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

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ADVISORY BOARD ON RADIATION
AND WORKER HEALTH

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WORK GROUP ON FERNALD SITE PROFILE AND
SPECIAL EXPOSURE COHORT (SEC) PETITION

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TUESDAY,
OCTOBER 28, 2008

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The Working Group convened in the Frankfurt Board Room at the Cincinnati Airport Marriot, 2395 Progress Drive, Hebron, Kentucky, at 9:00 a.m., Bradley P. Clawson, Work Group Chair, presiding.

MEMBERS PRESENT:

BRADLEY P. CLAWSON, Chair
MARK GRIFFON
ROBERT W. PRESLEY
PHILIP SCHOFIELD
PAUL L. ZIEMER

ALSO PRESENT:

TED KATZ, Designated Federal Official
LARRY ELLIOTT, NIOSH ORAU
JENNIFER HOFF, ORAU
MARK ROLFES, NIOSH
ROBERT MORRIS, ORAU
LEO FAUST, ORAU
BRYCE RICH, ORAU
JOHN MAURO, SC&A
NICOLE BRIGGS, SC&A
HARRY CHMELYNSKI, SC&A
EMILY HOWELL, HHS
NANCY ADAMS, NIOSH Contractor
LIZ BRACKETT, HHS
SANDRA BALDRIDGE, Petitioner
ALLEN CALLAWAY, Fernald Medical Screening
RAY BEATTY, Fernald Medical Screening
STEVEN HILL, Office of Congressman Steve Chabot
JIM NETON, Fernald
ARJUN MAKHIJANI, NIOSH
HANS BEHLING, NIOSH
BOB BARTON, SC&A
BRYCE RICH, ORAU

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

(9:01 a.m.)

MR. KATZ: This is Ted Katz. I am the DFO for the Advisory Board on Radiation Worker Health, and this is the Fernald Working Group. We are about to get started.

The first thing we are going to do is take roll, beginning with the Board members, beginning in the room. I am going to try to not leave anyone out this time, like yesterday. So, Brad, identify yourselves, please.

CHAIR CLAWSON: Okay. Brad Clawson. I am the Chair of the work group for Fernald. I am not conflicted.

MEMBER GRIFFON: Mark Griffon, a member of the work group, not conflicted.

MEMBER ZIEMER: Paul Ziemer, member of the work group, and not conflicted.

MEMBER PRESLEY: Bob Presley, member of the work group; not conflicted.

MR. KATZ: And on the telephone, do

we have Mr. Schofield? No? Okay, not present. It is early. It's fairly early right now.

Okay. Then starting in the room with the NIOSH ORAU team.

MR. ELLIOTT: Larry Elliott, director of ORAU; not conflicted.

MS. HOFF: Jennifer Hoff, ORAU team; not conflicted with Fernald.

MR. ROLFES: Mark Rolfes, NIOSH health Physicist. No conflict of interest.

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

MR. KATZ: How about on the telephone, NIOSH ORAU team?

MR. FAUST: Leo Faust, ORAU team. Not conflicted.

MR. RICH: Bryce Rich, ORAU team. Not conflicted.

MR. KATZ: Okay. That sounds like that's it for the NIOSH ORAU team. How about SC&A, starting in the room.

MR. MAURO: John Mauro, SC&A. Not conflicted.

MR. KATZ: And on the telephone?

MS. BRIGGS: Nicole Briggs. No conflict.

MR. CHMELYNSKI: Harry Chmelynski; no conflict.

MR. MORRIS: Arjun and Hans Behling will be joining us. I think they believed the meeting was starting at 9:30, unfortunately. So they may not be poking in until then.

MR. KATZ: Okay. Then let's go around, starting in the room -- well, federal employees first, in the room.

MS. HOWELL: Emily Howell, HHS. No conflict.

MS. ADAMS: Nancy Adams, NIOSH contractor. No conflict.

MR. KATZ: And on the telephone, federal employees.

MS. AL-NABUSI: Isaf Al-Nabusi, DOE. No conflict.

MS. BRACKETT: Liz Brackett with HHS. No conflict.

MR. KATZ: Okay. Then members of the public and petitioners in the room, please.

MS. BALDRIDGE: Sandra Baldridge, petitioner.

MR. CALLAWAY: Allen Callaway, Fernald Medical Screening.

MR. BEATTY: Ray Beatty for Fernald Medical Screening, assist the petitioner.

MR. HILL: Steven Hill, Congressman Chabot's Office.

MR. KATZ: Thank you. And any Congressional representatives on the telephone? Okay then.

Has anybody from SC&A joined us? Arjun? Hans? Okay, I think that covers it.

Just phone etiquette, please: For the folks on the phone, please use your mute button or your Star-6 except when you are speaking, please, so it doesn't interfere; and

if you disconnect with the phone, please actually hang up. Don't put us on hold, because that will also interfere with the call. Thanks.

Brad, it's all yours.

CHAIR CLAWSON: First of all, I would like to start out by thanking everybody for coming here for the Fernald Work Group.

One of the things I want to make -- especially since we have some people, the petitioner and so forth, we do have some material but, unfortunately, there was not enough time to be able to run it through the Privacy Act. So that material we will have to keep here, but once it is cleared, we will be able to forward it on. It is kind of the matrix and so forth that we are working toward.

We've got -- John has done a really good job. Our matrix has gotten very, very thick. So in being able to have something that we can handle with and work with, he has

reduced it down to a smaller one with the main tasks; but the big matrix is still tracking each one of the places where we have been, what we have done, how we have corrected it and so forth like that.

So with that, I will turn it over to John.

MR. MAURO: Thank you. Morning, everyone.

Our last meeting was about six months ago back in -- I guess it was March. So I thought it might be a good idea, given the amount of time that has passed, just to sort of set the table a little bit.

In the interim, I took over the leadership role of the Fernald work, and what I did to get ready for today is I gathered up the last versions of the matrices that were available. Turns out there were a couple of versions at the time.

I believe, Mark, you had one. Brad, I think you might have had one, and they

were very similar, but I sort of collapsed them together. Didn't take anything out, just merged them together.

What I did then is said, okay, that sort of brought me up to date up to the last meeting, October 2007, and the material up to but prior to the March meeting that we had in 2008.

So what I did was I said, okay, I am going to take that material, and I will bring it up to date by incorporating into it the last work group meeting material, and I would prepare that matrix and I would distribute it to everyone. I am assuming everyone has a copy of the memo that I sent out dated October 14th. I assume you have it either electronically or in hard copy.

In effect, what I did here was something a little different. I hope everyone is okay with that. In our previous matrices, if you recall, we had like a series of columns, vertical columns, and I found it

difficult to work with that.

So what I did was I made a bit of a change to the format, that we can stay with or we could change. It is really your choice, and if you would -- just by way of brief explanation of what was done, go to after the cover memo, you will see that right at the very top of page 2 of 39, there is the SC&A finding.

SC&A's findings, as you know, are all contained in a great deal of detail in our report dated may 2007. So this is where everything sort of begins from SC&A's perspective.

What I did was I said, okay, I took each finding, gave it a major heading, and right underneath the bold heading, SC&A finding 4.1. By the way, there really are five findings. There's 4.1 through 4.5.

Each finding has a number of sub-findings. So they are clustered, and that turns out to be a convenience that I am going

to take advantage of today by starting in the cluster.

For example, the first item really deals with uranium, and all the issues surrounding the bioassay sampling, the milligram per liter issue, and enrichment and recycled uranium.

So it turns out, I would like to start in the general and then make a specific for each issue. Anyway, to get back to the format, this format -- I took it basically from lessons learned from our procedures meetings, where we identified the issue. In this case, it is issue 4.1-1, and it is described.

You may have noticed that I describe it in a little bit more detail than it was in the original matrix, so that we know a little bit more about what that particular sub-issue is about. So it helps.

Immediately below that, you will see a row called "Draft NIOSH Response." This

is the original response to our issue that was provided, and it goes way back.

And below that, I have extracted from the previous work group meetings. On 10/27/07 we had a work group meeting, about a year ago, and this comes directly from that. And all that is really here is just the action items. It lists, okay, the work group said NIOSH would like you to do action items 1 through 7.

The next row below that is a summary of NIOSH's response to those seven items, and in large respect there is either response to it or it makes reference to something that was placed on the O: drive.

That effectively -- now just notice, we are just talking about the very first sub-issue, issue 4.1. Flip to the next page, and there is a little bit more supplementary material related to NIOSH's response to directives given on 10/24/07.

At that point, at the top of the

second page where you see 5 and 7, it says there, that sort of is the end of the process that took place up to and prior to the March 26, 2008 meeting.

Now starting with the next row, which is labeled March 26, 2008 Work Group Meeting, that is the material I prepared. Now I did something a little different here, because I felt I needed to do it for me, and I suspect it might be helpful to you also.

For this particular issue there was quite a bit of discussion held during the meeting, and so I tried as best I could to capture it. There may have been 50 pages. The transcript was over 300 pages, and there may have been as much as 50 pages dedicated to just this subject. As I said, I wanted to make sure that we captured it.

So this is something you haven't done in the past, tried to capture the essence of what was discussed. Usually, we limit our matrices to just action items and O: drives.

But I felt that it was important to me to set the stage, and I left it in. Especially since we haven't met in six months, I thought you might find this useful.

Then on the very bottom -- and this is going to be for every one of these issues that we talk about, in bold is what I -- again, this comes right out of the matrix. There is nothing here that is my -- in other words, there is nothing you are looking at here that is SC&A's opinion materia. This is material that just tries to capture what was in the matrix.

Now my plan would be, by the way, that below on this page 2 here -- below, we are going to start a new row, and it is going to be called October 28, 2008 Meeting, and just keep it rolling.

So that sort of sets the table on how -- if everyone is comfortable with that, that would be my plan.

MR. MORRIS: Excuse me, John. If

there are questions or opinions that would be different from your assessment of the 50 pages?

MR. MAURO: Absolutely. In fact, when I sent this out, the cover memo dated October 14th -- so it wasn't that long ago -- I indicated please, if I did not faithfully capture -- because when you read the transcripts, especially the version I got -- I don't know who provided it. Ted, you provided it to me -- it was not official yet. It was still crude, and I'm treating it as a control document, since it is not -- in fact, you know, I had it at home, but I just used it for my purposes.

I did the best I can to read it. And so, yes, anything in here that you feel is a misrepresentation, incomplete, missing something important, absolutely, let me know. And then mechanistically -- this is really maybe one of the things Mark is concerned about. We spend more time about form than

substance. Mechanistically -- and correctly so. I mean, I know on the procedures work, we spent a lot of time getting our procedures together.

MEMBER ZIEMER: Nonetheless, I think it is important that we don't let things slip through the crack, and this will help us track things.

It looks like you are moving toward something that looks very much like what the Procedures Work Group is doing in terms of capturing the results of each meeting and the responses back and forth. It is probably too late to convert the system to that.

MR. MAURO: I am just doing that. Quite frankly, I realize there may be -- the day may come when you would want to do that, but right now I have to say I found it impossible to work with the columns and the other approach and still capture, do the things that I felt needed to be done by way of telling the story.

I mean, the columns would go down for pages.

MR. ROLFES: This does look a lot better, I think.

CHAIR CLAWSON: Well, and something else, too. When asked a question, I guess John talking about this, we will be able to go back through. It will make it a cleaner way to be able to work our way back.

MEMBER ZIEMER: Well, this is what the Procedures Work Group has done. John is very familiar with that, because he helped develop that system also, and it does something similar to this. You can get the big picture with an overview or you can dig in and get the meeting-by-meeting process and progress.

MR. MAURO: The time sequence is tracked vertically down the page here.

Okay. With that, what I did was, realizing the sizes of this, I was starting to see, well, this is a useful archive document.

In other words, this in effect represents the history of the work group meetings.

I said, but for the purpose of this meeting, it might be helpful to try to boil things down a little further, which is what I did, and I've made some copies of this. No one has seen this before. It has not been PA cleared. Neither has this. No one has what I am about to hand out.

I have 10 copies of these. These may not be enough, but perhaps you can share. I'll hold onto one myself.

MEMBER GRIFFON: You don't have that electronically?

MR. MAURO: I do. I have it on the stick, in fact. I was hoping to project it.

MEMBER GRIFFON: Can you pass the stick around.

MR. MAURO: Yes, sure. You guys got it. Maybe you can load it and then transfer it to everybody. That might be a way.

MR. KATZ: For people on the phone, we are just getting some materials ready, but let me just acknowledge. Jim Neton has joined us. Jim, do you have something?

MR. NETON: I am conflicted at Fernald.

MR. KATZ: Conflicted at Fernald. And why don't I just check to see if Arjun or Hans has had a chance to join us yet.

MR. BEHLING: This is Hans, and I have joined you.

MR. KATZ: Okay, welcome, Hans.

MR. BEHLING: Good morning.

MR. KATZ: And how about Phil Schofield? Phil, have you joined us? I'm sorry, Hans, can you just address, are you conflicted?

MR. BEHLING: No, I'm not conflicted.

MR. KATZ: Thank you.

MR. MAKHIJANI: Hi, can you hear me? This is Arjun.

MR. KATZ: Arjun, welcome.

MR. MAKHIJANI: Thank you.

MR. KATZ: And can you just address whether you are conflicted?

MR. MAKHIJANI: Yes, I am conflicted, yes.

MR. KATZ: Thank you.

MR. MAURO: Okay. Mark Griffon has --

MEMBER GRIFFON: I don't see anything prior to this that is related on this document. I am just saying, if you wanted to share it, I think you can share it.

MR. KATZ: Let Emily do that then.

MR. MAURO: Mark has it, and is going to electronically forward it, so you will have it. This again is for the convenience of this meeting.

I would like to start -- first of all, I would like to point out that the way in which the tasks, the issues, are grouped are by groups of five. You will see on the very

first page of this action item matrix I have issue 4.1, your analysis data for uranium, and underneath that all of the subparts.

As you flip through, you will see next is 4.2, which has to do with K-65. So what I would like to do is go through each of these. First, I would like to talk about this first cluster called urine data -- urinalysis data for uranium, and sort of paint the big picture of where I think we are, and then we can move into the finer granularity on various action items and actions taken related to that subject.

Stepping back and going through the history, I went through both transcripts. First of all, let me tell you how important it is to go through the transcripts, especially with a lot of time passing by, and to sort of get everything that was discussed.

With regard to uranium, a good way to think about it is, to start off, the heart of the internal dose reconstruction for all

workers at Fernald for internal exposure is the milligram per liter measurements of uranium in urine.

Going from the general to the specific, one of the most fundamental questions -- points is that NIOSH's position is that over 90 percent of the workers throughout the history of Fernald had such measurements, at least one per year. That's very important, because that becomes the rock you are standing on.

Now one of the things that came up was, well, it's important that we confirm that. That is, confirm it with respect to -- though you have 90 percent, is it possible there might be some group or groups of people at different locations, different categories, different time periods where there is a paucity of data and, therefore, there might be some holes.

So in order to make sure that it is complete, one of the things that SC&A was

mandated to do during the last work group meeting was to go in and sample the database, the HIS-20 database, to determine if there are, in fact, any holes in the data, you know, the completeness question.

Even though that is one of the more recent things that we did -- we are going to get to other ones -- I felt that that was -- in the order and way in which you think about a problem on a hierarchy, that was like -- that's a big ticket item. That is, if we could get by that, then the other things can be addressed a little more easily.

So what I have is another handout that talks about where SC&A is in answering that question, because we think that that question, completeness, is fundamental to even moving on to talk about anything else related to internal dosimetry for uranium.

Think about it this way. When I mentioned uranium, I am really talking about unenriched, enriched, and recycled uranium and

the ability to reconstruct doses. As you will see, the heart of it starts with having a complete database on milligrams per liter in the urine.

So I have another handout. Again, this has not been PA cleared, and I'll tell you what it is, once everybody gets a copy. It's on there, too. Everything is on the stick.

It was my intention, by the way, to project all this on the wall, but we can't do that. So we'll work with hard copy or electronic copy.

SC&A prepared a sampling plan that was approved relatively recently. I would say a week ago, and we have been working on it. But I think it is important to understand what the sampling plan is and what it is going to give you, and how it will help the work group make judgments pertaining to the completeness of the database.

I have a three-page handout that I

believe everyone should have right now.

The first page is what I call the -- and you have seen this before in the little form. You know, the sampling plan was distributed to the work group, and it was a big statistical description of how things are going to be done, and some tables that look a lot like this. But I repackaged it for the purposes of this meeting and put it into what I consider to be something a little bit more understandable for me, and took away some of the -- lots of the statistical descriptions, and boiled it down to what I would say common sense language and what is it we are doing.

We basically -- using the collective judgment of SC&A, we said, listen, if you were to sort of parse out operations at Fernald over time and over different kinds of facilities and in different types of work categories, if you could demonstrate that the records are fairly complete for each of these strata, for example, the statement could be

made that we sampled the pilot plant, the workers that were in the pilot plant or the workers that were in the Plant 1, and we grouped plants 2 and 3 -- if you can go in and sample it and go grab people -- now these are people that we sampling -- that worked there from 1954 to 1967, and we grabbed the records of those people who worked in the plant --

MR. MAKHIJANI: John, can I interrupt?

MR. MAURO: Sure.

MR. MAKHIJANI: the dates are stated erroneously there as '54 to '67. It should be '51 to '67.

MR. MAURO: Oh, okay. I was wondering about that difference.

MR. MAKHIJANI: Yes, except for plant 750. The dates got mixed up.

MEMBER ZIEMER: I think what happened is you changed the order of the plants from your previous one, but not the order of the dates.

MR. MAURO: Ah, there you go.

MEMBER ZIEMER: As well as the periods.

MR. MAURO: Okay, thanks, Arjun. I'm sorry for that mistake.

So over that time period -- and by the way, I am going to -- as a preface, this is our plan. Now when you go into the -- we are into the database now. I have been talking to the folks who have been diving into this massive amount of material, and our objective is to do this. But based on our previous experience, sometimes it has to evolve a little bit because of certain constraints. But this is basically what we are trying to accomplish.

So we go into -- as I was point out, let's say we go into -- let's say we are able to, and we believe we are able to, go into identify workers, claimants. Okay? Claimants that worked in Plant 1 in 1954 through '67, and we say, okay, here is our

population of workers.

We are going to go in and randomly sample from those claimants, and then we are going to see how many of them have -- each year, how many bioassay samples for each worker for each year in that time period.

So we've constructed the strata this way, because we feel that these are -- different facilities, different job categories and different time periods have meaning in terms of things being different.

So if you are able to capture a representative sample or if we get a good feel for the completeness of the data in each one of these strata, we would be able to walk away and make a statement regarding the completeness of the overall dataset to support those reconstructions.

Let's go to the second page.

MEMBER ZIEMER: A quick question. I notice the millwright category has disappeared in here in your data. Is there a

reason for that?

MR. MAURO: Good question. I don't know the answer. I will have to go back and check that out. It may have been an error on my part, as I did with the dates when I prepared this or maybe it was determined that that would collapse within one of the other categories. I can't say. But we will certainly look into that.

CHAIR CLAWSON: John, also this other job category B looks like we've got an awful lot tied up into that one.

MR. MAURO: That's correct. That's where we are right now. We have that. In fact, in the process of preparing this work plan, we had a little bit of interaction not only with the Board but also at NIOSH, and this is the outcome.

We would be the first to admit that, once we move through this process, when we come out of the back end of the process and have the results, there will probably be a lot

of questions like that; and we will know the doability, where things really can't be done and where more needs to be done, but we've got to start somewhere. So you're right.

CHAIR CLAWSON: But the only thing that I was bringing up, John -- and this is Brad -- is, you know, I guess my big one that I was really worried about was labor pool. But looking at all of these, they are all pretty well roving people that rove from place to place, etcetera, except the laundry.

MR. MAURO: Okay.

CHAIR CLAWSON: And that was -- You know, I can understand how come they were in that group, because they would go throughout this facility, different places all the time, except it seems like the laundry kind of sat in one place.

So I was just looking at that, but you know, I don't know. Maybe they went out and picked up stuff or something else.

MR. MAURO: No. The feedback you

are giving me now is what we need, because we are doing the work now, and if we can deal with those issues as we enter into the process, we will.

MR. BEATTY: Excuse me, John, for interrupting. This is Ray Beatty. You mentioned the laundry workers being stationary somewhat.

CHAIRMAN CLAWSON; Yes, that it what I was just assuming.

MR. BEATTY: For a point of clarification, in the later years, especially in remediation years, they were assigned out in the project as well to go around picking up the laundry at what we call satellite stations. They did not just stay in a laundry room.

CHAIR CLAWSON: Yes. I just -- What I was looking at --

MR. BEATTY: The potential exposure would have been --

CHAIR CLAWSON: Okay. So that

answers that question.

MEMBER PRESLEY; This is Bob Presley. At other sites, you generally had a truck driver and somebody from the laundry that would go around on a periodic schedule and pick up laundry. Yes, there is a potential, very much so, because some of the dirtiest things are in the laundry.

CHAIR CLAWSON: So that may take in and incorporate all of those into that.

MR. MAURO: It might. That doesn't mean that we don't need more granularity. In fact, we were in the situation in one of those where you had to make tradeoffs. How granular do you go? It is basically our estimate that it is going to be one or two work hours per person, and the more we add, the more granularity, the longer it is going to take.

So we thought that we ought to strike a balance where we get some meaningful information. That doesn't mean we won't go back in again, if we need more granularity.

The important point, though, is that -- remember where we are right now. We are talking about the issue number one, and we are saying that one of the most important things we need to accomplish under issue number one, which deals with bioassay sampling of uranium, is its completeness, because that goes really much to the heart of any SEC issue.

What we are implementing as we speak is a review of the dataset for completeness purposes, and this is the way we are doing it, by creating this matrix of -- or strata of these categories of plants, job categories and time periods.

Now what Harry Chmelynski, who is on the line -- correct me if I'm wrong. We are one page number 2. What I have prepared on page number 2 -- this might have also been contained in our original work plan, but in different form, but I have boiled it down to a little simpler form for my purposes, is that,

okay, what does that mean, we are going to go sample from each one of those strata? How many people are you going to sample?

Well, it turns out that right now our plan is for -- By the way, we added the pilot plant. In the previous work plan, we did not have the pilot plant. When we started work on this, we all looked at each other and said, how come the pilot plant isn't in there? So we put it in.

So if you go back to the original plan, you will see that the pilot plant wasn't in there, and we judged that we had better put that in.

MEMBER ZIEMER: It's in.

MR. MAURO: I don't think it was in the original one.

MEMBER ZIEMER: Well, maybe there was an earlier version of this.

MR. MAURO: Well, the pilot plant is in now. It's covered. Okay.

MEMBER ZIEMER: This is the August

version, and it was in there.

MR. MAURO: And it was in there then. Well, there may have been some back and forth. But what you are looking at right now is what we are doing right now. That doesn't mean we don't need to change it, fix it, make it better, simplify it, but --

Now it turns out that -- Let's talk about ID number 2, which is Plant Number 1 during time period number 1, which is 1951 to '67. We are only going to sample three people. We are going to go in and pull all the records for a randomly selected three claimants.

In other words, we are going to have a list of claimants that we are going to do the best we can to sort according to these strata, and there will be presumably a lot of workers that were in Plant 1 in that time period, claimants. Then we are going to sample them.

Then we are going to make -- From

that, we have our folks then go into their records, year after year after year, and look at their records, and we are going to download what bioassay sample, but also other data. So it's not just urine data, but it is basically what are the data available for those people, and create a database.

Now so in effect, what we are saying is these are the number of people we are going to sample. I think it comes to about -- if you add them all up, it comes to about 150 or something like that. So in effect, about 150 people are going to be sampled, and they are going to be parsed into these different boxes.

Now -- and we are going to download all of the bioassay and other data pertinent to that person by year, and then create this database.

In the end, most importantly -- the next page is the results. What this basically says is, when we are done, we are going to be

able to say for -- the best way I think about it is we are going to be able to make a statement associated with the numbers.

We are 95 percent confident that at least 20 percent, 30 percent, 40 percent, whatever the percent is, of the workers in that strata have at least one bioassay sample per year, at least one milligram per liter measurement per year.

So that we could say what percentage. It may turn out it's 90 percent. We may be able to make a statement. It depends what the results come back when we do our sampling. So we will be able to make a statistical statement regarding each strata on the level of confidence that at least X percent of the people in that strata had at least one, two, three bioassay samples.

Now from that -- Now if it turns out the number is very high -- let's say we could say that we are 90 percent certain that 80 percent of the workers have at least one

sample. Well, I mean, intuitively that ain't bad, and you probably can not only reconstruct the doses for those workers, the ones that have the samples, and the ones that perhaps may not have it, which based on that sample would be not that many, you could build a co-worker model.

The co-worker model, of course, is a judgment call. For any given person that doesn't have data, you could use 90 percent value or 50 percent value, the full distribution. These are judgment calls that we consider to be non-SEC issues.

The SEC issue is whether or not that particular strata has sufficient data to build a co-worker model or not. So this is the philosophy we are operating under.

MR. NETON: I just have a general question. This is Jim Neton.

The general concept seems to be that you are approaching it that all of these strata were required to be monitored in the

first place. What happens if you get to the point where you have an administrative group that, by your determination, does not have sufficient bioassays to develop a co-worker model?

What does that really mean at the end of the day? But a judgment call has to be made, whether those people really required to be monitored. There is a gradation of monitoring in all these work groups. I can guarantee that.

So you've spent a lot of time here showing these strata, but really, it seems like it should be front end loaded and say which groups really needed to be monitored that had the highest exposure, so you can have some sort of a valid co-worker bounding model.

Where this is pushing it is to have something like 15 different co-worker models. I'm not sure if -- that's just my opinion.

MR. MAURO: Well, I don't want to leave the impression that this is a co-worker

model study. What this really is is a --

MR. NETON: Well, it's what you are talking about here.

MR. MAURO: Well, no. It goes there. It could get us there. But what it really asks is the completeness question, because there is a statement made, and it is an important statement, that over 90 percent of the workers were sampled, had at least one bioassay sampling per year. It's a very important statement.

The mandate that we were given -- that was at the last meeting. Pulled it right out of the transcripts. And the mandate we were given is let's go check that, and we came up with this plan.

So when we are done, you're right, we may find one of these strata -- in fact, we may find it's difficult to get any data for a given; strata, and we don't know that.

That may mean that we have to say, okay, what do you do when, in fact, a given

strata really doesn't have any data, for whatever reason. Maybe they didn't need to be monitored, or maybe there is a problem. But at least we are going to know that. I mean, that's just first things.

We are going to know where there may be people that weren't -- didn't have bioassay data.

MR. NETON: That would come out of the dose reconstructions. I mean, you have 200 dose reconstructions. You have the job titles for all of them. You are going to have monitoring data or you don't.

Then the judgment call has to be made: This was an administrative worker. Does the co-worker model that reconstructs from all the universe of monitored workers adequately bound this particular dose? It just seems like there is a lot of effort here that I'm not sure what --

MEMBER GRIFFON: Is there a co-worker model on the table? I don't think --

There is a uranium co-worker model now?

MR. ROLFES: There is. Correct. It wasn't available in the beginning.

MR. NETON: That's my point, though. So then you are --

MR. ROLFES: It was sort of like the Rocky Flats, that we didn't have enough data to do individual dose reconstruction for everyone. Then we were testing to see if you really had enough data for all individuals.

With the Rocky Flats, it wasn't perfect, of course, but when we went through, we found certain years that were limited, but we often had explanations. So that fell out, and that was fine.

MR. NETON: Right. That is my general question. We tried to relay that in the comments.

MR. MAURO: No, no. I guess I'd look at it like a dictionary. You say, listen, this is what we have. In other words, best we can tell, this is how complete a

record we have for all these different strata.

MR. NETON: Exactly. But at the end of the day, what do you do with that? That's my point.

MR. MAURO: Right.

MR. NETON: I mean, you stop short here saying, well, we'll see what the completeness is for these 15 different categories. So what does that really mean? You're going to spend a lot of time doing that.

MR. MAURO: Well, does that mean that it shouldn't be done?

MR. NETON: I don't know.

MEMBER PRESLEY: That's what I'm wondering. Should it be done? I mean, you are going to spend a tremendous amount of time and a tremendous amount of money, and you are going to come up with an upper bounding, the level up here that is going to be tacked on for everybody, and I'm just wondering if you shouldn't go in there and pick out what you

consider or what we consider as to be the most exposed people and start there.

MEMBER GRIFFON: SC&A is not going to come up with any upper bound, first of all. That is not their job.

CHAIRMAN CLAWSON; Let's go to the statement that was said. Everybody was sampled. NIOSH, prove to us --

MR. ROLFES: As we have indicated, 90 percent or greater than 90 percent of the people at Fernald did participate in the uranium bioassay program. The people that had the highest potential for exposure, such as chemical operators, were some of the individuals who participated in the most frequent sampling program.

Some of those people were sampled as frequently as multiple times per day. Other people that were less likely to be exposed were only typically sampled on an annual basis.

We did develop a white paper on a

uranium intake model, and at the last meeting I had forgotten that we had done this, because it was roughly produced about a year ago.

So I've got an e-mail from November 7, '07, which has a copy of the white paper for Fernald or the uranium intake. I don't have access to the O: drive right at this moment. So I can't verify that it is there, but I do have a copy of it in my notes.

MR. MAKHIJANI: This is Arjun. Could I make a comment here?

You know, we are required -- you know, a completeness check is an important part of an SEC review. This is not a dose reconstruction review.

The other thing is, yes, it's true that some categories had lower exposure potential than others, but you know, for production and maintenance workers it is not a priori a given that Plant 1 had more exposure potential than Plant 2 or millwrights had more than carpenters.

What this allows you to establish, besides looking at completeness, is it allows you to establish whether there is a category of workers that you can select, a major co-worker model that you can be sure will be bounding in an SEC context.

So I think this kind of exercise is pretty useful.

MEMBER GRIFFON: Mark, can you tell us what the file name is for that white paper? I'm on the O: drive right now.

MR. ROLFES: It is FENP Urine Co-Worker Study White Paper.

MEMBER GRIFFON: Okay, here it is. It is on the O: drive.

MR. ROLFES: Should have been placed there last year sometime, November, I believe, is what the date should have been, November 2007.

MEMBER GRIFFON; I guess when I was going through the O: drive, I had noticed that there were no Excel -- Often in these co-

worker models, I look for the Excel spreadsheets that have the by year, the distribution's years, etcetera. This is just the white paper.

MR. ROLFES: Let me open it up, and check to see whether it has those tables. Yes, the 50th and 84th percentiles are incorporated within that white paper by year.

MEMBER GRIFFON: I see that.

MR. MAURO: Well, all I can say is that we are only one week into the program. We could kill it. It's your call.

CHAIRMAN CLAWSON; This keeps coming up every time.

MR. MAURO: To me, putting this to bed is going to cost 200 work hours at $100 a work hour.

MR. MORRIS: From my perspective -- This is Bob Morris, I'm sorry -- the problem in the structure of your plan is that you haven't followed the classical data quality objectives process where you actually define

the decision to be made ahead of the work; because now we are -- the reason they define the DQO process, which is that you want to have a conclusion that you know what your answer is going to be as the basis for your design, and I don't see that you -- at the end, you are wondering, well, what do we do with it.

MR. MAURO: I hear what you are saying, and I understand the DQO process, but I think, when you sample data in an array like this, which captures the universe of workers without giving any special weight or make any pre-decisions, the data then speaks to you It's the first step in the process.

Okay, what does this data tell us?

MR. MORRIS: Okay, then in this context, your 200-hour study is a preliminary study that would go on to feed a second study.

MR. MAURO: Not necessarily. It may turn out that the outcome would be just about every one of these strata -- it was

written to have a lot of data. In other words, every single strata we have cut into and we have sampled from. There is a lot of data, and it certainly is a judgment call how much is enough, but it may turn out that everyone will agree, from that dataset and that strata, you could build a co-worker model.

