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

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ADVISORY BOARD ON RADIATION AND
WORKER HEALTH

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WORK GROUP ON FERNALD

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TUESDAY
NOVEMBER 9, 2010

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The Work Group met in the Zurich Room of the Cincinnati Airport Marriott, 2395 Progress Drive, Hebron, Kentucky, at 9:00 a.m., Bradley P. Clawson, Chairman, presiding.

PRESENT:

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

ALSO PRESENT:

TED KATZ, Designated Federal Official
NANCY ADAMS, NIOSH Contractor*
ROBERT ALVAREZ, SC&A*
SANDRA BALDRIDGE, Fernald Petitioner
BOB BARTON, SC&A*
RAY BEATTY, Fernald Worker
HANS BEHLING, SC&A*
MEL CHEW, ORAU Team*
HARRY CHMELYNSKI, ORAU Team*
LOU DOLL, Fernald Worker*
SAM GLOVER, DCAS*
EMILY HOWELL, HHS*
JENNY LIN, HHS*
JOHN MAURO, SC&A
ROBERT MORRIS, ORAU Team*
GENE POTTER, ORAU Team*
MARK ROLFES, DCAS
JOHN STIVER, SC&A
JIM WERNER, SC&A*

* Participating via telephone

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

9:00 a.m.

MR. KATZ: I'm Ted Katz. I'm the Designated Federal Official for the Advisory Board. We're going to get started now, beginning with roll call. Board Members, beginning in the room.

CHAIRMAN CLAWSON: Brad Clawson, Work Group Chair for Fernald, no conflict.

MR. KATZ: Thank you.

MEMBER PRESLEY: Robert Presley, Work Group Member, no conflict.

MEMBER GRIFFON: Mark Griffon, no conflict.

MEMBER SCHOFIELD: Phil Schofield, Board Member, no conflict.

CHAIRMAN CLAWSON: And Board Members on the line?

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

MR. KATZ: NIOSH-ORAU Team, in the room?

MR. ROLFES: Mark Rolfes, Health Physicist, no conflict for Fernald.

MR. KATZ: And on the line, NIOSH-ORAU?

DR. GLOVER: Sam Glover, NIOSH, no conflict.

DR. CHEW: Mel Chew.

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

DR. CHEW: Mel Chew, ORAU Team, no conflict.

MR. KATZ: Welcome, all of you. SC&A in the room?

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

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

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

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

MR. KATZ: Is that it?

DR. MAURO: No, there should be

others.

MR. KATZ: Okay.

DR. MAURO: We've got two others, maybe calling later.

MR. KATZ: Okay, and federal officials or contractors to the Feds, HHS or otherwise, none in the room, except me. On the line?

MS. HOWELL: Emily Howell, HHS.

MS. LIN: Jenny Lin, HHS.

MS. ADAMS: Nancy Adams, NIOSH contractor.

MR. KATZ: Very good. Members of the public, in the room?

MS. BALDRIDGE: Sandra Baldridge, petitioner.

MR. BEATTY: Ray Beatty, former Fernald, assisting the petitioner.

MR. KATZ: Great, welcome, and do we have any members of the public on the line?

(No response.)

MR. KATZ: Okay, not at this

moment. Let me remind folks on the line to, please, mute your phones, except when you're addressing the group. If you don't have a mute button, use *6 and then *6 again, to take yourselves off mute, and Brad, we don't have a published agenda for this meeting.

Maybe you'll just give us an outline before you get rolling into to, so that everybody could have a sense.

CHAIRMAN CLAWSON: Okay, this is Brad Clawson. For one thing, I'd like to tell Paul he doesn't sound all that good. So, we appreciate that he's not here, spreading it around.

When we finished up, we basically had six items for quite a while, and the first issue was that OTIB-78 did internal review of DCAS comments. They were due.

The HIS-20 database, the coworker construction model, which I understand we don't have, right, Mark?

MR. ROLFES: Yes, if we could get

back to OTIB-78, I attached the revision of the coworker uranium urinalysis study for unmonitored Fernald workers.

CHAIRMAN CLAWSON: That would cover the construction workers, is what you're saying?

MR. ROLFES: Well, no, issue two, if you remember, issue one and issue two were sort of tied together because issue two related more to the subcontractor construction workers, but it also was tied to the unmonitored employees.

So, we sort of, at the last meeting, separated the unmonitored construction workers from the unmonitored general population, and so, what we're doing right now, is looking at unmonitored construction workers, separately, and work is still ongoing on that. So, we don't have anything for you, today.

CHAIRMAN CLAWSON: Okay, so that was -- that was with OTIB-78? Is that what you're --

MR. ROLFES: Yes.

CHAIRMAN CLAWSON: Okay. So, the HIS-20 database, we've got the recycled uranium review. We got the radon breath data review for radon emissions, and we've got the thorium-232 daily weighted exposures, and these are basically, the six topics, the real cut and dry of where we're at.

But at this time, I'd also express a little bit of frustration. We were suppose to have this data in May, so that we could be able to review this.

A comment was brought up. It went to September. I followed up on it, said due to other workloads, it then went -- went another month, and now, we get this, five days before a meeting, of basically, six reports.

I really don't think that John, and I appreciate him getting on to that, and then to have it not even sent to the subcontractors is very, very frustrating, to me, to be able to do.

One of my issues is, is this has been five years from the beginning of this. We have gotten to a point where we're basically locked up, and at this time, as the Work Group Chair, I'm putting out to the other Work Group Members, just so you understand, at the close of our meeting today, after we hear what we can from SC&A, I would like to push this for a vote, to be able to put this to the full Board, because I don't feel like we're getting anywhere.

I don't feel that we've had any kind of movement. The DWA, the daily weighted average, yes, there has been some movement on radon breath. Radon emissions, no movement. Uranium, no movement.

It's been brought up many times on the HIS database, that -- and Sandra Baldridge, we've got to look into what the petitioners have said. There's falsification of documents there. That has never been addressed. We still don't have a construction coworker model.

To all the Board Members, I'm throwing out to you right now that as we -- at the end of the day, we make a decision on this because five years -- I'm calling it untimeliness and non-responsiveness.

So, as we go through this, I'd like you to keep that in the back of your mind. So, I'd like to be able to have the questions and so forth, answered, that we can.

With that statement, I'm going to turn it over to John, and John, I realize I put you in a bad situation, when I called you, and I know that you've had your team work all weekend.

So, if these are ones that you have not had adequate time to be able to really digest, I would -- just let me know, and we can go from there.

But let's start with issue number one, which is OTIB-78, and we'll go from there.

DR. MAURO: Sure, no, I'll be glad to take them one at a time, and let you know, you know, our perspective on where they are and where we're making some progress and where we're not making progress.

Regarding where we -- when we talk about OTIB-78, we're basically talking about the sufficiency and adequacy of the uranium bioassay data, and for the longest time period, after doing quite a bit of analysis of that data, and it was looking at how much data do you have, as a function of time and location, job category, et cetera.

You know, we looked at it closely, after all of this, and the good news is that we're coming down favorably, with regard to the -- that fact that you have sufficient uranium bioassay data, going all the way back to the early 50s in order to, basically, just about -- almost everyone, especially starting in 1957, has some bioassay data that we looked at in your database.

And so, in theory, you have the wherewithal to not only directly reconstruct the uranium intakes, based on bioassay data, but if you needed to build a coworker model, for those workers who weren't adequately monitored, you probably could build a coworker model that would capture different decades, different job categories, different buildings.

However, now, here is the -- there's always a however, unfortunately. One of the questions that came up, that we did not look at, and I believe you folks are currently looking at is -- and I believe Sandra had mentioned, well, what about construction workers?

I mean, in this massive amount of data that's out there, we don't really -- we never went into that data and parsed it.

Well, I believe there's great distributions in data, but are there construction workers and/or the subcontractors, that might have worked at the facility, well represented also, and during the meeting, you may recall, one of the things that I did, I got up to the blackboard and said, "Okay, yes, that's a good question," we've got to -- you know, that deserves an answer.

And all I did was make a suggestion that perhaps, you can go into your data set, and break out, maybe in the claimants files or the totality of the files, of the HIS-20 database, separate the population of construction workers or contractors from DOE workers, we'll call it, and make a plot of the distribution, and see if, in fact, the two overlap very nicely, and if they overlap very nicely, that means any coworker model you build for the construction -- for the workers, would also equally apply well, especially, right across the distribution, from the mean to the tail. But there's a difference.

MR. KATZ: John, let me just interrupt for a second. I think maybe some people have joined the phone call, since we opened up.

But I can hear some background talk, and it may be more disturbing to the people who are trying to listen in by phone.

People on the telephone, please mute your phones, so that we don't hear your discussions, side discussions, and if you don't have a mute button, press *6. That will mute your phone. When you press *6 again, that will unmute your phone, but please mute your phone. Thank you.

DR. MAURO: The bottom line is that it would be instructive and I think, useful to everyone concerned, to make a demonstration that the -- an understanding exists, of what the distribution of bioassay data are, for both -- for these two separate groups, and that if there are some differences, and often, there are, there are oversights, then you have the wherewithal to be able to apply adjustment factors, to account for the fact that perhaps, construction workers, perhaps in a given decade, because we've seen that too, are -- have to be treated a little bit differently.

This is something I don't believe -- now, I have to say, I did not have time to read your -- this June 3, 2010 version of OTIB-78. Is it in there?

MR. ROLFES: Well, let me explain what we've done --

DR. MAURO: Okay.

MR. ROLFES: Since I believe a couple of Working Group meetings ago, the issue was identified, that we had only developed the 50th percentile intakes for our coworker uranium urine study.

And so, at that time, we were asked by SC&A and the Advisory Board Working Group Members, to look at certain classes of workers, because there was some concern, for the urine samples from certain classes of workers who were monitored. There were also some unmonitored workers in those classes.

So, the idea was brought up by the Working Group, as to whether the 50th percentile would be appropriate, or whether we should add the 95th percentile into our uranium intake study.

And so, what we ended up doing is going back and adding the 95th percentile intake rates for OTIB-78, and also, presented that -- we subsequently presented that to the Working Group Members, but then, I believe at the last Working Group meeting, there a specific concern about construction workers.

So, what we decided to do, after the discussion, we discussed, you know, comparing construction worker intakes rates or excretion rates, to the general population, which is documented in OTIB-78, and we have done a limited sampling, but we haven't completed that sampling.

So, right now, we're doing exactly

that. We're comparing the excretion rates or intake rates between construction workers and the OTIB-78, 50th percentile intakes and 95th percentile intakes.

So, we should be able to get something done, hopefully, in probably a month or two, I'm thinking.

DR. MAURO: By the way, Mark just reminded me of something I should have mentioned, also.

One of the points of contention early on, this goes back a couple of Work Group meetings ago, was -- as you had pointed out, you know, you have a distribution of excretion, and the coworker model was going to work -- the best estimate, as their -- their coworker tool.

One of our recommendations was that well, listen, there might be some workers at times when they should use the 95th percentile. In other words, there are certain Classes of workers that might have gotten higher exposures, and -- agreed.

So, that was an important change. So, you know, we're glad to see -- we were aware -- well, we agreed in principle during the meeting, and you're saying now, it's actually the language is in here.

MR. ROLFES: That's correct.

DR. MAURO: So, the language -- so, one of the important issues that we did originally have, we've made some progress, going with the 95th percentile, when appropriate.

The place that we're -- the way we see it, that's still left to be dealt with is a demonstration that a coworker model, that also applies to construction workers, can also be built.

MS. BALDRIDGE: I have one question with this, and concern.

You are continually equating intake rates and excretions. That's only with the soluble uranium. It does not address intake from insoluble form. It doesn't address how severe the intake was, what deposition rate there was and what the systemic uptake was.

MR. ROLFES: Well, the --

MS. BALDRIDGE: And all you're measuring is the transient uranium that is passing through the kidneys, which there are variables there, that are presented as issues against the usability of the uranium data at Fernald, and these are from qualified people. Dr. Quigley was a medical doctor, as well as an expert in the uranium radioactive materials area.

He spoke for years and years, at symposiums or whatever, as an educator to the nuclear community, about the issues of uranium intake, and he consistently says, you cannot use the uranium urinalysis data to determine internal dose.

Now --

MR. ROLFES: But you have to take a

look at the context of that statement, and the time period, as well.

The important factor, back then -- we have the data. It was a matter of -- we have the uranium urinalysis data reported to us.

MS. BALDRIDGE: Right.

MR. ROLFES: What we didn't have back then, was a biokentic model, which showed the distribution of the different solubility classes and chemical compounds of uranium throughout the body.

Basically, you could make a judgment, as to how much uranium was inhaled, but the biokentic modeling, showing how much of that uranium was dissolved from the lungs into the blood stream and how much was deposited into the bone surfaces for those -- versus the liver, and how much came back out of the liver and back into the bloodstream and was redeposited into the bone or how much --

So, the complex biokentic models

didn't exist back then. We had the ICRP, the International Commission for Radiological Protection, too, back then. We now currently have more advanced biokentic models, which show very specific amounts of uranium being released back into the bloodstream.

It shows an extensive map of the body, as to what biological compartments that uranium enters and removes -- is removed from.

MS. BALDRIDGE: But it seems to me that those models all require some knowledge, about what the actual intake was, whether it was in the air, there -- you know, what's the distinction and particle size, in the density of the material?

You know, that can't be determined through the uranium urinalysis, and in -- in one document, I think it's listed as probably SEC-ISID-9362-165, they're opposed to uniformity and record keeping, and this is their statement.

"It doesn't make much sense to

keep very accurate records of industrial exposure, unless," and this is what was omitted from the online part of the petition for this document, "Unless complete medical information is available, radiation records will not be useful in worker's comp or epidemiological study purposes."

Now, they're acknowledging that they don't see any importance in the keeping of accurate records. So, how do you even know that the records that you have are accurate, when this is their mindset?

MR. ROLFES: Well, I'd have to take a look, once again, at the specific -- at the part of that you're referring to.

We've actually spent quite a bit of time making sure that the data that we have is reliable.

One of the things that we had done in the --

MS. BALDRIDGE: The only --

MR. ROLFES: With regards to

reliability --

MS. BALDRIDGE: The only thing you have --

MR. KATZ: Please, one at a time, please.

MS. BALDRIDGE: Is what was submitted by Fernald, to DOE, or to the -- whoever they -- DOE, Department of Energy.

MR. ROLFES: What we have done, to compare the data that we have received -- I know there's been a lot of concerns about the HIS-20 database and the electronic data being transcribed appropriately and making sure that the data is available, et cetera, for us to use, in dose reconstruction.

So, one of the things that we've done was to compare the hard copy urinalysis results to the urinalysis results in the HIS-20 database.

If you were concerned about some specific piece of information not being accurate, it would be very difficult to, you know, modify the data.

If you were concerned about a single urine sample or a group of urine samples for a given employee, you would also have to take a look -- you know, that's not the only source of radiation exposure information. You could also take a look at the individuals in vivo data, and also, air monitoring data.

There is many different factors and many different layers of health physics that's out there, and information at a site, that to, you know, change something or to be concerned about the accuracy of something, you'd have to -- you can't -- I think you understand what I'm saying, but --

MS. BALDRIDGE: Right, but this does not address specific individual data. This reflects a mindset that the record system, this documented dated 1966, this was the mindset that they used, when they were preparing data to keep in their own files or even to send onto DOE.

MR. ROLFES: Could you repeat that once again, and --

MS. BALDRIDGE: Okay.

MR. ROLFES: And also give us a reference?

MS. BALDRIDGE: Well, I did.

MR. ROLFES: Well, I didn't catch that part.

MS. BALDRIDGE: Okay, it's SEC-ISID-9362-165.

MR. ROLFES: Where can I find this document?

MS. BALDRIDGE: Well, those are the -- those are the ID numbers that you assigned to the documents --

MR. ROLFES: Okay.

MS. BALDRIDGE: In the petition.

MR. ROLFES: Okay, so, this has been provided to us?

MS. BALDRIDGE: Right.

MR. ROLFES: Okay.

DR. MAURO: Could I weigh in a little bit on this?

MS. BALDRIDGE: Sure.

DR. MAURO: Really, what I heard was two issues. I'll deal with -- something -- an oversight that I forgot to mention, when I first spoke is, during the last meeting, Sandra, I believe, one of the questions you had raised is, records falsification.

MS. BALDRIDGE: Right.

DR. MAURO: Okay, and I think that's an important question, that is, great, we have all of these thousands and thousands of measurements of milligrams per liter in the urine, and not withstanding this transcription issue, which has to do with HIS-20. Let's just talk -- whether they're hard copy of they're electronic, we got these records.

Okay, let's say, they're hard copy records. Go right back to the source.

At the time, you raised the concern, which has been raised at other sites, because of concern of falsification of records, and how can we trust those numbers? Someone may have gone in and put their numbers in there, and therefore, any distribution already to do with it, can't be trusted.

We, SC&A, were asked at the time, to say, "Well, what are you doing, that you're concerned about that," and we have to look at this in the past, and we wrote a report, in the interim, on strategies for -- that you may want to consider and we're saying, "This needs to be done or," you know, we're saying that we were asked to say, "Well, how do you come at a problem like this?"

And Bob Barton, who was on the phone, very innovatively -- because was a very different -- came up with ideas, ideas that I think emerged because we were asked very similar questions at other sites, and we'll put that aside for one second.

But I would like Bob to summarize the three strategies, I believe it was three, that he invented, as being a way to get at the problem, whether or not that's something that NIOSH or the Work Group or the Board would like to be done, is another question.

But I forgot to mention that yes, we were asked to write that report. We did write that report. I think I'm holding it in my hand, right now. Yes, I am. It's dated June 2010, and in a second, if it's okay with everyone, Bob could give a brief summary of what those strategies are.

Now, I want to quickly change subjects and in this case, I'd like to speak in defense of what Mark just said, regarding the bioassay data.

I agree with Mark, that there was a time when you had urine sample data, milligrams per liter, where you collected the data, but you didn't know what to do with it, because what does it mean? How do we know what that means?

I measured a certain amount of

uranium in urine. How do I know how much was inhaled? How do I know what the dose is to the lungs, to the kidneys, the bone and the rest of the body, and the reason I believe, and we discussed this at the last meeting, a reason to believe this -- the quote that you cited, had more to do with the fact that at that time, when that was written, no one understood the biokentics.

In other words, they -- it's, so, you know what's in the uranium and the urine. You don't -- that doesn't mean you understand what the health effects are, and how it behaved in the body. You really don't know anything. So, you can't use it.

But since then, and this is where -- an enormous amount of research has been done on understanding, what does it mean when I measure this much uranium in the urine?

Well, I believe, as health physicist, that now, we're at a place where you tell me -- you give me some good data on how much uranium is in the urine, and let's say, you have samples collected every quarter or every month, and now, I could look at that, and I feel confident, as a health physicist, I could go back and read that data, if you could trust it. If it hasn't been falsified, and it's fairly complete for a given worker, I could go back and reconstruct the dose, to just about any organ in the person's body.

So, I think that on that regard, SC&A's position, is that we agree with Mark, in the answer he gave.

So, this is where SC&A comes out on this, whether or not the Work Group agrees, you know, that's another subject.

But SC&A's position is, yes, you can use bioassay data, taken from urine samples, of uranium, and if records are complete and have -- can be trusted, that were done correctly, you can reconstruct a person's intake and you can -- and take into consideration, all the variables you pointed out, particle size, chemical form, they all have play, and they're all very important considerations, when you do that kind of calculation. But the wherewithal exists to do that.

MEMBER GRIFFON: And I think the important thing is there, John, is that there is two separate issues.

DR. MAURO: Yes.

MEMBER GRIFFON: And on Fred's initial agenda, one is the coworker model and the second is the sort of V&V, validation and verification of the data.

DR. MAURO: Right.

MEMBER GRIFFON: And obviously, if test two turns out that the data is -- and Sandra's concern is, is that it can't be trusted, then you can't do the model. So, you know, one relies on the other.

DR. MAURO: And there may be --

MEMBER GRIFFON: But I think --

DR. MAURO: No, I was starting to

say that --

MEMBER GRIFFON: I agree with you generally, that the --

DR. MAURO: Yes, you can --

MEMBER GRIFFON: There is a lot of uranium data, if we go through and say, it's all okay --

DR. MAURO: If you trust it.

MEMBER GRIFFON: Then, yes, right.

DR. MAURO: And there might be a bit of confusion, too, when you mentioned the HIS-20 database, we sort of left subject number one, and moved to subject two, and that's what --

MEMBER GRIFFON: Yes, and that's the --

DR. MAURO: Yes, and I think it -- just to help -- the point that Mark was making is that one of the things NIOSH did, and wrote a report, on, was the -- listen, we've got all this electronic data, okay, but that electronic data, someone had to take hard copy data, old stuff, and translate it electronically, and make it electronic data.

And one of the concerns that we always have is, how faithfully was that transcribed?

Now, this doesn't have -- this doesn't pertain to data falsification, which is -- that's a different problem all together. That goes, actually, to the original records, the hard copy records, can you trust those?

MEMBER GRIFFON: Right.

DR. MAURO: The question is -- now, the question that NIOSH tried to answer, and we believe that it's only been incompletely answered, is how faithfully was the hard copy data transcribed into electronic data, because it's the electronic data that's going to be used to do dose reconstructions and to build coworker models.

What NIOSH did was, they went through a process. It's a fairly formal process. It's really data verification. It has nothing to do with falsification -- verification, faithfully transcribe hard copy data into your electronic form, and they went through a process.

Now, we have been critical of that particular -- that's issue number two, by the way, so, leaving issue number one.

Issue number two is that we've been critical in only one area. They really never completed that. They began the process with a design, to say, "We're going to sample these many batches of data. We're going to pull them out, take a look at them and see how faithfully everything was transcribed," and they basically found that well, there were about, on average, six percent, what you would call -- six percent of the data was not entirely appropriately transcribed over, and it was mainly missing data that is -- found out there was some data that was there, that wasn't transcribed over.

Now, then they did an analysis,

that said, "Okay, if we did transcribe the data," because now that they went back and looked at it and found it, they did try -- it really didn't change things -- the distributions very much, and you made a very strong case that, really, the distributions wouldn't change.

But never the less, we felt that in doing that process, you were originally, I believe, were going to sample 25 data sets, and you cut it short. You sort of said, "Listen, we did enough," and our only recommendation is, why don't you finish doing that?

Now, at the time, I believe, that you said, "Well," you'll give is some thought, and you never made a commitment to do that, and we simply feel that you started a process, and you didn't play it out to its end, especially when you uncovered six percent errors, when you were hoping to be less than one percent.

Now, I'm not saying that's important. That's not going to -- everything is going to change, but in my mind, let it play out, finish sampling the 25 sets, get all that data completed, in place, and then say, "Okay, now that we understand how faithfully it was transcribed," then you could say a story that says, "This is the fact that we found," -- that turns out to be six percent, or whatever you find, in the end, what kind of effect does that have on our ability to reconstruct internal doses, or to build a coworker model?

I think that that has to be finished. So, that actually is issue number two, that SC&A has with the work that has been done, and I don't believe that's been done, yet.

MR. ROLFES: Right, I mean, we can certainly look at that, but more work is going to be more time. I mean, that's the bottom line.

We can certainly take a look at it. From what I recall, when we did the analysis and compared the hard copy data to the electronic data, most of the errors were related to name spellings, social security numbers, transposition of one of the digits or --

DR. MAURO: Yes.

MR. ROLFES: Date of birth, things that aren't really relevant to dose reconstruction.

And so, at that time, we had decided not to pursue doing this any further, just because the errors that we had observed were not important to the dose reconstruction process.

DR. MAURO: We understand that, and you know, certainly, that judgment, whether it is necessary or not, you know, right now, SC&A's position is, it seems that our recommendation -- I don't know how big of a job it is, but -- but I think -- and I think that's the story on HIS-20.

But I'd like Bob Barton -- Bob, are you still there?

MR. BARTON: Yes, I'm here, John.

DR. MAURO: Bob, would you mind giving us a relatively brief conceptual description of strategies that might be of value in looking into issues of data falsification?

I think this is an important issue. I know it's important. Sandra brought it up at the last meeting. It's ultimately, the rock we're standing on. You know, you have to be confident that the original record that's out there, can be trusted -- and --

MR. BARTON: Okay.

DR. MAURO: And -- I'm sorry, Brad.

CHAIRMAN CLAWSON: I just want to make something clear. It wasn't just last Work Group meeting, it's been the last four Work Group meetings, this has came up, and this is why you guys started into part of this problem.

So, it wasn't just last Work Group meeting. It was numerous.

DR. MAURO: Okay.

MEMBER GRIFFON: Well, it's in the original petition, too.

DR. MAURO: Yes, that was --

MR. ROLFES: Let me ask a question, because you had mentioned issue two was related to HIS-20, and it sounded like the entire population of HIS-20.

What my understanding -- the issue that came about as of the last Work Group meeting, was the -- specific to the subcontractor -construction workers not being monitored.

DR. MAURO: Two separate subjects.

MR. ROLFES: Okay, two separate subjects. So, my understanding of the original HIS-20 hard copy to the electronic data, as I had mentioned, we had looked at it and found that the errors would not affect dose reconstruction process.

So, we stopped doing the analysis, because we didn't feel that it would be appropriate to finish the analysis, just because we, you know, found the answer.

Separate from that, we're looking at the hard copy data from construction workers, and comparing that to the HIS-20 database and looking at the distribution of the coworkers intakes from OTIB-78 versus construction workers.

So, yes, they're sort of similar issues, and they --

DR. MAURO: Yes.

MR. ROLFES: Yes.

DR. MAURO: But I mean, this -- and I think the mechanics of going through the process you just described, it needs to be done, and when you're done, a story will emerge, as to whether or not construction workers and other workers are -- the HIS-20 database, a story will emerge, how faithfully was the data transcribed, and in fact, does it really have an effect?

These are the -- this a work in process. But now, but this other question that was in the petition, that was raised, by Sandra, a couple of meetings ago, has to do with a really tough problem, and that is, how can we be sure we can trust the original hard copy data, which is the rock we're all standing on?

And we are -- SC&A was asked to report on that, and granted we only came out in June with that report. So, it's been around now, for few months, and Bob Barton had some ideas.

If the Work Group would like to hear it, we could summarize briefly, some of the strategies that might be worthwhile looking into, or not. That's a judgment others have to make, whether it's worth going through such an exercise, but if that's what you'd like, I'd like to ask Bob to give us a quick summary.

MR. BARTON: Thank you, John, and I'd just like to say right off the bat, so everybody -- we're all on the same page, here, this is not a thing that we can really definitively say, one way or another, whether data was falsified in the hard copy records.

All we need to really do is come up with strategies that sort of put the data to the test, I guess for lack of a better term, but you know, give it the `smell test', to see if everything seems kosher, when we look at it from these different strategies, which I will describe briefly, or you know, if we put it to the test and you know, everything seems fine, and that's one thing.

But there is no definitive way to say, "Yes, it's clear that this data was falsified," unless you had someone -- or several people, come forward and say that.

With that being said, let me get into these different approaches that we came up, and there were three strategies that we came up, to sort of look at the data and see whether, you know, everything looks okay, or maybe we have a problem and we really need to look into it further.

So, the first strategy involves comparing -- looking at individual workers and comparing their urinalysis results against their in vivo monitoring, or whole body counting, and as Mark Rolfes mentioned earlier, this is sort of comparing those different layers of health physics data that -- you know, if they don't match up, you might have a problem there.

So, for instance, if you had a worker who had significantly elevated in vivo whole body counting results for uranium, you'd expect to see the same elevated results in his urinary excretion rate.

So, that's pretty much the meat of the first strategy, is just comparing these different types of monitoring data.

One of the limitations of that is in vivo monitoring wasn't present for, you know, the entire operational period at Fernald, so there is sort of only a limited time frame that we can look at for these types of workers, who had both whole body counting done for uranium and urinalysis performed.

But again, what we look for is, you've got a worker who consistently had elevated whole body counting results, and suddenly, you look at his urinalysis results, and wow, they're all coming up, you know, less than the MDA or zero or whatever, then you know, that might be an indication that the urinalysis results were maybe falsified. So, that's pretty much strategy one.

Strategy two involves just simply looking at the urinalysis results, and saying our -- you know, our understanding of how the biokentics work is, you can't have an extremely high uranium sample one day, and then a few days later, have nothing.

But if you had maybe a small group of monitoring workers, who were falsifying documents, you know, where they could, that would bear that out, because you'd have some that were correctly showing the elevated urinalysis for a worker and then some that are suddenly zero, all of the sudden.

So, you'd be taking individual workers and looking a just their urinalysis results over a certain time frame, saying, did this actually make sense? Is this possible, what we're seeing, the trend of their uranium concentration in urine?

So, that's essentially strategy two. Strategy one, we're going to compare whole body counting and urinalysis results to see if those match up. Strategy two, we're going to simply look at urinalysis results and see if it's physically possible, with our understanding of how uranium moves through the body and is excreted.

The third strategy involves these

daily weighted exposure reports, which I believe, are on the agenda today, to discuss, and looking at these essentially, is -- they take air sampling results and essentially, compare them to what the jobs -- what the different jobs types were doing.

So, you come up with essentially, an average air sample of uranium in the air that could be inhaled, and you attach that to a certain job title.

Now, if you go in and you find workers who had that same job title and worked in that same plant and time frame, you should see some correlation there, between the high daily weighted exposure for a certain job type and the urinalysis results for that same job type.

So, now, that one, again, is kind of limited. You have find the workers who had that same job type, was working in that specific plant, in that specific time frame, and then you can compare that.

So, if we, for instance, come across a worker who had a very high daily weighted exposure, but comparing him to the other workers in that same plant, you know, his urinalysis is coming up all less than the MDA or zero or something like that, you know, maybe you have a problem there.

If, on the other hand, you have a job title that has a very high daily weighted exposure, so, you'd expect him to have an elevated urinalysis result, and we see that yes, compared to his other workers, who had lower daily weighted exposures, and he -- his urinalysis results are maybe slightly elevated, compared to his contemporaries, then, you know, that would say that everything looks rather kosher.

So, those are the three strategies. Again, you know, the first one would be comparing urinalysis results to whole body counting.

The second one would essentially

be comparing urinalysis results against themselves, to see if it's actually physically possible, what we're seeing with the data, to what we know about human biokentics, and then the third one would be comparing daily weighted exposures, which were based on air sampling, which is completely different from the urinalysis bioassay, to see if the job types with the highest potential also had elevated results.

So, those are the three strategies, in a nutshell.

MEMBER GRIFFON: Can I comment?

CHAIRMAN CLAWSON: Sure.

MEMBER GRIFFON: I think each one of them has a lot of impending doom. The last one, I think the DWE stuff is -- if it really reflected thorium work, you got gross uranium alpha -- I mean, not gross alpha. You got uranium --

DR. MAURO: Uranium is messing you up.

