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
FEBRUARY 8, 2011

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The Work Group convened 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
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*
ROBERT ANIGSTEIN, SC&A*
SANDRA BALDRIDGE
BOB BARTON, SC&A*
HANS BEHLING, SC&A*
ZAIDA BURGOS, NIOSH Contractor*
HARRY CHMELYNSKI, SC&A*
SAM GLOVER, DCAS
DAN HENNEKES
KARIN JESSEN, ORAU Team*
KAREN KENT, ORAU Team*
JENNY LIN, HHS
JOYCE LIPSZTEIN, SC&A*
JOHN MAURO, SC&A
ROBERT MORRIS, ORAU Team*
GENE POTTER, ORAU Team*
BRYCE RICH, ORAU Team*
MARK ROLFES, DCAS
JOHN STIVER, SC&A
DAVE SUNDIN, DCAS*
JIM WERNER, SC&A*

*Participating via telephone

C-O-N-T-E-N-T-S

C-O-N-T-E-N-T-S

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

9:07 a.m.

MR. KATZ: Good morning, everyone in the room and on the line. This is the Advisory Board on Radiation and Worker Health, Fernald Work Group.

My name is Ted Katz. I am the Designated Federal Official for the Advisory Board, and we will begin with roll call as usual. Since we are talking about a site, please speak to your conflict of interest as well, for people, as I say, with the agency.

We will begin with the Board, with Board Members in the room, with the Chair.

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

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

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

MR. KATZ: Board Members on the

line?

MEMBER PRESLEY: Bob Presley, Board Member, no conflict.

MR. KATZ: Any other Board Members on the line? Okay. Zaida, do we have you on the line?

MS. BURGOS: Yes -

MR. KATZ: Yes, thank you Zaida. Okay, let's carry on. NIOSH ORAU team in the room?

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

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

MR. KATZ: And NIOSH ORAU team on the line?

MS. KENT: Karen Kent, health physicist, no conflict.

MS. JESSEN: Karin Jessen, ORAU team, no conflict.

MR. MORRIS: Robert Morris, ORAU

team, no conflict.

MR. RICH: Bryce Rich, ORAU team, no conflict.

MR. POTTER: Gene Potter, ORAU team, no conflicts.

MR. SUNDIN: Dave Sundin, DCAS, no conflict.

MR. KATZ: Very good, thank you. SC&A team in the room.

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

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

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

DR. ANIGSTEIN: Bob Anigstein, SC&A, no conflict.

DR. BEHLING: Hans Behling, SC&A, no conflict.

DR. LIPSZTEIN: Joyce Lipsztein, SC&A, no conflict.

MR. WERNER: Jim Werner, SC&A team, no conflict.

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

MR. KATZ: I'm sorry, the last one we couldn't hear you.

DR. CHMELYNSKI: Harry Chmelynski, SC&A, no conflict.

MR. KATZ: Oh, Harry, welcome, sorry. Thank you. Okay and now HHS officials or contractors to the feds, HHS, other agencies in the room.

MS. LIN: Jenny Lin, HHS.

MR. KATZ: And on the line?

MS. ADAMS: Nancy Adams, NIOSH contractor. Ted, the volume on a lot of the folks in there is really low, that are in the room.

MR. KATZ: Okay, thanks for that notice, we will try to do well with the mics. Might need to spread them around, too. All right, and now members of the public in the room?

MR. HENNEKES: Dan Hennekes, I'm

with the Building Trades National Medical Screening Program, and I worked at Fernald for 23 years.

MR. KATZ: And that's Dan?

MR. HENNEKES: Dan, yes.

MS. BALDRIDGE: Sandra Baldridge, petitioner.

MR. KATZ: Welcome, and on the line, members of the public?

MR. WEBER: Al Weber.

MR. KATZ: Welcome. Any other members of the public that want to be identified? Let me go back and just see if we have any other Board Members joined us.

Okay, they'll check in when they do, I'm sure. We have an agenda for the meeting. It's posted on the web. It was posted probably yesterday on the web, and, Brad, it's your agenda, so.

CHAIRMAN CLAWSON: I appreciate that, Tim -- Ted. There we go, sorry about that. We are going to start out with issue 1, which is review of completeness and adequacy for the uranium bioassay data available for dose reconstruction at Fernald.

And I believe that was --

MR. STIVER: Do you want me to discuss that?

CHAIRMAN CLAWSON: Yes, just if you would, John.

MR. STIVER: Yes, this is a recap, this was -- this issue has been resolved for all intents and purposes. This was a revisional language in OTIB-78 to allow use of the upper end of the distribution for certain classes of worker with higher exposure potential.

And that change was made as of last -- actually it was made after January 29th of last year, so we are in agreement that that issue is resolved.

The only remaining issue has to do with -- it's kind of related to -- issue 2, which is the coworker model, and so I guess we can kind of segue into that.

This is the -- the issue 2 is the validation of the HIS-20 database. There's really three subparts. The first subpart had to do with the completeness of the validation for the first go-round.

I believe there were 25 sets of data, or I believe five or six that weren't completely analyzed to the level of granularity as the others because of -- the first sets of data turned out to be very consistent. And so the issue that came up was that well, we felt that NIOSH should go ahead and continue and finish up that study, which they indeed did do in December of 2010.

They submitted a final revision called Comparison of FMPC Hard Copy Bioassay Records to the HIS-20 Database, dated May 10, 2010. And our review of that indicates that they have indeed -- are fully compliant with our suggestion, and so we can recommend closing that part of the issue.

The second issue was the construction workers, and this is the idea that maybe there is a subset of workers in the plants -- the construction workers -- who may have a higher exposure potential and would not be well represented by the distribution of bioassay data for the workers.

We noticed a statistical difference for the Savannah River Site and felt that it would be good to do a similar type of analysis for the Fernald site.

And I believe an action item came out of the November 10th meeting was -- that you guys were in the process of developing that coworker study, for the construction worker adjunct to it.

MR. ROLFES: What we have done is taken some hard copy records for some of the subcontractors at Fernald and have compared those urine excretion concentrations to the main coworker intake model in OTIB-78. And we are still gathering some additional data to make our comparison a little bit more complete, I guess, at this time. So as soon as that is completed we will document that and then send it out to the Working Group.

MR. STIVER: Any estimated time when that might be ready?

MR. ROLFES: Let's see. We have got some preliminary information, but let's see -- as far as a time, I couldn't give you a time on that right now.

MR. STIVER: Okay. But it is in the works now -

MR. ROLFES: Yes, correct.

MR. STIVER: -- the analysis is being done.

CHAIRMAN CLAWSON: So are we able to -- this is Brad -- are we able to segregate the construction workers out of the -- are they clearly identified then in all the --

MR. ROLFES: Yes, if you take a look at their urine bioassay request cards, you will see a card with the individual's name and the subcontractor that they were employed by, and then it also gives the sample results. And so we -- that's -- it's not in the electronic database so it's all hard copy records and hand-written results, and so what we have been doing is going back through the urine bioassay cards and we have got to enter those into, like, an Excel spreadsheet and characterize them that way, rather than already pulling them from an electronic database, like HIS-20.

CHAIRMAN CLAWSON: So I guess, Mark, one of the things that I am wondering on this is how much -- because numerous times we have heard from the construction trades that, you know, they have worked there for numerous years and they have never been -- had any kind of urinalysis and stuff.

I guess I was just wondering, are was also looking at the percentage of them that were sampled? Was this a random sample that -- construction?

MR. ROLFES: No, it wasn't random, it was usually following work. Most of the construction workers had pre-job bioassays taken and then post-job samples.

There were less post-job samples however, and a lot of the samples are identified as special samples, so we are still looking into the reason for why the bioassays were taken, and we are not sure if the special sample stands for something related to, you know, similar to an incident, but that is one of the things we are looking into.

CHAIRMAN CLAWSON: Okay, so beyond just looking at the construction workers' bioassay, you are also looking at the process of why they were pulled and so forth?

MR. ROLFES: Correct.

CHAIRMAN CLAWSON: Because what is interesting about Fernald is -- which is different than Savannah River -- each one of these sites have their own unique process to it. But one individual I talked with had been there for 24 years and had worked for four to five different contractors. He had never left the site.

And that is why I was wondering if also, when these contractors left, if they did an off-going bioassay or -- I just wanted to get a little bit more information of what the process was with it.

MR. ROLFES: Sure. That's something that we are certainly looking into, and we have Gene Potter on the line, he's the one that has been doing a lot of the comparisons and the analysis of the uranium intakes for the entire population compared to the subcontractor population.

Gene, I don't know if you have anything to add on what we have done or if I have captured everything accurately?

MR. POTTER: No, I think you have captured it accurately. We're unable to draw any conclusions of what we have done so far.

MR. ROLFES: Okay, right now I

think we have sampled some of the data from the `70s, `80s, is what we have focused on right now, and we are looking at additional data as well, so --

CHAIRMAN CLAWSON: Okay. Thank you.

MR. STIVER: Okay, the third part of this issue deals with the data integrity, and this was -- the issue was raised by Sandra about potential falsification of records.

And evidently at the last meeting we -- Bob Barton had presented a paper that looked at different ways that this data could be looked at in order to determine if there were some inconsistencies that might lead us to believe that there had been some tampering.

And one was to compare the urinalysis to the in vivo chest counts, in other words to look at the data consistency with biokinetic models, and a third was to compare DWE results with urinalysis for categories of workers we knew were in certain facilities at certain times.

And there was quite a bit of a discussion about this. In the end I believe no action item resulted because past efforts to address this type of thing had resulted in great expenditures of resources without any conclusive results.

And so that issue has been tabled to the best of my knowledge. So I guess in summary, what we are really looking for now is the construction worker comparison, and that would be the end of the discussion on issue number 2.

Which brings us to issue number 3, which is the recycled uranium issue, and has everybody got --

MEMBER ZIEMER: Well, hold on. So is that unresolved at this point? What are you saying in terms of the bottom line for that issue?

MR. STIVER: The bottom line is that in the past, wasn't it the same -- at NTS we had the same kind of an issue going on there.

DR. MAURO: We had a conversation regarding the merits of going through a process similar to the process we went through at Nevada Test Site, which was quite protracted, very expensive, and in the end -- we suspected in the beginning that well listen, in that case, for the purpose of due diligence, given the amount of attention that was given at Nevada Test Site, you may recall, that -- well maybe we should go through this exercise, at that time certain ideas came up about how to test it, which we did.

And in the end, as we suspected, it ended up being inconclusive. In other words we confirmed, yes, there was a lot of deliberate leaving badges behind. I'm talking Nevada Test Site.

But there was nothing about it that would prevent us or NIOSH from constructing distributions for coworker models that would apply because it was more or less across the board. It wasn't that the ones that were left behind were only in the upper end of the tail, thereby biasing the distribution. We found that it was all types of workers under all circumstances, after lots of interviews and lots of data comparisons.

So in the end we ended up being inconclusive. Now that was that experience. The question becomes, here we are at Fernald, and the question becomes do we want to and does NIOSH want to initiate any one or other of the types of strategies that Bob Barton laid out in his report.

Each of them would be quite an undertaking, and we suspect that there would be -- we would be in a similar situation at the end.

We may find yes, there may have been certain practices at work where bioassay samples were not collected, were not analyzed for whatever reasons, or were collected and were not analyzed.

I mean, these kinds of things, we will probably find these things. But then the question comes, is okay, is that going to affect the ability to build a robust coworker model that you feel does capture the full distribution of the kinds of concentrations of uranium in urine that cut across the board.

And until we get there we won't be able to say one way or the other. We suspect that this type of problem is very hard to come to some resolution after loss of resources. This is SC&A's perspective on it.

However I don't know whether or not the Work Group had actually come to the conclusion let's just put this one to bed, or do you want to go forward?

And if it's something that -- something to go forward, of course this would be something that NIOSH would need to initiate.

CHAIRMAN CLAWSON: When John spoke

with me about this, of different avenues that we would be able to proceed, and one of the things was, is at the very end, were we going to be able to actually prove one way or another. And I don't see any way that we would really be able to conclusively be able to do that.

MEMBER ZIEMER: I don't think you are ending up proving one way or the other. I don't think it's a proof. But you sort of have to determine whether it's reasonable to think that the coworker model, using the existing data, is greatly impacted by either absence of those or falsification of -- if it's falsification, I guess you assume that things are entered lower than they should be.

There's no reason to think someone would put in a higher number unless they wanted to get out of working by showing they had some limits. I suppose it could go either way.

If the data are absent, it would

be unreasonable to think that -- well, someone would have to know a priori that they were either high or low or whatever and say well I don't want that in the record. So if the analysis wasn't done, there's no way of knowing which it would be. So I think it would be reasonable to think that something that is missing has got to have a distribution like what's there.

So the only issue in my mind would be if people are falsifying it, why are they doing it and what would be the tendency. Would it be the tendency to put it in lower or higher or what? I mean, there could be all kinds of motives there.

DR. MAURO: That's exactly what we found out.

MS. BALDRIDGE: As I went through the documents that were -- the court documents when this was addressed by the federal court, the documents showed that Fernald had the tendency to appear and present themselves as being in compliance with DOL requirements, when in fact they weren't.

MEMBER ZIEMER: Which would imply you would want to have a lower number.

MS. BALDRIDGE: Which suggests to me that they would do everything possible to present themselves as being in compliance, even to the point, there's one document that says, you know, we were challenged on this. We told them what they wanted to hear as far as a worker exposure level that was extremely high.

I think we have satisfied them for now, but actually the situation is getting worse. So that shows me that there were those people in place who had purpose to misrepresent the actual working conditions, even to DOL.

Now any of the data that is given back to NIOSH for dose reconstruction is data that DOL was suspect of in the first place.

CHAIRMAN CLAWSON: Paul, when me and John spoke about this, one of the things is, and we have seen this at numerous other sites, is they -- especially in the `80s and `90 time period -- they were starting to get -- be given limits that they have to be able to stay under. So the only thing that I can, in my personal opinion, is they were always wanting to stay underneath that.

Now when you start talking about that, you have got to have some evidence of things higher, which could be the air sampling data or so forth, like that, but were showing incredibly much higher, but the people's dose were so much lower.

And you start to get into a situation where it would be very hard to be able to prove this one way or another. This is one of our big issues that we are facing. How do we prove if they were always -- you'd have to have some kind of data above same old, here this is, but we are still down, we are still way down there.

DR. MAURO: For example, let's say

you decide that, okay, does the data ring true, and one way to ring true is that, okay, here we have lots and lots of air sampling data, and we have lots and lots of bioassay data. Over 90 percent of the workers starting in `56, well over 90 percent, had bioassays.

CHAIRMAN CLAWSON: Urinalysis, right?

DR. MAURO: Urinalysis, milligrams per liter of the uranium in urine. And one could argue, okay, let's just go ahead and this is not unlike the type of thing that was done at Nevada Test Site.

Let's go grab all the high-end bioassay results for various buildings at given time periods, and let's go simultaneously grab air sampling data and see if they sort of ring true. Do people -- where we are seeing high air sampling data, that's where we are seeing the high bioassay data.

Now in my opinion, given the vast amount of data, bioassay data that was collected at every building, in every trade, and in every decade, it's so enormous, in order for there to be a conspiracy to deliberately bias low the high-end tail, in other words let's cut off the upper-end tail so that we look good, that would have been quite an effort because this would -- the amount of data that we are talking about, the number of people, the number of samples throughout the plant, throughout the decades, throughout the buildings, it would be quite an effort in order to systematically -- that doesn't mean it didn't happen.

But in order to study this and say the degree to which we think it might really have happened, there would have to be pretty clear and unambiguous evidence that for the various strategies that Bob Barton laid out, you could say gee, we look -- we could start, for example, with the air sampling data and compare that to the bioassay data and see if in fact they seem to ring true.

Or do we see a situation where, holy mackerel, look at this, we are seeing high air sampling data over and over and over again, decade after decade, building after building, and the people that were in those buildings in those years we're seeing low urine samples. Just doesn't make sense. If that came out, yes, we would say well, something is wrong here.

MEMBER ZIEMER: Well, look, if you have got those real high levels, number one, you are going to have some kind of --

DR. MAURO: And there'll be -

MEMBER ZIEMER: -- respiratory protection --

DR. MAURO: -- another confounding variable --

MEMBER ZIEMER: -- which if used properly, should result in --

DR. MAURO: There you --

MEMBER ZIEMER: -- you can use the argument, and we don't use the respiratory

protection category --
DR. MAURO: We do not.
MEMBER ZIEMER: -- in these figures.
DR. MAURO: You are absolutely right.
MEMBER ZIEMER: So that is a -- there's a mismatch there that would say if anything, you are overestimating because you are assuming no protection.
Now the other part of it is -- I lost that thought. Oh, yes, so you have that issue. The other part is if you are going to doctor a sample, you have got to keep doctoring the successive samples on that because one bioassay doesn't help.
And you would have to be really clever -- I don't think the people that are doing the sampling and the recording are in a position -- you have got to be able to manipulate that data out for years in order for it to fit --

DR. MAURO: Absolutely right.

MEMBER ZIEMER: -- a biological model. So it is not an issue of -- I mean, if you had one thing that is manipulated, it has almost no effect on the long-term thing if you have other samples in there.

DR. MAURO: That's correct.

MR. STIVER: Yes. And you also have the issue of, you know, workers moving among different sites, so you may have somebody who was in a highly-contaminated area and then a year later he is working in a different job --

MEMBER ZIEMER: And somebody would have to say, well, here's what I did to the data so now you have got to do this in order for it to --

MR. STIVER: Yes, and that is something you see with thorium-232 data later on, too, but it is a different issue, but it is the same kind of a confounding problem that comes up in trying to make those types of comparisons.

MR. BARTON: Yes, this is Bob Barton. To add on to this discussion here, it is not only a question of these variables about moving between job titles and needing respiratory protection. It is also very difficult to match certain workers' bioassay results to specific areas. There's some limited information in the HIS-20 about that, but by and large you are not going to have that information.

So, yes, 90 percent of the worker population has uranium data, but the percentage that we can actually match to a building and also have air sampling for that building and time is very low. So there's feasibility issues that go beyond just -

CHAIRMAN CLAWSON: And this is what -- what were we going to come up with, with the final project? You know, we had a lot more outstanding issues that we really needed to take care of before we got into that. And so, as I have told John, it may not be something that we even need to look into right at this time. We just want to make sure --

MEMBER ZIEMER: Yes, I just wanted to sort of get a feel for the nature of the problem and also, Sandra, if you could help me understand, on those past events where there was this apparent false representation, do you know if they simply were taking, like, the summary data for the year and presenting other numbers?

I mean, the true values might still be in the database, or did DOL or DOE or somebody go in and actually look at the database itself?

MS. BALDRIDGE: Well, the big issue, when this all went to trial, was especially with the stack emissions, where numbers were just -- zeroes were entered instead of an actual reading, and the explanation was, oh, well we were going to put those numbers in later.

MEMBER ZIEMER: Oh, I see, okay.

MS. BALDRIDGE: In other cases, numbers were just arbitrarily assigned to locations and that discrepancy was discovered and said, hey, how can you have this emission when that plant wasn't even operating and here this plant was operating, and you are not assigning anything there?

MEMBER ZIEMER: -- those numbers aren't used for the dose reconstruction.

DR. MAURO: No, so your experience where this problem arose is more towards the source term, the airborne emissions to the atmosphere, as opposed to bioassay data?

MS. BALDRIDGE: That is the point that came out in the trial. But it showed a pattern, based on correspondence that management had with DOL, giving them the answers, telling them what they wanted to hear, and then later on finding that they absolutely fabricated data.

It just shows a pattern that in my mind says this means -- what else were they doing? They discovered they were applying factors to the actual numbers to change the appearance of the outcome, and they were found to be significantly deceptive.

MEMBER ZIEMER: Okay, got you.

MR. STIVER: I think part of the problem with the airborne emissions had to do with the way they were calculating the releases from the stack samples, and as I recall, there was a -- a mistake had been discovered and it had never been corrected for a number of years after the discovery.

So there were -- I don't know if it was a matter of deliberate falsification or just sloppy accounting practices, and maybe some combination of the two, but that result was that there is a suspicion on the part of our people regarding the integrity of that data.

CHAIRMAN CLAWSON: And like any site, it brings into question any of the data from then, and as far as bioassay goes, if they would have done that, they would have had to have -- like you said earlier, they would have to have one set number, you know, 10 percent off or something like that, but through the whole thing, because there is no way you would be able to single anything out like that.

MEMBER ZIEMER: Right, and you'd have to get a lot of people involved in doing it.

MR. STIVER: Yes, exactly, and with the stack emissions you have basically one source term, one number that either might be right or wrong.

But with bioassay, you have hundreds of workers, you have got multiple samples, you have to understand the health physics, you have the biokinetics and you would have to be able to match that up to where it would appear to be real results, to be more -- enormous undertaking, more so than doing a good program to begin with.

MS. BALDRIDGE: And then there's the case where the record-keeper said you can't use this data for determining internal exposure. Now maybe they knew that there had been a factor --

MR. STIVER: I think that that particular issue had to do with the fact that you did not have biokinetic models in place at the time where you could really use that data in order to estimate the intake.

MEMBER ZIEMER: Yes, in those days they couldn't do it.

MS. BALDRIDGE: You know it said the data wasn't reliable.

MR. STIVER: Yes, and I think that was maybe be misinterpreted.

DR. MAURO: Oh no, because you're saying that it's more than that. You are saying that there was some question -

MEMBER ZIEMER: Well, no, at that time they didn't have -

MR. HENNEKES: May I ask a

question here? It seems like everyone is making an assumption that it was getting this air -- the data, but working down there, I know we worked demolition down there, and there was no air sampling done for a period. We didn't know what it was for like four or five years.

MR. STIVER: What time period was this?

MR. HENNEKES: This was about `82 to `86, and we worked in the old pile plant, which we did the demolition there. So there wasn't any air sampling. It was coming out of the stack, but -- we were doing the demolition and there was no BZs or anything. I mean we didn't even see a rad tech or an IH tech.

DR. MAURO: Were you getting urine collections?

MR. HENNEKES: You know I asked about that, and I'm not really sure if we did or not back then.

MR. STIVER: Did you personally

have -- were you monitored for urinalysis yourself?

MR. HENNEKES: I could ask -- I know about `86 we were, but those early years when I was down there --

MR. STIVER: You know what, that's -- the point that you are making here is that -- our research has shown that prior to `86, the program was --

MR. HENNEKES: Well it was a non-existent --

MR. STIVER: National Lead of Ohio was running the program.

MR. HENNEKES: Exactly, yes.

MR. STIVER: You know, when Westinghouse came in, they --

MR. HENNEKES: It got a little bit better, and then when Rust came in it got a lot --

MR. STIVER: And that would have been in about `85, `86 time frame.

MR. HENNEKES: When Fluor came in,

the -- got better, but in those early years, when we was working in the Pilot Plant, I mean, a rad tech or an IH tech was non-existent. We didn't even know what they were.

And we were moving around the different buildings, down at Plant 9, 64, 65, and there was no one to go in with us, you know, they said well this is your job, this is what you need to do, but there was no type of monitoring available at that time. Thank you.

CHAIRMAN CLAWSON: And so, Paul, and maybe this is wrong of me -- I -- we have kept this open. But it's like what John said. What are we going to come with at the end, you know?

It looked like to us that we were -- it would have had to have been a complete blatant or -- it would have been harder to do that than to run the program, so we kept that in mind but we decided not to do anything with that because we had bigger issues that were with the uranium processing and so forth like that.

We just wanted Sandra to understand that we have -- we have not forgotten as we've looked at this, and we have given it an awful lot of thought of how we would be able to address this, and it is one of these ones that I don't think that we could really come to anything conclusive with, but we haven't forgotten it. We have tried to address it and we have been thinking quite earnestly about how we would address it.

Now the air samples and stack emissions, we did understand them, we did see that there was issues with that. That was more of a procedural problem that nobody can judge what they did, but they knew of the issue for years but they never corrected the factor. You can say that it was to keep it under and it did, but this is what came out in the lawsuit, too. So -

MS. BALDRIDGE: And considering they were under cost plus bonus, you know, you keep levels here and there's more money into your pocket.

(Ms. Baldridge's references to DOL are meant to refer to DOE, as she clarifies in a statement prior to the lunch recess.)

CHAIRMAN CLAWSON: And as we have seen at every site that we have dealt with, any site, if they come into the 1985 to the 1990 time period, we see a big change in how things were done. That's when the DOE order RadCon Manual came out and everything transitioned. It wasn't an overnight change, but from `85 to `90, `91, you always saw a big change in how process -- and a lot of the data that we started receiving was so much better.

But anyway, John, I'll turn it back over to you and --

MR. ROLFES: Before we continue on, I wanted to add a couple of things. I wanted to keep in mind also that there was never a cross examination during the court -- when the judgment was granted and so the stack data wasn't represented very well.

And I just drew a little example up on the board here, for example for 1970. We had interviewed one of the individuals from the IH&R department at Fernald. And he had basically said that they would go and visually inspect the filters in the stacks to determine whether there was any visible material on them, and if there wasn't they would leave them in service until they did observe some visible uranium or anything else on them, and at that point they would replace them with a new filter and bring that filter back to their lab to weigh it for uranium, and they had a factor to apply based on the surface area of the filter and the flow rate through the stack et cetera.

So there are time periods in certain months when they were replacing the filters where they would enter a dash into the record or a zero. I believe they were actually dashes.

We did interview this individual. We documented that as a reference in our Site Research Database, and it does show that there are some dashes in for various months where they left the filter in service but then subsequently had pulled that filter out and reported that value for that month.

MEMBER ZIEMER: So the June filter would include all the uptakes or the depositions from February through June --

MR. ROLFES: Correct.

MEMBER ZIEMER: -- is what you are saying.

MR. ROLFES: For this example, that's correct.

MEMBER ZIEMER: So yes, got you. Okay, I'm good, Mark. Thanks.

CHAIRMAN CLAWSON: Actually, I wanted to make sure. There's been a lot of talk about this, of how and what we could do on this, and we didn't -- at the time we just decided there's too much, and I don't think we have come up with anything conclusive at this time, and maybe later on. But we have other outstanding issues that need to be addressed.

MR. STIVER: And probably the most important of those is the recycled uranium issue, and if everybody here, I believe you should have the email --

DR. GLOVER: Is there any action item on that, then, as we leave that subject? Is that -- there's nothing to NIOSH or -

MR. STIVER: At our last meeting no action item came of it and there's really nothing at this point.

CHAIRMAN CLAWSON: There's no action item. The only thing we have is on action item one, that you guys are still ongoing with the construction work. That's a separate issue altogether there.

MR. STIVER: Mark indicated that one was in process.

MR. ROLFES: Correct.

MR. STIVER: Okay, so if you would

all go to your email, and there's three presentations. There's two PowerPoints and there's one PDF file. If you could just --

MEMBER ZIEMER: This is what you just sent.

MR. STIVER: -- which I just sent. Open up the PowerPoint presentation entitled RU Issues, 110206a-NSJJHS. And this is the RU presentation.

Everybody have that up? Okay. All right. If you go to slide two, which is the outline. This is basically the road map of the discussion today.

I have a lot of slides. Probably about a third of them are kind of a recap of previous discussions. This is a very complex issue. It's been ongoing now at least since January 29, 2010. We have discussed this issue in detail, both at that meeting and again in the November, 2010 meeting.

What I am going to do is go through the background of the RU issue, the milestones and action items that led up to this particular review, then look at the historical perspective, basically the types of materials that were received, the processing that was taking place at Fernald, and the consequences regarding worker health that could have resulted from those.

But then we are going to move on and take a look at the NIOSH defaults, basically looking at default levels for plutonium-239, neptunium-237, technetium-99 and other fission products, look at the dosimetric implications, the basis underlying those default values.

And one of the -- probably the most important document is this Ohio field office report where the DOE reports on recycled uranium that came out in the year 2000.

And this really is the fundamental underpinning of the default level that NIOSH has used.

MEMBER ZIEMER: John, just a quick -- did you send one of these to Mr. Presley?

MR. STIVER: No.

CHAIRMAN CLAWSON: I've got his email. I'll send it to him.

MR. STIVER: Okay.

MEMBER ZIEMER: Bob, are you still there?

DR. MAURO: Also, is this -- PA cleared?