In other words, what we are saying is that it may turn out that everyone was sampled, but based on the sampling, the best we could do is say we are 90 percent confident -- 95 percent confident that 50 percent of the workers have at least one bioassay sample.

MR. MORRIS: Okay, but don't forget, it's not the question of you could build a co-worker model. There is a co-worker model.

MR. MAURO: Oh, okay.

MR. MORRIS: At some point you've got to say maybe you've approached it from the wrong end.

MR. MAURO: Well, I could reverse it and say how do we know your co-worker model is going to serve our purposes well?

MR. MORRIS: Fair enough.

MR. MAURO: This will do that.

MR. NETON: Well, it's always been an issue that the co-worker model assigned 95th percentile, and we have to establish then that the highest exposed workers were actually monitored and captured with that model. I think that's the approach that needs to be evaluated, not whether laundry workers were monitored more frequently than chemical operators, more frequently than work truck drivers.

I mean, to me, I don't know what that really shows you, other than a priori I can guarantee you that there is going to be a stratification of monitored frequencies in those populations. But were the highest exposed workers monitored?

What you are going to end up with

is 15 co-worker models, and you are going to end up reducing the dose.

MEMBER GRIFFON: You can still use the overall.

MR. MAURO: This is really a completeness issue. That was the driver, completeness.

MR. NETON: But, see, even if you still -- if you end up using the overall model, then you are really not proving anything other than the fact that laundry workers are less frequently monitored than chemical operators.

Have you captured the highest exposed workers in the co-worker model that NIOSH proposed to you? That's really the question.

MEMBER GRIFFON: I would definitely start there. I would say --

MR. NETON: That's a non-starter.

MEMBER GRIFFON: The question is how do you -- I mean, I think you have to use

a certain amount of judgment, but start by --

MR. NETON: But, Mark, you are going to be there anyway. You are going to have all these data, and you are going to say, gee, there's less monitoring data for people who sort of a priori appear to be less exposed. So now, let's go back and say, well, gee, where are the highest exposed -- were the people who were frequently sampled the highest exposed workers anyway?

I don't know. It would seem like you have to make an a priori judgment up front that there were certain categories of workers that were more highly exposed.

MR. MAURO: Now let's say it turns out you have decided maintenance workers during a certain time period. We are going to have that, and now we are going to have whether they were sampled. We are going to have their results. It's all going to be there.

MR. NETON: And at the end of the

day, the question is were the maintenance workers the highest exposed workers or the chemical operators the highest exposed workers You almost have to look at the values.

MR. MAURO: But, you see, that is sort of like a dictionary. I mean, it's sort of like it's all here. Then you pose those questions: Does this -- for example, let's say there are certain assumptions you made in your co-worker model. The data will say, are those assumptions you have made consistent with what we are finding in the sampling thing? That is, I guess, how I look at it.

That is one of the values of having --

MR. NETON: I don't know. You are proposing numbers, not -- I'm not sure if the sampling strategy there would reflect the values. You've got a sampling plan --

MR. MAURO: I want to drop the numbers in, too, not only say, yes, he was monitored, but what were the results.

MR. NETON: Well, I'm not sure that the statistical basis of the results would be sufficient to come to any conclusions. You build a sampling plan based on sampling frequency, which has nothing to do with --

MR. MAURO: That is correct. This is solely completeness.

MR. NETON: So what do workers mean? I don't know.

MR. MAURO: I mean, while we are there and we have the person's file in front of us, and we are counting -- We looked up the HIS-20. It's all there, right along the line. We have every measurement, every sample taken by that person on that date.

MR. NETON: I understand.

MR. MAURO: And it's all there. It was our sense that the idea of creating a completeness sampling plan and downloading the data in the form that we have it here would add value and help the work group make judgments regarding it.

MR. NETON: That is the work group's call.

MR. MAURO: And this is what we came up with, and quite frankly, we have accepted it as our own in terms of, yes, I think that by doing this, when we are done, there is going to be a lot of information here that we know is going to help us answer certain questions, and then may also help us get a richer understanding of where there may be some problems, problems that we are not aware of. Arjun, yes?

MR. MAKHIJANI: Just for clarity, at the present stage we do intend to compile the measurements as well. For those who are on the HIS-20 database, this will be very straightforward.

MEMBER ZIEMER: But my point is the sampling frequency is based on numbers of samples, not on the doses, the magnitude.

MR. MAURO: That's correct. That's correct.

MEMBER ZIEMER: So unless you are assuming -- and it may not be a bad assumption -- that the sampling frequency must be related to the potential exposures, which it probably ought to be, but you don't know that a priori, I guess, or you might want to look at that. But the question is do you have enough samples to answer the second question about doses.

MR. MAURO: Maybe not. Maybe not.

MEMBER ZIEMER: Versus simply sampling the frequency. So that would -- I think it goes to Robert's issue, is if you haven't decided how you are going to use the data, how do you know you have constructed it properly?

MR. MORRIS: Exactly.

MEMBER ZIEMER: Technically, you can only answer the first question.

MR. MAURO: I agree with you. Our objective and our design was completeness.

MR. BEHLING: Can I interrupt for a second?

MR. MAURO: Sure.

MR. BEHLING: This is Hans Behling. I sent to you yesterday by way of a fax a document that has the issue of control groups, which we have talked about personally, but I think some of the questions that have been raised in the last few minutes can be answered.

If you have that copy available, maybe during the first break you can discuss this issue and then bring it up again. But there are several pages that define various groups of individuals based on plants' location and their assignments and their recommended frequency by which they are to be monitored that measures everything from the service quarter, laundry people annually garage people annually, cafeteria annually, and then it goes to people like plant A monthly, higher plant monthly, etcetera.

It even gives you the exact numbers of people who were being requested to submit

their urine sample for analysis. If you take a look at that, there is a total of five pages. It actually identifies the dates and frequencies by these various -- for these various people, how they are to be monitored.

It may give you an understanding of the issues that were raised by Dr. Ziemer and others about how do you relate the frequency of exposure -- or the frequency of monitoring as opposed to the prospect of being exposed to higher levels?

So maybe you want to take a look at that during the break, and then come back and perhaps reorient our thinking.

MR. MAURO: Yes, Hans, I have it in my hand. I did not do anything with it, and I wasn't planning on bringing this forward during the meeting. But it sounds like there may be some value here. I'm not quite sure where it fits in, but I do have it.

MEMBER ZIEMER: Well, my initial reaction is that your plan would serve to

verify that they are doing what they say here. I don't know if it would go beyond that right now.

In other words, we already know the sampling frequency, according to that, and you would be --

MR. MAKHIJANI: But, Dr. Ziemer -- this is Arjun. I think that our work elsewhere has indicated that what is intended to be the sampling frequency was not always the sampling frequency.

MEMBER ZIEMER: Yes. That is the point I'm making. It would serve to verify that they were following that.

MR. BEHLING: And I think we have -- and Arjun would have to speak to this, but I think you have a sufficient dataset. At least when you combine the production workers together, and perhaps the maintenance workers together, we will have quite a large set of workers, and we will be able to make at least some semi-quantitative judgment about whether

the NIOSH model, co-worker model adequately represents the group with the highest exposure.

If the group with highest exposure was consistently monitored, then, of course, the co-worker model for them is entirely moot. But if they were not, then it will become a very important question, and you may have to look into whether further work is necessary. But it may not be.

MEMBER GRIFFON: I am trying to think of the history of this, too, that we -- I mean, I think initially part of the reason we wanted to look at data completeness, at least what was in my head, was the fact that -- I think it was the statements early on that most of the data reconstructions were going to be done based on individual data, and we weren't going to have to rely on a co-worker model very much.

One is in the works, I think, initially we heard. Now this has been going

on for a while.

MR. MAURO: That is correct.

MEMBER GRIFFON: But one is in the works, and we will use it, but only for a very few cases. And we had that sort of at Rocky, too. So I think part of what we wanted to make sure is -- and at the end of the day at Rocky Flats there was -- a lot of what we came down to is even the D&D workers, even though they didn't have a lot of urinalysis data, a good percentage of them, a high percentage of them had a close-out urinalysis sample.

So even if you didn't have annual, through that completeness review we found that we had enough that they could reconstruct. That was sort of my focus, was if we are going to do this individually, is the individual data adequate to support that.

It may not be an SEC procedure requirement, but it is a Board requirement that we look at this data completeness and validity issue. So that was sort of my

driver.

Now this co-worker model may -- you know, I'm still not sure -- and this is the proof of principle side of our Board procedures. I'm still not sure when, in fact, NIOSH intends on using the -- I'm just looking at it online while I'm trying to follow. So it may be in here, but I'm still not clear on when you are going to use the co-worker model.

Is it going to be if someone has no urine data. I don't know the conditions, and the other question would be the application of it. Are you going to use the 95th or the 50th, and that's important in terms of answering that question of can it be bounding for all members of the class, that sort of thing.

I guess my first driver for the completeness review was my understanding was that, for the most part, it was going to be individual DRs, not rely on a co-worker model.

MR. NETON: I haven't looked at the

SEC evaluation report in quite sometime, but usually there is a section in there that talks about the claimants that we have -- the claims that we have and how many actually have internal bioassay measurements, that sort of thing, and it's probably got us covered.

MR. ROLFES: That is right around 93 percent of the individuals.

MR. NETON: So for 93 percent of the claims we have in-house. So seven percent of the people don't have internal bioassay data. So that number is already known. We already know that seven percent of the claims, at least that we have in-house, don't have bioassay data.

So I would be surprised if this doesn't show something similar, you know. So I guess again that is my point. So if you look at the seven percent that don't have bioassay data, what are their job categories. You know, what did they do, and then how is NIOSH going to fill in those seven claims?

MEMBER GRIFFON: Out of 300 or more? I'm saying the other side. Look at the 93 percent and make sure that data is -- you know, you looked at -- that's probably based on just -- look at it and say is it adequate to do the reconstruction.

MR. NETON: That would seem to be the place to start rather than the entire population and universe of all workers that ever worked at Fernald. I don't know.

MR. MORRIS: Of all of the co-worker studies we have probably ever tackled, the data has been more abundant here than any other site. That is why I am puzzled about what is driving this.

There is a couple of hundred thousand urine samples that we were able to grab to pull this data together, and you know, it is -- the idea now of parsing it into smaller granularity to try to do anything else with it doesn't make sense to me.

MR. NETON: It would make sense if,

for instance, you could show that the chemical operators have no bioassay data, something to that effect. I mean, it would have to be almost that egregiously out of whack.

MR. MAKHIJANI: This is Arjun. You know, I think in the evaluation report it does that say that 90 percent of the workers or more have bioassay data, but it says nothing about the frequency of bioassay data.

Now, you know --

MEMBER GRIFFON: That's my point. People looking at Plant 2 were monitored once a year or once in two years. This would be quite material to your ability to reconstruct doses, because it's like the -- especially if you've got episodic exposures.

So I think -- and I think, while the statement that 90 percent of the workers were monitored may be right on the face of it, it doesn't tell you whether the frequency of monitoring of production workers was adequate, given the solubility, to do the job.

MR. NETON: Again, you've got the frequency list that Hans alluded to here just a few seconds ago, and basically you would be either verifying that they did what they said they did, and that's the whole point of that exercise.

MR. MAKHIJANI: Well, that's part of the thing, yes. There is an inkling about what was supposed to be done, and then there is a verification of that.

MR. BEHLING: This is Hans again, and I really do feel very, very strongly about John distributing that particular document that I made reference to a few minutes ago, because one of the things that I asked John to perhaps bring up at this meeting is whether or not even people who were monitored on a monthly basis, as we find here for Plants 2 and 3, Plant 4 and so forth -- and I have the numbers in front of me -- but the question is were, in fact, only a subset of those people monitored.

That is what, basically, I am beginning to believe in looking at these numbers, because the numbers given for Plants 2 and 3 for this one category only involve five individuals who were monitored on a monthly basis for the year 1982, and it is clear to me, or at least it seems clear to me, that those people do not represent the universe of the total people assigned to Plants 2 and 3.

So anyway, I think I would rather have John show you the document and perhaps make photocopies during the next break, and then perhaps we can look at some of that data and come to some conclusions regarding not only the frequency by which people were monitored, but whether or not the total universe of workers assigned to those particular locations were, in fact, monitored, or if it is only a subsample of those workers.

MR. ROLFES: Hans, this is Mark Rolfes. Were those five people monitored via

only urinalysis or did they also have full body counts or in vivo --

MR. BEHLING: No, this is strictly a urinalysis schedule. I'm looking at a urine schedule. I don't even remember where I got this document from, but it must have been part of the information that was just downloaded and was provided to me by the people who wrote the SEC petition.

MR. RICH: Can we get this document put on the O: Drive or e-mailed out?

MR. BEHLING: Well, I don't really have it, but as I say, John has that document in front of him.

MR. MAURO: Hans, I have five pages of the material that you are making reference to. I am sort of thumbing through it as you speak and trying to connect what you are saying to --

MR. BEHLING: Well, okay. Let me see.

MR. MAURO: Why don't we do it

during the break?

MR. BEHLING: It's the third page, John, that has, for instance, by plants and the total number of people and their frequency. And as I said, the document explains itself, if you just thumb through it.

MR. MAURO: Okay. I've got that in my hand. Yes.

MR. BEHLING: And I'm sure Arjun and other people from NIOSH will be able to instantly recognize what these data represent.

MR. KATZ: Sandra?

MS. BALDRIDGE: This is Sandy.

MR. KATZ: Sandra, can you just come closer to the mike, please.

MS. BALDRIDGE: I have a question. The historical documents in the petition that showed extremely high MACs and potential exposure -- were those ever correlated to see if, in fact, the workers in those locations at those times had urinalysis done? I mean, you may have plenty of records, but if it wasn't

done at the right time on the right people under the highest exposure, those records aren't going to give an accurate expression of what their actual exposure was.

MR. ROLFES: Off the top of my head, Sandra, I couldn't tell you if we went back and correlated. I apologize. I don't have the answer right now for you.

MEMBER ZIEMER: Could I ask kind of a reverse question. This is Ziemer. Maybe I'll ask -- maybe, Jim, you could help me on this.

If the SC&A approach were not used, how would the information on whether or not the proposed frequencies or the mandated frequencies were actually carried out? How would that come out in the dose reconstruction process or, in other words, if the frequency for the chemical operators was not what is stipulated, would this show up in some obvious way that you guys would say right away, oh, something is wrong here?

MEMBER GRIFFON: If you expected them to have a lot of data, and there's only a few data points or something like that.

MEMBER ZIEMER: Yes. And what would that do in terms of the co-worker model? How would the information emerge in some -- or would it emerge?

MR. NETON: I don't really think it would. I think --

MEMBER ZIEMER: I'm trying to see if there is value added in doing this or is this just something that is going to verify what would happen anyway?

MR. NETON: No. I don't think -- Mark Rolfes is closer to this than I am, but I don't think that we would end up sort of de facto demonstrating that frequency of monitoring in our co-worker model, but it is the age-old argument we've had since the beginning with this program. Were, in fact, the highest exposed workers targeted for monitoring or not, or was it a cohort sampling

or were they, in fact, under-sampling the highest exposed workers, and the people who were least exposed were monitored?

Those are sort of the three categories.

MEMBER ZIEMER: Yes. And would it show up, as you develop the model and begin to use it?

MR. ROLFES: I didn't hear your initial question. Bob just repeated it for me.

MEMBER ZIEMER: I don't know if I even stated it well, but I'm sort of trying to get an intuitive feel for what would happen. Does this add anything to the system?

I think I would like -- if there is value added in what -- if there is no value added in what SC&A does in some way, then we have to say why do it, from the Board's perspective and your own. If there is value added and something emerges that helps develop the model or --

MEMBER GRIFFON: I think what comes out of this granularity, as you discussed, is that -- I mean, I don't think anybody here is arguing that there is not a lot of data points. I think the question is, when you start to look by year and by groups that we think should have been monitored frequently, and if something falls out like for a couple of years, all of a sudden no sampling was being done, if there is not a good explanation for that, I think there could be a problem.

That happened in Rocky Flats. 1969 comes to mind. You know, there was the problem with the data there. So it did come out from doing that granularity check.

MR. NETON: The models are developed by year, of course, to start with, and you have a yearly co-worker model. It's not one model.

MEMBER GRIFFON: I know. I know. Right.

MR. MORRIS: In fact, this is a

quarterly model.

MR. NETON: It is a quarterly model. Then you have to start looking at, you know, so this shows maybe some people weren't monitored, and then you get into these investigations, which he posed the question on the original analysis. Well, are you going to take into account the fact that there were certain campaigns where the plant was shut down, there were strikes, there was this.

You would have to go back and then run to ground all of those different perturbations that could exist in the system. So you end up chasing a lot of issues that, given that there are thousands of samples per quarter, I'm sure -- you know, does that represent the highest exposed workers or were they, in fact, only sampling workers who were the least exposed. You know, I don't know. You would have to look at the distribution of workers by quarter.

MEMBER ZIEMER: But the SC&A sample

is across a big time period.

MR. NETON: I think one way to approach it maybe is look at the air sample results and say, okay, here is where the highest air samples were, you know. To me, that makes more sense than just sort of looking at a sampling frequency and either verifying or not verifying that they follow their plan, but where in fact where the chemical operators were the highest air samples, and did they sample those people as frequently or more frequently than --

MEMBER GRIFFON: Going back to Bob's -- I mean, part of the problem with setting up data quality objectives, I guess, is that we've got -- I've got a front end moving target.

If you are saying that you are going to do DRs based on individual sampling results for these 90 percent, whatever, I don't know when the other kicks in. So if we find -- I mean, if you had -- if I had a

better understanding -- maybe it's in there again, but if I had a better understanding, if we find -- you know, if there is an if-then tree and you look at individuals' results and you see that they have four urine samples but for the last 15 years of their work there, there was nothing, so we have nothing at the end of that tree, then we are going to be thrown into the co-worker model, and we are going to use this criteria to assign dose.

That is different than saying that we don't need the co-worker model for most cases; we are going to use their own data. My premise going in was that I want to look to make sure that the data is adequate to do each -- by sampling, to do each individual.

MR. NETON: You are asking a lot there, because each case is very specific, as you know. Many of these cases, I can guarantee you, you are going to use the 95th percentile for prostate cancers and such, and it is not going to make any difference in the

end of the day. I mean, let's face it. These metabolic organs, kidney, skeleton, liver maybe, and lung, are the ones that are going to be more detailed analyses.

Virtually, the other cancers you can use the 95th percentile for internal, no matter what the data say, and the files demonstrate that those cancers could not have a 50 percent probability of causation.

So you kind of got to look at the context on how these are done, too.

MR. MAKHIJANI: Can you hear me?

MR. KATZ: Yes. Is that Arjun?

MR. MAKHIJANI: This is an SEC investigation. I think, you know, if you look at this or Rocky Flats or what we've just sent you on the Nevada Test Site -- I mean, clearly, we think there are some questions about whether the data frequency are being carried out and whether the most exposed workers were indeed monitored for the relevant radionuclide.

This problem does become very acute, if you parse it by period, and I think, of course, it's up to the Board and the working group as to whether we do that, but I think some verification is part of our procedures, and this is why this was suggested or the working group had originally taken this up.

There are ways to slice it, and there are a lot of analyses that can be done. It surely is not going to answer all the questions, but it seems like a basic check that we have normally done, these days, I mean we normally do.

MR. NETON: It does seem to me, though, that if NIOSH has a co-worker model on the table by quarter for all these years, that's a starting point for now. I mean, you are going from the other direction.

MR. MAURO: Oh, there is no doubt that when this was prepared, it was oriented toward completion. It was oriented toward

completeness. There is no doubt that, when we started this, we didn't say, well, let's take a look at the co-worker model and see if we can validate that. That was not what we did.

MR. NETON: Right. So to me, it seems to be disconnected. That's my point.

MEMBER GRIFFON: That's right. Part of this -- the action came up before the co-worker model was completed.

MR. MAURO: Now the co-worker model may make --

MEMBER GRIFFON: Although it's been out there longer than I think we knew.

MR. MAURO: Well, yes. I agree.

MR. BEHLING: John, may I interrupt. I just talked to Kathy, and she is about to forward to you electronically those five pages that I was making reference to. The only thing that I need to know is who is going to be receiving this?

I have Paul Ziemer, Jim Neton, Mark Rolfes, Mark Griffon, Brad Clawson, Bob

Presley, and I have Arjun. Is there anybody else that I need to forward this to?

MEMBER SCHOFIELD: Yes. Hans, this is Phil Schofield. Could you forward it to me?

MR. BEHLING: Oh, yes. I'm sorry, Phil.

MR. KATZ: Ted Katz, too, please.

MR. BEHLING: I am going to try to get this to you momentarily, and perhaps that document will answer a portion of the questions that have been raised.

MR. ROLFES: Looking back in my notes, when I had initially seen the sampling plan, I thought that it might fit better if NIOSH were using multiple co-worker models, but that is not what NIOSH does.

If we had, for example, a co-worker model for secretaries, one for security workers, one for chemical operators, or a model for each Fernald plant, for example, a model for each subpopulation that was

mentioned in the proposal -- this is not what NIOSH does, though. NIOSH co-worker models are developed using all monitored workers= data fitted to a log-normal distribution.

Those Fernald employees who have less exposure potential were bioassayed much less frequently, usually on an annual basis. Those workers that had greater exposure potential were bioassayed much more frequently, sometimes daily, especially for those with urine concentrations above 50 micrograms of uranium per liter, for those that were involved in incidents or exposed to more soluble forms of uranium such as uranium hexachloride.

There are also less monitoring data for those with lower routine exposure potentials. Having 24 co-worker models would likely lower the assigned intakes for the unmonitored workers in a subpopulation deemed to have lower exposure potential.

MEMBER GRIFFON: Are you planning

on 24 -- I don't understand this.

MR. ROLFES: No. SC&A doesn't develop co-worker models, first of all.

MR. MAURO: I think that -- let me try. We are trying to make too much --

MR. ROLFES: If I can respond, Mark, please. Just a second, please.

The initial over 24 different classes that were presented in SC&A's model.

MEMBER GRIFFON: In their sampling plan, but they are not saying that it is going to end up being 24 co-worker models.

MR. ROLFES: Right. Correct.

MR. MAURO: All I was going to say is that it is simpler than what we are making the intent of this is. What the intent of this is, is when we were are done and we find out there is a load of bioassay samples for this, we could say with a high level of confidence that everyone of those strata, at least 50 percent, 60 percent, have at least one bioassay sample. Let's say we walk away

with that, and for every single one of these cells. That is going to be a strong statement of support.

Don't forget, this would be a 95 percent statement. We could be 95 percent confident that at least these many have at least one, and so that statement is by number, or two or three.

Now if that is not deemed -- and when we are done, if that statement can be made, I think that goes a long way to validate the position you are saying, that you have abundant workers of all categories, all time periods and all buildings, and as a result it goes toward supporting the idea that, yes, from that kind of dataset you could just about build any kind of co-worker model you might want to.

I'm looking at it from the positive -- if I was wearing your hat, how would I look at how this would help? Now, granted, if we come back with some holes, yes, that is going

to cause some headaches, headaches that perhaps are not real.

That is, because you have a paucity of data in that particular cell, what about it? But I guess I would say I'd like to know that, though, and if I do -- because don't forget, the granularity of this -- this is not that fine grained. I mean, it's not that -- I mean, we've grouped big chunks of years. Basically, that's what happened here.

If we do come back with a big hole in a given year or a segment for a given plant, I guess I would like to know why. In other words, there is very low frequency here, and the answer should be -- There should be an answer to that.

I think that that is what this will do. It will point us into the places where maybe we have to ask some questions. How come it's high everywhere else in the sampling, but in this particular cell it is not. It doesn't appear to be. It's a big difference. And

then be able to answer that question strengthens your position to say, yes, we can build a co-worker model.

MR. NETON: I hear what you are saying. I still -- my argument is that we have abundant data, and to me it would be reasonable to try to demonstrate why the data we have did not represent the highest exposed workers.

That's the whole point. So you have fewer samples in Plant 2-3 over a couple year period. Is the data we have of that quarter for all the workers on site representative of the highest exposed workers in that category? That's the end of the day. That's the bottom line question, not whether there was -- plant 2-3 was shut down for six months or broke or something like that.

I just don't understand what it is going to show.

MR. BEHLING: It is the question, and Mark just read out a series of criteria

for sampling, and surely some verification is needed whether that was done. If you find large holes in the production workers who had high potential for exposure, and you have all monitored worker basis co-worker model and you have to use that, or say chemical operators or other workers with high exposure potential, there surely would be a question.

It may not be that they would find these kinds of gaps in monitoring, but the issue was to verify those things.

MR. NETON: I have said all I am going to say.

MR. ROLFES: To address what Ms. Baldridge did say before, I apologize. One of the things -- I didn't have anything in my head at the moment, and I couldn't think back. But one of the things that was done at Fernald, for example, is the daily weighted exposure evaluations that were conducted in the early days, and those did track individual employees at each work station.

There were different categories of workers completing different job tasks at different stations, each with a breathing zone sample that was taken, as well as a general area air monitoring result.

Those were compiled for an individual on an eight and a half-hour work day to look to see what the exposure potentials were at different stations, etcetera, to see what -- I guess, what amounts of uranium a person could have been exposed to.

There is data that is available that could be used to go back and compare air monitoring data from those daily weighted exposure reports to the urinalysis data. So there is information that is available, and that was something that NLO actually did do on site.

There were some examples. There was a report from J.F. Wing at NOL who had found that one -- I believe it was an operator

-- had some high urine concentrations, and what he actually did is mirrored the individual's work. He tracked him around the work stations, etcetera, and actually used himself as another individual who basically was doing an experiment on himself to see what his urine concentrations would have been doing the same work.

So there were things that were done, but we have on the whole done something like that.

MS. BALDRIDGE: Okay. The point was that in the petition there is also the affidavit that challenged the practices in the air monitoring and put some of that data still in question, the validity of it.

MEMBER ZIEMER: Question. This is Ziemer. Mark or Jim, on the proposed co-worker model, does it take into account the -- Is it by years or by --

MEMBER GRIFFON: By quarters.

MR. MORRIS: With minor exceptions,

it is by year -- by quarter. When there were like plant stand-downs for funding issues or things like that, there a few years where it moved back to annualized.

MEMBER ZIEMER: So for a given worker, if you were using -- For a given claimant, if you were using a co-worker model, you would go back and make the assignment of dose by quarters.

MR. MORRIS: That's right.

MEMBER ZIEMER: And that would reflect, presumably, whatever campaigns were going on and so on. That information -- So if you have a quarter where there is -- or a year even, whatever time period there is where there is low activity or not much going on, then you would expect the sampling frequency, bioassay frequency, to drop off.

That information would not show up in this sampling plan.

MEMBER GRIFFON: Why not?

MEMBER ZIEMER: Well, I don't think

you would be able to -- You are sampling over the total --

MEMBER GRIFFON: Absolutely not.

MEMBER ZIEMER: So I'm trying to get a feel for whether it would be more important -- If we did a sampling plan, would it be more important to do it by year or by time periods and have less -- have two or three--

MR. MAURO: More granularity.

MEMBER ZIEMER: Well, it depends on what is more important to look at. In other words, you could take groups of operators and lump them together. I don't know. There are chemical operators and --

MEMBER GRIFFON: Do it more like production maintenance, administrative or something.

MEMBER ZIEMER: Less detail on the job categories and more detail on years, if that would help. I'm still trying to get a feel for what value added we get from doing

this sampling, because if we are really building -- Again, I know you are saying this isn't for building a co-worker model, but you are trying to inform the system on whether or not the data is adequate.

If they are looking at it more by time periods and you are looking at it more by job categories, we pass each other in the night, so to speak.

MEMBER GRIFFON: My sense is that -- and maybe I'm wrong, John, but my sense is that when you are pulling this data together, even if you look in a ten-year -- I don't understand it, but whatever the time period is, when you are pulling all these records, if all of a sudden you find that, you know, you are doing '60 to '70, but 1965 again and again is coming up as the place where there is less data, so you flag that. You make a note of it.

It comes back to the work group, and maybe there is a ready explanation, you

know that agrees with our co-worker; there was a down period. And it goes away.

MR. MAURO: And I am going to ask Harry, if he is online, you know, when I was looking at the HIS-20 database, and you are trying to sort, trying to do Plant 1 for this time period, well, you have no choice but to go in and grab all the years in that time period, all the samples in that time period.

In effect, we are going to have that, but we are going to collapse it into this form. Now what I am hearing is don't -- you know, there may be some great value to not collapsing that data.

MEMBER ZIEMER: Well, I don't know if there is.

MR. MAKHIJANI: This is Arjun. I think, as we did for the Nevada test site, there will be several things we could deliver to you that will be done during this project. There will be, as John indicated, a non-collapsed, you know.

When you grab these claim files, you will get all of the data, whether it is quarterly or daily or annual or whatever, and that will be in the file; and, certainly, as with the NTS, that can be submitted to the work group and NIOSH and put up on the O: Drive. But for the purposes of the procedures check, it would propose to aggregate.

It would propose to aggregate how many samples do you think you are going to have based on the monitoring schedules that are on paper and how many samples do you have, and for particular years. They just look like something leaps out at you as 1969 leaps out at us in Rocky Flats.

You know, we did establish an explanation for it, but it did require further work.

MR. MORRIS: If you would indulge me one last comment, going back to my first one, data quality objectives. John, you said something like, well, we could say with 90

percent certainty that 50 percent of the workers were monitored as expected, and that would be a great outcome.

MR. MAURO: No. I think a strong outcome would be if a statement could be made within that cell that you would be 90 percent confident -- 95 percent confident that at least 50 percent of the workers had one sample per year.

Now that seems to be a pretty strong statement.

MR. MORRIS: Okay. Well, let's stop right there, and then let me finish my point.

You could also, without saying what the scorecard would be for success ahead of time, say that is a very weak outcome, and that that is a matter of opinion that you are establishing later on instead of up front.

MR. MAURO: The only reason I say strong is that, from there, if you were going to say, therefore, for those people in that

strata who were not monitored, I am going to assign the upper 95th percentile from that dataset.

MR. MORRIS: Well, we don't do upper 95ths.

MR. MAURO: Okay, right.

MR. MORRIS: We do 50th percentile for most people or 84th percentile for highly exposed.

MR. MAURO: Then that would not -- In other words, you would not use this information in that form. In other words, whatever information is here, once this database is here, I'm just sort of speculating that besides giving a sensibility of the degree of completeness, is there other value that it might have.

I was thinking that it might have value in validating your co-worker models, but its primary objective was to make an objective statement regarding completeness in each cell. That's it.

MR. MORRIS: Okay.

MEMBER GRIFFON: Yes, I wouldn't even say that that -- you know, to sit here and say that was a good outcome or a bad, I think you judge that against the original policy or whatever, or the expectation. You know, if operators should have been -- If 90 percent of them or if 100 percent of them should have been sampled four times a year, and your outcome is that only 50 percent of them were sampled twice a year, then that is probably not such a good -- that is not a good result.

So I think it depends on the -- I think you are right about that. But I think just to look at the data and see and then I don't think we need those kind of -- because my feeling was that, if we try to define those things up front, then we are going to get down into this -- well, defining those can be difficult, I think, because you got to et into the policy. You got to figure out what --

I think, if we get -- we are trying to get a sense of this on the work group level. At least I am. And if it passes the kosher test, that's where we are going with this.

Then but to the -- to speak back to the co-worker model thing, I guess my concern is that, again, how -- I mean, I understand you can use this for a lot of cancers, and it is not going to make a difference anyway. But the point is for those other cancers, I think, that if the individuals don't have enough data, I don't think the DR team is necessarily going to readily go to that 84th percentile and assign it, because it is going to knock it over. That will be over-estimating probably.

So then they are going to go with the individuals' data, and that is where it comes into that data completeness question. Is it complete for these people? Are the individual set of records complete enough to do it.