MEMBER GRIFFON: So, you're not going to have any correlation, I don't think.

The first one, bioassay in vivo --

DR. MAURO: I'm sorry.

MEMBER GRIFFON: Go ahead.

DR. MAURO: Just to -- I just realized something.

MEMBER GRIFFON: I don't think it's --

DR. MAURO: If you have a grab sample, and you get your dpm per cubic meter --

MEMBER GRIFFON: Right.

DR. MAURO: Right?

MEMBER GRIFFON: Which is gross alpha.

DR. MAURO: Which is gross alpha in the air, and you get a DWE out of that --

MEMBER GRIFFON: Right.

DR. MAURO: And then you would take a urine sample, which is expressed in terms of --

MEMBER GRIFFON: Uranium.

DR. MAURO: Well, no, if it is your --

MEMBER GRIFFON: It's not gross alpha, now.

DR. MAURO: Yes, if it's -- all right, if it's milligrams per liter of uranium --

MEMBER GRIFFON: Right.

DR. MAURO: You've got a problem. If it's the --

MEMBER GRIFFON: But that's what it is.

DR. MAURO: Okay, if that's what you're working with, if you don't have counts per minute, it's alpha counts --

MEMBER GRIFFON: Right.

DR. MAURO: You've got a problem.

MEMBER GRIFFON: But if you had alpha, I agree.

DR. MAURO: Yes, you can't compare apples with oranges.

MEMBER GRIFFON: But it's gross uranium.

DR. MAURO: Yes.

MEMBER GRIFFON: I think it's all gross uranium.

DR. MAURO: You have to be very careful with that issue.

MEMBER GRIFFON: Right.

DR. MAURO: I agree with you.

MR. STIVER: This is John Stiver. You have to worry about any other work, other than uranium, going on in an particular facility -

MEMBER GRIFFON: Okay, and the other ones, the other ones, I mean, the -- out of all of them, I think A has the best potential. But B, looking at the urinalysis trend --

DR. MAURO: Yes.

MEMBER GRIFFON: I can tell you, we did -- I did one case, over the course of a year at Mound, and we --

DR. MAURO: And didn't find any.

MEMBER GRIFFON: Had as many people as we do at this table, debating --

DR. MAURO: What does it mean?

MEMBER GRIFFON: Whether this lady falsified, and intentionally contaminated her own samples, and we never came to a conclusion, you know, and this is with Ken Skrable and Tom LaBone, et cetera.

DR. MAURO: Yes, and that's the group that you'll find --

MEMBER GRIFFON: And it was all uranium data, and we never came -- you know, so, I'm concerned that we're going to really get an answer out of that.

The A, I guess, has the best potential, if you looked at body count versus in vivo, you might see some sort of trend, then you can't look at the -- you'd have to look at cases where they had high urinalysis results, I think, to --

MR. STIVER: And the problem with

Fernald being that data is only available from 1968 on.

MEMBER GRIFFON: Right.

MR. STIVER: So, we're pressing that whole --

MEMBER GRIFFON: Right.

DR. MAURO: I could tell you, when we went through this exercise, at the Nevada Test Site, it was a frustrating experience -- we spent considerable amount of time.

It was a relatively large effort, and in the end, we really could not say we have anything definitive to say. It's going to be inconclusive. I suspect that will --

MEMBER GRIFFON: Might not even know, yes.

DR. MAURO: Yes, we did C-

MEMBER GRIFFON: Kind of what the --

DR. MAURO: And we can't say -- we're inconclusive, and it's very frustrating, for everyone, when you go through a process like that, especially if it takes quite a bit of time, and walk away inconclusive.

MEMBER GRIFFON: Right.

MS. BALDRIDGE: In regard to the in vivo, the petition document states that from 1970 on, they were in-house and there was no data verification, that was done.

So, whatever they sent, was sent, and there was nothing in place, to verify that the process was being done correctly, and that -- there is a GA, Government Accountability, Accounting Office document that states their questioning.

You know, their process is error in data reporting to DOE and to employees, that's in document 2-37, and as far as the work records, there are at least two documents that workers were not working in the -- they were working in areas different than noted in the records.

Another one states, another problem, in determining internal exposure is the difficulty in obtaining good work records, which show how long an individual worked at a various job. Records tell us, plant assignment and classification, but not the specific job operation performed.

So, you can't compare one worker with another, just based on their job classification, because there is nothing in place to verify that -- where anybody was.

DR. MAURO: Well, what --

MR. ROLFES: If I could respond?

DR. MAURO: Sure.

MR. ROLFES: To tell you how we complete dose reconstructions, I can understand, you know, they're trying to come up with a regulatory compliance dose estimate for an individual that worked at a specific location.

What they would try to do, in the more recent time periods, they take that urine sample and look at the types of uranium he was exposed to, what the solubility, based upon the processes that were ongoing, in that particular plant that he worked in. They'd take a look at that information, to make a judgment as to the type of uranium that he was exposed to, and then also, consider what was going on in the plant, et cetera, and they'd likely come up with a best estimate of his internal dose from that uranium exposure, you know, that probably occurred within the past week or two weeks.

When we have urinalysis data at NIOSH, when we complete a dose reconstruction, we would assume a chronic exposure for the entire monitoring period, and the actual urinalysis data that is provided to us, you can actually make a good guess of the solubility of the materials to which the individual was exposed, based upon the changes in the excretion rate.

And we also assume the most claimant-favorable solubility class, based on the type of cancer that the individual has, and we assume that the individual was chronically inhaling uranium, because that typically gives the highest internal dose.

So, we generally don't do regulatory compliance dose estimates. We're doing more claimant-favorable dose estimates, that assume an employee was chronically exposed, in the manner that gives the highest internal dose.

MS. BALDRIDGE: Well, my knowledge of that is based on the assignment of dose, based on the best estimate and the assignment determined by OTIB's, that totally missed exposures because of the very reason they did not identify the work location.

So, the exposure, in my father's case, to the uranium hexafluoride, which caused the kidney damage was only -- could only have been incurred in the pilot plant, because that's the one that was open and operating when he was hired, but yet, his dose reconstruction was based on his assignment that was made, that he would be assigned to Plant 6, which didn't open until a year or year and a half later.

So, that whole part of dose was missed, was not assigned, it wasn't a very effective process, to assign that way. Maybe my father is the only case. Maybe he's not. We don't know.

But you can't make assumptions that the data and the assignments are where they really were, and you don't know the exposure dose, if you don't know the exposure dose.

MR. ROLFES: The exposure -- we do know the exposure, because the urinalysis tell us that, and we make claimant-favorable judgments to assign the highest individual dose, from the bioassay data that we have, based upon current ICRP models.

Essentially, if -- you know, urinalysis -- to determine uranium in urine, it's, you know, very similar to any other type of laboratory test that you might receive currently from your medical doctors.

If you have, you know, a cholesterol test, it is the exact same -- you know, if you have a high cholesterol result, you have a high cholesterol result. If you have a high blood test for high level of high density cholesterol, then that could -- and for a low density cholesterol test, that could indicate a problem with your low density cholesterol levels, in your body.

Same with uranium, if you're excreting a lot of uranium, you could have a lot of uranium in your body.

So, what we do is, we look at that excretion pattern to estimate historic exposures, that would have occurred.

Basically, what we'll do, we'll look at the first day of monitoring of the first day of employment for an individual, say, they started in 1952 and had urine samples, you know, for their entire employment period, up until 1980, we'll take a look at any uranium exposures that could have occurred, beginning in 1952 through 1980, using that urinalysis data, specific to uranium reconstruction -- internal dose reconstruction.

MS. BALDRIDGE: Another thing I has asked, probably two years ago, by now, or longer, was, did NIOSH bother to compare the documents to the actual doses that were recorded, for the high MAC levels at certain locations, at certain times?

Now, if those urinalysis didn't reflect the known exposures, based on the historical documents from Fernald, that would have shown whether there was any suspicious or anything out of line, because there should have been a direct comparison, and that was never done.

MR. ROLFES: This was something that SC&A just discussed in their report, and we all just said that it would likely come up in inclusive.

It would take a lot of work and a lot of effort, but like we wouldn't tell you, one way or the other, whether the data are valid or are not valid.

DR. MAURO: If you were to go out and take -- have a whole bunch of air samples, high MACs, say, really up there, and you know people worked in there, in a building, for extended periods of time, you're absolutely right, you would expect to see fairly high levels of uranium in that person's urine, and the point that Bob --

MEMBER GRIFFON: Assuming no protection.

DR. MAURO: Assuming no protection, assuming the --

MS. BALDRIDGE: And that's the --

DR. MAURO: You know what it is --

MS. BALDRIDGE: That's the assumption that you make.

DR. MAURO: Yes, here is the

frustration. You walk away from such an analysis, let's say, you say, "Well, you know, it's the same as," -- we're looking for a smoking gun.

We're saying, "Oh my goodness, we looked at 100 cases of workers, we knew -- we know they all worked in this area, with very high dust loadings of uranium," and every single one of them came back less than the total limits of detection, and we know they weren't wearing respiratory protection, smoking gun.

All of the sudden, your concern regarding falsification of records, at least the alarms go off.

This is the kind of thing, by the way, we try to do at the Nevada Test Site. We went through this exercise, by the way, and it took -- going into that -- into the hard copy records and extracting that information, mining it out and processing the data, we went through that process and in the end, we could not make a statement that we found anything like that.

We found things that were -- that make you say, you know, "I wonder why this was like that," but nothing that would be -- you know, it's almost like you really have to see the smoking gun, and it's going to -- now, that -- you may turn out, but your point is well taken.

If you did that, if we -- if that was done, and your saw -- and you sampled -- I picked 100 people that we knew were in the high area, and they -- and without respiratory protection, and you're not seeing any uranium in their urine, I've got to -- I'll be the first to say, what the heck is going on? So, yes, your point is well taken.

CHAIRMAN CLAWSON: Do you have anymore that you'd like to speak on? We've gone basically, in two little places there. We've got OTIB-78, coworker models. We've discussed part of that. The HIS-20 validation, we've gone onto this.

Mark, where are we at on the construction worker? I know that we don't have a paper on that, but I guess, I'm wondering where we're at on that paper?

MR. ROLFES: That was what I had referred to, that would be complete in roughly, a month or two.

CHAIRMAN CLAWSON: Okay.

MR. ROLFES: Assuming we -- well, I don't -- right now, we're undergoing some, you know, difficulties with funding and things. So, as soon as things get cleared up, I'll certainly, we'll give you an update as to how soon we might be able to complete something.

So, I'll have to get back to you on that.

CHAIRMAN CLAWSON: Okay, well, this brings us into the recycled uranium.

MEMBER GRIFFON: Well, can we just summarize those first two, because we did the two items and I think -- I'm not sure -- not withstanding the opening comments, I'm not sure if there's any clear actions. I want to be --

CHAIRMAN CLAWSON: We have.

MEMBER GRIFFON: Clear, what's going to happen. Like, the coworker uranium model is -- I mean, SC&A says they're okay with that. The construction worker, coworker model is a separate issue.

CHAIRMAN CLAWSON: Yes.

MEMBER GRIFFON: So, at least we want to hear -- we want to see follow up on the construction worker model.

CHAIRMAN CLAWSON: Yes.

MEMBER GRIFFON: And that's a clear action. The second item, there is two separate V&V issues, and one is the falsification issue --

CHAIRMAN CLAWSON: Right.

MEMBER GRIFFON: And the other is validating the HIS-20.

I'd like to hear a little more

from Mark, on the -- and it might be just because this was stretched over time here, but the HIS-20, I'm trying to remember what was reported on your findings.

I know you said a lot of them were, these kind of social security entry errors, et cetera.

CHAIRMAN CLAWSON: Right.

MEMBER GRIFFON: But can you refresh my memory on where we stand -- what you concluded and if that's -- I do remember, like John, that it seemed like the -- you were planning on, you know, X amount to demonstrate that and then stopped it short.

So, I'm not sure, when you say your analysis was complete, I'm not sure if we're all in agreement on that. I'd just like to hear sort of a summary of that.

MR. ROLFES: Sure, I actually -- since I had said that, we had basically gone to a reduced sampling plan, after we found no errors of significance in our comparison of the hard copy data to the electronic HIS-20 records.

We ended up copying the sampling -- or doing the -- we eliminated the reduced sampling plan and did the full sampling, and I didn't think that we had completed it, but according to what Gene Potter has just informed me, I believe he's on the line at the moment, he did inform me that he eliminated the reduced sampling of the HIS-20 hard records to the electronic records.

Gene, are you able to hear me on the line?

MR. POTTER: Yes, Gene Potter is on.

MR. ROLFES: Okay, could you give a brief update on what was done and how we eliminated the reduced sampling of the hard copy records, in comparison to the electronic records?

MR. POTTER: Yes, just briefly, at SC&A's suggestion, we did go back and do the full sampling, instead of the reduced sampling, which we had thought was justified, based on the experience of what we were seeing, and it turns out that in retrospect, we were correct.

When we went to normal sampling from the reduced sampling, it did not change any of our conclusions.

MEMBER GRIFFON: So, what are your conclusions?

MR. POTTER: Okay, I'm sorry you don't have this report, apparently, but --

MEMBER GRIFFON: Not in front of me. I probably -- you know.

MR. ROLFES: Yes, I just -- I can go --

MEMBER GRIFFON: I don't think we have it.

MR. ROLFES: I think we did keep it, a while back, over the summer, and may have not sent it out.

CHAIRMAN CLAWSON: My understanding

was, it was -- it has just cut it short, and that's where it ended.

MEMBER GRIFFON: Okay, well, maybe you can summarize, and then we'll get the report, and then we can discuss it more in depth next time. But go ahead, Gene, I'm sorry.

MR. POTTER: Okay, there were basically -- we went at -- well, other people went out, other team members went out and gathered the hard copy bioassay results, what we were looking for, to get some examples of each decade, that we then could compare to HIS-20. I think you've all --

MEMBER GRIFFON: Right.

MR. POTTER: Are probably pretty familiar with this. We had -- ended up with 33 different electronic files, and there were different sorts of things, from urine cards to annual summaries, and so forth. Preposition

Then there were eight files that were primarily subcontractor or gross alpha beta results, and so, we did not -- it became obvious that there were -- subcontractors were not going to be found. So, we eliminated those files.

And so, we ended up with 25 files, and 20 of the 25 files met the criteria that we had selected.

SC&A also criticizes us for being possibly too conservative in the criteria which we selected up front, and that's another issue.

The five files that did not meet the criteria, were not likely to result in any significant changes to the coworker study, and I think we've -- SC&A has agreed with that finding.

And so, if we would have included the missing results that we found in hard copy, it would not have made any difference to our coworker study, and overall, approximately 97 percent of the data that was found in HIS-20, and as I said, this is the same basic conclusion we came up with, before we eliminated the reduced sampling.

So, basically, it's the same thing we had before. We just eliminated the reduced sampling and finalized the report, which SC&A wanted.

MEMBER GRIFFON: When you said overall, not -- I'm sorry, 97 percent were found -- was the -- that's the overall cut. Was there anything -- did you try to parse it out by decade? Was there any difference over different decades, or anything like that?

MR. POTTER: I have the --

MEMBER GRIFFON: Or did you not have enough data to look at that? I don't know.

MR. POTTER: Yes, only a few -- well, only a few files -- yes, we did do the decade thing, and only a few of those files accounted for the vast majority of the missing results.

So, for the 1950s, the number of

files we evaluated were 14. The number that met the AQL of one percent, in other words, it was likely that 99 percent of the data was there, was 10 of 14, 1960s, five of six, 1970s, two of two, 1980s, three of three.

So, basically, the 1950s were the primary issue.

DR. CHMELYNSKI: Excuse me, this is Harry Chmelynski. For the 1950s, you said it was 10 out of 14?

MR. POTTER: Yes, 10 files met the AQL of one percent, out of 14 evaluated, and that was after eliminating subcontractors and just alpha beta results.

DR. CHMELYNSKI: And the four that did not, they included these three files with a lot of missing records?

MR. POTTER: Yes, that would be an indication that less than -- based on the statistical tests we used, less than 99 percent were likely to be in those files.

DR. CHMELYNSKI: Okay.

MEMBER GRIFFON: So, how does the -- I'm trying to compare that number, the 99 percent overall comparison versus these 10 out of 14 files -- Paul, you sound horrible, if it's Paul.

How does that -- okay.

MEMBER ZIEMER: It's not me.

MR. KATZ: Someone is coughing, you might want to mute your phone.

MEMBER GRIFFON: Yes, mute your phone.

MR. KATZ: It's the *6 to mute your phone, if you don't have a mute button.

MEMBER GRIFFON: That's right, Paul would definitely be muting his phone.

MR. KATZ: Yes.

MEMBER GRIFFON: So, how does the overall 97 percent compare to the -- when you say 10 out of 14 files, the four files must have had greater than one percent errors, right?

So, what would -- I guess, I'm

trying to figure out, what contributed -- you know, in the 1950s, was it like a 90 percent agreement or overall, that drove the average down to 97 percent? You know --

MR. POTTER: Yes, I don't have that analysis in front of me, but obviously, it could be readily done.

MR. ROLFES: Gene, I was going to call your attention to page six of 15 on the report here that you sent to me, and it mentions reference ID 3169.

It says, "This file consisted of 1950 to 1953 fluorometric analysis for uranium," done by the New York Operations Office, Health and Safety Division.

"After failing to meet the acceptable quality level, the file was given 100 percent inspection. Results showed 84.2 percent of the results in the file were in HIS-20. The 50^{th} and 95^{th} percentile results for this data were identical with and without the missing data."

Therefore, the coworker study would not be affected by the missing results.

MEMBER GRIFFON: That's repeating conclusions. Yes, that's repeating, okay. All right, we still need to look at this report, but thanks for the summary.

CHAIRMAN CLAWSON: One thing that keyed on me was the subcontractor, he says that that was cut out of that.

So, have we done any look at this data, of the subcontractors?

MR. ROLFES: That is what we're looking at separately now, as a result of our last Working Group meeting.

MEMBER GRIFFON: The subcontractors would include a lot of the construction workers, is that --

MR. ROLFES: Yes, that's the --

MEMBER GRIFFON: Is that a factor?

MR. ROLFES: You know C-

MEMBER GRIFFON: Yes.

MR. ROLFES: Individuals that

typically came on the site, for a week or two at a time --

MEMBER GRIFFON: Okay.

MR. ROLFES: To do a small scope job or, you know, even possibly, over longer periods of time, as well. That's something that we're looking into, as well.

MEMBER GRIFFON: So, that's an action to keep track of, that --

MR. ROLFES: Yes.

MEMBER GRIFFON: Subcontractor validation of HIS-20 is also an action, including the -- the coworker model, but also the validation of data, right?

MR. ROLFES: Essentially, what we're doing right now is, looking at the construction worker intakes versus the intakes in OTIB-78, which were developed on -- upon HIS-20.

MEMBER GRIFFON: Right.

MR. ROLFES: So, making sure that the distributions are, you know, the same or, you know, if they're not the same, coming up with the correction factor, to make sure that we're accounting for their internal doses appropriately.

MEMBER GRIFFON: But I guess, just to be clear, you're also looking at the records of the subcontractors to -- the validation piece, to make sure that there is a high percentage of those, like your 97 percent conclusion for the contractor workers?

MR. ROLFES: I think we said that we had eliminated the construction workers from the HIS-20 comparison because some of their data didn't make it into HIS-20.

MEMBER GRIFFON: Okay, so, you've already decided that you're pretty sure that it's not going to be --

MR. ROLFES: I believe that's correct, I believe -- Gene, could you verify whether that is correct?

Did we eliminate the subcontractor data from the analysis that we've completed, because it didn't go into HIS-20?

MR. POTTER: Right, yes, it became obvious, very quickly --

MEMBER GRIFFON: Yes.

MR. POTTER: I might just mention that this -- we went out and -- or other people went out and captured examples of bioassay data for this comparison, you know, there was no thought to capturing subcontractors versus regular employees.

So, we got -- we just got examples of the -- of subcontractor bioassay results, mixed in with everything else, and one thing that we found, for instance, that we have more pre-job samples than post-job samples, because there was no attempt to specifically capture subcontractor results, and so -- and we don't have very many subcontractor results.

So, that's why we're re-looking at how we should proceed on this.

MEMBER GRIFFON: But it seems like it will be a surrogate approach, right?

You'll use the -- the worker/coworker model, to bound the construction worker model -- internal doses, right?

You don't have -- you're not going to build a model out of that raw data. You're going to rely -- you're going to try to demonstrate that the one -- the one model bounds the construction worker hard copy data that you have, is that correct?

MR. ROLFES: I guess, it ultimately depends upon, you know, how much data we retrieve and what the results of the analysis are.

MEMBER GRIFFON: Okay, all right, but that's what you're looking at? That's what --

MR. ROLFES: Correct, that's correct.

MEMBER GRIFFON: Okay, all right.

CHAIRMAN CLAWSON: Well, on issue two, basically, we've got construction workers, validation of the HIS-20 data, correct? Is that it -- that was one part of it.

MEMBER GRIFFON: Yes, and they're going to provide us with this report, that they --

CHAIRMAN CLAWSON: Construction worker intake model --

MEMBER GRIFFON: Just described for us.

MR. KATZ: But it's not validation of HIS-20, is what we just said, right? It's not the validation of HIS-20, for construction workers, because that --

MEMBER GRIFFON: Oh, not the construction workers, no. I thought you were talking about the first part of what we were discussing. No.

CHAIRMAN CLAWSON: But it's construction worker validation of C-

MEMBER GRIFFON: No, it's the coworker model for the construction workers, or the approach, to reconstructing doses for coworker -- for construction workers.

CHAIRMAN CLAWSON: And I would have --

MEMBER GRIFFON: So, I guess, you have -- you know, you have the first issue was the coworker model, overall.

I think SC&A is indicating they're fairly happy with the uranium model for the regular workers, not withstanding the question of the data that went into it.

But the approach, if the data is good, it seems to be okay with SC&A.

The second issue would be the construction worker model, which Mark says is pending. They're going to get -- they are working on that.

The third item is the validation of HIS-20, which only includes the non-construction workers, and they -- their presentation on the phone sounds reasonable. We want to see the report, though.

So, we have to see this updated

report. So, that's an action for NIOSH, to get the updated report. And then the last part is the falsification question, and that's the last thing I want to know, if we have an action or a path forward on that.

I know that -- you know, I don't -- I didn't mean to be so flippant about your request, but I think they do lead us down a scary path.

You know, I don't think we're going to get there with those. I'm actually intrigued more by the -- Sandra was sort of outlining the idea of looking -- and I don't know if you have the data, but I know that at some work I've done, where we've looked at sort of departments. You can -- you know, from interviews, from your knowledge of the site, you know certain departments that were involved in the highest, nastiest operations. You can pull department data by decade and see -- and look at bioassays, of those workers.

DR. MAURO: Yes, I like that.

MEMBER GRIFFON: I know that that was very revealing at Paducah.

DR. MAURO: And if any place, it would be falsified for self-serving purposes, it will be these people that were in these areas.

MEMBER GRIFFON: Right.

DR. MAURO: And so, if you're going to find the smoking gun, that's where you'll find it.

MEMBER GRIFFON: Right, right, so, that might be one -- I'm not saying it's going to be a perfect system. I'm not -- I agree with Bob Barton, that it's going to be hard to, you know, sort of prove the -- find the smoking gun.

But that might be one way of saying, you know, where -- yes, I would see if there's issues of falsification, you would think it would be around the high values, the high portable kind of incidents or exposures to --

DR. MAURO: Right.

MEMBER GRIFFON: And if we could pick the nastiest operations, departments, buildings, I'm not sure how to stratify that, but I think if we did want to go down that path, that might be an action that would make sense, and we'd have an end point, and I'm concerned there is other approaches --

DR. MAURO: Yes.

MEMBER GRIFFON: That's going to be so open to debate, and interpretation, that we're not going to get there.

DR. MAURO: True, yes.

MEMBER GRIFFON: I don't know what other people feel, but --

CHAIRMAN CLAWSON: I think that sounds like a very good approach to really zero in on those potentially highly exposed sub-groups and get the --

MEMBER GRIFFON: Does anyone know in HIS-20, if we have that kind of information that would allow for that stratification? I'm not too -- I'm not intimately familiar with HIS-20, for Fernald.

MR. ROLFES: From my recollection, I don't believe -- I don't believe that that type of information would be in there. I'm trying to recall. I can probably pull it up here, and give you an understanding of what is in HIS-20.

MR. BARTON: Now, just to make a comment there. This is Bob Barton.

What we should discuss, this was sort of outlined in the -- our approach, number three, there, where we would look at daily weighted exposures by plant.

Now, whether there are daily weighted exposure reports for the really nasty work situations and years, we didn't specifically look to see whether those matched up.

But again, it would be looking at what were the real nasty jobs being done, by plant and year, and in HIS-20, there are, I'll call it limited information, when you look at bioassay samples, sometimes, they do actually indicate a plant, where the worker was working, when the sample was taken.

So, there is some limited correlation that we could look at, and again, that's discussed under strategy three of the June 2010 report, that I had outlined earlier.

MR. MORRIS: This is Robert Morris. I would like to point out that the nastiest jobs reported in the daily weighted exposure data sets, often times, were specifically noted to have used respiratory protection, which is going to confound your issue about making decision on it.

CHAIRMAN CLAWSON: Good point.

MR. STIVER: Yes, another potential confounding issue might be that a person who moved from a highly contaminated, dirty job, to a less dirty job, and you know, this data -- he may still end up with a high urine output, years later, if it's insoluble form, relatively -- to what his job category is at the time the data went into the database.

So, there is going to be -- you know, depending on whether there is --

MEMBER GRIFFON: Yes, right.

MR. STIVER: And particular categories, or whether they moved around, that could really be a confounding factor.

MEMBER GRIFFON: Yes, I mean, what we have --

MR. MORRIS: All good points. Although, I would like to point out that in the data -- in a few of the daily weighted exposure summary reports, they actually identified a few of the highly exposed workers, and compared their bioassay results to the values that might have been -- would have been identified for them in the daily weighted exposure reports.

MEMBER GRIFFON: I believe you have a section about that in your DWE report, Bob, if I recall.

MR. MORRIS: Yes, so, that may be an interesting place to look, since you do have, in few instances, people identified by name, in the report, that -- which identified a nasty exposure, as you described it. So, the --

MEMBER GRIFFON: But the --

MR. MORRIS: That could be something to look at.

MEMBER GRIFFON: I mean, I don't disagree with that, I just -- again, I thought that the DWE -- maybe I'm wrong, I thought the DWE focus was on thorium. You're going to get gross alpha, I know, right.

MR. STIVER: It's kind of a broad based technique, you can use to analyze any alpha emitting --

MEMBER GRIFFON: Yes, I know, but where they used it at Fernald --

MR. STIVER: Well, actually, it could be --

MEMBER GRIFFON: They used it at the --

MR. STIVER: That was a particular example, for thorium.

MEMBER GRIFFON: Yes.

MR. STIVER: But it can't -- that was a -- we'll get into this a little bit later, but on our review, we did of a Strom's paper, it was really intended because we've seen this issue come up in several different sites.

And so, it's not just related -- even though, it got into origins and thorium at Fernald, it has applications throughout the whole --

MEMBER GRIFFON: No, I understand, but if you don't have the data for the other --

MR. STIVER: Well, you often do. You have DWE data --

MEMBER GRIFFON: Oh, I didn't know.

MR. STIVER: You can link that to a

particular campaign or operation, then you can --

MEMBER GRIFFON: That's what I'm asking, I understand the concept, but if we don't have the data --

MR. STIVER: Yes, it's not just limited to thorium. Actually, that's one of the problems we had is, the fact -- to identify the thorium and tease that out from, you know, the broader issue, which was uranium.

MEMBER GRIFFON: Right, okay.

MR. STIVER: But anyway, that's a digression.

MR. KATZ: I would just suggest, I mean, unless you have very persuasive evidence of systematic falsification, at the end of the day, I'm not sure what you're going to do with sort of the ambiguous results that you seem to be indicating, are likely to be, and if it's resource intensive on top of that, that means it's time intensive, too.

And if you're, on the one hand, concerned about moving this along in a timely fashion, then the -- and on the other, you're going to engage in a costly and ultimately, probably ambiguous analysis, I'm not sure where there is to go with that.

But that's my -- I appreciate the concern. We've heard this at other sites, too, this kind of concern, and as they've explained, we dogged that in a number of sites, expended great resources, and gotten nowhere.

MEMBER GRIFFON: Right, all right. Well, I'm not sure we --

MR. BEATTY: Yes, I just want to make a comment on something here.

I was going to hold my comments and my thoughts until possibly, the end of the day, but I think now, might be a good time to interject.

There was a federal lawsuit filed at the Fernald site, known as Day vs. NLO.

So, a lot of the things that are smoking gun, so to speak, you're looking for, were discovered in that lawsuit, that settlement.

Even in the petition, some of the actual exhibits from the trial are included as exhibits -- or as part of the petition.

It named some of those very things of discrepancies in the record keeping, shoddy record keeping, numbers being altered or missing, zeros in the place of, like stack emissions and various things, the TLD readings or breath samples and various things being modified.

And in another Work Group meeting, there was a mention of a certain document and it was -- it was finally decided, by different ones at the table, that this person, or this document, had an axe to grid or was sour grapes, and this person is no longer with us, to rebuttal that decision, or those comments that was made about that document.

So, what I'm seeing is a -- you know, a back and forth thing here, that's absolutely getting nowhere, and that lawsuit should speak for itself, because out of that, was born a lifetime medical monitoring program, for the workers from 1951 through 1985. That ought to stand for something itself.

There is a lot of things that were discovered and discrepancies that were revealed, and that's how that thing was settled and was won.

Then along in the 2001 time frame, NIOSH themselves did an investigative sort of report, or study, on the occupational exposures for remediation workers.

Now, we're talking -- I heard the date of 1968 mentioned a while ago, about a database that was more supportive or whatever. That should hold some weight, as well, and be, you know, taken into account.

However, this study, by NIOSH

themselves, revealed, even at -- even in the 2000-2001 time frame, the record keeping -- there was like four questions, basically, asked in this report, and the short answer to all four questions was, "No, they cannot do what they say they can do with the medical records, training," -- you know, I hear HIS-20 mentioned a lot.

I wish I was privileged to look at the HIS-20 database, because I, as a former worker, would badge into buildings, and that was how they monitored whether or not I was certified or qualified, trained to go into that building, and I had to be aware of the PPE requirements, based on the system. That system was down more than it was up.

So, if the HIS-20 is looking at exposure times and like, try to associate our time in the building with that process, it's not real, real accurate information, because we had to do a manual sign-in process, when the scanner would be down. Our training records was incorporated into the magnetic strip on our badges.