MR. STIVER: Yes, this one has been PA cleared -

DR. MAURO: So this can be made available to anyone who wants to look at it?

MR. STIVER: And I have to send it out to the rest of the team.

MEMBER ZIEMER: Brad's going to email this to you, Bob.

MEMBER PRESLEY: Okay. Thank you.

MR. STIVER: So, Bob, just kind of follow along in the discussion, and we will have that in hand here in about a minute or two.

But DOE 2000b is the seminal paper and it has basically been taken to almost be the bible of RU issues.

Jim Werner, who is one of our associates, who is involved in the preparation management of that piece of work, is going to jump in at that point and give his own sub-presentation regarding that particular document and its applications in dose reconstruction.

The other thing we are going to look at is site-specific data. Part of the action item that the Board directed us to pursue at the last meeting was to look at, in particular, these baghouse dust collection samples that were taken in 1985, which were presented as an attachment to the NIOSH RU White Paper.

And so we have looked at that, and in the process our team has done an exhaustive research effort in the SRDB and other sources, and we have found two other sets of data, actually three other sets of data that bear directly on this issue of what the ratios of these RU contaminated water -- actually on site in various buildings at certain times.

And those are some boundary air samples that were collected as part of an environment compliance requirement for NESHAPs in 1983.

Air samples were collected in 1989 which were addressed, actually, in the NIOSH White Paper. And then also some Hanford uranyl nitrate hexahydrate solution production data that came out in 1970, 1972. And then in conclusion we will look at the summary of findings, and how that all ties together.

If we can move on to slide three, this is just a kind of quick overview of the milestones. In October 2008, SC&A was tasked to review the NIOSH White Paper on RU with the goal of identifying whether the default values were really bounding for all workers, and that's pretty much the same issue that is alive today.

January 29th, the White Paper was discussed in detail. SC&A's paper, we had 11 findings. NIOSH was going to prepare a response, which they then delivered prior to the November 9th meeting. Those responses were also discussed, and two unresolved issues emerged from that. If we go on to slide four.

The action items for SC&A was to provide a White Paper response looking at two things. First -- wait a minute, back up. At the November meeting we presented a fairly compelling argument as to why DOE 2000b, the Ohio field office report, was incomplete and was probably not suitable for a source document for dose reconstruction.

The Board requested that we put that down into a formal response. We believe that the transcripts of the previous two meetings and our original White Paper present that data -- that information fairly coherently.

But we went ahead and did a more detailed review and put some more information in and also brought Jim Werner on, who has this unique perspective of actually having been involved in the management and the preparation of that document.

The second was the focused review of the site-specific data, which I just talked about, and NIOSH was to provide a memorandum on the dust collector data, and basically if they could identify the sources where the dust collectors were taken, what the sampling period was, and that kind of thing.

And, Mark, I believe you did provide a memo recently, you posted it on the O: drive? And so they have fulfilled that requirement.

We also looked at the availability of the DOE subgroup data. There were about 4,000 data points all told, 3,000 of which came from Fernald. And this was really the basis for the statistical analysis that they used in this DOE 2000b report, to really -- it was more of a materials balance exercise to identify what processes did this material report to in the various time periods involved.

Move on to slide five. A historical overview. I am not going to spend a lot of time on these slides. I just want to kind of get everybody back on the same page here, since it has been a while since we discussed this.

Me on the other hand, I have basically been -- this has become all-consuming. It's basically all I've done for a while so bear with me if you will.

RU is basically uranium which was recovered from irradiated production reactor fuels and plutonium production target fuels. They were separated in the chemical processing plants at Hanford, Savannah River, West Valley and Idaho.

Basically ended up with two streams of reprocessed materials, one which was of most interest to the AEC for weapons production was plutonium recovery. The second, which was a lesser concern, was the uranium that was known to contain transuranics and fission products, but this is the primary concern for the workers at Fernald for the SEC context.

I'll move on to slide six. This is just a listing of different types of chemical forms to identify the amount of variability that there was in the data that were coming in -- or in the types of materials that were coming into Fernald.

There was uranium trioxide, scrap from Hanford, ash from the Paducah Gaseous Diffusion Plants and Portsmouth and also from Oak Ridge, various types of oxides, ashes, hexahydrate, and so forth.

And I guess the most important thing here is that there really was no agency-wide or even site-specific limit that were set for the radionuclide contaminants, all the way up until the 1980s.

On to slide seven. This is just kind of a summary from the DOE reports, basically the 2003 report. I looked at only receipts -- or only shipments, excuse me -- that came from the production sites. And this shows that about -- as we all know who have been involved in this -- about 80 percent of the RU came from Hanford, starting in 1953.

By 1960, there was about 45 metric tons of the material on-site. The receipts peaked in the 1960s and then again in the 1980s, and all told about 18,000 metric tons of uranium -- of recycled uranium was processed through Fernald during this period, which contained about 500 grams of plutonium, about 38 kilograms of neptunium and roughly about 900 kilograms of technetium-99 introduced into the DOE complex.

About 70 percent of the shipments

went to the Paducah Gaseous Diffusion Plant, and only 15 percent came to Fernald. And it's estimated that about 50 percent of all the plutonium that wound up in the Fernald site came from one shipment of the Paducah tower ash in 1980. It's a topic of extensive discussion at these meetings. The balance of plutonium came from West Valley, Savannah River, and other sources.

Okay. Previous findings related to receipts, they were from our last report which I'll call SC&A 2009. Findings one through three were in relation to inconsistencies and gaps in the amounts of sources of RU.

Finding five is a little more to the point, and this was a concern we had that the data were incomplete, that there were potentially very important sources, source terms that may have been missed. The one that we identified was the material recovered from the high-level waste tanks from 1952 to 1958 at the Hanford U Plant.

And so we are not really concerned about accounting for every kilogram of recycled uranium that came through Fernald. What we are really concerned with is this apparently incomplete accounting of the contaminant levels in those receipts and any distributions that could be built from that in order to assess worker exposures.

What I would like to do now is just go through a brief summary of the plants, the processing plants at Fernald, what the activities were, what the activities and sources of the high exposure potential were, and the particular compounds of concern, without spending an inordinate amount of time on this. This is all in the report, pages 15 to 19, and it's a very detailed overview of that.

Plant 1 is a sampling plant. This is a very important plant in terms of potential worker exposures. It was the AEC sampling station. They did isotopic analysis for uranium there. But they also received, weighed, sampled, and stored materials and sources process residues.

And probably most importantly is the milling of by-product slag from Plant 5, the burning and drum reconditioning, screening, milling, packaging, and various sorts of things that went on there. There was very high airborne dust potential for these milling operations, drum dumping, dust collection, and our concern mainly was magnesium fluoride and black oxide in these residues.

Plant 2 and 3, this is the refinery, and, incidentally, there is no dust sampling data available for the refinery, which is a finding we will get into later on. This is where the impure feed materials were processed into pure UO3. It was a three-step process which we have become pretty intimate with, acid leaching, solvent extraction, and then thermal decomposition.

The high exposure potential activities there were digestion and de-nitration, and, once again, they're pretty concerned with feed, black oxide, and hexahydrate.

MR. ROLFES: John, could you restate what you said about the air sampling data in Plant 2/3?

MR. STIVER: In Plant 2/3, the dust collector data we looked at, there was nothing for Plant 2/3. There was some DWE data. There was some DWE data. It was done by Wing and those guys back in, I think it was in the mid-`80s.

MR. ROLFES: I didn't know if you said air sampling or --

MR. STIVER: Yes, I may have misspoken -- regard to the -

DR. MAURO: So you make reference to the dust collector data because dust collection data is an important source of understanding the ratio of let's say plutonium to --

MR. STIVER: It really is one of the only sources of site-specific data we have. It's -- there are a lot of gaps and limitations associated with it, but we just don't have air sampling data of the type you'd like to have, the -- sampling, breathing zone samples, not until after `86, when the new procedures were put in place.

Plant 4, Green Salt Plant. This is the conversion of -- the reduction of UO3 to UO2 and the production of UNH. Let's see. How do I go through this.

The hydrofluorination bank is really the most important sources of exposure here. And we do have data for those. We actually have another set collected in 1989, in addition to the dust collector sample.

So there's a couple of situations. For Plant 8 and Plant 4 we have data that can be compared in kind of a generalized sense although they are separated in time for about four years, and one is an aggregate dust collection and another is an actual -- okay. Plant 5. This is a very important one here. Plant 1 and Plant 5 together constitute one of our main concerns regarding worker exposure potential.

This is the -- there were two areas here. This is metal production. There was a reduction area where the tetrafluoride was converted -- is reduced down to uranium metal. This process produced large quantities of magnesium fluoride that was commonly referred to as dolomite. This material was then recycled through Plant 1, through the Titan Mill because they could -- that particular mill had the ability to get very fine particle size, consistent particle sizes, and so they used it quite a bit for preparing feeds for the refinery, and also one of the main functions of that was to recycle this slag for refractory liners in the reduction pots.

DR. MAURO: Just a question. This might be important because this is a place where plutonium may end up?

MR. STIVER: This is important not only where it would end up but it concentrates. Every pass-through, about 50 percent of the plutonium and about 80 percent of the neptunium would report into the slag.

And so as you can see, if you keep recycling that through again and again, you are going to be building this material up.

DR. MAURO: And the uranium isn't coming with it?

MR. STIVER: The uranium -- actually one of the tables I have here, I added a column for the percent uranium content for a lot of these samples and that's one of the lowest. We'll get into that.

DR. MAURO: I'm just sort of -

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

DR. MAURO: Foreshadowing, that's

the right word.

MR. STIVER: Yes. The casting area was another area where the -- for high exposure potential. There was a graphite machine shop, where they had basically graphite crucibles and molds which they would -- they had the same type of a process going on here where this material would report into the graphite, and that is reflected in the data that we looked at.

So that all these activities here, basically every operation in this plant was high dust exposure potential, a very, very dirty environment, charging, blending, furnacing, break-out.

In addition to that you had these reduction bomb explosions. This happened on a regular basis, almost on a weekly basis, and when that happened, you know, you basically overwhelmed all the ventilation capacity, dust was just -- practically unbreathable.

And so you had a mixture of, you

know, the uranium, the slag and all these constituents that reported into it.

Onto slide 13. The scrap recovery plant, this is Plant 8. This is where material -- process residues, ashes, other types of scrap were pre-processed into a form that could be fed into the refinery, typically low-assay uranium materials, magnesium slag, filter sump cakes, incinerator ash and so forth.

A lot of chemical processing, the furnacing operations, screening and blending, hand-sorting, all these types of things were going on there, and all of those generate airborne concentrations of dust.

The last line there is the -- the constituents of concern would be, again, almost every one of these you got residues, you got black oxide, uranium metal, all these different components.

And finally the pilot plant. This is kind of a small-scaled production facility where all these different processes were occurring in one particular facility.

Basically you had a small-scale production of the tetrafluoride, production of sweetener, which was an enrichment so they could add it back in to get the proper assay content in the materials that were being produced, all kinds of areas with high dust potential there.

And basically you've got the whole smorgasbord of contaminants. You've got dioxide, trioxide, tetrafluoride, magnesium fluoride.

So that really is kind of the thumbnail sketch of the processes that were going on that could have given rise to worker exposures to this recycled material.

If you go on to slide 15, we are going to switch gears here and start talking about the NIOSH default levels, and this is the infamous table from NIOSH's White Paper, page 15.

Column 2 lists the default levels which are going to be added or assumed for the urine bioassay results, and over here, the last three columns are the presumed amounts of additional activity that would be added in to workers' exposure based on those constituent levels, the idea being that you have this one size fits all, you have kind of a bounding -- what is assumed to be a bounding level of a particular contaminant, which is then added back in to the bioassay data.

And the reason they are doing that of course is because you have got really good bioassay data. You have lots of it, for a long period of time, but you don't have any measurements of these constituents until much, much later, in the late 1980s.

And so you can see, plutonium-239, the default level is 100 parts per billion, neptunium-237, 3,500, technetium-99, 9,000.

And that also included a column for where this is reported in microcuries per kilogram uranium, because a lot of the data, historical data is reported in those units, so just for a quick comparison I put those in there as well.

If we can go on to slide 16. At the last meeting there was a bit of a discussion about, you know, what is really the dosimetric significance of these contaminants.

And various numbers were put out there and so we decided to take a look at that, and we looked at the -- basically -- the ones that are of concern.

Actually we looked at all the constituents that were in the dust data, including thorium, strontium-90, cesium-137 and we really wanted to get an idea, okay, at the highest level or at the default level, you look at the whole range, all the different combinations of solubility class, at those levels, what are the dose ratios going to be compared to uranium?

And the only two that really stand

out as being important -- we have numbers rated in unity. Basically you actually have a does potential higher than you would get from the uranium itself, or for the plutonium and the neptunium.

We kind of knew that, or we didn't really know, we didn't have it quantified, but basic health physics knowledge, you could kind of get a ball park estimate on that.

As you can see here, the numbers outlined in rather the highest values. This was for plutonium class M to uranium class S, and the dose ratio for bone surfaces is about 34 and for liver is about 52.

And so -- this is at 100 parts per billion. So as you can see, this is a significant issue.

DR. MAURO: So, just to -- in simplest terms, if I had a person who is inhaling soluble uranium and I assume there's no plutonium there, but there is.

But I assume there's no plutonium,

and then I say no, no, no, we made a mistake. There's 100 parts per billion of plutonium. My dose to my liver would go up by a factor of 51.

MR. STIVER: It would relative to what you got.

DR. MAURO: In other words I would get a dose from the uranium alone, but now if you add that parts per billion of plutonium, that same dose to the liver, instead of being one -

(Simultaneous speakers.)

DR. MAURO: So I think it's an important message here, is that this is -- these small amounts, parts per billion, sounds like very, very small amount, part per billion, do have very substantial dosimetric implications, especially for plutonium and neptunium.

Now for the others I guess they are not as important --

MR. STIVER: Well yes, the others,

really -- well, you know thorium obviously is high, but the default levels are -- recommending which is 10 to the minus 3. They are very small.

As you can see I believe I put it in here. Thorium, it was class M to U class S, it was only nine to the minus three.

A couple of lines attached at the bottom of the table, yes.

DR. GLOVER: So I would want to point out here though that uranium is being treated as a type S that's being bound in the lungs and we are allowing plutonium to flow out faster. Plutonium is a minor contaminant in a bulk matrix.

I don't know that I have ever seen the bulk matrix hasn't been limited, just like americium. Plutonium limits the solubility of the americium constituent, even though it's type M.

So anyway, we are using a very insoluble material, letting that stand alone like uranium, and letting the plutonium leave more quickly, so it's obviously going to -- if I change solubility classes for the same intake -- so you see why you know, there's biokinetic reasons why we are seeing that here.

MR. STIVER: Yes, oh obviously, and this is put in there just to show that, you know, if you are going to try to be as claimant-favorable as possible you -- a dose reconstructor might go with that particular solubility class, even if it didn't really make sense from the biokinetic standpoint.

MR. ROLFES: As Sam stated, for example, with plutonium exposures, if we have americium-241 growing into a matrix of weapons-grade plutonium, we can't assume Super S for the plutonium -

MR. STIVER: You would have to follow through.

MR. ROLFES: Correct. So you would have to stick with one solubility.

MR. STIVER: And so even -- but if you look at the first column, this is plutonium solubility class S to uranium solubility class S --

MR. ROLFES: Right, there's still an increase.

MR. STIVER: And it's about a factor of three to five higher at 100 parts per billion.

MR. ROLFES: I did briefly see this chart in the report that you had produced but I didn't see exactly how the calculations were done.

MR. STIVER: Actually that is in the report --

MR. ROLFES: It is. Okay.

MR. STIVER: There's a sample calculation right above the table.

MR. ROLFES: I did see that but it didn't really give me -- for example, when we complete a dose reconstruction, I don't know -- did you use a distribution of all the isotopes in natural uranium for example to --

MR. STIVER: Oh yes, yes, I assumed there would be a specific activity of natural uranium. Actually I used several. I've got a MathCAD worksheet that has all the different combinations in that.

MR. ROLFES: I'd like to take a look at that, just because when we complete a dose reconstruction for Fernald, rather than using isotopic distribution and natural uranium, we usually calculate the intake in that manner for interpreting bioassay data, but then when we assign dose, we assume all U-234 --

MR. STIVER: Oh, this is -- these are based on -- this is based on a U-234 --

MR. ROLFES: Okay.

MR. STIVER: So it basically -- the same methods as you guys used.

MR. ROLFES: Okay. If it's based on all U-234.

MR. STIVER: If you like I can

send you the actual workup -

MR. ROLFES: Well, no, no, if it is based on all U-234, then we are okay.

MR. STIVER: Yes, it is.

MR. ROLFES: That's representative. But if --

MR. STIVER: Yes, it's natural uranium 230 intake and then for the dosimetric -

MR. ROLFES: Can I finish please?

MR. STIVER: Go ahead. My apologies.

MR. ROLFES: If it's based on the entire isotopic distribution, then it would result in an elevated ratio compared to what we would do in dose reconstruction. I just wanted to make sure that if you used U-234, then we are okay. It will result in a ratio more representative of what we --

MR. STIVER: And that is indeed what we are doing.

MR. KATZ: Someone or at least one

person on the line hasn't muted on their phone and it doesn't really bother us so much but I am worried that it might be bothering other people trying to listen by phone.

So please, everyone on the phone, mute your phone. Use *6 if you don't have a mute button. But someone is shuffling papers or something and it is pretty audible here, which makes me think it's even worth for other people listening. Thank you.

MEMBER PRESLEY: This is Bob. We have got pretty good service today.

MR. KATZ: Okay. Good.

CHAIRMAN CLAWSON: Bob, have you received these papers yet?

MEMBER PRESLEY: No.

CHAIRMAN CLAWSON: Okay, I'll resend it again.

DR. GLOVER: Bob, I sent those to your CDC account.

MEMBER PRESLEY: I'm at home.

DR. GLOVER: Oh, okay.

MEMBER PRESLEY: I have a slight stomach problem today.

MEMBER SCHOFIELD: Can I ask you a quick question? On those reduction bombs, when they had explosions, were the workers -- did they get nasal swipes, urinalysis after that?

MR. STIVER: In those early years, there's on evidence on the records whether they did or not.

MEMBER SCHOFIELD: Okay.

DR. GLOVER: John, I would probably stipulate that I have done the calculations too and there is a small increase, obviously if you are adding plutonium, and you do see -- don't know if it's quite triple, but I know that there is an increase.

MR. STIVER: Yes, I can send you the calculations.

MR. ROLFES: I did some similar calculations as well and across the board, the recycled uranium intakes make about a two percent difference in the committed effective dose across the board for all organs.

But yes, as you pointed out, there are at least four organs -

(Simultaneous speakers.)

DR. GLOVER: I think we agree there is some --

MR. STIVER: And for any particular organs it could be an issue.

MEMBER ZIEMER: Question, John, in your columns that are in red though, you have different solubilities for the plutonium and the uranium, but you wouldn't use that right?

MR. STIVER: Yes, that was just an illustration.

MEMBER ZIEMER: Just to see.

MR. STIVER: Just to show you that this is the highest you possibly could get with these, even thought it may not make sense from --

MEMBER ZIEMER: Right,

biologically, it doesn't make sense.

MR. ROLFES: If you had a matrix of a combination of radionuclides and you assumed that you know, the uranium in it is all type S, then we would have to assume the entire matrix is type S. We couldn't, you know, selectively part out.

MEMBER ZIEMER: You just put that in for illustration purposes.

DR. MAURO: It's an important point though. So you are saying that really, in physical reality, you never have a type S uranium coupled with a type M plutonium?

MR. STIVER: Yes, you would have a -- an insoluble oxide, you know, with the plutonium in the matrix and it would all behave the same. It would all behave as type S.

CHAIRMAN CLAWSON: But Paul, as you remember, many times we hear well, that's going to -- that little amount is just going -- it is not going to be much of a dose, and I think what was trying to be shown here is that actually, the small parts were --

MEMBER ZIEMER: Oh yes, I'm not debating that, it's going to -- it gets down to an issue of whether there's another order of magnitude here, because --

MR. STIVER: Yes, well that's really the point, is that if you're looking on, forward, as the -- if you start looking --

DR. MAURO: Yes, I think it's important to point -- I mean you are making a very important point here. If it turns out, in physical reality, you were modeling a person and you were assuming that the uranium was type S, you would assume the plutonium was type S also, correct?

And if you were assuming the uranium was M, you may very well assume the uranium is M, or maybe not, I'm not sure.

MR. ROLFES: Correct, if the uranium was type M, we would assume --

DR. MAURO: And that being the

case, then, in terms of bounding the problem that we are dealing with is, yes, you might end up underestimating the dose to particular organs, like the liver, by perhaps a factor five, not a factor of 50.

At the default values. And so if the 100 parts per billion were off, let's say we had, oh yes, maybe it's 200, maybe it should be 300, so we are talking about a factor -- the magnitude of the impact on a dose reconstruction.

MR. STIVER: This really is just to illustrate the magnitude of how --

DR. MAURO: And this -- but it's good that we point this particular point out that you made regarding the reality of one of these scenarios really doesn't --

MR. STIVER: We understood that. I just put that in there just to demonstrate you know, as high as it could possibly get and kind of imagine a plausible scenario.

Okay, if you could go on to this

slide 17 here, I am going to start getting into some of the basis of the defaults and assumptions that were made to kind of justify these default levels.

And the first one, is kind of, there's really two sides to this. This is -- there's an assumption that there's a specific level of plutonium that was -

MEMBER SCHOFIELD: My hearing aid is --

MR. STIVER: I was wondering where that was coming from. There's a bird in your ear.

There's kind of two sides to this. One of this is that the health physics practices during the SEC period were sufficient to maintain worker exposures at levels that would not exceed the default levels.

And the other side to that is that there was a working specification that came out of Hanford, 10 parts per billion, very early on in the process of recycling this material, which was adhered to rather stringently.

So you had those two sides. You have got a low-level specification and then you have health physics practices that are adequate to enforce that, for all workers, for all periods of time.

We'll take a look at the health physics practices first, and this is in -- I also sent you guys all copies of the White Paper. This is on pages 21 to 27. There's a lot of quotes that came out of the DOE task force report in 1985.

And this was really one of the recommendations of that report, based on our findings, was to have a system-wide limit for these constituents in recycled uranium, because it didn't exist before that.

And also, as you can see, the DOE 2000b report and another report in 1989 by Bassett et al.

And so basically one of the findings of the task force report was that prior to 1986, the radiation safety programs at Fernald probably were not effective enough to control exposures and contaminants in RU.

And that was one of our first findings in our report. In 1965, there was a quote that there were on additional precautions for recycled uranium other than the standard.

Twenty years later, 20 years go by, this plutonium out of specification is POOS PTA is received from Paducah in 1980, and we acknowledge this is the primary documented source of plutonium contamination at Fernald.

But the task force observations on the materials were handled however say that there was marginal contamination control, five years after this material -- the original hoppers were -- the first five of them were packaged in the green salt plant, they were finding removable plutonium contamination there.

There was no survey done. There was a recommendation that workers who were actually involved in breaking the stuff up by hand -- they used big poles to allow packaging, wear -- it was recommended that they wear respirators. There was no documentation that it was actually enforced.

And so this is kind of troubling to us. Bioassay program. We talked a little bit about the bioassay program for the POOS workers, and we looked into that.

Actually this is documented pretty well in the Bassett report, and also in the task force report. But this was a program -- when they started processing this material when Westinghouse came on board, 1986, they started processing the stuff in Plant 4 and I guess they had a spill pretty early on, and it shut down everything.

So what they did is they went ahead and ran out what was already in the fluorination banks and during this time they started bioassaying the workers beginning in 1986.

They did this all the way up through 1989 I believe. There's like several hundred workers with a bioassay. And they found -- I think there were 10 or 11 of these guys that came out positive, and so they did an initial analysis using worst case assumptions, like you might do in a dose reconstruction.

And one of the guys, the highest one, was about -- they figured an EDE effective dose equivalent of about 3.5 rem.

And then they did the follow-on samples and those were inconclusive and then they finally sent these guys up to Hanford to get chest counts and those came back negative.

And so there's kind of a disconnect as to whether there were enough samples taken, did you really capture the people who were the most highly exposed.

And this is -- I guess this -- it's kind of troubling that there are no bioassay data for all the people prior to that. By the time this material was received, until the new contractor came on board, you got a six-year period there, and the stuff is on-site. We know it's being processed through the Plant 1 Titan Mill.

So -- but you don't have bioassay for those particular workers. So it's limited, and I guess you could say, in summary the results are somewhat inconclusive.

Let's go on. Slide 18. Let's take a look at this 100 parts per billion. I think we have talked about this a lot the last couple of meetings.

And this is the Hanford working specification of 10 parts per billion uranium. Now, if you look at the data that is reported in the 2000 DOE reports, yes, there's a lot of data, a lot of receipts, or shipments from Hanford that were less than 10 parts per billion.

We also see a lot that's higher. DOE 2000b report shows 4,000 data points in there, and the plutonium levels range over about eight or nine orders of magnitude.

So you have got one set of -- it's trioxide coming out of Hanford. Granted it's a large proportion of what comes in there.

But you also have other sources that are considerably higher, that represent different processes from different plants and different time periods.

And there was a protraction factor of 10 thrown on for claimant favorability so that's where you get your 100 parts per billion from.

Now the task force observations, there are several I listed here. One that was kind of striking was that the only formal limit that was ever adopted by the AEC was in 1971. This was for commercial fuel shipments of GDP and that was 15,000 dpm per gram uranium, which translates to about 110 parts per billion from plutonium and about 9,500 for neptunium.

And they also go along to say that a formal, technically sound, understood and accepted specification for maximum, transuranic and fission products contaminants and uranium recycled materials, has probably never existed either within or between sites.

And this definite guideline for 10 parts per billion did not occur until 1985, and there's a memo, an April 14th memo, or a letter to the FMPC management from DOE imposing that 10 parts per billion guideline.

So you had a working specification, you know, it probably was effective for large volume shipments.

But you don't have a reasonable health physics program, you don't have sampling going on, you don't have air sampling. So there is no way to document whether this was really effective or not.

And I said, the plutonium results that were reported -- and this is not a complete data set by any means -- you have got eight or nine orders of magnitude, you have got all the way up to, I think, the highest value was in the tower ash, was 7,500 parts per billion.

So there's a lot of uncertainty and a lot of variability that wasn't accounted for.

MR. ROLFES: John.

MR. STIVER: Yes.

MR. ROLFES: We certainly recognize that the shipments that were received in the `80s are certainly of much higher contamination levels, and everything we have seen from the recycled uranium report, and the data that we have looked at, indicate everything was typically below, typically two to three parts per billion plutonium.

And during the time period that they controlled things on basically a gentlemen's agreement type, with Hanford, they had the unwritten specification for products to keep it below 10 parts per billion.

And it was the plutonium that was received in the 1980s from the tower ash, the fluorination tower, and that was what encouraged us to bump it up to 100 parts per billion.

And that source term is a different type of source term than the typical recycled uranium, and it really should be handled completely differently than the rest, but you know, basically we could, based on additional data that are available, you could probably go back and justify reducing plutonium recycled uranium contamination levels for the earlier years, and increasing them for the 1980s perhaps.

MR. STIVER: You might argue that you needed to have more uncertainty in the earlier years, even though you -- because you don't have any data to document doesn't mean you don't have a complete data set.

We talked about this last time, about this U Plant from `52 to `58. We don't have any data. It looks like the DOE 2000 report didn't even include it.

And so you have an incomplete data set you are trying to use to justify a value that was based on a performance specification that wasn't even a requirement.

And so that is what kind of worries me. From different angles, you can see there's all kinds of gaps here.

MR. ROLFES: Yes, I understand, I mean, certainly -- there certainly are fewer data in the earlier years than there are in the more recent time periods, but then again, would you look for something that you knew wasn't there?

MR. STIVER: Well, actually in the `70s, they had this -- Bob Alvarez and Jim Werner are going to talk about this a little bit later.

But they had this complete change-out of the CIP/CUP program, and there was a lot of residues and ashes generated from that which were pretty high. They weren't necessarily as high as the tower ash. This material --

MR. ALVAREZ: This is Bob Alvarez. The CIP/CUP program was the multi-billion-dollar program that essentially expanded the installed capacity of the three gaseous diffusion plants by 61 percent.