MR. NETON: But that is a judgment call as to what is applied. Is it -- you know, we have a whole procedure on that, on what class of workers we see what type of exposure, whether they should have been monitored and were highly exposed, were administrative workers and probably didn't need to be, and then there is that middle category that we assign.

That has been proceduralized. We have been using that for five years. That's a judgment call that always happens on a dose reconstruction. It=s not unique to Fernald.

MR. ROLFES: Data completeness is evaluated for every dose reconstruction that is done. It is one of the first things that we do.

MR. NETON: I would argue that it works to the claimant's favor if only chemical operators were monitored, for example. If this study shows that no administrative workers were monitored, I would say it is

going to -- primarily, the higher exposed workers are monitored. That's my opinion, and I think that's true.

So this study shows that, and maybe there are some holes in the lower exposed workers. Well, that is as biased as the co-worker model high at the end of the day anyways. So I don't really see the value. I should shut up.

MR. MAURO: Well, I'm not here to sell this. I'm here trying to say that where -- and I was given a mandate to come up with a sampling plan that would evaluate completeness of the records, and this is what we came up with, and this is what we initiated a week ago to do that.

Now I think that this is fine. What we are really doing is second guessing that judgment. Is this going to add value? That's fine, but our intent was to try to address completeness questions the way I just described it.

We will do whatever the work group would like us to do.

MEMBER GRIFFON: What is the -- can I step back? What is the procedure for evaluating the data completeness for an individual DR claim? It's not Fernald-specific. It's global. What is the procedure?

MR. ROLFES: That's an important thing that --

MEMBER GRIFFON: What is the procedure?

MR. NETON: I don't know there is a procedure that says --

MEMBER GRIFFON: I don't think there is. That's why I'm asking.

MR. NETON: -- if the data are not -- if there are insufficient data in a record, what class of -- what part of the co-worker model was assigned? That's a procedure.

MEMBER GRIFFON: Right. That, I agree with.

MR. NETON: But the value judgment has to be determined whether or not the data are sufficiently adequate in the file itself. If you have one bioassay record representing 20 years of exposure, that's clearly not adequate.

MEMBER GRIFFON: Right. But that's kind of an internal dosimetrist's judgment, right?

MR. NETON: Exactly.

MEMBER GRIFFON: So we are saying let's look at the whole class and do that judgment.

MR. ELLIOTT: It is also reviewed, too, the peer review process.

MR. NETON: How could you proceed, Mark, if you got --

MEMBER GRIFFON: You can't. You probably can't.

MR. NETON: -- if you have a sample at the last day of employment and you can say, well, what was that guy's maximum exposure

that he would have that sample on the last day of employment, one bioassay point is adequate probably.

MEMBER GRIFFON: Right. You probably can't, but if you got a boatload of data and you are missing everything -- you are missing a big time period, then you might have -- that's what this is going to show.

MR. NETON: Then the internal dosimetry implementation guy talks about whether you use nearby data to fill in those gaps or you apply the surrogate, the co-worker model in the middle.

I mean, there's a lot of different ways to do this, and that's -- those have been done many, many, many times, many different ways, but always to the claimant's benefit.

There are many ways to fill in the gaps of the bioassay. The co-worker model is one of them. You just couldn't proceduralize this down to the nth degree.

MEMBER GRIFFON: No. No, I'm not

saying that. I'm not suggesting that. I was just asking -- the statement was made that data completeness is reviewed as the first thing based on procedures.

I don't know that a procedure exists. That's all I was asking.

MR. ROLFES: To my knowledge, I can't mention a procedure number or anything, but as part of the dose reconstruction process, if you look at the individual's dosimetry data and see large amounts of external exposure and see that the individual was, for example, a chemical operator we use, and you don't see any bioassay data in there, that certainly would raise a flag on the data completeness.

So a review like that would be done and would trigger us to know that, hey, something doesn't sound right here; we need to apply the co-worker model in this case, because the data appear to be incomplete, or that would prompt, for example, another

request from DOE to --

MEMBER GRIFFON: Yes. A new question: Where is this guy's records?

MR. ROLFES: Right. Things like that are done on every dose reconstruction.

MEMBER GRIFFON: I guess what this is doing is saying, if that is happening on a frequent basis, you got a problem, because then in that case you just described, that means you are missing those upper people that Jim described, and then your co-worker model is skewed.

So I don't think there is any -- I'm not sure how many person hours this takes to do, but I think it is beneficial. I think there is value added.

MR. NETON: Two hundred person hours?

MR. MAURO: Yes, 200 work hours. We estimate about 200 work hours for this.

MEMBER GRIFFON: Which we have just about spent now discussing it.

MR. ROLFES: Brad, should we take a break?

CHAIR CLAWSON: Well, I just want to be able to say one thing. I've listened to all this, and here it comes back one thing, and that is completeness of data.

The reason I wanted to start this up front is every work group I have been on, we end up coming back to this question at the very back end of it. I wanted to have it put up front now.

If we can't come to that, then I guess we can come up to the very end of it again and get right back into it.

So, John, you know, I kind of feel like John has been -- I asked John, because this has been an issue at, it seems like, every work group I've got on, and I wanted to try to get it done up front instead of at the very end of it, because it gets kind of convoluted there.

We will discuss that.

MEMBER GRIFFON: The last thing I will say before -- because we need a break to think about this, but I think we need to think about the other obvious audience in this, and the petitioner has brought this up, and I think it is up to the Board in some way to be able to respond to the petitioner's concerns.

If at the end of the day, like Rocky Flats -- you know, I'm not sure we've convinced everyone in the room. I'm pretty sure we didn't, but our conclusion and SC&A's conclusion was that it was complete. But we went through it, and we've made the effort to look at that closer, and it was in part to address the petitioner's concerns.

So I think we need to go through this process. I'll leave it at that.

CHAIR CLAWSON: That said, we will take a break.

MR. KATZ: Ten minutes?

CHAIR CLAWSON: Ten minutes.

MR. KATZ: Okay. So it's about

10:30. We will take up again about 10 to eleven.

(Whereupon, the above-entitled matter went off the record at 10:36 a.m. and resumed at 10:57 a.m.)

CHAIR CLAWSON: We can go ahead and start back up. We've got a few housekeeping issues to take care of.

MR. KATZ: Folks on the phone, this is the Fernald Working Group. We are getting started again. Sorry it was a little bit longer break than we intended. Brad, it's all yours.

CHAIR CLAWSON: First of all, I would like to apologize, some of this information getting out late and so forth.

As John would say, I would like to tell a little bit of a story. Part of the story is here a couple of months ago Hans Behling was doing a marvelous job at turning the reins over to John, and so John had to kind of start back up.

One of our goals, and especially with Fernald, is to be able to get this information cleared so that we can hand it out to the petitioners and so forth that are with us here today, and we weren't able to do that. I take a lot of the blame for it, because we were going through a lot of different things, changing the matrices and so forth like that.

I know that John did send it in for Privacy Act review, but we didn't get it back in time, and there have been some concerns with that. I would like to just let -- well, anyway, the petitioners that we have here and co-petitioners wanted to voice a concern, and I will turn that over to Ray Beatty who wanted to make a comment.

MR. BEATTY: Yes. I am Ray Beatty, a former worker, and I assist the petitioner. I am really not listed as a co-petitioner on the active petition, but I have worked with her rather closely for the last couple of years.

It was stated earlier in the meeting, it has been six months since we had a meeting, and at that meeting prior to this one, there was also a matrix handed out, and we were privileged to it initially. Then it was taken from us because of privacy.

Quite frankly, that is, I feel, very disingenuous to the petitioner. A lot of the people out there filing claims already see a lot of problems where they think it is unfair, and when the petitioner comes to something like this, that person should at least be privileged to the information.

I didn't know that this other transition had taken place with SC&A. So in fairness to that agency, you know, I kind of reviewed some of my comment, but I still think it is really not showing transparency to the petitioner to not be privileged to this information when it is presented so they can at least follow along and make rebuttal when it is necessary.

It's kind of like the laundry issue earlier, and about the millwright being omitted. You know, I would have seen that personally if I had looked through that list. So I am taking kind of personal exception to some of the things that are being developed, but we don't have input.

So that is just my comment in a nutshell, and I don't mean to take up the working group's valuable meeting time, but I just think this has some relevance. In the future, possibly, if there is going to be a document reviewed, if it is a new matrix or whatever, that it could be -- names could be redacted or, if there was a privacy issue.

Rest assured that -- I know Mr. Callaway and myself -- we received some very extensive training on the HIPAA consent law and disclosures and the Privacy Act. We are very cognizant of those requirements, and we respect that.

So rest assured that we just want

to follow along and be privileged to something where we can have comment.

Anyone else that would chime in, feel free to do so, but I just feel like it is a fairness issue, and I think it could be fixed at the upcoming meetings. Thank you.

MR. KATZ: Thank you, Ray.

MR. HILL: This is Steven Hill from Congressman Chabot's office. Just briefly, Brad, I appreciate you raising that issue, and I'm sure you will make the necessary steps to address that in the upcoming meetings. So I appreciate that, as well as the comments from Ray.

MEMBER ZIEMER: Yes, and I think, certainly, the Board members agree with Ray's statements.

CHAIR CLAWSON: Definitely do.

MEMBER ZIEMER: Actually, we also feel somewhat of a little frustration. The rules of the game have changed even for the Board as we have proceeded, and now there is

some additional security issues that have come into play that weren't there before, and it delays some of the documentation. But our General Counsel and others are working very hard to keep the turnaround time short.

So we just need to be careful when we schedule the meetings and get the documentation far enough in advance so that we can get it distributed. I think it is certainly our intent.

This is a little glitch today, but I think we can avoid it in the future.

CHAIR CLAWSON: And in all fairness to John, I am probably the main one to put the glitch in it a little bit, because he produced an awfully big document, and he did a good job. The smaller one -- that was to help us out a little bit on that, and John has been working very diligently.

I will personally take it as an action item to be able to make sure that these things are sent in to Emily and everybody in a

timely manner to be able to get this done.

This was a glitch, and I apologize to everybody for that.

MS. BALDRIDGE: I'm a good listener.

CHAIR CLAWSON: Well, I know it, but you know what? It's really good to be able to follow along, and we had this glitch the last time, and I swore we weren't going to get into that again, and it happened again, and I apologize to everybody that's here for that.

MR. ELLIOTT: You are right that we need to provide these documents as soon as we possibly can, but let's be real about this. In many instances, these documents come a week before, two days before.

I would offer that we have set a precedent with Ms. Baldridge in one of these meetings where the DFO and OGC and other appropriate individuals have looked at the documents about to be discussed and shared

those, if they didn't contain Privacy Act. In one case, they did contain Privacy Act, but we asked that they not leave the room and not take notes.

So I would offer that, you know, the precedent has been set in that regard. The two documents that were discussed in the previous discussion this morning, I asked if Emily would look at them, because I didn't see any Privacy Act information in those two brief documents and thought that it could be shared under that precedent that we had set.

So I think we all should remember that as an option if the documents are generated in such a late fashion that we can't get them fully redacted or Privacy Act reviewed for release. We can avail ourselves of that option perhaps.

MS. HOWELL: Can I just add? I mean, that's fine. It is okay when it is a short document. The problem is -- and Mr. Beatty brought up frustration with documents

being handed out and then taken back up.

If we are able to do an on-site skim of these documents and then give them to you, then I have to say, if they go out, they are going to have to be given back to us.

Document control is a really big issue for us, and we have to ensure that documents that are being made available to members of the public have the appropriate disclaimer language on them, headers, footers, so that anybody who looks at those documents later on, whether it be a staff person or a member of the public, can be aware of whether or not those documents have been reviewed, that they are pre-decisional in nature.

So I guess I would just -- You know, obviously, there are some things that we need to work on about this process, but if we are able to make compromises like what Larry has just spoken about, then there's going to be some other compromises like not being able to take notes or not being able to keep the

documents. Hopefully, we can not have this problem in the future, but there is a short turnaround often.

MR. ELLIOTT: I think the compromise has value, though, because it enables the people who are sitting in this room to at least understand what is being discussed and can refer to the written page. So I'm in favor just to strive to get these releasable and, if we can't achieve that, then we should be able to share what we can share with the understanding that it is a compromise situation.

MR. KATZ: I agree. As DFO, I just can say in the future -- What happened this morning is that we had decided the discussion while Emily is racing through trying to review the document, and we didn't have any extra copies and so on, and by the time we were done with that, the discussion is pretty much done, and it is not much of assistance to the rest of you.

I apologize for that, too, but in the future going forward, certainly, we can sort of practice -- If we know we are coming to a meeting and the Privacy Act review, the formal Privacy Act review, hasn't been completed on a document, we can try to do that for the preliminary scan before the meeting starts.

If it is a big document, it is not going to work, but for a short document like we had today, if I had it, we could have done it. We could have done it.

CHAIR CLAWSON: The Fernald?

MR. KATZ: Yes, the document presented.

CHAIR CLAWSON: The smaller matrix.

MEMBER ZIEMER: The other thing to keep in mind is many of our Work Group meetings get scheduled in advance in anticipation of documents. For example, we have a work group on November 10th whose deliberations are based on the assumption that

certain documents will be forthcoming from NIOSH by October 30th, but if that date slips a little bit, and it could for any variety of reasons, then suddenly we are pressed for time both for the work group and then for the Privacy review.

One of the options then is you reschedule the meeting. That is sort of one of the realities. Many of the Work Groups schedule based on when they anticipate being available.

CHAIR CLAWSON: Well, we are trying to be proactive in it, and also, too, last night was the first time I was able to look at the small one. John had it all set up to be able to put it on the board and everything, and I says I don't think -- it hasn't been Privacy Act cleared.

So that put him scrambling to try to make copies and so forth, I guess. So we will keep this in mind. We apologize, and we know what kind of a situation -- It's like us

trying to do something without a paper in front of us, too.

MS. BALDRIDGE: Two comments, the first being one of the disadvantages is that I don't know the format that is being followed, and I may interject and get things off schedule, bringing up topics that I am not aware are scheduled on down further in the proceeding.

CHAIR CLAWSON: Right.

MS. BALDRIDGE: And secondly, about forthcoming documents, I have a question for Mr. Elliott.

At our October meeting last year, you made a point of telling us that a draft for a portion of the site profile was pending and potentially available in a three-week window, which would have put it into November of last year. I believe that was on the environmental portion of the site profile that was being revised.

I was wondering what's happened.

MR. ROLFES: The only thing I can think of is that there could have been a portion, like a white paper, that was produced. I don't have any recollection of us actively revising --

MS. BALDRIDGE: I'm referring to the notes.

MR. ROLFES: Okay. We can take a look back in the transcripts.

MR. ELLIOTT: Let us look and see what we were discussing at that time. It may have been a draft. It may have been in the works. May not ever have been finalized. We don't share draft documents until we ourselves have --

MS. BALDRIDGE: I think there was an inquiry, and you gave a reassuring word that, you know, what they were asking for was potentially only three weeks away, at least in draft form.

CHAIR CLAWSON: And that was to the TBD?

MS. BALDRIDGE: I believe so.

MR. ROLFES: It could have been a component of our site profile that was pulled out of the site profile as a white paper, and will be incorporated into the site profile.

MS. BALDRIDGE: I believe that was maybe the 24th -- the October 24th meeting last year.

MR. ELLIOTT: I will check on that, and I will let you know.

MS. BALDRIDGE: Sure. Great.

CHAIR CLAWSON: Okay. With that, we will turn back to John.

MR. MAURO: I guess where I am right now is looking for some direction from the Work Group regarding the work plan, whether or not we hold off until we get further direction in light of the thinking or should we move forward?

CHAIR CLAWSON: John, you know, I've been sitting here listening to this communication back and forth a little bit.

Explain to me on the HIS database, are we going to be able to pull this up, plant building by building?

MR. MAURO: I am going to introduce Bob Barton. Bob, could you come up and -- I'll give the quick answer, and then you can tell a little bit more.

For the last several days, Bob has been diving into the database, and my understanding is we are having some trouble linking people with plants, but Bob feels that we might have found a way to get through it indirectly.

Maybe you could explain. In other words, making this matrix essentially links people, job category, with plant, with time period, and be able to do that.

It turns out that one of the problems we are encountering is linking the person at a given time period with the plant. I think you said it looks like you are on the track of finding a solution.

CHAIR CLAWSON: You can come up here.

MR. BARTON: As John said, my name is Bob Barton. I've spent a couple of days looking at the HIS-20 database.

Basically, what we have been able to do so far is match claimant Social Security numbers to breathing zone data. Now there are approximately, I want to say, maybe 3500 workers in that breathing zone file, and so far we haven't been able to match a significant number of claimant Social Security numbers to the Social Security numbers contained in the breathing zone file.

The problem, as I see it, is going to be getting the claimants matched up with a particular building that they worked in. As I see it, in the HIS-20 there is actually only two of the spreadsheets that make any mention of the building, and as I said, we have only looked -- we have only been able to -- we have only matched up one of those spreadsheets.

What we have so far is about 48 matches, which doesn't get us all that far. But again, I think the biggest roadblock we have in undertaking this is matching people up to the building they worked in.

MEMBER ZIEMER: So do you look at the individual's file, the actual claim file where they -- I mean, what do they indicate about when and where they worked or does it come out in the CADY interview?

MR. BARTON: Oftentimes in the CADY interview there will be some mention of what buildings they worked in. I wouldn't say that is the majority of times. A lot of the times it is family that is being interviewed, and they don't necessarily know that information.

In the DOE dose records for the site that I have looked at, I have not seen any mention of particular buildings.

MR. ROLFES: If you take a look in the DOE response files, there are codes that are provided next to the bioassay sample

typically, and some of them are very easy to track which plant the individual worked in. Others aren't as clear.

I believe in HIS-20 as well there are some older codes for buildings at Fernald, and it isn't as straightforward as this individual worked in Plant 7. They gave like a number, like 1045 or something, for a building, and that would have to be decoded as whatever plant. So that might be a way.

MR. BARTON: That is certainly information that will help us.

MEMBER GRIFFON: There is a column of plant IDs, and that's the one? Do you have -- I mean, do you have a listing of those plant IDs, a key? Someone must have that.

MR. BARTON: Something I have noticed with the plant IDs, though, is it seems as if every single worker has an individual plant ID. So I don't know if that is actually tied to a particular building on the site.

I would tend to say, looking at it, since they all seem to be individual plant IDs, I don't think they are tied.

MEMBER GRIFFON: Somebody on the line was trying to say something.

MR. CHMELYNSKI: This is Harry Chmelynski. I had the same question that Bob just asked.

MR. MAURO: Well, I guess we are at a point in this process where that, in effect, I started a discussion related to issue number 1, having to do with uranium bioassay data and its use in dose reconstruction.

This first step was completeness. The reason I started with completeness is because I guess that is a fundamental issue. If we could walk away saying, yes, the data are basically complete, then all of a sudden everything falls off pretty easily.

Then we could talk about recycled uranium. We could talk about -- and the plutonium levels. We could talk about

enriched uranium, because all of those hooks to deal with dose reconstruction for RU and for enriched uranium would require the milligram per liter number as your starting point.

So let's first -- I was hoping that we would agree, after we get through this completeness issue and, yes, we agree that this is a good way to get a sense that we've got a complete data that we could hang our hat on, then we could -- but it sounds like that we sort of got stopped at this stage.

What we can do is put that on ice right now. I mean this issue of the completeness analysis, whether you would like us to go forward with this or something else, and move on to the other issues related to internal dosimetry and bioassay data.

MEMBER GRIFFON: No. I think we need to --

MEMBER ZIEMER: I think, though, we need to answer the question of can you

actually do that by plant or do you have to go by job category only or instead?

It sounds like there is a possibility that, practically, you may not be able to do that. And if not, what would you propose as an alternative? Is it job category?

MR. MAURO: I don't have an answer yet.

MEMBER ZIEMER: That was sort of a rhetorical question.

MR. MAURO: What we are basically working at is real time work. As we said, we only started about a week ago, and the first thing is, okay, is it tractable? Then let's dive in, and we are finding that there are some challenges.

Now we just heard that there may be a way to link people's Social Security numbers with building, maybe not. I'm not sure. If we can, then we have something that we could -- and we could do this thing here.

Now that may be a showstopper. It may turn out we can't do it. Now we don't know yet. So the answer is that -- But I guess the bigger question is, even if we could do it, it sounds like we have a bigger question on the table, whether or not it is agreed amongst the Work Group that we should even try to do it.

MEMBER PRESLEY: That's my problem. I'm going to be honest. This is Bob Presley. Is there enough value added to do this at this point with the data that NIOSH has? Are we going to spend 200 hours and say, well, we are sorry, we didn't come up with anything or can we let you all work enough with Jim to where that SC&A can say, okay, we agree with NIOSH's data on this, and let's move forward?

MR. MAURO: Could I make a suggestion? I mean, this is just a suggestion. It sounds like there is enough of uncertainty regarding the value of this work and, as important, its doability because of

the nature of the data.

Now this goes back to the question, okay, there is a data completeness, data validation issue that goes to the heart of an SEC, and it has been our experience that sampling data in the database as a way to get a sense of whether or not you could build co-worker models, what those co-worker models might look like, whether or not you could do dose reconstruction for each category of worker, each time period.

These are fundamental questions at SEC. I think that perhaps, in light of the fact that there is a co-worker model out there now, and there is some question of whether or not we could actually do this, maybe it is time to regroup and say let's stop the presses on this, regroup, maybe have one of those teleconferences with members of the Work Group to rethink this.

That could be -- we will have to look at the co-worker model and give some

thought to it, talk to Mark. Now that -- We are pretty familiar with the HIS database, now that we've built in at least initially what is there. Perhaps we could construct something different or new that might be --

Mark, really, you know, I feel as if I'm stepping into your territory.

MEMBER GRIFFON: No, no. I just thought -- We had a little discussion during the break of maybe a sort of, I guess, a stepwise approach to this, that we -- because there is a co-worker model on the table now.

When we initially started this task, there wasn't. It was in development, and it did come out, but this has been quite a lengthy process. But my feeling was let's go forward with the sampling focus on what we believe -- and that's a question, too; that's a little subjective, too, but we believe would have been the higher exposed workers by, I guess, job type, is what we got to go by.

You know, the plant question -- The

plant problem is a problem, but I would say almost that we could start a sampling by a time period, by job type, and then backfill and see what plants we covered.

We have done that with our DR sampling, you know. It is not the best way to do it, but if we can't sample by plant, we don't have the data.

I'm looking at this other table in HIS-20 where you talk about breathing zone sampling with building ID. The problem there is it starts in '97 or something. It D&D period. It is not -- So you are not likely to see a lot of links there.

So I think I would say start with what we think -- what we believe to be the higher exposed workers by job type, take a sampling, look at them in those certain time periods, come back, report on that, and then we can compare that against -- juxtapose that against the co-worker model and say, okay, it looks as if they did sample the highest

exposed and, therefore, this co-worker -- you know, when there is inadequacies, we know now that they are going to use their co-worker model in those places, and is or is not that approach -- We can have that discussion.

So I think that would be -- and that is a stepwise approach to, instead of sampling all the job categories. I agree with Jim. I don't want to just throw money at the project. Let's do this in a sort of stepwise approach.

The difference, I think, at the start, like I said before, is that we didn't -- I had the understanding that there was a co-worker model under development, and I know it's been out there a while, and that it was also only going to be used in rare cases where they had little -- It's even stated in the minutes there that John compiled for this task.

It says that, you know, basically, it will only be used in those few cases that

are missing bioassay data. So it left me the impression that that was going to be the exception rather than the rule, if it is going to be used -- If it is much more of an overlap, then I think we can judge it this way.

We can look at the higher exposed workers for data completeness. If, in fact, we find that it is pretty complete and it is in agreement with the co-worker approach, then I think we can put this to bed now.

CHAIR CLAWSON: I also feel, you know, we still -- The petitioners, we still owe them, because there has been questions raised about completeness of the data, and this is what I was trying to come to a point with.

There have been questions about the air sampling data and everything else like that, and this is what I was trying to put to bed up front and go from there.

I would still like to proceed on.

How we do it -- you know, that can be up to us, if you want to go for the higher set or whatever, but somehow, to me, I would still want to be able to check this data.

MEMBER ZIEMER: I wouldn't object to a stepwise approach, as Mark described, which would allow us maybe to stop if we needed to.

MEMBER GRIFFON: To come back and reassess.

MEMBER ZIEMER: It would also answer the question as to how well you could do this. But the other thing is I think, before we make a final decision on this, I think in fairness the petitioners need to have this information and have input on it, including Hans' stuff.

MEMBER GRIFFON: I think they might have a lot of input as to what job categories.

MEMBER ZIEMER: And be informed how you proceed with this.

MEMBER GRIFFON: I agree. I agree.

MEMBER ZIEMER: And so I would like to see us delay our final decision on doing a full fledged thing, but maybe allow a first step, if the petitioners would agree to that, that would allow us to determine whether sampling would, in fact -- I don't know.

The first steps might be some examination of that data that was described by Bob or something like that, which would give us the option, if we recognize that that wasn't going to be fruitful, to say, well, we don't want to pursue this any further.

CHAIR CLAWSON: Yes.

MS. BALDRIDGE: Just a reminder. The petition contains historical documents from National Lead of Ohio that said they knew what the assignments were, but they didn't know where the men were working or what tasks they were performing.

When the bookkeepers were -- recordkeepers were asked, they said there were only 2.6 pieces of data per worker per year.

This, I believe, was in a 1982 or '84 inquiry by the Department of Energy possibly.

CHAIR CLAWSON: Ray?

MR. BEATTY: Yes. I agree with Dr. Ziemer there as far as getting some worker input. We have done some of this in the past in various interviews. Matter of fact, we met with NIOSH representatives from the metal trades department out of Washington, D.C.

Several union reps coming from across the country went to NIOSH to talk about the various things that occurred at the site and maybe what was missing, maybe even some of the CADY interviews, something that wasn't revealed but like, say, a widow or the sons and daughters of former workers wouldn't know those things, but the former workers would.

I would personally volunteer my time to sit down with SC&A or NIOSH to help out as far, well, co-worker model data and how it related, at least in remediation years.

I think you could get a feel for

how, especially in a maintenance division -- In those early years, people were assigned to a building for security reasons, while on overtime they may have to go to another building. Now that was mostly maintenance and transportation.

Chemical workers pretty much stayed within their own realm of responsibility in a specific building, and trying to connect into that building -- that might be a little easier. But I do know that other divisions or other groups and classifications were bounced around, but I do see the value of this information as it relates to trying to do a connection and assigning dose.

I do have a question. I don't know who this is for. Someone, try to tell me when HIS-20 come about, because I worked under HIS-20.

MR. NETON: I worked on HIS-20 when I was there.

MR. BEATTY: Okay. My question has

been answered, and I know the time frame now, roughly in the late Eighties.

MR. NETON; HIS-20 was backloaded with the historical --

MR. BEATTY: Well, and that is what I want to allude to. I am kind of questioning the reliability of that back data that was downloaded into this HIS-20, because we have challenged that in the past as labor representatives and with the federal court over it.

So there lies a problem, I think, with maybe some of the HIS-20 data. We worked under it in a scanning process out there with our badges. So when we entered a building, our specific ID number, our badge number, was entered into the HIS-20 database as an entry time, and when we exposed a certain length of time -- very valuable information.

I just kind of challenge, though, the fact of the old data being used as to the reliability of that data.

MR. ROLFES: I think that was an important point, Ray. I think we did -- We were asked by the Advisory Board to take a look at that, and we have provided our analysis of the hard copy data to the electronic data that was back-entered.

MR. BEATTY: And even a NIOSH study revealed some shortcomings of using HIS-20 as it related to trying to assign medical dose -- or dose and medical records and training records to the workforces. There was a problem of trying to accomplish that task.

CHAIR CLAWSON: Paul?

MEMBER ZIEMER: Yes, and Ray, I wasn't necessarily suggesting additional interviews, but more making sure that the petitioners have this information. I think several of you here could help inform us on these documents, based on your own knowledge as to whether the approach made sense.

You mentioned, for example, workers are assigned to a particular plant but

overtime may be somewhere else. That kind of information might be helpful as we make a decision on this.

So I was just suggesting that we not finalize this sort of approach until we have some input. In fairness, unless they have the papers, they can't really inform us very well.

MS. BALDRIDGE: There was a recent meeting last Wednesday that NIOSH, I believe, sponsored at the Fernald site, and I had the opportunity to meet people that I had not spoken with previously, and I don't know how many said, yes, they did a site profile, but they didn't have that Dad worked in 8 or they had missed me in Plant 4.

The frustration is that once the dose reconstruction goes to the DOL, you cannot get it corrected. I mean, you may as well talk until you are purple. They will not listen to you. They will not acknowledge the documentation that is sent to even prove

anything contrary to what has been submitted with the dose reconstruction.

CHAIR CLAWSON: Okay. So, Bob Presley.

MEMBER PRESLEY: One of the things that I would like to see Ray do and some of the people that work up there is the list that you have here. I'm going to be honest with you. I don't see a foundry worker on this thing. I don't see chemical workers on this thing.

Those are two that I personally know probably got very, very high doses, especially in the early years. So you know, I would like to see you all do some work with them to get a little bit better list of who we think got the highest exposure.

MR. MORRIS: Well, there is, I would think, helpers and such job classifications.

CHAIR CLAWSON: And also it is like every other site we've got into. We've got a

sub-group of all of those that are basically roving people that go everywhere. That is something we've got into in almost every site here, your laborers and, as we have said, the laundry and so forth like that.

We are going to have a group that is the most highest exposed. I think we need to be able to look at that. So I guess at this point, John, from what I am getting a feel from everybody and what the comments have had, and Ray has expressed a desire and so forth to be able to help with it, maybe if we are not going to be able to do it by plant or whatever, maybe we need to change this to highest exposed, get a better grasp of the classes that would be involved in that, and then go from there.

Would this be a problem?

MR. MAURO: No. I mean, what we would do, we would regroup. I would very much like to work with Mark on your -- It sounds like that the orientation would be worker

category and highest exposed workers as it relates to your co-worker model as being the sampling plan, the thrust of the sampling plan, as opposed to the way it is laid out here.

MEMBER ZIEMER: Perhaps.

MR. MAURO: Perhaps, yes. I guess it is a probing. Really, what I am hearing is -- and correct me if I'm wrong -- you would like SC&A to probe this issue further, working with the Work Group and working with NIOSH on how would we shift perhaps the emphasis on the sampling plan in a different direction.

That might deal with the issue you just brought up regarding the categories of workers, the issue that Jim brought up regarding the co-worker model, and get away from, I guess, the idea of completeness by strata, because that was the theme here, completeness by strata, creating the strata that we thought were meaningful and then determining percent completeness of the

dataset.

It sounds like that is not going to be as valuable as this other strategy. If we a -- Give us a week to probe it a bit. Bob has been looking at the database. Bob, working together with Mark and our folks, maybe we could come up with a new strategy that we could communicate in an e-mail to the Work Group who have a different strategy and get some feedback.

So I don't think we are talking about a large burden, a large delay. This is good. If it turns out we are going down the wrong path, fine. We will shift direction. So if that is okay with the Work Group -- Certainly, I want to make sure that --

MEMBER GRIFFON; I don't see it as a completely different approach. I see it more as a stepwise, but probing to see if it is doable is a first step.

MR. MAURO: The idea of completeness by these strata -- sounds like

that is an old concept that perhaps we should move away from. I'm not sure, you know.

MEMBER GRIFFON: I'm not sure that -- I mean, I think that what you want to do is look at people with the highest exposure and see if they were monitored adequately. Right? If you want to call that completeness or not, I don't know, but we are focusing on that high end group instead of all groups right now.

MR. MAURO: I understand.

CHAIR CLAWSON: We've still got to have a completeness.

MEMBER GRIFFON: And don't forget -- not to throw a monkey wrench in all this, but the data completeness review -- if you look down, I think, in the next -- it might be listed in a different place, but it was also to look at external dose completeness.