So, there is a -- and that's mentioned in this report, as well, by NIOSH, about the lack of -- or I'll call it the reliability of the data, the -- I've said this before, I'll say it again.

You know, I'm -- I can't challenge the methodology and the science of dose reconstruction, but I can sure as heck, you know, say how I feel about the reliability of the data that's being used, and the lawsuit and this study, should, you know, leave some -- I don't know, have some weight towards that. Thank you.

CHAIRMAN CLAWSON: Ray, I just wanted to clarify something. When you were talking about the individual that they were talking about, the axe to grid, you were talking about the air sample?

MR. BEATTY: Yes.

CHAIRMAN CLAWSON: Okay, I just

wanted to make that clear.

MR. BEATTY: Yes, sir.

CHAIRMAN CLAWSON: I had missed that.

MS. BALDRIDGE: Can I also add, along with that? The federal judge found it acceptable, and it did have a weight in his ruling.

So, whether people debate his credibility, or whether he had an axe to grid now, is kind of after-the-fact, when federal court accepted the testimony.

CHAIRMAN CLAWSON: I appreciate that. One other thing I'd like to point out.

You know, I'm not the swiftest person here, and I'm just trying to stay on track.

Well, that, John, if you'll help me keep track of these action items, no matter what the outcome at the end of the day, that would be -- just so we can go through that at the end of the day, and kind of, where we're at.

So, if that's all right, I just want to make sure -- to tell you the truth, I'm having a hard time following where we're at, and at this time, I was wondering if we could take a 10 minute break, and --

MEMBER GRIFFON: Yes, why don't we come back and recap the actions of the first two items?

CHAIRMAN CLAWSON: All right.

MR. KATZ: So, about 20 of, we'll reconvene, for folks on the phone. I'm just putting the phone on mute, so you don't have to listen to the chatter in the room.

(Whereupon, the above-entitled matter went off the record at 10:25 a.m. and resumed at 10:43 a.m.)

MR. KATZ: If you could mute your phone, because we keep hearing back chatter and it's probably more disturbing to other people on the phone, trying to listen, than it is to us in the room.

But for everyone's sake, if you'd please mute your phone. If you don't have a mute button, press *6. The star button on your phone, and then six, that will mute your phone, and then folks won't have to listen to your coughing and your conversations and so on. Thank you.

CHAIRMAN CLAWSON: Okay, we're going to review kind of the action items that we had in the very beginning, but also, to -- since this morning meeting, we have had somebody walk into the room that never was put on, and I'd just like to have him introduce himself, and go onto the record that he was here.

MR. DOLL: I'm Lou Doll. I worked construction at the plant from 1983 through 2004, originally for Rust Engineering, in 1983 until Fluor came in, in 1993, and worked in construction in most areas -- well, every area of the plant. I'm not sure there's any areas down there, that we didn't work in.

CHAIRMAN CLAWSON: Thank you for coming, and John, if I could kind of have you go over the first two items that we've covered, issues one and two, what was brought for action items, so that we can make sure we're on board with that.

MR. STIVER: Okay, for the first issue, the OTIB-78, I think for -- NIOSH has gone ahead and allowed for other fractions of the distribution, other than the 50th percentile, to account for higher exposed individuals. We're okay with that.

So, that, I believe, has been resolved.

In regards to the construction workers, there is still a review of the database and sorting that all out, to how it applies to these construction workers.

That's still an ongoing effort, evidently, Mark. That's in the process. Any idea on when that might be finished up?

MR. ROLFES: I'm going to have to

get back to you.

MR. STIVER: Okay.

MR. ROLFES: After this meeting.

MR. STIVER: Okay, so, that's in process on that particular aspect of it.

I believe we're satisfied with the explanation that Gene Potter provided on the validation, using the mil spec versus that one percent standard versus --

CHAIRMAN CLAWSON: No, we -- our last understanding was, they had not finished it. Now, we come to find out that they have finished the --

MEMBER GRIFFON: And we want to see the finished --

CHAIRMAN CLAWSON: And we need to see the finished --

MR. STIVER: Okay, we still need to see that, okay.

MEMBER GRIFFON: The explanation sounds reasonable, but --

MR. STIVER: Yes, it sounds

reasonable. We need to see the report, and --

MEMBER GRIFFON: And the action is, NIOSH is going to distribute the report.

MR. STIVER: And send the report out.

MEMBER GRIFFON: Yes.

MR. STIVER: Okay, so, the --

MEMBER GRIFFON: I think that was it, for action items.

MR. STIVER: I think that was it, yes.

MEMBER GRIFFON: Yes.

CHAIRMAN CLAWSON: Now, I just want to -- I went back and forth a couple of times on that, and I apologize, I'm having a hard time keeping up with it.

But I just wanted to make sure we were right on those action items, there.

The next item that we're going to come up to is the recycled uranium review.

DR. MAURO: The Fernald approach, to dealing with reconstructing workers doses, associated with the uranium that contains plutonium, neptunium, technetium, because it's recycled uranium, they've laid out their protocol, and embedded in that protocol is, okay, if a person were to inhale some uranium, and we look at the bioassay, and we see his urine, and from that, we could figure out how much uranium the person inhaled, but you really don't know how much plutonium there might have been in there, because they don't measure the plutonium in urine. They don't measure the neptunium.

So, what NIOSH did is, they went into some records and some history and said, "Okay, well, we could assume and place an upper bound, or at least a plausible, high end position," to try to capture the position, certainly, that by -- and said, okay, for every, you know, gram of uranium or unit of activity of uranium inhaled, along with that, would come a certain amount of plutonium, neptunium, and a number of other radionuclides.

Now, and it's like a default. They would automatically assume that, because everyone gets that.

We were asked to look into that default mix, that is, is this appropriate, and that's bounded, it is possible, there might have been some workers or some sub-group of workers or at some period of time, where that particular assumed mix under-estimates or results in an under-estimate?

So, that was the concern. There was an exchange of White Papers, where -- so, this subject has been going on for some time now, where NIOSH, and in fact, most recently, NIOSH delivered to us, on Thursday, basically, what I would consider to be a reiteration of their original position, perhaps, a little bit more material, to develop the story a little richer, a little more deeply, and of course, we really didn't have too much time to look at this latest version.

But John Stiver, sitting to my left, has spent some time, since Thursday, going over it and doing some calculations and going into the records, to try to see if, in fact, the mix that was selected as being the default is, in fact, appropriately conservative and applicable to the vast majority or workers that might have worked at Fernald.

And with that, I'd like to turn it over to John, and just let him know that though we have only had a few days, we did what we can, to give some insight into what we found out, regarding NIOSH's position.

MR. STIVER: Okay, thank you, John. I'd also like to, at this point, is -- do we have Bob Alvarez and Jim Warner on the line?

MR. ALVAREZ: Yes.

MR. WERNER: Yes, this is Jim.

MR. STIVER: These are two of our new Fernald team members, that bring a new perspective to this issue, I believe, and Jim, could you just give the Members here, kind of a brief overview of your expertise in this area of recycled uranium?

MR. WERNER: Well, I hope I'm able to contribute, based on my involvement, while working, both as a contractor for DOE, during the 80s, and later as a DOE official, during the 90s, where I was involving in putting together the -- what we now know as DOE-2000/DOE-2000B, the recycled uranium report and the balance elements, as well as, I think, the plutonium report that we released.

We, I'm going to use the pronoun here, for Department of Energy, when I was there, and I was involved in also, the report called `Plutonium, the First 50 Years', that we released in 1996.

It included a special appendix, that I was the primary author for, about plutonium and waste, and that -- that we can get into perhaps, the nuclear materials management safeguard system.

MR. STIVER: Okay, thank you, Jim, and Bob, you've been involved in this issue quite a bit, yourself. You're the Senior Policies Advisor to the Secretary and Deputy Assistant Secretary for National Security and the Environment, and what other things have you got, to bring to the table here?

I know you've looked into the -- you've been researching this type of issue for quite some time, now.

MR. ALVAREZ: Well, along with Jim, I was involved in forming the 1999/2000 study, to look at the -- pardon me, I have cold. I was the person who was coughing on, so forgive me.

And so, I involved in helping them put together 2000 -- DOE 2000 study on mass balance of recycled uranium, and prior to that time, I served as -- on the professional staff of the Senate Committee on Government Affairs, and was involved in investigating the original problem of recycled uranium at Fernald in

1985, that resulted in a DOE Task Force report, and the matter of the product specifications material came to light.

More recently, I completed a study of how much plutonium has been discarded by the U.S. Nuclear Weapons Complex, and it has resulted in the DOE Office of Nuclear Material Safeguard and Security proceeding to update their inventory.

I found that about three times more plutonium has been discarded, based on DOE's own records of waste and its original official estimate in 1996.

MR. KATZ: So, before you go on, both of you, I don't think you were on the line when we began the Work Group meeting. So, this is -- I'm addressing Jim and Bob.

If you would just -- what we do in roll call in the beginning is, speak to -- if we're here part of the meeting, that's covered, but also, whether we have a conflict with respect to this site.

So, if you would just both state clearly, for the record, whether you have a conflict with Fernald, based on your past employment.

MR. WERNER: I do not.

MR. KATZ: Thank you. That's Bob, right?

MR. WERNER: Yes.

MR. KATZ: Right, and Jim, can you also just --

MR. ALVAREZ: Yes, I do, and that's been evaluated by all the relevant parties.

MR. KATZ: Right, thank you. I just need that for the record. Thank you, okay.

MR. STIVER: Okay, I'm really glad that we could have you guys on board, because a lot of the issues that we've discovered the last few days, related to the validity of these default values, for the transuranics and fission products, really have their basis in these two DOE reports, the DOE-2000, which came out of the Richland operations office, and deals primarily with the -- with Hanford, and the materials that were produced and sent out to the other production sites, and the 2000B, which synthesized a lot of this material in the -- from the Ohio operations office, and only is applicable to some assessments that were done in that report, for Fernald.

And in addition to that, there is a report by the Office of Security that came out in 2003, that really looked at these other reports that had been -- you guys can verify this for me, it was done in kind of a hurried way, under the -- Bill Richardson's tenure as Secretary, and there was some internal consistencies regarding, you know, the materials accounting amounts that were produced and shipped among the various sites, that were inconsistent -- there were inconsistencies between those documents and also, within what was quoted in the White

Paper.

And the 2003 report, I guess, based on a more definitive definition of what recycled uranium was, revised those values, to some extent.

However, we still have some issues regarding the completeness and the applicability of some of that data in those reports, and if you guys -- I apologize to the people who are on the phone right now, I've prepared a packet of materials for the people here in the -- at the meeting today.

However, I have posted, for those of you who have access to the O: drive, under O:/Stiver-Fernald-10/11/09, I believe there are -- I've posted a set of references, which has these DOE reports and also, there's another set that has extracts from those, that we will be discussing.

And if you guys -- before we get started, if you just take a look at that packet in handed out, it will explain what these are, so that you can follow along, all right. It's a little confused. Let's see where we are, here.

The first few pages are tables that came out of this DOE 2003 report. The first one is A9, which is Fernald receipts data, that shows the amount of material received from the various sites over a period of time, from 1953 to 1989.

The second is the Hanford shipment data. This is table A1, and that shows the amount of materials shipped to all the different sites, as were involved. So, this is going to be important, in a minute.

The next set of materials, there is a graph here that came out of Stu Hinnefeld's report, 1988, and this really tracks the plutonium C- the recycled uranium that was sent from 1961 and the plutonium content, in that material, and the next page, half of the graph has an accounting of the plutonium, both in grams -- there is total uranium kilograms, grams of plutonium and parts per billion on the uranium basis.

As John reported to you earlier, all of the -- the dose assessments performed by NIOSH rely on uranium as the basis, because we have this very comprehensive database of uranium bioassay results, but not for plutonium or for the other transuranic, neptunium or for the fission product.

And so, this all related back to the uranium basis, parts per billion basis, for the most part, and the next page is a table from 1980, and this is this material that came from packet about Paducah tower ash, and the number of hoppers here, there are like 16 of them, and it lists the -- the second column from the right gives you the parts per billion of plutonium on the uranium basis, which you can see, ranges from 6,700 up to 7,700. So, there is huge amount of variability in that, which we'll also discuss in a minute.

And then, the next packet is a series of colored graphics, and this is C- I think you guys have the -- yes, this very first one comes form Table 5 of the NIOSH White Report -- White Paper, and this lists the different sub-groups -- process sub-groups, 19 of them that DOE-2000B produced.

And we took a closer look at that and looked at the ratios of plutonium, neptunium and technetium, and let's see, the second table here is basically the restatement of the table M from the White Paper, to show the default values used by NIOSH.

The next one, Table 2, here, on the following page, is the dust collector data that NIOSH used as -- to help confirm that their defaults were, indeed bounding, and this was a set of dust collector data, collected throughout Fernald at several different plants. You can see the sampling plant, the green salt plant, which is Plant 4, metals production plant, from Plant 5, scrap recovery and the pilot plant.

These, you've got to keep in mind, this data here represents integrated aggregates collection, in these dust collectors, over an undetermined period of time.

So, we don't know what the time frame for this collection represents, and the reason this is important is, NIOSH used this as a site average.

They took all the values, all 36 samples, for the dust collectors, for the entire site, with -- in sensor depth, to remove the high end ones for reasons that we'll get into, in a minute, and then reported that, and then compared that back to the defaults, as confirmation.

And what we did is, we went in and looked at this data, with a little more granularity. We looked at it by each individual plant and looked at the average values, the range.

We did some descriptive statistics on each of these groups, you see the N values, over here, on the left-hand column, so you have an example, we are in each of these buildings, and then we compared -- the very top row here, you have the NIOSH defaults, and these are all in the microcuries per kilogram of uranium.

So, it's just easier to work with that unit, because that's what most of the data reported in, in the Appendix B to the White Paper.

That gives you -- for each of the different nuclides, we've got plutonium-239, 240, 238, neptunium-237, thorium-230, radium-226, thorium-232, thorium-228, cesium-137, tech-99 and strontium-90.

So, we had 10 radionuclides that we looked at here, and then the next page, basically, is some summary data, the summary findings, based on our review, and then finally, plant process description.

So, that's what is included in this little packet, and now, what I'd like to do is get back -- take a step back, here, and get back to our White Paper, which we produced in March 2009, and NIOSH has reviewed and provided comment on.

When I read through the commentary, I was kind of struck with a sense of deja vu because my very first meeting that I attended here in April 2009, when I started reading these responses, it set off a memory and I said, "Wait a second, we've already talked about all of this stuff, for the most part."

So, I went through that transcript and basically, these issues are our position, and it's stated pretty clearly, in that transcript, and rather than go through in intimate detail and address every single finding, what I'd like to do is just really talk to the issues that are important in the SEC context, and also, one issue that we feel is important, in terms of the -- it was more of a Site Profile issue, which relates to the date of attribution of doses from this material, and so, I'll go ahead and jump in at that point.

This issue of the attribution of doses, if you look at that first table, Table A9, this is -- the data was evidently cleaned up in the 2003 report by DOE, and you'll see that you don't see any material coming from the Hanford until 1958. You've got 5, 19, 21, and so forth here, on the left-hand column.

And NIOSH has chosen to use a start date for dose attribution of 1961, based on their data set and their process knowledge that indicates that it was 1961 when this material was first introduced into the Fernald processes, and before that, they acknowledge that material was received from Hanford in 1953, again in 1957, and in small quantities, which were then stored on site.

Well, we understand that, you

know, on the larger scale, there may not have material introduced in production until 1961, but you can see that up until that point there are still 45 metric tons of the stuff in storage, and if we just accept outright, this table with the A9 values, then there is material that is in storage, and we have concerns about the exposure, not necessarily to production workers, but to the workers who were handling this stuff.

There is evidence, from our work on thorium, that there was re-drumming that had to take place every couple of years, because corrosive material and stuff sitting out there in 55 gallon drums, that it started to rust away, and so, that stuff had to be re-drummed.

Now, we don't know whether this happened with the RU, but there is this issue, people -- warehouse workers, what have you, who were handling this stuff, and any potential exposures they may have gotten.

So, regardless of any other amounts that may have come and gone through Hanford, through other sites, we feel that 1953 is really the date that the dose attribution should be started, for this site.

MR. ROLFES: We had discussed this at -- like you said, at a previous meeting, and we said we could take a look back at earlier years, because right now, we've got a default to 1961.

MR. STIVER: Yes.

MR. ROLFES: And interviews to back up, you know, that the material didn't enter the process until 1961.

MR. STIVER: Yes, and we're willing to concede that it may not have entered the process, but we still -- it's on site and being stored there, and there is always the potential --

MR. ROLFES: To be honest, we're cutting fractions of a millirem and it's --

MR. STIVER: Yes, well, it's --

MR. ROLFES: I mean, in this assessment, because if you take a look at the uranium intakes that we are already assigning, a chronic exposure, and then assigning internal dose from U-234 solely, rather than looking at the specific isotopic compositions, 238, 235 and 234, the majority of the dose comes from the higher energy alpha emitter U-234.

And when you take that into consideration and also take into consideration, the transuranic contaminants and fission product contaminants, those are a small fraction of the uranium dose.

One other thing to consider is, well, is the earlier materials that were coming from Hanford, were much less contaminated, the recycled uranium had much less transuranic contaminants in it, plutonium, neptunium, fission product contaminant levels were much lower than the later shipments sent to Fernald in the 70s and

80s, which came from Paducah.

So, I mean, we're talking about -- you know, we've had a controlling from the get-go, Hanford had established a 10 parts per billion contaminant level for plutonium on uranium mass basis, and the earlier recycled uranium, coming from Hanford was likely much lower than that 10 parts per billion.

It wasn't until like, the 1970s, the Paducah tower ash shipments, which, we, for the first time, exceeded our default of our 100 parts per billion, which we're using for dose reconstruction, and you had indicated earlier that there were no bioassay data available for Fernald, for plutonium. That's not true.

We do have bioassay data in the 1980s and it --

MR. STIVER: For the 1980s, yes, I mentioned an earlier period --

MR. ROLFES: Correct.

MR. STIVER: Of time.

MR. ROLFES: And so, with a transuranic contaminated material --

MR. STIVER: Right, and this is all as a result of this interest in the transuranics that came about during the early 1980s.

But my concern really is, you know, I'm not going to get into the dose reconstruction aspect, in terms of the magnitude of the doses involved, but I think from an SEC context, we really need to look at whether there -- the ability for you guys to reconstruct all the doses, for all the personnel, from all sources, for all periods of time.

If we can't get a firm handle on what these fission product and plutonium and that neptunium levels are, then I think we've got a problem, regardless of the actual dose that may be or may not be contributed to that, and we'll also get into it, in a minute, here, about the veracity of using a performance spec from a production standpoint, in the -- that was coming out of Hanford, when this material was going through different chemical processes, it tends to concentrates these materials.

And so, for the worker in a particular building and time, the performance spec isn't necessarily as important as what's going on and what part he is being handled in a particular moment in time.

If I could just get back here, to the source of data, you know, regardless of the time frame, we also came into another issue here, which was the amount of material that was produced from 1952 to 1958, from the high level waste at Hanford, and you know, this is the area of expertise for Jim, so, he can jump in here, if he needs to, if I'm not really answering it.

But this material, because it was being used in a -- there was a -- evidently, redox and PUREX processes were both being employed during this period of time, and this material was coming -- it was already depleted in plutonium, because it had already been processed to extract the plutonium, during that period of time, and there was also, analogously, a high concentration of fission products.

Now, we're concerned that, when we look at the materials balance sheets, let's see, if I've got that particular one right here, this material was produced in a graphite reactor, I guess, it was using a natural uranium, and so, it was lightly irradiated and we'd expect that material to be slightly depleted, and yet, when you look at the manifests here, or the summary data that came out of Table ES1A, I believe this is the -- yes, this was the 2000B report, if you look at the depleted and natural uranium contents, basically, all that's coming out of Hanford here is -- let's see, if I can find this.

It's just cold metal scrap in the

normal and the depleted components, and then we're concerned that there is an entire source of not necessarily high plutonium, but high fission product levels that may be missed in these data that were then used to compile these 19 different sub-group averages.

MR. ALVAREZ: May I explain the, sort of, history of this, it was called the TBP Plant, the U Plant at Hanford.

MR. STIVER: Go ahead.

MR. ALVAREZ: This material in the -- during the original operations of the Hanford plant, until they built a second reprocessing plant, based on a different chemistry of redox, these were what were called bismuth-phosphate separation plants, and they were only separating plutonium.

So, the AEC and the contractors realized there was a great deal of uranium that could be recovered at a time when there was a much greater need for uranium that could not be met by mining, and they utilized one of the cannons that they built in 1944, called New Plant, and removed the high level waste from the tanks and used tributyl phosphate, that's why they called this the TBP plant, to extract this uranium.

The uranium -- and it had to be -- they had to be very careful about how they -- the timing of processing this material, because they had to make sure that it was aged at a certain level, and they also became concerned that even after it was aged, because it was high fission product content, that it would have to be, again, pre-treated or blended with material that was coming out of the newly operated redox plant.

The pressure to produce this uranium was very high, because it was considered a serious short fall of uranium that was necessary to run particularly, the reactors that were going online at Hanford.

So, their records pertaining to the U Plant, or the TBP Plant, and how much was extracted there, is quite substantial and they were having problems dealing with the decontamination factors, and the process was such that they would separate out the uranium using tributyl phosphate and then convert it to uranyl hexahydrate, and then have the material calcine due to uranium trioxide.

And they were finding that if there -- they were running through both pure TBP material
that was not at all blended or pre-treated at the redox plant, as well as material that was blended with redox material, and pre-treated using silica gel and ozone, to try to reduce the short-lived fission.

But the decontamination factors, the DFs, in the UO3 plant, ranged from one to 16, 16 being very high, high dose rates, and some of the dose rates that were coming off the pots, the calcine pots, were as high as two rad per hour.

So, there was a considerable

amount of this hot stuff being run through and sent off site. They weren't letting it sit there because this had to be processed and there was a great amount of this material to be rendered into fuel for the growing number of reactors at Hanford. This went on until 1958.

So, that's sort of just a brief thumb-nail sketch history of this.

The other thing about that is that if the -- with respect to plutonium, recent studies done of material mass balance out of Hanford have in there that the assumptions about the amount of plutonium that were showing up at the reprocessing plant from the reactors were not necessarily correct, and that the reprocessing plants were actually finding more plutonium.

The reactors were producing more plutonium, and more plutonium was winding up in the waste treatment, and there were all these little problems that, you know, came out about the 1950s, where a lot of this was new, first of a kind type facilities.

You know, if we didn't control the entrainment of solvents, for example, in your process of sending your material to the UO3 plant, your actinide levels would go up rather substantially.

So, there were issues that were -- and not everything was perfect, and the material, you know, until 1970, that were -- there was absolutely no mass balance system put in place.

So, there is a lot of uncertainties about how much plutonium was actually -- wound up in waste streams and how much was sent off site.

Plutonium, they kept an eye on, because it was considered a valuable product, but other materials that were of no concern to them as a product, they never really took the time to measure, unless it became a product.

So, there were a lot of things

missing in the specification requirements, for example, at the Hanford Site where a lot of this uranium trioxide came from the Savannah River Site.

There were no requirements made until the 1980s, to even bother to measure for things in the air, for workers being exposed, to other actinides, to any actinides, for that matter, and essentially, they went -- the contaminant of primary concern was uranium.

So, and I'm not so familiar with Fernald and it's early history, and one of the questions I've been asking is, how adequate was the film batch program at Fernald during this period, when this TBP or U Plant material was being sent, and you know, I'm somewhat familiar with the Y-12 Plant, where until 1960, only a very small fraction of workers were badged.

So, it's a question I have, but anyway, I'll shut up and --

DR. GLOVER: Well, thanks a lot.

Ted, this is from the -- it's hard to get from the phone side. This is Sam Glover from NIOSH.

MR. STIVER: Okay.

DR. GLOVER: There were two points, if I can, very briefly.

One, there was a comment made earlier about the 234, putting that into isotopics and reducing the dose and it was -- that comment would need to be -- some more review at NIOSH, before we would develop that as a -- put forth as a strategy.

I did want to make one brief mention of Hanford to my site. I'm a radiochemist, and the initial stuff that came out of Hanford, now, Bob may be talking about the stuff that went farther down when PUREX started, but the stuff that the true -- that the uranium plant did initially, had tanks since 1943 or 1944, and that solution was stored, short-lived radionuclides would be less of importance in that initial stuff that would have come out.

Anyway, I'm not going to belabor that, but the uranium plant was pumping stuff out of the tanks to reprocess that.

MR. ALVAREZ: I must admit though, that the dose rates from the U Plant material that were being run through the UO3 plant, in terms of contaminants, such as ruthenium, niobium and zirconium, were significantly greater than those coming out of PUREX.

MR. STIVER: Yes, you know, niobium and ruthenium were limited by half-life of one year for ruthenium-106. So, I'm not too concerned about those particular isotopes.

However, we do have -- the data we looked at showed high levels of strontium-90, cesium-137 and technetium-99, which is also going to be a long lived one.

So, the short lived stuff may burn up fairly quickly and holding it up in the tanks for a couples of years would -- you know, you would get two or three half-lifes.

You pretty much decayed it away.

MR. ALVAREZ: Well, if I may say so, but there were finding, however, what they call age-based that was two or three years old was quite hot, and so, even though you -- a large fraction of these short lived isotopes were the --

MR. STIVER: Yes, the longer lived isotopes, if you're contributing to that dose rate, that's what you'd be seeing.

But I think the important thing is that trying to get a handle on when this material might have entered into Fernald, and if you look at Table A1, you know, we -- we back up a minute ago, and I showed you the -- there was no accounting of the natural and depleted, in those particular categories of this material that might have come out of high level waste plant.

But Bob, and I talked to you a couple of days ago, you indicated that maybe this stuff had been shipped off to an enrichment plant and then found its way back into Fernald -- not as a direct source, but not as a shipment, but kind of coming in from other sources, in kind of an indirect way.

And if you look at Table A1, you can see that even in 1952 and 1953, 1954, there is fairly large shipments. In 1953 you had 227 -- 2,257 metric tons of this material, which is -- you've got 740 grams of neptunium. You can follow these numbers down, there is quite a bit of this material coming in to other sites.

I believe, if you look at the fourth column over, under other, this is all the remaining, Y-12, Oak Ridge. So, you've got this material circulating throughout the system, and we don't really have a good handle on what may have come in indirectly to Fernald, via one of these other pathways.

And so, that's kind of an open issue, here, and that kind of leads us into the -- you might also go to Table 5 of the graph that I sent out, if you look down, now, this is the sub-groups that DOE had -- I'm kind of getting ahead of myself right here.

But these are 19 process sub-groups that DOE-2000B produced with the best estimates they had, given their limited data for plutonium, neptunium and technetium, in parts per billion uranium.

If you go down to 6A, line 6A, this is UO3 PUREX source, okay, unblended. Okay, look at the technetium-99 levels. It's very close to the NIOSH limit of 9,000 parts per billion, and I might also add that this what they call a bootstrap mean. This is basically an arithmetic average. You have highly skewed data.

If you look at the data that this number was generated from, it goes way, way over by a factor of ten or more, beyond that bootstrap mean.

So, you know, I'm not criticizing the methods that DOE applied, in generating these numbers. I'm just kind of questioning how they might justify these values that were chosen by NIOSH.

While we're on the subject of these -- the sub-group means, one of the interesting points that Arjun had made at the very end of the April 2009 discussion was the data were used to generate these values.

If you look, I believe it's table -- it's F.51A, calculated and constituent masses referenced by receipts. This is off of the DOE 2000B report.

So, any of you who have that, can look at it, and you'll see that there's basically, in this first set, under enriched materials, there are only one, two, about four different concentrations and what they do -- what this represents is that there is very limited data available.

And so, what NIOSH did was -- or not NIOSH, but DOE, they looked at process knowledge and they would take a set of data that they had for a given site or a given process, and look at other sites, throughout the complex that had similar processes going on, and they just assigned them these values.

And so, you have a situation like, say with West Valley, they're basically giving them -- for the -- they're giving them the exact same values for PUREX that were coming out of Hanford without one data point to justify that.

And so, this is the -- brings up the whole issue of surrogate data, as defined by the Board and how it's going to be applied in the dose reconstruction.

MR. WERNER: John, this is Jim Warner. I apologize, I had to step away for a little bit.

MR. STIVER: Okay, do you have something to say, Jim? We lost you.

MR. WERNER: Yes, if I could -- yes, if I could provide just a little bit of background?

MR. STIVER: Okay, sure, glad to hear it.

MR. WERNER: On that source of that data, just briefly, in general, the process, as you alluded to earlier, for putting together that report, was very hurried. It was extraordinarily fast, and I think rather than -- you know, at least from my perspective -- I don't want to be defensive about it, but it's actually remarkable that so much was able to be put together in such a brief period of time, given the complexity of it.

There was some aggravating things that made it even more challenging, aside from just the time.

But as some of you may recall, at the time, there was -- at the time, the news media revelation about the plutonium, the presence of plutonium contamination in the recycled uranium, and that was very much in the news media, and I don't mean suggest that we were just responding to that, but that was certainly a major factor, going on.

There had already been going on, the efforts regarding the legislation we're now implementing, the EEOICPA, the law, the worker compensation law, and that was already kind of on the table, and this really just -- this revelation about the plutonium issue just accelerated it and of course, the bottom line, in terms of the legislation, was that it expanded it.

You may also recall that there was the 671 site list that was then released in USA Today, what is sometimes called the pre-FUSRAP site, and so, there were these things going on that expanded the list, and again, I don't want to be apologetic, but I think it was just extraordinary, what was put together in a very brief period of time.

We relied on the input from different operations offices, to provide the recycled uranium data, and the major focus was relatively qualitative and binary, that is did you or did you not receive recycled uranium and then secondly, how much, from what source, at least in a gross sense, and then, as available, some data and analysis on the nature of that material, and I think the record reflects the quick -- the questionnaires that were sent out and the responses we got back.

And I think it was remarkable. People did a terrific job and the offices and contractors worked very hard, long hours, but I think in the -- you know, the luxury of hindsight, there is all kinds of questions we now ask, that -- well, we knew them at the time.

I mean, among the ones that we were asking at the time was, what about the -- this -- the distinction of the different sources, that you were just mentioning here? You know, the material from West Valley, on the one hand, West Valley was a process didn't change much, like in terms of how the process was done, and so, whatever recycled uranium came from there, probably did not vary because of the process, but certainly, varied a lot, as a result of the inputs.

West Valley, and you see this in the 1996, "Plutonium, the First 50 Years" report, got inputs from a wide variety of sources, both commercial and defense programs materials.