It went on from 1972, `73 and 1981. It involves the opening of 4,000 20-foot converters, the removal of the barriers, the compressors, the blades, the other equipment, and an enormous amount of D&D work that led to a very large amount of uranium oxide and ash that was shipped during this time period to Fernald, and the -- I kind of think about the POOS material as part of that batch of material.

There is very limited data, hardly

any information about this, but it involved, as I said, when you are talking about 40 -- 4,000 20-foot long converters at the three gaseous diffusion plants that have been accumulating recycled uranium for decades, and then removing that equipment and D&D and then sending the recovered uranium to Fernald without any data indicating what the contaminant content was, it raises some questions.

There is absolutely no reference to the CIP/CUP program for example in the TBD written up for the K-25 Plant. However for Paducah, the -- I'm not sure if it's the TBD or it's the occupational internal dose -- they did mention that the mere opening of one of these converters would yield concentrations on the order of 2,700 parts per billion of plutonium.

So we are -- this -- it was about 55 metric tons of this ash material that came from the D&D of the CIP/CUP program that is just not -- there's no data on it, except the POOS data, which was sort of, I thought, probably part of this, because this was a major clean-out of the three gaseous diffusion plants.

So this was a very unique situation and it kind of has not been subject to much attention.

MR. STIVER: Okay thanks.

MR. ROLFES: Thank you. Is Bryce on the line there?

MEMBER ZIEMER: He was. Bryce?

MR. RICH: Yes, I am.

MR. ROLFES: Bryce, are you aware of this and do you know what -- well I'm not sure, I guess I could ask Bob there what the CIP/CUP program stood for.

MR. ALVAREZ: It stood for the Cascade Improvement/Cascade Upgrade Program.

MR. ROLFES: Okay, thank you. Bryce, do you recall seeing any information on --

(Simultaneous speakers and telephonic interference.)

MR. STIVER: Actually, there's some from Oak Ridge that wasn't accounted for in the mass balance.

MR. RICH: That's K-25 that's accounted for in the mass balance. Well, the data -- the only data that I see that is actually quantifying the ash that came out of these plants during that period is in the question and answer correspondence with National Lead in 1985, where the DOE asked very specifically what types of material went to Fernald, when.

And there's a table in there that lists the different categories of material, which includes U3O8 incinerator ash.

About 21 -- about 22 metric tons came from Oak Ridge. About 42 metric tons came from Paducah. And about 20 metric tons came from Portsmouth I think.

Now, the process involved in doing

this, when they were doing this CIP/CUP program, these are massive -- this is a massive undertaking and so removing the equipment from these converters is no small task, and it involves a greatly expanded crafts and trades working group and going to three shifts for a period of about a decade.

And it involved taking large amounts of contaminated equipment to their D&D facilities at these gaseous diffusion plants, for example Building 1420 at K-25.

And D&D is material using roughly equivalent of something if you can imagine a car wash type operation except they are using dilute nitric acid, citric acid, some fluorine compounds to clean out the barriers and then these wastes would then be gathered and sorted for recovery, and those wastes that would not be sorted for recovery would be measured for transuranics and if they exceeded 10 nanocuries per gram, they had to be stored for future retrieval.

So there was a recognition at the gaseous -- at least Paducah -- that the laboratory, that one of their procedures involved in terms of measuring the decontaminated material, the material that was removed from the contaminated equipment, they weren't measuring transuranics for purposes of retrievable disposal of TRU waste, and so -- but I saw nothing about how much would wind up in the ash and what measurements were taken.

But this material I think -- I think the POOS material has to be considered in the larger context of the cascade improvement cascade update program.

And the POOS material includes an additional, I don't remember the number there, but an additional 19 or 20 metric tons above and beyond this ash material that was sent.

MR. ROLFES: This is Mark again. Thank you Bob. Bryce, I am looking at -- I don't know if you received the presentation that I forwarded to you from John Stiver.

I was looking on page 20 here, and we have got a subgroup, subgroup 9 under recycled uranium summary values by process subgroup, and we have indicated there was a receipt of Fernald of incinerator ash and scrap residues from the gaseous diffusion plants, and this is one of the elevated plutonium shipments.

Was this -- do you happen to know, might this have been the result of the CIP/CUP program that Bob is referring to, or --

MR. RICH: I don't know for sure.

MR. ROLFES: Okay.

MR. ALVAREZ: Well, the CIP/CUP programs were written up in different -- DOE had different reports, and there was a independent investigation done of Portsmouth in the year 2000 where they mentioned -- they didn't quantify, but they simply said that transuranic contamination from the residuals involved in this program were significant, and that the workers were not, at that place, being offered adequate protection.

The Paducah -- I am not sure if it's the TBD or it's the internal dose section, I need to go back and look at that, let me pull up the memo here.

It's the site description, it's in the TBD. Basically the Paducah TBD mentions that workers involved in the CIP/CUP program, we counted residual amounts uranium were estimated to have plutonium levels ranging as high as 2,740 parts per billion.

MR. STIVER: Hey Bob, I think I have a table F.51A from DOE 2000b, lists all the constituents, and that Bryce Richards writes there is incinerator ash for K-25 and also for Paducah, but there's nothing there for Portsmouth.

And this was -- these are the values that were assigned for the process and all its determinations to the subgroups, and then --

MR. ALVAREZ: The data that I am

referring to comes from a set of information that is on the O: drive that is essentially a package of correspondence dated 1985/86, and in the midst of that package is a letter by the manager of National Lead to Jim Reafsnyder, who was the DOE manager of Fernald at the time, answering a set of questions.

And in that attachment to that letter, is a series of graphs and tables and one of those graphs and tables, they provide you a break-out of the types and forms of RU, of recycled uranium, that were shipped to Fernald and there is a set of tables, two or three pages of tables, I don't recall, that sets forth the amount of what's called U308 incinerator ash that came from the three gaseous diffusion plants.

And if you look at the table and also transpose over that the time period of the CIP/CUP program, you see that the major preponderance of the ash and U308, which is probably a product of either calcination or some sort of incineration, that was shipped, came during this CIP/CUP program.

DR. MAURO: Could I -- in terms of when I am listening, and I think about the history of our discussions, I remember the tower ash at a very troubling part of our discussion and it was well-contained.

It was my understanding when I went back to our original discussions that yes, everyone agreed that the tower ash was a very specific issue with Paducah, and that it was as high as 7,000 parts per billion, but it was well-defined, well-controlled and when it showed up at Paducah, it was something -- I'm sorry. When it showed up at Fernald, it was something that was handled in a manner that minimized the potential for people to actually experience any exposure: they had respiratory protection, I guess it was confined.

What I am hearing now is that there is this other category of material called the CIP/CUP, which was another source, I mean, I'm -- I'm really trying to step far back and say another source that also went to Fernald at a different time and also had very high ratios of plutonium.

MR. WERNER: I think it was part of the CIP/CUP program and they were cleaning out the converters, you know, 4,000 converters.

I think that one gaseous diffusion plant, I think Paducah had 1,600 converters, so they were doing major renovation and clean-out and replacement of equipment, which involved an enormous -- or let's say unprecedented D&D program at their D&D yard, in an effort to recover uranium and to segregate out that uranium, which would be discarded.

MR. STIVER: Hey Jim, could I jump in for a minute? This is John Stiver. You know, we went into this in the last meeting quite a bit, into this CIP/CUP program.

I think it does illustrate that

there may be certain amounts of this incinerator ash that may have been missed in the DOE 2000 statistical analysis.

I am looking at the values that were reported, and once again these are the mean values, these aren't the entire range. I would kind of -- in response to what John just said, we did -- I think we laid out pretty well that there were definitely some serious gaps in the radiation protection program during the entire time of, even the Paducah tower ash was being processed, there may have been certain situations where they claim that in-line respirators were used -- maybe they were, maybe they weren't -- for certain categories of processed workers.

So this is a separate source. It's group 10a in the analysis.

DR. MAURO: I need a touchstone in these conversations, so one of my touchstones at one time was that there was a great deal of control over the tower ash, so there were two things that happened here. Maybe that control wasn't there the way we like to think that it was, and in addition, beside that, there's a this other source that may have come in around the same time that has a different name, but also had levels that were very high that may not have been a counterpart.

MR. STIVER: And actually I can tell you that those lows in the statistical analysis, they range from a minimum of about 0.6 up to 3,500 parts per billion.

DR. MAURO: So these things challenge the 100 parts per billion number --

MR. STIVER: And we are going to get into that.

DR. MAURO: We are going to get into that -- in a way I like the idea of foreshadowing, as we are talking about it, remember, this is why this is important and its relevance to our previous conversations, so those -- sort of anchors me as we talk these things through.

MR. STIVER: Yes, this is a very important source that Bob brought up and it has been addressed in the statistical analysis in DOE 2000b, but we are going to get into the inadequacies of that particular report and some of the uncertainties involving that.

CHAIRMAN CLAWSON: I am wondering if before we talk to that, if we could take a 10-minute comfort break, if we could. Those on the phone, we are going to -- we are going to take a 10-minute break and we will come back then.

MR. STIVER: I think everybody could use one.

MR. KATZ: What time do you have right now Brad?

CHAIRMAN CLAWSON: I have got 10:43.

MR. KATZ: Okay, so about five to the hour we will get started again. I am going to put the phone on mute.

(Whereupon, the above-entitled matter went off the record at 10:42 a.m. and resumed at 10:57 a.m.)

MR. KATZ: Okay, we just took a short break. This is the Fernald Work Group and we are ready to go on. Bob, do we have you on the line?

MEMBER PRESLEY: I am.

MR. KATZ: Great, thank you. And for the record, Mark Griffon is not joining our group, and let me just ask to check your emails please, if you are on the line. Thank you.

MR. STIVER: Okay, shall we jump back in?

MR. KATZ: Yes.

MR. STIVER: This is John Stiver. I want to continue the presentation. Slide 19, about half way down that slide is the crux of this, is that for the default radionuclides other than plutonium and NIOSH relied on the DOE 2000b report and the statistical analysis that was produced from that, to arrive at the 3,500 and 9,000 parts per billion for neptunium and technetium, and also to validate the default values for plutonium.

And it was done because they are just -- as of 1985, as a matter of fact, there was no --

(Simultaneous speakers.)

So anyway, let's go ahead and start segueing into this DOE 2000b report and Jim Werner is going to take over in a minute here, but let me just lay kind of a framework for you.

This was produced under the Clinton administration towards the end, under the Secretary Richardson, his direction.

It was basically an incredible program in terms of the amount -- the intensity. The whole thing lasted nine months start to finish.

Four sites -- Fernald, RMI, West Valley and Weldon Spring reviewed and assembled all of their RU data and basically resulted about 4,000 points. Fernald had about three-fourths of those and is really kind of the repository for all this information that was available.

And it then took all this data set and he had an assemblage of experts, process experts, who had been involved in RU protection over a period of years.

And these people basically assigned this data into different subgroups, process subgroups that defined certain types of materials and certain processes, and they came up with a total of 19 of them.

Then they did -- performed a statistical analysis which was then reported in Appendix F of DOE 2000b. And table F.31 is the basis for table 5 of the NIOSH RU report and that's on page 20, or slide 20.

And this itemizes all the subgroups and then gives what is called the bootstrap mean and it's very similar to an arithmetic average that controls for the influence of outlier data points.

And then these are the values that were then used by the process knowledge team to assign to different sites for different sources of data.

And that particular information, for those of you who are interested and have access to the O: drive, I sent out an email yesterday or the day before giving you directions to the references, and so this particular table, F.51A, is there.

And this shows how those various bootstrap means for different processes were then applied to different facilities, different shipping sites, throughout the entire complex.

And you see a lot of the same values repeated again and again, because these are basically process knowledge determinations and assignments.

So let's go back to the slides

again. If we move on to slide 21. This is about the basis for the NIOSH defaults, a continuation of it.

What you see when you look at table F.31, even a cursory review of that table shows the enormous amount of variability in the level of the constituents.

And this just really indicates that you have got all these different processes moving over time, different feed materials, a tweaking process, the processes were changed and improved over this period. So it's not surprising that there is so much variability.

One of our findings was we questioned how the values of 3,500 and 9,000 came out of that data set. I think it's not really all that important exactly how it was derived.

I assume it was some upper quantile of the distribution of values, is that basically how it was done Mark?

MR. ROLFES: Bryce, could you answer the question on how we developed the default ratios of 100 parts per billion and 3,500 parts?

MR. RICH: It's a function of the upper limits of the distribution.

MR. STIVER: Okay, that's all right, it was just some operating portion of the distribution. And we tried to replicate that. We came close but we didn't quite get to those values.

So in summary, the DOE 2000b, we have laid out our position. It's there in the transcripts. It's in our White Paper, and basically our position has not changed.

We feel that there's still a lot of outstanding issues and on slide 22, those are kind of summarized here, in four bullet-points.

First of all, the DOE analysis was accepted without question. There was no uncertainty analysis performed to verify the estimates that those bootstraps were actually bounding.

We questioned some of the assumptions that were made in that analysis, one of them being the partitioning of plutonium, how it was partitioned.

One example we list here is that initially they thought about 80 percent was going to report into the raffinate but it turned out that only 15 percent did for one particular process.

But more importantly there has just been no independent analysis of the data for suitability in dose reconstruction, in particular for an SEC petition.

And this gets back to the whole surrogate data issue. You are taking data that may or may not apply to a particular site or a particular process and it is being assigned and so we know there is this enormous amount of variation in the actual data that were reported, and that doesn't even include the uncertainty that goes into the process and all its determinations.

And so we have some issues regarding the statistical analysis and how that justification was performed.

I put a couple of quotes here from DOE 2000b, at the bottom of this slide. They even acknowledge -- you will see that throughout that document, they caveat it continuously.

One is that they stated the small number of values represent approximately 40 years of Fernald shipments, receipts and productions, and also represent other DOE site recycled uranium receipts.

FMPC data from the middle through the late 1980s, when back-extrapolation was possible, the limits of it -- the applicability must be understood.

So that they are telling you that this data set is limited, it can be used for various purposes, dose reconstruction, maybe not, but they were very much aware of the limitations of that study.

On to 23, finding 11. This one had to do with the statistical analysis, and when you look at the distributions of data, I know the DOE people said they didn't feel that that data showed a log-normal distribution, so they used this other methodology.

Well, our own statistician here, Harry Chmelynski, did his analysis on it and found that in fact, most of the data sets, or at least several of them can be more represented by log-normal distributions.

Those are laid out in the report, pages 35 to 37, all the details are there. We just feel that when it essentially amounts to an arithmetic average of a very large, uncertain and variable data set, it's just not claimant favorable for dose reconstruction.

You see here at the bottom we have got the bootstrap mean analysis Harry went through, and even just from this analysis, you look at the GSDs for plutonium, they range from 8 to 16, neptunium 4 to 10, technetium all the way up to 20, and if you look at the log-normal plots in the appendix, the data seem to fit that little bit better than a standard arithmetic analysis.

Now at this point, Jim Werner, are you still out there?

MR. WERNER: Yes I am.

MR. STIVER: Okay Jim, I'm going to go ahead and bring out your presentation. I'm just going to introduce Jim. He participated in the last meeting.

Jim is an SC&A associate. He was formerly employed by the EM office for the DOE and he was involved in managing the production of this DOE 2000 report.

He did work as an engineering contractor at gaseous diffusion plants from `86 to `89, conducting environmental surveys, and the Linking Legacies 1997 report by DOE was prepared through Mr. Warner's office under his direction.

And at this point I would like to go ahead and turn it over to Jim. Let me bring up your -- everybody here in the room, if you go to the other PowerPoint presentation entitled RU overview, Jim Werner final.

MR. WERNER: Okay thanks John. While you are bringing that up, let me try introducing it and maybe a sound check at the same time, to make sure you can hear me okay.

As John suggested, I am going to describe a little bit of the background and limitations of the DOE 2000b report, and I think the basic question to ask and the reason why it's useful perhaps to examine carefully this report, is to try to determine whether or not the data being used is really representative of the breadth of recycled uranium that was used over the years.

And there's a number of slides but they basically fall into three categories and to me the biggest background that I have in working in this area is in reprocessing, that is the operation to extract certain isotopes, particularly plutonium from spent fuel and irradiated targets.

And so I think it is important to make sure we look carefully at what reprocessing was in all of its variations, to understand recycled uranium. That's really where recycled uranium came from, of course, so to understand then recycled uranium you have to understand reprocessing.

And then secondly there's a little bit of background on the report itself and its production, and lastly the -- I think an important issue of what are the other issues that should have been examined in more detail to ensure that the data was representative.

So with that, let me get started on a little bit of background here, and some of you already know this in some detail so I am going to go quickly and get on to the other issues.

Reprocessing, again, is really the essential process in linking the production of plutonium that occurs when you bombard a target uranium-238 with neutrons and produce plutonium.

Plutonium isn't really available until you purify it and extract it, and that reprocessing, as it is known, chemical separation, was really a very large industrial operation particularly at Hanford River and furthermore specialized in Idaho National Engineering Laboratory where they had really interesting capabilities, and of course at West Valley, where an attempt at commercial reprocessing was made for a number of years.

And I wish I had a little model here to show you, but if you imagine concentric rings with a target ring on the outside of 238, and that's the target because that's where the neutrons were aimed at, essentially, inside, with this driver fuel typically high-enriched uranium.

And this is a set-up that is quite different from naval reactor fuel. It's quite different from commercial fuel and the idea wasn't just to generate heat and steam, it was to irradiate the targets to produce plutonium, and that then the extraction process was really what we were talking about here.

But I just also wanted to note that it wasn't only plutonium-239 that was produced in some cases, and this is a big issue. I'll put a little commercial interlude here that the nation really faces a big issue now with that 238, because we produced it at one time, we stopped, we then bought it from the Russians, but the -- and I'll get to this maybe at the end --- is the radioelectric - thermoelectric generators, the RTGs used for like the Apollo space missions and certain deep space missions, used 238.

So, it wasn't all 239 in other words, but most of it was 239. And let me go on to slide number three, where it says uranium refining.

And the important thing is not all of the details comprehensively -- I'm not going to go through every single box on this -- but there's essentially a contrast between this slide and the next one I am going to show you.

You'll see in this uranium refining slide that -- you'll see all these very familiar operations occurring within Fernald, and at the very top, the feed material is characterized as uranium ore and concentrate production residues.

Okay, and this came from the document that was done in the very early `90s, before `93 and its genesis was actually in the mid- to late-`80s, characterizing Fernald operations before the real shutdown in `89.

And this document came out to characterize it and you will see there is no mention of recycled uranium in this.

And the next slide, if you could

just go to slide number four, this is actually a page from Linking Legacies, and this is more of a complex-wide diagram.

It shows the operations in most cases flowing from facility to facility, so you have the radiation and separations at Hanford and Savannah River, and then shipment of the material to a fuel or target fabrication facility, like Fernald, for uranium refining.

But I am not going to make you do an eye test here and look in detail, so I blew it up on slide five. In slide five you will see that the little arrow coming out of chemical separation saying recycled HEU/LEU/NU for high-enriched, low-enriched and natural uranium, to refining.

So far as I know this is the first time this appears and at the time, it was simply a matter of accuracy, because I had been aware that the previous flow diagrams lacked this recycled uranium and it was something that I became aware of pretty acutely working at the gaseous diffusion plant as a contractor on the EH surveys during the 1980s.

And I am an engineer, not a health physicist, and that's my excuse for not being really as aware of the health physics implications.

But I was very much aware of the implications of the recycled at the gaseous diffusion plants in terms of the impact it had on operations, and mostly the production of waste and contamination, particularly in the CIP/CUP program, and Bob Alvarez discussed some before.

It really had a big impact and the employees of the facility where we were all working out there, I was an outside contractor, I was visiting the various facilities for maybe two weeks or a month at a time and spending six months reviewing documents, producing the reports.

But the full-time employees were very acutely aware of the impact recycled uranium had on the separative work unit efficiency at each of the gaseous diffusion plants, and of course one of the big impacts was, at each of the GDPs, when it went back through for enrichment, it had an impact on the barrier in the diffusion tubing, which is, as far as I know, still a classified technology in the details, but essentially, as you probably know from the gaseous diffusion, that that barrier tubing in each of the converters, is very much engineered to allow the flow-through of the UF6, uranium hexafluoride and allow the enrichment process to occur.

So introduction of the contaminant, the fission products, plutonium, had a big impact on the barrier tubing and reduced the efficiency, so when Bob says that going through CIP/CUP improved efficiency by 61 percent, it was in part an improvement caused by the reduction from the introduction of recycled uranium.

So the point is here the flow diagrams weren't widely known publicly. Perhaps other people knew the implications. Perhaps it was simply an omission.

For whatever reason, we made a point of bringing that out and producing Linking Legacies, that finally came out in `97. It took seven years to produce that document. So it was a lot of work and analysis to go behind it.

The next slide is just one of the overall flow charts. I didn't go through all the flow charts. These are in Linking Legacies and they are reproduced in DOE 2000b. I made sure to put them in to explain the process overall.

This is just the MED process from `42 to `46, and you will see there is no recycled uranium flow-through. If you look at the chemical separation in the upper-right corner, where you have the Oak Ridge National Laboratory X-10 Semi Works, the huge Hanford U and T Plants, those are just the very primitive reprocessing plants operated during the Manhattan Project in the rush to produce materials in World War Two.

And there was, again, no recovery of the uranium because the focus was on the plutonium. That was what we were trying to get then.

If you flip to the next slide, AEC from `46 to the mid-`50s, again this is from Linking Legacies. You see that big loop at the top. Essentially, the recycle would flow through uranium out of the U plant, the U03.

This is before the big construction expansion in the `50s when it was just Hanford doing the chemical separations on an industrial scale at that time.

But that's when the recycled uranium of course started. Somebody realized hey, this is valuable uranium we are sending to the waste tanks, and as I think Bob wrote in some detail, there was an attempt to recover some of the uranium, some of the recycled uranium actually came from the tanks at Hanford.

And the flow charts, then for the `50s through the `70s and `80s are somewhat similar but you obviously have the expansion with the addition of the F and H canyons and the chem plants and West Valley later on.

So it would just be more complicated but the same idea as you will see.

The next slide is simply to illustrate the first of what are many reprocessing facilities, not a huge number but there were more reprocessing facilities than there were enrichment facilities, and this is just the T-Plant at Hanford, and this is just the very large-scale reprocessing, what we called canyons of the Queen Mary buildings, they are sort of shaped like an ocean liner so that's why we called them the Queen Mary buildings.

And they are very intensive operations and very important in terms of improving the efficiency of plutonium extraction.

If you think about, you know, the earliest days when Glenn Seaborg did the first micro-extractions of plutonium, you know, we were just then beginning to learn about the basic chemical engineering, how do you extract plutonium from fission products, how do you separate from the uranium target material.

And that process of improving efficiency went on continuously and frankly, it was a competition, particularly in the `50s, between Hanford and Savannah River to see who could do it more efficiently.

And I don't mean to say Idaho wasn't in that competition. They certainly were. But Idaho had some very unique capabilities, and we can get into that a bit more, but in terms of large-scale industrial reprocessing from plutonium extraction, these facilities were constantly adapting their processes and figuring out how they could be more efficient later on.

And of course the same scale of facilities was built at the Savannah River site, particularly the S and the H canyons, but at Hanford you had the U-Plant, the T-Plant, PUREX, and later on the, the Plutonium Finishing Plant to add some even more hi-tech capabilities if you will.

And frankly some of the PFP capabilities were trying to I think learn some of the very exquisite lessons they learned at Idaho about how to do better extraction, and Idaho had a lot of unique capabilities.

The next slide is just the inside of the canyon and you will note the scale is different from the outside only because the walls are about eight feet thick with lead impregnated concrete.

And once you get a canyon working

you really can't change it very easily because you can't just send a worker in there. It's a very highly-intensive radioactive atmosphere.

And so there's a lot of documentation about all of the process changes that occurred. When you make one of those process changes, you spend a lot of time writing about it and getting it okayed but nonetheless they continued changing how you do the processes, what solvents were used, what equipment was put in there, the temperatures, the pressures, the addition of different catalysts, all to improve your extraction efficiency.

The next slide, and Brad, I guess it's here for your benefit, because you will see Idaho, it's somewhat smaller but again has unique capabilities, as you know very well.

And of course one of the main differences there is the mission was to extract high-enriched uranium particularly from naval reactors fuel, which is fundamentally different in design.

And at the risk of extending this discussion, I'll just say that to truly get into all the variability about recycled uranium, and the variability of reprocessing, one perhaps should really go back to the engineering of the target material, the fuel and the targets themselves.

And particularly, you will note that at Idaho, one of their big missions was naval reactor fuel, which is engineered very differently, and beyond that we really probably can't say more because it is still very classified.

But suffices to say that it required Idaho to step up its game to do that, I mean you have got a different level of criticality, different health physics issues, but just different engineering issues.

For example they used -- they operate generally at somewhat higher pressures. They use hydrofluoric acid instead of nitric acid. So a lot more difficulty, a lot higher level of difficulty, so what they may have lacked in size, they made up for in the level of difficulty.

Of course a lot of that HEU went back to Y-12 at Oak Ridge for driver fuel, but some of the material, because they did a variety of materials.

The Fort St. Vrain fuel of course, there's the unique sodium-cooled reactor in Colorado went there, you know, any time you had a difficult fuel that was somewhat unique, you know, you would send it to Idaho, just because they have capacity and flexibility to do some difficult things there.

So again, adding to the variability, that's the picture that I'm trying to paint to you, that recycled uranium wasn't just one constant source. It was a variety of facilities, a variety of engineering operations, a variety of production processes and constantly changing over time.

So even if you took a snapshot of say a half dozen facilities one year, two years later you would have somewhat different operations in the canyons there.

And then just the outside of the chem plant, you see it's a smaller building compared to one of the big Queen Mary canyon buildings.

And then the last one is the inside picture of the PFP, the Plutonium Finishing Plant at Hanford, and on the walls you will see there what was called the pencil tanks and that just shows you some of the technology that was implemented later, that even although they produced super grade and that at the Savannah River, and the Savannah River folks are very proud of their super grade, a very high level of purity of 239 compared to some of the other isotopes of 239, and that was, I think, you know, they succeeded in integrating their target engineering with their reprocessing there, at the PFP gave you a capacity of doing even more extraction of plutonium.

So I would respectfully disagree with somebody who said earlier that the concentrations of plutonium in general were higher in the `80s than they were in the earlier years.

And I don't doubt that there may be evidence for that, but I would suggest that frankly the overwhelming trend was for lower and lower plutonium concentrations in the residuals in the raffinate side streams compared to your plutonium production, just because we got better at isolating plutonium, we got better at purifying it.

And that was just for the point of illustrating with the PFP and this was a particularly challenging facility to manage.

And just lastly, the goal was, you know, what you end up with a puck, there's your basic puck of 239, it ended up getting engineered into a spheroid for a primary and a thermonuclear warhead.

And then there's the last photo, is just the RTG from the Apollo mission on the moon because it was the Atomic Energy Commission, later DOE, who used neptunium-237 targets that were irradiated to produce 238.

And you know, we didn't produce a large quantity of this material, but we ended up using the same facility. So you know, you didn't necessarily flesh out everything. You got your main stream of 238 from processing your neptunium-237 target material at each of these facilities.

And we continued doing it even after Apollo for a variety of other missions and then we stopped, again something that we are going to continue exploring in space during these missions. There's really not a good alternative except for 238.

But the point is not to advertize again for deep space mission budgets, but to again note the variety of operations at the facilities.

This is, I'm sorry, last couple of photos are both at Portsmouth. This is one of the yards where uranium hexafluoride, in this case tailing cylinders were stored, and this guy is just doing a basic sonogram inspection of the cylinders to make sure that they were sound and not leaking.

And it was one of our concerns that the folks who were working out there day after day doing the inspections, there were certain assumptions about their exposures and if it was just alpha emitters, that was one issue, but these were the waste crews who were out there inspecting tail cylinders and there was a need to examine better what they were being exposed to.

And the last one is just an aerial view of the, in this case the K-25 Gaseous Diffusion Plant, the Oak Ridge Gaseous Diffusion Plant.

And what you see is just the three big enrichment buildings in the background, the big K-25 U and the K-30 and K-33 buildings across the creek.