MR. MAURO: Oh, yes.

MEMBER GRIFFON: And beyond '68 or whatever it is. So there's two other parts of that.

MR. MAURO: I only -- I was talking milligrams per liter, but yes, the other data would be there, too. All the data would be there.

MS. BALDRIDGE: Putting people in lists, categorizing them -- I know they can't -- You can't just use one category to determine what their dose was, because they were stepping out of that role or they worked in production, and then they moved into machining, and then they moved into something else, and the records don't necessarily show that.

So to say -- Who has determined the model to say that inspectors have the least amount of exposure or that another group of workers had an exposure greater than someone else, when they are crossing those boundaries?

MR. ROLFES: That is true, but the bioassay results would have integrated all previous exposures and be reflected in a person's urine.

MS. BALDRIDGE: That is assuming that the people who were examined -- that their data was accurate. Now in my father's case, it was not included in his dose reconstruction that he had worked Production before Plant 6 was in operation. He was not dosed for UF-6 which resulted in the renal damage, which that issue is still kind of floating around and how renal damage causes a retention of salts which may affect the excretion levels in the uranium analysis.

Those issues have not been addressed and established in such a way as to validate the use of the uranium urinalysis in 100 percent of the cases.

When I asked you if you could distinguish which workers potentially had renal damage whose results could have been compromised because of it, you said you don't know. That's not in their records.

So do you go back and look to see who might have been exposed to something that

could have caused renal damage and question the validity of their uranium urinalysis? I mean, this process -- you know, the little bit of research that was presented in the document covered two people in China, each of whom had one exposure.

You have people who are working day after day after day in an exposure level, and there is no data, there is no research, there has been no study to determine what effect that has had on their --

MR. ROLFES; I think we presented at a previous Working Group meeting that there were autopsy studies for people that were highly exposed in the uranium production facilities. That was the important thing that was of concern for uranium exposures in general across the United States at the DOE facilities.

There were several studies that were done. I don't have them right in front of me. I've got them somewhere in my box of

records here, but that was one of their concerns, to determine how much uranium was being retained within an individual's body after chronic exposure.

There were some individuals that had excreted -- I think some of them were up near 100 milligrams of uranium in urine over their working history.

There were kidney sections taken and also tissue samples taken of some former workers to determine if they could discern whether this individual had renal impairment or kidney damage based on the chronic exposures, and they were unable to determine which individuals had been exposed to uranium versus those that had not been.

So it basically found that there was no differences from the normal population for those individuals who were exposed to uranium versus those who were not.

MS. BALDRIDGE: Well, the study they did with 17 men at Fernald, and all 17

had renal damage. My father wasn't one of those 17, and who knows how many other men were damaged that were not included in the preliminary determination that that event had even occurred.

When I spoke to the researcher, he didn't even know that there was a group as large as 17 people to search out the documentation for the study.

MR. MAURO: I might be -- During the last Work Group meeting -- It turns out, in preparing for this, I very carefully read the transcript, and in my write-up I could actually read to you where we came out at the last meeting, because a great deal of discussion was held on that subject. Let me just read it to you, in response to this concern, which had a lot of discussion.

"NIOSH indicated that reports addressing these incidents" -- these were the 17 incidents -- "were placed on the O: Drive. Further discussion was held regarding the

group of 17 workers that were exposed to high levels of UF-6 at Fernald pilot plant in August 1966."

So there was some discussion here. Anyone who wants it can go into the record. It is there. Bottom line, though, is NIOSH stated that follow-up investigations of the urinary excretion patterns of these workers did not reveal any unusual excretion patterns, excretion patterns meaning the highest excretions were on the first day post-exposure. That what would be normally expected as opposed to delayed.

Then there was almost another layer of discussion. It's all in the transcript. I don't know if the transcript has been posted yet, but I have privilege to it because I was given it in advance, so I could do this. Let me read this.

"In an effort to further address this issue, the transuranic registry was discussed and the degree to which autopsy data

revealed kidney damage experienced by workers in the registry."

So apparently that was a subject of great interest in the transuranic registry.

"NIOSH cited a specific paper on this topic where no such damage was observed. The paper cited is 'Histological Kidney Study on Uranium and Non-Uranium Workers' in Health Physics, volume 70, Number 4, page 466."

Then there was a conclusionary statement, which I have in bold in my matrix that says: "THE WORKING GROUP CONCLUDED THAT NO FURTHER ACTION IS REQUIRED ON THIS PARTICULAR ISSUE EXCEPT THAT NIOSH WOULD DOCUMENT THAT WHEN A PERSON EXPERIENCED VERY HIGH EXPOSURES, THAT PERHAPS MIGHT BE INDICATIVE OF A POSSIBILITY OF KIDNEY DAMAGE, AND SPECIFICALLY LOOK AT THE BIOASSAY DATA WITH THIS PARTICULAR ISSUE IN MIND."

That is what came out of the last meeting. So that's where we are on this issue.

MS. BALDRIDGE: I would like to make an addition. In that discussion, I also brought up the point that I had personally contacted and spoken with one of the authors of the document, and he felt that his conclusions had been misrepresented by a group that expounded on that in the development of this paper.

I had used one of the footnotes, the reference document, and checked with the researcher. So when people are taking research and they are misrepresenting it to present a position that supports their own, I think that is questionable.

MR. MAURO: We just went over one of the issues that I was planning on discussing right now, regarding the kidney damage issue and its effect. I guess I will just speak for SC&A.

The place where we are right now is no further investigation on this matter as a result of the previous direction we were

given, and we have not taken any additional steps.

We are really now getting into the finer structure of issue number one dealing with uranium bioassay data. So we are into the matrix. We have sort of left -- The first subject was the sampling plan. Sounds like it is in my hands to work with you folks.

MEMBER GRIFFON: I think so, and the petitioner. To go back to Sandra's initial question, I don't think it is going to be -- It may not be perfect, but I think if we get SC&A to work with NIOSH but also with you guys, selection of the jobs that we think, like the highest exposed.

I know that is not a perfect way, because like you said, some people could have a certain job type and go many different places. But I think also you would probably have a sense of, at least during certain time periods, you know, that was a really dirty job, they were real high exposed, you know.

So we might be able to at least get some of those job types in and start that process that way. I think, yes, we should move forward with that in that way.

MR. MAURO: Fine. So that is an action item. I'm writing this down in my notes as an action item for SC&A to follow up on refining as necessary the sampling plan, and we will do that and get back to the Working Group with an e-mail shortly after we have a chance to deliberate amongst all interested parties.

I presume this particular handout that I had that was not PA reviewed -- I presume a copy of this can be made available to Ms. Baldridge. It sounds like that is something --

MS. HOWELL: There are some things that need to be changed.

MR. MAURO: Well, I'm not saying now. I'm just saying that it would be helpful for all interested parties involved with the

sampling plan to have this paper.

MEMBER ZIEMER: Only the first three pages of that, I think, are pertinent for what we are talking about. There is a page that instructs certain people to report to Medical for bioassay or something, and certainly, you are going to be redacting almost that whole page anyway, I would guess.

MS. HOWELL: I haven't seen it.

MEMBER ZIEMER: Well, the fifth page is a list of people who have been instructed to report to Medical for bioassay. That doesn't help us in any particular way.

MR. MAURO: What I guess is there is a package of material apparently that we will need to get to the claimants and other interested parties that relates to the sampling plan.

MEMBER ZIEMER: Right. The stuff you handed out, plus Hans' material.

MR. MAURO: Plus Hans'. What I will do is I will get that material into your

hands, say this is what I believe to be the material, and then you can clear it and move it out.

MS. HOWELL: And this document, we will need to have some changes made.

MR. MAURO: That would be, certainly, another piece which is separate. So that is separate from the sampling plan. That more goes to the bigger picture. That is also the matrix.

MEMBER GRIFFON: I'm sorry. Just as a point of meaning protocol, in the past meetings I have been keeping the matrix updated. I just don't see that I added in that column on that first -- As you said, I added it in October 28th, and I added in --

MR. MAURO: Well, we have it right now.

MEMBER GRIFFON: Right. I'm working on it now. I think it's a Work Group responsibility, really, to manage the matrix going forward rather than NIOSH or SC&A. I

think we should manage it, and I'll help Brad with that.

MR. MAURO: By the way, to a macro level different strategies are being used in different work groups to deal with this.

MEMBER ZIEMER: For example, the Procedures Work Group, Steve is actually handling the data, Steve from SC&A.

MR. MAURO: I am working with Steve Hedafeld.

MEMBER ZIEMER: And they are doing the entries.

MR. MAURO: But if this group --

MEMBER ZIEMER: At the request of the Chair of that work group.

MR. MAURO: Yes, yes, oh, yes, and that's fine.

CHAIR CLAWSON: Mark is helping me.

MR. MAURO: Okay. Anyway, as I said, I took it upon myself to write this big piece. It was done as much for myself as it is I thought it would be helpful to the group.

But what I'm hearing is I guess the next phase of this is in your hands as well as SC&A's hands.

CHAIR CLAWSON: That is correct.

MR. MAURO: That's fine.

CHAIR CLAWSON: The path forward now is we've got to begin to sit down with the petitioners and so forth, the other interested parties, and set up this sampling plan. We are going to change a little bit of direction there.

We've got this other issue, but right now, according to my watch, it's about time for lunch. Is that correct or am I still on Mountain Time?

MEMBER ZIEMER: It is time for breakfast where you were.

CHAIR CLAWSON: Yes. So what I was going to suggest is that we could pick up with this right after lunch, and then be able to proceed on there, if that is all right with everybody. Don't want to miss lunch.

MR. KATZ: Okay. So we are breaking for lunch.

CHAIR CLAWSON: Yes.

MR. KATZ: And we will resume at one o'clock.

CHAIR CLAWSON: Yes.

MR. KATZ: Thank you, everyone on the phone, and we will start back up at one.

(Whereupon, the above-entitled matter went off the record at 11:51 a.m.)

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

(1:02 p.m.)

MR. KATZ: Good afternoon. This is the Fernald Working Group, and we are starting back up again.

CHAIR CLAWSON: Okay. This is -- Appreciate everybody coming back in. I am going to turn the time back over to John.

I felt like -- Are we going to start with complete of one or --

MR. MAURO: No, no. We didn't even warm up to one yet. This is a macro issue related to one that really crosses all the data completeness issue.

We are really now going to get into 1, and the big picture. One is doing dose reconstruction to workers when you have to contend with some of the workers may have been

exposed to enriched uranium. Some of them may have been exposed to recycled uranium. And there was also an issue related to data quality, related to transcription of the data from the original database, the hard copy, to the HIS-20 database.

So these are the issues that fall under number 4-1. So let me -- Let's first go to recycled uranium.

After reading the transcript -- and I tried to capture it as best I could in the matrix -- NIOSH's position is that the false assumption that all workers that worked with uranium worked with recycled uranium, and please correct me if I am misrepresenting what I believe was in the transcript.

It is going to be assumed that all workers, even beginning as early as in 1955, even though recycled uranium really didn't start, I guess, in any serious way being processed until the Sixties, and you are going to start saying all these milligram per liter

uranium numbers that you have in bioassay -- it is going to be assumed that accompanying data was 100 parts per billion of Plutonium-239 and the associated other radium --

MR. ROLFES: Correct.

MR. MAURO: -- in appropriate proportions as a default assumption.

That was the position. Now that was the position that was described at the last meeting.

The mission that was given to SC&A is to take a look at that and see if that seems to be a reasonable assumption to make as a bounding assumption.

Basically, what we did is we -- and if Hans Behling is on the phone -- Hans gave me some material to hand out. It is -- I guess it is a DOE report by a fellow named Bogart, and we have extracted certain pages from it that we looked at.

I would like to hand that out to everyone. I think I have 10 copies, and again

it is not PA cleared, but it is material -- not PA cleared. What I am going to do is just draw your attention to a particular page.

I will give you some of what I believe represents the SC&A perspective on this, but at that point I would like either Arjun or Hans to help me out a bit after everyone gets a copy.

What everyone is looking at is selected pages from this report referred to as the Bogart Report, which is a characterization of the recycled uranium that -- the various campaigns and the various materials that were sent to NIOSH for processing.

I would like to propose -- Just to move through this pretty quickly to see where we come out, I would like you to first go the -- I guess it's the -- starting with the first page, the third page that says "Recycled Feeds" as the title, and also has -- On the upper right hand corner it says Page 9.

What we are looking at are some

data that represents -- The reason I bring this up is that, when you ask yourself, is 100 parts per billion a good number -- and one of the questions that came up at the last meeting is, wait a minute, what about the Tower ash, because the 100 parts per billion -- The theme went like this. The 100 parts per billion appears to be a pretty good number to place an upper bound on exposure to recycled uranium, except for Paducah Tower ash, which was a special problem.

So I guess this first page we are looking at is some of the data showing the -- I guess these were the different campaigns where ash from Paducah was sent, and what its -- If you go to the far righthand column; it's called plutonium parts per billion. Well, there is a uranium basis and sample basis.

I guess it's the uranium basis that we should be looking at from a parts per billion. You could see that there are a number of campaigns of processing that were

well above 100.

So then we will get to the 100 in a minute, but the most important point that was being made -- Everyone, I think, around the table agrees that, yes, the Paducah ash was a special issue where the level of plutonium in the uranium, recycled uranium, was unusually high.

In this case, we could see it as high as 7,000 in one particular batch, and that overall the material that came in that was processed -- and this apparently occurred in about 1980. So it was a fairly well defined time period.

So to characterize NIOSH's position on this as best I can, is that, yes, there was elevated levels, well above 100 parts per billion, but it was confined to primarily -- to entirely the tower ash that came in the 1980s, and that -- So we know when it came in, where it came in, and we also know that when it was handled, people wore respirators, which

had, I believe, a protection factor of at least about 50.

MR. ROLFES: They had airline respirators.

MR. MAURO: Airline. So it would be better than 50.

MR. ROLFES: Right.

MR. MAURO: Airline respirator will give you 1,000.

MR. ROLFES: Correct.

MR. MAURO: Good. I didn't get that. The point being, though, so by assuming everybody gets -- In other words, this does not defeat -- the argument being this does not defeat the assumption of 100, because if you apply a 100-fold or 1,000-fold decontamination factor to these numbers, you are well below 100.

So NIOSH's position, as I understand it from reviewing the record, is that we could deal with this problem, and the 100 parts per billion assumption looks -- you

know, we could live with it, because we have a way to contain and manage the people who were exposed to tower ash because of you know when it came in, you know who handled it, and you also know that they were using respiratory protection. Yes?

MR. ROLFES: John, also furthermore, there are bioassay results for plutonium for the individuals that were involved in that.

MR. MAURO: On top of it all. Okay. So on that basis, it certainly sounds to be a reasonable approach to sort of bounding their problem.

Let's go to --

MR. RICH; John, this is Bryce Rich. Could I just add one note, having dealt with the recycled uranium issue for a long time, the tower ash was quickly blended, and the 100 parts per billion was driven by a blended value, and that was available to more than the initial people who handled the ash

itself.

So just to indicate to you that the 100 parts per billion, when you consider a short exposure period and for the entire period at the site, 100 parts per billion is very, very conservative.

MR. MAURO: I understand, and we are going to discuss that a little bit, but with that said, I would like to jump to two more pages. Just flip over. On the top of the FA, you will see page 007.

Now what this shows is the average parts per billion of plutonium in uranium by year from this same report. You could see that in general what -- you know, you can look at it for that year. Any year, 100 parts per billion overall would be certainly bounding by at least tenfold.

Then, of course, you hit that 1980 year. You see you got 1,122. That harkens back to the previous table. So I mean, this page seems to support that, yes, there was

something very unusual that occurred in 1980 in dealing with tower ash, but when you get away from that particular year and the ash and you return to other forms of uranium, recycled uranium, in each year the numbers are well below 100 parts per billion.

So on first blush, the implication is it looks like your solution -- your approach to this problem is well in hand. However, in discussing this matter, the one issue that came up is -- Now let's look at, for example, -- Let's look at 1970.

We've got 1.2 million kilograms of uranium, total uranium that was processed, and the overall average for that was 3.964 parts per billion.

Is there any -- You know, what level of confidence do we have that some subset of that 1.2 million kilograms may not have been enriched above 100 parts per billion, where the idea of going back to it -- we've talked about this before -- is it

possible there may have been some people that were handling some subset of this large amount that could possibly have been above 100 parts per billion.

So it was like hidden within these averages, which are clearly by year well below 100. is it possible that hidden within some of these averages are elevated levels? Is there something about the process whereby that -- I guess this is our question -- follow-up question to NIOSH, is that: You know, what level of assurance is there that some subset of this, other than perhaps the tower ash, might well have been above 100 parts per billion, some shipment from some other facility, maybe from Hanford or some other facility that might have been relatively small compared to the total amount that was processed in that year, but could have been above 100 and, therefore, in that year there might have been some people that could have been exposed to something well above 100 parts

per billion.

I guess that is the question -- SC&A's question back to NIOSH regarding this issue. Anything else?

MEMBER GRIFFON: Yes, maybe along the same lines, but a more fundamental question. Just how was that number in the final column there determined? Was it -- Did they assay every batch that came in or did they -- you know, is this an average of two sample points in the year or is this -- I don't know. I'm just asking.

MR. ROLFES: Bryce, this is Mark. I know for the recycled uranium report, that was certainly very well studied by the Department of Energy, certainly in recent years. Do we have more detailed information by year that has plutonium amounts in relationship to the uranium content of the shipments?

MR. RICH: This is Bryce. There are more detailed reports in the 2000 reports

on recycled uranium to for my spot-on studies. Also there are reports in the plant that were studied carefully from a process standpoint.

We were careful to make sure that -- -Well, that's where the 100 parts per billion was bounding in the analysis that was done.

It is possible that for very short periods of time that some values were above 100 parts per billion, particularly with the blending operation from the particular tower ash program, but those are very short in duration and well controlled.

Then other parts of the program, including -- well, the break-out of the metal production program, the magnesium fluoride that was recovered, and there were some concentrating points in the processes, but those have been carefully evaluated for maximum concentration, and that fell below the 100 parts per billion, plus the fact that those process streams were relatively small in volume and had not -- and did not represent

the major process load of the plant.

MEMBER GRIFFON: Is that an evaluation that you wrote up, Bryce, or is that--

MR. RICH: It is summarized in --

MR. KATZ: I am sorry. Can't hear. Bryce, is there someone in the background on your phone?

MR. RICH; No. That is not on my phone.

MR. KATZ: Okay. There is someone on the line who hasn't muted their phone. If you would, please, you are interfering with the person who is trying to speak right now. Someone is still talking right now. We are listening to someone talking on the line. Would you please mute your phone, Star-6.

I'm still hearing you. It's a woman speaking. Please, can you mute your phone.

MR. RICH; I can continue, if you like.

MR. KATZ: Okay, why don't you continue, Bryce. You are much louder than she is, and it should work.

MR. RICH: I don't quite remember exactly where I was, other than to indicate that all of the available data at Fernald has been accumulated, and we are in the process of updating the test basis documents.

There has been some -- the OTIB 53 is still -- well, if it has gone through our system, it is still in final review, but that has some additional references and information related to the generic process streams, of which Fernald played a very key role, because Fernald was one of the DOE sites that pretty well handled all of the process streams that you could call recycled uranium.

I would mention one other thing. That is that there were a number of periods in the Fernald process system where they processed uranium ore, and injected that as blending material into the process stream

within the plant and, of course, that dilutes considerably.

The material that came from the generating site, primarily Hanford and Savannah River, were carefully monitored and documented. All of that material that came into the plant as primary recycled uranium was at five parts per billion or less.

They were working to a 10 part per billion maximum limit, and of course, we documented not only plutonium, neptunium and technetium as a primary recycled uranium contaminants, but the bounding levels of other contaminants as well, fission products like ruthenium and the like.

So there is a body of documentation that gives assurance that the choice of default contaminant levels is very conservative.

MR. MAURO: Bryce, a question that came up in some of our discussions is, certainly, the feed material, as you have

pointed out, controls over what was shipped, but you had mentioned -- but there is also this -- once material arrives, there is some blending which would valuate it down. But, of course, there's also other aspects to the process, which was the concentrate.

Does part of the OTIB 53 report address some of the steps at Fernald where the potential for re-concentration existed?

MR. RICH: The potential for concentration at Fernald is dealt with in a separate paper. The steps associated with -- you know, the likely concentration mechanisms, of course, have been -- you are trying to -- you run it through the liquid extraction plant, in which point you are trying to purify the uranium.

At Fernald, the purification system was not "tuned," if you will put that in quotation marks, to the recycled contaminants, but more heavy metals and other contaminants that dealt more with the neutronics associated

with the use of the recycled uranium further in the reactors, the raffinates being one of them, and we have satisfied ourselves from analyses at Fernald specifically that the -- first of all, there was an amount of uranium in the raffinates that still bounded where the 100 parts per billion was bounding, plus the fact that, of course, the operators who attended the raffinate system also attended other points in the process where the primary exposure was to uranium.

We feel like we've done an exhaustive study there, and are satisfied that the bounding will be very conservative.

MR. MAURO: Yes, I have nothing more to offer here.

MR. MAKHIJANI: John, can I make a comment?

MR. MAURO: Sure. Sure.

MR. MAKHIJANI: There is a page in the Bogart report that I failed to provide you with, and it does include the actual absolute

quantities of plutonium content that were used as feed materials by year, and it really shows that in the year of 1980, which obviously, as you already pointed out, is a high point where the total number of grams of plutonium that were part of the feed material amounted to close to 26 grams.

That towers over all other years, according to this particular figure that I am looking at. The next closest one was 1970 where the total number of plutonium in terms of grams were less than 6 grams.

So we are talking about a difference of 3.5 or close to fourfold difference between the peak year of 1980 that involved plutonium contaminated feed material versus the second next largest year of 1970 where the quantity of plutonium went from nearly 26 grams to less than six grams.

So I believe that for that year -- Obviously, that is a bounding value, but as I already pointed out, the 100 parts per billion

would appear to, obviously, cover us for all other years as a bounding value, other than 1980.

MR. RICH: Hans, if I could add just a little bit, we calculated, and it is well documented, that the relative small MAPs volume of tower ash that came from Paducah and a couple of other sites, in effect, doubled the amount of plutonium in the plant for a period of time until that worked its way through the system.

So in other words, when you were dealing with a normal input of five parts per million, the total quantity in grams of plutonium in the plant suddenly in that plant year did double. But what we have satisfied ourselves is that even in the year of 1980 with the blending and the careful control, that the 100 parts per billion adequately provide a very conservative bounding.

MEMBER GRIFFON: Hey, Bryce, this is Mark Griffon. Earlier when we had the

little interruption, I was asking: The evaluation you are talking about -- is that in this white paper that is on the O: Drive, this RU white paper?

MR. RICH: Mark, what evaluation are you referring to?

MEMBER GRIFFON: Well, your evaluation basically demonstrating that the 100 parts per billion --

MR. RICH: Oh, yes. Yes, it is summarized, that information of which you are speaking.

MEMBER GRIFFON: Has SC&A reviewed that or just reviewed the source document? I don't know.

MR. MAURO: As far as I know, the only document we looked at was the one I just handed out. We did not go deeper.

MEMBER GRIFFON: Yes.

MR. ROLFES: The recycled uranium white paper was placed onto the O: Drive. Looks like there was one that was placed in

March of 2008, it appears.

MEMBER GRIFFON: Is that -- that white paper, is that the predecessor to the TIB or, no, they are separate things? There's a TIB.

MR. MORRIS: The white paper was specialized at Fernald.

MEMBER GRIFFON: For Fernald. Okay.

MR. MORRIS: We have been hashing this back and forth for quite a few months now, and --

MR. RICH: Years.

MR. MORRIS: Years. Sorry, Bryce. I would say that there is probably not substantial differences that have come out over the years. There have been tune-ups.

MR. RICH: That is true, Bob. I would say that there have been a number of white papers dealing with the various topics that we have generated to address the Fernald issues, and -- well, Mark has a better idea of

what has been placed on the O: Drive, but --

MEMBER GRIFFON: The only reason I'm asking is because I am thinking that SC&A -- to finally close this out, I think it might be worthwhile looking at that, but I'm wondering, if this had been revised in the TIB, you might want to just look at the latest thing, you know.

MR. RICH: Then there is not a TIB. There is a TIB that is in the final phases of review, which is OTIB 53, which deals with precisely uranium complex-wide.

MEMBER GRIFFON: So this has more Fernald specific information.

MR. RICH: Yes. There is a white paper that has been developed since.

MR. MORRIS: It would be a significant review effort to do a TIB 53. You may want to refocus on --

MEMBER GRIFFON; I think you want to focus on this white paper then, yes.

MR. ROLFES: This white paper is

titled "White Paper: Dose Reconstruction Considerations For RU Contaminants at Fernald," and it is dated March --

MR. RICH: That sounds like it.

MEMBER GRIFFON: That's the title of the -- I was looking at the file name.

MR. ROLFES: The file name is RU White Paper 3308-Final Draft..

CHAIR CLAWSON: Hey, Bryce, this is Brad Clawson. Didn't we send some product out there, too, and it kind of didn't work very good, from Idaho?

MR. RICH: The recycled uranium came out of the Kent plant. There may have been a small quantity that went to Fernald, but they were very careful not to mix that material through INEL because of its very high enrichment, at 99 parts percent of it went to White Trail and directly to Savannah River to be used in -- as driver fuel, and there was a little bit that went to Rocky Flat and a little bit that went to Portsmouth. But I

don't have any records that indicate that there were any substantive amounts that went to Fernald.

Fernald really could not handle the enrichment of the product from INEL.

CHAIR CLAWSON: I just remembered, we classified it as trash, but it had enriched -- smaller enrichment of uranium, and we were trying --

MR. RICH: There could have been a -- It would really had to have been lower enriched, Brad.

CHAIR CLAWSON: More like trigger fuel that we had worked with.

MR. RICH: And, frankly, that didn't show up on any of the inventory. So if it were, it was a minor quantity and possibly used for experimentation.

CHAIR CLAWSON: Okay.

MEMBER GRIFFON: Hey, Bryce, this is mark Griffon. I think we will end up asking SC&A to look at the white paper, but

the first question I had -- or a little while ago I asked the question about the annual parts per billion, the average levels.

Do you have any sense of what they were derived from? Were all the batches in a year sampled or was this -- do you have any sense of that, how many samples went into --

MR. RICH; No, I don't have -- the information that I took came from the mass balance report at Fernald, and they extracted that information from plant data itself.

I did not go back and look at the data that was used by the DOE mass balance team at Fernald. I worked with them during the period of time that they were doing that study, and was satisfied that their work was comprehensive. but I can't tell you how many samples and how many per year and that.

I just know that there is a general knowledge base associated with what came in, even from the secondary shippers like, you know -- but the primary shippers, being

Savannah River and Hanford, injected the contaminants into the system. But of course, the material that came from the secondary sources were -- carried an additional load of contaminants, which was accounted for also.

MR. MORRIS: On that topic, my sense of it is that nuclear materials control regulations would have specified the sample protocols, and also product specification drivers may have specified that.

MR. RICH: They were -- all of the plants were working to the 10-part per billion contamination limit, even on the secondary sites, with the notable exception of those where there was a concentrating mechanism, fundamentally and primarily in the diffusion enrichment program.

In those cases, they were quite conscious of the contamination concentration and, as a matter of fact, as a side note, Fernald objected to receiving the tower ash, but the amount of uranium during that period

of time carried a value that they were directed, and consequently their concern level and the retention program associated with the receipt of that material were extraordinary.

MEMBER GRIFFON: And that is part of the reason I asked, because I also remember there was a discrepancy with the Paducah sampling of that tower ash versus the Fernald sampling, if I recall. The numbers were quite a bit different. That is something I remember during the Paducah mass balance reading.

MR. RICH: Yes.

MEMBER GRIFFON: So that is why I was asking about the sampling and the frequency.

MR. RICH: The sampling results that were used came from the Fernald sampling on receipt.

MR. MAURO: I wrote that as an action item.

MEMBER GRIFFON: The White Paper, yes.

MR. MAURO: Okay. So I will get direction from the Board. Right now -- from the Work Group -- I have identified that we are going to track down that White Paper. I didn't get the full citation, but I will get in touch with you, and we will download it.

MEMBER GRIFFON: I guess with the focus, the clear focus, being on is the 150 ppb.

MR. MAURO: Oh, yes.

MEMBER GRIFFON: You know, bounding, but also, I guess, I would look at the neptunium numbers and the other numbers for the other components.

MR. MAURO: The ratios.

MEMBER GRIFFON: It seems to me plutonium in this case is the more -- a higher dose consequence. I haven't looked at it. I don't know. I haven't had much of an opportunity, but it looks like plutonium.

MEMBER ZIEMER: Could you go ahead and give us that reference anyway? It was RU

something?

MR. ROLFES: It was recycled uranium White Paper. It was RU White Paper. That is what it is listed under the O: Drive as. Let me log back in and confirm what I've said.

MR. KATZ: While we are a break sort of in discussion, just let me remind folks on the phone, please mute your phone if you are not speaking or Star-6. There was a lady earlier who took a phone call while we were trying to have a discussion, and interfered with about five minutes of discussion. So please, mute your phone or Star-6. Thanks.

MR. ROLFES: It is RU White Paper, and the date was 3/3/2008, and -- okay. It is ruwhitepaper3308_finaldraft.doc.

MR. MAURO: Well, if I look for a RU White Paper, I'll find it. All right. I'm good.

CHAIR CLAWSON: Okay. Move on to

the next.

MR. MAURO: Move on. We are still on uranium. Now we are on the issue of enrichment. In reviewing the transcripts from the last meeting, it was NIOSH's plan to assume that all individuals were -- the uranium they worked with was two percent enriched, starting in a given year. I forget the exact year. Then prior to that, it was one percent enriched, the feeling being that, though there is evidence that there may have been some campaigns where the enrichment could have been as high as five, seven or 19.9, if I remember, which is the upper limit, those campaigns were relatively short.

So that the way I understand it when you described it is I visualize this person who is working, doing his job. Moving through his responsibility might be some material that is above two percent, but if you look at the overall year that that person was working his job, my understanding is it is

virtually impossible for his exposure to have been greater than two percent when you average it out over the total throughput that he handled that year.

Now your position with regard to that matter -- that is that, yes, there was some campaigns that were greater than two percent, but they were relatively small amounts. Any individual that might have worked on it would have been for a relatively brief period of time. Is that a fair characterization of how you came about your two percent as being bounding?

MR. ROLFES: The majority of the data that we have indicates that natural materials were processed in certain years. Depleted uranium was processed other years. Slightly enriched materials were processed.

The enrichment isn't typically given in some of these reports that you see. It just refers to the material as enriched uranium, and in other reports, though, it will

actually say the enrichment, whether the air monitoring data will have enrichment.

There is reports that indicate that these three individuals worked on a special project involving alum sodium reactor fuel elements, dissolving the reactor elements. I think the enrichments of those elements were five percent and 6.5 percent. There's actually corrections to the mobile in vivo radiation monitoring laboratory data for those individuals that show that the specific activity of the material that they were handling was different than the norm. So they made adjustments to the maximum permissible lung burden for the higher enrichments to ensure that the people didn't have greater than their control levels.

There is documentation of the different enrichments. The majority of the enrichments, as we have been discussing, enrichments that were handled were typically very low enrichments, slightly above natural

uranium, and some of the common enrichments that were used at Fernald, for the Hanford pile and for the Savannah River site typically were less than two percent.

Some of the material that they received from those sites, slightly enriched, about .8 percent versus the .71 percent U-235 content.

The two most common enrichments for Hanford, for example, that Fernald received were .947 percent and 1.25 percent. There was some 2.1 percent produced for the N reactor as well.