The others sites, and Bob was alluding to this in some detail, when I had to step away briefly, that this is a phosphate process and tributyl phosphate processes, those, I could tell you, you know, having worked as an auditor, visiting those sites, they did vary in processes and the inputs may not have varied a whole lot, from the reactors, but the processes did vary, and the -- you know, the people in chem-ops, the chemical operations staff, I think they really prided themselves on constantly tweaking the processing systems, with designs and operation of slab-tanks, solvent mixtures, temperatures, you know, the exact flow rates and patterns.

So, they were constantly varying it, and so, we put together this recycled uranium report in 1999/2000, some of us were very aware that we were just kind of glossing over these distinctions of the variety of input that were going out, as recycled uranium, because we knew there was such a variety, and of course, you know, Department of Energy, in April 1992, made the decision to shut down all the reprocessing facilities.

And so, a lot of the staff, by 1999, had already gone. So, we had a limited number of staff immediately available.

Now, with a little more time, maybe we could have hired some contractors and gone back and gotten that sort of expertise about the variability's of recycled uranium, but given the time we had, it wasn't really possible to bring on some of these people who had been let go or retired or whatever, as a result of the April 1992 decision to stop any reprocessing.

The other factor is the time, and this may seem like a detail, but I think it's relevant, with today's discussion of Fernald, was we had recently reorganized Fernald under the newly created Ohio operations office, and previously, of course, had been under Oak Ridge, and Oak Ridge, as many of you know, was responsible for uranium operations in secondaries, really, dating back to the Manhattan project.

And so, there was enormous amount of expertise in Oak Ridge operations office, that was then, I wouldn't say lost, the people and the expertise were still there, but it made it a little bit difficult -- more difficult and clumsy when, you know, we were assigning responsibility to the operations offices, like Ohio, to provide the data, but in fact, the expertise was in Oak Ridge, and there hadn't really been enough time to change it.

The higher operations office creation had only occurred, you know, a couple of years before, so, they were completely cut off. The Ohio operations office, of course, was focused on the environmental management and all the old expertise on, you know, these historical issues, at the time from the 1999 perspective, you know, were still back down at Oak Ridge operations, from the reorganization.

So, all that, to say that it's really remarkable that as much data was able to assembled so quickly for that DOE 2000 report, and it's no wonder that we're now here, maybe second guessing, the quality of it, but it's certainly not a surprise, to those of us who worked on it. But I think we were --

MR. STIVER: Well, Jim, you're probably in a pretty good position then, to comment on these limited values and the constituent values in table 51A.

Do you know how these were arrived at and what the source data were and whether that data is available in greater detail than what's presented in this table?

This really is the under-pinning of what's going into these values that NIOSH used as their default. So, the quality of this data is incredibly important here.

MR. WERNER: Okay, you've asked a series of questions, and let me attempt to tease them out, and also, go back to something -- I'm sorry, the NIOSH or other person said earlier, about putting in it the context of uranium exposures.

First of all, with regard to the question that you just posited, the source was generally, as you say, process knowledge, and that's why I mentioned what I did, about the expertise regarding reprocessing.

We always knew that to really understand these, you had to understand not just, you know, the operations at the uranium reprocessing facilities, like Fernald and Weldon Spring, but you really should go back to the original reprocessing facilities. That's where you could sort of get --

I mean, these folks really did fingerprint the fission products, and they were very good at it, if you go back to the original chem-ops people, the chemical operations staff at wherever it was, and I mean, very basically, I mean, if you look this DOE2000, we were not even able to distinguish the input and the fingerprinting from the sites, much less the operations within the sites, much less the change in operations within the sites.

So, the process knowledge was limited at the time. But generally, you know, we relied on process knowledge and what -- it was almost anecdotal, I guess, a summary word.

If somebody had something, they threw it in, knowing that, you know, really to know it better, you are going to have to go back and dig into that process knowledge in more detail.

But we didn't even have -- if you look at it, you don't even see the difference between clearly, you know, INL, Chem Plant versus Hanford, PUREX versus U Plant, versus you know, Savannah River versus West Valley, each of those had different -- somewhat different operations and they were, in some cases, almost competing against each other, which would do a better job of more efficiently doing separations.

So, that process knowledge, of course, changed over time, but we were grabbing what information we could at the time, knowing a lot of our chem-ops people had left after the April 1992 decision.

MR. ALVAREZ: The source of the data that has been used by NIOSH was the result of a task force that was called in, to investigate the circumstances surrounding the processing of the product of the specification material, the material that was sent up from Paducah, to Fernald, in the 70s and early 80s.

That report is very revealing, in that this was a task force of DOE officials, who basically concluded the following, that there were no -- there was no consistent technical standard setting a limit on transuranics or other isotopes throughout the system. They were more or less, implemented on an informal basis and could be changed on the basis of phone calls.

They showed examples of where this happened between Savannah River, for example, and Fernald.

They also pointed out that the health physics regimes for the Fernald workers did not include any form of protection, from -- in terms of bioassay requirements or say, dosimetry requirements, to measure for intakes of the radiocontaminants that would be present in the recycled uranium.

They also -- and so, in 1988, the

department eventually, actually established formal specs, that set these limits for technetium, for neptunium or for plutonium, and prior to that time -- and this is what they -- and so, there was a scramble on the part of the DOE, after the `85 task force report, to start to collect data, and several sites began to look for this information, Hanford and Fernald.

So, but prior to that, there was some spotty data there, but there were no requirements to measure the air in the workplace where they were producing trioxide for these contaminants. There were no requirements to measure in the air at Fernald or no requirements to measure it in the urine or in other ways in the bodies of workers.

So, we have this long gap, especially during the period of peak production, when there did not exist any truly formal technical specifications limiting these transuranics.

MR. STIVER: Okay, Bob, given that you've said -- that may be true, I guess what I'm trying to get at is this data that Jim has alluded to, this chem-ops documentation, and most of those people left by the late 90s.

Is that type of data available, that could then be used to sort of sort out what levels could be applied to different streams of materials that were coming into Fernald, just to get to that level?

I mean, regardless of --

MR. ALVAREZ: Probably not.

MR. STIVER: Probably not, and what were these values, the constituents values in this table 51A, where do they come from?

Say, for example, you've got Oak Ridge -- the first line, Hanford metal, UO2, U3O8 residues. You've got a plutonium level of 4.297 parts per billion. You've got these values for neptunium, and they're repeated for other processes that are listed here, and I was just wondering, where did those numbers come from?

MR. WERNER: As I said earlier, I think that they were somewhat anecdotal. If somebody had some data from their facility --

MR. STIVER: Okay, so, that -- okay, so, if you had some data that were applicable to that process, and that was just determined by this group of experts, that this was probably the best that you had, that's what went into that report, then?

MR. WERNER: Well, we threw that in --

MR. ALVAREZ: But what I'm saying is that --

MR. WERNER: But it's not reflected in the --

MR. ALVAREZ: But what I'm saying is, that this is data that was collected after the fact, from the `85 task force report.

MR. STIVER: This is actually -- this is basically data that was collected in the 80s. So, this was the --

MR. ALVAREZ: That's right, and this does not reflect the data that necessarily would have existed during the period of peak flow on production of recycled uranium.

MR. WERNER: I think that's fair, Bob, but that was --

DR. GLOVER: This is Sam Glover. Hanford, again, is my facility, and I have looked at specifications and reports that they have pulled out, from the late -- the early 50s time frame.

So, there are measurements and they did have those, so, I think we're speaking pretty broadly here, and it's difficult for me to understand which section of time you're trying to respond to.

MR. STIVER: Basically, trying to respond to the section of time before the material arrived in 1980.

MR. WERNER: But Sam, you would agree that for plutonium, that was correct.

For neptunium, when was the first time they began to measure neptunium in workers, working in the trioxide plant? Did they ever do it?

DR. GLOVER: I'd have to verify that. I don't have the --

MR. WERNER: Well, it would be --

MR. ALVAREZ: Well, the answer is no. The answer is no, not until the late 1980s when these sites and plants were basically on the path of closure. So, the --

DR. GLOVER: So, I would take a little bit -- I mean, as far as Hanford, looking at it, they were measuring it, because I have the notes where they were worried about neptunium in the product. We've got the memos from 1948, when they knew they were going to start pulling the stuff out of the tanks.

So, I think we should be a little bit careful. We're putting facts into evidence, that I don't think are --

MR. WERNER: Well, I'll tell you, that I --

MR. ALVAREZ: I can point you to the technical manuals, pointing to what the requirements were to -- what was to be measured in the air and what was to be measured in workers, who were working in the trioxide facility during those periods, and they were not requiring them to measure neptunium nor in airborne concentrations, plutonium, for that matter, only concerned about plutonium in the product, and if it met the spec.

But from the health physics point of view, there was no data there.

MR. STIVER: Well, I guess my concern is what is going -- what is the variability in that performance spec that actually found its way into Hanford -- I mean, into Fernald? I'm getting confused here.

So, you know, regardless of what vision you -- the health physics aspects were at Hanford, we really are trying to get a handle on what's coming into Fernald that could have exposed workers.

And so, I guess, we have these performance specs that Sam was alluding to, we have values in this table and in 51A, that were then used to produce the values that went into table 5 of the NIOSH report, without any verification.

That's my problem here, it's the -- these seem to be very tenuous numbers, and from what Sam is saying, there may be better data out there, upon which to base the defaults, and I've got a real problem with this.

The more we dig into this, the more kind of shaky it all appears.

MEMBER GRIFFON: This is not a new comment, by the way. We've said, at I don't know how many meetings, that this RU stuff is all based on the DOE summary reports, and we're back to the source data.

MR. STIVER: And from what Jim is telling me, that source data is -- just is not

-- it's not there. We just don't have it anymore, and that gets back to that issue in our report about the destruction of the classified documents. Rather than declassify them, they just shredded them, and so, that data may be lost forever.

MR. WERNER: This is Jim Warner, and I guess I tend to be a technology optimist, that with enough effort, you can perhaps, get you know, some data, possibly. I'd never give up hope.

But I did spend some time at the Hagley Museum and library, where the old -- some old DuPont/Hanford data is there, and I actually expected to see more chem-ops, and I know some of the ORAU people were there, too, from the visitor logs, and I wonder what they view as the possibility of getting it.

I did not see it there. I did not spend a huge amount of time, but I, you know, have gone through national archives before, and wasn't able to get it easily.

Now, possibly, it could be done, but I think it would require a bigger effort than people have put in so far. But based on the DOE2000 report, again, I would just vary a little bit, maybe different from what Bob said, that it was -- you know, his limitation.

I would just say that it was limited, but I think were are just aware that it was not fully representative and was not really an attempt to be fully representative. It was an attempt to provide what data was available then, you know, before the clock ran.

We had a very certain deadline. We provided what we could then, and but everybody working on it was aware that this was limited and not fully representative, and some of it was --

MEMBER GRIFFON: That's the key, right there, not representative.

MR. STIVER: Yes, Jim, as a technology optimist, do you feel that a retrieval effort would yield a more reasonable -- not reasonable, necessarily, but a more accurate or applicable set of numbers that could then be used, to --

MR. WERNER: Well, that would be --

MR. ALVAREZ: No. I think the two major efforts right here, really need to dig into this -- one of these that you -- the recycled uranium piece.

I think we've maxed out, in terms of trying to get to the bottom of it.

MR. WERNER: Well, Bob, from my point of view, this is Jim, again, you've asked one side of the question, which is the amount of effort you put in, in terms of retrieving it, but I think one needs to also look at sort of the supply side question of quality of what is there, and what we know about that.

You know, the fact is that, and again, somebody alluded to it earlier, that every time we sent out a query, a request, I mean, headquarters doesn't do this research itself. I mean, sometimes, we took credit for it, but really, the heavy lifting happened in our ops offices, and whenever we sent something out to ops office that says, "Please provide data on the following questions," you know, how much uranium, where did it come from, what quality.

First, the response we got virtually every time from an ops office manager or site manager was, "We did not measure it carefully," you know, and they were very frustrated, we kept asking these questions, and asking for data, on something that they didn't feel comfortable providing it.

They did a yeoman's effort of getting the data together and they were all good soldiers and they provided it, but they were always providing it with the caveat of saying, "Look, we just didn't analyze for these materials, very precisely," and of course, that point that all the ops office managers made to us, as they still provided what data they had, was made in abundance officially in the 1996 report that I think Bob alluded to earlier, regarding NMMSS nuclear materials management safeguard system, that tracked fissile material.

Well, I mean, I'm happy to get into the details of plutonium and waste versus normal operating losses, but suffusive to say that as a result of that report, the Department acknowledge that there was a need to redo the NMMSS system to more precisely characterize plutonium and normal operating losses, because it certainly had not been done prior to that.

Now, we can debate whether it's been done adequately, subsequently, but that report at least, planted a flag in the ground and said, prior to 1996, that the -- the culture and really, the resources, it always come down to budget, is that if a facility was dealing with plutonium and waste, it simply didn't have the equipment, the personnel and the time, most importantly, budgeted, to analyze for the plutonium with the same level of rigor.

And that's what I want to emphasis, the same -- I think, precision and accuracy as a facility like Rocky Flats or Hanford, even within Rocky or Hanford or INL, that you know, there was certain divisions that handled plutonium as a product and there were other divisions who dealt with it as a byproduct or a waste, and those decisions simply didn't form the same level of analysis, and it wasn't even considered in NMMSS and that's -- I was initially named the co-chair of task force on revising NUMEC, and that, frankly, didn't get very far. It's a challenging internal debate, that's another topic.

But if you look at, particularly at Appendix B, in that 1996 report on plutonium waste, they -- we indicated that it just wasn't measured at the same level and it was a commitment then by the Department, to change it's NUMEC and change the accountability.

So, the same level of really, pretty good rigor and you know, some might dispute -- you know, it was a pretty high level rigor for most plutonium, but that same level of rigor, just didn't apply to anything where plutonium was in a byproduct or a waste.

MR. STIVER: Well, you know, I'd like if we -- I think we talked about this a lot, and there was -- kind of an impasse as to whether good data are available that could be used to really bound these transuranics and fission product levels.

I'm also concerned about the whole issue of just using performance spec data as a -- in relation to uranium, as -- for dose assessment, when we know that there were processes that were going on at Fernald that concentrated this material.

MR. WERNER: No, you didn't -- you had varying specs, is the other issue, and there were no specs for --

MR. STIVER: Regardless of the specs --

MR. WERNER: For some of the key contaminants, and so, they didn't exist until 1988, and this is where this data comes from, after that -- during that period.

MR. STIVER: Right.

MR. WERNER: So, the --

MR. STIVER: But say, if you did have good performance specs and you used that, you're going to find out, like, say, in Plant 5, where they're doing the reduction to produce the metal, they put this material in these bombs and it's got this lag liner, made of magnesium fluoride, and we know that this material concentrates transuranic fission products, and we have data to show it and it's pretty well established, that it does take place.

And then, we know that this material is -- we also know from DWE data, that we analyzed for thorium, that some of the highest exposures took place to these guys who were breaking out the bombs and charging them and this kind of thing.

And so, you know, you've got real high thorium content, and you are breaking open one of these bombs, to get the metal, but what does that -- what kind of implications does that have for the neptunium and plutonium and the americium-241 and fission products that are also being entrained in that slag?

And then they took that material and put it back into Plant 1, into this Titan mill, and ground it back up for re-use, and so, it's being recycled throughout the system, and we've got people who were involved in those aspects of production, who were getting potential exposures that are, in no way, related to the original performance specs of the material that came into the plant to begin with.

So, there is another big problem I've got with it.

MR. WERNER: And as I've mentioned, in the 1985 task force report, there is a specific reference to Savannah River reaching a point where they could not meet their tech spec. They made a phone call and everybody agreed to double it, and that was the -- that was how the decision was made.

MR. STIVER: Okay, Mark, did you want to say something?

MR. ROLFES: Yes, I just wanted to add, there are product specifications for recycled uranium, if you take a look at Site Research Database number 4971, it's the Richland Field Office report information.

MR. STIVER: Let me get back on here.

MR. ROLFES: Anyway, I wanted to point out a few pages here that have some product specifications.

Back in 1951, they had a tentative UO3 product specification, defined in the redox technical manual, and it gave specifications for the beta and gamma activity and the contents of the uranium, and in 1951, at Hanford, they started off with 100 parts per billion of uranium, and then, let me --

MR. WERNER: Were there any specs there for neptunium?

MR. ROLFES: I don't see any, readily, but it did describe the beta and gamma activity. So, you could make a bounding assumption --

MR. STIVER: Yes, they didn't have the gamma spectroscopy -- or the level of detail to identify the various isotopes until 1967. So, they basically took gross beta measurements, and they knew that -- they had a pretty good idea of what the isotopic concentration was, and from that, they could get a handle on bounding levels of what fission products might be.

MR. ROLFES: If you move onto page 51 of that same document, it gives a specification from around 1959, showing 10 parts per billion and then subsequently, page 137 shows 10 parts per billion.

MR. STIVER: And what are the SRDB?

MR. ROLFES: It is 4971.

MR. STIVER: Four-nine-seven-one, and what is the title, again?

MR. ROLFES: Let me pull it up here for you. This was the review of generation and flow of recycled uranium at Hanford, from the Richland Operations Office, dated June 30, 2000, and DOE/RL-2000-43.

MR. WERNER: Yes, I mean, that's the same report that also admitted that they did not have specifications or protection requirements for neptunium and technetium.

MR. ROLFES: Okay, all right.

MR. STIVER: All right.

MR. ROLFES: Could we make a

bounding assumption of the quantities of fission products or transuranics, based upon the beta and gamma activity that was measured, described in that report.

MR. STIVER: Well, you can get that as a performance spec for a certain period of time, from a certain plant.

I guess, my problem here is that you've got very sketchy data, upon which you've based your defaults, as we've already discussed here, and you have performance specs, but you don't have a full set of documentation of what was coming and what was going, what were the levels in that particular shipment.

And I've got a problem with that, and when you work in -- also, look at the -- you have the dust collector data, if we could take a look at the dust collector data here C-

DR. MAURO: Let's just move on to another topic.

MR. STIVER: And we looked at the

dust collector data, there were 36 samples --

DR. MAURO: Let's look at this table.

MR. STIVER: Yes, let's take a look at the --

DR. MAURO: This is the rock, right here. I mean -- I want to just -- a lot is going on.

I will be -- what we just heard, that prior to this dust collection data, at Fernald, everything we talked about, on what might have showed up, at Fernald, is a construct, a construct that clearly, we could argue about the amount from now until the end of time, but can you trust it or you can't trust it, whether the specs are something that were held strictly to or were they loose, they're changing?

I think all -- that side of the discussion is not going to be productive. All it will do is reveal that it's a construct and we really don't know where, you know, whether or not that's going to help us resolve this issue. It probably won't.

What will help us understand this, is where do we have the data, and it looks to me that the one place where we've got a considerable amount of data is the dust samples summarized in Table 2.

And to me -- and let that data speak to us, and what does it mean, with regard to NIOSH's default mix, and whether or not we're talking about a mix that just missed the boat, a mix that is -- where there might have been individuals that could have been 10 times or 100 times above the numbers that are assumed by NIOSH, as being the correct mix.

If we can't get -- you know, if this table can't help us answer this, I don't know where we go.

MR. STIVER: This data was collected in 1985. These are dust collector samples from 36 different dust collectors at five different plants. Basically, it was a sampling plant, green salt plant, metals production facility, scrap recovery and pilot plant.

What is notably missing is Plant 2 or 3, where the oxide would have been fed in to produce the material to go into Plant 4, and there would have been raffinate steam coming from that material, for at least -- not from the UO3, but from tower ashes that were recycled.

But let's get on to take a look at the table here. As I said, NIOSH's White Paper, they basically -- what you're looking at -- what they did was this very first row on Table 2, this is the site average, what the Titan mill, in NF-35, and you can see the numbers, if you compare it to the NIOSH default, the values on that first line, the average values look pretty good, with the exception of strontium-90 and thorium isotopes.

DR. MAURO: But let me -- you're so

close to it, what are we looking at?

In the -- right now, in this table two, which I consider to be the one that got my attention, okay, I'm trying to understand what's going on, you're going to look, there is a green strip on the top, all right, that green strip, and you're going to see the one that -- the third column over, it says plutonium-239/240 in the yellow strip, okay, I don't know if everybody can see that.

Just above that, you see 6.3, okay. Six-point-three is microcuries of plutonium-239/241 per kilogram of uranium. That 6.3, let's remember that number. That number is the number that NIOSH is assuming is the amount of plutonium that's present in the uranium, relative now, the plutonium that's in the uranium, as being default.

So, every time they do dose calculation and they say, "Okay, this is estimated how much uranium was inhaled by a guy, because we have his urine data," we're going to assume along with that uranium that he inhaled, there was also 6.3 microcuries of plutonium-239, for every kilo -- you know. So, that is an anchor.

Now, the question becomes, now, that number, can we say with a degree of confidence, based on the data in this table -- I'm sorry, the whole problem in telling the story, is the preface, to set the table, set the story, what are we looking at, because it's so easy to go inside here and use -- you've got to step back for a minute. I do this all the time.

Now, so, what we're really asking ourselves, listen, we've got a lot of dust data here. Look at the first row, called the Titan mill, NIOSH site of -- in other words, 35 separate -- and I'll turn it over to you in a second, but I -- but you have to get oriented, otherwise you don't know what you're looking at.

You've got 35 samples, okay, of

dusts were collected in 1985, and analyzed, for all these radionuclides. That's data. That's nice, for that building, and what we have is some information here and what does it say? It says, well, out of those 35 samples that were analyzed, plutonium-239, they found out that the number, the microcuries per kilogram is .9.

Okay, the first piece of information, well, at least the average, at least the average, out of those 35 samples, came in well below the 6.3. You know, you start saying, "Man, not bad."

You know, at least in this particular case -- and then you say, "Well, what about the range?" Wherever the word range -- I don't know what it means, when you tell me 6.1 --

MR. STIVER: Basically, the high minus the low, in that case.

DR. MAURO: Okay, so, but what's the high? So, it's about 6 -- is the high probably around 6.1?

MR. STIVER: You can see, yes, the high is very close, and that particular one, since they omitted the Titan mill, yes, the highest low 6.1.

DR. MAURO: Okay. So, what we're really saying here is, at least in this first Titan mill of 35 samples, the highest that they saw out of a sample that was collected, of dust, was probably pretty close to what the -- what the default assumption of 6.3 was.

So, what I'm getting at is that when I look at this, and just take my -- just looking at it, you know, I'm just a health physicist looking at data, the 6.3 is looking pretty good, at least with regard to that particular cell, all right, and I think that what we need to do, is we all have to walk away form this table, looking at this data, and then making a decision, based on these data -- this is the only data we have, you know, as far as I know, that we can hang our hat on. We have other data?

(Simultaneous speaking.)

Good, see, I'm wrong, remember, I came into this Thursday, that's when I came into this picture and --

MR. ROLFES: Keep in mind, that in 1986 --

DR. MAURO: Okay.

MR. ROLFES: Following these events, in 1985 and these unusual occurrences, that's what prompted the bioassay for plutonium.

DR. MAURO: This is the --

MR. ROLFES: We're not looking at plutonium. This is the air --

DR. MAURO: If there is more data, good, we'll go there, but right now, to me, after all this, I say, "But what do I have?" I know there is problems, obviously, the records stink, the heroic effort that was made to recreate history, did what they could do, but it's an aggregate number. It's out there, out there in stratosphere.

I don't care about the stratosphere. Even if -- you know, I want go down, some guy worked in a plant, a given year, working with recycled uranium and over the course of that year, is it possible that he was exposed to more than this 6.3 number, and if it is, that means this mix is no good.

And to the degree to which this table could help me get the defense out of whether or not that 6.3 is good or not, that's important to me.

Now, you may have other data like that, that we could look and make that decision. So, what that -- I'm sorry, I have to do this, with that as an introduction, I want to go down these numbers, because I'm going to tell you right now, in my opinion, this is -- I'm going to -- I do this when I get a little frustrated, this is where the SEC sits.

In other words, you can't get by

this one, you can't get by this one, I don't know how you're going to fix this problem. In other words, if we can't find the mix, that's -- everyone walks away from this table saying, "You know, I'm feeling pretty good about that mix," or, "I think I can fix the mix," if we can't get by that, I don't -- I think that we -- this story over.

We are going to be talking about other subjects, but I walked away from this weekend saying, "This is the table," and that's -- so, we may not even get through it today, but this whole -- in my opinion, we have other problems we're going to talk about, but this is the one that is -- the one that troubles me the most deeply, because right now, I can't tell you whether or not I think this is tractable, and I want to hear more about this table and whether or not the 6.3 number -- and every other one of those numbers that were on the green strip, are numbers we could hang our hat on, and if we can't, can we find a number we can hang our hat on? I'm sorry, just let me --

CHAIRMAN CLAWSON: Okay, let's -- boy, this has been stimulating conversation, I realize that, but I want to -- it is lunch time, and I'd like to take a break, before we get into this, really dive into it.

Also, to -- and Ted, I know this is your department, but people on the phone, a lot of times, we get excited to be able to get back into the conversation. We do have a Court Reporter that's trying to always keep track of who is talking and so forth, so if -- and Sam did a very good job of it, and others.

But explain who you are, so, that we can be able to keep a record of this conversation, so that we know who is speaking. I would appreciate it.

MR. KATZ: Yes, two things to add to that, it's -- one is, please identify yourself, because the Court Reporter is not familiar with all of your voices, particularly folks who haven't been with us all along.

The other thing is, please, exercise a little restrain or courtesy, because sometimes, in your enthusiasm, you're trampling each other, and that makes for an impossible transcription, and it's also just sort of frustrating for each individual to not have their chance to say their piece.

There is time for everyone to speak. So, try to exercise a little restraint on that line. Thanks.

CHAIRMAN CLAWSON: We could break for lunch for an hour?

MR. KATZ: So, we're going to break. It's about five after.

(Whereupon, the above-entitled matter went off the record at 12:02 p.m. and resumed at 1:05 p.m.)

MR. KATZ: I think we have a number of other people on the line. I don't need to check on you all, but let's get started then, and let me just remind everyone who is on the line, when you're not speaking, please mute your phone, and if you don't have a mute button, use *6, that will mute your phone. Thanks very much.

CHAIRMAN CLAWSON: We'd also like to bring up too, that be very cautious to be able to announce yourself, for the Court Reporter, but also, try not to talk over one another. I know that's human nature sometimes, but on a phone call like this, it's important that we get all the information down.

So, if you could be cautious with that, we'd greatly appreciate it.

At this time, I'll turn it back over to John, so that he can continue on.

MR. STIVER: Okay, all right, where we left off, we were starting to look at the dust collector data from 1985, for Fernald, and that was table two in the handout here.

As John indicated, across the top, you see the NIOSH defaults. We have those in units of microcuries per kilogram, uranium, and we really want you to look at -- let's not look at the top two rows, because those are the NIOSH site averages. That shows everything combined, without regard to the building of concern.

Now, let's look at the individual buildings, because I think this is where we really need to concentrate, given the different processes that are going on in these different facilities.

If you look at the third row down, this is the sampling plant. This includes the Titan mill, and the Titan mill was a -- is one sample in this set that was very high. The high value was 220 microcuries per kilogram, and the average for that value -- or the average for the entire plant, when you include that, is 38, and the NIOSH default there is 6.3.

So, as you can see, we're considerably above that. Also, in that plant, because of the Titan mill inclusion, we have high neptunium, which is a factor of 10 over the default, thorium-230 is way up there, it's just -- it's 625. All the thorium isotopes are high, and I want to just stop for a second, and we'll talk about the thorium thing here.

As you see, every one of these buildings, the thorium values are very high, and on thing we have to keep in mind here is that the plant actually used -- they had thorium campaigns, from 1954 and 1955, there was a -- one of the largest thorium metal production campaigns.

And so, what we're seeing here is probably not reflective of thorium levels in recycled uranium. There's probably a small component related to that. We can't tease that out here.

But my general feeling on this is that we don't need to dwell too much on those values.

The one that is kind of interesting, though, you see the strontium-90 values are high. This is the far right-hand column. The default value is 4.4, and you'll see the sampling plant and the metals production plant are quite high.

The metals plant is about six times higher than the default, and the sampling plant is just a little above the default.

Technetium-99 is high in the green salt plant, about a factor of two higher than the default, and that's kind of interesting when you look at this -- the green salt plant, remember, this is where the oxide is being fed in to into convert to UF4, and one of the feed materials you see from that table five was based on the averages that we looked at before, and we had the long discussion.

Those are very high in the technetium. These are just the average values, and we've determined that these are not really credible values to be used in reconstruction, but it's illustrated, to see that in the process that they did look at, they have very high technetium, and we see that same thing in the plant that would have used that product, we got technetium values that are about a factor or two higher.

So, it does kind of lend credence to this idea that what's coming out of the high level waste and U Plant at Hanford, may very well have made it into the green salt plant.

One thing we started talking about, and never really got to before lunch, was the whole idea of using the performance specs to base our defaults values on to begin with, when we know that there are processes that are concentrated in this material.

Two that come to mind, of course, being the metals production, where the material is reduced in the bombs, and then that material is then -- the slag is then recycled through this Titan mill, and the Titan mill, I think is what is really reflective of what's going on in that recycling process, because every time you use that, you bring it back to the Titan mill, grind it up, recreate another slag volume to be used on the reduction phase again, this material is concentrating and then Phil Schofield can attest to the levels that can result over long periods of time from this, high levels of all the -- radionuclides of concern, and also, americium-241 is something that we need to be concerned with.

And so, we have this issue in the metals production plant, where you're seeing a neptunium, you can't -- the average is within the specs, but we have a high value of four, which is certainly above the default value, and the strontium is the highest of all, there, and tech-99 is not really all that bad, and then of course, we have a high thorium.

So, really, we're looking at, in

that particular case, the neptunium and the strontium-90 are high. Scrap recovery in the pilot plant are only elevated in thorium.

So, one thing we have to keep in mind here, of course, is this data set is -- we're taking integrated collections over a long period of time, and we don't know what period of time those were actually taken over. We know they were done in 1985.

It is the only data set that we were able to find, that really kind of can be used as a comparison to the default values, and I think what we're seeing here are those numbers, the defaults are really not high enough to account for the one data set that we do have, and we have demonstrated that the basis for those values is certainly in question.

And so, I guess at this point, if some of the other Members would like to give their thoughts on this, I mean, at this point, I would say that we need to try to find new sources of data that would possibly corroborate those air sampling data, and see if there is any other information out there that we could use to get a better handle on what would be bounding values for different Classes of workers, at different facilities over time.

So, Mark, you mentioned bioassay data being available for later periods of time.

MR. ROLFES: Correct, 1986.

MR. STIVER: Oh, 1986, and it's plutonium only?

