a carryover from a previous version that did not get scrubbed at the final draft. So my apologies for including that.

And the finding that emulates to the thorium time line itself, now, it's a vast improvement over what was in the Site Profile. However, the document itself indicates that it's based on fairly limited resources and subject matter experts' recollections. So there's some uncertainty remaining as to whether it's accurate and complete or whether it could ever be made so.

So this is a source that may or may not be a subject that could be resolved. I don't know the extent to which it is complete. It seems to be fairly complete from what I've read in the underlying references. It's probably as good as it's going to get, but we don't know that for certain.

DR. MAKHIJANI: To my memory, John, we did identify one or two gaps in that.

MR. STIVER: Actually that turned

out to --

DR. MAKHIJANI: That was not a gap.

MR. STIVER: It was not a gap.

DR. MAKHIJANI: It has been fixed since I looked at it.

MR. STIVER: Our Findings 10 through 13 relate to the NIOSH's proposed facility distribution model, and these findings gave us some problems. Let's take a look at this first one.

This is kind of a generalized finding. The NIOSH approach to building a single distribution of air concentration; it doesn't appear to be statistically valid using the DWE data to the extent that it is transparent.

If you go over to column 4, there is some italicized information that was taken directly out of March 2009. This is really equation 1 here, the fundamental statistical unit for the distribution. The job average is going to be the value upon which this whole distribution is built, and this Y bar I represents the job averages, our interpretation of this, where I is equal to 1 to the N jobs in the facility and Yi1 and Yi2 up to Yi10 are presumably the task averages -- in this case, there would have been ten of them -- divided by the number eight, where eight is the assumed number of operations that contribute significantly as a job exposure.

Now, this development breaks across the page. Let's take a look at the next page, equation 2 and 3, and then based on this group of job averages, they then create a facility distribution, which is Y double bar, which is just the sum of all of these job averages divided by the other number. So you have now a mean value, a mean of a group of pseudo-averages, I suppose is the only way to put it, which is weighted by the number of jobs, and then they introduce an estimator of the standard of variance, which can then theoretically be used to get the non-log-normal parameters and build the facility distribution.

We have some problems with this on a number of levels, and we'll get into those here in the next finding, Finding 11. There's apparently two pieces of critical information that were available in the DWE reports that were not used, and that being the time weighting and the number of tasks per job.

And remember before I said time weighting is really that linchpin that ties the exposure potential to the air concentration in the facility at a given time and place, and without that you really are kind of drifting around. You've lost that connection between exposure potential and the air concentration.

And the number of tasks is really

also a variable associated with any given job on DWE. It's variable and it has to be included as such in the development of a model.

So actually when you look at Finding 5, you've lost three pieces of information, two of which were available and weren't used and one which has yet to be determined, which is the uncertainty or not the uncertainty -- well, I guess there is no other -- the variability within the data set.

So you've lost three things. You've lost the time weighting. You've lost the number of tasks and you don't have a true estimate of variability within each job.

And then you have this number eight, eight tasks. We were kind of left scratching our heads over the number eight because Morris 2009 states that typically the fire operations contributed significantly to the exposure. So we're wondering why they chose eight.

I understand why you guys did this is to kind of normalize things where we didn't have to build all of these different distributions and you could just kind of give some normalized value that's fairly representative and then build the distribution from that, and it would be a little more straightforward a method.

However, we feel that the resulting value isn't really interpretable in statistical terms. It's not an average of all the data that went into creating the task averages or the values for that job. It's not a weighted average. It's just a sum of average values divided by the number eight.

And so we're kind of left at the point of not being able to apply the rigorous discussion of the statistics that arise from this other than if you look at it in an empirical way, and if you go on to Finding 13, it is probably the last thing related to that particular -- well, it's not. This relates to the focus on the distribution of job categories and not the actual number of workers in the job. It's kind of this underlying assumption here that personnel are kind of equally apportioned among different job categories for the facility.

And we demonstrated mathematical notation, but on page 31 of the report what they should be looking at is the number of workers in the category, not the categories themselves.

Let me go back to an example in our Attachment 1 from 1955. Let me go ahead and pull that back up here.

MR. ROLFES: John, if I could ask just a quick question, since we have people from ORAU on the team, I know that we've received this report from you. We're currently preparing responses to that. I think in the interest of time, I think we don't want to go through all of these because we're not going to have the opportunity to respond today.

MR. STIVER: Okay.

MR. ROLFES: I don't know if it would be best to wait until next time so that we can get the discussion it needs.

MR. STIVER: Okay. I can do it pretty quickly. I don't have too much more left on it.

MR. ROLFES: Well, before you start that, I'd like to ask Bob Morris if he has anything that he would like to provide an update in case we don't have time at the end of your presentation.

Bob, could you possibly give us an update as to the status of our responses and maybe when we might be able to have those to the Advisory Board?

MR. MORRIS: Yes. Can you hear

me?

MR. ROLFES: Yes, Bob.

MR. MORRIS: We're working on it right now, and internal review is going to take some time after it's done. So it's probably a month out.

MR. ROLFES: A month? Okay.

MR. STIVER: Well, let me just do some quick finishing statements here.