There is quite a bit of documentation on enrichments, and so I think what we've got --

MR. MORRIS: We do have interviews of people in charge of the blend-down process that you may be referring to that, when batches of -- small batches of products that would come in, they were immediately blended into a process stream at a certain point in

order to, as they call, sweeten the enrichment.

It was always a planned operation. It was a formulary that was done, and it would be a dilution of a few kilograms into a much, much larger volume with a known outcome that would come out at one of those pre-specified enrichments there.

MR. MAURO: When you folks described that to us during the last meeting, one of the things we were asked to do was to check -- I think there was a particular report by Dolan and Hill. I think that's one of the reports.

Hans had taken a look at it. Basically, I think where we have come away from this is that we absolutely understand and agree that on average assuming two percent for everybody is certainly an overarching conservative assumption. But we are asking ourselves the question: Is it possible -- I always come back to this circumstance. Is it

possible for some relatively short period of time -- say one year, two years -- where a person comes in and he works for a couple -- let's say we have a person that works there for 10 years.

In our mind, over that 10-year period, he is going to average out well below two percent enrichment. But we have a person who comes in, and say he just works there for a year, and he is brought in to handle a particular campaign, and it happens to be that person's job to work with the seven percent or five percent. I don't know if that happened or not.

So we were looking at the Dolan and Hill report from that perspective. Is there any information that will give us some level of assurance that that is unlikely that anyone, for a protracted period of time -- say a year or more -- came in, did that work, and then basically if we were to do his dose reconstruction based on his milligrams per

liter, you could underestimate his dose?

It turns out -- and Hans, if you are on the line --

MR. BEHLING: Yes, John. Regarding the Dolan and Hill report, I did look at it, and there is very little, if any, quantitative data. In fact, in Section 5.2.1.1 in the TBD where we talk about the uranium enrichment, Dolan and Hill did cite it, but it is also acknowledged that -- and I quote -- as another point of reference, in 1951 to '68 history by Dolan and Hill, 1988, of the average uranium enrichment collected discharges, it basically says there is no data really that is documented.

I went through the report. It is a 100 page report, and there is really no reference at all. In fact, there is only one statement here that I can quickly read to you, and it basically says that, in order to convert from microcuries to grams of uranium, the source of all radiation measured by the

continued -- this was assumed to be normal uranium. This assumption is reasonable, since the plants process large numbers -- large quantities of depleted uranium and lesser amounts of low level enriched uranium.

That's the only statement I found in the report, and it is certainly not a quantitative statement.

MR. ROLFES: Okay, thanks, Hans. I wanted to call everyone's attention to the sample dose reconstruction that we've put to address this specific issue.

On the Advisory Board's document review folder, we have placed the FMPC internal number 14 sample dose reconstruction, which illustrates the methodology that we would use to reconstruct someone's internal exposures to enriched uranium.

The alum sodium reactor fuel element processing is described in the sample dose reconstruction briefly. This was placed onto the O: Drive back in October of 2007. I

think we had discussed this at several working group meetings in the past. I just want to make sure that we have considered that information before we go into any new information.

MR. MAKHIJANI: Also, John, can I interrupt. If you go back to my original write-up of the SEC review under finding 4.1-4 that starts on page 30, I did, in fact, identify and quote a number of memoranda that make reference to enrichments that range from three percent to five and up to 10 percent in the time period of 1968 to currently.

At least in that memoranda there was a reference to enriched uranium that ranged values up to 10 percent. Then I don't know if any of the other records would support that, but I guess they were concerned about criticality, and in anticipation of highly enriched material, they were obviously concerned about reconfiguration of various containers that were used to process this

material.

So 1968 seems to be a target year for enrichments that well exceed the two percent that is currently assumed.

MR. ROLFES: Right. There are exceptions to our defaults, and it appears to us that they were documented very carefully, because that material was controlled differently than the normal products in Fernald.

Let's see. I wanted to make a correction to what I said before. The alum sodium reactor fuel elements had enrichments of 4.9 percent and 6.5 percent enriched uranium U-235.

Like I said, we have described how this is an exception to the normal work that was done at Fernald, and used this individual's whole body counting data and the documentation of the uniqueness of his exposures in our assessment.

MR. MAURO: Mark, we have no doubt

that, if you know the individuals that are dealing with the enrichment and the level of possible enrichment, you can reconstruct a dose.

I guess our question goes toward that -- you know, you wrote that, when you come across a dose reconstruction, is enough information available to you that you know the people that you should be treating as a five percent enriched or a seven percent enriched. The ones at two percent are going to be down there.

So yes, we would certainly agree that if you know who the people are and what they were handling, such as you make reference of this alum sodium reactor and other aluminum form of reactor -- I don't know if that is the same material, but in any event, sure, you could compare your milligrams per liter.

Our concern is are there any surprises? Are there people that might have gotten it, and are the records that you are

dealing with such that you have a level of confidence that you could parse that, make that distinction?

MR. ROLFES: Right. And I think we had addressed this previously as well. I don't have a date in front of me, but I do remember specifically going through some of the mobile in vivo radiation monitoring laboratory results that we have and pulling out the ones that had notations of enriched uranium or special projects, things like that.

We also did the same thing for thorium workers. I believe those two finals were put onto the Advisory Board's document review folder.

MEMBER GRIFFON: The question is: Is that inclusive? Is that some people that were monitored or is that every person?

MR. ROLFES: It would certainly be the ones that were highest internally exposed. Based on the information that we have, the mobile in vivo radiation monitoring laboratory

results, the people that were most highly exposed had the most routine schedule for counting.

What we found is that some of the people that had the higher exposures were counted sometimes more often than twice a year.

MR. MAURO: That's a chest count.

MR. ROLFES: Chest counting, right.

MR. MAURO: Now that started in '68.

MR. ROLFES: That's correct.

MR. MAURO: Now I guess my --

MEMBER GRIFFON: Just to follow up on that before you go, even if that is the case, then how do you -- there could be some people in those areas that weren't the highest exposures, but still got exposed to that enriched -- that different enrichment level. Right? So how do you identify those people, or do you just figure two percent will cover it, is what I'm getting?

MR. ROLFES: Well, for example, like Plant 9 was the enriched uranium handling plant after it produced thorium. It was basically handling some of the higher enrichment materials, and by higher enrichment I am talking just slightly enriched, even generally above .71 percent.

Let's see. How would we handle?

MEMBER GRIFFON: So does everybody in Plant 9 get a higher assumption on this?

MR. ROLFES: Everyone across the site is being given the higher default.

MEMBER GRIFFON: Given the two percent?

MR. ROLFES: Right. Right.

MEMBER GRIFFON: But nobody is getting higher than two percent?

MR. ROLFES: Except when it is documented in a report like we have for the alum reactor or when there is a note on the individual's mobile in vivo data or there is a different ratio of U-235 to U-238 for total

uranium in the individual's lung count results. So that piece of evidence would allow us to --

MEMBER GRIFFON: That would trigger something else. Right.

MR. ROLFES: Right.

MEMBER GRIFFON: And the two percent -- I think you answered this already, but your example with the two percent, that is your default across the site?

MR. ROLFES: That's correct. I have to take a look at the specific year. I believe it is after 1965.

MR. MAURO: There was a break point, I forget the date, but there was a break point, and the rationale for the break point is provided. My question -- I guess I was less concerned with chest count, more when you start with milligrams per liter.

MR. MORRIS: But there is a lot of information in the chest count record, because they did make specific notes on files when

there was reason to believe that the work assignment had them in a different location.

They are so consistently done that they are notable.

MEMBER GRIFFON: It is more than a question of coverage. A lot of times those lung count programs, they do sporadically choose some workers.

MR. MORRIS: It wasn't sporadic.

MEMBER GRIFFON: Again, it is identifying the people.

MR. MORRIS: We have a health and safety manager's memo that came out probably beginning in 1968, and then again a few years later. It actually specified the exact criteria for why somebody would be pulled into the chest counter.

For example, they pulled in all the thorium workers initially, but then there was also a logic chart about who would be counted and why and how often.

MR. MAURO: For pre-'68, though,

would I be correct in assuming that it is process knowledge that would allow you to parse who to go to treat special?

MR. MORRIS: I think that is fair.

MR. MAURO: And the process knowledge is available to you?

MR. MORRIS: We have quite a bit of interview information that tells us when and where that a sweetening was done.

MEMBER GRIFFON: But my concern -- and maybe I am just restating the same thing I said, but my concern is not as much the process knowledge but connecting people with the process knowledge. It seems clear to me in the database and in some of the interview records and stuff like that that we have, that is not always intuitively obvious, how you do that.

You don't have building information. A lot of times you are dealing with survivors. So you don't know, that is my question, is could someone be in those special

campaigns and not have been sampled at all, and you may never -- you know.

MR. MORRIS: Special campaigns, at least my perception of them from the interviews and the reading I have done, is that they were really brief. Now they would have been, you know, go get this three-kilogram batch of five percent uranium and mix it into this vat today. That would be like a two-minute job to do the injection of the new material into the big batch.

It wasn't like weeks and weeks of working with that material.

MR. MAURO: It sounds to me like it's not so much process knowledge. It is knowledge that the campaigns were so short lived that, when you average that --

MR. MORRIS: Mark and I have not talked this out.

MR. MAURO: I know, and that certainly sounds like a reasonable answer, as long as there is reason to believe that you

didn't have processes that went on for a year.

MR. MORRIS: Right.

MR. MAURO: And if you have records where you could say, yes, it does not look like we have any evidence that there was five percent, seven percent, 19.9 percent process for an extended period of time -- and if turns out when it --

MR. MORRIS: I didn't see anything that would suggest that, John.

MR. MAURO: Okay.

CHAIR CLAWSON: So where are we at on this issue then?

MR. MAURO: Well, we are at the last item under what I call the uranium internal. Give me one second, please.

MEMBER ZIEMER: Well, were you going to close this item?

MR. MAURO: Right.

MEMBER GRIFFON: I mean, I would say, short of information showing that there was more extensive use of higher enrichments,

I am comfortable with it. So yes.

MR. ROLFES: I found a reference here that I can mention, from 1975, February 10th, on permissible lung burdens. There is attached to this individual's mobile in vivo results, there is some indication that he was working with higher enrichments. It gives the necessary adjustments for specific activity and its effect on the maximum permissible lung burden.

I know that there is a document -- I don't know if it is this one -- that has descriptive information about the process that was going on, what plant that was done, and in this specific case it lists four specific individuals that were working on this campaign.

Let's see if there is any additional information.

MR. MAURO: Duration.

MR. ROLFES: That is what I am looking for.

MR. MAURO: You've got like a weight of evidence argument, I would say. Not only do we believe they were short, we actually have a case where we have a person where we know and we can see how long he worked at that level, and what his annual enrichment exposure might have been.

We know that for some period of time, it might have been above two, but it would almost be like a case study where, when you annualize it over the full year he might have worked, it effectively comes down to an average of less than two, and that would be sort of like a case study that demonstrates your argument.

MR. ROLFES: When you look at the actual enrichment information presented in the mobile in vivo results, this individual had participated in some of the campaigns where there was 6.5 percent enriched uranium that was handled. I could take a look at the average enrichment that he was exposed to in

his in vivo data. It shows roughly .5 percent enrichment for 1970, .64 for 1971, .79 for --

MR. MAURO: That shows an example of demonstrating your case that you just made.

MR. ROLFES: So everything -- and those are all -- typically, the first two measurements were less than an actual. So roughly depleted uranium that the individual was exposed to, and then slightly enriched material in 1972, which was .79 percent average. Subsequent to that, it was 1.5 percent enrichment, and in 1974 was 1.3 percent.

MR. MAURO: Also, even this fellow wasn't exposed at anytime to something above two percent.

MR. ROLFES: It is possible that he was exposed to the 6.5 percent enrichment. However, he wasn't solely exposed to that.

MR. MAURO: I'm sorry. You do information it was at some point prior to the process. Okay, good. I would say, that is

sort of why it reinforces the position you are taking.

CHAIR CLAWSON: I guess my question is: So on this one, what do we want to do? Where do we want to proceed? Are we satisfied with that?

MEMBER GRIFFON: That is my -- I'll restate what I said, is that short of -- you know, with the assumption that these were short campaigns, I think that I'm okay with this two percent exception, and if we don't find any other evidence, then I'm okay with that. Paul?

MEMBER ZIEMER: That would make sense to me. Basically, every case you have seen, they have simply blended the higher enrichment stuff into the system. Is that right?

MR. MORRIS: That was the ordinary process.

MEMBER ZIEMER: That was the practice, you're saying.

MR. MORRIS: Yes. They had depleted uranium. They would sweeten it up to whatever the enrichment of slightly enriched was, if they needed it.

MEMBER GRIFFON: Your sense is that at least some of the people that were involved in the blending operation were sampled, because you've got data to that effect.

MR. MAURO: Yes.

MEMBER ZIEMER: And the blending wouldn't take that long.

MEMBER GRIFFON: A short time, and Mark just gave us an example.

MR. MORRIS: The blended -- the higher enriched material was very valuable. So it was Fort Knox kind of stuff, and they would bring it in and add it to the large volume.

MR. RICH: This is Bryce Rich. One further note to what Bob indicated is that the use of what I will call the higher enriched stuff was very carefully controlled from a DOE

accountability standpoint. Some of the material -- they were not allowed to use it for blending and, matter of fact, sent it off site without using it in the process.

MR. ROLFES: As I recall with some of the individuals, if they were to use a higher enrichment than a given amount -- and I don't recall the enrichment -- they had to get a specific approval from DOE headquarters in order to do that, because it would basically be throwing money away.

So if there was an exception, it was very infrequent. The typical enrichment that they would use would be close to the other material. They wouldn't try to blend 90 percent enriched uranium with depleted uranium to try to get five percent, you know.

MR. RICH: Particularly if they didn't have 90 percent on site.

MR. ROLFES: Exactly.

MEMBER GRIFFON: And I think the other -- I think also on the other side of

this is the two percent, given what I have read, what we know about the site. Well, you guys know more than I, but it looks like a lot of it was depleted. Overall, the average was probably much lower than two percent.

So I'm fairly comfortable. Given that the campaigns were short with the higher stuff, this is more than a reasonable approach, to me.

CHAIR CLAWSON: Is that -- I would say it's closed unless something else raises its head there. So is everybody in agreement?

MR. MAURO: We'll move on -- like a field goal, a red flag after a play in football. I have one more -- as I mentioned very early on, we have five major areas of concern. We are about to deal with the last item in the first area, and that has to do with what is called a finding on the 4.1-2, and I will read the issue, and you will know what I am talking about.

The questionable integrity of

fluorometric urinalysis -- the concern is that there are numerous statements in memos and from all the records that the bioassay data cannot be reconstructed -- cannot be used to reconstruct doses. This is a data quality problem.

Earlier during the previous -- much earlier meeting, a year ago, the October 2007 meeting, Mark had described in considerable detail a special study that was performed and put up on the Web related to data validation of the data. I guess it is to confirm the reliability of the data.

We did not discuss this issue. We never got to this issue, and -- let me see if I got this right. Oh, I'm sorry. I take it back. Give me a second, one second.

Oh, okay. In your review, you made reference to a military spec for doing these kinds of statistical -- you know what I am referring to now?

MR. ROLFES; I think so. That was

for the HIS-20 comparison.

MR. MAURO: Yes. That's what this goes to, I believe. That's how I happened to mention it.

MR. NETON; I don't think so. My recollection was that the argument was that the data couldn't be -- there were memos saying you couldn't use the data for internal, because the old ICPR-2 model weren't valid.

MR. MAURO: So there's two problems. Yes. You are absolutely right. Now as I'm reading my notes here, there was two aspects to it.

One was what you just mentioned, and I think the general agreement at the time was -- well, no, no. The point was -- in our critique Hans made mention of numerous places in some old records and says you can't use this data for dose reconstruction. But upon our discussion, that statement was made, because at the time we did not have --

MR. NETON: The knowledge of

histodosimetry was not as fast.

MR. MAURO: Right. Now that you do have the biokinetic models -- so, yes, I think we put that to bed.

MEMBER ZIEMER: And it wasn't the fact that it was fluorophotometric data.

MR. MAURO: There was no way -- at that time there was no reliable way to connect the urine analysis data to -- with inhalation and the subsequent dose.

MEMBER ZIEMER: I thought we had closed that.

MR. MAURO: Right. I'll just jump to the other item, which was not closed, and we were given some marching orders, and that has to do with -- we were asked -- in that quality assurance investigation that you folks performed, I guess, and looking at how faithful was the HIS-20 database, you sampled from 1950s, '60s, '70s and '80s.

I guess you sampled from the original data, hard copy data, and compared

those hard copies against the data that was loaded into the HIS-20 database, and you summarized in some great detail in the transcript your results, identifying where there were matches, where there were some problems, and you had a lot of statistics describing the quality of the -- or the degree to which the material was transcribed.

SC&A was given a marching order in light of that, and I will read it: It says, "A discussion of the types of disparities that were observed in your work indicated that some were more significant than others. For example, in some cases it was simply the misspelling of a person's name would be considered to be an error, and it was scored as such. In other cases, it might have been a more severe, serious discrepancy."

The work group did ask us -- you may take a look; I have it here -- an action item was suggested to have SC&A look into this matter and then hold a separate telephone

conference call related to the nature of the places where there were some disparities. That was the marching orders that we were given.

I do not believe we ever held that conference call.

MEMBER GRIFFON: Can I ask a question about -- I was looking through that HIS-20 data comparison, and the hard copy that you judged HIS-20 against were these analytical datasheets.

My question was: Obviously not or you would have used them, but I guess I will ask it anyway. Were there any laboratory logbooks available?

MR. ROLFES: I think you had asked that last time.

MEMBER GRIFFON: I think I probably did, yes.

MR. ROLFES: And we did look, and I don't recall seeing any.

MEMBER GRIFFON: So that is the

most primary source you could find.

MR. ROLFES: Right.

MS. BALDRIDGE: I have a question. Were those entries made by name, by number?

MR. ROLFES: What entries? I'm sorry.

MS. BALDRIDGE: Into the database from the old records, because I was going through my father's papers from National Lab. He had probably three different ID numbers assigned.

MR. ROLFES: The HIS-20 data --

MS. BALDRIDGE: So, you know, I'm sure two of them were errors, but still if some of his records were listed under the wrong number --

MR. ROLFES: The HIS-20 database has Social Security number. I can open it up and explain what we have within the database and then see if I can get a copy of the hard copy data that we used to compare to HIS-20.

I don't know if Gene Potter is on

the line either. He would probably be able to answer right away. Do we have Gene Potter on the line? Doesn't sound like it.

MEMBER GRIFFON: I think we should plan this technical follow-up call anyway.

MR. MAURO: So I will take that as an action item. Those calls we didn't do last time, we will do it now.

CHAIR CLAWSON: Okay. Also to be able to address your concern of how they were entered in from the hard copy, be it name or number.

MR. ROLFES: What we had, I believe, were the -- in the early days, this is the one that I pulled up. The New York Operations Office, Health and Safety Division, had done some of the initial bioassays for the Fernald workers. They would collect urine samples and analyze them at the Health and Safety Lab.

What we did is compared scanned images of those -- I'm trying to recall. I

think they were like an industrial hygiene sheet, like a data sheet with individuals' names, and I forget all the other parameters that were on there. But it typically would get a name and the bioassay results, in addition to the date of the analysis and the data collection.

I would have to take a look back to see what specifically is in there. Let's see. The HIS-20 comparison has been out on the O: Drive since March 10th of 2008 as well.

MEMBER ZIEMER: What specifically is going to be the nature of the technical call?

MR. MAURO: Well, right now my understanding is that there were certain disparities in the numbers, where there were errors --

MEMBER ZIEMER: The database versus the original sheets?

MR. MAURO: Yes, the original sheet. It sounds like there's two aspects to

it. We have the original sheets that were just handwritten hard copy, and --

MEMBER GRIFFON: Not handwritten. Typewritten.

MR. MAURO: Typewritten? I haven't seen them, but they are hard copy, and they were transferred into the HIS-20 database. Now originally there was a concern of how faithful was that transcription, and there was a report given on that.

There were a number of findings, and they were complex. It wasn't that it was a simple -- in the minutes, if you read the minutes of the last meeting, how they characterized the different kinds of findings -- there were all different types, and some were more important than others.

I guess at the time the work group felt that we would like to hear a little bit more about the nature of the disparities, the extent of those disparities, and for SC&A to look at those disparities and say, okay, we

understand how the work was done -- in other words, how the sampling was done. They sampled, basically, and based on that sampling scheme, which followed a certain mill spec for doing these kinds of samples, using that mill spec they were able to make certain judgments about what number of errors there were. What is the percent of errors.

In other words, out of the thousands and thousands of numbers that were transcribed, what percent contained a type one error, type two error, type three error, type four error, and of those, which ones were the more serious.

In other words, have SC&A look at that a bit.

MEMBER ZIEMER: This is something NIOSH has already done.

MR. ROLFES: We have already done that. I can get the executive summary, if you like.

CHAIR CLAWSON: You've already

evaluated the database in the manner described. So you are just saying let's take a look at what they did and make sure we are comfortable with their analysis. Is that what we are talking about? Sounds like it's already been done.

MR. MORRIS: Brad, if I might offer a suggestion, I would suggest that the charter you give to SC&A would be look at the errors that were identified and see if they might have an impact on a co-worker study, because that really is the big picture for why you are doing it.

CHAIR CLAWSON: Well, if you remember right, when we got into this last time, the mention came up there were so many discrepancies. But if I remembered right, it wasn't broke down, and it's like what Mark -- you know, some of them were just a misspelled word or so forth like that, and many of them were into this.

This is why we tasked SC&A to look

into that.

MR. MORRIS: Because it would be easy to come to a conclusion that there was a failure of the dataset for some reason that didn't impact anything you cared about.

MEMBER ZIEMER: Sure.

MR. MORRIS: But, you know, if you are really focused on what do you want the dataset to do for you, accurately reflect population doses or accurately reflect the individual doses or whatever the goal is, don't get sidetracked by looking for some trivial error that really is not important.

MR. MAURO: It goes to the DQO argument, which I completely agree with. But when you are doing your work, figure out what it is you are doing and why you are doing it.

MR. MORRIS: Because we scored it fairly hard. I mean, if there was a mistake, it was a mistake. But is it an important mistake is the next question.

MR. ROLFES: I can present the

executive summary. It should only be a minute.

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

An accepted sampling plan was developed using a statistical method known as sampling by attributes. Hard copy records were acquired independently during data capture trips by members of OCUS and the ORAU team. They consist mainly of analytical datasheets, urine request cards and annual urinalysis summary reports.

For this study, 33 electronic files scanned from hard copy bioassay results were examined. There were eight files which were primarily subcontractor or gross alpha and beta results. These files were eliminated, since they would not affect the co-worker

study for feed materials production center employees.

Twenty of the remaining 25 files met the criteria selected. Five files did not meet the criteria, but were unlikely to result in anything that became changes to the co-worker study, if the data missing from HIS-20 were to be included.

Overall, approximately 90 percent of the data was matched, with only a few files accounting for the majority of the results that were not located in HIS-20.

MEMBER ZIEMER: I think that is very helpful, and from my perspective it would be fine if you had your statisticians look at that design and see how we agree with that design, and then look at the conclusions and see if you agree with those or if, for some reason, you think that design is completely -- I mean, I can sit here and hear it, and it sounds good, but I don't now if you had enough samples or not.

MEMBER GRIFFON: I have a question about the part you left out, that it wouldn't affect the -- you have already excluded some data, because you said it wouldn't affect the co-worker model, but we are not only concerned about the co-worker model. We are looking for systemic problems within the data, I think, aren't we?

You excluded -- I missed it -- I'm sorry -- when Mark was reading this -- excluded gross alpha/beta. Reread that.

MR. ROLFES: I can reread that, but this was done for the co-worker study to determine whether the uranium bioassay data was sufficient to generate a co-worker model.

Yes, these files were eliminated -- let me get back to that. For this study, 33 electronic files scanned from hard copy bioassay results were examined. There were eight files which were primarily subcontractor or gross alpha/beta results. These files were eliminated, since they would not affect the

co-worker study of Fernald employees.

MEMBER GRIFFON: And you were doing that, because that was your purpose. I got it, yes.

MR. MORRIS: Yes. We wouldn't want alpha/beta -- gross alpha/beta, because we were looking at a different set of -- we don't care.

MEMBER GRIFFON: But our objective is maybe a little farther than that.

MR. MORRIS: Could be. I'm not suggesting that -- you just need to figure our why you want to do this, because you will find things like that in here that we scored as failures that may not be a failure from your perspective.

MR. ROLFES: I can break down those 33 finals. I've got some of the data here as well. It's a 15 page White Paper, if you are interested in looking at it in more detail. But I believe we were asked to select various decades for the study.

So what we did -- let's see. For the 1950s we selected 16 files. For the 1960s, we selected seven files. For the 1970s, we selected five, and for the 1980s we selected five. So that gave us a total of 33 files which were evaluated.

MR. MAURO: Are these people?

MR. ROLFES: No, these are files, like they could have 200 pages of different individuals listed and multiple urine samples in there.

MEMBER GRIFFON: And was this selected -- I mean, did you weight that for a reason or was it just that there are more reports available?

MR. ROLFES: I think a couple of years back there was some concern that some of the individuals in the earlier days might not have been monitored as frequently or that the correct people might not have been monitored.

MR. MORRIS: Also, the shift from the New York Operations Office monitoring to

the on-site monitoring. So those were the key areas that we started to focus.

MR. ROLFES: And also, the Fifties, we would be relying primarily on uranium urinalyses to complete those reconstructions. We have additional information. We would have air sampling data, if necessary, back then as well, but in the Sixties, that is when the mobile in vivo unit came to the site. So we have another piece, essentially a confirmatory piece of data to use in addition to the urinalysis results.

So we've got two different data sources that we can use to do a dose reconstruction. So I think that was -- there was a gradual change to, you know, do fewer sampling or fewer files to sample in the more recent time period.

MR. MAURO: Should I move on?

CHAIR CLAWSON: If you would. I was just going through some of the data here and stuff like that, looking at some of what

they have. But go ahead and go on.

MR. MAURO; Well, we have basically concluded the first major set of findings related to basically uranium bioassay samples.

Now the second finding deals with the K-65 silos and exposure to radon and exposure to, I believe, some -- there was some discussion of radium and radon emanation.

We never got to that subject in the last meeting on March 26th. We skipped over it.

MEMBER GRIFFON: What are you on?

MR. MAURO: Oh, I am on Number two. I am on my big matrix.

MEMBER GRIFFON: That's 4.2?

MR. MAURO: I'm on 4.2. It's on page 12.

MEMBER ZIEMER: K-65 wastes and raffinates.

MR. MAURO: Right. Now previously this just had a single page on 4.2-1 and -2. There are a couple of issues, and we never got

in the last meeting to this, because we went on to 4-3 which deals with thorium, I believe. So we never really got to 4-2 in the last meeting because of the overriding concern regarding reconstruction of doses to workers exposed to thorium-232, especially during the years pre-1968 when you were basing --

At that time, if you recall, at the last meeting, it was an important meeting, because we spent most of our time discussing the daily weighted exposure data.

So I guess I have a question for the work group at this time. One of the action items that came out of the last meeting which was very important was that NIOSH would look at the download and report on the daily weighted exposure data for 1955 and 1966 for all buildings as being a way to demonstrate to the work group that we have a very complete daily weighted exposure dataset.

If you recall, at the time of the meeting there was a nice matrix table that was

handed out that had by year, by building, and there were little dots in each box, and each dot represented a report, and you have a total of 160 such reports.

It was all agreed that, if you really have lots of daily weighted exposure data for everyone of those dots, in theory you could build a co-worker model for everyone of those boxes or some -- when you didn't maybe aggregate, but the point being that you had such an abundance of daily weighted exposure that in theory you could build one or more sets of curves representing different time periods, different worker type, different buildings; and once you have that distribution of daily weighted exposure, you then have the luxury to select the upper -- for any given worker, you have the luxury of selecting the upper 95th percentile, the median, maybe a lower end value.

In other words, from an SEC perspective, if you have that data the way it

was represented at the last meeting, you were in a very strong position to say you can reconstruct doses with sufficient accuracy.

MR. MORRIS: The normal reason to do that would be for thorium, though.

MR. MAURO: For thorium-232 only. See, the big problem previously was you were going to go to 1050 MAC --

MR. MORRIS: That's right.

MR. MAURO: -- MAC continuous exposure. For 2,000 hours per year, people were always exposed at 1050 MAC to thorium-232. We had a lot of criticisms of that, and that goes way back. Then --

MR. RICH: John, I am a little confused. I thought you were discussing radon.

MR. MAURO: No, no, no. I'm sorry. Let me clarify.

What I'm saying is we have -- right now we have one set of issues dealing with the K-65 silos and radon, and there's a bunch of

issues there. Then we have -- I'm going to call that issue number two and all its sub-issues.

Then we have issue number three and its all sub-issues, which deals with thorium-232 exposure. All I was trying to explain, and we sort of got down the road a little far, was that at the last meeting we elected to jump over the K-65 silo set of issues and go to issue number three, 4.3, which deals with thorium-232 and the daily weighted exposure.

At that time, an action item at that time was given to NIOSH to look into that data. I just wanted to point this out to the work group, that we have a choice now. We can continue and go in sequence and go on to issue number two, which has to do with the K-65 silos and the radon issues and other matters, or we can jump over that and go to the thorium-232 issue, given that it is already 2:30.

So I guess we will do whatever you

folks would like to do.

MEMBER GRIFFON: I guess it depends on how much material we have to discuss. I'm not sure we have -- do we have any new information on either one of these topics? I think we are waiting on the thorium.

MR. MAURO: Will you mind if I ask, did you folks have a chance to look at the 1955 and '66 dataset?

MR. ROLFES: No. That was the only thing that I was aware of that we owed the working group on Fernald. That was the only open issue that NIOSH needed to produce something for the Advisory Board. We still owe that.

MR. MAURO: Okay, then you know what? Then there is no reason to go there.

MR. ROLFES: Right.

MR. MAURO: Let's go hit number two.

MEMBER GRIFFON: I agree.

CHAIR CLAWSON: But we've got it

documented that that is still -- right?

MR. ROLFES; Yes. Yes. I believe we've got the White Paper written that will allow us to assign the intakes from the daily weighted exposure results. We had some internal comments which we've resolved. They are in the process of resolving, and then this second step that we would need to take is to download that data or populate that data into our exposure model to determine what the intakes would be for those plants for those years of 1955 and 1966.

MR. ELLIOTT: Mark, the product that we owe is a White Paper?

MR. ROLFES: Correct.

MR. ELLIOTT: And that is in review, you think?

MR. ROLFES: That's correct.

MR. ELLIOTT: So it's imminent, I would say.

MR. ROLFES: It is just really final comments. We have some comments on the

statistics that were used, and I think we have pretty much resolved the path forward, and it is just a matter of documenting them in the final product.

MR. NETON: That would have been a while ago.

MR. ROLFES: Yes.

MR. NETON: I remember reading it.

MEMBER GRIFFON: There is a sub-folder on the O: Drive that says exposure studies. Is that where that is going to end up?

MR. ROLFES; Yes.

MEMBER GRIFFON: Because it's blank right now. Okay.

MR. ROLFES: Yes, with an empty folder.

MEMBER GRIFFON: That's where it will be. All right. We'll keep an eye out.

MR. ROLFES: It's a placeholder.

MR. KATZ: You will send out a notice. Right?

MR. ROLFES; I sure will.

MR. MAURO: We are on 4.2-1. This has to do -- fundamentally, it has to do with the K-65 silos, raffinates and radon exposures, and there are three sub-issues, and I guess we could take them one at a time, but in concept. I always like to look at these things almost like a picture.

You know, what you have is workers that either loaded the silos and, therefore, were exposed to these raffinates, which had high levels of radium and thorium in it, and the question becomes how are you going to reconstruct the dose, internal doses, to those workers? That's an important category of concern.