MR. ROLFES: Both uranium and plutonium were present on site in 1986, both types of sampling were done in 1986, as a result of the high plutonium contaminated material coming on to this site, and workers handling that material and processing it, downblending it, and producing uranium metal out of it.

Ultimately, they had -- I don't

have the number of -- it is several hundred plutonium bioassays that were taken in 1986 time period, as a result of the high transuranic contaminated materials.

MR. STIVER: Okay.

MR. ROLFES: These data that you are referring to here in table two appear to me to be process data, not necessarily air concentration data in the work environment. So --

MR. STIVER: Well, they are not DWEs. They are basically integrated collections.

MR. ROLFES: Right, correct.

MR. STIVER: And so, really, that was my other point, is, I'm asking you, you know, if based on your knowledge and expertise, if you know of other data that may be available, that could be used to corroborate these values.

MR. ROLFES: Okay, well, the bioassay data that we have for plutonium would certainly allow us to bound plutonium intakes for workers that we exposed to the higher transuranic contaminated materials.

I mean, that's really, the concern here. This shipment that came from Paducah is really the sole shipment of greatest concern, and that didn't take place. This was the dirtiest recycled uranium and it was a completely different type of material that came into the plant, than the typical recycling of regular uranium, coming from Hanford and Savannah River Site.

The earlier materials that were shipped to Fernald were controlled at 10 parts per billion, and usually were much less than that, around one, three, five parts per billion. Sometimes, they approached ten, but we have got summary data from a recycled uranium report, showing the average concentration of the various shipments, and really, we've come down out of thousands and thousands of shipments, into and out of

Fernald, we've come down to a small handful that occurred, basically, right around 1980, late 70s, and that's what -- you know, we're most concerned about.

The more recent time period is really the concern for recycled uranium, and as I said, just looking outside the normal recycled uranium shipment --

MR. STIVER: The plutonium bioassays that you do have, for workers, were they the ones who were handling the tower ash in the Plant 2/3, to purify it, to extract the uranium, and how was those C- which workers were sampled?

MR. ROLFES: Plant 2/3 actually had very low concentrations of radioactivity in the air, just because it was a --

MR. STIVER: Well, I know it did, but I'm just trying to get my mind around what the groups of workers were actually --

MR. ROLFES: You know, I recall there were several different categories. I mean, there were some, even forklift operators that had provided --

MR. STIVER: So, these were the guys in Plant 1, that actually received the material and --

MR. ROLFES: I'd have to take a look back at the data, to see what --

MR. STIVER: Yes, okay. Well, the reason I'm asking is, Plant 1 seems to be the -- by far, the highest values, by virtue of this being included in the data set.

But the Titan mill isn't exactly relegated only to this one Paducah ash sample that came through. I mean, this was a plant that was being used, to my mind, at least, and correct me if I'm wrong, but this would be going on, on a regular basis, whenever new materials were coming in that needed to be sampled or needed to be ground down to the proper particle size, they needed this Titan mill.

You know, five years after the

high POOS material came in from the Paducah ash, we still have a high value in this dust collector, that was half the maximum value in that shipment. It was 3,500 parts per billion. This is five years later.

So, the argument, that we've got workers who were wearing inline respirators and all this protective gear, to handle this one batch, I find that hard to believe, that they would still be applying those same standards three and four or five years later, assuming they are still using this mill.

I'm not exactly sure what the Titan mill looks like, what its configuration is, what kind of dust control measures are in place, what -- whether a person at a job station 10 or 20 feet away may be getting some of that material that is coming out of that mill?

So, we don't know that kind of information and without -- you know, we're looking at, at Simonds Saw, we had the same kind of situation and you've got -- you know, near the milling area, you've got really high concentrations and you also have this material, you know, the air currents moving it around to different job locations.

So, you know, you can't just take that piece of equipment in isolation and exclude it and say everything is fine now, because we don't know what kind of cross-contamination is going on here.

If you had air sampling data, DWE type data for that building and that period of time, that five year period, I think you could get a better handle on maybe what the actual concentrations might have been for workers that had been exposed.

MR. ROLFES: Yes, we can take a look for additional air sampling data, if that's what you'd like for us to do.

MR. STIVER: Well, what we're trying to get a handle on here, is really a bounding value, you know, given this data, given that we no longer have much faith in these table five values, then where do we hang our hat?

It looks like this is the only data that we've got, right now, and if we could possibly supplement this data with something that's more pertinent to actual exposures, I think we'd be on our way to solving the problem.

MR. ROLFES: These data came about as a result of the high plutonium material, the exception, coming on to say --

MR. STIVER: Well, I know that's what triggered the interest.

MR. ROLFES: Right.

MR. STIVER: But what we're seeing here is, there is concentrations of this materials in residual quantities that are in the system, five years later, that aren't related particularly, to the exposures that took place during the initial processing of that material.

So, we can't just isolate that from everything else and say, it was a different process and we've got that under control, and so, it doesn't make any difference. I think it does matter.

MR. ROLFES: Well, unfortunately, the person who is the subject matter expert from our team, on recycled uranium, he was also part of the recycled uranium team, he is not available on the call today.

MR. STIVER: Yes.

MR. ROLFES: We certainly want to make sure that we take any concerns into account, and we certainly want to make sure that we're in the right direction and make sure that we have a sound technical basis for assigning intakes to the other radioactive materials.

MR. STIVER: And plutonium, is of concern, but you know, we also have this issue of the high technetium in the green salt plant, which seems to be in inconsistent with our initial premise about the high level waste uranium coming into that plant.

So, that has nothing, what so ever, to do with the produced processing. The concentration in the metals production plant, the strontium --

MR. ROLFES: Sure, but if you were to take a look at the dosimetric impact from all the various radionuclides that are coming on the site, plutonium and neptunium are going to account for the majority of the small fraction, in addition to the uranium.

I mean, the uranium alone is going to be the majority of the --

MR. STIVER: Well, I understand from a dosimetric standpoint, that it makes perfect. As a health physicist, a practical kind of guy, say, yes, two and a half percent or 100 parts per billion, it's basically lost in --

But we have this issue of concentration. We have this issue of uncertainty, as to whether that 100 is bounding, even for the shipments that came in, when you look at the graph of the hoppers that came in -- or the table here, table one, 7,700 parts per billion in one of the hoppers here.

I mean, there's a lot of residual material that's found its way into the plant.

MEMBER SCHOFIELD: And that recycling of the light -- the mag chloride is just going to increase those concentrations.

MR. STIVER: And there is something we don't have here, which is important, is we don't have anything for Plant 3, for the raffinates produced from the tower ash. We don't have any information about the guys that handled that.

You could say that, well, it's being -- you know, it's enclosed in pipes and workers are protected from it, but the --

MR. ROLFES: There is a handful of exceptions, and I understand that there is certain interest in these and concern over these exceptions. Those received a lot more attention than the normal routine material.

If you take a look at the thousands of shipments and the quantities of plutonium in the uranium that was sent between Hanford, Savannah River Site and Fernald, we're talking one or two parts per billion, in the earlier years, you know, 10 parts per billion, and really, we have a sound basis to use a 10 parts per billion control level for plutonium.

We defaulted a factor of 10 higher, to 100 parts per billion --

MR. STIVER: All right, but you used a factor of 10, to be on the safe side and C-

MR. ROLFES: And you know, if you continue to look -- I mean, the real concern isn't until that tower ash shipment, in the late 70s, and still, I mean, if you consider the handful -- you know, we've got a handful of shipments, you know, 10, versus the thousands that were done --

MR. STIVER: I know.

MR. ROLFES: I mean, to focus on one very unusual occurrence, when there was much additional concern by the Health and Safety staff, you know, to say that, you know, everything else could have been that way isn't a true representation.

MR. STIVER: Well, I'm not saying that it -- this is a whole issue of, you know, do you put everybody in the highest exposure category or is there the possibility that one person could be in that high exposure category? It's kind of a paradox, you know.

It's like, if you set an error rate of five percent, you know, any one person who engaged in that activity has a five percent chance of having a negative outcome or whatever, but take 20 people, one of them is definitely going to get it.

So, we have the situation where you've got a small number of workers, potentially, you know, guys who were working in the dirty jobs in Plant 5 or handling raffinates, people who had been in the green salt plant, exposed to high levels of technetium, so, they're a small group and they might be a sub-population, but you know, in an SEC context, you really have to demonstrate that you can reconstruct the doses for all categories of personnel, for all periods of time.

MR. ROLFES: As far as technetium, though, if you take a look at the dosimetric impact, we don't assign doses of less than a millirem per year in our dose reconstruction.

MR. STIVER: Yes.

MR. ROLFES: So, I mean --

MR. STIVER: But your defaults have the values in there, so, you know, and we're questioning the veracity of those defaults.

MR. ROLFES: When we complete a dose reconstruction, we look at the bioassay data, reconstruct the uranium intake, calculate the uranium internal dose in a claimant-favorable manner, and then on top of that, add in 100 parts per billion, 3,500 parts per billion of neptunium in the 9,000 parts per billion of technetium-99.

Technetium-99 doses don't result in anything.

MR. STIVER: Well, I see where you're basis for the 100 parts per billion plutonium comes from. I'm not quite sure where the neptunium and the technetium values came from.

MR. ROLFES: Okay, well --

MR. STIVER: Now, according to your White Paper, they came from that table five, and until we -- we argue -- table five is highly questionable.

MR. ROLFES: Table five?

MR. STIVER: That's the process --

MR. ROLFES: Yes, I mean, again, if you take a look at the other shipments, though, what you're referring to here are the exceptions that exceeded the control level at the site. These are the ones that received additional --

MR. STIVER: Well, basically --

MR. ROLFES: Contamination levels, if you go back and look at all the other thousands of shipments, you'll see one or two parts per billion plutonium, three, sometimes five. If you look at the average, they're all less than 10 parts per billion, historically.

We based our 100 parts per billion default for plutonium, based upon these handful of shipments that exceeded the levels, and in addition, there were, you know -- it was during this time period, these shipments prompted the plutonium bioassays that we reviewed.

MR. STIVER: No, I understand, but what you -- where do you get the neptunium value of 3,500? Where did that come from?

MR. ROLFES: That was based upon the ratios, I believe, that was -- I'd have to ask our subject matter expert, but I believe that was based upon the relative production of neptunium. I don't know if that was based upon actual measurement data. I don't know if that was --

MR. STIVER: Well, if it was based on table five, we know it's not actual measurements. Jim Warner gave a pretty compelling argument, why we wouldn't believe that.

MR. ROLFES: It could just be from, you know, first principles, you know, the production of neptunium in a reactor. I mean, that's -- I'd have to check on that.

MR. STIVER: Yes, the values change a lot, and looking at magnesium fluoride, I keep getting back to that, but I look at the statistical basis for this, that was provided in attachment one, to the 2000B, and there is 100 times difference between the bootstrap mean and the high value.

That doesn't say that -- you know,

they could be a few exceptions, but when you look at the log-normal means, you might end up with factor of four higher on that.

So, I've got a problem with that 3,500 number, and the 9,000 number, and granted, I know technetium doesn't contribute much to dose, but you have it in there. It's obviously there for a reason. It was measured. It was considered important at the time that these tables were produced. You have the other fission products that really aren't accounted for. We don't know what the situation with cesium is. The value seems exceedingly high.

But we have the strontium-90 issue. We have the concentration issue in the metals and in Plant 1.

I guess, we've just got to come to some kind of consensus here. Maybe not today, but it would be nice if we could do it today, as to what is a bounding value that's acceptable and what is the technical basis for it?

DR. MAURO: I stepped out for a minute. I had to do something important, that I had to get something, so, I apologize for that.

So, I may be asking a question that you already talked about, while I was out, but I'm looking at table two, metal production, this is number 5, what -- and I when I read this, I say, what is it telling me?

So, okay, it was 14 dust sample collections and basically, the highest value of plutonium that was observed is this 5.19, that's the upper range, which is probably close to 5.19 microcuries per kilogram of uranium.

Okay, so, one would argue that well, out of the 14, the highest values, knocking on the door of your 6.3, okay.

Now, on first blush, one would say, that looks like it's okay, right? But then I said, wait a minute, wait a minute, what is this 5.19?

This is -- as it visualize it, there is a building, the building 5, is that what that is?

MR. STIVER: This is building 5, the metal production.

DR. MAURO: Now, we've got a building, all right, and it's got all these different areas, HVAC systems with headers going to each room, headers come up and meet, they go up a stack some place, and some place along the line, there is a filter collector. So, it's an integrator, all right.

MR. STIVER: It's an integrator.

DR. MAURO: Integrator, so, it's collecting dust that's coming from a lot of different locations, all right.

Now, so, now, right off the bat, watch out a little bit, that means this 5.19, that was the highest value of the 14 samples. It represents, though, the average concentration in the building, because it's drawing from the entire building.

So, in theory, in my mind, there could have been some locations that are relatively low, and others, you know, if different campaigns were going on, different mixes were being used.

Then I say, but not only that, that 5.19, that is the dust that's coming from the building, going up the ducts. How long C-how many -- is this like one month worth of --

MR. STIVER: We don't know that. We don't know what the time integration on that.

DR. MAURO: So, this could be years?

MR. ROLFES: Conceivably.

DR. MAURO: Okay, let's make believe this is, okay, let's say, 5.19 represents the highest of 14 measurements of dust that was collected that represented the average dust loading in a building over some extended period of time, could be months, could be years, don't know.

So, then I ask myself a question, is it possible that at a particular location in that building, in a particular year, that the number of the actual ratio, is a lot higher than 5.19?

I can't -- I don't know that, and the --

MR. STIVER: You can't say that it isn't, because --

DR. MAURO: You can't say it is. You can't say it isn't.

So, one of my problems has always been, is whenever we've been talking about recycled uranium, we're always talking about it in the aggregate. We've got this -- for example, you know, you've got a sample that represents some kind of integration over space and time that may be the highest value in that building, out of 14, which is certainly, in your favor, but at the same time, I don't want to be surprised and find out, well, you know what? In this year, in this portion of the building, there was a shipment that came in, that some group of workers worked on, that could have been 10 times higher than that. I don't know. I don't know, one way or the other. Maybe not.

But you see, this is the -- the way I look at it is, I just let the data tell me what it tells me.

Now, an argument could be made, I heard you say something very important, do we have actual samples of the -- I picture these trains showing up at the back door of Fernald, carrying loads of uranium. Were they sampled? Each time -- before, they were let in the door, and they showed up and they went into their various campaigns for recycling.

Is there samples that were taken to confirm what the plutonium levels were in the stuff that arrived at the door, or we don't have data like that?

MR. ROLFES: I've seen bits and pieces of it. There are certainly, uranium samples that were taken and process samples and things.

As far as, you know, how much we would have, a better place to find it might also be the originators of the material, you know, where the material originally came from, where it was shipped from, to Fernald.

MR. STIVER: Yes.

MR. ROLFES: So, it's in the --

DR. MAURO: If I could have hung my hat on the spec, you know -- we heard a story this morning, you know, you can't --

But we heard enough today, you say we've got to be very cautious, saying that everything always was put into the spec.

I'm sort of like, saying, well, listen, I can't put my money on that. So, what I'm left with is this data, okay, and I say, all right, what can I do with this data, and the data tells me that on average, when you integrate over space and time, several of the -- certainly, I would say the scrap recovery, well, you know, you got 4.54, knocking on the door of the 6.3, and it is possible, over some relatively short period of time, or some location, you could have been much about that? I don't know.

Of course, we got to these other ones, you probably talked about the sampling plant. Is there any reason why we're just disregarding the --

MR. STIVER: You were out.

DR. MAURO: I was out, when you talked about that?

MR. STIVER: It was kind of intimately related to Plant 5, because there is this process by which the slag comes back through the sampling plant and this Titan mill is used to reprocess it.

DR. MAURO: Is this within play or not? Is there a reason to take this off the table?

MR. STIVER: Absolutely not. I think it's got to stay in.

DR. MAURO: Well, I mean, do you agree that the Titan plant, the dust -- the dust the workers might have experienced at the -- at sampling plant one, needs to be part of the -- at play, and we have to factor that in.

What do you do when you realize that you average 38, as compared to 6.3? We have to move off the -- in other words, are we off by this factor of whatever the multiplier is, five, six?

In other words, is the 6.3 too low, when it comes to Plant 1, sampling plant? I mean, that's what it tells me, and if it is, then that means that something -- you know, the number right now that you're using is not going to work, but there may be a good reason to take it off the table. Is there any reason why you'd want to disregard the sampling plant Titan mill data, what we're looking at there, that 38 number and the 219?

MR. ROLFES: Well, these are dust collectors, essentially, not occupational air concentrations, and so, it's a little bit different. It's sort of like taking a process sample to understand --

DR. MAURO: Okay, good, so, you're saying that this air that's being collected, okay, I could see that, stay with me.

So, you've got a glove box and you're venting that glove box. Well, it would be crazy to say that the air the person is breathing is the same as the air in the glove box. If that's the case, then I back away.

But if that dust sample really represents the integration of the air in the building that's being headed on, and going out and it hits this filter, then the people in the building, on average, are experiencing 38.05, and you're saying that's not true?

MR. ROLFES: It's not representative of the air concentrations. It's more of a process related, I mean --

DR. MAURO: Do we know that? That's important.

MR. ROLFES: The Titan concentrations -- yes, maybe Bob Morris or Mel Chew could verify, you know.

What we're talking about is table two in this handout, and I, unfortunately, I know you guys don't have that, but what -- we're discussing the Titan mill data, and we've got some different ratios of the concentrations which appear to exceed our defaults in the Technical Basis Document for Fernald.

We've got some, you know, plutonium samples taken from the Titan mill -- or taken from -- some process samples, essentially, is what I believe they are, taken from the Titan mill, which indicate, you know, higher concentrations of plutonium and neptunium.

Is that data -- that's not reflective of the air concentrations in the operational plant. That is more of a process sample. If you might be able to, you know, correct me if I'm wrong, or elaborate on that, I'd appreciate it. Mel or Bob?

MR. ALVAREZ: I think we've got to defer that to Bryce, when he's available.

MR. ROLFES: Okay.

DR. MAURO: I think we just -- so, see, I am convinced that if we all look -- looking at the data, and we all understand what the data means, there's not going to be any disagreement around this table. We have to just understand what we're looking at.

The only disagreement would be weight of evidence. There might be different judgments, but I don't want there to be any disagreement of factual information.

MEMBER GRIFFON: I agree with that.

So, I want to understand what that 38 is, and if we all agree that 38 represents air that people could have been breathing, and it represents average for a building that perhaps, extends over a long period of time, that tells me very clearly, that your default 6.3 is not going to work.

MR. STIVER: Hey, John, can I say something here?

DR. MAURO: Sure.

MR. STIVER: I think what have here is we have a particular piece of equipment within a sampling plant, and the question, in my mind, for the purposes of dose reconstruction is, is that sample -- or is that plant -- this is taken from the dust collector, so, I assume it's like a big funnel up above the piece of machinery, like we saw at Simonds Saw, when you went for the visit there.

And so, this stuff is sucking up all this dust, while these guys are working there, and there was a filter or some kind of sampling mechanism up there that integrates over a period of time, and we don't know what that time period is. It could be a week. It could be a month. It could be a year. It could be multiple years.

What concerns me, is there potential -- and it's not going to necessarily represent an entire building. It's going to represent some portion of a building, for certain workers who were involved in that process, and to what extent is that dust collector efficient at removing that material and to what extent are other workers in the vicinity of that mill subjected to that concentration?

DR. MAURO: So, when you call this a dust collector, is this something, like a device that was installed to help clean the air before people breathe it, or is it just the dust that's collected at the back end of the process, before it's exhausted, so you get a sample of the air, before it's exhausted?

MR. BEATTY: Most of them backed off the process equipment, they pulled back in and sent it up through the dust collector, before it goes out of the plant.

DR. MAURO: Okay, are the people that are working -- are they breathing in the same air that's going up to the dust collectors?

MR. BEATTY: No.

DR. MAURO: They are not?

MR. STIVER: It's integrating. It's concentrating.

DR. MAURO: It's higher? The stuff is going to the dust collector -- it's concentrated, the dust -- so, really, then --

MR. STIVER: This is on per uranium basis here. So, this is the fraction --

DR. MAURO: It's the ratio? Oh, I'm sorry, you're right, it doesn't matter.

MR. STIVER: Right.

DR. MAURO: It doesn't matter.

MR. STIVER: Also, adds another wrinkle into this issue --

DR. MAURO: It doesn't matter.

MR. STIVER: The other wrinkle is,

you might have had --

MEMBER GRIFFON: Five years of processing after the --

MR. STIVER: Yes, but what if you have one --

MEMBER GRIFFON: One week of processing --

DR. MAURO: I got lost in the woods, you're absolutely right.

MR. STIVER: You could dilute -- that's the other factor, nobody has talked about a dilution factor.

CHAIRMAN CLAWSON: One person at a time, please.

MR. STIVER: Okay.

MEMBER GRIFFON: It was hard to get in between --

CHAIRMAN CLAWSON: I know, between John and them --

MR. STIVER: I guess if you just had one week -- or just the one short period of time, whatever it happens to be, where you're handling this plutonium, and then you had a year or two of just uranium, and you can see how the dilution is coming into play here.

This 38 may actually be -- referring to that week when the material was present, it might be 400 or some other higher number.

DR. MAURO: It's a ratio.

MEMBER GRIFFON: Can anyone tell me why this report -- why this study was done? Is there are report that goes with the study? It may just be that I've forgotten, to this point.

MR. STIVER: I believe there is a 1987 --

MEMBER GRIFFON: What was the intent of the research, is what I'm getting at?

MR. STIVER: I think it was just to try to begin to get some sort of handle on what the --

MEMBER GRIFFON: I think it was --

if it's anything like the similar sampling I saw at Paducah and other places, it was to establish presence. They were concerned, where did they have plutonium and neptunium to get, and really, it was to establish presence.

I don't think you're going to get -- I don't think this is representative either way, high or low, necessarily, because I can see a dilution factor, but I can also see Mark's point, and that is it's concentrating it from a processing --

DR. MAURO: Well, it's the ratio, you see, the -- I was corrected. In other words, it doesn't really matter.

MEMBER GRIFFON: But the ratio is affected by later processing, if it's done five years after --

MR. STIVER: But if anything, it's going to be a diluting factor, if you're not --

MEMBER GRIFFON: So, either way, it's kind of like --

DR. MAURO: No, it only could be worse. In other words, if we're looking at an integration --

MEMBER GRIFFON: The ratio would go lower, right?

DR. MAURO: It's only --

MR. STIVER: It's going to go down if you dilute it out.

MEMBER GRIFFON: Assuming you're doing pure uranium -

MR. STIVER: Yes, yes.

MEMBER GRIFFON: It could only go down after that Paducah --

(Simultaneous speaking.)

MR. STIVER: Well, another problem is, the other one we get -- well, we're talking about the sampling plant, that would probably be true.

Although you might actually have an increase due to what's going on in the metals production plant, because you concentrate the material in the slag, and then you process it in Plant 1, you're adding more material in, every time you go through that cycle.

MEMBER GRIFFON: Yes, the flow, but --

MR. STIVER: Yes, assuming that you just had a simple flow diagram, but for this plant, and the other -- each time you go through the metals plant, you're adding another increment of TRU and fission product.

MEMBER GRIFFON: I'm just saying that there's a lot of uncertainty.

MR. STIVER: Absolutely, yes, and that was my point about the need for, you know, trying to find additional corroborating data, if such data exists.

CHAIRMAN CLAWSON: Let's also look at this time frame, too, when this was in. This is when the DOE orders were coming out that basically, we were going to one RadCon program.

We know, for a fact, that Fernald

was not run as a -- it was run as a heavy metals plant. That happened to go over to the DOE standards and start a lot of different monitoring and what I think you're saying there, Mark, is true, is that this is to establish what was really there -- what was --

MEMBER GRIFFON: Well, I would like to know, do you have the reference where this --

MR. ROLFES: No, I don't.

MEMBER GRIFFON: Because I would imagine they did a write up with this.

DR. MAURO: John prepared this over the weekend.

MEMBER GRIFFON: No, but the sampling came from somewhere.

DR. MAURO: Oh, the data itself, the original data.

MEMBER GRIFFON: And why -- you know, if it was in a report -- I'm assuming it wasn't just a random piece of paper.

MR. ROLFES: We need to take a look

at it.

MEMBER GRIFFON: Yes, yes, because maybe the authors will say, you know.

MR. STIVER: Well, we're not going to solve that today, but we can see if there's a reference to it.

DR. MAURO: One of the things we never talked about, and I'm not prepared to talk about it today, is that when you're at 100 parts per billion of plutonium and you're inhaling that, it has dosimetrics that are relative to uranium.

If you're just inhaling uranium-234, you know, whatever, say, 10 MAC, you're inhaling per year, you're going to have a dose delivered or dose commitment to the various organs in your body.

Instead of that, you also have -- also, you have the 10 parts per billion of plutonium. What is the dose going to be to the different organs now? Now, we're talking about a big effect.

Now, we're going to go back and look at those numbers, and you folks need to also, because it's important to know that is 100 parts per billion dosimetric really going to change it?

In other words, all of the sudden, do the doses to the bone, let's say, that go up by a factor of 10, if you have 100, compared to -- if it wasn't there.

If it turned out to be instead of 100 parts per billion, some people might have experienced 1,000 parts per billion, because apparently, there was some places where it was that high, some batches, somewhere. Does that, all of the sudden, increase the does off the charts, that that person would get, if it turned out, it wasn't 10, it wasn't 100 or 1,000?

And right now, I have not sense, and I should, but I don't right now, have a sense for the magnitude of importance of having these so-called trace levels of plutonium.

You had pointed out that technetium really isn't a big contributor. I know technetium-99 is a pure beta emitter, it takes a lot of it, it takes a lot of it to give you a big dose.

But plutonium-239, no. Plutonium-239 has a very high dose conversion factor, for different organs. So, it may turn out that 100 parts per billion is not insignificant, relative to the uranium contribution, and I think that needs to be put on the table, so we understand how important this difference is.

CHAIRMAN CLAWSON: Well, you know, to be honest, we didn't get these until last Friday. You guys really haven't had a chance to be able to respond to this. We want to proceed forward with this work.

So, let's take a look at what the action item would be. One of the things is, is that we need SC&A to be able to write a response to this, an official response back to NIOSH, so that we know what their stance is at, and John, this is at the time that you could do those calculations that you're thinking of, and so forth, and we need to give that to NIOSH, although, we know -- so, they know where we're coming from on this stance here.

My personal opinion is, is we could try to validate this and we're already into the same thing. It's the question of integrity of the information that we're dealing with, and I don't know if we'll ever come to that, but let's -- we've got to start out with a report from SC&A to NIOSH on this, and go from there.

Also, too, Paul, are there any questions that you have? You know, I know that you're not feeling all that good and stuff, but is there anything that we could help clarify for you?

MEMBER ZIEMER: No, I think what

you proposed, Brad, is the way to go. We need to get a formal response from SC&A on this.

CHAIRMAN CLAWSON: Okay, well, I just want --

MEMBER ZIEMER: I'm having a little trouble with locating the documents that they said were on the O: drive today, those ones mentioned earlier. I don't find them, but --

CHAIRMAN CLAWSON: Well, I have that trouble quite often, myself, but okay, well, we'll continue it on, and I'd like to make sure that these are sent out in the response, with SC&A, so that we have -- and that everything is PA cleared, everything is --

MEMBER GRIFFON: Is there any other action for NIOSH, at this point? I mean, the one small thing that I asked was for the reference, for that dust C-

MR. STIVER: I've got that reference. It's in the report.

DR. CHEW: You have it, okay.

MR. STIVER: It's in the White Paper.

MEMBER ZIEMER: Was Table 5 on the Titan mill, what document was that referring to?

MEMBER GRIFFON: Table 5, I think he's talking about this --

MR. STIVER: Table 5, that was what NIOSH --

MEMBER ZIEMER: What paper was that?

MR. STIVER: That's from the NIOSH White Paper. It's an extract from that White Paper.

MEMBER ZIEMER: Which White Paper?

MR. STIVER: Hang on a second. It's the dose reconstruction considerations for RU contamination of Fernald. I believe that was one of the NIOSH White Papers that's been sent out, in relation to this.

MEMBER ZIEMER: Do you have the data on that one?

MR. STIVER: What is it, 2008, Mark?

MR. ROLFES: Let me -- I don't have it open, and let me see if I can C-

MEMBER ZIEMER: Okay, I can go back and pick it up. I was looking at more recent documents and didn't pick it up, okay.

MR. KATZ: Yes, the authors are Bryce Rich and Paul Ruhter.

MEMBER ZIEMER: Yes, okay.

MEMBER GRIFFON: What's the reference for that raw data in that report?

MR. STIVER: Fernald 1987. FMPC-2082.

MEMBER GRIFFON: Okay.

MR. STIVER: The history of FMPC radionuclide discharges.

MEMBER ZIEMER: Okay.

MR. STIVER: There is a whole series of authors, here.

MEMBER GRIFFON: That's right.

MEMBER ZIEMER: Yes, I'll track it

down.

MR. STIVER: It's 1987.

MEMBER GRIFFON: Paul, that was the reference for the data in that table, the dust sampling that they did in 1985, or thereabout.

MR. STIVER: Correct.

MEMBER ZIEMER: Right, right, yes.

DR. CHEW: Thank you.

MR. STIVER: And the raw data, or the summary data are included in the Appendix B of the White Paper that we just mentioned.

MEMBER ZIEMER: Right, thanks.

MEMBER GRIFFON: But is there action for NIOSH? I'm just wondering about the source-term verification issues, and I'm not sure what we can ask, in that regard, whether -- you know, I think we've asked it before, on the raw data. I don't know if Bryce has anything to add to that, if he's on the phone.

MR. STIVER: From what Jim told us, it's not available.

MEMBER GRIFFON: Yes.

CHAIRMAN CLAWSON: You know, I'm going to be honest with you, this is part of the issue. This information and this data is sparse and convoluted, and why did we even get some of this?

That is the frustration with this. I've said it before, this whole program is set up like a big computer, and it's only as good as the information we put in there, and if there is questions of the material, that's where we get into trouble, and I really don't know how we would -- on this recycled uranium, how we'd be able to really verify it.

I've dealt with the -- I know the errors that we've had in the programs. We deal with them for years. Yes, we've got them better, as we've grown up, but I really wonder.

So, I guess I'm looking to all of you, on this, because actually, what we have to do is respond to NIOSH. SC&A has to respond to their recycled uranium and generate a paper for it, so that we know our stance.

But is there any -- you know, like you say, Mark, is --

MR. KATZ: Well, is there an additional explanation that you just -- I thought someone referred to getting additional explanation from Bryce Rich, about --

MR. ROLFES: Right, we were going to consult with Bryce, to see -- well, we had been talking about Table 5, --

MEMBER GRIFFON: Table 5, yes.