This shows the distribution of Plant 1, `55. You've got these workers. There's 16 up on the high end and basically if you look up here, it's Table 1-1. NIOSH had two categories that were in the high exposure potential, and if you come down here, you see that if you use two out of 12, you see 17 percent were in the high exposure category.

Well, it turns out there's actually 16 workers. So you're looking at about 57 percent actually in those high categories.

So using job weighting as opposed to worker weighting can distort the low end in trying to assess the intake.

Let's see. Related to that was the idea of the questionable use of the log-normal distribution, and we see this is clearly not log-normal. This is bimodal, and if you look at the score plot here, it shows the same thing.

And so we were kind of questioning the idea of using a log-normal distribution when we've shown that it wasn't applicable in 50 percent of the data sets which comprised 82 percent of the workers involved and didn't really confer any advantage in claimant favorability over the empirical distribution.

And so that is the end.

Finding 7, this finding really relates to this whole concept of using distribution of weighted averages. Even if you could develop a statistically defensible facility distribution of averages, assigning various categories based on exposure potential is not going to capture the highest exposure, exposed individuals within that.

And this is what we saw in the very first finding, even within one category. If you assign somebody to that category, you're not going to give them the highest potential.

And we went and constructed empirical distributions for each of these plants, and in every case the 95th percentile in empirical distribution missed the average, not the highest within that average, but the highest average. So assigning somebody to the 95th percentile if you don't know anything about the job and they happen to be in the high category, it's not even the average. It's way below the average.

So this is a real problem when it comes to reconstructing bounding doses under

Part 83.

I won't go into all of the details here. It's just a listing of different things that were problematic.

Finding 19 is essentially the same as Finding 7. The subjects are pretty much the same as seven.

Fourteen to 16 related to how you go about building the defensible job-based distribution, and coming back up here, Finding 7, we feel that some approach that's based on the actual job distribution appears to be essential, and this is even in cases where the data workers filed are not providing new information in this regard.

And so these other findings related to building statistically defensible distribution, we still feel that NIOSH has the basic approach in place. I mean, there's not a method by which you can generate the distributions for the tasks. What they don't have is a way to combine those, and we demonstrated also in this example how you can go about using the method that NIOSH proposes for generating the task distributions. So we have used their method, took the number of samples, sampled that particular distribution for the task, 96 of them, and then plotted the average that we got from that and compared it to task coverage to what was reported in the DWE reports, and you see there's very good consistency there.

So I mean, in this case we believe the log-normal is probably applicable for the task distributions.

We then, using a crystal ball, we had generated the DWEs for each based on each -- we combined them and we followed technique, and these are the reported means. You can compare that. Up here you see that they're fairly consistent. I could split the screen, but I don't think we really need to at this point.

But for each of those we have summary statistics based on the Monte Carlo, and so you can see that the highest exposure category, the mean and the upper bounds is about -- the 95th percentile is about 4,700 as opposed to an average of close to 1,500.

And I can go back here. I don't want to take up all the time.

So we have demonstrated this is a tractable problem to be solved in a number of different ways. We also reported an analytical approach in our review of the Mallinckrodt Chemical Works petition in 2005. Supposedly these are just methodologies whereby a job exposure distribution can be developed.

Finding 15 relates to trying to locate the source data, and we feel that any distribution that's based on actual source data by virtue of a reduction in the uncertainty would be preferable and more defensible, and we have already discussed that.

Finding 16 is another aspect. Fifteen and 16 kind of relate to each other. If you don't have the data, then, you know, this log-normal approach is probably adequate.

Seventeen and 20 are really Site Profile issues that aren't really pertinent to this discussion. One is related to the ingestion model. This is TIB-0009 -- and we've discussed that many times in the past.

Finding 20 is related to construction trade workers. TIB-0052 is invoked where it's actually for bioassay data. It's not really applicable to that situation.

Let me go back to the presentation here.

In looking forward, we feel that the data could be improved by some of the things here. Obviously the newly transcribed

DWE data should be posted. A search should be conducted to locate the source data, documentation to allow for off-normal exposures, and documentation of fugitive emissions to really identify where surrogate data may be required. All of this is you know what you have and then you can make a better determination of how surrogate data can be used.

It also did benefit from focusing on jobs, specific DWEs in the facility distributions for the reasons discussed, while establishing variability in the task, concentrations, combining those distributions into a defensible job distribution, providing a method of assigning bounding job category for those claims about a defined job.

I know it would probably be something analogous to what we already attempted to do with the three categories, groupings of like exposures, the highest distribution or the assignment of the 95th percentile in that distribution, we feel that would be the best approach.

And then find a way of ensuring the alpha air data that could be used in the DWE relative to all cases. This relates to the uncertainty for all the different aspects that we went through before.

And finally, a process should be provided in the White Paper and a methodology for the use of surrogate data, and we feel that should be consistent with the Advisory Board draft criteria that have been articulated.

And that is it for me. Hopefully I haven't gone too far beyond your time frame, and so I look forward to your responses and working that out.

MR. ROLFES: All right. We'll work it out.

(Laughter.)