And they are certainly large, remarkable buildings. The K-25 U for example is one mile if you go from one end of the U to the other, so you know, we rode bicycles around when we worked there. You know, if you wanted to go to lunch, you had to ride a bike just to get some place.

But the point here is all those other buildings around it, there was an enormous amount of support work that went on constantly, and I think there was only one building added since the CIP/CUP program, and as Bob alluded to, this was a very big deal.

In removing the compressors and the other equipment in there, they were roughly the size of a boxcar, each one of them, about 4,000 had to be cleaned out.

And you were lifting them with

these overhead cranes, they needed to have overhead cranes, putting them on railcars, shipping them over to the different buildings that -- at Oak Ridge you had 1420 and you know, different, very large support buildings.

The support buildings were nearly as large as the facilities doing the enrichment itself, just for the cleanup, particularly the CIP/CUP program was a massive undertaking, where you had to dip these pieces of equipment in large tanks of nitric or chromic acid and TCE, and it was sent then to disposal.

One of the things that I always thought would have been a good idea to do, if you had more time, is to go at the daily log data about what was discharged, because some of those tanks, where they dipped the equipment in to clean them out, some of that was just discharged to a ditch out back and of course that was our environmental concern in our investigation.

But there was an improved ability to do analysis of that and that might have been one source of data had we had more time to analyze it. But it would take a pretty big effort, and I can't guarantee there would be something there at the end if you did the analysis of all the waste coming from the CIP/CIP program.

But again, that was just one of the suggestions I made in entering into this.

And so let me just talk about the production of the report itself on slide 18. Overall, I appreciate John being complimentary, because it would be sort of a gift horse for me to say it myself.

Because I think that despite the limitations, it really was a massive and extraordinary effort and I think, again despite its shortcomings, it was pretty impressive that so much material was put together.

It was a pretty big team effort,

people were working at the top of their game under a lot of pressure. And the kick-off really was around Labor Day in 1999.

But I think it's important to understand some of the background of it. Remember, if you go back to Labor Day 1999, and you may -- everybody here may have had something else going on in their lives, but for many of us this was a pretty intense period.

You recall that the Wen Ho Lee scandal was going on, the New York Times burst that out in March of 1999, the first polygraph, and the security concerns were raised in December of `98, and then after the New York Times burst it in March of `99 to be actual -- there was just a lot of pressure about what was going on with DOE, and who was in the headlines with security concerns in the Secretary's office, and just a real concern that why was DOE in the newspaper.

We had to just get DOE out of the

newspapers day to day, you know, all the way up to the indictment, and it didn't do anything to stop the indictment of course, which was December of `99.

And you know, while all that was happening and people were working to try to get DOE out of the newspapers day to day, you had the whole issue of recycled uranium came up, initially with the qui tam lawsuit.

Qui tam refers to somebody acting on behalf of protecting the interests of the government, from some ancient Latin legal phrase, in this case from a false claims lawsuit that was led by the Natural Resources Defense Council in cooperation with some of the labor unions at the time.

And the central assertion was that there was new information made public by DOE that indicated that the use of recycled uranium had been known previously but not reported to DOE by the contractor, and so whatever bonuses the contractor had received had been based on lack of full disclosure, and the allegation of a false claim that a contractor made a false claim and received some benefit based on information that was later known publicly to be incorrect.

And so the initiation of that false claims lawsuit, the qui tam suit, actually occurred when they actually came to visit me and delivered the initial documentation.

There is apparently some legal requirement that you notify your target in advance, like an advanced notice of intent to sue. In this case you have to deliver the documentation to your target and to the relaters, as it is know, that's the equivalent of a plaintiff in a lawsuit, the relaters came and brought that to me because they knew I had been involved in recycling uranium and making public that information and you know, for whatever reason I was just a convenient person to deliver that information to, to inform the

Department officially of the intent of the relaters to bring this suit.

But of course, that was just the beginning, you know, it really went into the legal department mostly after that initially. But there was a lot of media scrutiny.

The Washington Post, as I think I list on the next page, l9, had two front-page articles about the whole issue of recycled uranium.

And of course, recycled uranium is sort of a complicated issue to explain in the public so they mentioned that I think only in passing in the articles, but essentially that there was more exposure than previously known, and I think that was the basic message to it.

But there were congressional hearings that went on in September. I just mention a couple of them there just because O&I and government affairs were doing some pretty heavy -- O&I is Oversight and Investigation -- and governmental affairs, it had been previously chaired by Senator John Glenn, for whom Bob worked as an investigator for a while before he joined DOE himself.

But there was just an enormous amount of other types of pressure going on at the time, just constant letters, inquiries.

Even hearings that were not about recycled uranium -- if you went up to the Hill to do a briefing on your budget, very quickly the question started to turn around to what about recycled uranium, what about this lawsuit, you know, how big a deal is it, and most importantly for the appropriations committee, how much money is it going to cost us.

And that is really how 2000b came about, is trying to answer the question, well, how big a deal is this if there is a worker claim bill, as we were then proposing, and that is what eventually came to be enacted, as the Energy Employees Occupational Injury Compensation Program Act. We just call it the worker comp bill.

If that bill was going to go to through, people didn't want to enact a bill without knowing what it was going to cost.

Well, one side wanted to know what it was going to cost, they were concerned about high cost, and another was simply concerned with, is this legitimate, is there any basis to this, or is this a spurious allegation.

So DOE 2000b was -- the intent was to just answer those two questions initially, and if we succeeded in answering those two questions, that would be a successful report.

And we had to constantly reminder ourselves, people working on it, that as much research as we went into to detail all the technical details, and I do think there's a remarkable amount of material given the short time period, it wasn't necessarily intended to be the last word, it was in a way the first word on it, to address those two questions.

Again, number one, was there any legitimate technical basis to allege that there was the use of recycled uranium, and number two, how many facilities and consequently how many workers were affected to get kind of a ballpark estimate of the cost if you were to enact a worker comp bill.

I should mention one of the other pressures, and this may seem trivial, but it wasn't at the time. We were still trying to absorb a pretty major reorganization.

As you know, DOE's organization was, going back to the Atomic Energy Commission, was done by operations offices, and so you think of Hanford as a site or Savannah River as a site, and each of those had an operations office, Ridgeland or Savannah River.

But Oak Ridge operations office wasn't just Oak Ridge, Tennessee. Oak Ridge operations office had a functional responsibility, largely for what we called the secondaries. You know, in a nuclear warhead you have got a primary and secondary, and the secondary was mainly a uranium component.

So they were responsible for all the uranium facilities, so that included all the GDPs, Portsmouth, Paducah, Oak Ridge, you know, K-25, but also Fernald, Weldon Spring, all those facilities were managed out of Oak Ridge.

And there was somewhat of a geographic proximity but it was mainly a functional organization. It was one place where you had all the expertise for managing and processing uranium.

Well that changed, officially, in 1994, but the changes were continuing right up until the late `90s, because you had, you know, 50 years or so of tradition and people and employees and contractors working for Oak Ridge operations in Tennessee, where the folks at Fernald reported to.

And what we did is we created a

new field office called the Ohio field office, and the mission of Ohio field office of course was clean-up, environmental management and not doing uranium production operations any longer.

That meant the type of personnel and the type of contracts and management you had, were, instead of operations processing experts who -- some of them retired or were reassigned -- we had project managers overseeing clean-up contractors and clean-up experts.

And the reason this is relevant is because that changeover that happened in the mid-`90s, that was still going into the late `90s, I think it was somewhat of a problem trying to get those process experts working.

At Hanford it was a simple matter. You'd go down the hallway, the same guy was there at Ridgeland, or at Savannah River or at some of these other facilities.

In the case of Oak Ridge, they

were no longer involved or no longer even there. In some cases we brought them back and people were interviewed, wherever, you know, brought in for interviews and got the information.

But again, it was in the midst of this massive reorganization of -- going from an organization at Oak Ridge that had been there for 50 years to a whole new set of people.

New people were hired and the old people who had been responsible for Fernald were simply not there anymore. You were trying to do a report through a new organizational structure and that is the way these reports are done.

People at headquarters don't do the research, which probably doesn't surprise anybody that people in Washington don't do a lot of work.

But headquarters' job is to set up the structure, oversee it, get the funding mainly, you create templates, you organize people.

But it was the people out in the field offices who did all the heavy lifting and in the case of Fernald, we didn't have the same people doing that heavy lifting who had been there.

So I don't want to dwell on that too much, but I think that was an important context of what was going on at the time. It just was an extra little barrier going on.

Okay, I mentioned the congressional hearings and the lawsuit and the lawsuit was found to be at least valid enough for the Justice Department to get involved in, and I am not even sure what the final outcome was.

But it's not really significant here. The important thing was that the lawsuit was going on at the time and Lockheed Martin was one of the targets and so it just, you know, it caused everybody to be very careful about what information you provided to whom to make sure there wasn't any conflict of interest going on.

It wasn't a huge deal, but you know, the main thing is just added pressure. People kept asking about what's the status, DOE is being sued, or the contractors are being sued here.

But going back to the variety of reprocessing operations that we had to examine and obtain data from, just at Hanford you have got the multiple plants going on up there and as John said earlier,
80 percent of the Pu was said to have come from Hanford, and that is not surprising given the long history and the multiple large-scale plants there.

Savannah River, you had to look at both F and H canyons, who had different missions, you know, F specialized in plutonium extraction. H did a variety of things, but mainly did some of the HEU recovery.

West Valley was probably the smallest in terms of total throughput, but West Valley may have been the largest in terms of diversity of target.

They took a lot of -- of course the main goal was to try to demonstrate commercial reprocessing, which failed for economic reasons.

But it also took some of the material from Hanford and then we used some of the plutonium and in fact some of the reactor grade plutonium that was extracted there was used for a test out at Nevada Test Site to demonstrate whether or not you could actually make an operational warhead out of reactor grade plutonium.

And that was one of the diversity of things done at West Valley. The answer by the way was yes, because that doesn't have a very good yield.

And then the next slide, 24, if you think about this in terms of at least a two-sided matrix and permutation, each of these facilities was constantly tweaking their processes, and I am not going to get into all the details, but suffice it to say that people were changing the solvents they used, the organic phase compared to aqueous phase.

You had different additives being used, again HF was used more commonly at the Chem Plant in Idaho, but rarely used elsewhere, and that had very different impacts on the efficiency of extraction that occurred.

The technologies changed. We to this day I think try to keep classified some of the geometries of the later slab tanks that were developed for non-proliferation reasons, but suffices to say that the geometries were important and they were improved greatly over the years.

The geometry of a slab tank or an extraction column from the 1950s and `60s and `70s was much different, significantly different from what we later developed during the mid-`70s and `80s to really improve the efficiency.

I mean, we would have really never had the super grade material we got out of Savannah River except for the integration of the target and the extraction and the improvement of the extraction efficiency.

And then of course temperature and pressure changes, even subtle things like that, people kept changing and the records are replete with examples of memos of chemical engineers trying to adjust everything to improve efficiency in whatever way they could.

So again going back to the question, was the data representative, and I can't necessarily answer that. That is a statistical question that would need to be examined.

But I could tell you that the use of data from the `80s, I would say was not necessarily representative of the multiplicity of operations and facilities that occurred to produce recycled uranium over the many years, and it was something we were very aware of, as John mentioned earlier.

But getting this report out, the report was officially published in June, but the draft, which essentially when all the technical work was done before the lawyers and the policy people got into review, was done in April.

So if you kick it off on Labor Day and finish it in April, you have got nine months of very heavy duty work. It was not a fun Christmas, I can tell you.

But, I think everybody involved was pretty proud of what they did, but I also think that few would argue that it was fully comprehensive and necessarily representative, and that to do so would have required some follow-up work. It would have required quite a bit more time.

But, you know, I left DOE soon after that in 2001 and it appears that none of the expected follow-up analyses occurred. With that, am I going back over to John, or --

MR. STIVER: Jim, I have a follow-up question for you. At the end of your write-up that we put in the report, you listed three areas where you believe that -- you used 2000b as kind of a starting point, and made some significant improvements on that effort.

Could you talk about that just for a minute?

MR. WERNER: Well sure, and that is what I was alluding to at the end. I mean, if you know, I had my druthers, there's a number of analyses that it would have been good to back on, and I think as perhaps Paul at the table knows, I was, I wouldn't say obsessed, but certainly very interested in all the reprocessing facilities both at DOE when I was at a non-profit, and later in the later `80s and early `90s.

And for each one of these reprocessing plants, one could have, I think, gone through all the, you know in some cases there's daily records or certainly monthly logs, and I actually went to the Hagley Museum, and I know some of the NIOSH folks went to the Hagley library too, and I was pretty dismayed to see that they had only very superficial information about that, when I know that there is more detailed information about the operations in each of those facilities, and it would have been interesting to have time to follow up on just going to each of those, and whatever archives exist.

And I know enough to know from having gone to NARA, the National Archives that those files are classified, so you would have to have somebody with a security clearance or wait for possibly two years or more for those to be declassified to look at each of those facilities.

But when I would look at them, I would want to ask the question, how did the RU characteristics change for each of the locations, how did the RU change within each location for the process operation that was going on, and lastly, how, within a process operation at a facility, how did the RU change depending on the feed material that was being processed?

And those are some of the details that I think deserve to be examined and make sure that you are really understanding the characteristics of the RU being produced.

And then of course at each reprocessing facility you are getting consistent improvements in it, you know, how much did that change even within a facility, within a process, within a target, how much did sort of the tweaking of things like flow rates and temperatures and catalysts and things affect it.

And then you know, lastly, when you got the target material within a process, within a location, within a particular operation, how did switching from one target material to another affect things?

Were there offset conditions going on, when we know that they didn't always have time to clean out the facilities.

Cleaning out one of these canyons is a huge operation and you don't do it lightly. So if you could avoid having to do, you know, flush it out, you do it. You sometimes, you just, you bring in the next operation behind it. You know, they are called runs or kind of batch operations, so you dissolve a new feed material and then you feed in the dissolved material into your extraction process without necessarily flushing out. What effects did that have, too?

I think all those are more than just interesting. I think that they are relevant to answering the question about what the RU data was really representative of the range of conditions over time that occurred.

MR. STIVER: Okay, well, thank you Jim. Appreciate that very detailed historical account. Is anybody ready for a break?

CHAIRMAN CLAWSON: It's lunchtime and so I was thinking that we would break for an hour, be able to go get lunch and --

MR. STIVER: That's a good break point. We come back and talk about the site-specific data and wrap it up probably in about half an hour or so.

MR. KATZ: It would be good to have some response too.

MS. BALDRIDGE: Could I make a correction? When I was speaking earlier, when we first got started, I was referring to the DOL instead of DOE and I think that was because I watched where AOL made this big purchase on the television before I left and I had the OL on my mind.

MR. KATZ: Okay. Right. Maybe for the transcription, if they could -- you just put a note by her comments, you keep the comments as they were said but if you could put a little parenthetical note that she corrected herself with respect to attribution there.

So we are breaking for lunch and we will be back. It's by my watch, it's about -- so we will be back at one. Thank you everyone.

(Whereupon, the above-entitled matter went off the record at 11:51 a.m. and resumed at 1:03 p.m.)

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

1:03 p.m.

MR. KATZ: Good afternoon. Advisory Board on Radiation and Worker Health, Fernald Work Group. We are reconvening after a lunch break.

And let me just check on the line and see. Do we have Mr. Presley on the line?

MEMBER PRESLEY: I'm here.

MR. KATZ: Great. Thank you, Bob. Let me remind folks on the line to mute your phones. If you don't have a mute button, use *6. That will mute your phone. Use *6 again to take it off of mute, and Brad, we are back to your agenda.

CHAIRMAN CLAWSON: We got done finishing SC&A just giving a -- going through their presentation. Wanted to give NIOSH an opportunity if they had anything to say on it or --

MR. ROLFES: Well, thank you, Brad. I know we just recently submitted a

White Paper, a memorandum as John had alluded to earlier, basically responding to some previous issues that SC&A had asked us about.

Since we have just seen these presentations today and our subject matter experts haven't had an opportunity to review this new information, we would probably prefer to wait until we have had more time to go through in detail each of these presentations, and we will prepare a written response to these.

CHAIRMAN CLAWSON: Okay, with that I'll turn the time back over to John.

MR. ROLFES: One other thing, I'm sorry, I forgot to add one point. We were talking about uncertainties in the levels of transuranic contaminants earlier on, and really, you know, what it comes down to is, you know, there are uncertainties.

And when we have uncertainties, we apply those to the benefit of the doubt of the claim when we are completing a dose reconstruction.

That's not necessarily something that precludes a dose reconstruction from being done. It's just an added uncertainty. It doesn't become a precise estimate because we are applying what can be considered worst-case assumptions to an employee's claim in the dose reconstruction process.

So that's all I had to add, and if you would like to carry on with the rest?

MR. STIVER: Okay. All right.

MR. ROLFES: Thanks.

MR. STIVER: We will go back to the original presentation on RU issues 110206a, slide number 25.

And what I would like to do now is kind of move into the second half of the action items provided to us, which was to go out and look at what site-specific data we could find that might help clarify whether these bounding default values are in truth bounding, or if there are data that indicate that there may have been higher levels encountered at certain times.

And on page 25, the first set of data we looked at was this set of baghouse dust collectors that was done basically with a new M&O when Westinghouse replaced NLO, in, I believe, 1985.

And a part of that program is to go back and look at, ascertain what kinds of emissions took place during the previous M&O's operation period.

And so what they did is they took a whole series of samples from these stacks downstream of the dust collectors, and what they were really trying to get a handle on was what were the uranium discharge rates over that 34-year period.

And as kind of a -- not really -- I guess a side-study, and, you know, it would be really nice to know what other radionuclides were in these emissions.

And so what they did is they went

and they took these baghouse dust collectors and I believe the little memo that Mark sent out had a nice diagram of what the dust collector looks like.

It's kind of like what you would expect in a wood shop, we're not talking about a giant scale. You have got an inlet. It's got a cyclonic collector with a bag at the bottom and then you have got a set of filters and then the air is drawn up through those filters and then out through the stack.

And where most of the sampling was done is on the stack, downstream of the collectors. That is what was used to ascertain what the releases of uranium were to the atmosphere as part of NESHAPs compliance.

And so they collected a set of data that were -- you can see on the slide here, kind of summarized it here. They analyzed for 14 radionuclides in addition to uranium.

They took 36 samples all told and

there were actually 53 functioning collectors at the time of 1985, when these were done, and they selected 36, but they didn't really explain why they didn't look at some of the other samples, except for three.

And these were samples that didn't have high uranium content. And in the context of what the study was all about, which was to ascertain uranium discharges, I can understand that.

However some of -- at least one of the sample was for a magnesium fluoride dumping station in Plant 1 and given these mechanisms and chemical processes that were taking place that could arise and elevate and enhance concentrations of plutonium and some of these other constituents, it would have been nice to have that data as well.

And I might also point out that this is not exclusive from the data that was analyzed in DOE 2000b. This was actually part of the data set.

So it not only includes receipts to Fernald from other process plants or from production plants, but it also includes data that were actually collected on site.

We limited our analysis to plutonium-239, 240, neptunium, technetium and strontium-90. We also looked at cesium-137 because there were enhanced levels, even though there is no default level assigned to cesium.

We did not look at thorium even though there were very high levels of thorium with dose potential far in excess of uranium doses.

And the reason being is that thorium production took place in Fernald from about 1954 on into the `70s, and so most of the thorium we are seeing in the 232 and the 228 is a result of those manufacturing processes.

And so NIOSH has a different methodology in place for analyzing those types of doses and it's really outside the scope of this particular study.

We can move on to page 26. First thing we need to talk about are the gaps and limitations in this data set. First of all it's obviously a sparse set, 36 samples collected in one year.

So it's a snapshot in time. Like I said, there were 53 operational collectors and processes flow with uranium content were omitted, and well, this is understandable, as I said, in the context of this study.

There were no data reported for the refinery, Plant 2 and 3, although there was a study that looked at DWEs, I believe, in `83, I think it was like `85 and `86, which is actually reported in your RU report. I believe you have that as an attachment to that.

MR. ROLFES: There were daily weighted exposure results, a lot of them done in the earlier years at Fernald, prior to 1968.

MR. STIVER: Yes, so there were -- there are some other data but they are not tied to uranium content, but there are some air concentrations of uranium from Plant 2/3.

MR. ROLFES: That particular plant or grouping of plants was one of the lowest air concentrations.

MR. STIVER: Yes, they were fairly low level, fairly low concentrations at 2/3. At Plant 8, there are limited data there, and this is an important plant because of the preprocessing of the incinerator ash and tower ash which had to go through that plant in order to prepare for feeding into the refinery.

Another big issue here is that these baghouse collections are aggregated in mixes that are collected over an indeterminate period of time.

We don't know how long those collectors were sampling before these grab samples were taken. It could have been a year.

It could have been a few months. It could have been a longer period of time than that.

So what we have then is a situation where if you have episodic releases into the atmosphere, or into the workplace environment, of high ratios of constituents, intermingled with dispersion uranium production or low content uranium, you are going to have a diluting out process that takes place.

So what you see in that sample is not going to necessarily represent the concentration that a worker might have experienced during the time that that RU material was actually handled.

So you can only go down and those values never represent a maximum of what was actually present in the atmosphere.

Finding 5, I put here at the bottom that we feel the data is not an adequate basis for establishing default levels. However we do feel it is useful in determining whether the NIOSH defaults are bounding for all classes of workers.

If we go on to slide 27, this is kind of a summary of the dust collectors covered data and what we looked at here, of all the 36 samples for all the different radionuclides, we picked out those that were at or above, or close to or above the NIOSH defaults.

And you can see, those are all highlighted here in red bold. You see Plant 1, there's three samples there. Obviously, the Titan Mill, the sample GT64 is the highest by far.

You can see the very top line across the second row on the table is the default levels, the NIOSH defaults, in units of microcuries per kilogram uranium.

And then down the left-hand column are the plants that had samples that exceeded or were close to at least one of these constituent level defaults.

DR. MAURO: So plutonium 6.3 is the anchor for the default?

MR. STIVER: 6.3 is the anchor that corresponds to --

DR. MAURO: And we want to compare the other numbers with the 6.3?

MR. STIVER: Yes.

DR. MAURO: At least for plutonium. Okay.

MR. STIVER: And you can see Titan Mill is obviously sky-high. It's about 3,500 parts per billion and the packaging station also is knocking on the door, 6.3.

Plant 5, there's a couple of samples that are at the jolters - GT67, I'm not exactly sure what that one was. It should be 2.32, not 232.

And then Plant 8, one of the -- the box from the scrubbers was the second highest plutonium concentration. We are not really sure what the reason for that might be.

Neptunium, you can see, Plant 5,

you have got three samples that are fairly elevated. One is actually higher than the default, that was from the graphite machine shop.

And recall that the graphite and the casting area -- the graphite had -- tended to concentrate the materials in similar ways that the dolomite did with the magnesium fluoride.

And also for Plant 5, and we'll stick on that for just a second, you'll see that strontium and cesium, basically the calcium and potassium analogues in the periodic table are also elevated, and I think this relates to the tendency for them to migrate into the magnesium fluoride.

MR. MORRIS: John?

MR. STIVER: Yes.

MR. MORRIS: This is Bob Morris here.

MR. STIVER: Yes, Bob, go ahead.

MR. MORRIS: Could you clarify the

units on this? Are these -- for example, the 220 value for the Titan Mill?

MR. STIVER: Yes.

MR. MORRIS: Is that microcuries per kilogram of uranium or microcuries per kilogram of mass sample?

MR. STIVER: Microcuries per kilogram uranium.

MR. MORRIS: Okay. Thank you.

MR. STIVER: Okay?

MR. MORRIS: And that's true for all of these numbers?

MR. STIVER: That's for all those numbers, yes.

MR. MORRIS: Okay.

MR. STIVER: I probably should have put that in the table.

MR. ROLFES: John, I got a question also.

MR. STIVER: Yes.

MR. ROLFES: These were from 1985 now?

MR. STIVER: This is from 1985, that's correct.

MR. ROLFES: Okay. Then I believe these samples were collected as a result of processing the POOS material, then.

MR. STIVER: I think this was -- it was part of an overall process of the new M&O coming on board and trying to beef up the health and safety program and I really wanted to do some --

MR. ROLFES: Okay. From my recollection, you had earlier on said that 500 grams of plutonium was in the recycled uranium but you didn't really specify which site received what.

And I wanted to clarify that that 500 grams wasn't necessarily all handled at Fernald.

MR. STIVER: Oh no, no, no, not, that was for the entire complex.

MR. ROLFES: Right, right, okay.

MR. STIVER: Yes.

MR. ROLFES: But I think that the 74 grams that were sent to Fernald over the entire history, and I believe about 25 grams came in the one shipment from the Paducah --

MR. STIVER: About 50 percent came from -- at least of the documented materials, about 50 percent came from that.

MR. ROLFES: And so --

MR. STIVER: And that's what we are seeing here.

MR. ROLFES: And what we are seeing here, correct, is the results of processing that high plutonium bearing material, which was a different kind of material separate from the remainder of the recycled uranium that was processed.

MR. STIVER: It would be different than the trioxide coming in from Hanford and some of the other feed materials.

MR. ROLFES: Okay.

MR. STIVER: The tower ash and the incinerator ash were elevated and these would then -- they would feed through Plant 8 and then back into the refinery.

MR. ROLFES: And I don't recall the numbers, but I believe the first year that they started bioassaying the workers for plutonium exposures was 1986.

MR. STIVER: Yes, they started in `86. That was -- I think I brought that up earlier on in the discussion.

MR. ROLFES: Okay. I just wanted to clarify.

MEMBER ZIEMER: Could you clarify now, these values are the values found in the collectors, or is this the output here?

MR. STIVER: No, no, this is the actual dust that was collected in the bag.

MEMBER ZIEMER: That was what I wanted to --

MR. STIVER: Yes. The stuff that went out would have gone through the filter and down range.

MEMBER ZIEMER: Right, this is --

MR. STIVER: This is not an outdoor area. We do have some outdoor samples we are going to talk about in a minute.

MEMBER ZIEMER: Right. I just wanted to clarify that. So this is the higher concentration.

MR. STIVER: Yes.

MEMBER ZIEMER: Okay.

MR. STIVER: And let's see. Let's move onto the -- let me bump down here. Slide 28, this is the discussion of some of the higher values. Obviously, the highest reported plutonium neptunium values came from this Titan Mill sample, GT64 in Building 1, and I just have those values restated here.

And it should be interesting to note that when you put this back in the units of PPP, that plutonium content was about half the maximum that was ever reported in the tower ash.

The tower ash, the highest in the 16 hoppers was about 7,700 parts per billion.

And here we are three years later, after the material was begun to process, I believe in 1982 and so right on to about throughout `89.

So here you are three years into the processing, and you have this baghouse dust which we know typically is going to under represent workplace exposures, and yet you are still at half the maximum value.

So that is a -- it's a high value but I think it represents a data point that is not an anomaly, that really represents a concentrating process which we are going to be getting into in just a second.

At the Titan Mill, I'll tell you a little bit about that, it was a ring-roller mill and they processed the enriched slag and recycled materials for use in Plant 5 and also for chemical processing in the refinery.

So this would -- essentially they -- you can imagine the scenario where they bring in one of these hoppers of this POOS material and they want to break it up, process it and mill it so that they can run it through the refinery.

But after it has gone through the refinery, through Plant 4, Plant 5, the uranium metal has been produced, then you have this magnesium slag that is becoming enriched, and these materials.

And then this material then is recycled back through Plant 1, through the Titan Mill, to be ground up into the proper consistency to generate these refractory liners with the double pots.

And I believe it was 50 percent of the plutonium and about 80 percent of the neptunium reports into the slag. And so every pass around, you are getting an enhancement, you are getting an enrichment in these constituents in that magnesium fluoride.

And so I think what this represents is this kind of concentrating loop going on between Plant 1 and the Titan Mill in Plant 5, by which plutonium and these other contaminants concentrate and persist.

And this is the, in our view, is a source of elevated exposure potential in those two plants during the time of processing, and in addition to that we feel that, you know, due to the fact that if you look back -- I think I -- if you go back to slide 27, I have the percent uranium by weight over here in the second column, and you can see the graphite machine shop.