MR. ROLFES: And whether they were processed, samples versus, you know, representative of the air concentrations in the buildings.

MR. STIVER: But I think that was what we already established, because that came directly out of DOE-2000B, that information comes exactly, right out of the table in the DOE report.

So, NIOSH really didn't do

anything beyond what has been done by DOE.

MR. KATZ: But we're talking about the dust collector information.

MR. STIVER: I thought you were talking about the --

(Simultaneous speaking.)

DR. MAURO: If you recall, when I came in, there was this Table 2 I was looking at, and that's, you know, one of these, and I was looking at that -- the 38 -- the sampling plant, number one, where there is an average ratio of 38.05, and that ratio -- and my question goes toward, well, that's certainly higher than 6.3 and it's an average for a building. Is it possible that -- first of all, that number, that's higher than the 6.3 ratio.

In addition, is it possible that there were time periods within that building and locations within that building, where the airborne dust loading over some time period could have a ratio that's even higher than

38.05?

MEMBER GRIFFON: Right.

DR. MAURO: And that's when we asked Bryce.

MEMBER GRIFFON: Right, yes.

DR. MAURO: That's when Bryce came in.

MEMBER GRIFFON: Yes, this is all about the dust sampling and the --

DR. MAURO: The dust sampling issue. To me --

MEMBER GRIFFON: What we have for the source-term, what came in the door?

DR. MAURO: Or coming in, the shipments?

MEMBER GRIFFON: Yes, the shipments, yes. Beyond the DOE report, it seems like NIOSH didn't go any further than that, but it's -- they didn't get -- they didn't look for the source data --

MR. ROLFES: We looked at the thousands of shipments described in recycled uranium flow report, and we took the bootstrap means, I believe right -- Bryce Rich had done that, took bootstrap means of each of the shipments and came up with the average, and --

MEMBER GRIFFON: When you say, looked at the shipments, you didn't go back to --

MR. ROLFES: We didn't -

(Simultaneous speaking.)

MEMBER GRIFFON: Right.

MR. ROLFES: Right, we relied upon the report.

MEMBER GRIFFON: Right, and I don't even know if it exists. So, that was the question --

MR. STIVER: In our discussion this morning, it appears that those chemical process data don't even exist anymore, and that's really what we heard.

DR. MAURO: That's what we heard.

MEMBER GRIFFON: Well, that was their speculation.

MR. STIVER: Well, actually, he was involved in compiling that report, pretty intensively, Jim Warner was.

MEMBER GRIFFON: Yes, well, what data did he use to compile? Somebody had to have the data --

MR. STIVER: A lot of that was based on process knowledge. They just -- they were best-guess estimates, and that's what --

MEMBER GRIFFON: It's a house of cards, that's the problem that I have. I mean, if there's nothing there, then there's nothing there.

MR. STIVER: Well, that's why we -- we determined that those 19 process means of bootstrap means, earlier, are not -- they're highly questionable.

MR. ROLFES: I think that was also one of the reasons that we decided to add in a factor of 10 conservatism. I mean --

CHAIRMAN CLAWSON: So, we're actually -- our action items, you guys have got to generate a White Paper response for NIOSH, and I guess, I'll ask you, Mark, I'm not -- Bryce Rich, I guess we ought to -- my personal opinion is, is we've got to look at it, of where this data came from.

MEMBER GRIFFON: Well, that's going to be two questions, I think, the one on the dust sampling question, but then the other, where -- at least, the status report on, to date, have you found any of this raw data to -- or the DOE report?

I don't know if it's turned up in the -- you know, or if they've even looked for it.

MR. ROLFES: Since Fernald was part of the Oak Ridge Operations Office at the time, you know, when many of the shipments were sent, do you if it could be at Y-12, or something, perhaps?

But no, we didn't go back and look for, you know -- to make sure the DOE averaged the numbers correctly and things, and made sure that -- you know, I mean, there is -- I think we discussed this before and there is thousand and thousands of shipments, that you know, ultimately went -- I mean, it's just -- we could do a lot of work, but once again, it's not going to get us to, you know, a better understanding of, you know, what we have right now.

MEMBER GRIFFON: Well, validating it doesn't mean necessarily looking at 100 percent of them, either. Some shipment records, you know, to corroborate with the DOE -- you know, you're just sort of validating it, not 100 percent validation. I don't know.

I've got to believe some of these records still exist, and I would like to hope they would, anyway.

CHAIRMAN CLAWSON: But you know, that looks -- so, let's have NIOSH check into that, and also, this sampling, what drove the sampling plan?

MR. KATZ: So, just to button up on

NIOSH, so, that might even be just a memo from Bryce Rich, to the Work Group, whatever. It doesn't have to be formal, depending on what extent he needs to dig, to answer the question.

MR. ROLFES: Right, we can send an email and let you know in an email and then, you know, if we find, you know, that we already have some of the data, perhaps you know, we can look at it and we can, I guess, take that step when we accomplish the first part.

MEMBER GRIFFON: Right.

MR. KATZ: All right, sorry to interrupt.

CHAIRMAN CLAWSON: Oh, no, I'm having a real hard time getting a handle on which way we're going, because this comes back to the whole thing of the data, if the data is flawed, then -- and I'm just trying to figure out which way we can go.

We've already spent how many years

trying to get to this point, and where else do we go, you know?

So, Mark, do you understand what we're asking from you, for Bryce Rich? You know, I know that you want to refer to him, and go from there. It is clear, what we're asking?

MR. ROLFES: Yes, we'll look to see if there's any, you know, data that supports -- we'll look to see if there's any original data from the recycled uranium field office report, for Fernald, and then, also, look at the source of this Table 2 results here and determine, you know, what prompted this and why -- why the samples were collected, what kind of samples they were --

CHAIRMAN CLAWSON: Would that be sufficient and make everybody happy? Paul, are you okay with the way we're proceeding on this?

MEMBER ZIEMER: I'm good on that.

CHAIRMAN CLAWSON: Okay, SC&A is

going to do a formal report back, because we understand, you guys didn't have time to do this. We'll go back --

MR. STIVER: If we could get the memo from Bryce Rich, that would help us, in determining the quality of this data.

CHAIRMAN CLAWSON: Okay, well, that put number three into motion, and we've got issue four, which is radon breath data review, and --

DR. MAURO: Yes, I know folks didn't submit a report yet, and just so everyone knows what that is, there are workers that worked at the site, that handled, I guess we would call it raffinates, which included a mixture of radium and thorium progeny, the uranium separations.

And one way to get a handle on what exposure they were --

(Simultaneous speaking.)

MR. ROLFES: Small quantity of uranium.

DR. MAURO: Small quantity, very small quantity --

MR. ROLFES: Very small, about five percent of that came in --

DR. MAURO: Right, so, we took a urine sample, you do an analysis of the urine, you know, you're not going to be -- you're not going to find uranium in these workers, and if you're not looking for other radionuclides -- we know that they worked with these raffinates, you've got good reason to believe that others inhaled some radium-226, some thorium-230. How are we going to estimate the doses of the workers who worked with that particular product, with raffinates, and include stuff that went into the silo, the K-65 silos?

Now, you came back with an answer and said, "Listen, we have a good portion of the workers that were doing that. We have radon breath analysis," -- the exhaled breath from a person contains radon, and it turns out, there is a very well established, scientifically sound method to say, "Well, I could predict what the radium -- the radium-226 body burden is," by measuring how much radon is in a person's breath.

If we review that OTIB and we have -- and we like it, that's a good way to do it.

Now, and you folks are -- your plan is that, well, we'll collect all that data and we'll develop a model, to say, "Here is all the data we have for -- on radon breath data," and in theory, you could build a coworker model that says, here is the distribution of radium body burdens, and workers that might have worked with this material had experienced some -- and on that basis, you know, for the people that we do have the data, we could reconstruct their doses.

For the ones that we believe might have been exposed to this, but we don't have radon breath data, in theory, you could build a coworker model, perhaps, assign the upper 95th percentile. We're okay with that.

But here is where the challenges come in, and how we sort of left it, in our White Paper, how were you going to identify the workers that you're going to assign that to?

Now, there's workers that might have done it, but you don't know. So, you have this problem of saying, who are we going to put in that box? People who worked with the raffinates, with the radium raffinates, that's issue one, and how are you going to deal with that?

Issue number two is another subtlety. So, it turns out -- and I didn't know this, but Arjun explained it to me, and others also, it turns out there actually some streams, waste streams that were handled at Fernald, where it was -- the radium was separated. It wasn't -- and it just was thorium-230, without the uranium, with the radium, so, now, you've got just thorium-230.

Well, what happens now? Now, how do you estimate the amount of thorium that person inhaled, because you can't use radon breath analysis, because there is no radium. You can't use the uranium in the urine, because there is no uranium. There is just thorium.

Now, what method are you going to use to reconstruct the doses to workers who might have been handling just the thorium, and one -- and maybe you could come up with a method to do that, but you're probably looking into that, but once you do that, you've got the same question again, who are you going to put in that box, the people that you're going to assign?

Let's say, you can build a coworker model, for thorium exposures, and you're going to have to decide then, who are the people we're going to assign that to, all right? That was the issue.

MR. ROLFES: Correct me if I'm wrong. I believe we sent out a White Paper, prior to last Working Group meeting, on the dose reconstruction considerations for the raffinates, and that did include an assessment of the materials that went into Silo 3.

I believe that -- I'd have to look back, but I believe that we have a methodology that we proposed.

But one of the things -- since we're discussing the first issue, the estimation of radium body burden, using radon breath data, the methodology we have basically had since the SEC petition -- as a matter of fact, before that even, the first go-around of the Fernald TBD back in 2004, had a default to assign radium and raffinate concentrations to workers that were involved in dumping the K-65 materials into Silos 1 and 2.

If we look at the data, the radon breath data, that we have referred to in the SEC petition presentation, I believe we had done a dose estimate, using a radon breath sample, and provided that to the Advisory Board.

But if you take a look, we have gathered the data, the radon breath data, looked at the positive results and come up with a distribution of the concentrations, and it turns out that our default, in the Site Profile right now, is higher than what the actual data, the bioassay data indicate.

So, we have put together a report, and unfortunately, we didn't -- we were trying to consolidate all these internal doses issues into one consolidated report, over the past several months. That's one of the reasons that we had a due date, back in September, that we had hoped to get this completed by, and then it was -- you know, there were other shifts in priorities.

And so, the consolidated report didn't get completed, but we have individual reports for many of these things. This raffinate coworker model, if you will, is embedded within this larger consolidated report, and we didn't break it out separately for -- you know, as a separate White Paper. We had hoped to get this consolidated report out.

But we've got something documented in there. If we can't get the consolidated report out in a short amount of time, then what we'll do is try to break out that radium body burden, or the raffinate discussion back out, and send it to you guys for review.

And also, look at that compared to the other reports. You know, early on, one of the indicators of these individuals that were working with the raffinate material -- they had quite a bit of high external doses on their film badges, just because of the radium content of the material.

And so, they were rotating workers out of the area, because of the -- so, there is some indicators that might help us to identify who would have been exposed to the raffinates, and you know, when it comes down to it, for a dose reconstruction, if there is a uncertainty that the individual was involved in this operation, we would assume that they were, and we would assign that intake of raffinates to them, as claimant-favorable, benefit of the doubt type situation.

DR. MAURO: Okay, so, then we're going to see a report.

MR. ROLFES: Yes.

DR. MAURO: Yes, great, looking forward to it.

CHAIRMAN CLAWSON: Okay, an issue for, I guess that's in NIOSH's hands, we're still waiting for the report on that, but we've also got the issue five, and that's on the radon release K-65 silo.

They have generated a report for that.

MEMBER GRIFFON: Can we -- I hate to go back, but can we just go back to --

unless we're going to be kept in suspense.

How are you going to handle the thorium question, yes? Can you tell us --

MR. ROLFES: From what aspect?

MEMBER GRIFFON: From the stream that John described, where it's radium deficient and it's mostly thorium?

MR. ROLFES: Okay, that would have been the time period, first of all, we're talking about the early time period where they were filling Silos 1 and 2, and they were processing the higher uranium content ores that didn't have the benefit of being striped of the radium at the process mill.

I don't have the specific dates, and I know Bob Morris is on the line. I'll give a brief introduction, and see if Bob might be able to add anything to what I have to say, or correct me.

But I thought this was --

MR. MORRIS: Mark, this is Bob. Give me a minute or two, to look at that again, before you put me on the spot.

DR. MAURO: How do you --

MR. ROLFES: Bob, you're on. All right, basically, in 1958, I believe, there was a change over where they started receiving the milled ores from the local United States mines, and so, it's really that time period, where we were receiving at Fernald, the ore concentrates, which were striped of the radium bearing materials.

I believe what we had done, previously, is put together -- I know we had looked at the daily weighted exposure reports and looked at the concentrations of contaminants in the refinery, where they would have been processing the ore concentrates, and from what I recall, this operation was essentially, one of the cleanest on the site.

I mean, Plant 2/3 air concentrations, since it was a wet process, it was pretty low air concentration hazard.

But you know, the contaminant

there was, essentially, the thorium-230 that was of concern, and I'll have to rely on Bob, to hopefully --

MEMBER GRIFFON: If you give him enough time.

(Laughter.)

MR. ROLFES: Basically, the process, ultimately, the contaminants, after they extracted the uranium from this ore concentrate, they sent the contaminants to be calcined and blown into in Silo 3.

So, these were low moisture contaminants that were blown into Silo 3, were striped of radium, but still had the same essential concentrations of thorium-230 and the other contaminants in there.

Bob, did I give you enough time to look at --

MR. MORRIS: Not really, I was still trying to find the original document.

MR. ROLFES: Okay, I know we had also spoken with a couple of long-time process engineers, and personnel from the Fernald site, who were knowledgeable of the changes in the process and the material source, and I'll have to take a look back. It's been a while, since -- I think we sent out our report on this issue, back prior to the previous Working Group meeting in January of this year. So, from memory, I don't recall at this time. Let me --

MR. MORRIS: Mark Griffon, Could you put this on hold for about 30 minutes and come back to it?

MEMBER GRIFFON: Yes, sure, sure.

DR. MAURO: It's really a preview.

MR. ROLFES: Let me see if I can --

CHAIRMAN CLAWSON: One of the things I want to bring up, and this comes from the workers comments and so forth.

You're correct, in the standpoint of that because it was a liquid base, it was a little bit cleaner.

But where a lot of the problems

came in, was the leaks, leaks from the process system, them having to be cleaned up, so forth, like that, replacing a lot of the piping and everything else like that.

That is when they really got into some of the big issues and some of the big problems with it. It may have been cleaner, because of the wet, but the leaks, the issues -- and we heard this from Fernald workers, when we were out here, it wasn't that much, it really wasn't that much cleaner, the process.

The process itself was clean, but the plant really wasn't that much better.

MR. ROLFES: I'm looking at the air concentration data and the air counts -- I'm just reporting the data they gave to me.

CHAIRMAN CLAWSON: All right.

MR. ROLFES: I'm just relaying the relative concentrations of alpha emitters in the air, in this plant versus other plants.

CHAIRMAN CLAWSON: Right.

MR. ROLFES: So, it's not

subjective. It's based upon what is said here.

I did pull up a White Paper on Fernald for thorium-230 and other associated radionuclides Rev. 7, the date is, let's see, back in November of last year, is when we would have provided it to -- White Paper, Fernald thorium-230 and other associated radionuclides, Rev. 7. Let me see if I can find an email, if you like, to give you a date -- or I can resend it, if you like.

DR. MAURO: I wasn't aware that you actually answered the question already. Is the question answered, or is -- was there more to come?

MR. ROLFES: Well, one of the intents, also, of the consolidated report that we're going to send out, this was going to a portion of that consolidated report, as well.

So, you know, if we have -- we can send this out in advance of the consolidated report, if you don't have it already, or we can, you know, once again --

MEMBER GRIFFON: But I guess what John is asking is, is the position the same or is it --

DR. MAURO: You see, my recollection of thorium-230 was the way we were going to deal with it is assume it's in equilibrium with the radium-226, and therefore, once you know the radium, you know the thorium, and we're fine with that.

But you're saying that, no, you agree that there are streams where it was just thorium-230, without the radium, and you're saying this report talks about that, and I have to admit, I didn't know that.

MR. ROLFES: The Silo 3 material was a different material, different -- if you -- and that's what we've spoken about. Let me see if I can -- the basic topic of this paper was to address the concern of elevated thorium-230 concentrations, in the raffinates, in Silos 1, 2 and 3.

We addressed, let's see, the radium dose from Silos 1 and 2 in a separate White Paper. So, this is -- essentially, should be the answer that you're looking for.

DR. MAURO: Then we should have seen it. My apologies, I wasn't aware that there was a White Paper, specifically on this.

DR. CHEW: Mark, I think I just sent it to you.

MR. ROLFES: Okay, all right. Let me see here, did you send my email that I sent out?

DR. CHEW: Yes, I sent it to your email account.

MEMBER GRIFFON: Well, I guess the action shifts to SC&A reviewing that report.

DR. MAURO: Yes, I forgot to say --

MR. ROLFES: Here we go, thank you, Mel. I apologize, John, to cut you off, there.

I've got the Fernald thorium-230 Rev. 7 and it looks like I have put some comments in there, possibly, and it would have been dated January 6, 2010. Let me see, I can look for an email to see when I had sent it out, if you like.

CHAIRMAN CLAWSON: January 2010?

MR. ROLFES: January 2010.

MR. ROLFES: It would have been 2010. So, this year.

MR. KATZ: You don't have to hunt that down right now.

MR. ROLFES: I'll make sure that I sent it, and if I didn't, I can resend it.

MEMBER GRIFFON: That's fine.

CHAIRMAN CLAWSON: Okay.

MEMBER GRIFFON: Moving on.

CHAIRMAN CLAWSON: So, SC&A's action item on that is to review that. I thought we had already reviewed that. I thought there was some issues with that.

But anyway, let's go onto the K-65 silo, the radon emissions from K-65 silo.

DR. MAURO: I'll get started. This

has been a problem we've had for quite some time, and I did read the latest report that came into me on Thursday, and we have not made any progress, okay. We really haven't addressed our concerns.

I've reiterated, if you like the -- this Piney or Pinney report, and I have a lot of reasons why I don't. I think Hans in on the line -- he probably has a lot of reasons.

You really have never answered the question, what happened? Why is there a disequilibrium between the radium and the polonium and lead, in the samples, and the idea is, the fact that there is this disequilibrium, it means, where did the radon go, and you can't ignore that.

All of the other arguments that you folks make, using atmospheric dispersion, using -- this transport, diffusion calculations, all these are, are second and third order methods at getting at a problem, at -- that when you have primary information on this disequilibrium, along with these external gamma measurements that were taken before and after the head was vented, and what that meant, we feel that you folks keep side-stepping that and not confronting it head on, because to me, you say, "I have a lot of different ways I can skin this cat," one of which is to go to these Pinney data, which is some -- I don't know how far away this was, or you come up with some diffusion model, transport model, you resort to those as like, your last resort, and it's so far removed from the problem and there's so much uncertainty, the list -- I could go on forever. So, I'm not going to go on with that.

Why don't you folks confront that fact that there is this disequilibrium, where did that radon go, and we're coming in -- and even modest estimates show that you had a release, at least not 6,000, but 60,000 curies of radon and you have not -- you guys never said, "Listen, this is why you're wrong John, or Hans, this is why you are wrong."

This equilibrium does not prove -- and you know, I don't -- we all heard that, and this gamma reading, you know, before and after the vented the head space, if those two facts together, to me, are so sound, and you really have not said why that's wrong.

You've got to tell us why that's wrong, and I'll tell you, I haven't heard that answer and it's not in the report. So, Hans -- I probably stole some of your thunder, but I know you and I spoke about this on many occasions, but did I do justice to this thing?

DR. BEHLING: Yes, except I think there's been so much history, that it really needs to be some extent, summarized, to bring everyone back into the same arena, as to what we, or what I proposed in the two separate reports, and that's -- as far as I'm concerned, shows such strong evidence that the disequilibrium exists, and I think, if I may, I would just like to verify a few things that were part of the history behind this whole issue, and summarize it and sort of systematically -- especially since we have a new person, and I'm going to ask the question, is Dr. Field in the -- at the conference table?

CHAIRMAN CLAWSON: No, he is not.

DR. BEHLING: Well, that's unfortunate, because I was hoping that as a radon specialist, he might actually take this particular issue on and review the historical data that involved the two White Papers that I submitted and perhaps, as a Member of the Board, and perhaps, as an adjunct Member of the Working Group, could assist in this effort.

MR. KATZ: Hans, I would be happy to invite him, but no one gave me a clue that that was of interest. I'd be happy to invite him, to look at any material then.

DR. BEHLING: Yes, as a matter of

fact, in the 2008 Richard Hornung and Susan Pinney report, he's actually referenced, because he apparently had conducted a study of radon induced lung cancer in that immediate vicinity, and as a radon specialist, I would assume that he will have a very, very educated background in understanding the two White Papers I wrote and perhaps, he can provide the Working Group with an independent assessment of what the information that we have provided to NIOSH and the merits of that information, versus what the response of NIOSH has been.

But I would like, if I have a few minutes here, to go over the history of what these data that I presented in two White Papers really amount to, and as already, John alluded to, we are talking about first order empirical measurements. We're not talking about a model that you can, in a subjective way, introduce various parameters and make the data fit to whatever you want to.

These empirical measurements --

and if I have a -- if I'm given a few minutes here, I would like to go back over the data that I initially introduced in the first White Paper, then the issues that prompted the second White Paper, and why I still believe that all of the original data -- and as a matter of fact, in the second White Paper, I also introduced some additional paper, regarding the original 1995 RAC report and the 1998 RAC report, that was not available in the original report, and was prompted by the intermediate discussions that have been held over the period of the last three years.

MR. KATZ: Carry on, Hans.

DR. BEHLING: Okay, let me start out. This whole issue of the K-65 Silo started when I reviewed the TBD for Fernald, and also, the SEC petition for Fernald, and that review of the Site Profile for Fernald occurred back in 2007, and one of the findings I identified there, identified this issue of the radon emissions that were based on a 1995 RAC model, which had assumed that somewhere between 5,000 and 6,000 curies of radon were released.

And I looked at that and I looked at the actual data that also were contained in the TBD, and specifically, I addressed the issue of disequilibrium, and we have 1991 sampling data, that were samples taken out of Silos 1 and 2, that show a disequilibrium between lead-210 and radon -- radium-226.

In the 1991 sampling data, this disequilibrium showed a 37 percent equilibrium ratio between lead-210 and radium-226. For the second silo, the ratio was 38 percent.

The following -- there was also a 1993 data set which showed slightly higher ratio, but this disequilibrium is real, and no one would deny it. In fact, when we discussed this right about the time that we reviewed the TBD and the SEC, it was everyone's agreement that this disequilibrium was, in fact, an empirical fact that should not even be discussed -- disputed, and I think we can go back on the records, to verify that NIOSH agreed with this.

However, NIOSH basically then defaulted to the issue that said, okay, we agree with the decision -- with the disequilibrium, but never the less, we believe in the RAC model and the reason we believe that the RAC model of 5,000 to 6,000 curies, prior to June 1997, is correct, is the fact that the radon accumulated in the head space and decayed in the head space, and therefore, was never vented out.

Now, I looked back and I looked at the 1995 RAC report, and in the Appendix J, you have data there, and the strongest point, as John already mentioned, and summarized it, briefly, there were measurements taken in -- by the RAC Committee, which is so surprising to me, because in effect, they have the data that they should have used in assessing what the radon releases were, and I included that as part of my Exhibit-1 in the original White

Paper that I had submitted, and that was back in November 2008.

For those who may have access to it, it is Exhibit-1, and the key issue here is the following empirical measurements.

They had, obviously, made major, major renovations to the dome in June 1997 -- 1979, and that included removing of a big vent that was a gooseneck, 15 centimeter diameter gooseneck, and huge, huge numbers of cracks and fissions that obviously vented whatever was in the head space, into the environment.

And one of the things that they realized, if they tried to put workers up there, they would be exposed to very, very large doses, or dose rates, to gamma radiation and they intended to fix that, in addition to obviously, avoiding the venting of radon.

And one of the things that I looked at, and you will see in Exhibit-1 of the original White Paper of 2008, were dose rate measurements, and I want to mention --

point out to you, these dose rate measurements, among the ones that I highlighted, were taken in May 1973, so, we're talking about six years prior to the actual sealing of the dome. In other words, the remediation effect that really made a radical change in the release rates of the head space air.

And the dose rates, in May 19 -- and I'm reading right now, from my Exhibit-1, the dose rates in May 1973 were about 70 to 75 millirem per hour.

Then in 1979, in June -- or let's go jump just a few months ahead, in April 1980, this follows the major sealing of the silo openings. We now look at dose rates that are in the orders of about 200 to 250 millirem. In other words, we jumped dose rate by a factor of three, as a result of the remediation effect on the top of the silos.

DR. MAURO: Hans, I'm going to --

DR. BEHLING: In other words, some

how or other --

DR. MAURO: Is there -- this is so important, it's got to be written down, because there's a lot of numbers here, and I --

DR. BEHLING: Yes, and well, it's -- I also included that same exhibit, as Exhibit-5 in the second White Paper, which I submitted to NIOSH and the Board, April 2010, and the same exhibit is shown in those papers.

DR. MAURO: Yes, I'm just trying to help the folks around the table, because not everyone has that.

So, you're saying, in 1973 -- you gave me a date and you gave me a 70 millirem per hour number. Give me that date and the dose, again.

DR. BEHLING: Okay, the date was -- and there is numerous ones.

DR. MAURO: Okay.

DR. BEHLING: And these were contact measurements on top of the dome and the average doses -- dose rates then, at the time in May 1973, were about 65 to 75 millirem per hour, okay?

DR. MAURO: Okay, good, I wrote that down on a blackboard, Hans, because it's going to be an important number.

DR. BEHLING: Okay, and then after the sealing of the silo openings, there were a whole series of measurements, and this is in April 1980, the dose rates went to 200 to 250.

So, approximately three-fold plus a higher dose rate measurement, and that obviously was the result of the hold up time of radon that was no longer being vented, okay.

So, we have, after 1980, a three-fold increase in the dose rate measurement, on top of the silos, so, it was --

DR. MAURO: So, Hans, I'm sorry to interrupt, because I want to make sure it's locked in.

So, the unsealed -- the dome, when

it was unsealed, it had cracks, it --

DR. BEHLING: It was about 70, and then it went to 200 to 250 --

DR. MAURO: Right, so before -- but it was unsealed, it was cracked, it had goosenecks, back in 1973, the dose rate --

DR. BEHLING: Yes.

DR. MAURO: Was 65 to 75, but after they corked it up, sealed the top, the dose rate --

DR. BEHLING: And that was in June 1979.

DR. MAURO: Okay.

DR. BEHLING: Okay, and so, we increased the dose rates on top of the dome, by a factor of three, that were clearly, the result of the effort to seal up the dome cap.

Now, let's go back, and on that same Appendix J of the RAC report, and this is taken from the 1995 RAC report, we have another measurement in November 1987, okay, that again, it verifies the 1987, the dose rate was 221 to 250 millirem, with an average of 230 millirem per hour, a gamma.

So, this higher dose rate was consistently demonstrated for years after the sealing of the dome, which occurred in June 1979.

Now, again, because of the accumulation of high dose rates, they decided to introduce what was called a radon treatment system, RTS, and in my Exhibit-1 in the original report, and again in Exhibit-5, the same table shows that in November 1987, after the radiation treatment system -- radon treatment system was engaged -- let me just briefly -- make everyone understand what that system does.

This radon treatment system was basically, nothing more than a ventilation system, which removed air at the 1,000 liters per minute from the head space, and it was run for a full three hours, until the dose rate no longer dropped.

And it is assumed that 97 percent plus of the radon that was now -- that had been in the head space, had been thoroughly removed, and guess what? The radon treatment system that reduced the dose rates to an average of 68 millirem per hour, and that coincides with the exact dose rate that you get, prior to 1978 -- 1979 time frame, when the dome was sealed.

So, the radon treatment which essentially avoided and -- up to 97 percent plus of the air space in the head space, reduced the dose rate to the exact same number that you observed to prior to June 1979.

Now, what that tells me is that the accumulation of radon in the head space, prior to the sealing, was about as -- the same as it was in the activation of the radon treatment system, meaning that all of the radon was basically vented from the head space.

That's the only conclusion you can

come to, which means that the disequilibrium that you observed in the waste package was the result of all of the radon that was basically removed from the waste package, but not held up in the head space, but released from the head space into the environment.

And that, to me, speaks volumes. These are empirical measurements. They were part of the RAC 1995 report, and to this date, I cannot understand how they, themselves, did not come to look at this data, and say, "We have the answer," instead of modeling their 5,000 to 6,000 curies per year, based on a model that relied on the thermal expansion -- diurnal thermal expansion of the air in the head space, in saying that if you pressurize the head space, then you will obviously expel some of that hot air that's build up in the head space, as a result of daily diurnal heating effect, and when I looked at the report again, and this is something that was introduced new in my report, the people from

RAC fully recognized that the diurnal thermal ventilation was really only part of the means by which radon could have gone out, and what it -- one of the things that I wanted to bring out, and I hope people had a chance to read it, was the issue of the Venturi effect, which I made reference to, early on, and that is the Venturi effect that's created by wind, that flows over top of these curved domes, and these curved domes, in effect, are very similar to what an airplane wing has.

That is, when an air flow occurs over a curved surface, like an airplane wing, it creates a partial vacuum, and that partial vacuum, in an airplane, gives the airplane its lift.

So, when you see a jumbo jet flying in the air, the lift of that airplane is due to the curvature of the wing, that is obviously creating a partial vacuum above the wing, and provides a lift.

Well, in this dome, as far as I

can see, this is what happens. When you have a prevailing wind that flows over top of the dome, what you create is a partial vacuum of pressure differential between the pressure outside the dome cap versus the inside, and the outside flow of air actually provides a suction of whatever you may have had in the head space, and prior to 1979, that suction basically evacuated the radon, as it built up.

In fact, it actually drew the radon out of the waste passage, as you would have in a house.

The reason you have radon in a basement, if you have an underlying soil problem with radium, you actually draw radon into the house. It's not through natural diffusion. That is a minor, minor process by which radon enters the house. It is in a house, when you have a radon problem in the house, it's due to the operation of the house, meaning that if you have a wood burning stove or if you have bathroom vents -- ventilators that are running, that small pressure differential between the outside and the inside of the house, it's a driving force of bringing radon into the living space, and this is what you have there.

And one of the things that I quoted in my report, the most recent report, White Paper that I issued in April 2010, was the very issue that was neglected, and I went back through the Appendix J of the RAC report, and they clearly state that, and I quote right now, because I'm on page 16 of my report, I report -- or I quote directly from the RAC 1995 report, and this is verbatim.