There is also the issue, and very important issue, that Hans had brought up in one of his analyses, is the radon emanation rate from the K-65 silos. The issue has to do with a specific model that was done by John Till's organization, I believe, where the way

in which the exhalation rate of radon from the silos was modeled. It was by a diffusion model which took into consideration the breathing rate, so to speak.

As the temperature and pressure changes outdoors, you get a delta P across the cap, and you get exhalation, and you came up with a source term for radon. I remember the number will being about 6,000 curies per year per silo, or maybe on that order.

When we reviewed that -- let's go down that road. Let's talk radon. There are other issues, but we might as well go with that one first. That's the one that is freshest in my mind.

When we reviewed that, we first said let's see if we can get the same number you folks got, the 6,000 curies per year number by running our own diffusion models and transport, using the diffusion coefficients and looking at the literature and all that, basically checking the work done by -- I think

it was Till and Meyer -- or, no, John Till and Rack who did it, I guess, as part of the dose reconstruction for Fernald, off-site dose reconstruction for Fernald.

So we had our folks that do that kind of analysis look at that, and the bottom line is you have about the same number. The 6,000 looks good.

At the same time, Hans was looking at it from a completely different perspective. We like to try to come at problems from other directions, and he said, well, let's look at the inventory of radium-226 and its progeny based on the sampling that was done inside those silos, pulled samples, lots of samples.

It turns out there was a deficit of lead-210 and polonium-210. In other words, the progeny of radon were lower than what we expected it to be. In other words, if there was full equilibrium between the radium-226 and all its progeny, you would expect the inventory or the concentrations in the silos

to be more or less the same. By and large, the radon, most of it, stayed --

MR. NETON: The radon didn't go anywhere.

MR. MAURO: Didn't go anywhere. You would get equilibrium.

MR. NETON: That can happen.

MR. MAURO: And we know that can happen. So Hans went through a set of calculations. Hans, I'm stealing your thunder, because it's a good story.

MR. NETON: And you can do it.

MR. MAURO: Bottom line is Hans came up with -- based on the deficit, came up with a number 10 times higher, 60,000 curies per year being the exhalation rate.

MR. NETON: Well, it's nice to say that. We would like to see the analysis

MR. MAURO: Oh, no, no. Hans, was that written up? Are you on line?

MR. BEHLING: No, it is just basically a back-of-the-envelope calculation,

but it really links the data in Table 5-16, isotopic composition of K-65 silos 1 and 2, and there you see the silo 1. The radium-226 activity was 477, and for the lead-210, 202.

So you realize you are way out of equilibrium, and that deficit has to be accountable by the escape of radon-222. That's the only explanation you can have, assuming these numbers have merit.

On that basis, I calculated the loss of about 60-some-thousand curies per year for silo 1 as an average value. I don't have the numbers in front of me of the calculation I did on a back of a scrap piece of paper, but it is clearly a value that does not jibe with John Till's numbers.

MR. NETON; Hans, is it not possible that radon decayed in the head space? I mean, you are assuming that, since it is not there, it all went out the stack, but you've got a head space there that holds up the gas. Do you think that --

MR. BEHLING; Well, it is possible, but for the majority of years there was no cap on those silos, meaning that the head space was free to vent into the atmosphere. I don't remember the year that there was a cap put on top of the silos, which would make that argument a moot argument.

MR. NETON: Well, the silos I've seen have always had caps on top of them. They weren't open structures.

MR. BEHLING; If I recall, the caps were put on very late, maybe Eighty.

MR. NETON: Well, we need to look at it.

CHAIR CLAWSON: Excuse us. Did someone try to say something?

MR. BEHLING: Arjun, go ahead.

MR. MAKHIJANI: I think Jim and Hans are talking about two different things. There is the vent cap over the --

MR. NETON: That was inside the dome itself.

MR. MAKHIJANI: Yes, inside the dome, and what Jim is talking about is the head space.

MR. NETON: They weren't open cylinders, Hans. They were closed domed structures. In fact, that was -- the bentonite clay cap was added to retard the migration of radon out of the material itself.

MEMBER GRIFFON: There had to be a diffusion -- we need to see -- submit something.

MR. MAURO: I think that is a wise decision. It sounds like SC&A needs to submit to the work group the calculations of the deficit, and then -- and take a look at it and see if you -- we don't want back-of-the-envelope.

MR. NETON: The head space concentrations were horrendous in those silos.

MR. MAURO: I think we have that data, too, because that is what --

MR. NETON: Right, but that needs

to be considered in the compilation. Yes.

MR. MAURO: Yes, and the end result. Very good. That's a good point. So, Hans, we are going to put together a White Paper, and factor in the measurements -- because they had to make those, because the way John Till did the work is he took samples from the head space, and then owing to delta P and diffusion coefficients through the cracks, he had a breathing rate.

Now you are saying that that may be where the radon is. The deficit is --

MR. NETON: There is a huge equilibrium concentration of radon in the head space.

MR. MAURO: But wouldn't that -- where then --

MR. NETON: Oh, drop on top of the dome, played out on the inside.

MR. MAURO: So in other words, the deficit may be that it is not -- the polonium and the lead may not be where you think it is.

For example --

MR. NETON: Well, the radon is going to migrate far from its site of origin. It is not going to stay there.

MR. MAURO: Right. I remember they described a sample of it. I think they took cores that went across and down, and pulled the sample and got the concentration; because there was some kind of a -- it wasn't necessarily uniform, because what they were trying to do is get an inventory.

So we are operating on the premise that the inventory inside the box, inside the silo, has a certain number for radium, and a number for the progeny is much lower than we thought it would be.

MR. NETON; I'm saying it is not uniformly distributed inside that box.

MR. MAURO: Right. But the intent of the sample --

MEMBER GRIFFON: Taking into account the headspace.

MR. MAURO: Taking into consideration, okay.

MR. NETON: I don't know if that's half or not, but I think that is something that needs to be considered.

CHAIR CLAWSON: Will SC&A write something up for the work group and NIOSH to be able to review?

MEMBER GRIFFON: Can I ask? I think we got to pay attention to speaking one at a time, but can I ask: In the matrix on this, Mark, 4.2.1, 4.2-1, whatever, in the NIOSH response there's a couple of things that indicate that you were going to provide more information. I just wanted to follow up on that.

The first one says provide radon breath data. Raffinate air data is being assembled into a spreadsheet. Is this completed? I guess it comes up in the later -- or in the earlier action. I don't know.

MR. MAURO: Yes. You just moved on

to the second.

MEMBER GRIFFON: Oh, okay, I'm sorry.

MR. MAURO: No, that's okay.

MEMBER GRIFFON: Well, then the last thing, it also talks about more data being gathered. So we will get that in the next item, I guess, but I just wanted to make sure we didn't miss any NIOSH actions.

MR. ROLFES: Right. Like I said, to the best of my knowledge, we have provided everything that we have been asked and put it onto the O Drive. The radon breath data are there on the O Drive under breath radon bioassay, and I believe there is data here for 1952, 1953 and 1954.

Let's see. The raffinate air monitoring data: There is air monitoring data in the daily weighted exposure reports as well. I know some of the thorium air monitoring data that we had entered into an Excel spreadsheet did contain some raffinate

air data, but ultimately we have radon breath data, which would have recorded an individual's exposures to raffinates.

MR. MAURO: And as you know, we did not discuss this issue at the last meeting, and SC&A has not taken -- I have in the records that, yes, the way to deal with one of the problems, the raffinates, is you could estimate radium intake based on radon exhalation.

Apparently, you put the material on the O Drive, and that's where we are right now.

MR. ROLFES: There is also -- in some claimants= files as well there is radium excretion data in some.

MEMBER GRIFFON: That was the one point I was looking at, was that after that statement -- at the very end of that statement, it says radon breath data is also radon excretion data, and more data are being gathered. I didn't understand, like what's

that mean, you know.

Everything you've found is posted there. Right?

MR. ROLFES: Right. Right, with the exception of the daily weighted exposure studies, which, for example, you know, an individual could have been exposed to raffinates in plant 2/3 or something.

Those additional reports are available in the site research database. They haven't been pooled because there's 160 of them, and I think that is why we had agreed just to do those two years, for '55 and '66. That could sort of take care of two items with one set of reports.

CHAIR CLAWSON: Mark, was there -- on site, was there any radon measurements or so forth that you know. The reason being is because in my Mound interviews there is an individual that came down that was requested by Fernald to come down and do some radon measurements.

I think we have discussed this once before, and we could never find --

MR. ROLFES: What they would do when they would collect an air sample is typically allow it to decay for, say, three days or a week to allow the radon to decay.

There are documented air samples for radon concentrations in the early years, and for example, if an individual was exposed to radon and inhaled only radon and gave a radon breath bioassay sample, they would be exhaling some of that radon that they breathed in.

What we are doing with that radon bioassay data is assuming that that radon source also had associated with it the radium and other radionuclides that the individual would have inhaled to be excreting that amount of -- or exhaling that amount of radon.

There are data. In the more recent years, I know that was one of the things that was pretty commonly done. There was a lot of

research with window panes and CR-39 track edge detectors to determine what some of the historical concentrations of radon in some of the different plants, inside and outside of the plants were done.

The Susan Pinney report that was produced and just discussed at a public meeting by a different division of NIOSH or a different office of NIOSH focused on the reconstruction from 1952 through 1989 of historical radon exposures to workers by plant, by shift. That was also information that we did consider in the SEC evaluation for Fernald.

MR. NETON: Just to clarify, those are for outdoor exposures, though, not in the plants.

MS. BALDRIDGE: Could I ask? I attended that meeting, and I asked Dr. Horning, who did the research along with Dr. Pinney, as to whether the CR-39 process of reading the radium off of the glass panes

differentiated between radon-222 from uranium and radon-220 from thorium.

I was told it did, but none of the thorium radon was included in the report. They also brought up the issue of the Q-11 silos near the production area in the center of the facility.

MR. ROLFES: Right. Those Q-11 silos were found to be a contributor as well to the radon exposures that were incurred on site, and that was considered in the study as well.

MR. NETON: Well, I can attest to what was measured, because I placed those detectors myself on some of the buildings, and the analysis that was done in England by Professor Henshaw did indeed differentiate between the daughters of thorium and the daughters of radon, radon-222.

MEMBER ZIEMER: Based on track length?

MR. NETON: Track length, yes,

because you got a very high energy alpha from the thorium. It=s like 8.78 MeV, something like that. So that was the initial question when they identified the large -- I was shocked at the number of tracks these detectors pick up on plant 2/3, I think it was, on the outside windows.

So I actually called him, and I said could this have been from thorium, and he said, no. He measured the track lengths, and they were not long enough to be related to thorium exposure.

The other thing about thoron daughters is thoron gas has a 55 second half-life. So in general, it doesn't migrate very far from the source, as like radon gas has a 3.8 day half-life.

MS. BALDRIDGE: How would you -- is there any data or comparison as far as how much thorium there was on site as compared to uranium? Thorium would have been how much of the --

MR. MORRIS: I don't recall, but I could answer that question for you.

MS. BALDRIDGE: She indicated that the reason it was insignificant was that it was less than a half of a percent.

MR. MORRIS: That sounds, by mass, about right to me.

MR. NETON: There were 15,000 containers of thorium at one time on this plant site.

MS. BALDRIDGE: Even considering it was a national repository?

MR. NETON: It was in one location stored in a building.

MR. MORRIS: Building 65. The thorium campaigns were tiny compared to the uranium campaigns. I mean, they weren't tiny by anybody's standards, except by comparison to uranium, which is, I think, what you just asked.

MR. NETON: I think we have measurements for thoron concentrations in

building 64-65.

MR. ROLFES: Correct. We do have that data for Building 64 and 65. That was the largest repository on site, and let's see. We have data. I don't want to get an incorrect date, but I know we have data from back in the large production campaign in 1954-55 from plant 9.

There is some air monitoring data where they were allowing the thoron to decay before they determined the long-lived activity.

MEMBER ZIEMER: Well, I might insert here, just as a reference point, that the process you describe is always done for air samples anywhere, as far as I know. That would not be unique to Fernald. You are going to get radon and thoron daughters everywhere in the world virtually.

MR. NETON: That's true, but it is somewhat exacerbated by the Fernald situation.

MEMBER ZIEMER: Yes, but to get the

long-lived end product, you always have to let radon and thoron decay, and you can use that measurement also to back-calculate the amounts of those as well.

MR. MAURO: What I'm hearing is that thee are two methods that are available to us to evaluate the doses to workers on site from radon. One method is apparently a lot of radon exhaled from this K-65 silos, and based on that exhalation rate -- and let's say it's the 6,000 curies per year number, as estimated by Till. Then from there, if I recall, you use some atmospheric dispersion assumptions to estimate what the airborne concentration of radon would be in the vicinity of these silos, and that would be the concentrations that would be experience by on-site workers.

I'm hearing that, independent of that, you have these other on-site measurements of actual -- as if you had some kind of detector.

MR. NETON: CR-39 detector like you

tape to the glass, the window glass.

MR. MAURO: On the buildings?

MR. NETON: Yes.

MR. MAURO: And so in effect --

MR. NETON: The glass is sort of a long term integrator of accumulator radon exposure, because you get the deposition of the daughters on the pane, and then when the ones that ejected out were going out embed the polonium-210 in the glass itself. You put these CR-39 detectors on the surface. You measure the alphas coming off that's embedded in the glass.

MR. MAURO: Now is there a way in which these two independent sets of measurements that deal with the same subject can be used to confirm each other?

MR. NETON: Well, actually, see, there was two separate source terms. There was the K-65 silos themselves, but there was also a certain amount of ore processed through the plant itself, and that is where this

analysis comes in, because it was measuring another source term.

There was another -- not to confuse terms, but a silo near plant 1 and 2-3 that contained at times the ore that was being run through the plant in the very early years, and in fact, at one point I think the -- the material was in K-65. It was actually stored on pads in these containers near plant 1, I think, the plant 1 pad.

MR. MAURO: So then these two numbers -- I was just look at the way --

MR. NETON: Yes. The rest is complementary. I mean one is the radon source term from the silos. The other one is the radon more localized due to the storage of the K-65 type material and processing.

MR. MAURO: On the pad.

MR. NETON: On the pad.

MR. MAURO: Never mind.

CHAIR CLAWSON: So --

MR. MAURO: So I see we have one

item, action item. Tell me if there is anything else. We need to deliver a report to you, Hans' calculations showing why, based on our analysis, that the 6,000 curies per year might be too low, substantially too low, and we can certainly take into consideration your recommendation to look at the head space.

MR. NETON: Yes, and I don't know that -- has the Penny Horning/Horning Penny study been incorporated into the site profile yet?

MR. ROLFES: We were in the process of revising the information, but it hasn't been released or submitted to us. We have been working on white papers, et cetera.

MR. NETON: Right. That is something that needs -- probably also needs to be looked at, because that's a separate source term that is now going to be included in the site profile.

So virtually anyone who ventured on the site during those years would be assigned

some level of radon intake, based on the Horning Penny site.

MR. MAURO: So let me see if I --

MR. NETON: We already do assign radon intakes or radon exposures, and that is documented in the site profile. But it is going to be adjusted based on new data that we receive.

MR. MAURO: So there are two major sources of on-site exposures to radon. One is the early days. There will be actual drums.

MR. NETON: Belgian Congo ore, too.

MR. MAURO: And there was ore, and that was separate. So right now in the TBD the exposures to on-site radon -- am I correct? -- are limited to this 6,000 curie per year.

MR. NETON: I believe that is correct.

MR. MAURO: Are you saying there are other sources that need to be factored in.

MR. NETON: Right.

MR. MAURO: And the Susan Pinney report is going to help with that. Okay.

MR. NETON: That is being incorporated now. It's actually in publication. It just was released in the Journal of Exposure or something, assessment. It was, I know, the NIOSH funded study, by the way. That's how I know. I was the project officer when I was over in the other division.

MR. MAKHIJANI: Can I ask a question about the radon? In the response here NIOSH says that you have radium breath data for the raffinates, and I presume that's relating from any intakes from the silos, but silo 3 had a significant disequilibrium between thorium-230 and radium.

There's a lot more thorium than radium, because it was the cold raffinate, and the radium is already gone, if I remember right. I don't have the data in front of me.

So how does radon breath data help

with that, because a lot of the silo 3 dust would not be reflected in radon breath data?

MR. ROLFES: Arjun, this is Mark.. If you take a look at the concentration of thorium-230, we are talking, in silo 3, the concentration is actually less than the concentration in silos 1 and 2, based on the actual measurement data.

So if we are using radon breath data to reconstruct intakes of the raffinates, and we are applying the concentration that is shown in silo 1 or 2 to that radon breath data, it is going to overestimate the actual exposure from thorium-230 that would be obtained in comparison to the one that we would get from silo 3.

The caution is that there is virtually no radium. There is very little radium concentration in silo 3. So the approach that we have said we were going to use would be to use the silo 1 or 2 -- I believe we said the higher of the two -- in

interpreting those radon breath samples.

MR. MAKHIJANI: But workers who handled silo 3 waste would have almost no radium. So you won't detect any radon breath in them. So unless you are talking about workers who handled waste from all three silos, I agree, and your scenario will be claimant favorable. But it would not be for workers who did not handle -- and silo 1 and 2 -- I mean, silo 1 was just for a couple of years. Silo 2 also, just in the Fifties. But in the later years you just had silo 3 being handled, and I can't see how this method can be applied to those who handled silo 3 waste.

MR. ROLFES: I would have to take a look back in our interview notes, but as I recall, workers would have worked on both the hot side and the cold side of plant 2-3. They wouldn't have worked in only one side and been solely exposed to radium depleted materials per se.

MR. MAKHIJANI: But the old

concentrates that came to Fernald were radium depleted. So the whole process that resulted in the silo 3 waste was radium depleted. The radium never came to Fernald. I stayed where the concentrates were made.

MR. ROLFES: No. We just said that 211 was processed, and that was radium bearing material.

MR. MAKHIJANI: Well, no, no, I'm not talking about the Congo ores. I'm talking about ore concentrates. Jim Neton, correct me if I am wrong. You undoubtedly know this better than me. But as I understand it, silo 3 has materials from the ore concentrates, and the radium -- ore concentrates themselves are depleted in radium rather than to thorium.

MR. NETON: Yes. I think that is correct.

MR. ROLFES: Yes. It is depleted in radium and uranium as well.

MR. NETON: But I think Arjun's point is that the material arrived at Fernald

depleted in radium. So if one worked only with silo 3 material, a radon breath analysis would not be informative of your exposure to silo 3 material. That's true.

MR. MAKHIJANI: That is my point.

MR. MAURO: But I understand -- I'm listening to this now. But you are saying that silos 1 and 2 contained radium-226 and thorium-230.

MR. ROLFES: Yes. Correct.

MR. MAURO: And thorium -- and they are in equilibrium, I guess, or close to.

MR. NETON: The equilibrium is pretty well established.

MR. MAURO: Pretty well. The ratio is pretty well established. So now we have radon breath data for people who worked with silos 1 and 2. Okay. So we could estimate the radium-226 intake for that person, and you are going to assume that the equivalent amount of thorium-230 was taken in also by that person. Okay. So that's how you deal with

the people who were exposed to radium-236 and thorium-230 at silos 1 and 2.

Now move over to silo 3. We got a problem. Let's say all it contains is thorium-230. Now if the concentration of thorium-230 in silo 3 is less than the concentration of thorium-230 in 1 and 2, you would argue that any dose you calculate for the people from 1 and 2 from thorium-230 would be bounding for the people from -- that is not correct?

MR. NETON: No, because you've got a separate source term here. I mean, you are missing -- there is no --

MR. MAURO: You just changed hats, by the way.

MR. NETON: I know.

MR. MAURO: I'm just looking for the right answer.

MR. NETON: Believe it or not, I'll say what I believe is correct.

MR. MAURO: No, I'm trying to

figure out the right answer.

MR. NETON: No. You have a separate source term in silo 3. I mean, it's an independent source term. So the amount of radium in your body is totally irrelevant to how much thorium you could have been exposed to in silo 3.

MR. MAURO: But I heard it said, though, that any estimate for the intake for 1 and 2 for thorium is going to bound.

MR. NETON: No, because you could have been working on -- it's only going to bound what was given in silo 1 and 2, but silo three --

MR. MAURO: It might have been much worse.

MR. NETON: You could have five times more thorium-230 in your body, say, for example, and not show any radium coming out.

MR. MAURO: So you're saying that workers at silo 3 could have taken in more thorium than workers at silos 1 and 2?

MR. NETON: Well, possibly. I don't know.

MR. MAURO: That's what I heard you say. I heard you say somehow you had a hook on the problem, but maybe you don't.

MR. ROLFES: I would have to take a look back. I know we did discuss this, but based on interviews, we -- Bryce, I don't know if you might be able to better answer this than I.

From my recollection, the individuals who would have been exposed -- it wasn't a separate defined process where they would do only ore concentrates at one time and radium bearing ores at another time.

From what I understood, both types of ores, ore concentrate and the radium bearing ore, would have been processed on different sides of the plant at the same time, and those same individuals, because of external exposure concerns with the radium bearing materials, would have gone over to the

cold side of the plant to work with some of the ore concentrates.

Does that sound familiar to you or am I incorrect?

That was for Bryce Rich. We don't hear you, Bryce. If you are out there, you might be muted.

MR. NETON: I still think we need to go back and look at this issue. Silo 3, if indeed it came in as depleted radium, we need to look at the process of what transpired from arrival on site to entombment, so to speak, in silo 3 and the potential for exposure and figure out what we are doing with that.

I might be missing something, but I think we need to look a little better at that.

CHAIR CLAWSON: Could we take a break for 10 minutes?

MR. KATZ: A 10-minute break.

MR. BEHLING: Can I ask a quick question before we break? This is Hans.

MR. KATZ: Yes, Hans.

MR. BEHLING: Am I to assume that we are going to assess internal exposures for silo workers on the basis of radon breath samples?

MR. ROLFES: That is correct.

MR. BEHLING: As opposed to what was discussed in the TBD, because I am looking at the TBD, and I am looking at the comment that goes as follows: If sample datasheets from 1953 time period have been found and provide insight in the operational exposures -- and that in essence was the method that was derived -- that was implied for use in dose reconstruction.

Then it goes on: The only bioassay information related to internal exposures to radium or the associate contaminants are a few radon breath samples in 1953 time period.

Am I right in assuming that you are now going to use those radon breath samples in lieu of air sampling data?

MR. ROLFES: Let me make a

correction to the data that we do have. It's roughly 200 radon breath samples per year from 1952, 1953 and 1954. I believe we have also provided a sample dose reconstruction using those radon bioassay results to reconstruct raffinate exposures.

MR. BEHLING: Are those for K-65 workers exclusively or for everybody?

MR. ROLFES: I believe there were individuals that were working with the K-65 materials and also people that were working with radium bearing ores in plant 2-3.

MR. BEHLING: Do we have a breakdown as to how many people fall in each of those categories?

MR. ROLFES: We didn't break it down. We just provided all the data that we had recovered.

MR. BEHLING: I'm just questioning how it is that in the TBD we refer to the breath analysis as a very few bioassays, and now we are, obviously, looking at that data

set for dose reconstruction.

MR. ROLFES: At the time of the Technical Basis Documents' writing and approval back in 2003, we didn't have as much data as we do now. Following as part of the SEC process, we went back and recovered countless additional records, thousands of additional records, and those additional records are now being incorporated into our files and into the Technical Basis Document as appropriate for dose reconstructions for Fernald workers.

MR. BEHLING: Okay.

MR. NETON: Break time?

MR. KATZ: Okay. We are breaking. We will set up about quarter after.

(Whereupon, the above-entitled matter went off the record at 3:00 p.m. and resumed at 3:17 p.m.)

MR. KATZ: This is the Fernald Working Group, and we are starting back up again.

CHAIR CLAWSON: Okay. John, we are going to -- are we finished with this part?

MR. MAURO: Yes. The only other point is, you know, we jumped to what I call task 3 dealing with thorium-232 based on the derived daily weighted exposure.

There were a number of issues. There were maybe seven or eight, maybe more, issues that were surrounding that. But I figure -- but the heart of the matter was the downloading of the data that NIOSH is going to do. So might as well just leave that on ice.

In other words, let's leave the thorium DWE issues and all satellite issues. Maybe we'll just move on to -- move on.

CHAIR CLAWSON: Then we have some action items, though. SC&A was going to write up the radon.

MR. MAURO: Oh, yes. Oh, yes, sure, the ones we just talked about.

MEMBER GRIFFON: Yes, we captured

those.

MR. MAURO: Oh, yes. I sort of jumped right over that, saying where do we go next.

CHAIR CLAWSON: Right. I guess one of the things -- because I've got to apologize. I got a little bit confused.

The Pinney report is in draft form.

MR. NETON: No, no. The Pinney report is complete. We are working on incorporating it into the site profile.

CHAIR CLAWSON: Okay. But could we get a -- so that when SC&A does this, I'm wondering if we could get a copy of that sent out to the work group or so forth.

MR. ROLFES: I want to take a look to see. I've got a draft report. I've got the Fernald exposure assessment and a letter with some slides as well.

Now, additionally, there is another manuscript that was submitted to a journal. So I've got that from her as well. The letter

is on the O: drive under Fernald Pinney Report from back in -- let's see -- 2006, correct. Thank you. These are from August, September and November of 2006. One is from 2004 as well, which is the date of the -- by someone else besides myself.

CHAIR CLAWSON: And that is the same stuff that is going to be implemented into that, because I know we may have a report, but I want to make sure that we are reviewing what is going to be used for the dose reconstruction.

MR. ROLFES: Sure. That hasn't been documented yet in a report that is releasable for a website. The Pinney report CADA is in the NIOSH site research database. However, we haven't incorporated that fully into the site profile yet. So as soon as that is done, we can make that available.

CHAIR CLAWSON: To SC&A and the working group?

MR. ROLFES: We can also submit the

manuscripts, if you would like, that have the raw data and the discussion of the data if you would like that.

MR. MAURO: Okay. Let me understand. Do I have an action item here?

CHAIR CLAWSON: Well, yes, you've got an action item.

MR. MAURO: In other words, the action item isn't the Susan Pinney report. It is some other report that is coming out or you will be providing?

MR. ROLFES: It would ultimately be our site profile.

MR. MAURO: Site profile? Okay. So we sit tight until we see that or do we start work and take a look at Susan Pinney?

CHAIR CLAWSON: Like I said, the Susan Pinney report, if I remember what I breezed through it, it explained quite a bit. But what I want to make sure is what we are -- the portion of it that is going to be used for our dose reconstruction.

MR. ROLFES: Sure.

CHAIR CLAWSON: So sit tight.

CHAIR CLAWSON: Sit tight. Yes, sit tight. What about the silo 3 that we discussed in there where it was storing depleted --

MEMBER GRIFFON: I have that as a NIOSH action, that NIOSH is going to evaluate the ability to reconstruct raffinates, specifically for silo 3.

MR. MAURO: No action.

CHAIR CLAWSON: Okay. I asked Jim off to the side there. There was a Jenkins report, and I was just going to see if he could find it out, because it gave a little bit of information on the radon issue. Jenkins was out of Mound. I was just going to see. If he could find it, I would appreciate it. I just talked to Jim about that. So --

MR. MORRIS: What is Mr. Jenkins' first name?

CHAIR CLAWSON: Phil Jenkins. And

this came just merely by accident, because in a Mound interview we were talking about radon at Mound, and he started going on to some information about Fernald and talked about a report that he had done down there on that. So --

MR. MORRIS: What time frame do you think that is in, Brad?

CHAIR CLAWSON: Probably in the early eighties is kind of what I got information from. And if we can't, I just may -- he just expressed that he had been asked to come down there and do some measurements inside of the plants and so forth like that, that were a little bit different. We are just going to see if we could find something on that.

MEMBER ZIEMER: Phil still lives in this area. He lives in Dayton, and he is listed in the Health Physics Society directory, if you end up needing to contact him. I'm not sure what kind of report it was.

Is it an agency report?

CHAIR CLAWSON: It was actually in cooperation with Fernald. He was asked to come down and do some evaluations.

MEMBER ZIEMER: It isn't a "Jenkins Report" is what I'm asking.

CHAIR CLAWSON: No, we are not for sure, and that is what I was asking Jim to kind of look into, because it was kind of an environmental report that he was a part of that had some information in it.

MR. NETON: It sounds like, when I was talking to Brad, that he might have been actually measuring the radon in the head space, just kind of a very specialized thing.

MR. MORRIS: I know that Grand Junction -- GJPO radon staff measured the head space radon in the mid-Nineties.

MR. NETON: I don't know what -- we could do a search.

CHAIR CLAWSON: If we can just do that, I would appreciate that, and then go

from there.

Is there anything that we have missed, because there is a lot of different things going on. Anything that you can think of?

MR. MAURO: I just want some clarification, that we do not have an action item on the Pinney report or on silo 3. We are just sitting tight until --

CHAIR CLAWSON: Correct, until NIOSH -- that is in NIOSH's court. Okay.

MR. MAURO: Okay. I am going to move on to --

MEMBER GRIFFON: Now were -- I was going to ask: 4.2-2 and -3, they sort of overlap the discussion we've had already, but I don't know if you --

MR. MAURO: 4.4 -- yes. We are about to do 4.4.

MEMBER GRIFFON: No, 4.2-2.

MR. MAURO: Oh, let me see if we missed that, 4.2-2.

MEMBER GRIFFON: It is related to raffinate exposures in plant 2 and 3.

MR. MAURO: Yes. I was assuming that the same issues -- it was the radon. There was the issue of -- I think your position was radon breath, and the question was do you have enough -- that was put to you -- enough data to be able to reconstruct the radium intake and the associated thorium intake based on radon breath data.

That would apply to not only -- to a lot of places where you had, I guess, raffinates. It had to do, I think, with the silos or with the opening. There were several places where you had that.

MEMBER GRIFFON: Let me just understand. Up here I said that there is data available, and there is a spreadsheet that has been assembled. Did I get that correct? Raffinate air data is available in spreadsheet form, or did I jump the gun on that?

MR. ROLFES: No, that was -- I

don't know if that was some sort of --

MEMBER GRIFFON: Okay, but there is radon breath data.

MR. ROLFES: I'm trying to think of an appropriate word, but I think it might have been from an earlier approach that we were going to use and like an artifact thing.

MEMBER GRIFFON: But radon breath data is -- I put radon breath data is available on the O: drive. Now is the approach -- I mean, how you are going to use that or where you are going to use that, is that outlined in that?

MR. ROLFES: The radon breath data is on the O: drive.

MEMBER GRIFFON: The data is there, but is the approach?

MR. ROLFES: The interpretation of radon breath data is documented in OTIB 25.

MEMBER GRIFFON: OTIB 25.

MR. MAURO: And we reviewed that and found it positive.

MEMBER GRIFFON: And you have looked at that already?

MR. MAURO: Looked at that. We've reviewed it.

MEMBER GRIFFON: You looked at it specifically for --

MR. MAURO: We looked at it solely from the point of view of the protocol for taking radon breath samples and converting that to whole body --

MEMBER GRIFFON: So I think SC&A needs to follow up on that for Fernald.

MR. MAURO: Okay. Now that becomes a new action item now that I did not have.

MR. NETON: Well, TIB, though, is not specific to Fernald.

MR. MAURO: No, it's not.

MR. NETON: It's a generic radon--

MR. MAURO: Correct. That's why I asked the question. It sounds like there is something that you would like us to look into related to the radon breath data as it applies

to Fernald. I'm not sure what that is.

MR. NETON: Well, I guess I want to understand -- you know, if there is no action left for NIOSH on that, you know, through the radon breath data -- In other words, if the data is there and, based on TIB 25, I should know how it is going to be applied. Is that accurate or not?

MR. ROLFES: TIB 25 would allow you to determine an individual's body burden of radium. Knowing that known body burden, you can calculate an intake of radium-226, and from that intake you can --

MR. NETON: It is a simple conversion.