This is analogous to the magnesium fluoride. That's about 0.1 percent uranium, so it's very low in uranium and trying to tie that back to uranium bioassay results, it almost indicates to me that there should be a different way of looking at this particular source of exposure separate from the method that has been proposed. But we will get into that in more detail in just a minute.

So basically I put in a little discussion on the high values here in Plant 5. This is all documented in DOE 2000b and in our report.

This thermite reduction process, how you get basically the magnesium turnings mixed up with the fluoride. They put it in this -- essentially it's called a bomb, they put it in the furnace, they heat it, it gets to a certain point, this thermite reaction takes place, you have got thousands of degrees.

Occasionally these things blew up and contaminated the entire building, but in the process the magnesium gets converted to magnesium fluoride, uranium fluoride gets reduced to uranium metal.

So a lot of this magnesium fluoride is being generated. I would say about half of that was recycled. Some of it was disposed of, so there's a certain percentage that is being recycled -- it's absorbing more material on each pass, and so there's a concentration mechanism here.

I also put in here that in the

NIOSH report, they basically said that this measure is meaningless in subgroups in which there is very little uranium, and I guess it is if you are looking at only that particular assay method.

I think what this really points is, as I said a minute ago, that we may need to look at a different approach to determining what the concentration might have been in the fluoride.

I know there are data out there that report parts per billion of uranium in that part, so they had to get a measurement of the plutonium and the neptunium at some point, and whether that data are available or not is a point of question.

Slide 30. Continued discussion of the high values. This is -- some of the workers in certain jobs, as we said, may have been exposed to higher levels.

These would be guys who were manning the dumping stations and cleaning equipment. We have one example of graphite molds that were cleaned out -- these were interviews with the actual workers.

And one guy said they would actually stick their heads way down as far as they could in these pots. They had this, basically a broom handle with steel wool on the end, and they would scrub the inside of this thing out.

And while they were doing this, their head was down in the pot and they were not wearing any respiratory protection at all.

And so you definitely have a high potential for intermittent exposures to this material. Actually, for the guys who were in that particular job, this would be a very high exposure.

And you also have the uranium, I mean, look at the DWE data for Plant 5 and Plant 9 for thorium, these are some of the highest feeding zone samples were for the guys that were in the bomb breakout areas and pot-cleaning operations.

Also, the bomb explosions, these resulted in very high dose loading, so you have these different factors all combining together to create an environment for a certain category of workers, which may not be amenable to this approach in the NIOSH report.

So this Slide 31 is kind of a summary of what is going on in this Plant 5, Plant 1 loop. The data for -- the dust bags data show that -- would tend to corroborate this as a concentrating mechanism.

The neptunium levels were elevated in three of the 14 Plant 5 dust samples, strontium-90 was high, we saw that.

The highest neptunium level was in the graphite machine shop and so we feel that -- as opposed to the sources of elevated exposure, it wasn't limited to the building up of POOS material and then so you have -- it is indeed the case that the workers were provided with airline respirators when they were processing the POOS material to go into the refinery and also in Plant 4 and Plant 8.

It doesn't appear to me that this particular category of workers would be captured in those types of processes, and especially when you look at the long period in which this was going on from `82 all the way through `89.

It's hard to believe that -- you know, given the state of the health and safety program when Westinghouse came in, that during that intervening, earlier four- to five-year period, that these workers' health and respiratory protection was paramount.

Slide 32. We also went ahead and did some statistics on the dust data. We did log-normal fits. We got normal score plots for all the different buildings and all the radionuclides.

These are our figures, 1 through 32 of attachment 3. They show the log-normal distribution does fit the dust data very well, despite the limited sample sizes.

And we just summarized here for the different plants, 1, 4, 5 and 8, the GSDs, we list what those are, and in certain cases, the log-normal -- for Plant 1, the log-normal mean was consistent with the arithmetic mean.

But the little caveat here that you know, given that data set, the 95th percentile could be more representative of Titan and general milling workers, or workers that were proximal to those operations.

Plant 4 was kind of interesting. You saw for technetium-99 there's some samples basically from the hydrofluorination banks that were really, really high.

The arithmetic -- the log-normal mean was 15 times higher than the arithmetic mean, so that shows you that there is some little subgroup that is getting smeared up in this giant log-normal distribution, a GSD of 20.

And that is -- when you go back to

the source documentation on that, they did identify that technetium was volatilizing during the high temperature processes in that --

MR. MORRIS: This is Bob Morris.

MR. STIVER: Yes.

MR. MORRIS: So you are saying that you have GSDs of 20 or 36? Are those realistic approximations of a true data set?

MR. STIVER: I think what's that telling you is that there are some high-end activities -- that there is some -- there's probably a separate sub-distribution, but in the overall data set, it's driving the upper bound of the GSD, yes.

MR. MORRIS: It makes me think that that's not a very well picked data set.

MR. STIVER: Well, maybe it is, maybe it isn't. I think for strontium-90 there's just a few samples that were elevated, and those were related to the activities in Plant 5, where there was a concentration in those few samples that were involving the bomb reduction operations.

So yes, you can really identify -- overall, it really indicates statistically that there is a sub-population possibly, either that or a very, very widely diverse set of different processing materials being analyzed here.

In this case, I think we are looking at a sub-population and our concern is that the sub-population is not being adequately addressed. Plant 5, you have got the strontium-90, it's just ridiculously high.

MEMBER ZIEMER: I have never heard of a log-normal distribution with GSDs as large as --

MR. STIVER: Well, yes, this is just a log-normal fit. This is just to show, just to illustrate the fact that there are some points that are way out there.

MEMBER ZIEMER: Yes. I mean, the 36 --

MR. STIVER: Is that really a log-normal?

MEMBER ZIEMER: Well --

MR. STIVER: Is it really best defined by a log-normal? All the data are --

MEMBER ZIEMER: Yes, I mean, a standard deviation of three to five is pretty big, I mean it gives you a big tail. I can't even think --

MR. STIVER: Think of what it looks like, you have got two or three samples that are very high --

(Simultaneous speakers.)

MEMBER ZIEMER: There's a point way out there that are part of the same distribution --

MR. STIVER: And this also relates to the fact that there was a very sparse data set.

MEMBER ZIEMER: Yes.

MR. STIVER: And you have captured some very high operations and you have got some others that are very low. And so, you know, if you were able to go through and do a comprehensive sampling process, you would probably see two or maybe three distributions --

MEMBER ZIEMER: We don't have the true mean, which may be much higher actually, or it's a very different distribution. It does look very strange.

MR. STIVER: Yes, it does. And so that is really what we were able to discern from this dust collector data. Like I said, it's not adequate for generating any kind of a bounding value, but it certainly asks some questions on whether the defaults that NOISH are using are really applicable to all categories of workers. They have to be addressed.

The next set of data is on page 33. This is the perimeter air sampling data that Bob Barton located in the DOE task force report, 1985, also reported in the Fernald `87 data set for which the dust collector data were a subset.

And this is looking at the 1983 environmental monitoring report. I have listed the reference here as NLCO-2018. They have a nice little map there that shows where the samples were collected around the site boundaries, and they went out, they had a very detailed description of how the samples were collected, the filter dimensions, the flow rate, the diameter of the sampling apparatus.

And they changed the filters out weekly, and what they reported was an annual average of 53 samples, and as you can see here, samples 1 through 5 are clearly over 100 parts per billion plutonium.

Sample 6 is close at 94 and then sample 3 is about half the default level -- sample 7, excuse me. Neptunium was high but not exceedingly. The default BS3 was actually getting close.

And it's interesting, if you go on

to slide 34, the report in the narrative here is that the values in `83 are about 10 times higher than in `82 for plutonium.

And this also coincides with a period of the POOS processing that was taking place and so you have kind of a confluence of data sets here.

You have got the baghouse data for Plant 1 and Plant 5 and now you have the perimeter samples that are also showing for that year there was a 10-fold increase in plutonium concentrations -- or ratios, excuse me -- for five out of the seven site boundaries.

So this would tend to corroborate this notion that there's a process by which this material was being concentrated and it's actually being reflected in the downstream samples.

And even given the dilution that is taking place, by the time this material is collected on the boundary, we are still seeing about twice the default level.

DR. MAURO: So you have got a kilometer away, whatever the distance is, that's picking up air dust samples over the course of a given year, and in that year, the ratio, the default number we are looking at is 100, you are seeing plutonium concentrations that exceed it.

Now the numbers that -- the air samples that are being pulled have to reflect the integration of all of the releases occurring from the plant, so it's sort of like a smearing average of all the different lots of stacks sending stuff out, and also time.

Now, when I saw this, it said to me, my goodness, that 100 can't be a good number, because in fact, that means there are locations in the plant that are -- where it's got to be a lot higher than 100.

Now the only thing that came to mind that would say that maybe NIOSH is okay with 100 is that if the source of that --

let's say the one that is causing this to be so high, happened to come from a location in the plant where let's say the what-do-you-call-it is being handled, the tower ash.

Is it possible that the tower ash is, where it's 7,000 parts per billion, is one of the contributors, and look, I'm always looking for places where our position might be soft. And I say how in the world can you get this number and still say the 100 parts per billion is a good number?

The only thing that I could think of is if we are looking at -- there's only one source of high plutonium and that is the tower ash, and that is coming out of a particular building at a particular time.

Now I don't know if that was going on in `86 and it's making its release but the workers that are working on it inside that building, they are all protected, so they are not experiencing that ratio, and -- but there is something going out and that commingles with everything else that is coming out that is much lower.

And what you are seeing at the back end of the process is an integration where you are still above the 100, but that doesn't mean that you have got lots of locations where there are workers in the plants that are above 100.

That's the only way I can reconcile these numbers with the possibility that 100 might be okay. Just look, this is all. Forget about everything else we have talked about.

Now, was the year that the air samples was collected, we were seeing this, was that the very same time period when the 7,000 parts per billion material was being processed?

MR. STIVER: They started processing the POOS material in 1982.

DR. MAURO: Oh.

MR. STIVER: `83, you see a

tenfold increase. Now we have previously discussed this operation going on in Plant 1 of the Titan Mill, where the POOS material and also the incinerator ash and other high sources were being ground up and particalized to go through the refinery in Plant 3, and also through Plant 8, so you have got Plant 3, 4 and 8 all involved.

And ultimately it ends up in Plant 5, where you have this concentrating process. And so you do have a situation where you probably have limited stack effluent -- you have hotspots.

You have point sources here that are elevated, and those are being diluted out with other sources that are obviously not, so there are certain areas in that plant, certain categories of workers that we have kind of tried to demonstrate here that could possibly have been exposed to these elevated levels during the processing chemistry that was going on.

And that is in turn reflected in these somewhat diluted yet still high values --

DR. MAURO: Ratio to ratio, and we are seeing the ratio after it's been commingled with all the other uranium coming from the rest of the --

MR. STIVER: It's also in every direction, of course the winds blow prevailing from one direction or another, but all told, except for one sample -- one station, they are all elevated.

DR. MAURO: Just to jump in a little bit, as you iron out this process I was staying close to the whole thing and I always like to listen to -- collectively, when the story starts to coalesce in your mind.

The weight of evidence seems to be, to me -- and this is what I would believe no matter where I was sitting -- the weight of evidence seems to me that there's 100 numbers a week. There's just too many places, given the story Jim told -- combine that with the grab samples from the dust collectors and the fact that some of them were above, combine that with the dolomite issue and combine that with the boundaries, air sampling of the boundary.

You know, to me, it all boils down to, you know, we don't have an answer to this, but I got to tell you that 100 does not look good, notwithstanding the argument about the 10 parts per billion and multiply by 10, you know, on first blush, sounds like, well, that's pretty good.

But then when you look at the data, the way data just screams at you. Isn't there something wrong with that 100 parts per billion? I mean, I am ready to hear an argument why this data and everything you just said does not undermine the 100 parts per billion, but I've got to tell you, I can't think of a way to prop up that 100 parts per billion.

MR. ROLFES: Let me ask a question of SC&A then. What did the environmental perimeter concentrations for other years besides 1983 show? Did they show ratios that exceeded our --

MR. STIVER: This was the only data set we were able to locate.

MR. ROLFES: Okay, well, you did mention that the air concentrations were 10 times higher than 1982.

MR. STIVER: It was mentioned in this report. Now there may be additional samples from other ASERs. This was the one that was kind of unique in that this was when Westinghouse was trying to really get their house in order and identify what the releases were for the previous years.

And so that was reported, whether the subsequent years may have data similar to this that we could then go back and compare, that would certainly be one of the first things I would look into.

MR. ROLFES: Right. Right. We can't really base a decision based on one year --

MR. STIVER: Yes, exactly.

MR. ROLFES: -- when we have got a previous year that says that the concentrations were 10 times lower, which would make them less than 100 parts per billion.

MR. STIVER: But yet you have a process that occurs in that year which gives you a plausible explanation for why it went up by a factor of 10.

MR. ROLFES: Exactly.

MR. STIVER: And if you can then look at subsequent years beyond that, that would --

DR. MAURO: I'm not disagreeing with what you are saying, it's just that this does raise into question, and if the outcome of everything you looked at, the dust collectors and these air samples all came in under 100, and perhaps well under 10, because we keep talking about the 10, I'd say, their arguments are compelling.

If everything seemed to ring true that 10 is really a roof, but that's not what came back out of this, just, that's what it says.

Now I'll be the first to admit, you know, there may be nuance here that I'm missing, but it's simple. This 100 is not holding up very well.

MR. ROLFES: Sure. I would expect that if we had plutonium being processed, this special material that was being processed and we didn't see that elevated concentration on the air monitoring data, that would raise some suspicions.

But since we have that data and it has shown that the concentrations were in fact higher, that tends to corroborate the story that the material was different and received special focus.

And so, you know, we have chosen our defaults based upon the processing of this single shipment or single handful of shipments of material versus the other thousands of shipments of material that get concentrations below 10 parts per billion.

And as you have said, we have jumped up in order of magnitude, essentially because of the small set of higher transuranic contaminated materials that were sent to Fernald in the late 1970s and early 1980s, processed in the 1980s.

So you know, we could certainly look back. We feel that what we have got is claimant-favorable and I'll let John continue his presentation here and we will certainly respond to this in writing.

MR. STIVER: I think that you are right in that you have -- 100 parts per billion is probably good for a good number of workers for a good part of the time in certain areas of the plant.

I think what this shows is that there are classes of workers for which the 100 parts per billion is just not a strong number. It could have been significantly higher, even up to an order of magnitude higher.

MR. ROLFES: That's true and you have got to also consider the set of bioassay data that we have for plutonium.

MR. STIVER: Oh, I know, I know. We looked at that.

MR. ROLFES: Okay.

MR. STIVER: And unfortunately it is a very limited data set, basically it was done in response to a spill that took place in `86, I believe, and they sampled the workers, I think there was only a total of about 400 of them that were in Plant 4 and Plant 8.

MR. ROLFES: However, any previous exposures that those workers incurred would have been detected if they had a significant enough intake of plutonium.

MR. STIVER: If they were the same

workers.

MR. ROLFES: If they were the same workers, that's correct. And so you have that data, if that is available to us, and as you had indicated before, you know, there were roughly 10 individuals who had results which were around the MDA, and they were lung-counted.

I guess the person that had, I think there was one person that actually had a positive or what was deemed to be a positive sample based on the calculations that were done at that time, and that individual ended up not having a positive lung burden when he was counted at Hanford.

MR. STIVER: Yes, I think there was some issue about the counting interval or the elapsed time being too long for it to have been detected.

But yes, it shows that there is a subgroup who were sampled with this one particular operation in these two plants, during the initial processing, until they had the spill and they had to run it out.

That doesn't count for the people who were employed under NLO the previous five years and whether they were the same workers and whether they were groups of workers, say in the -- we keep getting back to the metal production plant, who were exposed to significantly higher values for which uranium bioassay would not be a viable method for determining a dose.

MEMBER ZIEMER: John, or maybe Mark, in these perimeter samplings, were these at ground level or where were they sampling?

MR. STIVER: That's all in the report. I believe there were about -- about a meter high or so. They are all on the perimeter fence --

(Simultaneous speakers.)

MEMBER ZIEMER: -- points were from stacks?

MR. STIVER: Yes, from stacks.

MEMBER ZIEMER: Okay --

(Simultaneous speakers.)

MR. STIVER -- the ratios --

MEMBER ZIEMER: Well, no, my question has to do with particle sizes. At ground level, what you are most likely to see are the heavier particles. I wondered if we know whether -- what the particle sizes are. Are they actually respirable?

MR. STIVER: Actually, there are particle size data out there. But we are really kind of concerned about right now about the ratios, not so much the absolute values of the different materials, but what were the ratios, what will we see --

DR. MAURO: It is going to be low concentrations.

MR. STIVER: Yes, the concentrations are very low.

DR. MAURO: So that's why I keep reminding you --

(Simultaneous speakers.)

MR. STIVER: There are particle size data for the samples available, if you want to use that data alone to generate an intake from.

But what we are really concerned with is here we have got ratios, they are low concentrations but the ratios are twice the default. And if you are going to use that with uranium bioassay data to bound transuranic intakes and doses, that becomes a problem.

MEMBER ZIEMER: Well, I think you can still make the argument on the ratios, if it's a ratio, the ratios could be different for the respirable particles, that's the point I was trying to make.

MR. STIVER: Yes, you could have some -- yes, there could be some fractionation coming up.

(Simultaneous speakers.)

MEMBER ZIEMER: Do they go all the way around the perimeter, these are not just downwind values?

MR. STIVER: These are -- that's the interesting part, is that they are not all downwind, they're basically all around, and there's an actual map --

MEMBER ZIEMER: There's a lot of mixing.

CHAIRMAN CLAWSON: You know, something that I am looking at is -- and we have seen this at numerous other sites -- and that is it's trying to fit all the people in one mold, and, you know, I can -- my small assumptions here, a lot of these plants are a lot higher.

And were these people all separated out into different jobs? My whole issue is that I am having a hard time fitting everybody into one mold, because --

MEMBER ZIEMER: Well, that's why you try to find an upper bound so that you can do that, otherwise you are exactly right.

You work out the wrong upper

bound, and you are going to have a lot of people that are above whatever you called it.

CHAIRMAN CLAWSON: Well, I'm just sitting here looking at like the Titan Mill and so forth like that and those people are going to be far, far higher. It's just -- anyway.

DR. MAURO: We would be the first to agree that to integrate the average across the site over all time, you are going to be on to 10, I mean, that's what it seems to me.

But what the real problem is there are periods of time and locations in particular streams where you are well above 100, and if that's what the guy -- now, if he had to work, would he -- you have not gone to his dose.

We are always in this situation. There's a small group of hard-to-define people that, we have to ask ourselves, do we feel that the bounding number you have is convincingly bounding?

And right now I think there's enough evidence here that says that it may not be, and --

MR. MORRIS: This is Bob Morris.

DR. MAURO: Yes, please.

MR. MORRIS: Are all of these data that are on site 33 statistically significant above the detection limit threshold?

MR. STIVER: Yes, they were.

MR. MORRIS: Okay.

MR. STIVER: There were some that were under the threshold which we didn't analyze.

MEMBER SCHOFIELD: One possibility is the fact that you could have people in the process still working out in the plant actually be getting less exposure via inhalation than those people working outside that plant because -- based on the ventilation system.

How much is actually being filtered going out the stacks?

DR. MAURO: I agree with you. The ratio is going to hold.

MEMBER SCHOFIELD: But now those people out there actually had the potential for seeing the higher dose.

DR. MAURO: Yes, and that's okay if we have got bioassay data --

MEMBER SCHOFIELD: But you don't.

DR. MAURO: -- and over 90 percent of the workers have it. But if you don't, you have got a problem.

MEMBER SCHOFIELD: Yes, if you don't have the bioassay then you have a problem.

DR. MAURO: Yes. We are operating on the premise that there has been -- the vast majority of the workers, over 90 percent, at least beginning in around `56, I think earlier than that it was a little lower -- but even in 1953, it was 25 percent, not a bad number -- have urine bioassay, milligrams per liter in the urine.

And so whether you worked inside, whether you worked outside, everybody had this -- just about everybody, 90 percent, had bioassay data.

So we think you could reconstruct the doses to uranium. That's what it comes down to, because of the vast amount of bioassay data, notwithstanding the, what do you call it, construction worker questions on the table.

That is still on the table, I agree with that. Of course that could upset the apple cart a little further. But let's say for a moment that we have got -- we are pretty solid on that, and then we come in and say all right, well, the approach, this one size fits all, with 100 possibilities et cetera, you know, if that holds for everyone, or the vast majority, well, then you know you have got a pretty good way of reconstructing the doses that are plutonium too.

But what happened was, when we

went through this process, we were hit with these numbers. And these are numbers and they speak to you.

What does that mean? That means that 100 doesn't look so good, at least for some workers. That's what we walk away with. What the right number is is hard to say.

And it's not only these numbers that we are looking at in this last table, but it's the arguments we heard earlier, about what went on, the complexity of the problem.

So just because we are looking at `86 doesn't mean that there wasn't anything unusual going on in `57 and `58 or `60, whatever, I don't know.

And then, of course, there's the dust sample collection, the argument being, well, places where you are seeing the high dust collection levels have something to do with this POOS that may have come through the tower ash, came through.

But then again you see it in more

than one place. In other words, the dust collecting, the problem you are seeing wasn't only in the building that would have received this material. It was in other places too.

MR. STIVER: And that was our main concern, that it wasn't just isolated, you know, a certain processing facility in one certain building, which then moved to another process, so those workers are using airline respirators or being bioassayed, which they weren't until later.

But the basic argument being that the health and safety processes were not adequate to capture what portion of the workers. Most of them? We don't know. We just don't know at this point.

CHAIRMAN CLAWSON: Let me get a better handle on this bioassay that we keep bringing up. Now, this was urinalysis for uranium.

DR. MAURO: Right, milligrams per liter of uranium. That's where we have got tons of data.

CHAIRMAN CLAWSON: And that's -- and you know, I have seen that at 250,000 or something like that. But that just looked for uranium.

DR. MAURO: Uranium. That's it.

CHAIRMAN CLAWSON: It doesn't do anything else with any of the other radionuclides.

DR. MAURO: Correct.

CHAIRMAN CLAWSON: So, when they brought in these raffinates or transuranics, I believe that's the right term, it's put in a whole other issue?

DR. MAURO: That's the essence of the problem. You are working. They take a bioassay sample from you, they look at your milligrams per liter of uranium, they can predict what your intake of uranium was.

But then all of a sudden you say, wait a minute, by the way, he was handling recycled uranium, and we know there was some plutonium and some neptunium in that stuff that you inhaled. How are we going to account for that?

Well, once we know the number, in effect, we know the ratio, you can predict how much plutonium you inhaled along with that uranium, and that's the way to track the problem, if you felt confident that you had a good appreciation for how much plutonium was associated with the uranium that you inhaled.

And the argument being made that 100 parts per billion is that relationship, and our position is that, you know, when we came into this, we'll see if that's good.

And now you are looking at our data and it says that, hmm, it is not as good as you might think. We have got some real serious questions for the -- for reasons that are right in front of us, that 100.

And if you made a factor of two error in that for you, for example, that would have a substantial increase on the dose to certain organs. Not all organs, but certain organs.

CHAIRMAN CLAWSON: That's what you showed earlier.

MR. ROLFES: To clarify that factor of two though, you also have to keep in mind the claimant-favorable assumptions built into the reconstruction of the uranium intakes, which is used as the basis for adding in those other radionuclides, the transuranics.

We are not doing a best estimate type fit. We are not looking at individual acute intakes. We will basically assume a chronic exposure of the most claimant-favorable solubility type for the target organ in question, estimate our intake -- if it was before 1965 we use natural uranium, after 1965 we default to a two percent regimen -- then on top of that we use RU-234 to assign the internal dose for the target organ, which gives another 30-something percent that we are

--

DR. MAURO: Did those conversion factors between 234, 235, 238, for picocuries or becquerel inhaled are not that different.

MR. ROLFES: Well, I'll have to take a look at the numbers -- but if you take a look, I'll have to take a look back at the U-234 versus the isotopic distribution of natural uranium.

But that internal dose is pretty claimant-favorable, and that's the basis for us to add in the transuranics on top of it.

So I mean, basically, to start off, we are assigning uranium as a chronic exposure, most claimant-favorable solubility class, calculating all internal dose from U-234 and then adding in the 100 parts per billion plutonium, the 3,500 parts per billion of neptunium-237 and then 9,000 parts per billion of technetium-99.

MR. STIVER: You know, Mark has a good point. What they are trying to do is bound the dose, and our concern is, are we really bounding the dose for all categories of workers for the SEC period?

Now, all these claimant-favorable assumptions are great, but is 100 parts per billion a claimant-favorable assumption for all categories?

DR. MAURO: And I've got to say, I do have to take on one of the points you are making. When you are trying to reconstruct a dose from uranium -- let's forget about the plutonium; make believe there's no plutonium -- what you are doing is reasonable.

This is what you have done everywhere, and that is, let's use the form -- because very often you are not quite sure what the form is. If you knew for sure what the form is, you would use that form.

But maybe we don't know, and there is some question, because we all know uranium is sort of a strange animal. Sometimes it's M, sometimes it's S, sometimes it's something in between.

So you are doing the prudent thing for uranium. So I wouldn't -- now the fact that, embedded in that, there may be a certain degree of conservatism and claimant-favorability because you had no choice. You had to do that, in order to make sure that you were treating that worker claimant-favorable.

I separate that now. Now we are going to move on to plutonium, and we are going to layer in plutonium, and I think that now when you are dealing with plutonium, you have to deal with it in a way that is going to be claimant-favorable for the worker.

And the 100 seems to be a really good number for most workers, but we certainly -- now we see that certainly there were probably categories of workers where that may not be.

So I mean, what you are doing is say, well, we threw in so much conservatism over here, that is going to protect us from all other things that we might have missed over there. I don't know that you want to do that.

MR. STIVER: It's kind of an apples to oranges issue, isn't it?

MEMBER ZIEMER: Are we confident that it's only this few years during this particular episode where these numbers are high? I mean, you are assigning your 100 for every year. You assign the doses year by year.

MR. ROLFES: Correct, starting in 1961, because that was the time period when they first started processing the recycled uranium that they had received.

MEMBER ZIEMER: Right.

MR. ROLFES: So for all uranium intakes that we assign to an employee, we would assign the transuranic intakes as well, from 1961 forward.

MEMBER ZIEMER: Now, is there any indication that outside of these years where you have this information, which seems to correspond with that --

MR. STIVER: It corresponds with that tower ash.

MEMBER ZIEMER: Right, but the prior years and the after years?

MR. STIVER: The after years, I think you have got this influx, this injection of plutonium into the system and it persists up until `89.

So it's really the pre-1980 period, and the problem we have there is that before 1970, we just don't have data. You have shipment data from Hanford and that is pretty much about it.

But as you can see there's chemical processes going on that concentrate the stuff. Regardless of whether it was POOS or not, you are still going to have that same concentrating mechanism going on.

And as Jim said earlier, the process improvements over time result in less transuranics and fission products making it through in the final product.

So if anything, in earlier years, you would expect to have more of the material, notwithstanding the POOS material.

That is kind of a unique -- in addition, it's an order of magnitude higher, but you know if you look -- without POOS I think you would see a trend towards less and less concentration over time.

So, I guess the -- you can make some kind of a common sense judgement that well, you know, without the POOS, we have got this data here from the Hanford for the `70s that shows that you have got a lot of data down in the three to five parts per billion range, that's probably a good number.

MEMBER ZIEMER: Well, but the argument earlier was --

(Simultaneous speakers.)

MR. STIVER: -- you just don't have the data.

MEMBER ZIEMER: -- that these were

years where they had big input of plutonium into the system, so you can't have it both ways.

MR. STIVER: Yes, they are going to be bounding for those early years, you can say maybe it is, maybe it isn't.

MEMBER ZIEMER: Well, yes, I was kind of thinking about the possibility of having a default value for a certain time period, and --

MR. STIVER: Yes, it's not one size fits all.

MEMBER ZIEMER: -- and then at the part where you knew -- I mean, yes, you may have these concentration things going on, but the source term has got to have been much lower.

DR. MAURO: And you have got that dolomite problem.

MR. STIVER: You have got the dolomite, you have that concentration problem going on, and it's not really reflected in --

MEMBER ZIEMER: Well, I was just starting to think about the possibility of having a set of default values for a certain period, and then a different --

MR. STIVER: I think the only way to really get a handle on this is to go back to the source data and -- see you later, John -- John Mauro has left us. He is trying to get home in a reasonable amount of time.