It states, in pages J31 through J32, for those who may have a copy of the 1995 RAC report, and it states the following, "For the present workers, it is assumed that the silo ventilation is the sum of the ventilation rate due to temperature effect," that I just explained, "and a ventilation rate, due to wind effect," and it gave the equation of lambda V equal to lambda V external and lambda V of the wind.

Then they go onto say, "As discussed earlier, it is possible that the cracks in the silo domes are numerous enough and large enough that they action of winds on the domes could create additional ventilation in the silos, represented by lambda V wind."

"However," and this is important, now, "no data have been found to substantiate an estimate of lambda V wind, and since additional information has not been located to substantiate a value for lambda V wind, we now assume a value of zero."

In other words, what the RAC people did, they realized that winds would probably play a very, very important role, but because they didn't have a quantitative value for lambda wind, the ventilation rate, due the wind effect, they simply said, it doesn't exist.

And my estimation, based on the

empirical data, namely, the disequilibrium, only 30 -- about less than 40 percent of the lead-210 versus the radium-226, existed in the waste package, that was verified on two separate sampling counts, and the dose rate measurements taken on top of the silos, before June 1979, and after 1979, show that there was an increase, a three-fold, more than three-fold increase in the dose rate, which when you introduce the radon treatment system, is reduced to pre-1979 levels, that again, shows that the radon treatment system which vented the air -- the head space by -- at a rate of 1,000 liters per minute, had the same effect as the cracks and the gooseneck, prior to 1979, meaning that there was no build up of radon in the head space, and to me, as far as I'm concerned, that, in itself, should answer the question, as to what went out, and I calculated what the source-term of radium was, and on the basis of the actual production of radon, I concluded that the actual ventilation of radon during the period prior to 1979, when the dome was, obviously, mediated, would have been somewhere around 60,000 to 90,000 curies per year.

And I can go back and talk about the additional things that I was asked to -- when I introduced -- well, presented this information earlier, it was dismissed in saying, "Well, no, no, we believe in the RAC model," and that was blessed by the National Academy of Science. That was one of the first issues that was NIOSH's response.

Of course, I looked at the RAC model and the evaluation by the National Academy of Science, and they said, "No, we don't agree with it either," and I think they came to the conclusion that the National Academy of Science did not bless the RAC model.

Then, there was the issue of, "Oh, no, we never really believed that the RAC model was really the holy grail. We believe that the Pinney model is really one that validated the RAC model," and I went through that, and I don't know if you want to get into that, but I looked at the Hornung-Pinney report that had a date of 2008, and I don't believe that they validated anything, other than the dispersion model, which is quite different from the source-term of radon releases.

What they validated was the Chi over Q value and it has very little to do with the actual source-term.

Then, at one of the previous other meetings, and it was Brad, who said, "We will ask NIOSH -- SC&A to respond, with two other sets of information," one of which was the radon measurements take with canisters on top of the dome, and I think I addressed that in my second White Paper, and the conclusion there was, the measurements have very little or no chance of really estimating the actual source-term.

And also, another paper that was written by one of the co-authors of the initial RAC `95 report, as well as the second RAC `98 report, who then came up with a new release value that was a fraction, but it never explained how he came up with those numbers.

So, I think I tried to -- you know, I don't want to continue talking, because I think maybe I'll let the other people comment on this issue, but I don't believe that there is anything that I failed to address in my second report, that was asked of SC&A in response to comments made by Mark Rolfes, regarding these other two sets of information, that would potentially support the lower value as defined by the RAC `95 data.

DR. MAURO: There is one thing I want to say, we just made our case, for why we believe we're right. NIOSH has not addressed why that's wrong.

Now, I'm going to tell you why the Pinney report is not right.

You release radon from the cracks out of that stack, mostly likely, it's radon by itself coming out, none of the particulates, none of the lead-210, none of the polonium-210, all right.

Now, we realize, this radon gas is leaving -- the radon gas is leaving this thing, okay, coming out and it's flowing, okay, and it's dispersed, calcium dispersion, and now, you got some windows over here. I don't know how far away this is, a mile away, half-mile away, whatever, you made --

CHAIRMAN CLAWSON: They were off the plant.

DR. MAURO: They were off site. Now, what happens is -

CHAIRMAN CLAWSON: Well, is that --

MR. ROLFES: Hold on, they were process --

DR. MAURO: Process buildings.

MR. ROLFES: I think it was the refinery, Plant 2/3. So, you know, several hundred feet.

DR. MAURO: Several hundred feet. Now, stay with me, for a minute, all right.

Remember that the daughters have to grow in. In other words, you're going to -- your collecting polonium-210, I guess, and lead-210, right. These are -- I think at least, maybe both, are alpha emitters, and it's these alpha emitters that are sort of sticking to the grass, right, all right, now --

MR. ROLFES: Embedded in it.

DR. MAURO: Embedded in it, they would stick to it, they decayed, the alpha is emitted and it leaves an etch, okay.

But now, I'm telling you, there's not too much lead-210 in the air here, because the tank doesn't -- if radon is leaving -- so, in other words, so you folks came up with a concentration of progeny, based on etching, in the glass, and said, "Okay, based on that concentration," and it's an integrated concentration, you could back calculate what the source time would be.

But the reality is, there isn't any lead-210 and polonium-210 in the gas that's passing by that window, because it's too -- because what's coming out of here is probably mostly radon, without its progeny, and there is not enough time for the progeny to blow in.

So, you're going to get an under-estimate over here, of what the airborne radon is. If you're looking at the progeny, the progeny may eventually -- now, the air handler -- it's going to take towers for the progeny to grow in.

DR. BEHLING: John, can I interrupt? Actually, it takes only that long of time, because you're really looking at the in-growth of lead-210, which has a 22 year half-life.

DR. MAURO: I was giving the answer to polonium, which is shorter. But so, what I'm getting at is, I was trying to -- see, just like we were hoping that you could come at us, and say why we're wrong, I took a look at your stuff and said, "Well," you know, I take the Pinney -- and see, now, why is it that you're coming up with source-terms that are grossly smaller, by a factor of -- now, you're down to about 300, instead -- you went from 6,000 to several hundred, and I think --

Now, why would the Pinney data give you such bad results, you know, when you're using it to back calculate, besides all the problems with meteorology, we're not even going to talk about that, and what it -- you know, because the wind is always blowing in different directions. We know that.

But even more fundamental than that, there is not enough time -- if the -- if this is heavily radon without the -- but see, the particulates aren't going to seep out as easily.

The particulates, they are going to be sort of trapped a little bit more, and there may be some coming out, but if the radon -- you know, gas is coming out and it's going to flowing down and it's going to get to you in a matter of seconds, minutes, I don't know, and in those seconds to minutes, you're not going to grow in the progeny.

So, whatever you see here, is not the progeny and not going to be an equilibrium to radon gas, and I think there, is the fundamental, I guess, problem, with looking at the track etch.

I just thought about this over the weekend. So, you know, but I think that that's it. Why are we having this disagreement, and I think that that is the root cause to the disagreement, and why we think our approach to coming at the problem is a lot more fundamental and -- then using the Pinney data or the diffusion model, that was described.

In fact, the diffusion model came in the report, that we just read, now, I just read it again, and I read it -- now, he wrote a diffusion equation of the movement of radon here, but I think it was diffusion, not invective transport.

MR. STIVER: But the one-dimensional diffusion model --

DR. MAURO: Yes, now, you see, when you -- so, he's saying that oh, the radon is -- moving very, very slowly, it's never going -- now, this is 36 feet, by the way, all right, this thing -- I think from here to here, 36 feet.

All right, now, he's saying -- what Sam was saying, well, listen, you know, if the radon -- once -- well, the radon -- when the radium atom decays, it turns into a radon, a gas, noble gas, it's going to start to move, and if it's only diffusion, it's going to move very, very, very slowly, and his argument is, it's going to decay, and once it decays, it becomes the progeny, it's a particle, it's not going anywhere. It's stuck there.

Well, the point there, if that was true, it would be an equilibrium. You'd have the same amount of progeny as you would radium, but you don't.

Now, so, you say, "Then how is the radium," -- now, wait a minute, then how does the radon that's produced down here, move up 36 feet?

Well, the reality is, yes, you put a delta P across to here, because of this wind, and that radon is going to move, and you -- it's not surprising, now, that you're going to get a substantial amount of the radon that as it's being produced in the -- in this stuff, the raffinates that are in here, as it becomes radon, with that kind of delta P, and don't forget, this -- I don't know the temperature of this thing, but remember, there is a lot of radium here, the thermal temperature, I don't -- that's another thing that's a driver. You know, you cause a chimney effect.

So, what I'm getting at is, I'm not surprised that a lot of the radon, as it's being produced, or as it was being produced in this silo, is moving up and leaving behind the particulates, to a large extent, and the radon is leaving, and all of the sudden, you're fooled, you know, we're fooled by the Pinney data, because we think we're looking -- you know, we're looking at the progeny, which really didn't have a chance to grow in.

It's got a delta P, by far, especially, you've got a goosenecks or cracks in here, that -- there's wind blowing over, the radon is going to -- and everything rings true. The gamma readings, they are -- I mean, there is the final nail in the coffin.

So, I'm telling you, I don't care if the RAC -- I don't care who said it, National Academy of Sciences, you know, I don't -- Hans' answer is the right answer, and the releases of radon from that -- those silos, are not 6,000, it's 60,000.

Now, see after all that is said this is not an SEC issue. We could do it. And I hate to be the one to come and tell you guys how to do these calculations, but that's how you do it, and if you do it that way, the problem goes away.

DR. BEHLING: And let me just add this, this is Hans, again, just to -- on the basis of the strength of the data, we always talk about the hierarchy of data, which has merits in this world.

When we talk about dose reconstruction, we talk about the value of empirical measurements, the primary empirical measurements, and when you have, obviously, disequilibrium, a primary measurement it's not -- it shouldn't be something that NIOSH is even willing to dispute. We have, obviously, disequilibrium.

Now, what happens? The original argument was that, well, the radon does migrate into the head space and then decays, but it's not released.

The truth is the empirical data of dose rate measurements, prior to and after 1979, show clearly, that the hold up of radon in the head space was essentially equivalent prior to 1979, as it was in -- at the time when the radon treatment center was in effect, which is -- it completely vented the head space at a tremendous rate, and obviously, let no accumulation of radon occur.

And so, what you ended up with is about 70 -- 65 to 75 millirem residual dose rate, which does not come from the head space, but that is the radioactivity that is in the waste package, below, which you essentially reduced it to a baseline level.

In other words, understand that 70 millirem per hour is really the radiation dose rate that is affected by the waste package itself, and has nothing to do anymore, with any of the radon build up in the head space, in their short lived daughters, and to me, as I said, these are empirical measurements.

Now, we're trying to compare these strong data measurements to something that is -- and I gave you previously, a parallel. What if you had a reactor facility that had a release point, a stack release point, where you had a very, very accurate understanding of what you're releasing, in terms of curies per unit time?

And I think we have that equivalent value, and then somebody says, "Well, you know, I was downwind with a gamma rate meter, and I measured something," and on the basis of a Chi over Q value and my gamma dose rate measurement that was taken 1,000 feet or so from there, I'm contesting your dose rate measurement that you measured in a calibrated system, at the point of release.

Now, anyone who understands priorities in giving strength to data, would say, "I believe the dose rate measurement taken in the stack, well over any kind of secondary measurements that you may have taken downwind, 1,000 feet," and then had to somehow or other, use that dose rate measurement, 1,000 feet distance by means of a Chi over Q value, in order to end up with a value that you're now contesting as a release quantity.

I can't understand how anyone, in his right mind, would take that data and say, "I'll accept that data more than the stack data," and that's exactly what we have here.

MR. ROLFES: Okay.

MEMBER ZIEMER: Brad, this is Ziemer, could I ask a question here?

CHAIRMAN CLAWSON: Sure.

MEMBER ZIEMER: John, I'm trying to understand fully, what you're asserting here.

Are you -- you're indicating that you believe that all of the particulates played out as the radon is removed from the head space?

DR. MAURO: No, but I'm saying that it's preferentially, when the radon gas is being exhaled, for this chimney effect, or Bernoulli effect, all of the C-

MEMBER ZIEMER: Now, the head space is actually filled with air, to start with, anyway, right?

DR. MAURO: Yes.

MEMBER ZIEMER: So, most of the molecules in the head space are not radon, they are air?

DR. MAURO: Yes, okay.

MEMBER ZIEMER: Is that not correct?

DR. MAURO: Yes, probably. I would have said -- I have to say, yes, yes.

MEMBER ZIEMER: Well, I would think so. I mean, probably, billions of tons more air molecules. Now --

DR. MAURO: All right, we could

figure that out.

MEMBER ZIEMER: But what I'm trying to get a feel for here is, obviously, you don't have instantaneous turnover of the head space. You've got to be removing air at some rate, and some fraction of that is radon.

Has anyone sort of tried to estimate what would be the residence time of a radon atom, that's generated into the head space, the average resident time?

I would think that you might get a fair amount, and this is just intuitive, now, I'm just thinking off the top of my head, you might get a fair amount of daughters, and I'm not sure why those wouldn't be exhausted with the gas.

DR. MAURO: Well, it's coming out of the --

MEMBER ZIEMER: Many of those would be, you know, attached to minute dust particles in the air. They attached to things, obviously. But why wouldn't a lot of them be expelled, as well?

So, those are questions I have in my mind, but it's not clear to me, why we would necessarily say that everything coming out is pure radon gas, or mostly pure. Sort of intuitively, there has to be a residence time on a typical molecule --

DR. BEHLING: Okay, Dr. Ziemer, this is Hans.

MEMBER ZIEMER: Yes.

DR. BEHLING: I can give you the answer, because they did, in fact, based on the 1,000 liters per minute of ventilation rate of the head space, and I have to go look exactly how many cubic liters the head space really represents -- but I do remember, and I can quickly verify it by paging through my write up, the ventilation rate of the head space, with the radon treatment system operational was 1.2 ventilation rates per hour.

So, that's approximately what a

house has, when it is modestly sealed, during the winter months, when you have radon in the --

MEMBER ZIEMER: Right, it's the --

DR. BEHLING: Ventilation rate of about 1.2 per hour.

MEMBER ZIEMER: Yes, in those cases, you get a fair build up --

DR. BEHLING: Yes.

MEMBER ZIEMER: Not necessarily equilibrium, but you certainly get build up of the daughters, and those don't -- a lot of those do, you're quite right, attach to surfaces, but many of them remain within the microscopic dust particles in the air and in a sense, act very much like a gas, in terms of being, you know, following, if there's ventilation, they follow the radon out, as well.

DR. BEHLING: Yes, you know, and I didn't want to question John's interpretation, but my gut feeling is, you probably have an equilibrium value of radon daughters to the ventilation rate of 1.2 per hour, that is possibly the equivalent of what you get in a house, which would assumedly be about 50 percent.

MEMBER ZIEMER: Yes, that was sort of my gut feeling, too. I would think that, you know, unless you're really -- you know, it would be very different in everything in there was radon, but this is mostly air.

DR. BEHLING: No, there is no question, and as I said, my argument is here is really the fact that we have empirical data that I think, has orders of magnitude, higher validity in representing the values that I predicted, as opposed to the CR-39 measurements downwind, which, as I said, I liken to a dose rate measurement from a nuclear reactor, when somebody is standing 1,000 feet --

MEMBER ZIEMER: Yes, I understand that part, I just wanted to make sure I understood what John's argument was, on the exhaust of the radon.

DR. MAURO: Yes, what I was doing, Paul, I was trying to say, you know, what it is about the Pinney data, that may be misleading, and one of them says, well, maybe the progeny was not present along with the radon, at that location, where the glass was, in equilibrium, and I'm saying, what would prevent that from occurring?

And so, one of the things that would prevent that is that, the radon that's breaking surface in the waste package, or the waste, is breaking surface, becomes airborne, okay, then, as you point out, it's going to decay to some -- it has a residence time in the head space, and reach some degree of equilibrium, depending on the air turnover rate, in the head space.

But then that gas has to move out of the head. Now, I'm picturing, you've got these cracks, and so, the air that's leaving the head space, is sort of going through a torturous path, and you're going -- you know, it's going to remove some of the particulates -- well, it's not going to remove the radon.

So, here is another place where you're going to get disequilibrium. Now, all I'm saying is that --

MEMBER ZIEMER: Well, keep in mind, the particulates, at this point, are atoms of other elements. So, we're not talking -- well, you know, we're sort of all just postulating.

DR. MAURO: We are --

MEMBER ZIEMER: I'm just saying that --

DR. MAURO: I'll be the first to admit.

MEMBER ZIEMER: I think there is a fairly good chance that you get a fair amount of daughters out, too, certainly not 100 percent, maybe not even 50 percent, but certainly, not pure radon gas.

DR. MAURO: Okay.

MEMBER SCHOFIELD: But here, I mean, here again, you have to start looking at the wind patterns, wind speed, as to how long that residence time at the head is. I mean, the higher that wind is, and then wind starts swirling around, now, that glass pane there is not being -- that flow is not directly to it, it would be away from it.

DR. MAURO: That's part of the Chi over Q problem, that we didn't even talk about.

MEMBER SCHOFIELD: Yes, but I mean, it's definitely going to have bearing on what you're seeing there.

MEMBER ZIEMER: That's entirely correct, right. And, of course, the dosimetry systems are really integrating systems. They sort of give an average of what the thing is seeing, but Hans' point is probably well made that, you know, why not depend on the actual measurements?

DR. GLOVER: This is Sam Glover. I

have to sign off, because I do have to leave.

I think there was one point that I wanted to ask Mark. The measurement, the stuff that was actually measured by the CR-39, that was restricted to polonium-210, right?

DR. BEHLING: That's correct.

MR. ROLFES: I'd have to check the data. Separate from that set of measurements, there was another set of measurements that were conducted as part of a degree -- a Master's degree thesis, I believe, by University of Cincinnati student, and he had actually done some sampling of the radon concentrations, near the K-65 Silo.

I believe in our most recent response, we've prepared a response, which was sent out with the latest email that I provided to the Working Group Members --

CHAIRMAN CLAWSON: Signed by [Identifying information redacted].

MR. ROLFES: November 2nd, correct. There was two attachments, that [Identifying information redacted] had put together. I believe the -- I don't have the date on the one, but there is two separate attachments, radon diffusion mechanism one, revised, and then also, the RSI response to SC&A second White Paper, submitted 6/15/2010.

Those are both in that email and also in the Advisory Board's document review folder.

So, we can -- you know, if there is something in there that you feel we haven't addressed, we'll certainly, you know, look at, you know, what your comments might be on our White Paper.

DR. BEHLING: Mark, this is Hans. Just in response to Sam's question, reading from the Pinney report, and to answer your question, here is the statement.

It states in the Pinney report, "The CR-39 film records tracks from polonium alpha particles that decayed from lead-210, which is a long lived 22 year half-life decay part of the radon-222 embedded in the glass," and then it goes onto say, "The polonium alpha tracks were selected," and they discriminate against other tracks, based on the energy.

So, to answer your question, it was -- they tried to essentially count only tracks made by the alpha emissions from polonium-210, and I think that was your question Sam.

DR. GLOVER: It is, I just want to make sure, since we were talking equilibrium, which, there's a lot of short --

DR. BEHLING: Yes, exactly, that's exactly the noise they tried to eliminate, but by using the energy alpha as a track measure length and discriminated against other shorter lived radionuclides that also are alpha emitters in this decay chain.

DR. GLOVER: Well, I appreciate the opportunity to have a quick question there. I do have to sign off, so, I hope you guys will have a constructive discussion. Talk to you guys all later.

MR. KATZ: Thank you, Sam.

MR. ROLFES: The one other thing that I wanted to mention is really, the target organ of concern for radon exposures is the respiratory tract, and if you take a look at the -- in the dose reconstruction process, you know, I don't want to throw out a specific number, but it's greater than 90 percent of the respiratory tract cancers at Fernald, are already compensated.

So, you know, we're not going to compensate them more, based upon more radon exposure. It's, you know, sort of something that, we're adding dose, but it's not going to benefit anyone. I mean, that's --

MEMBER GRIFFON: It might benefit those other 10 percent, but anyway --

MR. ROLFES: Well, I can get the specific numbers.

MEMBER GRIFFON: I mean, we've been down that path, too, that's --

MR. ROLFES: Most of the people that haven't received compensation are usually -- don't have, you know, the sufficient amount of latency between their exposure and their diagnosis of cancer.

MEMBER GRIFFON: Yes, but we've been down this path before, that it's an exposure cohort, and not a disease cohort, so --

MR. ROLFES: It's a very small number of people that would be affected.

MR. BEATTY: Brad, if I could comment, please?

CHAIRMAN CLAWSON: Okay.

MEMBER GRIFFON: Can I say one thing, on this -- on just John's comment, on the radon?

I followed you, with everything, until your last statement, which was that the good part is, this isn't an SEC issue and --

DR. MAURO: I don't think so.

MEMBER GRIFFON: And the only thing

I question is, how do you assign dose, from this? You know, you could -- you're working on the source-term definition, but who? It's always the `who' question, right?

DR. MAURO: Well, all I could say is that, I think you could come up with source-term that could be bounding, using Hans' strategy.

MEMBER GRIFFON: Right.

DR. MAURO: Next, you have to say, okay, when you've got an atmospheric -- there's an atmospheric dispersion model, which is your classic health physics atmosphere, but --

MEMBER GRIFFON: So, you just assign to the whole set population?

DR. MAURO: Well, yes, we're saying that, okay, if there are workers, you know C-the whole site, anyone down-wind, don't forget, the wind is always shifting, you know, so, what you could do is, you could say, all right, what is the exposures that workers might have experienced?

Now, I'm -- I mean, if were doing it, I would say, okay, well, you got workers that, you know, where they worked, workers all over the place.

MEMBER GRIFFON: Right.

DR. MAURO: And you could come up Chi over Q values, any way you want, and you could decide what you want to do about that. You could use high end Chi over Q or accepted average, I mean, you pick the atmospheric dispersion method that best suits the problem you're trying to solve.

But I think, just like we'd do it at a nuclear power plant, we always are calculating doses to nearby residents, from releases that are occurring, whether they're episodic or they're chronic.

So, I mean, the technique for evaluating doses from airborne emissions is well established and I think there's a way to deal with it. You could skin this cat.

Once you've got the source -- once you have the source-term, and I think once you have the source term, you're probably in pretty good shape.

MEMBER GRIFFON: Then there is this other source-term, also, right, the other silo or the --

DR. MAURO: Three?

MEMBER GRIFFON: Or the Q?

DR. MAURO: I don't know about that.

MR. ROLFES: But to explain a little bit more on what John has said -- this is Mark Rolfes, the Pinney report has actually done that.

They had, over several years, gone back and interviewed workers at the Fernald site, and I believe, had interviewed 2,000 or 3,000 different workers, regarding job practices, areas of the plant that they worked in, what shift they worked on, et cetera, their distance from the K-65 silos, looked at wind direction and speed, inversions, adiabatic inversions in the atmosphere, basically, in these seasonal variations and many, many different factors.

But they actually placed each work in an area of the plant, based upon the interview that they had with each of those employees, and came up with an individualized working level month exposure for that employee.

We have agreed, since the Evaluation Report was presented, to use that data, and I also wanted to make sure that everyone is aware, we now use that in our dose reconstruction process.

So, we have an individual exposure estimate, based upon the Pinney model residing as another personal exposure information. This is within our NIOSH claims tracking system, so, our dose reconstructor would see that, and if the individual has a lung cancer, for example, and needs to consider the radon exposures, they would be able to go into that person's other exposure information and pull that up and get the working level months that that individual exposed to for each year of employment at the Fernald site.

MEMBER GRIFFON: I'm generally familiar with this study, but how many people did -- how many workers did they interview?

MR. ROLFES: I think it was 2,000 or 3,000 people that -- you know, this has been five years since I looked at this, since I looked at the study. This was five years ago, that I had looked at the study, in preparation for the --

MEMBER GRIFFON: But that was my real question, was the movement of the workers around the site, not so much -- you know, you couldn't established quadrants, but how do you establish the path of the workers through those quadrants, and that might answer it.

CHAIRMAN CLAWSON: My understanding, K-65 wasn't one of the worst players, too, though.

MR. ROLFES: Right, the earlier concern was the open silos for the radium bearing ores in process, the Q-11 silos that were outside of the plant, that were processing that.

It was the feed material that -- it was basically, an open -- it didn't have a dome on top of it, like the K-65 materials. It basically then, vented to the air, and it was really the materials from the Q-11 silos that were responsible for the higher air concentrations of radon, on the site in the -- I believe it was the 1953 to 1958 time period.

So, those were where the higher radon exposures were incurred by employees, certainly, much higher than the K-65 silo area.

CHAIRMAN CLAWSON: K-65 becomes the big issue, because it was stored the for so long, if I'm not mistaken.

MR. ROLFES: Right, it was stored

there, subsequently.

CHAIRMAN CLAWSON: But also too, when they took and they sealed up the K-65 silo, that was not really to seal it off, it was structural -- the top of it was ready to cave in.

My understanding is that when they sealed it, it was to do the structural integrity of the K-65, which then increased the dose on top of it, when they realized, we've got to be able to go out and make a release on this.

I thought that they had put the ventilation system in, after they had sealed it. The reports I was reading, that was the gist that I got from it.

MR. ROLFES: The radon treatments -- I'm not exactly --

CHAIRMAN CLAWSON: The radon treatment system, as you call it, I thought that was a later --

MR. ROLFES: Correct.

CHAIRMAN CLAWSON: Later addition, because they had sealed off K-65 silo, figured, this is all going to be wonderful, and then ended up with 250mR up on the top of it, and then all of the sudden, they realized, we've got an issue, we need to -- now, we need to be able to vent this off.

DR. BEHLING: Yes, this is Hans Behling, Brad. The radon treatment system was installed in 1987.

CHAIRMAN CLAWSON: Okay.

MR. ROLFES: Yes, like you said, when they had sealed it around 1980, one of the concerns was, because of the degradation of the thickness of the dome, and I think it went from about six inches on the outside perimeter of the dome, down to about four inches in the middle, and I think the concrete was beginning to spall off of the center -- basically, the top of the dome, and they were concerned about the structural integrity of it. I think it had gotten down to a couple of inches.

So, you know, you've got half the concrete there, essentially, which also could be one of the reasons that would account for the higher dose rate, because of the less shielding from the concrete.

So, there is many other factors that you would have to look at, and we've tried to, you know, speak to those factors in our responses.

CHAIRMAN CLAWSON: Need a lot more -- two inches of concrete, to reduce that.

MR. ROLFES: Well, a factor of -- I mean, you're talking about, you know, 75 to 250, and if you've got different detection equipment over the time periods, I mean, it's not too unusual.

I mean, two inches of concrete, you know, versus whether the detector was placed in the exact same location on the dome.

You know, we've tried to address some of these concerns in our most recent --

CHAIRMAN CLAWSON: I understand that. Ray had something he wanted to --

MR. BEATTY: You touched on it already, Brad. This is Ray Beatty. The Q-11, I wanted to really throw some emphasis there, because of the -- we're talking like a quarter-mile away from K-65, 1, 2, 3 and 4, like Plant 2/3 would probably have been the closest production facility, or a maintenance building we use, 3045, and that CR-39 study, also revealed that that higher level of radon, around the 2/3 area, because of the Q-11s, well, the majority of the workforce was right there, at ground-zero. It wasn't a quarter-mile away.

I think it's rather significant, if you do try to established a -- what you said, a bound -- yes, to do dose reconstruction, that really needs to be taken into consideration, and those interviews you were talking about, that you asked about, Mark, that is being done through the Fernald

Medical Monitoring Program.

The people do an annual physical, they're actually using that data from their physicals, and they interview them about job classifications and time worked, and location. So, I just think it's the significant.

MEMBER GRIFFON: No, that's the -- yes, I mean, that's -- I think, if you can, like John, the source-terms, if we can come to agreement on that, and it's really kind of Site Profile issue.

My question was not so much establishing the exposures over the site, it was placing the people in those quadrants, you know, if you will, so -- and I think the questionnaires might answer that question. I didn't know if they had done -- I didn't know it was that extensive. I knew they had done some interviews, but the --

MR. BEATTY: Pardon me, and while we're on that issue, I'd like to ask if NIOSH has considered the Pinney study, especially the Q-11 information, into the Site Profile.

I think you mentioned something about the dose reconstructionists do recognize that?

MR. ROLFES: Correct, yes.

MR. BEATTY: But you're saying if it's pulmonary or respiratory system related -- there is evidence now from the Pinney report, of radon actually causing -- having a skin cancer association, as well.

MR. ROLFES: Okay, that's something that's different, because you know, what we are doing in dose reconstruction, that's something that, you know, can be looked into in a generic sense.

But the alpha particles for -- I'm trying to remember off the top of my head, I think the most energetic alpha particle would be around seven-and-a-half MeV.

From what I recall, I think it takes about MeV -- eight MeV of alpha energy to penetrate the dead layer of skin, to become a concern for dose to the living layer of your skin.

You know, it would certainly be, you know, much, much less of a concern for a skin cancer, than inhaling radon and getting a risk of lung cancer, from inhaling radon.

It's not something I've looked into very closely, but the major, you know, thing of importance for skin cancers at Fernald would be, you know, direct handling of uranium and the progeny, which goes in there, and if a person, you know, had concern about radon contamination and there was a significant -- well, anyway, it's typically not something that's considered as a concern for skin cancer. It's very low risk.

MS. BALDRIDGE: Could I make a comment?

CHAIRMAN CLAWSON: sure.

MS. BALDRIDGE: You know, I've been listening to this, you know, what data can be used and how it can be interpreted, and it kind of goes back to where I filed this petition, and I filed it based on the TBD on Site Profile, especially as it regarded thorium processing.

But, you know, when it said, and I notice the situation has changed, I provided documents concerning thorium exposure, you located them, or NIOSH located them.

But the point is, there were a lot of dose reconstructions done, based on what you considered to be the best available scientific information in reconstructing data, and that's kind of what some of this discussion has been here, about how to interpret, how to do this and that.

But those dose reconstructions that were done under the flawed Site Profile are locked up, I mean, you can't get the Department of Labor to do anything about them, until the Site Profile is revised, and you have had that information for five years, concerning some of that exposure and those exposures have never been applied to the claims of the people involved.

Now, that's kind of frustrating, you know. So, I don't know how many people that involves, but you know, even with this process, I went online the other day, and I couldn't believe there are 55 SECs, and probably 40 or more of those were filed after this petition was filed, and in my claim, that is not timely.

You're talking about priorities, it has been over nine months between meetings of this Working Group, and I know, seeing how busy you've been, not just, you know, the Working Group for Fernald, in general, with that many petitions being submitted and how many are still under consideration, why you are swamped.