MEMBER GRIFFON: But what I'm asking is what is this -- I mean, radon breath data -- is it individual data?

MR. ROLFES: Yes.

MEMBER GRIFFON: Or do you need a co-worker model for some of these others, like plant 2-3. I'm reading these findings that

says the data is inadequate.

MR. MORRIS: I'm reporting that there may have been, for 1953, maybe 50 or 60 individual results.

MR. ROLFES: Roughly 200 per year, I think, for '53, '54 and '52.

MR. MORRIS: No claim on being right on those numbers. But we then figured out -- we fitted it to a distribution and calculated percentile values that could have been used then into that TIB 25.

MEMBER GRIFFON: So you have sort of annual co-worker models?

MR. MORRIS: Yes, for three years.

MEMBER GRIFFON: And they would apply to what areas?

MR. MORRIS: The raffinates area, plant 2-3.

MEMBER GRIFFON: Anybody that was in those buildings or whatever?

MR. MORRIS: Yes.

MEMBER GRIFFON: Okay. But I think

you have to review that.

MR. MAURO: Did you want us to take a look at that?

MEMBER GRIFFON: Yes.

MR. MAURO: Okay.

MS. BALDRIDGE: I would like to make a correction here. If you are trying to find her report, her name is spelt P-i-n-n-e-y on here.

MEMBER GRIFFON: P-i-n-n-e-y, yes, I have that in my note. So the radon breath data also has the -- it's not just the raw data.

MR. MORRIS: No. It was pretty simple, actually.

MEMBER GRIFFON: Pretty simple, but, yes, there is an approach of a model.

MR. MAURO: So the action item is really -- there is a set of radon breath data, and with that data there are certain worker groups at certain time periods that you feel you can reconstruct the doses -- the

raffinates that contain radium.

MR. MORRIS: That's right. That was all focused on plant 2 and 3 and the silos.

MR. MAURO: And the silos, and that's the main areas where they could have had those type of exposures.

MR. MORRIS: Right.

MR. MAURO: And your question is check that to see how -- I guess, data adequacy. It's not the question of the conversion of radon breath data.

MEMBER GRIFFON: For data accuracy for reconstructing those populations we just discussed.

MR. MAURO: Got it. Okay. I missed that one. I'm glad you brought it up. I didn't have that.

Issue number four -- five issues; we are in the home stretch. Number four: This has to do -- this was discussed at length at the last meeting, and it has to do with

chest counts where you are trying to estimate the thorium-232 intake rate based on chest count.

The nature of the discussion that went forward was you are looking at specific photons, I think, from one of the progeny of thorium-232, and from that count you could estimate the body burden.

Now one of the assumptions -- one of the questions that Hans raised during the meeting was how you convert those counts to an intake rate of thorium-232 and associated doses very much depends on what assumptions you make regarding the equilibrium between the radium-232 and its progeny, because that is what you are looking at, progeny being, I guess, the radium-228 and then there is thorium-228.

MR. NETON: It's the radium-228.

MR. MAURO: Yes, that's what you are counting.

MR. NETON: 220 has got 911 KED.

MR. MAURO: Okay, and that's the one you are counting, but when you have separated -- I'm thinking about it like this. Someone just separated in your rate the thorium from your original ore, and when you do that, you've got thorium-232 and thorium-228. Okay? And the thorium-228 has 1.9-year half-life.

Now that is going to start to decay. So that's going to start to go away, and the radium-228 is going to start to come in. So it's complex daughters growing in, but the point being Hans pointed out that, depending on what assumption you make regarding the degree of equilibrium between thorium-232 and its progeny when you make the chest count, will affect the estimate you are going to come up with per dose.

The outcome of this is that, well, push comes to shove, you assume it is full equilibrium, which is your worst case condition, as opposed to the assumptions that,

I think, were made, which were not quite some other stage of equilibrium.

As a result, if was at full equilibrium as opposed to partial equilibrium, you could underestimate the dose by a factor of 2.4.

It was generally agreed now and there were also some issues regarding the way in which the counting was done. It was a thick crystal versus a thin crystal type of detector, which was potentially problematic in your MBAs.

In other words, we have a thick crystal. You might have to count for a longer period of time, which is --

MEMBER ZIEMER: I thought we eliminated that as an issue.

MR. MAURO: That's where I'm headed. I'm bringing everybody back, that there was one action item. In other words, I'm trying to bring everybody up to date.

So all of those issues went by the

wayside as an SEC issue. One, push comes to shove, you make the appropriate corrections for MBA. Push comes to shove, you make the appropriate corrections for degree of equilibrium, so you could place a plausible upper bound.

So everyone agreeing, that's what it said in the transcript. Not an SEC issue. But there was one item that was left somewhat open. I'll read it.

A review of the transcript -- this is my notes here -- a review of the transcript seems to indicate that NIOSH was asked to follow up and make a clear determination that the workers that we were chest counting and are being used to develop a co-worker model for post-1968 thorium-232 exposures are the workers that experienced the higher thorium-232 exposures.

So it is not a measure of the -- a question of the methodology. It is, when you do have your data and you do come up with your

array of intakes or exposures and you use that for your co-worker model, what level of assurance is there that you've captured the high end of the distribution?

That's my understanding of the question. That's how we left things at the end of the last work group meeting.

MR. MORRIS: What we do know is that there really were no elevated counts of -- you know, it's not like you had lots of exposures that were detectable here. So we do know from memoranda that were contemporary with beginning rollout of the mobile in vivo lab into Fernald that they identified specifically thorium workers.

Then we have found in the dataset of those first and second year of lung counting that the thorium workers that were identified really did get counted, you know, some minor exceptions of people who were no longer working there et cetera.

So if the question is focused on,

of those 50 or 70 people, were those the highest 50 or 70 people, I don't think we can ever answer that question, because the data -- the detection limits were not good enough for us to actually say there was a big pod of elevated workers -- elevated lung counts that are associated with those workers.

MR. MAURO: So, if I understand what you are saying, it is that the workers that were counted, you feel, there is a high level of assurance that you caught the higher ones. In other words, there is some level of assurance that, because all the workers that were handling and working with this material that might have been exposed were chest counted, and were part of --

MR. MORRIS: That's right. We know from some memoranda that were contemporary that they actually identified a set of workers to be early counted in the lung counting.

MR. MAURO: Okay, and it is unlikely that there could have been a group of

workers that could have gotten substantially higher exposures that were missed?

MR. MORRIS: No reason to believe that, I don't think.

MR. ROLFES: Keep in mind that 1968 was the first time anyone from Fernald ever had a chest count. There were people that were historically working with thorium that were sent off-site as early as 1960 time period that went to the University of Rochester and provided some thoron breath analyses that were looked at to determine how much thorium was in the individual.

They were also given a chest count at the University of Rochester. There were other trips to Y-12 for a select number of individuals, as well as a trip to Wright-Paterson Air force Base, and this is all prior to 1968. They range from 1960 through 1965 when some of the individuals who had been working with thorium were sent off-site to see how much thorium remained within their body.

So we do have that data. When the individuals began receiving whole body counts at Fernald, the documentation of their in vivo count data -- we found, looking through the data, that a lot of the records had notations in the top right hand corner.

What I did, I pulled all those notations. You can't see it from here, but it says "former thorium worker, former thorium worker, former thorium worker, former thorium worker." Somewhere in here there is one that says "current worker, current thorium worker." But I have put a list of all the thorium workers based on the in vivo datasheets onto the O: drive. That was placed out there in October. It was October 16th of 2007, so roughly a year ago.

Additionally, there is -- I've got some other data here that I am referring to, also from October of last year. It is a report of the trip to the University of Rochester, New York, on November 30, 1962,

with some individuals' names.

It says, "These individuals were sent" -- there's two individuals -- both of whom had been exposed to thorium in our plant non-thorium operations. They were sent to the University of Rochester for breath, thoron and total body radioactivity measurements.

In short, it says the conclusions and recommendations -- that is some meaningless background for the trip. This information -- I don't need to go through all of it, unless you would like for me to. This is on the O: drive as well.

Furthermore, we did find, as Bob alluded to, a list of individuals, and I haven't been able to locate it right here in my box of records, but there was a list, roughly a one or two-page list. Do you happen to have it, Bob?

MR. MORRIS: No, I didn't pull that one up. What I've got is a memo to all NLO employees from Heatherton. "The following is

information for the purpose of providing employees with an explanation of some aspects of the NLO in vivo monitoring program" about the body counting. And it tells who is going to get counted and why, and the thorium workers are in that.

MR. BEHLING: This is Hans Behling. I just want to make a comment, because I think this addresses the finding in 4.4-3 in my report, and the reason why I rate that as an issue, because in a given memo that was issued in '68, I believe, in a Health Protection Appraisal report, the following statement appears, and I will read it for you.

"Recent in vivo monitoring of NLO employees utilizing the IDRML indicated eight employees occurring sustaining 70 to 100 percent over permissible lung burden of uranium. A serious question has been raised regarding the validity of the job, suggesting that the lung exposure for these employees in vivo indicated a level that probably was not

expected," meaning that the use of air sampling data was used to identify candidates for high exposures which, it turns out, would not have suggested that these individuals should have had lung burdens ranging from 70 to 80 percent permissible levels.

So I think that was the reason why I questioned the whole issue of whether or not the people who were maximally -- potentially maximally exposed were, in fact, the people who were actually counted.

I think it goes to John's question.

MR. MORRIS: Well, John, more specifically in response to what Hans just said, in this Heatherton memo each employee's potential for inhaling uranium or thorium determines if and how often they will be counted.

For example, a water plant worker's potential for exposure is practically nil, and they are not included in the routine in vivo counting program. Chemical operators who work

daily with uranium or thorium have the greatest chance of accidentally inhaling these materials, and are counted at least once each year.

Workers with only a slight possibility for such exposure, such as mechanical employees, are monitored about every other year. Of course, any employee, regardless of classification, would be counted if air dust data or milling results indicated elevated exposure levels. If an employee was involved in an incident which might have caused significant exposure to airborne uranium or thorium, they would also be counted.

MR. MAURO: I think I hear where we are on this. That is, our concern was that the air sampling data alone was not necessarily a good enough indicator of who might be having been exposed.

Your retort is that, well, that wasn't the only criteria used to determine who

was going to be chest counted, and you listed a number of criteria just now which go above and beyond just air sampling.

I guess, you know, that's your response, and I don't know if that is satisfactory to the work group, whether or not that is your criteria. In other words, there was a full array of criteria, job descriptions, urine analysis, and air sampling, all of which triggered -- and incidents -- which would trigger when a chest count would be taken.

MR. MORRIS: Right.

MR. MAURO: So you are saying, given those criteria, it is unlikely that there were any or very many people who could have experienced a high level of thorium exposure and were missed by the chest count program. That's what I --

MR. MORRIS: Right.

CHAIR CLAWSON: I guess I am just kind of coming from the question. That sounds

great on paper, but was it really done?

MR. MORRIS: We looked at that, Brad, and in fact, the thorium workers were brought in in the first year when things were available to use the counter. They actually did get counted in greater numbers.

We found the memo identifying thorium workers, and we then went back into the first year of data, and they were counted, to a large degree. Most of them were counted in that first year when the mobile lab was available.

MEMBER GRIFFON: When you say thorium workers, that's different than what you just described in those criteria. They were talking about chemical --

MR. MORRIS: But there was a prior population of, you know, people that had worked thorium chemical processes in prior years. They identified them early on and said, when the lab becomes available, let's count these guys.

MR. ROLFES: The main thorium campaign at Fernald in the early days was in Plant 9, 1954-1955, and they produced roughly 30 percent of the thorium that Fernald ever produced. It was that population of workers that they focused on when they brought the mobile in vivo unit to the site.

There was a memorandum actually shortly thereafter, shortly after the in vivo unit came, saying that these were the individuals who would have had the potential for exposure to thorium. They were working in Plant 9, and they were largely unmonitored during those years.

They were concerned about the amount of thorium potentially building up or that had built up in their bodies, and they wanted to prioritize those individuals for a whole body count or for a mobile in vivo count.

MR. MORRIS: And then when you do see the logbook from the in vivo counting

laboratory data, there will be a notation on the page that says "Thorium worker." Oftentimes, that exactly matched that list.

MR. ROLFES: That document -- I scanned those lists -- that listing of in vivo count datasheets for the individuals who had that notation on their open safety in vivo count datasheet and put it onto the O: drive. It's titled "List of Thorium and Former Thorium Workers at FMPC."

MEMBER GRIFFON: And then what is the approach for other workers that were in Plant 9 during that time period? I'm just guessing that there were maintenance people in and out of there or others. These are probably the routine chem op thorium workers, but is the approach to use --

MR. MORRIS: That is where the DWE reports come in. Right.

MEMBER GRIFFON: That is covered in that part.

MR. MAURO: Well, the DWE reports -

- that was for the pre-'68.

MR. MORRIS: Yes.

MR. MAURO: But now it is all from post-'68.

MR. MORRIS: I misunderstood the question. I'm sorry.

MR. MAURO: What I thought I heard, it was post-'68, you got yourself a collection of workers that you believe may have been exposed to thorium, and you have identified them as potential thorium workers. You do a chest count. You have a dataset of those workers.

Now you are in a position where you can do dose reconstruction for those workers.

MR. MORRIS: Right.

MR. MAURO: But there are a lot of other workers that might have been working in that area or near the area that were not checked. Now do you assume that they might have gotten exposed?

MR. MORRIS: There is a good answer

for this. We developed a co-worker model off of that data, that in vivo dataset, and that you will find on the O: drive.

MR. ROLFES: It is on the O: drive, and that is titled "The Fernald Thorium In Vivo Co-Worker Study Final Draft.."

MR. MAURO: Okay. That's post-'68. Okay.

MR. ROLFES: They are on March 12, 2008.

CHAIR CLAWSON: That would be an action item.

MR. MAURO: Got it.

MR. GRIFFON: I guess, again, that's the same age-old problem, but how do you determine if someone was -- if someone gets that dose assigned.

MR. ROLFES: In the worst case scenario, you would assume that they were a thorium worker, and by --

MEMBER ZIEMER: Based on building?

MR. ROLFES: We certainly could do

that, if an individual said that they worked, for example, in Plant 9 and were not monitored -- well, let's keep it in the -- let's say in the more recent years there were some thorium campaigns in the pilot plant.

If an individual said that they worked with thorium in the pilot plant and never had a whole body count, we can say, okay, what job category? If they were in a high potential exposure job category such as a chemical operator, we could apply, you know, the 84th or the 95th percentile or whatever as well.

You can also consider an individual's external dose, the penetrating dose. Usually, those individuals have a little bit higher peak dose than standard uranium workers as well.

MEMBER GRIFFON: That is assuming the individual didn't -- I mean, we have a lot of cases that you don't have the individual to talk to. So --

MR. ROLFES: Right.

MEMBER GRIFFON: Then do you have -- I don't know if you have a detailed employment card for this site or you would know if they went in and out of those areas, or how do you -- when you are saying worst case, I'm saying best estimate, you know.

MR. ROLFES: Well, let's keep in mind -- because we are -- best estimates that we actually do, that's a very, very small population of our total claims that we can do dose reconstructions for, maybe one percent of that.

For Fernald, specifically, I really don't know of any best estimates that we've ever done for Fernald. I don't know if we have completed one, and usually we make claimant-favorable assumptions in a best estimate that are still giving the benefit of the doubt to the claimants.

MEMBER GRIFFON: Well, if you are going to make worst case for everybody, that

makes this go away, this discussion.

MR. ROLFES: I think, as Jim had mentioned earlier --

MEMBER GRIFFON: If you are only going to do it for prostate cancers, that's a different thing.

MR. ROLFES: Right. For example, for a lung cancer, if we were to use even a small partial dose reconstruction, a small intake for one year, for example, at the 50th percentile could result in greater than 50 percent. But in the dose reconstruction process, before we would even really consider thorium exposures, we would take a look at the bioassay data that we do have for the individual.

For example, we would start the dose reconstruction for lung cancer by looking at essentially all the data that we have, and then start with the bioassay data that we do have, and complete an internal assessment using that uranium bioassay data.

If it was not over 50 percent, then we would have to consider some of the other radionuclides to which the individual was potentially exposed, and the next one on the line would likely be the thorium issue.

So we could apply a 50th percentile based on the data that we do have. We would have to examine the facts of the case in order to make an informed decision.

MR. MAURO: And that is all written up in the co-worker model. In other words, is that all written up?

MR. ROLFES: The thorium intake model is presented in the Fernald Thorium Intake Model here.

MR. MAURO: The rationale for who you assigned what percentile to is described in the co-worker model?

MR. ROLFES: Let me open it and take a look.

MR. MORRIS: It's been a long time since they wrote that.

MR. MAURO: I realize you didn't memorize all 300 procedures.

MR. ROLFES: This is from January 8, 2008, the date that we have on it. Let's see.

MEMBER ZIEMER: That's awfully recent. It should be right there.

MR. ROLFES: Sorry. Sorry. Let's see, it's the thorium in vivo co-worker study for Fernald, and it's 19 pages. We go through a little bit of the decay of thorium, the selection, statistical description of the data.

MR. NETON: I guess it probably doesn't say.

MR. ROLFES: It might just call out what the specific intakes are based on the matrix.

MR. MAURO: Once you decide who you want to give it to. That's the big ticket item.

MR. NETON: Well, that's the TIB

that I just talked about earlier that refers to what job categories are given what level of exposure based on administrative, clerical --

MR. MAURO: That's an overarching philosophy.

MS. BALDRIDGE: Can I ask a question here? When you are talking about clerical or any of those people that were considered working in offices, where did the draftsperson, the draftsman or an engineer fall into, what category?

MR. NETON: It depends on where they worked. I mean, what they did. A draftsman who worked only in the non-process area would fall in the ambient exposure category.

MS. BALDRIDGE: How do you know they worked in a non-exposure area?

MR. NETON: If you don't, then they would be given the benefit of the doubt and could receive up to the 50th percentile of the worker exposed.

MS. BALDRIDGE: Especially based on the Fernald documents that state design problems, you know --

MR. NETON: Well, when it is not known --

MS. BALDRIDGE: -- maybe breaking down, where engineers had to go in and try to solve design problems to make a safer environment, they were at risk by the very exposure that --

MR. NETON: Well, when it is not known to any certainty, they would be given the 50th percentile of the co-worker's doses, but for uranium exposures, typically, most people have at least one bioassay sample a year, because for many years it was part of the annual physical, for uranium.

Now when you get in the other scenario, the thorium and radon, it's a little different issue, but when there is a benefit to conducting that procedure, that the 50th percentile would be the exposure.

Then for those who were really hands-on workers working with material, grinding, lathing and processing, then those would be given a higher level exposure than that.

MS. BALDRIDGE: You know, as I look at the lists that I've seen in some of the documentation already, it was like inspectors were at the bottom of the list. They were expected to have the least exposure when some of the processes involved taking chemical samples, core specimens of slugs. They were working on the machines to do some of this stuff. To put them not off, just counting how many cartons were leaving or how many boxes were leaving, and there's other examples.

When you list it as a category and then you are talking about assigning --

MR. ROLFES: Those are guidelines, not absolutes, for those procedures. They are a starting point, but you have to look at the entire file and look at the individual and the

interviews and the bioassay samples that were taken and the work areas that were frequented based on the job categories.

There's a lot that goes into this other than just that starting point.

CHAIR CLAWSON: Okay. Go on.

MR. MAURO: We are almost through the fourth out of five.

The last part of the chest count issue and thorium issue has to do with -- I'll read the statement, the improper correlation of the chest count at the MIVRML, whatever that stands for -- I assume that's the chest count, thorium lung count -- with the air sampling data.

Now the issue goes toward -- it sounds like you've got data from chest counts, and you also have data from the derived daily weighted exposure together, which raises an interesting situation.

You've got two separate sources, two different approaches post-1968. Now when

you discussed at the last time, and we asked, well, which one are you going to use, the answer was, well, when we have the chest count data, we use that, which, of course, is in accord with the overall hierarchy of data.

What came to mind, though, was what happens -- well, you are in a unique situation now. You could actually validate your derived -- the daily weighted exposure. In other words, you are going to be using the daily weighted exposure pre-'68 as your method for reconstructing internal doses of thorium, which -- and, you know, you are going to demonstrate to us how you do it and all the data, and that is something in the future you can put on the O: drive.

Something we didn't talk about was, when you -- post-'68 apparently you need them both, and one way to confirm that you could trust the daily weighted exposure is to show that it works well post-'68.

MR. MORRIS: One thing you could

say about that is we already know that the daily weighted exposures were significantly overestimates of the true exposure.

CHAIR CLAWSON: Compared with the lung data.

MR. MAURO: Is that right?

MR. MORRIS: We already know that, because we already know that --

MR. MAURO: That's important.

MR. MORRIS: -- there was never a counting for respiratory protection daily weighted exposure measurements, and we know that people did wear respirators. So just that fact alone would skew it.

MR. MAURO: That's where I'm heading with this. You see, the day is going to come when we are going to be looking real hard at the DWE data, and that is going to be a critical factor in terms of the SEC related issues. Can you reconstruct now?

MR. NETON: There is also very good evidence -- I think it has been published --

that the air concentration data doesn't match very well, because it is not particle size selective either. I mean, when you do an air sample, you suck in everything from boulders down to ultra-fine aerosols.

Somebody actually did a study at Fernald where they looked at -- you know, cascade of macro studies, and you significantly overestimate exposures using the entire air sample.

MR. MAURO: If that can be part of the package in terms of not only do you demonstrate that here is all the data we have -- in other words, here is all the data we have pre-'68. In other words, when you do the 1955, 1966 sample for every building and you show you have an abundance of data with which to construct -- do dose reconstruction and construct a co-worker model for thorium intake, you also could demonstrate that, and we know that when we do it this way, it is claimant favorable, because it usually always

results in overestimate.

The reason we can say that is for the reasons you just gave.

MR. NETON: Right.

MR. MAURO: That would -- I just messed up. I just went too far.

MEMBER GRIFFON: It's getting late in the day, and I can't reach you to kick you.

MR. MAURO: I'm sorry.

MEMBER GRIFFON: There's other factors. I don't disagree with what Jim said. The other factor that I looked at when we first discussed that study was do you have daily weighted averages, and in some cases for some job titles I saw, it was like a value of five in 4,000, and it was averaged to about 2,000, and that's the value that you are plotting.

So there's some -- I want to see what is happening with this data.

MR. NETON: I think you will be happy with that, because I reviewed that

dataset, and it would go into the higher end. We're not taking averages.

MEMBER GRIFFON: Okay. Anyway, until we see this --

MR. MAURO: I got carried away.

MR. NETON: A guy may have only worked that job -- you don't know how many days the guy worked that job, too. I mean, that's for a guy full time working that job one day a week, two days a week, five days?

MEMBER GRIFFON: But I also don't know how often that -- was that one day for the year, the sampling of it? I don't know.

MR. MORRIS: For highly exposed jobs, they sampled, you know, a few times during the year. That was the typical --

MEMBER GRIFFON; I haven't looked at it. So you know more than I do, but I am just saying let's wait until we see it.

MR. MORRIS: In fact, if you ever took an industrial hygiene class and the theory of how you do industrial hygiene

sampling was presented to you, this is how they did it. They really did it that way, and you would find that there was a journal of who did what, how many minutes they spent on that, how many minutes they spent on this, how long they were at work.

MEMBER GRIFFON: No, I don't disagree with it for that individual. It's a pretty good assessment.

MR. MORRIS: It's not for the individual. It's for the work, the work task.

MEMBER GRIFFON: Anyway, there is some interpretation there, because I know I have done some of these, and they are not invasive, and they are also -- you know, when you are looking over the shoulder of these people doing this, you are not sure you are getting it the way all of the work is always done, as they tell you later, well, we did it that way when everybody was watching us.

MR. KATZ: Arjun, you had wanted to speak.

MR. MAKHIJANI: Yes. Sorry, I can't hear everybody. So I don't know when exactly to speak. Thank you, Ted.

I have a question about the in vivo counter, the specific measurements you get out of it. I think you were measuring titanium and lead-212. But that lead-212, is a product of control rods, and so I am wondering how you actually reflect that lead-212 back into thoron-232 when you don't know how much thoron has actually escaped.

In different circumstances, the re-drumming may be a different situation than in processing.

MR. NETON; I think, Arjun, this is something that we need to go back and look at. I don't remember the algorithm that was exactly used, but I think it was a combination of actinium and a lead-212 somehow, and I have forgotten exactly how that was derived and what they did with it. But you're right. Thoron gas does escape from the body to some

degree, although albeit not a lot, because it's got a 55-second half-life.

MR. MAKHIJANI: Yes, but the thing that concerns me is the significant weight of that 212 -- radiological or gamma, and the emission characteristics and, you know, I don't know all the radiochemistry on the top of my head.

MR. NETON: Well, but there have been papers published on this issue, like how representative lead-212 as a measurement of thorium in the lung.

MR. MAKHIJANI: I am just thinking that the thing that needs to be clarified in this process is at what point does it affect your measurement and your attribution.

MR. BEHLING: Can I interject here?

MR. MAKHIJANI: A lot of negative measurement and a lot of measurements that seem to be below some level of detection or very low, a negative number, and that kind of concerns me.

MR. BEHLING: Arjun, can I interject, because you raised that question with me earlier. Whenever you -- obviously, the in vivo measurement chest counting tried to test both the activity associated with actinium-228 as well as with lead-212, and if you have -- you always have a pretty good understanding of what the thorium-228 is, because it is very closely always in equilibrium with lead-212, because the intervening daughters are very short-lived.

If you start out with a pure sample of purified thorium at time zero, you can reasonably assure yourself that the thorium-232 and 228 are in equilibrium. However, you won't know that, because the actinium-228 is actually going to be zero, because it is a very short-lived daughter of radium-228, which has been chemically removed.

So at times zero the only real measurement you have -- if you know for a fact you are dealing with a very, very fresh sample

and the only measurement you are going to get out of that is lead-212, because you can reasonably conclude that within days of thorium extraction that the lead-212 is once again in equilibrium with the thorium-228.

What you don't know is if thorium-228 is in equilibrium with thorium-232. So this is one of the handicaps. And of course, as time goes by, you will, obviously, have an in-growth of radium-228, which is the first daughter decay product of thorium-232, but you will not have a full equilibrium of the radionuclides you are trying to measure, 228, for about 30 years.

So there is always this disconnect between thorium -- the lead-212 and actinium-228, because they have very different relationships to their parent, which is what you are trying to measure.

At the worst condition, is approximately three or four years after extraction where you bottom out in terms of

understanding what the relationships are, and you could be off by as much as 2.3 or 2.4, and I think John mentioned that number, in underestimating your actual body burden. That's the worst it can ever happen.

MR. NETON: But, Hans, I think that is what that algorithm attempted to do, was -- you know, if you start seeing actinium-228, you know that you are dealing with an aged sample.

MR. BEHLING: Yes, exactly.

MR. NETON: And then you can correct for that, and you are right. The worst case without any correction for actinium ingrowth would be, you know, 2.3 or 2.4 or something like that. This is an issue we've just -- we talked about earlier, I think.

MR. BEHLING: Yes, we did.

MR. MAKHIJANI: The point I was trying to make was somewhat different.

MR. NETON: Yes, I know. You are talking about the escape of thoron gas from

the body, but I think that that is -- well, I could point to some papers that have investigated this, and we can shore that up a little bit.

MR. MAKHIJANI: I am just wondering how that would take into account, because there are a lot of different thorium processes, and people's thoron must be very different in different circumstances.

MR. NETON: Correct.

MR. MAKHIJANI: In your sample, they say, you know, five or ten years old. It may not have a lot of -- you know, five years old, you now have a lot of actinium, and you may not have much lead-212. So I just am wondering as to -- well, I think we need to verify --

MR. NETON; Yes. We can work on that.

MR. MAKHIJANI: Great. Thank you.

MR. MAURO: This is just a matter of housekeeping. We actually had an issue

number 4.4-5. It is the last of the 4.4 series. That had to do with OTIB-0002.

At the time of our review of the site profile and the evaluation report, I believe that -- this was not discussed at the last meeting. It was discussed at the October 24, 2007, meeting, and it had to do with under what circumstances is OTIB-0002 at play.

I think that the events have overcome us, and that is no longer an issue. I don't think OTIB-0002 is used in any respect. This is just bounding analysis.

I think at one time this default intake that is embedded in OTIB-0002 was a way that you would bound -- place an upper bound on some internal exposures that may not have been bounded for some of the exposure experience at Fernald. That was some of our concerns. But I don't think that has anymore play.

In other words, I don't think you are using OTIB-0002 for any of the dose

reconstruction at Fernald any longer, and I just wanted to confirm that, and we could then close this particular item out.

MR. NETON: Mark is more familiar with the day to day dose reconstruction, but I don't think we would use 0002.

MR. ROLFES: You know, there may have been some revisions. You know, if we issued a program evaluation report for Fernald and we had basically reviewed the dose reconstruction -- I couldn't really answer directly.

I don't recall off the top of my head. We could find out.

MR. NETON: We could take that as an action item and verify.

MR. MAURO: That was your last action item. In fact, at the end of the meeting a year ago, it said: NIOSH response: A formal PER is performed for previously denied claims -- if you denied a claim based on this 0002. This would be done after the

site profile revisions have occurred.

So apparently, as I understand it, this comes right out of the matrix after we had a meeting a year ago.

MR. NETON: We need to look at that, because I am not aware -- I'm sorry.

MS. BALDRIDGE: I think that applies to my father's claim, because I was challenging the use of OTIB-0002, which is time sensitive, and the revision -- or the original OTIB-0002 had a time restrictive application that you had to have been hired in 1969 or later.

OTIB-0002 was applied to my father, who retired in 1964. The use of that also did not dose him for the uranium hexafluoride, and did not take into account the thorium that we later discovered in Plant 6.

So my question to Mark at the time was: If this document is time restrictive, how do you justify using it to do a dose reconstruction for someone who doesn't fall

under the limitation parameters of the document, and he told me sometimes it is necessary to do dose reconstruction.

I thought, well, I thought the law had the stipulation that if NIOSH didn't have the data, the information that they needed, then it was to be referred to the Department of Labor who would, in fact, then tell you to go ahead and apply for an individual SEC based on the fact that there was no documentation.

MR. ROLFES: I think the concern was that, because the TIB 2 at the time had a restriction in there that limited its usage to 1970 forward -- I think we've revised that to say that that can be used for earlier time periods with specific justification for a claim.

MS. BALDRIDGE: It was revised, but the document that was used for my father's dose reconstruction was not the revision. It was the original that still contained the time restriction.

MR. ROLFES: I don't want to speak about your claim, but --

MS. BALDRIDGE: Since OTIB-0002 came up, that's why.

MR. ROLFES: Okay. As we indicated, though, it has been revised to basically explain that it can be used with specific justification on a case by case basis. I believe that is documented in there. Is that correct, Jim?

MR. NETON: I don't remember now. I can't recall, but I'm concerned about the PER that was talked about, because I don't recall us doing a PER for 0002.

MR. ROLFES: What I was going to explain is that, if we had a dose reconstruction that was completed using TIB 2 and we had a program evaluation report that was issued, we wouldn't change the methodology. We would likely use TIB 2 once again, if we had to reevaluate the claim. But I'm not sure of that.

MR. MAURO: Maybe I can help a little bit. It was my understanding that TIB 2 was used for denials. In other words, it was, across the board --

MR. NETON: Overestimate.

MR. MAURO: -- overestimate, just to put the value, internal dose, now. And then subsequent to that, we did some reviews of TIB 2 in the default set of radionuclides and intakes embedded in it, and it was our finding that, when it comes to Fernald, that may not be bounding..

MR. NETON: Yes, and I think what would happen was, if we did a review of a case, and it came back through, it would not be TIB 2, because that was written at a time probably when the site profile for Fernald had not been completed or something of that nature; because if there is a site profile and there are prescribed approaches to doing dose reconstructions, we would always default to the site profile. But early on, when we were

doing dose reconstructions, we believed that you could deny cases using the TIB 2, because it was bounding. Apparently SC&A had some issues with some of the --

MR. MAURO: Some concerns, right.