DR. MAURO: I'm leaving.

(Laughter.)

MR. ROLFES: John? This is Mark Rolfes. I am looking back at my -- our response to the SC&A findings related to the White Paper on recycled uranium at Fernald and it's from October 2010.

I think we have actually described fairly well about the changes in the processing from different sources of uranium and the potential plutonium concentrations over time, and we provided a summarization that starts back in 1944.

MR. STIVER: All right, yes, we read that.

MR. ROLFES: Okay. Now from my recollection, the earlier materials that were processed had some of the lowest plutonium concentrations in the complex, and then --

MR. STIVER: For which we have data.

MR. ROLFES: And then subsequent to that, it really was that 19 -- late 1970s, early 1980s POOS material which was separate from all the other recycled uranium materials, that was its own special case, own special class of materials, where we had the 7,700 parts per billion plutonium on a uranium mass basis.

The earlier stuff, the earlier uranium that was sent to Fernald based on everything we have seen, was typically less than the agreed 10 parts per billion.

There may have been some exceptions, and I'm sure there were, but I don't recall seeing anything that exceeded our default of 100 parts per billion.

I don't know, Bryce, this is Mark, if you have anything to add on our research that we have done on the recycled uranium issue, if you --

MR. RICH: I'm having a little bit of trouble with my phone. I am losing contact every once in a while. But the Hanford data, for example, did start out in the five parts per billion range, and over the years it did drop into the threes.

So there is a slight reduction as indicated, until of course then we hit the POOS material and then everything went up by an order of magnitude.

A couple of other things -- Plant 1 had a role of handling and feed preparation. Titan Mill handled much of the POOS material and that was prepared for introduction into the rest of the plant, that the Titan Mill was a grinder reducing the particulate size so it could be dissolved and/or blended into other lower-grade, lower-level material.

So the stuff that came out of Plant 1 either went directly into processing or it was blended and reduced in contaminant concentration that way.

The -- and again, as has been mentioned I think several times, is that the air filter data was used only for illustrative purposes not for default evaluations.

The default streams the process streams were used to derive the maximum feasible that appeared to us at the time and default values, and even the magnesium fluoride, regardless of the fact that it was a process, it was a product stream that was reduced to metal, was -- had significant levels of the Pu but not above the 100 parts per billion.

MR. STIVER: Actually, I think the issue is that Titan Mill was not only used for preparing this material for feed into the refinery. It was also used to reduce down and pulverize the slag for use in Plant 5.

And so we had a situation where you've got this concentration mechanism that is causing a persistence in the -- between Plant 5 and Plant 1, and I think that's really our concern, is that you have a --

MR. RICH: A good deal of that slag, magnesium fluoride was reprocessed for the uranium that was remaining also.

MR. STIVER: It was reprocessed for realigning the pots.

MR. RICH: It was, and also there was a remainder of uranium in that slag also.

MR. STIVER: But there's very little actually. I mean there is some, from what we are seeing is it's about 0.1, 0.2 percent compared to some of the other values. It's quite low. There is some. There is some.

MR. RICH: But generally not a lot. In other words that process stream was sampled routinely and that is indicated in the process analyses and --

MR. STIVER: Well that's another issue we have, is with the quality of that process data, which we had discussed earlier.

MR. RICH: I understand.

MR. STIVER: Yes. We have a few more, if we could I would like to go ahead and talk about some of the other data sets we have, if we could go ahead and move on, and then maybe we could take questions, some more questions, after.

We still have some other issues that hopefully we will be able to get to today.

Slide 35, these were some air sample swipes and swipes that were reported in a U.S. testing company report from 1989, and also this Bassett report in 1989 as well.

And this is -- what's really important to our analysis here, these additional air filter samples that were taken in Plants 4 and Plant 8. During -- and this would have coincided with the POOS processing that was going on in those plants, for which the bioassay data that Mark had alluded to, were collected.

There were 54 total results they had about 20 smear samples for reach building, none of which exceeded the default levels, and then they had air samples, and they have a very good description of how the samples were collected, where they were.

They have survey maps for both Plant 8 and Plant 4 that describe where the samples were collected, and also demonstrate where the dust collectors were relative to where their samples were collected.

So we were able to do kind of a generalized comparison and -- granted there is a four-year differential in time, so you can't make any concrete deductions from it, but you can certainly, it's interesting from just kind of a general perspective.

If we can move on to 36. For Plant

4, air filter samples, two of them, AF1 and AF4, are significantly higher than the NIOSH default and we had one neptunium value that was getting close.

And when you look at the survey, these samples that were collected in `89 were over the hydrofluorination banks, and if you look at the survey map, they are about anywhere from about 20 to 150 feet away from the packaging stations where the dust collection samples were done forty years earlier.

So what you could be seeing is just a variability within a plant relative to the processes, and the amount of material that happens to be collected in a given place.

We also found the technetium volatilization was not expected but -- and it was detected in the hydrofluorination, so we think it's possible that the neptunium could have volatilized as well, and possibly the plutonium.

We have no concrete evidence that that is the case, but it is kind of inconclusive as to how those samples would be so much higher other than just the basis of their relative location close to the fluorination banks.

Plant 8, none of the samples are higher than the defaults for plutonium or neptunium. However we were able to pair up three samples with the dust collector data based on the survey maps, and while you didn't have any higher than defaults, we had values that were probably by a factor of 10 higher than the later dust collection samples that were in the areas around the drumming stations where the workers would be, which kind of lends credence to this notion of a dilution in the baghouse concentrations over time.

Let's see. Anything else in this slide that we should discuss -- one thing that we noted about the Bassett collections, these were 24-hour collections and they don't really tell us anything about off-normal occurrences. All they do is kind of give you a really generalized comparison to the previous data set.

So you know, there are potentials for failures in both Plants 1 and Plant 8 which wouldn't be captured by this data set.

Finally, the last data set we looked at, Bob Alvarez was able to pull some PUREX UNH data from Hanford from 1970 to 1972, and this is really kind of unique because we had about 330 data points for plutonium and neptunium in this material over about a two-year period, so we can actually generate a distribution for a -- one subset of feed material.

Now granted, this material is several steps removed from what workers at Fernald might have experienced, because it would have gone into the -- but would have been calcined to produce the trioxide powder, and then shipped to the receiving sites.

But it does -- it's illustrative in that there are -- it demonstrates that this 10 parts per billion production specification was not -- was violated in a couple of different -- I don't know if violated is really the best term -- it's just that you had material that was in excess of 10 parts per billion that was actually being produced there in their own plant.

If we can move on here to the next slide, 38. This is kind of a summary of what we found here. The plutonium-239, there were 329 samples.

The highest was about 1,550 parts per billion and if you look at arithmetic values, you have got a median of about 15 and an SD of 98.

About 15 percent of them were over 10 parts per billion and about, only seven were over 100 parts per billion. If you look at the normal score plot, basically you've got a log-normal distribution up to about the 95^{th}

percentile.

But you have this high, you have seven high batches up in the high -- up in the upper tail.

Of the neptunium, we had 84 that were less than the detection limit out of the 336. You look at the plot, look at about one GSD, you have got a fairly good log-normal fit, then below you have so many down in the detection limit, and above you have got some that really aren't modeled very well by the log-normal either.

So it's really, what this tells us is that you have got high batches, we isolated those to -- there were only about six batches that came through that were high in about the same time period.

And so the question remains, the story is in our minds, is, is this an isolated occurrence, and we have got one data set that demonstrates there are feed materials that are out of spec but whether they left Hanford --

And so I guess the question in our mind is, is this isolated and if this material was not blended before shipment, was the Fernald personnel aware of its arrival beforehand. So it's just --

MR. MORRIS: Bob Morris with a question.

MR. STIVER: Yes, go ahead.

MR. MORRIS: On the plutonium-239 data set on page 38, you didn't provide the log-normal GM GSD as you did for neptunium. Is there a reason for that?

MR. STIVER: I just didn't put it into the slide. That's in the report though. All that information is there.

MR. MORRIS: And was it well fit.

MR. STIVER: For the plutonium?

MR. MORRIS: Yes.

MR. STIVER: Yes. It fit within that. There was really only those seven batches that were clearly up above the log-normal fit. Those were the ones that were the outliers that we identified in certain batches at certain -- in a given time period.

MR. MORRIS: So when you say two percent above 100, that's not the log-normal, predictive two percent. That's -

MR. STIVER: No, that's the actual data. That's what -- the actual number that we are above. Yes.

MR. ROLFES: It seems like the bigger question might be whether this material even went to Fernald, since --

MR. STIVER: Well yes, we don't know. We don't know if it went to Fernald or not. It was just to illustrate that it was the only data set we actually found of a production run, and we were actually able to look at it and do some statistics on it.

MR. ROLFES: If it went to Paducah

(Simultaneous speakers.)

MR. ROLFES: It would certainly -- it would purify essentially the uranium and remove those contaminants, the neptunium and the plutonium, from the recycled uranium, just by process.

MR. STIVER: It's really just illustrative of what a distribution material would be in a production setting, and we weren't able to find any other data from -- especially prior to 1980, other than this particular data set.

MR. RICH: So this is Bryce. A quick comment. Those records of the peer shipments from Hanford to Fernald, those were over the ten parts per billion limit and there was an agreement to ship them as they came, in the 28 to 30 parts per billion range.

So it was not -- if the analysis were done carefully by Hanford and any violation of intent was communicated and an agreement reached.

MR. STIVER: Certainly, this does not illustrate or indicate there is a smoking gun or anything. It's just to show that you know, we had a data set that we were able to evaluate and we felt it was worth putting in the report just because there is kind of a dearth of data on the actual production side.

So yes, whether it actually made it to Fernald, and whether, was it down blended or not, that's all open to speculation.

So we can't really draw conclusions on that particular data set.

DR. GLOVER: If you look at SRDB 67613, actually it stored U03 quite a long time at Hanford, and they talk about the product specifications that were going to go to Fernald in -- about that same time frame, about `69 is the heavy specs.

And they said, they sent those four, so these things -- this is U03, what the assays were, and also it describes specifically that anything exceeding that had to -- when AEC -- no, they would require a waiver by the AEC, so there was a mechanism to ship stuff higher than that.

But I did want to say that that did go to Fernald, it looks like, they did have material, obviously --

MR. STIVER: Yes, there is always some going to Fernald. This particular data set, it's not really clear if it went to Fernald or not.

DR. GLOVER: Sure you don't know if they were.

MS. BALDRIDGE: I have a question. When they determine what the bounding level is, I assume they will assign those doses based on the employment records as far as who was supposed to be working, where?

MR. STIVER: They are based on the actual bioassay records.

MS. BALDRIDGE: On the bioassay rather than the --

MR. STIVER: What they have is kind of a one-size fits all approach where they have got it evaluated at what they believe is a bounding value.

MS. BALDRIDGE: Okay.

MR. STIVER: And so, given the uranium content in urine they can calculate an intake and a dose from that, and they can add an equivalent amount that would correspond to 100 parts per billion for plutonium and --

MS. BALDRIDGE: Will they be doing that for everyone that was working in that year or just those people that were supposed to be in Plant 8 and Plant --

MR. STIVER: I believe this applies across the board, the dose reconstruction --

MR. ROLFES: Yes, this is a dose reconstruction question, I guess, that we should probably answer rather than SC&A. Anybody that worked on site from the years of 1961 forward and was involved in uranium operations, and had a bioassay result, would be assigned recycle uranium intakes as well.

Now, some people weren't monitored. We would also assign a coworker intake to those individuals and also assign the plutonium and other transuranics to them as well, based upon the coworker intake models of uranium.

MS. BALDRIDGE: But this is across the board?

MR. ROLFES: Across the board, for everyone. Now, separate from that, there are some dose reconstructions that were completed early on where we used this completely separate methodology, OTIB-2, where we assigned 28 radionuclides, a worst-case approach that was used for dose reconstruction.

And so those dose reconstructions likely did not have recycled uranium components assigned in the method that we are discussing.

However, when we go back and look at those cases and compare the doses that we have assigned based upon that OTIB method versus the individual's own bioassay data, the dose reconstruction using the individual's own bioassay data typically result on lower internal doses.

So we wouldn't have to go back and assign a smaller dose to those previous dose reconstructions. Did I address what you are asking? I know it's not something we typically talk about in normal conversations when we --

CHAIRMAN CLAWSON: Let me ask you this. My understanding that if you showed -- if you took a bioassay urinalysis and you showed uranium, then you got the other radionuclides. This is where this 10 parts per billion was coming in.

MR. ROLFES: What we would do, let's see here. Let me write up on the board here. Let's say we have got an individual that worked from 1965 through 1980, and let's just say they had a urine sample taken once a month. We'll just say that.

MS. BALDRIDGE: Once a year.

MR. ROLFES: Once a year, okay, we

can say once a year too, I mean, either way it's you know, but basically the limit of detection for Fernald was about 14 micrograms of uranium per liter of urine, depends on the year.

But what we would do, and we will say, for example this individual had 100 samples over their employment history during these years, all of them were at the limit of detection so they never had a positive bioassay result, what we would do is take each of those bioassay results reported to us and we would convert those 14 micrograms of uranium per liter into a value that was excreted per day.

So we would multiply this value by 1.4 to account for a number of liters of urine produced per day to get a 24-hour excretion rate.

So this gives us -- I am not going to do the math here in my head, but if we multiply 14 micrograms by 1.4 liters, that gives us our excretion in mass quantities per 24 hours.

What we would want to do if the individual was exposed to natural uranium, we would use a default of 683 picocuries per milligram of uranium, or 0.683 per microgram.

We would use that, we would plug that data into a computer program called the integrated modules for bioassay analysis, IMBA, and that will give us an estimated intake rate.

Then we typically use the solubility class that results in the highest internal dose to the target organ. So once we have this intake in activity, in picocuries, we would go back and assign the intakes of plutonium, neptunium and technetium on top of that uranium intake, and also calculate the internal dose from those.

And we would assign that dose from 1965 -- those intakes from 1965 through 1980, and then we would calculate the internal dose through the year of cancer diagnosis.

When we have a large bioassay data set, and multiple results, you can go and assign acute, small duration intakes, but usually those approximate a larger chronic intake.

And so what we do, rather than trying to get a best estimate, we will assume that that individual was chronically exposed for his entire employment period to uranium.

So the way we complete the dose reconstructions, we are making some very claimant-favorable assumptions regarding the exposure duration, the types of materials that the individual was exposed to, the enrichments that they were exposed to, and then on top of it, the plutonium and transuranic intakes are added in.

CHAIRMAN CLAWSON: I thought earlier that if they came up with a -- if they came up with any uranium I guess it was when Jim Neton was here, that they came up with any uranium in their bioassay, then they got all these other radionuclides.

MR. ROLFES: Yes, and even if an individual -- say for example, during some years in a more recent time period, the sensitivity of the uranium urinalysis method that was used decreased, so during the more recent years, say this limit of detection dropped down to about five micrograms per liter, and then after that, using different analyses, like inductively coupled plasma mass spectrometry, they were able to get less than a microgram per liter of uranium in their minimum sensitivity values.

And so even if an individual -- my point of this is, even if an individual had a result reported -- if they had a bioassay sample, whether or not it was positive, we still would use that in dose reconstruction, even if it's a non-positive result below the minimum detectable amount, we would still assign an intake of uranium and then the subsequent plutonium, neptunium and technetium radionuclides, regardless of whether they did in fact have a bona fide positive result.

MR. STIVER: So it's all tied back to your bioassay, any result is going to have with it the transuranics that go along.

CHAIRMAN CLAWSON: Do we need to take a break or anything?

MEMBER ZIEMER: It's up to the chair.

CHAIRMAN CLAWSON: Why don't we take about a 10-minute break and we will continue back on with this.

MR. STIVER: I think we are just -- I am just about done with my presentation. Down to the last slide. But yes, let's take a break.

CHAIRMAN CLAWSON: Let's take a break real quick and --

MR. KATZ: Okay, about 2:35 then.

(Whereupon, the above-entitled matter went off the record at 2:22 p.m.

and resumed at 2:35 p.m.)

MR. KATZ: Okay, we're back after a short break. Do we have Mr. Presley?

MEMBER PRESLEY: Hey Ted, this is he. I had trouble getting back in for some reason.

MR. KATZ: Okay, but you did it.

MEMBER PRESLEY: Yes, finally.

MR. KATZ: Yes. Good to have you. Okay.

CHAIRMAN CLAWSON: Okay well John, you are just about finished --

MR. STIVER: Yes, I have just about finished up here. Go to slide 40, this is basically the summary of our findings here. And the ones that I have highlighted are number 4, 6 and 7.

Number 4 relates to the DOE RU reports and our summary here is that that report is questionable as a basis for the NIOSH defaults.

We believe that the source data

that went into that statistical analysis, those 4,000 data points, should be reviewed in the context of dose reconstruction, and especially for an SEC petition, particularly as regards the statistical analysis and the type of distributions that are assumed.

Finding number 6 and number 7, the dust data and the boundary air concentration data do not support the NIOSH defaults, and they are consistent with the elevated levels observed in the dust collector data that in turn would tend to indicate that there are classes of workers in certain types of jobs of which the NIOSH defaults are clearly not bounding.

And that is basically all I have to say regarding this particular paper. Is there a follow-on item here?

CHAIRMAN CLAWSON: Well, I guess I am looking at what type of an action item we have. We have got to be able to give NIOSH the opportunity to be able to respond to this, and issue their paper to us on this.

But I do want to keep us all in mind of the timeliness of this. This has been almost five years now, so I guess in tasking NIOSH that, to be able to respond to the paper that has been submitted by SC&A, I guess it would be a response to SC&A's RU paper.

And we will get into that and go from there. I know this is a hard one to do, but what type of a time frame do you think we would be looking at for -- to be able to --

DR. GLOVER: One suggestion I would have is that I mean, you have an entire paper, you are going to have an entire paper back. There are certain things that are more focused on what are either SEC-driven or -- I'm just, you know, if you really want a timely solution, and you want to be very specific, then we would probably be quicker in response if we were focused.

CHAIRMAN CLAWSON: As far as an SEC, a lot of it brings your default value in question. This is what I glean from this paper, that the default value that we have, if you are not able to really justify that, that is an SEC issue.

And I guess I don't want to push NIOSH into a position of just -- I want them to understand where our issue is at with it.

DR. GLOVER: So the nine findings here -- this is really Mark's thing -- but you basically would like a response on the summary of these nine findings?

MR. STIVER: Yes, the really important one I think is the basis for the defaults, this -- the DOE RU reports. The way that data was analyzed, you have got distributions that are probably more characterized by a log-normal.

The analysis, I looked at the arithmetic mean or some derivation thereof, as the basis for the defaults, and also to justify the choice of the 100 parts per billion.

I think that that's probably one of the most important of these findings, is to go back and ascertain that data is available for review, and if so to analyze it in terms of its adequacy in dose reconstruction.

Instead of just taking it directly from the report, do your own uncertainty analysis and your own review.

DR. GLOVER: I guess in your discussion you sort of seemed to throw in there about it being a snapshot in time and about its ability to be back-extrapolated back and --

MR. STIVER: Yes, but that's an issue as well, is you know, this scenario would involve really reviewing the adequacy. I mean, I guess, you would almost need to do a scoping study to determine if it's worth the expenditure of resources to go down that route.

DR. GLOVER: I guess I just want to make sure that your question, what you were trying to get a response back on was fully fleshed out, so we were provided adequate response.

It was a very long presentation, obviously, you know, multiple slides that we haven't seen before, and I just want to make sure we came -- we get from it what the Board would like us to be responsive on.

MR. STIVER: Yes, I really believe that at the reevaluation of the available data and using that to bound sources of exposure for different categories of workers, and I think the other issue that is kind of related to that is the idea of the magnesium fluoride and the concentration processes, and potential exposures to those workers as well.

And the other back-extrapolation. Are data available for the early years? Is it possible to bound doses during those times for which data don't exist?

So really a kind of three-part --

MR. KATZ: So, may I make a

suggestion? I mean you have got -- I think that was helpful, that why don't you, DCAS, write up a proposal for what you think are the sort of critical issues that you would address in a White Paper response, share that with SC&A, in the Work Group, SC&A can say yes, that seems to pin down the critical issues, or not, whatever, elaborate and then we will have a clear path forward.

At the same time I would give you a little time to figure out not only what you are proposing but a good sense of how much time you need to be able to deliver that.

That way the path forward is clear rather than -- I mean this is still kind of vague, this discussion, but --

MR. ROLFES: Yes, let's see here. I certainly would want to respond in writing to the findings that we have, but I also want to keep in mind that we have responded to these same findings previously in some of our responses.

You know, it's coming down to what I am seeing, because SC&A is just -- as you had pointed out you know, the distribution that you guys derived from the data is slightly different than what we have derived.

And so, in my mind, that necessarily isn't of itself an SEC issue. It's more of a dose reconstruction on what uncertainties we are applying.

So you know, if you would like us to back and look for some additional data, that is going to take a lot more time than it is to just look at the data that we already have.

I don't know if we want to have Bob or Bryce add anything to what we are discussing on the time line et cetera, or what things we haven't answered previously or what we feel we should clarify in our response. Is there anything that you want to add Bryce?

MR. RICH: Not right now, Mark. I think what you have said is sufficient.

MR. STIVER: I just am afraid that, we kind of got to this impasse really, where NIOSH has their position and we have our position, and the two are kind of at loggerheads here, and I have laid this out as best as I think I can, as to what our concerns are, and that is the issue of Classes of workers for which the defaults are not bounding, and we would like to see some response as to how different values might possible be applied.

There may be a situation where you can't have a one-size-fits-all, where you may need to look at different bounds for different categories of workers. It's not our position to really give that kind of guidance.

CHAIRMAN CLAWSON: Go ahead Paul.

MEMBER ZIEMER: Well, we already know that they can't easily put workers in certain spots, locations and so on. So you are really going to have to deal with some defaults and some ratios and so on.

I am kind of thinking what information is new that was brought to the table? Some of it is sort of the same stuff recast a little bit.

But the numbers on the plutonium ratios, those new numbers today are -- that's new information isn't it?

MR. ROLFES: Our ratios have not changed.

MEMBER ZIEMER: No, not yours, the numbers that they brought.

MR. ROLFES: Correct. His environmental data, his analysis for the particular year when the POOS was being processed is above our defaults.

MEMBER ZIEMER: Right, and it seems to me that that sort of focal point, which NIOSH needs to sort of say okay, does this impact on what we are proposing to do?

And it may be that it would impact on the specific year, or it may be that taken in combination with other years for the plutonium, it's a no, never mind that, I don't think we necessarily know, but you know, one's gut feeling is well okay, that year is high, and are there some other years like that, or is that -- see I still think if we make the argument that it's associated with bringing in the 20 percent or whatever, yes, if you make that argument then you can sort of say okay, there's a period of time for which these higher default values may in fact be the ones that you use, and maybe you develop a model that --

I am just saying it seems to me that that is where they have to respond. That's new information and you sort of have to say okay, is this sufficient for us to modify how we are going to do dose reconstruction, or does it mean we can't, which is the SEC issue?

MR. STIVER: From 1980 on, you definitely have a sea change in the environment there, and so that really needs to be addressed.

MEMBER ZIEMER: Right, and it seems to me that the other issues are sort of less important.

MR. STIVER: Well the other issues, yes, they really relate to a lack of data and a lack of system-wide, agreed-to specifications and still, the chemical processes for concentrating and potentially exposing workers are still there in the early years. It's just that you don't have, as far as we know, this injection of plutonium with transuranics until 1980.

CHAIRMAN CLAWSON: Well when did they start receiving?

MR. STIVER: They started receiving -- the Paducah ash? Or the other --

CHAIRMAN CLAWSON: Well, the uranium, the recycled uranium. I thought it was --

MR. STIVER: Oh that was about in the early `50s when they first started getting the recycled uranium. The very first batch came in `53. There was a peak in the `60s and again in the 1980s.

MEMBER ZIEMER: But it seemed pretty clear that the earlier stuff, we know had a lower concentration.

MR. STIVER: Yes, yes, it would be kind of a stretch to -- you're not going to get the same kind of concentration you got in the tower ash.

Now in the 1970s of course there were several batches of tower ash and also incinerator ash which have also, this was that CIP/CUP program that Bob Alvarez was talking about.

So you have that period, you know, from the `70s you get kind of a build-up and then in 1980 you get a big spike and so you have that period that --

MEMBER ZIEMER: Well, I'm sort of interested in finding out whether the existing model still covers everything or has something changed here, are there two periods, are there three, or what?

And I guess NIOSH has to look at that. I don't know Mark, but how do you feel about that?

MR. ROLFES: Yes, that's -- I mean that -- our awareness of the tower ash that's coming to the Fernald site is one of the reasons that we defaulted an order of magnitude above what the controls were from the very beginning.

That's what it comes down to. We can certainly look into providing additional justification as to why we still feel that answer is not bounding, and if there is an exception for example, you know, it may be that the 10 parts per billion plutonium concentration on the uranium S basis is bounding for all years except for the time period where they received the Paducah tower ashes.

So maybe we would need to go back and maybe we could provide a -- it may be, like I said earlier on in the meeting, that the earlier materials could have been much less than 10 parts per billion.

So it might be that our dose reconstructions, by assigning 100 parts per billion, are certainly very claimant-favorable, and maybe it could be that the 1980s forward time period maybe for certain workers, the mass concentration or excuse me, the plutonium concentrations could be lower for certain operations.

We will see what we can do to look through our data that we have and also see what additional data is available to us.

But we can certainly do our best to research this more, so the more data that we go and look for though, the longer it is going to take, so we will focus on what we currently have and go back to the records and to DOE and see what additional information we can recover.

MEMBER ZIEMER: I wasn't

suggesting you go back and look for more data so much as saying does the current model handle the issue that was raised or not, and if not, how do we do it?

MR. STIVER: That's really the crux of the issue. Is the current model adequate for all workers in the SEC period and I think we presented a pretty compelling argument why --

MEMBER ZIEMER: I mean it may be one thing for a worker who has worked a whole span and it sort of averages out. It may be very different for a worker who started at that time.

MR. STIVER: Say the worker who was involved in one of these high concentration processes, from 1980 to 1986, when they started instituting the health protection measures that were really more robust at that point, so you have got that aspect of it as well.

CHAIRMAN CLAWSON: As the Work

Group chair I am kind of sitting here in a situation, are we also spinning our wheels, you know, it kind of seems like we have been at loggerheads for the last four to five meetings of -- on these issues and I am wondering if, you know, this is why I put this on for the Augusta meeting, because I want to start -- I want to get this before the Board, because I don't think as a Work Group here we are going to be able to come to -- there's an awful lot there, so.

You know, but we have got to be able to give NIOSH the opportunity to be able to respond to these findings and come forward here and --

MR. KATZ: I was just going to suggest, I mean, a part of this, for some of the questions that have been raised at this meeting and probably were raised before but were raised more elaborately in this meeting, I mean in terms of uncertainty of the data that you are relying on or what have you, I mean you may answer it by saying we can only, you know, sort of make any progress in resolving this uncertainty by getting more data.

You can simply -- that could be part of your response: that's the only way we can resolve it. Or you can say you know, we don't really need to go to more data to answer that question, and just lay it out as it is.

Then they are not hostage to another data capture or whatever, but they know what is involved, and if you -- and the Work Group can decide, they can say, look we are not going fishing for more data at this point. We are going to decide based on information that is available currently.

And then you are not sitting doing a lot of work that possibly may or may not move the ball forward.

MR. STIVER: Yes basically you need to say this is a tractable problem and here's some proposed methods that we could use to bring it to closure.

CHAIRMAN CLAWSON: Because this is, in my personal opinion, this is a significant SEC issue. Are we able to bound this with this, and with this presentation, you know, in my mind brings a question.

But also too, at the same time, it comes into timeliness. We have been at this for an awful long time.

But then, on the other hand too, any of the sites that I have seen or been involved with, here we have this large amount of urine data that is sitting out there too.

So it's a complex issue and I'm really having a hard time with how we are going to proceed forward with it. First of all we need to be able to allow NIOSH to be able to digest what has been presented here today and to deal with it, and decide which way we are going to go, and then it may end up just coming to the full board to be able to look at this and make their decisions from there.