But it seems to me that there is an obligation to deal with those that have been on the Board, been on the table for as long as this one has, with a little more diligence.

I mean, there are petitions, I don't know how many people they involved, or whatever, that were filed in March and April of this year, that are already SEC.

I know this is a big one, covers almost 40 years, and a lot of people and a lot of processes, and it can't just be categorized by uranium processing or thorium processing, or, you know, all these other things that came in.

But I think it's time that something be done. It's not fair to the people who are dying.

CHAIRMAN CLAWSON: Thank you.

MR. KATZ: I mean, I think it is important to sort of press this to a close, in the reasonably near future, both. As well, to get those dose reconstructions that might have been done under previous methods, re-done under methods that -- perhaps, have sort of the benefit that all of the scientists have been discussed here. I think that's important, in both points.

MS. BALDRIDGE: The response is always, "We cannot do anything, until the Site Profile is revised."

You asked -- I asked Larry, when he was still here, and Stu, too, and the answer I get is, "It's NIOSH policy, that we don't address this until the SEC is dealt with."

MR. KATZ: I think when we recently had this effort of sort of, coordinating, trying to coordinate priorities between the Board and NIOSH, to get certain work done, sort of ahead of other work, so, that certain Work Groups can complete work, and this is sort of a nice example, I think, of a Work Group that's been toiling time, and it would be good to bring this work to a close.

So, I think when we talk -- whenever Brad gets to the point, where we're talking about scheduling and so on, as to when we'll get deliverables, to be able to close issues that are still open and then, meet again. You know, we have another Board meeting, for example, in February, I think, and we need to keep this in mind.

CHAIRMAN CLAWSON: And I was going to bring that up at the end, but I'm glad that you did, right now.

Let's look for an action item on this. I know that SC&A has made two attempts at this, and they really haven't changed that much, but NIOSH has responded with this radon emission, John, I guess what we'll need from you is another -- is another paper back, explaining the --

MEMBER GRIFFON: What I would like from SC&A is a position, sort of like what you just discussed, with John, that there might be differences in our acceptance of the source-terms, however, here is our position on the ability to bound, and considering the Pinney -- the approached used in the Pinney data or whatever, I want to see SC&A's assessment of that.

And then, if it just comes down to differences in the source-term, we can move it off the SEC, yes.

CHAIRMAN CLAWSON: I understand, I understand.

MEMBER GRIFFON: Because we can go back and put on our source terms --

DR. MAURO: Yes, our position on the source-term, we're not going to talk about that again.

MEMBER GRIFFON: I don't think you should.

DR. MAURO: We're just say, given the source-term -- I'm willing to start that way, given the source-terms, here is --

MEMBER GRIFFON: All right.

DR. MAURO: Now, don't forget, the only thing I'm concerned about is, in effect, I'd be laying out a strategy that I didn't use to reconstruct doses, to workers --

MEMBER GRIFFON: No, no, no, I want you to opine on what is laid out, using the Pinney -- I mean, is it clear, is your approach spelled out?

MR. ROLFES: The Pinney report, all of the --

MEMBER GRIFFON: How are you using the Pinney data to --

DR. MAURO: Oh, okay, I misunderstood you, I'm sorry.

MEMBER GRIFFON: Yes.

DR. MAURO: So, you're basically saying, right now, you have a method for reconstructing doses to workers, outdoors from airborne radon and its progeny, and it's all laid out, not withstanding the Pinney source -- you are using the --

MR. ROLFES: Yes, this is just an output of the Pinney report, and it's all in NOCTS under each individuals claim --

DR. MAURO: So, you're not using the source-term? I mean, did we just go through all of this for nothing? You're not using the 6,000 curies per liter?

MR. ROLFES: Well, the --

MEMBER GRIFFON: No, you're using --

MR. ROLFES: The RAC report is the one established the 5,000 to 6,000 curies per year, effluence from the K-65 silos. That was used by the Pinney -- that was used in the Pinney study, plus, an additional source-term of the Q-11 silos.

DR. MAURO: Oh, okay.

MR. ROLFES: And so, we've got two source terms, and the K-65 silos are only one of them. We have two that we're using for dose reconstructions, and all the data, the output of the exposure model that was produced in the Pinney study is now tagged as working level month exposures in each individual Fernald claimant's exposure information, that is considered by the dose reconstruction.

MEMBER GRIFFON: So, I guess that's

the piece you should focus on. You could say -- we have disagreements on the source term, as discussed in prior papers, but the Pinney approach -- here is our discussion of the Pinney approach versus taking that source term information and assigning doses to individuals, and whether you'd buy-off or not.

MR. ROLFES: To get back to the report that we had discussed before, the thorium-230 Revision 7 report, I wanted to make sure I let everybody know, we did send it out in January 2010, and it's in the A: drive Advisory Board document review under the titles "Fernald Thorium-230 Rev. 7 MR-11510".

CHAIRMAN CLAWSON: John, have you see that one?

MR. KATZ: That's the one we talked about earlier.

CHAIRMAN CLAWSON: Yes.

MR. ROLFES: Okay, I can repeat it for everyone.

It's in the -- under the Advisory

Board document review folder, in the Fernald folder, and the title, it's a Word document, is "Fernald Thorium-230, Rev. 7, MR-11510", dated January 15, 2010.

MEMBER GRIFFON: Can we get -- Brad, I think you have one -- we have one item left, right?

CHAIRMAN CLAWSON: Yes, we do.

MEMBER GRIFFON: Can I ask, can we take a little short break?

CHAIRMAN CLAWSON: Sure, let's take about a 10 minutes.

(Whereupon, the above-entitled matter went off the record at 3:20 p.m. and resumed at 3:45 p.m.)

MR. KATZ: Okay, we're just reconvening after a short break. This is Fernald Work Group, and Brad, why don't you --

CHAIRMAN CLAWSON: Yes, we've got -- Lou Doll is here, who was a construction worker out -- a worker out at Fernald. He was asking us questions on the coworker model of

Mark Rolfes, and I'd just like him to go -- I'd just like him to refer back to what he was discussing and what part of the construction issues are with that -- with the model, possibly. Lou?

MEMBER GRIFFON: I think, for those on the line, he has to leave in a few minutes, so, we want to let him have a chance to speak to this issue, before he has to go.

MR. DOLL: Thank you. I've requested my records from the plant, as far as dose reconstruction -- or dose or security records, or anything, twice -- well, three times, over the past couple of years, just recently, two days ago, and I was down at the new Morgantown record center, a couple of weeks ago, and they gave us a presentation.

I asked a question at that time, Department of Labor was there and also, Department of Energy, about what records were available from the early days up through 1992, and I was told that there are little or none, closer to no records for any of the subcontractors that worked at the site, because the subcontractors, they were responsible for their own records. The Department of Energy and the primes, being National Lead of Ohio Westinghouse, never took care of those records.

We, as construction, were never told what was in the plant. We were definitely treated differently than the in-house workers, because they had a working knowledge of what they were getting into, in the plant. We also had different standards, as far as like, with the work, and we were never monitored. We never went through the urinalysis program. There is no data on us.

So, I guess my question is, as far as the construction workers and doing dose reconstruction, through the EEOICPA process, I'd like to know what is going to be used to do the dose reconstruction, if we have no records?

When we went to work in these plants, we weren't monitored. We weren't given respirators in that area, in 1983, I worked in the pilot plant, putting together and working during process of green salt process, and there is nothing there.

It just concerns me that we're going to be lumped into a diluted process with people throughout the plant, that didn't get near the same dose or exposure that we got, working these processes, which was in all the buildings, doing the demolition and the reconstruction of a lot of these processes on contaminated equipment.

So, I guess that's my concern. I'd like to have an answer on that, on how you're going to treat the construction worker, as far as dose reconstruction is concerned, and if anybody has got a question, I'll try to answer it.

CHAIRMAN CLAWSON: One of the things is, is NIOSH is in the process of sending us a coworker model, or coworker paper, for the construction workers, which has not come right yet, correct, Mark?

MR. ROLFES: That's correct.

CHAIRMAN CLAWSON: And when that -- Lou, when this does come out, after it's been PA cleared and everything else like that, we'll make sure that you get a copy of it, so that you can see what the process is. That would be fine.

MR. DOLL: That would be great. Thank you.

CHAIRMAN CLAWSON: Thank you. Okay, we have -- I just wanted to make sure that on the radon, we've got a clear line on that, before we broke on that, and everybody knows what it -- their issues are.

The next one we've got is issue six, which is the thorium-232 daily weighted exposures, and let's see, were you going to do that?

MR. STIVER: Yes, that's my issue.

I'll go ahead and lead out on that.

For some of these other issues, thorium-232 DWE, daily weighted exposure issue, those got some time, at the -- at January of this year, I had prepared a White Paper review of Revision 2 of the DWE report that Bob Morris had written.

We proposed using the DWE data to reconstruct doses for thorium inhalation, prior to 1968 and when the in vivo lung accounting system came onboard, and in that review, I had identified -- there were 20 findings.

Our main concerns had to do with the accounting for the variability and the uncertainty in the data sets, and the extent to which the data was complete for the sites that were -- that processed thorium, and there were several other findings related to the methodology that had been proposed by NIOSH, in order to reconstruct distributions of DWEs, to assess doses on a facility plant-wide basis.

And recently, I think it was at a Weldon Spring meeting, which I did not attend, this issue came up again, in kind of a global context, and Bob had, at that point, mentioned that they were following the methods of this paper, published in the Health Physics Journal by Davis and Strom, in 2008, and where they essentially use a lot of the techniques that we had recommended in our report, basically, taking a distribution of these air sample concentrations for each task, doing Monte Carlo simulations to generate output distribution of DWE, that you could then use to assign a claimant-favorable percentile of that distribution.

And we had not seen this new revision of the report, this Revision 3, which we got last week, and so, I had prepared, under the direction of the Board, a White Paper, or not really a White Paper, more of a memorandum, outlining the Davis and Strom methodology, the extent to which we felt that NIOSH was following that methodology, and then also, whether the methods that we had proposed in our reviews of NIOSH's methods were in the same spirit as the Davis and Strom memo.

I had -- I believe that went out to the Board last week. I don't know how many of you had a change to really delve into it. It's a 30-page report, and over the weekend, I took a look at this Revision 3. I haven't had a chance to really go through it in detail.

But it appears that a lot of the issues that we had raised in our report have, in deed, been addressed.

If I could back up for a minute. As I mentioned, Davis and Strom looked at five different AWE sites, that had DWE data, and they basically -- and the main difference here that we need to bring up, is that Davis and Strom had the DWE reports that they had, the HASL reports, actually, had the raw data, as an Appendix B, I guess, they included the raw data sheets.

And so, they had the advantage that we don't have at Fernald, in that they were able to look at each one of those reports, see the raw data and go through and analyze it and look at its -- whether it had been transcribed with a degree of certainty and things of that nature, and also, to use that data to generate their distributions.

And so, they basically came up with three distributions. They did the version that HASL had done, historically. They took a Monte Carlo analysis where they used just the actual data that were collected. They randomly sampled that and generated DWE distributions, and then they fit a log-normal distribution to the data sets and ran the simulations that way.

So, we had three different constructs, and it appears that what NIOSH is doing is, they're using the results from the Davis and Strom analysis for the log-normal fits, where they assess the GSD for the sets of those different data, and they came up with a range, I believe it was about three to seven and on average, about five.

And the way the paper is proposing to assess dose for the personnel in the given plant, for a given year, is to take -- for that plant, in combination, the highest DWE for any job in that plant, which is an average, and then assess it, given a -- and they created a log-normal distribution from it with the GSD of five, and then from that, use that to assess the dose for members of that Class, for that year, at that facility.

And while it's not in exact -- they haven't really gone through the same review process that Davis and Strom did, in using the site specific data. The range of GSDs appears to be fairly close to what would be expected.

So, without really looking into that, it appears at least to be a credible methodology for assessing dose to a given Class of workers, or a particular worker, when you have a DWE for that facility in that year.

They also proposed a couple of other things. I guess there was a few situations that we identified in our paper. I think for the pilot plants, there was some missing data, over three years, and there was some missing data for Plant 1, in one year.

And so, we raised this whole issue of how are you going to use surrogate data, in order to account for the doses for the people who were in those plants that had missing data?

Davis and Strom have a section in their paper, where they look at -- at just the air concentration, unweighted air concentration. Basically, every measurement was taken during that plant in that year, and they look at that distribution, and they said, "Well, you know, because an actual worker's exposure is time weighted, then the highest exposure tasks typically occur over a few minutes," and then the kind of ambient exposures occur over long periods of time. You ended up with the DWE, when you weight by task that is considerably lower than just the raw air concentration data.

So, if you look at the raw concentration data, you might have values that are in the tens to, you know, over a hundred-thousand DWE per cubic meter, and those are given the same weight as the ones without regard to the time of exposure.

And so, you end with a distribution that's really skewed up to the high end, and Davis and Strom said, "You know, if you just take the average, the mid point of that unweighted distribution, you basically include all the DWEs, except for just," I think there was only two or three that even exceeded the average, and if you look at the 95th percentile, you basically get them all. I think there was one DWE for one facility that was higher, it was only like five percent higher.

But they have a caveat here, you know, and I'll read that to you, if I can find that paper, here.

Yes, they say that, "Using the distribution of all air samples for a plant without time weighting or assignment to a specific job does not produce a DWE or GSD, which is representative of any individual worker for that site," and they conclude that using the upper 95th percentile of a site-wide air concentration while, will almost always be claimant-favorable, it is unrealistically high for almost everyone.

So, it raises the plausibility issue. You know, you can throw a high number at this, if you don't have the data, but it's not plausible that any person in that building would have ever gotten that exposure.

And so, if you go back to Rev. 3 of the NIOSH report, there is this situation in here. If you look at, I think it's Table 2, they have the DWE data availability by a thorium plant and by year, you see, for the pilot plant, for those three years we identified, 1964 through 1966, data doesn't exist, and then you look at 1967, you've got a value that's 77 MAC, and that's a factor of 10 higher than the next highest value.

And it makes sense, when you look at the advice here, they say, "In instances in which time weighing data are not available," they determined that the 95th percent -- and this is -- according to Davis and Strom, they put -- they determined the 95th percentile will almost always be favorable to the claimant, in making compensation decisions.

They neglected to include the component about it not really being applicable to any individual workers, but they go onto say, in Section 5 here, that, "When time weighing average data aren't available, then the upper 95th percentile of the air sampling data for that facility may be used instead."

In the next paragraph, "When neither DWE data nor air sampling data are available, or when they're judged to be inadequate or incomplete, then DWE at the same facility may be assigned from an adjacent year," and this third bit of guidance makes sense, and if you have adjacent data within a given year, if you're just got one gap one year, you could use whatever is on either side of it, and you know, it's not really surrogate data. It's coming from the same plant, same processes, we're fairly sure.

But this idea that when you don't have data for more than a consecutive year, you just take the entire upper 95^{th} percentile of distribution, we have issues with that, and the plausibility of that really being applicable to any potential worker.

I haven't had time to really go through this in a lot of detail, to look at the data that were published and check the

DWEs in that, but I'd say that this is a marked improvement over Revision 2, but I think you're getting closer to Davis and Strom. I think it's in the spirit of Davis and Strom, which we believe to be a credible method, I might add, as part of our review, but I sent out -- we did compare two methods that we had generated, one that was in my DWE report.

We look at one plant, with a combination, and we essentially, without knowing -- having read this paper, we ended up doing the same thing that they did, with regard to doing the log-normal fits.

We did demonstrate that, here is a way that you can -- here is a methodology that could be used to really get your handle on the job specific DWE, where you know you'd be claimant-favorable to the workers, and we also developed another method that was not quite that way, that -- it was for Mallinckrodt.

Harry Chmelynski had developed a

method where you look at the student's T-distribution for the average, because you're -- if you took it one step farther and said, "You know, we're not just looking at one measurement in time. This is a repetitive task that's going over again and again and again."

So, what he looked at was the uncertainty and the mean, and then proposed a way that that could then be used to get an percentile of the mean, and then that could then be propagated again, as we've mentioned two different techniques, in order to get a job description.

But I guess in summary, I'm fairly happy with the methodology proposed here, there are a couple of things that still need to be resolved.

I think we have issues with the unweighted air concentrations for the entire facility, and another thing that Davis and Strom brought up was the whole issue of human error, they called it blunders in their report, and which is an actually, an ISO term. It's not meant to imply stupidity.

It's just kind of a gross error in methodology, and in the data set that they looked at, which were -- I think there were like, 400-some samples total, through the five -- there were 65 jobs in the five facilities, there is 11 percent -- they said the average blunder resulted in about a two-fold under-estimation of the DWE, and there's about an 11 percent error rate, but the most egregious examples were off by a factor of 10 low, and so, that gives us concern.

One of our recommendations in the report that we presented at last year's meeting, or earlier this year, the January meeting, was that some undertaking of the be initiated to assess the availability of the raw data, and what's out there.

I know some of it's available. I've found it on my O: drive, but we know the extent to which it's complete, and the fact that these early sites have the actual data sheets available, and some of the later ones, Mallinckrodt does, too, they have the raw data.

We think that the data are available. They just weren't put into the reports, and we don't know if they were destroyed or what the fate of that data set might have been.

But I think the due diligence needs to be made here, in order to identify if the data are available and then to generate some kind of a sampling plan, maybe not as rigorous as what went on for HIS-20, but just some sort of data validation exercise to get some sort of a confident estimate of what the error rate might have been and the transcription of what the blunder rate could have been.

MR. ROLFES: You're concerned about the blunders. Does that mean a factor of two versus -- or a factor of ten, not to mention the ones that they had over-estimated?

MR. STIVER: Oh, there was over-estimates, but they weren't the same. We're not concerned about the over-estimates. We're concerned about the under-estimates.

MR. ROLFES: Right, obviously.

MR. STIVER: So, say, if you have the --

MR. ROLFES: Neither are we.

MR. STIVER: Say if you had the high DWE --

MR. ROLFES: But keep in mind, though, I didn't get to finish what I wanted to say, I'm sorry.

The reports, the DWE reports, themselves, there's a couple of important things that were not taken into consideration.

The two big ones are respiratory use. We're taking the daily weighted exposure concentration of the, you know, the materials dispensed into the air. No respiratory reduction factors are applied.

So, that easily can account for, you know, a factor of 10 and a factor of 50 -- I mean, depending upon the type of respirator that's being used, and then the other important thing to keep in mind is that the quantity of material suspended in the air, we're assuming that's 100 percent respirable.

MR. STIVER: Oh, sure.

MR. ROLFES: So, I mean, those are two things that are very conservative --

MR. STIVER: Absolutely.

MR. ROLFES: More than account for that factor of 10.

MR. STIVER: Well, I know that you're applying conservative measures here, but I still think that it would be -- you know, just -- especially in an SEC context, to do some sort of a data validation exercise, just to -- if you could identify the error rate and data sets, it wouldn't have to be across the board.

I can't tell you how to go about doing it. I think it would be important to assess the validity of the underlying data.

MR. ROLFES: I mean, I understand where you're coming from, but I think it would be more important for us to do that, if we were using an actual distribution of the data.

We're not doing that anymore. We're using the highest result in any of the buildings, any of the years --

MR. STIVER: Well, actually, it would be the highest -- for a given year, it would be the highest DWE in that facility --

MR. ROLFES: In that facility, right.

MR. STIVER: But that's the DWE.

MR. ROLFES: Right.

MR. STIVER: So, say if you had the highest DWE and it was off by a factor of 10 and then you throw a factor of five on top of it, you know, what do you come out?

(Simultaneous speaking.)

MR. STIVER: Yes, but what I'm saying is, if you could -- it would be very unlikely, but there is the possibility that the data could be not really necessarily corrupt, but there could be significant errors in that.

But I think you guys are definitely in the right path. I mean, I would certainly try to identify some of the underlying data and look it over, and --

MR. ROLFES: Well, I guess --

MR. STIVER: Come up with some kind of a plan --

MR. ROLFES: Maybe you'll provide a written report to us, then on what your thoughts --

MR. STIVER: Yes, I think that would probably be a legitimate action item. I didn't get a chance to --

CHAIRMAN CLAWSON: I know that you haven't had the opportunity to look at that, to really be able to --

MR. STIVER: Yes, I was all set to go after -- and looked at it, but this is completely different. So, it looks, at face value, you know, at the -- the small amount of time I've been able to devote to it, it looks fairly good, like I said, aside from that one issue, about the unweighted averages and then assessing the blunders, I'm okay with that.

CHAIRMAN CLAWSON: So, I guess the action item would be in the SC&A's -- to be able to give us a response --

MR. STIVER: Give us a response and then --

CHAIRMAN CLAWSON: And give a write up in the response.

MR. STIVER: Yes.

DR. MAURO: Just to point out, this is one -- you know, we talked about a lot of subjects today. This is one area where were really made some substantial progress, and as we all know, the DWE issue, we've been struggling with for quite some time.

The methodology, as described to me, over the weekend, by John, seems like that, you know, this Davis and Strom is a very -- it is the right way to do it, and I understand that the folks -- your folks have fundamentally adopted that philosophy.

It would be, I would say, and abbreviated version of it, almost an -- but like John said, it's -- all I can do is say is that this is good news.

I think that, you know, some minor work dealing with this -- the time periods when you don't have DWE that you could draw upon, to do this upper end, and this is judgment call, of whether or not you want to go with this 95th percentile approach, which -- you know, now, of course, whether that meets the plausibility issue or not, this is in the eye of the beholder, very often, or you elect to go with one of the other strategies that you identified, and this is --

I don't consider this to be a

major barrier. I consider this to be a home stretch issue.

Now, this issue of blunders, given the importance that this issue of blunders was raised, in Davis and Strom, it does seem to be prudent to try to explore that a little bit, and how it might have an bearing on your work, so, you could put that to bed.

It's a legitimate concern that someone could raise and the degree to which you could, you know, address that issue, as best you can, given the limitations you have to access the original data, that would be a good thing to do.

This is not like the areas, where we're like -- where we have some real nuts to crack. This one, I think we're in the home stretch.

MEMBER GRIFFON: I tend to agree with John's opinion, not a written --

DR. MAURO: I'm just -- this is -- right, at the Work Group meeting --

MEMBER GRIFFON: This is where we are on it.

DR. MAURO: Yes, I think that's where SC&A is.

CHAIRMAN CLAWSON: And I understand what you're saying, John, I just want to make sure that we also understand too, that I just don't want to be high numbers thrown at something, be able to get out SEC area. That's my issue.

As I spoke earlier this morning, I talked about a vote, and as Ted and I have spoke, I don't feel that that's in the best of everything. We have too many things to be able to go back over.

I have asked him to put Fernald on the February Advisory Board meeting, to be able to bring it up. We would like to try to be able to set up another Work Group before that time, to be able to iron these issues out, before we bring this to the full Board.

MR. KATZ: Could I just add

something to that, Brad?

CHAIRMAN CLAWSON: Sure.

MR. KATZ: So, I mean, one thing I suggest, I know, Mark, and folks on the phone, from DCAS, I know it's always an issue, as to resources, as to when products can be delivered.

But I guess what I would say on sort of Brad's behalf, in this case, and it's really -- it's not just Brad, I mean, it's really, the Work Group needs to speak to this, because it's ultimately their say, as to pace, as well.

But I think this is probably a high priority for the Board, or at the Work Group, to sort of try to wrap things up, get deliverables on the table for that next Work Group meeting, so that the Work Group can make an attempt to come to some decisions about the SEC portion of what this Work Group is doing with Fernald.

So, I mean, I'm happy to send an

email to Stu, too, but if you carry that water -- I mean, we're aiming to try to bring this to the Board, for the February meeting, which is the third week, which means that getting deliverables in a timely fashion to the Work Group, for whatever we set that date, just would be a high priority, probably a higher priority than some other deliverables, and then I realize there are all these balls in the air at the same time, but I think this is an important one, to try to achieve some closure here.

DR. MAURO: I'd like to remind, there is one are that we had neglected to mention.

When it comes to the thorium issue, you can talk about a DWE, which is the thorium and reconstruction, pre-1969.

MR. ROLFES: Right, I was going to bring that up, earlier.

DR. MAURO: Okay, and you know the --

MR. ROLFES: Yes, the other thing that we're working on at DCAS now is, SC&A provided comments on our thorium-232 mobile in vivo radiation monitoring data coworker model for thorium-232 intakes for the years of 1968 forward, and I think we had put together a White Paper, about three years back.

SC&A has provided some comments to us on that, and we're currently working on attempting to resolve those comments, at this moment.

So, that and the construction worker issue are the two things that we have in our court right now, that we're working on.

DR. MAURO: I'd like to confirm with Ted, as these White Papers become available to the Work Group, does SC&A have the green light to go --

MR. KATZ: Possibly, I mean we want to be prepared at this next Work Group meeting, to make an attempt to wrap of the work of the Work Group, with respect to the

SEC.

You know, whether you accomplish that or not, is another question. But that's what we're shooting for.

DR. MAURO: Good.

CHAIRMAN CLAWSON: Do we want to -- I guess my bottom line is, I'm looking for a time frame that we could basically be setting up a Work Group meeting.

MR. KATZ: Yes, let's look at calendars, if everybody has them available to them.

DR. MAURO: While you're doing that, I would like to also -- I'd like to propose that John Stiver take over as the lead on Fernald, as opposed to me.

MR. KATZ: Happy to have him, sure.

DR. MAURO: As you can tell, he has been a force of nature, on this work and he's been really helping me get through this. So, it will allow me to take care of other matters on the program.

So, John, as long as it's acceptable to the Work Group, I'd very much like to have John take over as the lead. I'll certainly be very much involved, as you know.

MR. KATZ: And you will be present for the next Work Group meeting?

DR. MAURO: And I will be present.

MR. KATZ: Okay, good.

MR. STIVER: To be able to get the presentations of the Board --

MR. KATZ: The big picture. So, if folks have their calendars out, I mean, I would suggest we push this pretty late, although, once we have that Work Group meeting, and part of that Work Group meeting, I think, would be to help the Work Group prepare whatever its recommendations might be to the Board, to not just to decide them, but then to figure out, in a general sense, and then SC&A can help and -- as might be needed with those reports out to the Board.

But the Board meeting is the 23rd,

24^{th} and 25^{th} of February, and let me just see what else is -- I mean, early the week of the 14^{th}, does that work for folks?

It would have to be -- I mean, you'd want it early -- you know, maybe the 14^{th} or the 15^{th}, or going back, the week of the 7^{th} to the 11^{th}.

CHAIRMAN CLAWSON: I'd rather go back to the 7^{th} to the 11^{th}.

MR. KATZ: Yes, how about the 10^{th}, that's a Thursday, of February, does that work for folks?

MR. STIVER: It works for me.

MEMBER GRIFFON: The 10^{th} of February?

MR. KATZ: Yes.

MEMBER GRIFFON: You don't want to move it to the end of January?

MR. KATZ: Well, I'm just thinking --

MEMBER GRIFFON: I'm just concerned that we might need a phone call follow up or something before the meeting.

MR. KATZ: I'm thinking that this meeting would be where things got decided. But you're saying --

MEMBER GRIFFON: Well, I'm just saying, my past experience tells me that even when we say it is the last meeting, there is always one more you need, and we might want to leave time for a phone call --

MR. KATZ: Yes, I'm trying to also leave time though, for work to get done and --

MEMBER GRIFFON: I know.

MR. KATZ: And you have Christmas holidays and Thanksgiving --

MEMBER GRIFFON: Considering that is what is on the --

MR. KATZ: If you -- I think we want to get deliverables on the table two weeks in advance of the meeting, too, then it's hard to push it back too far.

MEMBER GRIFFON: Right, okay, all right.

MR. KATZ: Well, how does the 10^{th} or the 9^{th} of February work for folks?

MEMBER GRIFFON: I'm fine for that.

CHAIRMAN CLAWSON: I would rather go towards the 8^{th}.

MR. KATZ: The 8^{th}?

CHAIRMAN CLAWSON: Yes.

MR. KATZ: The 8^{th} is open too.

CHAIRMAN CLAWSON: Okay, I'd rather go --

MR. KATZ: Does that work for folks?

MEMBER GRIFFON: Tuesday the 8^{th}?

MR. KATZ: Does that work for you, Mark?

MEMBER GRIFFON: I might have to phone in, but I'll -- you know.

MR. KATZ: Yes.

MEMBER GRIFFON: Yes.

MR. KATZ: As long as we --

MEMBER ZIEMER: This is Ziemer. It's okay for me.

MR. KATZ: For the 8th, okay.

MEMBER ZIEMER: Yes.

MR. KATZ: And we'll send an email to Bob, checking with him. He is not here, but -- Presley, that is. That's good for you, Phil?

MEMBER SCHOFIELD: Yes, it looks good to me. I don't have a life.

MR. KATZ: Okay, so, let's say February 8th. I don't have a life, either.

CHAIRMAN CLAWSON: I would like to be kept apprised of the process, as these are coming up.

MR. KATZ: Yes.

CHAIRMAN CLAWSON: We've got this meeting up there, I would like to be able to have plenty of time for both sides to be able to review the information, so that we can come at this with a better feel for everything.

So, if things are being pushed back or whatever, or whatever avenues we need to be able to do, to put -- to assist in the matter, I would appreciate letting it be known.

MR. KATZ: Absolutely. Anyone, either side, if you are having issues with getting a deliverable out? Please, let us know, as soon as possible.

CHAIRMAN CLAWSON: Okay.

MR. KATZ: Are we adjourned or do we have more?

CHAIRMAN CLAWSON: Actually, I would just like to run through the action items, to make sure that I'm - that we're clear with what each group has to be able to do.

Mark, you already mentioned that you had two deliverables for us. That was the -- well, go ahead.

MR. ROLFES: Do we want to do this again, or we've been sending emails. We can exchange emails and if there is --

MR. STIVER: Do you want to do that, just drop a list and exchange --

MR. KATZ: I think we'll be good. We've gone over them already. If SC&A and DCAS send out their action lists -- then we can carefully go through the notes and make sure they've got everything, and that they've described it fully enough, everybody can say, "Yes, that's it."

DR. MAURO: There were a lot of action items.

MR. KATZ: Yes, and there are some nuance as to what is to be done.

MEMBER GRIFFON: You're going to send that to the Work Group too, right?

MR. KATZ: Yes.

MEMBER GRIFFON: I've got good notes.

MR. KATZ: To the full Work Group and to the staff, and I'll forward it, whoever doesn't get it.

CHAIRMAN CLAWSON: If that's all, then the Work Group is adjourned.

MR. KATZ: Paul and everyone on the

phone, thanks for hanging in with us.

MEMBER ZIEMER: Okay, thank you.

(Whereupon, the above-entitled matter went off the record at 4:18 p.m.)