MR. ROLFES: I don't know if Bob wants to say anything final about this --

MR. HINNEFELD: Mark, if he does, I'll shoot him.

(Laughter.)

MR. ROLFES: All right. We'll call the meeting closed.

CHAIRMAN CLAWSON: Hey, hey, hey.

MR. HINNEFELD: We're not closing the meeting. We're just finishing our participation in that part.

CHAIRMAN CLAWSON: Okay. And so the task on this we already discussed earlier. NIOSH is going to provide us with a response to this. That's correct, and we're looking at probably about a month out.

MR. HINNEFELD: That's our estimate today. I think it's a little difficult to say with any --

CHAIRMAN CLAWSON: Right.

MR. HINNEFELD: -- clarity today

what it will be.

MR. STIVER: Certainly by the time we have another meeting.

MS. BALDRIDGE: And whatever we can do to fast track.

CHAIRMAN CLAWSON: Yes.

MR. KATZ: So if anybody has any questions about the tasking today, raise them now because the next thing would be for SC&A and OCAS to send an e-mail confirming what their tasking is. So if you have any questions about your notes right now that you want to ask while we're still together.

MR. ROLFES: I was just going to ask a logistical thing. Maybe is it possible that we could expedite the transcript, that portion of the transcripts where there are some tasks listed or --

MR. KATZ: Well, I mean, sometimes they get those transcripts to us in less than 30 days, and certainly if I get them sooner, I can send them to all of you sooner. It's sort of very variable as to how quickly we get these, but 30 days is the outside limit.

MR. ROLFES: Okay.

DR. MAURO: I hate to do this, but does everyone here have a copy? This is a question to the folks at SC&A on the phone. Joyce and Rich both looked at the chest count, had a number of concerns. I know you sent that material to me. I read it. I have it with me. Has that material been sent to NIOSH?

DR. MAKHIJANI: It couldn't be.

DR. MAURO: It couldn't be?

DR. MAKHIJANI: It hasn't even gone through DOE yet.

DR. MAURO: Okay. Well, so we have a number of concerns with the post '68 aspect of this problem, namely where chest counting is used as opposed to breathing zone.

And it sounds like that we have an

action item here to make sure we package that and deliver it to you because we really haven't had a chance.

I see we're coming to the end. I just want to make sure everybody understands that we owe you that write-up so that you can respond to it also.

CHAIRMAN CLAWSON: And, John, is this part of this?

DR. MAURO: No, it's separate.

CHAIRMAN CLAWSON: It is separate.

DR. MAURO: Yes. I can see that, you know, the gas tank is empty, and I know that Joyce and Rich Leggett have been on the line. If they're still on the line I'm not sure.

MR. LEGGETT: We're here.

DR. LIPSZTEIN: Yes, we're here.

DR. MAURO: Okay. We're not going to go over that material today, but please fold together your material into a single integrated story that we could move through the system and get into the hands of the Board and to NIOSH as soon as possible.

DR. MAKHIJANI: Actually it's nearly there. I read it this morning, and maybe we need a little bit of an internal review and then go off.

Just to remind you, Brad, these are the in vivo data and they are '68 to '78 and '79 and '89 measured in different methods. That's what Joyce should be.

MR. KATZ: It's another White Paper.

MR. ROLFES: The method was the same, but the reporting was different.

CHAIRMAN CLAWSON: Okay. What I want to make sure is that we capture that as an item.

MR. KATZ: Yes.

DR. MAKHIJANI: They've got it as an action item.

DR. GLOVER: John, we certainly can maybe pass e-mails back and forth, and then we could send one out after we go through and read it and capture all of that.

DR. MAURO: And see what it looks like, yes.

DR. GLOVER: Yes.

CHAIRMAN CLAWSON: With that is there anything else that needs to come before the Work Group or what's left of us?

MR. KATZ: So I'm assuming we should wait a little bit before we try to schedule our next Work Group meeting.

CHAIRMAN CLAWSON: Yes.

MR. KATZ: And see how things turn out in terms of the timing of some of these.

CHAIRMAN CLAWSON: Yes. John, I ask for your help to make sure that -- because I'm sorry. This started going way over my head early on -- a copy of your action items.

DR. MAURO: Yes, I have a -- I

have -- what I did here during the course of this meeting is write down all of SC&A's action items. I didn't summarize the meeting, nor did I write down NIOSH's action items.

CHAIRMAN CLAWSON: Right.

DR. MAURO: So the next thing I am going to do is simply put a memo out that says here's my understanding of SC&A's action items. I will send that off to Mark to make sure, and you may want to add. Maybe you have a single -- you'd rather have a single package with all of the action items or should we just put ours out?

MR. KATZ: You put us in it.

DR. MAURO: We'll put it out.

MR. KATZ: OCAS will put out its own.

DR. MAURO: Okay, fine.

CHAIRMAN CLAWSON: All right. I kind of was losing track of what to write down because the first part of the meeting went a lot of different ways, but I would like to see that so that we make sure that we keep track of these and we proceed forward with them and so forth like that.

If that's it, I'll call this meeting adjourned.

(Whereupon, the above-entitled matter went off the record at 4:50 p.m.)