MEMBER ZIEMER: Of course this was just the recycled uranium issue. There's several other issues.

CHAIRMAN CLAWSON: But this one, you know, I will be right honest with you, this is a big one, because we don't have much data. We have got some samples and so forth from other sites. We were playing with a lot of things back in there. I know that there was even some HEU that came into Fernald one time and that made a big mess there and it ended up the rest going to Oak Ridge and so forth like that.

But there was a lot of things that we were dealing with there. There was a lot of unknowns that came into this site, and I am just -- I'm really wondering which way to be able to go.

But anyway, that's the tasking for NIOSH. We will wait for that. We have got to be able to give them an opportunity to be able to respond to this, to be able to address the issues and as Ted put it, to let us know which way we are going.

Because we have been at this too long. It's not another big data capture plan, I'd just rather say enough is enough and go from there.

MR. ROLFES: One other clarification I guess I wanted to ask. We are basically relying -- our 10 parts per billion was bumped up to the 100 parts billion because of that tower ash material, that we are using the concentrations of plutonium reported by DOE in their 2000b reference.

And if you look at those shipments, the material balance data. Now correct me if I am wrong Bryce, we looked at that data and one of SC&A's concerns is that we didn't reanalyze the data ourselves, but we are relying upon a bootstrap mean analysis of those shipments.

Bryce, could you --

MR. RICH: That's correct.

MR. ROLFES: Okay.

MR. RICH: We did look at distribution and if you look at the chart for example, you can look at the distribution graphically and as has been indicated, some of the data looks like it's a log-normal distribution but there's a very wide spread, and it appears that the defaults that we chose were bounding the high values in all, but the gaseous diffusion plant POOS material.

MR. ROLFES: So do you have an idea of how many of those results would have been less than 10 parts per billion versus how many of the results or shipments would have been above the 100 part per billion default that we currently use?

MR. RICH: I don't have that number except to say that most of them are a bit off.

MR. ROLFES: Okay thank you.

MR. STIVER: Actually, if you go to -- we can argue about this I guess, forever, but Appendix F has the analysis here, so all these categories are low but you know, there are quite a few, 6c, 6e, f, and the -- particularly the magnesium fluoride, the incinerator ash, the tower ash samples are significantly higher.

And also, you know, the bootstrap mean is --

MEMBER ZIEMER: Higher than the --

MR. STIVER: Higher than, definitely higher than 10 and in some cases higher than the --

MEMBER ZIEMER: Than the 100.

MR. STIVER: Well, there's only one bootstrap mean that is higher than 100, but when you start looking at the spread in the data, and the log-normal means, and the uncertainty bounds on those log-normal means, they are significantly higher than 100, and they are certainly higher than 10.

You know, this is one thing we think that if you are going to really capture the upper bounds for all classes of workers, you have to do something other than, whether it amounts to an arithmetic mean for an incredibly diverse and variable and uncertain distribution.

Jim has laid it out in our paper very well, about the limitations of this data set. This is not the bible. This is a starting point. It's a framework that was intended to be built on beyond that.

This data cannot be used to justify 10 parts per billion in any way shape or form, for 100, and certainly not for all Classes of workers. And that is probably the biggest issue we have with the NIOSH methodology.

MR. ROLFES: I just wanted to make sure that we point out basically the control level was 10 parts per billion. We --

MR. STIVER: It was a production specification. It was not accepted throughout the facility. It could be changed on a matter of a phone call.

MR. ROLFES: Yes, with awareness, but basically we have defaulted to, we have gone from 10 parts per billion up to 100 parts per billion, and all of the recycled uranium, plutonium concentration data that we have looked at, only that one set of data essentially exceeded the 100 part per billion default that we currently use for dose reconstruction.

MR. STIVER: If you looked at the bootstrap mean, but if you look at the spread of the data, you will see that that -- it's significantly higher for at least three categories.

MR. ROLFES: Okay, so that particular fact then, doesn't necessarily make this an SEC issue. It's an application of what distribution.

MR. STIVER: Well actually it does, because there may be categories of workers that you can't -- the one that I think is really the most problematic is the Plant 5 metalworkers. You have got people, you have got concentrations making magnesium fluoride and a very low uranium content, and to tie that back to uranium bioassay is really problematic.

So there is a potential SEC issue there. There's the earlier period where you have no data. You are basing this off a production specification but you have no data on receipts, you know there is chemical processes that concentrate the stuff.

In my mind that's an SEC issue.

MR. RICH: And the magnesium fluoride process stream was still based upon parts per billion uranium, even though the uranium was --

MR. ROLFES: Well, it is in this analysis, but I am questioning the validity of that approach for that particular source of exposure.

DR. GLOVER: So this is why I was

asking about the -- making sure we frame the questions, because we actually, like, finding forward what you ask us to go back and review the data.

I think one of your main findings is that the source term coming in, does not necessarily reflect what the workers could have got because of chemical changes along the operations, and therefore we need to show that our data deals with that along the various steps and that in addition to the snapshot in time, that it is back-extrapolatable when controls throughout the DOE system were not in place maybe as well as --

MR. STIVER: Yes that is a good summary, that's a good summary.

DR. GLOVER: Is that reasonable? Because that seems to be one of your key points.

MR. STIVER: And if you read through the details of the report, this is all laid out there. It's just too much to try to present in one bit. As you read through it you will get a good feel for exactly why we feel this is a big issue and what possibly could be done to rectify it.

DR. GLOVER: And then you had another point, which was after the POOS came, you had outside external stuff at the edge of the boundary which is above what we found. Another clear thing that we need to make sure we -- maybe it's a separate point, so that is another clear one that our number didn't seem to be --

MR. STIVER: Yes, it's just this idea that you have got data within the plant that represent worker exposures that are above the defaults, and you also have boundary data that tend to verify that.

And so that kind of casts doubts in my mind on the bounding nature of that particular number that we selected.

MR. KATZ: We'll get an action plan from DCAS, which you guys can take a look at and say yes, this is what will move us forward, and then we will have agreement and it will be clear. What's to do we will be clear.

MR. STIVER: Okay I guess we can go on to --

CHAIRMAN CLAWSON: Number 4. This is review of radon data for adequacy.

MR. STIVER: There was evidently some confusion at the last meeting on this about the version of the report that SC&A had reviewed, and this gets back to the use of radon breath analysis data to -- as a mechanism for calculating the doses or the intakes from thorium-230.

And I guess the remaining issue there was whether -- what would you do in a situation where you have a thorium-230 that is depleted and radium-226, so you don't have a radon source to use to bound those or to even, to use as a surrogate for determining thorium intakes.

And I believe the latest version of the paper, the NIOSH White Paper by Bryce Rich as revision 7, called the White Paper on Thorium-230 and Other Associated Radionuclides, and dated January 6th, 2010.

And that is the latest version, Mark?

MR. ROLFES: That is correct.

MR. STIVER: And we did indeed review that and I believe Joyce Lipszstein, Joyce are you still on line?

DR. LIPSZTEIN: Yes I am.

MR. STIVER: Joyce is the primary author of that report and we asked her to go through and summarize our findings and at this point we have not received a response from NIOSH on our review.

So this will be mainly just laying out what our concerns are for the most part and discussing them. So Joyce, would you like to just go ahead and I'll turn it over to you.

DR. LIPSZTEIN: Okay. Thank you.

So the purpose of the NIOSH White Paper was to address the elevated thorium-230 concentration in areas as you say that we don't have radium-226 and so we cannot calculate intakes based on radon in breath.

In the White Paper, NIOSH proposes to calculate bounding intakes of thorium-230 based on intakes from uranium. So with respect to reconstructing doses in thorium-230, NIOSH White Paper presents a dose reconstruction strategy that takes advantage of the preparation of thorium-230 relative to uranium-238 and the changes in operations as a function of time.

Basically, NIOSH White Paper describes four different categories of areas where workers could have been exposed to thorium-230.

There was first areas where uranium and the uranium-238 daughters including thorium-230 and radium-226 are present, as for example the Pilot Plant, Plant

1, Plant 2/3, where the chemical processing of uranium ore took place through the three step process of digestion, extraction and de-nitration and Plant 8, the Recovery Plant.

Facilities that fall within this category are distinguished by the fact that uranium-238 and its progeny are all present.

SC&A agrees in theory that bioassay data that is providing for maximum concentration of uranium in the urine, can be used directly to estimate intake rate of not only radium, but also its progeny including thorium-230 and radium-226.

What we have to say is that NIOSH have to show us that the workers that worked in those areas did not perform jobs or did not spend time in the raffinates areas of Plant 3 for example, or the silos areas where exposure to uranium was negligible.

Because if you have the areas where there was no exposures from uranium, you cannot calculate the intakes based on the uranium bioassay data.

So we think that, for this study, we are waiting for further data from NIOSH, characterizing with respect to the work who was working in which area and if the workers rotated and how they rotated in time.

Now, so there are other areas like the raffinate areas located in Plant 3. In these areas, thorium-230 is present after separation from uranium. Radium-226 is present in some of the operations but not in all the processes conducted in the raffinate areas of Plant 3.

And then, what we see in the -- well okay, in the raffinate areas we had the hot and cold sides. There were two streams depending on where the resinate originated.

Hot resinates were those resulting from radium-containing oils, while cold resinates were radium-free.

And then these filtered hot and cold resinate streams were received in the combined resinate area, and the combined resinate stream was evaporated to obtain a concentrated methyl nitrate solution.

So in these areas, we have workers that are exposed to thorium-230 and radium-226, but they are exposed to insignificant quantities of uranium. So it is not possible to estimate uranium-238 or thorium-230 based on bioassay results of uranium-238, in areas where exposures to uranium were negligible.

And what NIOSH states is that in the resinate process, there was a -- the resinate process was essentially contained in a closed piping system and was not a source of significant exposure to workers in Plant 3.

And NIOSH concluded on this, based on historical DWE results. And this leads us to what we put in our papers, finding 3 and finding 8.

In finding 3 we have seen some papers saying -- there were reports by, for example, Wing, it's a 1958, 1959, 1962 and from Ross from 1968, on exposure of personnel from Plants 2 and 3, which is in-site pipe leaks.

So this contradicts the presumption that the high thorium waste observed in single-stream material is associated within a safely confined system, which presents little, if any exposure potential to workers.

And then also, we have looked very carefully at the DWEs that were given to us by NIOSH. It turns out that these data are not complete. They were not derived within a complete set of results taken during the whole years.

And for example, the area 3 DWEs in 1958, for example, are based on August to October sampling, and we have documents that show that the breathing zone as samplings for operators in the Plant 3 hot raffinates are much higher than the maximum permissible, the MAC, in the Plant 3 hot raffinates building.

So, and when we looked at the Appendix A results for Plant 3, they don't show this result. So we have all those listings of results that we show that many times the GA and the breathing zone samples in Plant 3 are much higher than the DWEs that are shown in Appendix A that were used to conclude that results in the raffinates area 3 are very low.

For example, NIOSH says that the DWEs in Plant 8, which houses the raffinates operations, were low essentially at background levels, and we found other documents showing that the DWEs were much higher and the breathing zones also.

Also, then we have another area where thorium-230 and radium-226 are present and there is no uranium exposure, which are silos area 1 and 2.

And there was a time that there were radium monitoring data for this period of workers, then radon in breath is available, then radium-226 intakes can be determined and thorium-230 also can be determined.

But there are other periods of time where radon in breath was not data, when radon in breath are not available. So we want to ask what -- how can we calculate the thorium-230 intake when bioassay doesn't make -- uranium bioassay doesn't make sense because there was essentially no exposure to uranium.

The same happens with silos 3 area, where thorium-230 is present in much higher activities than uranium-238 or radium-226.

So, we, I think we expect an answer to all those questions from NIOSH and I think in summary that's the problem. We agreed that bioassay uranium can be used to calculate thorium-230 intakes, if for workers that have worked solely on areas where they were exposed to uranium, thorium and radium.

But for workers that worked in areas that they were not exposed to uranium but only to thorium-230 and/or radium-226, then we need further guidance from NIOSH on how they are going to calculate the thorium-230 data. I think that's it. John? Did I --

MR. STIVER: Yes, I think that summarizes it. Evidently at the last meeting there was some confusion about which version had been reviewed and so I guess at this point NIOSH has not issued a formal response to our paper.

I would think that would be the best logical choice, would be for you guys to go ahead and put together a formal response for us. It's been a while since we talked about this, and I guess the issue of how to do thorium-230 in the situation where there has been depleted radium-226 and no radon breath data, is probably the key issue here.

MR. ROLFES: I was just looking back at your notes and I just wanted to make sure that you have submitted a paper. It says: White Paper on Thorium-230 and Other

Associated Radionuclides Rev 7. All right, we will take a look at that and get a response.

CHAIRMAN CLAWSON: So that's already been cleared to --

MR. STIVER: Yes. We produced that last June, I believe.

CHAIRMAN CLAWSON: Okay.

MR. STIVER: It's already been cleared and all. Okay, Bob Anigstein, are you online still?

DR. ANIGSTEIN: Yes, I am.

MR. STIVER: Okay, I realize you -- it's been a long wait for you.

DR. ANIGSTEIN: Okay.

MR. STIVER: Bob has followed up on the issue of the radon emissions for the K-65 silos and this particular issue has a storied history, much like recycled uranium, in that we had, over the past two Work Group meetings, over the course of action items that arose from that, SC&A has produced two different White Papers and NIOSH has issued response to those about the source term.

And we have provided a fairly comprehensive paper that really lays out our position on that. And I think at the last meeting, the Board had agreed that there was really nothing more to discuss on the source term but that what they wanted was an evaluation of whether the model used by NIOSH would result in bounding doses to workers on site.

And Bob Anigstein has generated a review and a very nice work-up that looks at that model and how it was generated and all the details of it and the implications of combining that with the source terms that have been derived either by SC&A and also in comparison with NIOSH's source term.

So Bob, at this point I'll go ahead and let you take over and the third -- I sent around a PDF file, which is Bob's latest presentation, I believe it's called Anigstein 3, and you should have that available. We distributed it. That's the most recent one.

DR. ANIGSTEIN: I also emailed it to everyone.

MR. STIVER: Okay, so everybody else has already got that, then okay.

DR. ANIGSTEIN: Yes, it's called presentations2.pdf.

MR. STIVER: Okay.

DR. ANIGSTEIN: The only difference is there was some formatting glitches in the early one.

MR. STIVER: Okay.

DR. ANIGSTEIN: Some symbols -- there's no substance change. It was some symbols didn't appear properly.

So shall I go ahead?

MR. STIVER: Sure, yes, go ahead.

DR. ANIGSTEIN: Okay. So if you start on -- slide 1 is just a title page -- start off with slide 2. I just listed the objectives for doing this calculation.

And we were specifically asked by

one of the Board Members at the last meeting to explain how NIOSH, explain the NIOSH methodology of data readouts.

And so we then went ahead and calculated the relationship between the radon concentrations and the emission rate, which is a term commonly called chi over q, chi is the concentration, q is the source term.

And then we evaluated the chi over q derived by NIOSH where we had some questions about it, and so we performed an independent assessment so that we could have a basis for comparison.

And finally, there were two reports by the Pinney Group, Dr. Susan Pinney from the University of Cincinnati, studying radon at Fernald. We were also asked to look at that.

Our finding, to start with the end, is that we find that NIOSH used an unrealistic model to calculate the atmospheric dilutions, otherwise known as chi over q.

It was not a model that is applicable to steady states from the extended structure, and it did not use the correct site-specific meteorological data that is available.

The results fortuitously were higher than the one that we calculated using a general site-specific model, using the substantive information applicable to the particular exposure conditions.

However, it was not high enough to compensate for the underestimated radon release rate.

As far as the Pinney studies are concerned, they do not validate the RAC model. RAC is the Radiological Assessments Corporation.

The RAC model prediction were actually used to calibrate the Pinney measurements, so you can't have a circular thing. You can't say we have to calibrate the measurements and then the measurements confirm the data, the model.

They did describe a Masters thesis, a Masters student from the University of Cincinnati who did some measurements, but that was in 1991, so they do not apply to the period of highest releases in 1959 to 1979.

And also, we could not -- I could not establish that even in that limited sense, the RAC model was validated. The information presented was not conclusive.

And finally, the Pinney -- I could find no indication that the Pinney work was used in actual dose reconstructions that had been done during the past year.

So going into greater detail, on slide 4, I am trying to explain, perhaps for people who are not familiar with this air pollution dispersion modeling, just a very, very, very quick tutorial on this model.

On the left, there is a model of an elevated release from a tall stack. So here you see the grey area, the plume, as it spreads downwind. The stack is correctly represented as a thin, narrow structure, so of a certain height. In the NIOSH model, they assumed a 10-meter height at the release point. It seemed rather arbitrary.\

And the plume gets wider as it goes away and you see the two curves in the middle of this grey area, a horizontal and a vertical one.

And this is the Gaussian plume model. It's assumed that as you go away from the center, either up or down or left and right, you get a normal distribution like this typical Gaussian curve, which you can see on the right, which is known as the bell curve, and the sigma here is a standard deviation and that is used to characterize the vertical, there is a sigma y, which is the horizontal, and the sigma z that is vertical, to characterize the dispersion of the plume.

The problem with this model, if you look on the next slide 5, here is a cross-section of the actual silo. Initially they stood alone, but even then they were wide, relatively low, wide structures, 24 meter in diameter, eight meter high plus the dome.

In 1964, mostly to support the crumbling concrete walls, or the concrete walls that were in danger of crumbling, they added an earthen berm.

So essentially now you have a little hill and just the tip of the dome sticks out, and the small drawing on the right shows you the size of the silo in the center, and this is the earthen berm all around.

So you can visualize the wind coming, blowing, let's say arbitrarily from the left in the drawing. The wind is going to come up to this berm, or even without the berm, it's going to come up to the silo and start sweeping above it.

It has to go somewhere. It doesn't just come to a dead -- it's going to go above it and around it. Then all these air streams are going to then meet on the other side.

So it is not true that a release will simply remain elevated, even the part that goes through that gooseneck vent that shows on top, which incidentally as shown, goes up and then it curves down again. So the actual gases, if there is any velocity, will be pointing downward.

So the whole thing gets mixed in together and you do not have an elevated plume and the guidance -- we did not just make this up -- the guidance from the Nuclear Regulatory Commission specifies, for steady releases from a structure, unless the stack is at least twice the height of all surrounding structures, which includes the structure that it is on, you cannot treat it as an elevated release.

And even then it's only the certain gas -- velocity of the effluent vertical blocks or the effluents.

So the appropriate way to treat it

would be a ground-level release with additional dilution due to the mixing, there's mixing in this because of the size of the building.

The next slide, 6, is taken out of that same Regulatory Guide and this also appears in the NIOSH TBD, a very similar drawing, showing how sigma z, the vertical dispersion, the one I am showing here, varies with the stability class.

Stability class is just the dispersability of the atmosphere. So F or G stability classes mean the plume is very tight. There's very little turbulence, you have a smooth flow of air, very little turbulence.

And as you get up to A, the atmosphere is more and more turbulent. The sigma z is much larger and the plume gets dispersed much more quickly.

But interestingly enough, this works in the opposite direction if you have an elevated release, because then a narrow plume means if you are near the stack, it passes right over your head.

There is virtually no effect from the ground, whereas, let's say with class A, there would be much more hitting the ground.

Then the next page 7, table, these are data copied from the TBD and it shows that for ground-level releases, which NIOSH did use, but only for a very limited source term, it's only for the K-65 material that was stored in drums on the pad in Plant 1, near Plant 1, and was only there for two or three years -- `52 to, middle of `52 to middle of `54.

So there it would be appropriate to use the ground level releases. For the silos, they used the elevated release, which was by far the more important term.

And if you go down the table, particularly where it says elevated, you go down and under the TBD column, you see that it starts off extremely low, then it reaches a maximum at about 500 meters, actually 550 -- yes, 500 meters. Sorry. And then it starts to go down again.

That's because first the plume hits the ground and then it gets more and more diluted as it goes further away.

And we calculated these numbers just as a QA check using the NIOSH formula, and I got different results. I don't know why. I think there was an error. We checked on this several times.

So we get much higher -- even using the NIOSH model we get much higher values up until you get to about 400 meters, then it becomes essentially the same. So I think there is a calculational error there.

It doesn't really affect the results because they only use the numbers from 250, starting with 250, and they were off -- the discrepancy is a factor of two. I think it's something that NIOSH should look at to see where these errors crept in.

Going on to slide 8, this is taken straight out of the TBD and is showing the NIOSH method, which is I simply added the red, to show the location of K-65 silos, and what they did was, using that elevated release model, calculate the chi over q at each location of each of these 11 -- exposure areas, they called them, one of which actually includes the 16 K-65 silos.

And they used that, then they took, on the next page, slide 11, you see those are wind rows, taken from a later year, but probably not very different, the year 2000.

And they simply multiplied, they calculated the chi over q for each exposure area by the frequency that the wind blows in that region.

But curiously enough, they did some summing, because apparently if the exposure area fell into more than one compass direction, which took the 16 compass directions and if it's wider than one sector, they actually added to this.

So if the wind is blowing simultaneously to the northeast and to the east-northeast, for instance. Anyway, that is the way it was done.

The commentary on page 10, the summation of our take on this, is that there were -- on the one hand they were underestimated because they used an elevated release.

Secondly, the wind speed, which was based on an accident analysis done by Parsons for some thorium redrumming, and Parsons simply used the Cincinnati area average wind speeds, which came out to -- they quoted it at 7.1 miles per hour, which is 3.2 meters per second.

The actual wind speed at the Fernald site is 2.1 meters per second. This can be verified by looking at the wind rows on the previous page and also on the next page -- getting a little out of sequence here -- we actually have the detailed data.

We have five years' worth of measurements done on-site between 1987 and 1991 of hour by hour, of wind speed, wind direction and stability class.

And that is the data that should be used in an alternate on-site analysis, and we get 2.1 instead of 3.2 meters per second.

On the other hand, they overestimate because they assumed that all year long, you have the worst atmospheric stability, almost a class F, rather than looking at how the atmospheric stability changes, hour by hour, day by day, month by month.

And they also make the unrealistic assumption that the receptor is always dead center, on the center line of the plume.

And that is appropriate for an accident analysis. If you are doing the a priori accident analysis, the accident hasn't happened, but what if an accident happens, and it would be a short-time release, then it's appropriate.

You don't know which way the wind is blowing, so you assume the worst case. You assume that you have a receptor, somebody nearby, you usually have the sense lag because you are doing off-site impacts, and the weather is blowing straight at him, and then you have -- and you say class F because that is about as bad as it can get.

But that is not appropriate when you have a year-long release, steady and going on year after year.

So therefore, either you don't use the center line, you just use the general direction of the plume and I will get to that in a moment, how that's done in a moment, and you use the actual stability class.

So on the next page, page 11, is the data that I was referring to. This is --

there are six such tables and they would take much too much room so I am just showing one for the class A.

And this was data that was the hourly data that was then used by the RAC team to compile what is called a joint frequency table and this is something that is used consistently for -- at nuclear power plants when they have to report their releases and the impact on the surrounding environment.

And so you see the first number in the upper left-hand corner, the wind blows from the north and the wind velocity between zero and two meters per second, is 0.005, half of one percent is in that direction, and so forth for the rest of the table.

And then this is, assume that it's always class A and then you multiply each of these by the class A frequency, which is on the top, or about six percent, and you get the actual frequency, we call it joint frequency.

You get the frequency of a given

wind direction, a given wind speed, and a given stability class. And with all of that data you can do a very site-specific, accurate analysis of chi over q.

And the equation to do that is shown on 12. This was again taken, based on this US NRC Regulatory Guide.

And it is simpler than it looks. What it really means is that you start off after the big sigma. These are just the numbers I was showing on the previous page.

The frequency of a given stability class, a given wind speed and a given sector, meaning 16, one of those 16 compass directions.

And you divide by the wind speed and you divide, which you always -- which we just take as the middle of each range, and you divide by this calculated sigma, and then to the left, you multiply, you divide by x, which is the distance in meters and this factor of 2.03, which I won't go into.

And then the sigma, as it is called, it has a sigma prime. And the sigma prime takes into account the building. So it's your sigma taken off of that curve and then you add a number that is sort of roughly related to the cross-sectional area of the building, not exactly. The square of the height divided by two pi, that can be a prescribed number.

But it's always less than the square root of three times the lesser of these two, of the second and third line.

So this is the regulatory approved way of calculating these releases. So how have we applied this model to the event site then? You took the map of the site on slide 13, on the left. This is simply a drawing taken from the TBD but I added the red outline to simply have a simplified mathematical model, because I didn't want, I couldn't follow every single turning, kind of left out that upper right-hand corner just to simplify the model.

It's well away from the silos, so it wouldn't have much of an effect.

And then on the right, this is generated by the computer program. This is an Excel file taken out from the resulting computer program, which shows we reproduce, nicely reproduce the outline of the site.

We show the little rectangle in the middle, that's -- I just pasted that in, just a representation of the silos. The red line is part of the computer model and that is the security fence around the silos.

Nobody is allowed inside that fence, so we didn't model that. And the small, little fine blue squares, these are your 9,586 locations of possible places where a worker could be and the chi over q at that location, given the site-specific direction, speed, and speed of the wind and stability class.

So we assumed the source is the center of the silos and these are each of -- and each of these locations were calculated separately.

And the result was we took, on page 14, under SC&A, this is the arithmetic mean of those 9,586 locations, and the 95^{th} percentile makes no assumption about distribution, it simply ranks them in order and takes the -- starting at the bottom, the 95^{th} percent high value is the 95^{th} percentile.

On the left, NIOSH did not -- the TBD does not give the chi over q -- it gives the chi over q at each of the exposure areas. It does not tell how they were combined. We actually could not reproduce the numbers, it just said it was the 95^{th} percentile of the distribution.

It is a little confusing. I think the geometric mean originally. So I take back what I just said.

But I could not reproduce the numbers. But nevertheless, what we could do is simply take the number, the exposure assigned in working level months for each period of time, and there is a conversion factor which was presented in the report, that was standard.

So you divide by that conversion factor to get the concentration and then you back-calculate a source term, a release rate, and you end up with a chi over q.

So we took a little indirect calculation but it was based on the data presented. So as I said, they have a -- their mean is about a little over twice ours, their 95^{th} percentile is 1.5 times ours.

So the argument is not so much with the numbers but with the method. So our position is, finding position is that their methodology is unrealistic, page 15, does not use the appropriate site-specific meteorological data and it is potentially overstated by roughly a factor of 2.

On the other hand, their estimate, and this goes back to our previous White Paper, so this is not new information, the estimate of the radon emissions from the silos fails to account for the deficit of the lead-210 with respect to radium-226.

Had there been no radon whatsoever, the two would be essentially in equilibrium and actually the lead-210, having a shorter half-life, will be something like 1.4 percent, if my memory serves me, higher actually than the radium.

Instead, it's lower, it's somewhere around the order of a 50 percent deficit, just round numbers, from memory.

So this potentially underestimates the release of radon by about an order of magnitude, depending on the temperature, it could be a factor of 10 or 20 lower.

However, the question we were asked is: well, do we believe that the radon concentrations can be bounded? And the answer is yes, we can, we do.

One reason we went through the exercise of calculating chi over q is to see is there an acceptable method of calculating it and we found at least an acceptable method.

So we believe that you can calculate the historical source term that accounts for the deficit of lead-210, with respect to the radium-226, account also for the other sources, the drum K-65 waste, which NIOSH did account for, and the Q011 silos which was something new that was uncovered by the Pinney study when they found unexpected radon -- a history of unexpected radon exposure and interviewed workers and found it had been smaller silos but they were nearer to the production buildings, so they actually resulted in higher radon concentrations during the early years of operation. They were only there for a few years and I forget exactly what the years were.

And then if they use an acceptable and appropriate model with respect to the data. It is possible to create, to have bounding radon exposures.

We were also asked to discuss the Pinney reports, because they were -- NIOSH had cited them as another source of information.

So here is another very quick didactic, going to page 16. This has a couple of formatting errors, where you see the a with a little accent mark, that was supposed to have been an alpha. It got garbled.

I hope nobody minds a quick physics lesson. What you have -- we are showing the right just of the K scheme, of radon-226. If you go down, a little over half-way down that decay chart, opposite where it says 130, which is the number of neutrons.