MR. NETON: -- some of the approaches that we used. But I'm not sure where we are at with that, other than the fact that I don't think TIB 2 would be used currently.

MEMBER GRIFFON: I guess, if NIOSH ended up agreeing with SC&A that it is not bounding for Fernald, that TIB 2 is not then a PER, that would be done.

MR. NETON: Yes, that would be the case, but see, I don't know that we --

MEMBER GRIFFON: But I don't think we are that point.

MR. MAURO: We have just left it off, the same place we left it off a year ago.

MR. NETON: Have we responded to the TIB 2 review yet or is that still in the

Procedures Group process? I guess that is the question. I don't know where we are with the review of TIB 2.

MR. MAURO: I don't know.

MR. NETON: We would have to look at that. And you're right. If the Procedures Review Group made a determination, we agree that TIB 2 is inappropriate for certain cases at Fernald, then a PER would be issued. We would go back and, more than likely, it would be not -- all those cases --

MEMBER GRIFFON: I don't think the Procedures Review Work Group would look at specific sites. Wouldn't that be deferred to this group to see if Fernald --

MR. NETON; Yes, but where did the analysis of TIB 2 against Fernald cases come from?

MEMBER GRIFFON: I think it came up here, didn't it?

MR. MAURO: Yes, we have this as a finding. We can go back; we could find the

history of it.

MEMBER GRIFFON: And I think this is something that we got to say, you know, does NIOSH agree with SC&A's finding or no.

MR. MAURO: And we have not responded to that, apparently.

MR. ROLFES: I am trying to think about some of the claims process. You know, for example, if we have an individual who has bioassay data, and that individual's bioassay data are largely unremarkable, has no greater than the detection limit results, TIB 2 would be a bounding approach, and that approach would typically be used for a non-metabolic organ -- for cancer of a non-metabolic organ, for prostate cancer.

MR. NETON: I am not sure about the nuclide mix. I think we need to go back and revisit it.

MR. MAURO: Well, fission products. Reactor mix, you name it.

MR. NETON: I thought it was

uranium.

MR. MAURO: There were two mixes. There are two mixes, yes.

MR. NETON: But I am not certain whether it incorporated some of the issues that we are talking about today, like the thorium and then the radon and that sort of thing. We need to go back and read it. Sorry for our response.

MR. MAURO: We are on the last finding, 5, dealing with external dosimetry. Let me see where we are on this. Give me a second. I didn't think we would get this far.

In fact, from the --

MR. MAKHIJANI: John, did you deal with the Parker Report?

MR. MAURO: Oh, yes, thank you. Thank you. There are a number of findings on what I call the external dosimetry, 4.5-1 through 4.5-5.

The first one has to do with quality assurance. Namely, we had a finding

that questioned the program -- the quality assurance of the program, the training of the workers in terms of NIOSH being in a position to put out quality data related to externa dosimetry.

The response at the time by NIOSH was, well, Herb Parker put a report out that demonstrates that the quality data is okay. Hans, in fact, you may want to take it from here. Hans faxed me last night the Parker report, and I have it here with me.

I have to say that it doesn't seem to be fully responsive to our concerns. Hans, maybe you want to just summarize, because I read it last night quickly. Would you mind just summarizing why there is some residual concern?

MR. BEHLING: I guess the most important issue here is that the Parker report is dated 1945.

MR. MAURO: Yes.

MR. BEHLING: And it really has

three sets of dosimeters that were being evaluated for three different laboratories. To some extent, they really assessed the method by which these dosimeters were irradiated against a known exposure dose and then, obviously, processed and assessed for how was the response compared to the known exposure.

While there were differences, they seemed feasible. I just don't know how the Parker report really addresses the issues that were raised in Section -- in finding 4.5-1 which talks about the fact that there were no standard operating procedures. The individual who processed these dosimeters was a person who had no really formal training or qualification.

There were issues associated with the handling of dosimeters and even maintaining dosimeters. In some instances, they were left in cars which were overheating in summer months and so forth.

Basically, what I was raising is the question that we don't have really quality assurance data that says this is the protocols that were used.

In today's world, we would have qualifications regarding the persons doing the work, the methods used to process the doses, the films, in terms of developing the film, the use of control badges which my write-up says they didn't use control badges to assure that each time when the batch, film badges, were issued and then returned that they were essentially done correctly.

It was just basically things that, I guess, in a 1980 assessment were identified as efficiencies, and that is where I raised it, and again in light of the Parker report, I don't see anything here that I consider relevant in addressing those issues.

MR. ROLFES: All right. I am searching for some information. I know we have addressed this previously, but I will

have to take a minute to see what we provided in response to this.

MR. BEHLING: And let me -- while you are looking, Mark, let me just make a couple of comments.

Oftentimes the justification saying things are okay just based on the use of a specific film dosimeter, that they have used a Dupont 508 film, etcetera, but that is really not necessary. The only criterion for judging the performance of a dosimeter in many instances, if you are talking about a film dosimeter program that was handled in-house, the real critical issues that have to be addressed is what were the doses for radiation exposures in developing a dose response curve? Was that properly done? Were there specific procedures in place regarding how the film was developed; that is, the chemical methods for developing the time, the solutions, the temperature of the solutions, etcetera, etcetera.

Those are other critical variables that go above and beyond the type of film that was used in that timeframe.

MR. NETON: And I would say that we must have some information on this. This is Jim. I have not looked at this in much detail, but I know that they had calibration curves of all those films going way back, but we will have to look -- rely on Mark.

MR. ROLFES: Yes, I will have to take a look back.

MR. NETON: I agree with you, it is more than just what dosimeter was used. But I know -- I recall -- I think those data still are there. I mean the actual pieces, the calibrations, film and the stuff, were still available a long, long time ago.

MR. BEHLING: Yes. If you look at the finding 4.5-1 in my report, these quotations come out of an assessment fact sheet that was dated September 11, 1981. So as late as '81, obviously, they were still

suffering from certain deficiencies regarding personal film dosimetry programs.

MR. NETON: I need to go back and refresh my memory of what was said there, because I remember these comments now. These are actually site profile comments, I think, from a long time ago.

MR. MAURO: They may be carried over.

MR. NETON: I think that is reasonable.

MR. MAURO: Right.

MR. NETON: I'm just stretching to try to remember. We spent so much time on internal that I've forgotten what we've done in the externa area to address those issues. We may have to get back to you on this.

MR. ROLFES: The only thing I am seeing that jumps out at me immediately is the FMPC external dosimetry program quality assurance manual that was placed onto the O: drive August of 2007, and let's see, it was

during the Westinghouse Materials company years, roughly 41 pages. There is some description of the luminescent dosimeters that were issued at Fernald.

MR. NETON: That is later.

MR. ROLFES: Right. Right. I do see the Herb Parker analysis that is out here.

MEMBER ZIEMER: The Parker analysis probably precedes any commercial film badge work. They had to be doing in-house stuff or using Oak Ridge's system or something.

What you would be looking for, I guess, would just be some -- I mean, there were no national intercomparisons or anything, as far as I know. So you would be looking for what standards did they use to calibrate and the variables that Hans mentioned, which can affect the darkening of the film, and then the development process.

As far as qualified people, probably all the lines probably were returning people as they were. Forty-five would have

been just as the war ended, and in the first couple of years of that.

MR. NETON: Fernald didn't start until '52. I think the Parker report was kind of going to what Hans said, that eliminate the dosimetry stuff.

MEMBER ZIEMER: Yes, yes. Okay.

MR. NETON: But the actual processing -- and you're right. In the 1950s there were no standard operating procedures like we would consider today that are controlled and reviewed and that sort of thing. But I recall us pulling out at one point descriptions of what was done, and we need to piece that back together again, I think.

MR. ROLFES: I am looking through what I've got. I'm not seeing anything. We will take a look back and see what else we have received from our data captures that we've done since the SEC evaluation and make those available to the Advisory Board Working

Group as well.

MR. MAURO: If I may move on, issue number 5-2 has to do with something we have not discussed since a year ago. We did not discuss this matter in the March 2008 meeting. It has to do with doses to extremities.

Now basically, we have some tables here on how was that monitored. Now let me preface this. Hans pointed this out to me last night.

I don't know if there's too many people that are claimants that have cancer of the hands or fingers. So maybe this is not -- I don't know the degree to which it would be considered an important SEC issue. But the data we do have -- and I will hand this out, not PA cleared -- is basically a summary of the number of individuals that had extremity monitoring as a function of the year.

As can be seen, it was not until the 1980s when there are -- that people really started to have extremity monitoring. The

early years are very limited.

The importance of this, you know, I will leave it into the hands of the work group. The reality is that's the data as best we can capture it, and it definitely shows that the number of -- the amount of extremity monitoring was extremely limited in the early years.

MR. BEHLING: And then let me also add to that, in looking at this data that you probably don't have access to, but I have given it to John and he may show it to you on a personal level, because it does contain names of individuals. So we are not privileged to hand it out. But the doses in the early years, beginning in the very early years, for those people who were monitored was quite substantial.

We have doses, yearly doses, up to 33 rads, as well as high as even 55 in one case, 63 in one case, to extremities. So the doses were very, very high, and the number of

personnel who were monitored for extremity exposures in the early years was very, very marginal.

You can see the rise, the dramatic rise, in numbers from the handout that John gave you. So the question that was raised in my finding is that -- and I back that up by a couple of in-house memoranda that talked about the need for further monitoring among people who were currently not monitored, and that was the genesis of the finding, that according to some of the interoffice memos that were cited in my report, there were substantial extremity doses among a handful of people who were monitored, but it was also a matter of fact that there were other people who had finger exposures or likely finger exposures who were not monitored.

I think this table demonstrates the limited number of personnel monitored during the early years when, in fact, the exposures to extremities were substantial.

MR. NETON: I agree with you, Hans. It seems like it would be very straightforward to develop some sort of a ratio of shallow dose to extremity dose, you know, the beta dose. There's ratios that can be developed based on the monitoring data we have.

MR. BEHLING: In fact, that ratio is given in my write-up. In fact, they made comments regarding that ratio of external whole body. Obviously, your chest badge will pick up also a skin dose and, of course, that can be compared to perhaps the wrist badge that was worn by those individuals.

So you can, obviously, draw some correlation.

MR. NETON: Yes, you can draw a correlation, and then you would only assign that dose to people that -- you know, if you had a substantial shallow dose to the chest badge, then you could apply the ratio to the extremity dose.

So I think this is what John might call a tractable problem.

MR. BEHLING: And I would recommend using the chest badge beta dose and use that as a way of correlating the potential exposure for extremity dose.

MR. NETON: I agree with you. Well, that is assuming we can get through this QA issue that we were just talking about 10 minutes ago, though.

MR. MAURO: The next item I have here -- and I am going to B- again, I've got to punt to Hans. It has to do -- and I'm not sure if this is -- it has to do with unmonitored shallow and deep dose resulting from skin/clothing contamination.

This subject was discussed a year ago, the 10/24/07 meeting, and the work group basically asked NIOSH to examine whether -- in other words, how to deal with the fact that there might be clothing contamination, and, as a result of that contamination there is a beta

dose associated with that, and that that may need to be taken into consideration. That is the fact that people are wearing contaminated clothing.

Then the work group, based on that concern, which is described in our report -- and Hans, if you would like to describe it further, but that there may be some need to -- the work group said NIOSH will examine whether an adjustment is necessary.

MR. BEHLING: If I can just make a comment again. If you do have those who have access to the original review of the SEC petition report that I wrote, finding 4.5-3 pretty much gives you a background against which this issue was raised.

That is, apparently as of even 1985 the Fernald facility did not really monitor people for skin contamination, clothing contamination, by having portal monitors, as you would in today's world.

So people may have been

significantly contaminated, going home with that contamination, with the exception of some people who were, I guess, expected to shower at the end of the shift. Others were, obviously, not.

So the issue of skin contamination is a significant potential for large doses, given what we already talked about, that involves extremity skin exposures. But in addition to those doses, you may have had skin contamination that would have continued to expose an individual for, obviously, longer periods of time other than the dosimeters that you wear would indicate.

That is, if you are not one of these people who thoroughly scrubs your scalp every day by washing your hair or necessarily taking a bath every day, you may have long time skin exposures associated with contamination, skin contamination, and/or clothing contamination, clothing worn for days on end, possibly.

It is just an issue that I raised here, and I'm not sure I know how to go about coming up with an answer. But you may have looked into it, and you may have some comments.

MR. ROLFES: Hans, this is Mark Rolfes, and I think we addressed this and a couple of the previous issues in our previous working group meetings.

To the best of my recollection, we came into this meeting with one action item, to provide our thorium intake model to the Advisory Board working group.

I would like to take a look back at the transcripts or if someone else would like to take a look back at the transcripts to see what we said back then.

I have been looking through the data that we have provided on the O: Drive, and there is a procedures folder, an FMPC Procedures Folder, reference ID 33975. Looking through the table of contents, there

is a procedure for writing procedures for the industrial hygiene and radiation department.

I don't need to go through the entire list in detail, but I want to point out some of the important things that I see looking through the table of contents.

There is some information on the special operating procedures -- standard operating procedure for the issuance of equipment and material pass, SOP for conducting ground contamination surveys or issuance of notice of contamination source, guides for radiation monitoring, film badges, dosimeters, and pocket chambers, SOP for investigation of possible radiation exposures, SOP for the use of the radiation monitoring record, procedures for the safe use and control of radioactive sources.

Like I said, I don't want to go through all of these, but there's roughly 113 pages of industrial hygiene and radiation department SOPs and procedures that I think

will address some of the concerns that we have.

MR. BEHLING: When was that procedure dated?

MR. ROLFES: Let me get up to the top page here. There are several revisions of this document. There is Revision 1.3, 1.12, 1.13, 2.5, 2.16. Let's see. It looks like there's, well, several major revisions. I'll see what the date on this one is.

I have a letter dated June 25, 1965 for the all industrial hygiene and radiation department members: AThe attached procedure manual Number 10 is being sent to you for your use. As changes are made or additional procedures formulated, this material will be given to you for placement in your manual. Information contained in the manual is not cleared for publication. The manual is to remain the property of National Lead Company of Ohio and is to be surrendered upon termination or transfer.@

So this one was from 1965. Let me see if I can find a version number on here. Many of the pieces of data that we are discussing now, I believe, will be addressed or at least suspect they are discussed in this manual, without reading the entire manual.

I think maybe that might be a good place for us to start with reviewing, as well as the other procedures that are within this folder, to see if the data that I thought we had addressed previously is actually addressed in this folder.

MR. MAURO: Okay.

MR. MORRIS; I found another informative item on this SRDB that you might want to take a look at. it is reference ID 3173, Personnel Monitoring Film Badges. In it you will find a record of just, like, one week's worth of data. Maybe it's a month's worth, I'm not sure, and it has contact data measurements, calibration films contact data exposures for different time frames, similarly

for gamma exposures, and I think the year on this is 1952. So for what that's worth.

MR. NETON: This is interesting. I'm looking at these finding that John just excerpted, and 4.5.1 which talks about the quality assurance says this is not an SEC issue.

MR. MAURO: Well, that is --

MR. NETON: Well, but when you say you put QA procedures for '53 to '85 on the O: Drive, and then we made a reference to the Parker report -- so I don't see where you guys actually looked at any of these other procedures.

MR. MAURO: No, we didn't, no. The only one we looked at is the Parker report.

MR. NETON: So your analysis is incomplete of the data on the O: Drive.

MR. MAURO: May very well be true.

MR. ROLFES: Another folder here or another procedure manual that I've placed onto the O: Drive -- let's see. This was submitted

as part of the NOO Fernald litigation.

We've got the National Lead of Ohio procedure manual, industrial hygiene and radiation department, health and safety division, copy number 3. There are 712 pages of procedures. That was dated -- I think it has expanded upon the one that I just referenced, but I don't have the date right here in front of me.

MEMBER GRIFFON: I just put that down as a dual action, and SC&A should review the procedures that are there. NIOSH might want to relook at them and, if you have anything to add --

MR. NETON: Yes, I agree. It also says here NASA ran the program in the very beginning, and these procedures were based on the -- so there's a lot of work.

MR. MAURO: Yes, there is no doubt that in the notes there was reference to -- in a year-ago meeting, reference was made to a lot of material. We did not review all that

material.

MR. NETON: Okay.

MR. MAURO: I am going to move on to the next to the last item under 4.5. That has to do with neutron doses. Now it turns out that one of the questions we raised was the neutron to photon ratio, and you folks had indicated in your procedure that you are using a ratio of .23. That is, if you know the photon, this is your neutron. Use a multiplier of .23.

We had looked at that by doing some calculations, and we assumed different kinds of geometries and arrays of, for example, UF^4. That might be in drums or in piles, and we came up with a higher number, but we made a mistake.

We made certain assumptions regarding what might be there. That was so large that it would have been a criticality issue. So we made a mistake, and we redid the numbers and checked it again, and we concur

that your neutron to photon ratio of .23 is claimant favorable, and as far as we are concerned, we no longer have an issue on that matter.

We did not discuss this at the last meeting. I'm glad that we were able to get to it at this meeting.

MR. BEHLING: Can I also make a comment here? In addition, I reviewed a position paper on neutron monitoring ascent, it=s called. It is dated 1/17/2001, and it, by and large, looks backward in time, and it says empirically, neutron dose rates and photon dose rates and, in fact, that was approved by Hennifeld, Sue Hennifeld.

So I looked at that, and rather than looking at theoretical calculations that are the basis for the 0.23 neutron-photon ratio, I looked at these data.

It turns out that if you look at the empirical data in that particular report, the 0.23 is very claimant favorable. So

again, I will only reiterate what John just said. We agree that .23 is a claimant favorable dose ratio for neutron-photons, and I think we could drop the issue.

MR. NETON: Good.

MR. ROLFES: In looking through the data on the O: Drive once again, in the procedures folder there is a procedure on the policies in place at Fernald for the requirements of showering, which would speak to the personnel contamination issue.

MR. MAURO: Last item? The last item that we had not had an opportunity to get to at the last meeting in March 2008 had to do with unmonitored female workers.

Now I guess my understanding is -- this is bioassay -- apparently, the issue had to do with the fact that female workers were not bioassayed. Now as I understand it, your co-worker model basically is going to assign everyone some intake, no matter what worker they were, and you are going to use your

protocol, as you had indicated.

So my understanding is if, in fact, that is correct, that female workers were not bioassayed during certain time periods, as I understand this issue, the resolution -- and we didn't discuss this, but I guess I'm intuiting it -- is that you are using a model now that is going to assign intake to everybody, and it would follow your procedure.

So now in effect, my understanding is that, if a person, including women who worked there, were not bioassayed, you would make certain assumptions regarding their intake that was compatible with their job descriptions and where they were, etcetera, and assign an intake.

If that is the case, I guess unless anyone else has more to say about it, it seems to be a reasonable approach, given that we resolve all these other matters.

MR. NETON: I would certainly agree with that. I think up to a certain point

women were prohibited from working in the process area at Fernald, and that is probably a good reason why they weren't monitored.

MR. MAURO: Well, how this issue came up, and I think Hans can confirm it, is that we actually found that three women were, for some reason, sampled at one point in time, and not expecting to find anything, they found something.

So it meant that there was enough residual contamination throughout the plant that, even if a person had a job description that would seem to be they really didn't have potential for internal exposure, had internal exposure.

MR. NETON: Do you know what urine levels they had?

MR. MAURO: No, but I'm sure we can track that down.

MS. BALDRIDGE: It's in the SEC.

MR. MAURO: Okay, there you go.

MR. ROLFES: There is a document,

that report that indicates that those women had positive urine samples essentially, and I think we had a discussion about the process -- excuse me, the bottles for urine sample collection could have been stored in a process area. There could have been contamination in the bottles.

I think we resolved this at the last meeting that we discussed this in saying that we would take a look at that data and use the data as if it were valid results. I don't recall any other further discussion of it.

MR. NETON: But I agree with John's original statement that we would evaluate each case, female or male, based on the merits of their job description and assign them what we would believe the appropriate co-worker model, appropriate value from that worker model.

MR. ROLFES: Right.

MR. MAURO: Of course, that -- I mean, we are going to be looking the co-worker model. Okay.

MR. NETON: Nonetheless, you would use the co-worker model for females, as appropriate.

MR. BEHLING: This is Hans. Regarding those individuals that John made reference to, they were identified in Finding 4.1-3, and they were, I think -- three or four of them were female.

Was it your understanding that the high levels that were observed among those individuals were really due to cross-contamination as an explanation for the unexpected finding?

MR. ROLFES: That certainly is one possibility.

MR. NETON: It wouldn't be the first time that happened. The reason I asked is it was a major issue with the EPA visitors at one point that showed up positive samples based on an analysis, and it turned out to be false positive as well.

MR. BEHLING: I am looking at the

actual - You can see or read for yourself in Attachment 4.1-3. It is our report, page 29, is the actual reproduction of the document in which the individuals were cited, and it states: The following urinary uranium results were investigated, first because there were no apparent reasons for the high uranium results, and the investigation failed to show why these urine samples were high in uranium, meaning that, obviously, you must have looked at it and perhaps speculated the potential for contamination, but obviously, that was not cited as the reason.

MR. MORRIS: Well, that was a discussion in one of the interviews that's available for you of the interview of the health and safety manager, and I recall him talking about that one in specific. So I'll just refer you to that.

MR. BEHLING: Okay.

MR. NETON: There's a number of reasons for high uranium values. We had

people show up with high values that were actually drinking this mineral water that came from a source that just had naturally high contents of uranium, and there's a number of reasons. Doesn't mean it's not real, but I'm just saying there are plausible explanations for high uranium values other than an exposure in the plant.

MR. BEHLING: I don't doubt that, Jim, but the fact that they were three of the four were women is a little bit more difficult to assume that this was due to cross-contamination or something like that.

MR. ROLFES: I think the concern is that you've said that doses to female workers were not monitored during two operating periods, but it seems to indicate that these three women's results were, in fact, documented; and I do recall looking at the HIS-20 database to see if those high results were incorporated into the HIS-20 database, and they were, in fact, included in HIS-20.

MR. BEHLING; Well, it may very well, Mark, be due to the fact that their analysis was linked to a yearly annual, which really was something that doesn't necessarily tie in a radiation worker, but perhaps the unexpected results were part of -- I'm not sure, but it could have been part of an annual physical where anyone was potentially subject to this evaluation without regard to their potential for exposure.

MR. NETON: That could be true, and then that just, I think, speaks to the quality of the program in a way. I mean, they are monitoring people that had almost no potential, and they do find positives periodically. They follow them up.

MR. MORRIS: Help me, if I'm wrong here, but wasn't this whole monitoring question of females addressed at external when you made the original comment?

MR. BEHLING: To a certain extent, because we were talking about the laundry

personnel.

CHAIR CLAWSON: They were -- I thought that, if I remember right, the person became contaminated, but the problem was they came up with an internal dose for uranium. They submitted their sample and came up positive.

MR. MORRIS: But we do know for a fact that women were not monitored for external dose for some period of time. In fact, they were excluded from operating areas for several years. So that is a fact of history, not --

MEMBER ZIEMER: For some reason, these women were sampled. So there must have been a reason for sampling.

MR. MORRIS: Well, we do know that there were times when, although that was the rule, that they always made exceptions as necessary, like if somebody needed to come see something, they would bring them in. But I don't think that the workers that routinely

reported for work in a foundry or a process area were women at some time during the history of the plant.

MEMBER PRESLEY: One of the things that backs that up: At Y-12 we had men working in the laundry, but there were two women that worked in the laundry. They repaired the clothes after they were washed. They were both seamstresses, and there were two that worked down there.

MR. ROLFES: For some reason, I think as Bob had alluded to, I think this started off as an external dosimetry issue.

MR. MAURO: It did. It did.

MR. ROLFES: And we had said, you know, these are the three different ways. The current approach that we would use to reconstruct an unmonitored female worker would be to assign 500 millirems per year, which would be bounding even for some of the monitored process workers.

There were other alternatives for

external dose. I thought that this was a concern abut whether unusual results like this would have been included in HIS-20. For some reason, that is what rang a bell in my mind.

I'd have to look. I don't have a copy of my old matrix, unfortunately. I only have this updated one that you put together.

MR. MAURO: No. What you are looking at right now is from the old matrix. In other words, there is no new material that I added to this, because we did not discuss this issue at the last meeting. So all you are really looking at is the old matrix.

MR. ROLFES: Okay. So it is everything --

MR. MAURO: This is roughly the old matrix, and we didn't visit this the last time out.

MR. ROLFES: I think there were more details about how we would assign an unmonitored dose. That's why it didn't ring a bell with me.

MR. MAURO: As I mentioned, I grabbed two, what I believe to be, the most recent matrices we had available as my starting point.

MR. ROLFES: Let me take a look to see I have a copy of Earl's matrix. I'm not seeing it.

MEMBER GRIFFON: Draft NIOSH response, the second -- the big paragraph there, draft NIOSH response.

MR. ROLFES: Okay. Yes. So we did. I'm sorry. It's getting late. I just assumed I missed it when we took the notes.

MR. MAURO: It's here.

MEMBER GRIFFON: I don't think we have anything more to do with that, other than the co-worker model should cover it.

MR. ROLFES: Right. I think we had committed that, if we had an unusually high result that, if we didn't have an investigation report which explained it, we would assume that it was a real result and use

that in dose reconstruction. That would be the claimant favorable thing to do.

MEMBER GRIFFON: I think the other issue that sort of came up in our discussion was this question of why -- these people with significant exposure: Why weren't they in a routine program or something like that? But I think we are covering that with our first item that we discussed all morning, the co-worker model. Did they get the highest exposed people?

If that's fine, then this is fine.

CHAIR CLAWSON: Okay.

MEMBER GRIFFON: John asked me to look at this last -- there's these last items, and I don't want to just forget about them, but I think several of them have been addressed with NIOSH responses. So just to go through them.

I know that, number one, you've posted transcripts, I believe, on the O: Drive. Correct, Mark? Pretty sure I've seen

those.

MR. ROLFES: Right.

MEMBER GRIFFON: And then there's also a couple of references noted there.

The Tiger Team reports: Now they have been posted, right? I believe there a reference they have been posted.

MR. ROLFES: That's correct.

MEMBER GRIFFON: I'm not sure that SC&A has looked at these. Just to be complete, I think we've asked you look at those. You should look at those with an eye toward does it have any impact on the dose reconstruction aspects.

There was a previous item that said SC&A will review the posted reports.

Item 3 is -- Mark, can you help me with this one, these other groups that did --

MR. ROLFES: Yes. It says NIOSH will follow up on whether other groups or agencies did any off-site monitoring at Fernald.

I think our response was that the following agencies are known to have participated in environmental monitoring programs at Fernald. We put the Ohio Environmental Protection Agency, the Centers for Disease Control, Agency for Toxic Substances and Disease Registry, and the Ohio Department of Health.

MEMBER GRIFFON: I'm not sure exactly when this question arose, but I guess, in addition -- yes, what relevance. That is my question is what relevance? Are there reports that would be pertinent, that they published that would be pertinent?

MR. MORRIS: Now I just recall that Brad asked -- Ian talked to us about that and tell us who may have had other sources of data, and that was just what we came up with.

CHAIR CLAWSON: With off-site monitoring. Do you know what this French & Bell report might come into, or Phillips report may have come into it.

MEMBER GRIFFON: Right. So that might be the one pertinent lead worth following up on.

CHAIR CLAWSON: Because in reading through a lot of that, there were some other off-site monitoring and so forth like that, and we were just -- what was coming from the plant?

MEMBER GRIFFON: It seems to me, the only follow-up on that one would be that second drive.

CHAIR CLAWSON: And I think that you've put -- that Mark has put some environmental ones on there. I know that I read some environmental reports and so forth like that that they had put in to it.

MR. ROLFES: There is an environment safety and health progress assessment of Fernald, and I think that was -- let's see, and then also the environment -- there were other environmental surveys that were conducted by Fernald personnel as well.

Off-site monitoring was also done.

MEMBER GRIFFON: You know what I want to do with this, just so we don't miss anything. I think we should ask SC&A to review what you have posted on other agency reports for relevance, and report back to us, and don't go any further.

If they are completely environmental and you don't feel they are relevant to occupational dose reconstruction, then you can tell us that. Just review for relevance, I guess, is what I would say.

MR. MAURO: Got it.

MEMBER GRIFFON: And then the fourth one is committee formed to reconstruct thorium operational history. I do remember discussing this. Basically, I think you've concluded that you have a reasonable thorium operational history and, from the raw data that you have looked at and, I think, covered, and there is no need to go any further into this other report. Right?

MR. MORRIS: I think you are right. We've done a pretty nice historical timeline on thorium, got it presented pretty well, and I think it actually correlates pretty well with the research we have done in the last few months at Savannah River for the evaluation report there; because the thorium from Fernald actually was a feed stock to Savannah River.

So we have actually gotten -- been able to line those two up, and they make sense together.

MEMBER GRIFFON: I don't think there is any further action there, unless -- and we will see this thorium operational timeline sort of laid out when we look at the --

MR. MORRIS; It's on the O: Drive now.

MEMBER GRIFFON: It's on the O: Drive, but also when we are going to look at closer, I guess, is with the valuated averages and how they work together. Right?

MR. MORRIS: Sure, yes. And it will be in the revision when we redo Chapter 2 of the site profile.

MEMBER GRIFFON: Then unless anybody stops me, Item 5, follow-up on doses assigned in the beginning years of '83 to '85.

CHAIR CLAWSON: This has to do with the skin dose correction.

MEMBER GRIFFON: Yes. And the last thing I see is interviews are continuing to discover if additional corrections were applied. So I don't know where this stands, really.

MEMBER ZIEMER: Is that a NIOSH comment in red?

MEMBER GRIFFON: I am not sure.

MR. NETON: I think that's a NIOSH comment.

MR. MAURO: That would have been something that had been added after the October 2007 meeting as additional information that was -- that's usually how that works.

MEMBER ZIEMER: Well, you put it in right here, John.

MR. MAURO: No, no. It was in red.

MEMBER ZIEMER: So it was in red. Okay, I see.

MEMBER GRIFFON: I don't know if I put it or, Mark, if you put it in, because we have been sending these back and forth.

MR. ROLFES: To the best of my recollection, it was our statement that -- well, you know, I don't know.

MR. NETON: There was one issue when they initiated the -- they switched to the thermal luminescent dosimeter, that there was an incorrect algorithm. They fit like a fourth order polynomial to a few data points, and, unbeknownst -- before computers were really readily available, it put an extra loop in there that really wasn't justified, based on degrees of freedom of the data, and they went back and re-analyzed the calibration curve, and redid the data. Walt just actually

redid that analysis.

MEMBER GRIFFON: That last statement is what is making it an open action item.

MR. NETON; Yes. I don't know why.

CHAIR CLAWSON: But I think you are right or I'm remembering right, because I think it that additional interviews came from people questioning when they did that, when they changed that.

MR. NETON: Right.

CHAIR CLAWSON: There were some issues there with it.

MR. NETON; I think it was fairly well documented through internal memos what transpired there. At least that's my recollection.

MEMBER GRIFFON: Okay.

MR. KATZ: Okay.

MR. ROLFES: The interviews that we have alluded to, I believe, were conducted in August 2007, plus we've got a couple of

interviews from November of 2007. So this was likely alluding to a couple of interviews that we put onto the O: Drive back in 2007.

MEMBER ZIEMER: We need to close that out then. Make sure that it=s -- well, they need to make sure.

MEMBER GRIFFON: All right. That's it.

CHAIR CLAWSON: That sounds good to me.

MR. KATZ: Folks on the phone, if there are any still left, we are adjourning. Thank you for participating.

(Whereupon, the above-entitled matter concluded at 5:01 p.m.)