You see polonium-214 and this is an extremely short-lived nuclide, has a half-life of 1.6 times 10 to the minus 4 seconds, like one sixth of a millisecond, and it decays by alpha emission to lead-210.

So on the left is a little picture of this. So you have -- what you have is the alpha comes out with certain energy, certain velocity which gives it a momentum, and what you have is a recoil, just like if a rifle or a pistol fires a bullet, the bullet goes forward, the gun goes backward. Anybody who has fired a gun will realize that.

So in this case, if it happens that the lead-214 -- actually the polonium-214, which is the parent nuclide now, happens to be attached, sitting on, very close to a pane of glass, and if the alpha shoots out in the opposite direction, the remaining atom, which is now lead-210 --polonium-214 minus an alpha becomes lead-210 -- so the remaining atom now gets shot into the glass with a certain force and if it goes in the right direction and if it is close enough so it doesn't bump into too many air molecules on the way, it can become embedded in the glass.

So this is the basis of that analysis done by the Pinney team. And then, so what they did was they took this -- there is this plastic film called CR-39 that is useful as an alpha detector. They pasted it on the glass, both on the inside and the outside, and left it on for a couple of weeks.

So in equilibrium with the lead-210, lead-210 is just a beta emitter so it doesn't -- it won't show up. But the polonium-210 is its daughter product. It will be in equilibrium.

And it is another, the last alpha emitter in the chain. So the polonium-210, the alpha from polonium-210, will cause like a defect in this film, like a little groove so to speak. Basically they leave a track, on a photograph it's going to be called a track.

So they take these films and send them off to a lab in England, it was the one that originated this process, and they will come back and tell them how -- basically how many polonium alphas there were per square centimeter, and then if you do some very elaborate calculations, you can figure out how much lead-210 there is in the glass, and therefore, how much radon it had been exposed to over the years.

Now the limitation of this process is you have to -- these processes have been developed and used for assessing radon exposures in homes, in epidemiological studies to try to relate incidences of lung cancer to the radon concentrations.

One thing they did was, let's see if we can figure out over the years how much radon was in this home on average. And they would put this film, and they did it both on the insides of the windows and over glass covering, picture frames.

But there, they had the assumption that it was a steady situation, that it didn't change year by year, and also, they had a test chamber.

So they would have a -- the glass would be exposed to a known radon concentration and they could use the same film to see what the lead in it was.

So they had something, basically they had a calibration so they could relate the readings on that CR-39 film to what was a known radon concentration, and then you can say okay, this home has twice as much, it has half as much, you could do a straight ratio.

They didn't have that for Fernald. The process by which the lead-210 goes into the glass is very different in an indoor environment and an outdoor environment. The source term is not steady, and we know the barriers over the years. For instance, there is a very big difference if the lead-210, was it positive a year ago or 45 years ago? Because that is two half-lives of the lead and so it decayed about 25 percent as much.

So if they don't know the concentration history, they can't do it. If they don't know -- basically, they don't know the concentration and so they can't do it.

So they used a RAC model. They had

somebody from the RAC team. They hired him as a consultant, to do the calculations for them, and then to give them a history. This is the radon concentration year by year for each of these buildings.

And from that they say okay, now we know how to calibrate the film. So that's fine for their study, but that does not validate the -- the RAC model validated their CR-39 film detector. So you cannot then use the same detector to validate the RAC model. That would be circular reasoning.

The additional data they furnished was this 1991 measurement done by a Masters student. First of all we could not verify this because we could not obtain -- we asked, true, we did not independently, we had very little time to do this actually, we did not independently contact the University of Cincinnati to obtain the thesis. So NIOSH did not have it available.

DR. GLOVER: Is that Cardarelli's

report? Do you really want the data?

DR. ANIGSTEIN: I don't know, at this point --

(Simultaneous speakers.)

DR. GLOVER: Do we really need it?

DR. ANIGSTEIN: Well, at this point, I will defer the answer to that question.

DR. GLOVER: All right. Okay.

DR. ANIGSTEIN: I will put off answering that question. I will defer to John Mauro, project manager, to see if we are going to continue anything with that.

DR. GLOVER: Okay.

DR. ANIGSTEIN: But even so, what I did look at, there were these two Pinney reports, the 2004, which was a report to the project sponsor, which has to be NIOSH, and then 2008, there's a journal article.

And the way they cited the data, which, by the way, I was told I cannot mention the student's name, I think it was just mentioned, the way they reported it was inconsistent between the two reports, and we could -- and even, and we are not convinced that it really validated the mean.

It was inconclusive. But more important, even if it did validate, even if it was in the ballpark from 1991, it doesn't tell us anything about the 1959 to `79 period, before the dome was sealed.

So that's the size of it.

MR. STIVER: Well, thanks Bob. That's a very thorough and succinct presentation.

DR. ANIGSTEIN: Thank you.

MR. STIVER: I think at this point we have -- SC&A has done basically all that we can do. I think we have addressed the source term in our previous White Papers, and Bob has laid out in very crystal clear detail what the issues were regarding the model, and the implications basically that, in the worst case, it looks like we have got factor of two overestimates, but the chi over q combined with an order of magnitude, potential underestimates of the source term, which would result in underestimates of about a factor of 5 in the first order of approximation.

Something else that we really need to bring up, though, is that despite all this, this particular issue really in our opinion does not rise to the level of an SEC.

And I believe Mark had brought up at the last meeting that I think almost all the lung cancer cases are compensated on the basis of uranium alone, and I think there was only a handful of cases where radon is --

DR. ANIGSTEIN: I can add something to that.

MR. STIVER: Okay.

DR. ANIGSTEIN: I had noted that from the -- I wasn't involved in the last meeting, but I did read the transcript.

I went back and picked out the cancer diagnosis code, I believe it was 1.62

was the one for the --

MR. STIVER: 1.62 sounds right.

DR. ANIGSTEIN: -- for lung. So I did a query on all the cases from Fernald that had been processed in 2010, figuring it would give me the most up-to-date result, and there were 12 such cases that had actually been processed and of these, 10 had been underestimated, meaning deliberately underestimated.

They left out the environmental exposure. And then they were compensated.

MR. STIVER: An expedited case.

DR. ANIGSTEIN: Pardon?

MR. STIVER: Yes, they called those efficiency methods where they --

DR. ANIGSTEIN: Yes, exactly. Thank you for clarifying it for other people. Right, they used the efficiency method of underestimating the exposure and they found that the cases could be compensated primarily on the basis of uranium intake.

And there were two others, there were only two where the radon calculation was actually made, and it did not -- the only way it used was it took the exposures and working level months straight out of TBD, I believe it was labeled 4-12.

So there was no indication, perhaps there was some confusion where it appeared that NIOSH was using the Pinney data to do dose reconstructions. But there was no indication of that.

I just wanted to sort of throw that in.

MR. ROLFES: I was just going to say, from the beginning we have said that we would use the Pinney data for dose reconstruction, and actually last year it was actually SPEDELite linked to all Fernald claimants' files.

So there is data now from the Pinney model in the NIOSH OCAS claims tracking system and what we would have to do for any claims that used a previous model that had a lower internal exposure assigned, we would go back and look at any claims under 50 percent Probability of Causation, under a Program Evaluation Plan.

So we would have to take a look at those two cases to determine whether in fact we would need to reevaluate the radon exposures, because it may be that the Pinney model for those particular years actually resulted in lower radon exposures than what we assigned.

I'd have to take a look at the specific --

MR. STIVER: You'd have to look at the PoCs.

MR. ROLFES: Yes. Yes.

MR. STIVER: And all of the specifics.

MR. ROLFES: But for the -- there's you know, 90-something percent of the respiratory tract cancers from the Fernald site have received compensation, and it's typically from uranium exposures alone.

MR. STIVER: Do you recall off the top of your head what the magnitude of the doses from radon might be, compared to, say, over a given year for --

MR. ROLFES: From the Pinney model, the highest were from the earlier time periods, from the Q-11 silos, for basically the Q-11 material in process. That was for the years up until 1958.

And then from `59 forward, the working level models dropped pretty dramatically. And there's ranges reported in the Pinney -- I can probably pull some up if you like.

MR. STIVER: No, I was just curious.

CHAIRMAN CLAWSON: So basically, with this K-65 issue, we have had to deal with this for quite a while. This is -- so we have come to the determination that we can bound the dose using whichever --

MR. STIVER: Yes, I think the issue more is that the scientifically valid models and the source term are being used. That would be the issue of going through and doing a program evaluation on the cases for which there may have been an impact on the claimant side.

CHAIRMAN CLAWSON: Well, this could be more of a Site Profile issue. Okay. So I guess, Mark, you have just stated that if we do have any of these in any of the does reconstructions, that you are going to have to reevaluate it or --

MR. ROLFES: Yes, that's something that we have done. Very early on, you know, we started off with efficiency methods for our dose reconstructions to get as many claims that we could out with, you know, worst case scenarios that we would assign.

And you know, we may have to go back and look at some of the -- and, excuse me, for a lung cancer case, you know, if an individual was bioassayed, we would use that uranium bioassay data to calculate an intake and the resultant dose to their lung.

Usually that's enough to put them over 50 percent. However, there are some cases where that doesn't happen, and those typical types of cases may be individuals who were employed on site for a matter of days or weeks and didn't have much exposure potential, or the other -- these are two, you know, this isn't an all-inclusive type of explanation but these are a couple of examples of why someone with a respiratory tract cancer may not have received compensation.

The other would be, the type of cancer may not in fact have been a lung cancer, sometimes like a mesothelioma or something for example.

It's associated with the lung, but it's in the spacing between the lung and the chest wall so it's not lung tissue.

MR. STIVER: It's not respiratory tract cancer.

MR. ROLFES: Correct. Which may be perceived as a lung cancer, but is not technically a lung cancer. It's a different type of tissue, different location.

The other thing is the latency time period between the exposure and the date of cancer diagnosis, and sometimes there are individuals that don't have more than the required five years of latency between their exposure and the diagnosis of a solid tumor.

So there's several reasons, and we can look into, you know, if you would like an explanation for the couple of cases that may not have been compensated with uranium plus any other exposures that we assign in addition to the radon exposures, then we can look at those.

But we always do -- continually, when dose reconstruction methodologies change, we do go back and look at those cases that are less than 50 percent, based upon new data. If we receive new data, then it is considered.

MR. STIVER: I guess I have one question mark. In regards to the program evaluation, would this involve a rewrite of the model itself and the process that would go into the procedures then as well?

MR. ROLFES: It would ultimately come down to the significance. It may be that -- we would have to see if there are a significant number of claims that are affected first, before we --

MR. STIVER: What is the threshold for that, for triggering a revision of a document, of a basis document?

MR. ROLFES: Off the top of my head, I don't feel -- I'm not involved in the program evaluations typically, so, but if it's -- if we receive new data that warrants, you know, we can get you an answer for that if you like.

MR. STIVER: Okay. Yes. I guess

the next step would be to go through the evaluations and determine what number of cases, if any, are affected.

CHAIRMAN CLAWSON: And this has basically become a Site Profile issue. We have got -- Item Six is -- can we take a 10-minute break and --

DR. BEHLING: Brad, can I make a comment before you take a break? This is Hans Behling.

CHAIRMAN CLAWSON: Sure, Hans.

DR. BEHLING: Yes, I was listening ardently to Bon Anigstein to elaborate this discussion about chi over q, and I can only assume that his testimony will play a part in this.

But I have not really heard what NIOSH really intends to do with regard to the issue that is a much broader and larger issue, and that is the two White Papers that I authored that identify a source term for the K-65 silos that may be as much as a factor of

20 higher than the 5-6,000 curies that have been assumed as being the least quantity, prior to the 1979 remediation project.

In other words, for the early years prior to 1979, my calculation would suggest, and I have shown that in two White Papers, that the source term for the radon releases may be a factor of 20 times higher than the assumed value of five to 6,000.

Now if there is a PER, will NIOSH actually then make use of those revised release estimates and incorporate that into the other factors that Bob Anigstein had identified with regard to the changes that might have to be applied in terms of chi over q?

MR. ROLFES: I guess ultimately it will depend if there's any claims that would be affected by this, and you know, the discussion of the source term is a slightly different issue because we are not using that model per se anymore. We are using the Pinney exposure model. So --

DR. BEHLING: Well, I hate to interrupt you, but I think what was clear in both the report that I had offered -- in fact there's a third report which was never released.

The problem is both the Pinney model and the TBD-4 approach model that is identified in the Site Profile, they both suffer from the same problem. They both used the RAC 1995 source term as a starting point, and then they simply made it a chi over q approach for modeling the actual concentrations that individual workers were exposed to.

But in both cases, the errors that I see that have not to be -- has not yet been addressed in this discussion, is the fact that both models have that same error, in other words, prior in 1979, during 1979, the assumed releases from the K-65 silos was 5-6,000 curies, which I have shown are likely to be an error of 20-fold, and that is something you cannot ignore.

MR. ROLFES: Well, I think we disagree with your assessment, Hans, and I think we have documented the reasons that we disagree. I mean, that's, I think, as far as we can go. We have provided our basis and you have provided yours.

DR. BEHLING: Well, I think there's a gross misunderstanding, because from what I recall, and this is a discussion that John Mauro and I had, he came to me and said, you know, they have basically conceded that your estimate prior to `79 was potentially twenty-fold higher and now I am hearing that you are disagreeing with it and you are basically backing away from that, and I think I want to have this on record.

MR. ROLFES: I think I just stated that we disagree with your assessment, Hans, that we provided some evidence, essentially, and some pretty detailed research projects that documented worker exposures in quite a bit of detail, and --

DR. BEHLING: I just don't believe that I am going to accept that explanation because this is what we have been talking about for the last three years, and I think this needs to be aired, and I'm going to ask John Stiver and John Mauro to make some comments to this.

MR. STIVER: Yes, Hans, I believe you are right. I don't have the transcript in front of me at this moment, but that was what I gathered from our discussions at the November meeting, that the source term had been accepted as flawed and that ours was going to be utilized. I thought that would be a part of this overall PER process, would be to evaluate the terms of both the atmospheric dispersion model and the source term.

DR. ANIGSTEIN: Perhaps I could -- this is Bob Anigstein again. Perhaps I could interject something at this point, which again, going back to my discussion, and that is -- please correct me if I am wrong.

What I am hearing from NIOSH is that they are treating the Pinney data as an independent source of information that is separate from the RAC calculations of the source term. Is that correct?

MR. ROLFES: No, but the Pinney data also has additional information in it that the RAC source term didn't really consider, and that's the Q-11 silos for the earlier years.

So ultimately we are taking, you know, in addition to the RAC source term, we are also taking the Q-11 silo data and we had individualized exposure estimates based upon very detailed analyses and worker interviews which positioned those workers in the worst case location of highest concentration if there was uncertainty as to where they were working on site.

DR. ANIGSTEIN: I read that report

very carefully. But the point is, the concentrations, that's fine, I mean, I have no quarrel on how they assign doses, how they assign locations.

But that data still is based on the RAC model. There, the measurements that they made on the window glass, it's my impression, and I'm not an expert on this and of course, you see a journal article, you don't see their notebooks and every detail of the calculation, but that they used the RAC model to calibrate their method.

So if the RAC model is incorrect, the calibration is incorrect. And the validation, also the RAC model, without meaning to be disparaging of it, has several -- besides the fact that it does not account for the lead-210 deficit, which is very large -- now whether every one of those atoms of radon got out into the air, you know, it could have been held up somewhere, it could have been held up in the walls of the silo, but not to that extent.

It just seems much too large an amount to have been held up, and also, as was pointed out actually by Hans Behling in the earlier paper, and also the RAC report itself acknowledges it, the fact that there were early readings of gamma exposure rates on the roof of the silo that showed relatively low exposure rates compared to what was later found, after the ceiling, indicates that the radon was not held up in the dome.

If the radon had been held up in the dome for many days, had decayed in the dome and perhaps the lead-210 did not go back into the raffinate but was plated out on the surface where samples were not taken, so you could say okay, this accounts for the fact that radon was transferred from the raffinate to the head space. It decayed in the head space and therefore you did not see the lead-210 in the raffinate.

However, it did not decay in the

head space, because it didn't stay there or it would have given rise to much higher gamma readings.

So there is this -- and the way the RAC model got calibrated against some on site measurements, they used so many arbitrary parameters to make it fit, that if you throw in enough parameters -- I mean, the model that I show, which I take no credit for, it is copied straight out of the US NRC Reg Guide, it's based only -- it makes no assumptions -- based only on measurable data, meteorological measurements and measurement on the building.

The RAC model is a very convoluted model. I tried to understand it and I honestly could not. There's probability distribution thrown into it, not a probability of it coming out of it, but a probability solution inserted into it.

It has all kinds of parameters to enable it to fit the data. Well, you have enough parameters, you can fit any data.

DR. BEHLING: And Bob, let me also bring it back to a single point that has been brought up over and over again, that relates to the November 2008 White Paper, the first one I wrote, and subsequently to the April 2010 second White Paper.

And the fundamental issue here is this is so, as far as I am concerned, so compelling as evidence. In 1987, the dose rate measurements were so high that they installed a radon treatment system.

That system, by and large, was able to evacuate the head space air at 1,000 cubic feet per minute. It was operated for three hours continuously to the point where essentially all of the air that had been accumulated had been vented, to the point where less than three percent of the radon was remaining in the head space.

When you look at the dose rates on top of the silo prior to 1979 and look at the dose rates after the radon treatment system had been operating for a full three hours, they again identified a dose rate that is virtually identical, meaning that the natural ventilation rate prior to June of 1979, had in essence the same effect as the radon treatment system that was run for three hours at 1,000 cubic feet per minute.

Now if that doesn't comply with the understanding that there was no hold-up in the head space and no decay, then I don't know what else would.

And to me, that is the compelling evidence that says that radon was, in fact, prior to `79, released into the environment with no hold-up and no deposition in the head space or anywhere else, and that accounts for this equilibrium between radium-226 and lead-210.

And if that doesn't register with anybody, then I have to say, then there's nothing left to argue here.

MR. ROLFES: I think what NIOSH

can do to move forward on this issue maybe is to go and look at the cases where the Probability of Causation for a respiratory tract cancer was less than 50 percent, and look to see if any change in the radon exposures that are assigned would put that case over 50 percent.

And that would be one of the things that would trigger us to do a Program Evaluation Report.

And if we are concerned about pre-1979, since the silo was capped in 1979, we can focus our efforts on that time period, the earlier years from `51 to 1979. Does that sound like something that, you know, would be satisfactory to everyone?

We don't want to go and do, you know, I mean to revise a model is going to take a significant amount of effort for a low number of claims, and it's going to cost a significant amount of money for this work to be conducted.

We don't want to go and do that work if there are no claims that would be affected. It wouldn't be responsible of us. So.

MR. STIVER: I guess the gap here is that you may have a model that is, I am not saying this one is necessarily, but if, hypothetically, you have a model that is just wrong, and it is giving you bad results, but you come to find this out after the fact, but yet there are no more claims coming in for which that model would be applied, what then happens to that discredited model?

Is it just -- is it rescinded? Is it altered? Is there some sort of statement that this was done incorrectly, and the results that were based on it are no longer valid?

I mean, what kind of closure do you get on a situation like that?

MR. ROLFES: Well, right now what we would do is basically put a statement into the Site Profile that says, you know, we wouldn't be using this current approach in the dose reconstruction methods. We would use the Pinney data, which is linked to each individual claim.

DR. ANIGSTEIN: If I can interrupt --

MR. STIVER: Please, Bob.

DR. ANIGSTEIN: I keep hearing the same thing over and over and over again about the Pinney data. SC&A does not agree that Pinney data can be used any more than it agrees that the RAC model can be used.

MR. ROLFES: Right, but my point is that if no claimants' dose reconstructions would be affected by a change in the model, then it's not worth revising the model.

DR. ANIGSTEIN: I understand.

MR. ROLFES: Okay. And what I propose to do is to look to see if any claimants would be affected, and we would plan our path forward from there.

MEMBER ZIEMER: Well, aside from that, though, I don't think NIOSH has agreed that the Pinney model is invalid.

MR. ROLFES: No, we haven't, didn't reach that conclusion.

MEMBER ZIEMER: So I mean, the assertion that it is doesn't have any more weight than the assertion that it isn't. I think you presented your evidence and they have theirs. If that model needs to be used in the future, then that may be an issue.

But part of that revolves around how it was calibrated and I am not sure we know how it was calibrated. Bob, that seemed to be a fuzzy part of the argument. You're thinking that it was circular calibration --

DR. ANIGSTEIN: Well, according to the report, I think they say very explicitly. They hired, I think his name was Killough, as a consultant. They took certain buildings that were, they said, far away so they would not be affected by the Q-11 silos, and they would be only affected by the K-65 releases and the natural background.

And they calculated the -- they had Killough calculate the radon concentration history. Then they had their health physicist calculate the decay rate, so how much was deposited in year one, how much was deposited in year two and so forth, assuming a constant deposition fraction.

And then they took those window panes and said okay, here is our calibration standard. And now we will apply this to other window panes where we have not done this calculation, which simply sounds to me like an interpolation procedure.

Well, we don't want to ask Killough to calculate for every single building, so for the buildings in between we will ratio it based on the window pane measurements.

But there is no absolute measurement of radon exposures using the window glass. They don't claim to have made one.

So if you don't accept the RAC model, you can't accept the Pinney data. If you accept the RAC model, you don't need the Pinney data.

MEMBER ZIEMER: Well, it seems to me, Mark, that would be an issue one way or the other. It's sort of this thing, are you going to keep it on the books, even if you don't use it, or are you going to declare it to be invalid?

I mean, I'm not even sure you should be going back and looking at all those other cases. I mean, if you can show that the Pinney data is okay to use, then let it be.

If you can't, then --

MR. STIVER: Then it needs to be taken off the books. Yes.

CHAIRMAN CLAWSON: But what about, you know, we keep missing the source term on this. Now, if the Pinney report is good, then it doesn't matter what the source term was?

MEMBER ZIEMER: They are saying the calibration in essence is circular. You are using the data that you want to study to calibrate it and then you are going back again, so, I understand the argument. That may be a very well and good argument, unless they somehow have isolated their data.

MR. STIVER: Unfortunately, these two issues were separated at the last meeting. There was disagreement on the source term. I've got it right in front of me here on page 329 of the transcript.

MEMBER ZIEMER: I was checking my notes.

MR. STIVER: It's a statement by Mark Griffon. He says, "What I'd like from SC&A is the position of what you had just discussed with John, and there might be a difference on our acceptance of source terms, but what's our position on the ability to bound considering the approach Pinney used to the data." Basically --

MEMBER ZIEMER: Hans raised the same question but I don't see anything that says NIOSH accepted that.

MR. STIVER: We never accepted it.

DR. ANIGSTEIN: This is Bob. The response which we processed which was between John Mauro, John Stiver and myself was that if, here in the example of what we considered an approach, if we had a release rate which was consistent with the lead-210 deficit in the silos, and if we have an atmospheric dispersion model that is site-specific, it doesn't have to be exactly what we did, we made some arbitrary decisions about, for instance, the worker can be anyplace on the site with equal probability, maybe there are areas where those workers would never spend time, waste disposal areas or something, it's just a simple one.

But if we used those two things, then we will conclude that yes, the releases can be bounded and it is just a matter of detail of how it is done.

But if we -- if the insistence is on sticking with the RAC source term and with the Pinney data, then our opinion is that NIOSH has not demonstrated that the ranges can be bounded.

I mean, we think they can be.

MR. STIVER: But to do that they have to have a model that is validated and scientifically robust, for lack of a better term.

MR. KATZ: It seems to me that it still devolves to a TBD issue, either way, whether there remains disagreement on the source term, or whether DCAS decides that they agree with you about source term, it's still a TBD issue.

I think just the way to move this forward is to -- if we don't have an in-writing response to the whole package, because now we have both sort of pieces of the question addressed, from SC&A, the source term and the other, and we need a response on both pieces, whether DCAS decides in the end that they disagree with the source term and agree with this latter part that Bob Anigstein’s analysis has produced, or however it be, once you have that, then I mean, once you have that response, the Board can then make a decision itself.

Do we think this should be changed in terms of the TBD or not, and that will be then in their lap to make a judgment as to whether they are going to comply with, depending on what the Board's decision is, with the Board's recommendation to change the TBD or not.

But that sort of fulfils the process.

MR. STIVER: Yes, that sounds like a reasonable way to proceed on it.

DR. GLOVER: Brad will put before the Board the recommendation of the Work Group that it's a TBD issue.

MR. KATZ: I mean, it's a TBD issue but I mean it's --

DR. GLOVER: That sounds -- they don't have to agree to that. They could say that that's unmodelable, they could -- I mean there's been lots of different ways --

MR. KATZ: It's sort of inherent that it is a TBD issue because either way, if you disagree about the source term in your case you are still saying there is a source term that that can be derived, that is valid, and that bounds it.

MR. STIVER: If you couldn't derive a source term then it would become an SEC issue but we had demonstrated that it is not an SEC issue. Now, you just -- a scientifically defensible model has to be applied and a source term that comports with the observations.

MR. KATZ: But that's a TBD --

MR. STIVER: That is a TBD, we

have shown, SC&A has shown, that it is a tractable problem.

MR. KATZ: Part of the Board's charge -- so even, it's not an SEC, it's a TBD matter, the Board's charge with dose reconstructions is to make judgments about the validity and quality of dose reconstructions, and this falls squarely into that camp, and the Board can make a judgment about that, and then DCAS has to wrestle with whatever the judgment of the Board is about the validity and quality of dose reconstructions.

This is an element of those dose reconstructions. So I think that's --

MR. STIVER: So the action item then would be for Brad to bring this up as a TBD?

CHAIRMAN CLAWSON: Well, this would be probably part of the -- at the Augusta meeting, to be able to bring this up of where we are at with Fernald and some of the overlying issues that we have.

MR. KATZ: Yes, I mean, you are going to give them an update at the Board meeting, right?

CHAIRMAN CLAWSON: Right.

MR. KATZ: And this one, I don't know whether you are going to get a final word from DCAS in advance of -- that's just a week and a half away, so you probably won't have resolution of this for then, but you can certainly tell them about this issue.

CHAIRMAN CLAWSON: I think that if we look at the transcripts, they are pretty well held -- been holding to this for a long time. This is what the dispute has been and so I don't think that will change before the meeting.

MR. KATZ: But I guess we could -- we have Sam and Mark here, I mean, if they want to take a stand now that this is resolved as far as they are concerned, and that they disagree --

MR. STIVER: As far as an SEC?

MR. KATZ: No, in terms of the source term, with this question that Hans is raising. If you want to take a stand now you can, otherwise, it sounds like this didn't really get resolved in the transcript, is what you are saying. We moved on from it.

MR. STIVER: We moved on.

MR. KATZ: And then we don't have a piece of paper, I don't believe, from DCAS that actually lays the line down and says no, we disagree with Hans's analysis and we are sticking with our source term, whatever it might be.

MEMBER ZIEMER: Actually, wasn't -- the work Bob did was a result of the last meeting.

CHAIRMAN CLAWSON: Yes, it was because we had, Chew did a report on this, this is where we got separated.

MEMBER ZIEMER: Right, and Mark, according to my notes, and you have the transcript there, John, but my notes said that you had indicated the Pinney study and then there was a question of whether or not SC&A agreed with the source term involved there and I think they were going to go back, which is what Bob did, and take a look at that, which he did.

And now you are saying no, we don't agree with it.

DR. BEHLING: Let me also, Dr. Ziemer, let me just make a point here. I have in both White Papers stated very, very distinctly that the -- both the Pinney report and the NIOSH's assessment models as defined in TBD-4 of the Fernald Site Profile, they both essentially used the RAC 1995 and modified by RAC 1998 data.

In both cases the central value or median value prior to 1979 is assumed to be 5,000, 6,000 curies, and that is basically the fundamental issue that I have argued from day one.

And I have shown in both White

Papers my assessment, based on the disequilibrium, based on the dose rates that were observed on top of the silos and on the basis of the radon treatment system in being able to evacuate the head space, that those numbers were essentially identical, which essentially provides indirect proof that the release of radon prior to 1979 was essentially 97 percent into the environment.

And I don't know how anyone can argue with these issues. We're just going in circles at this point, going back and forth on the Pinney report.

MR. STIVER: I guess we need a statement from NIOSH as to whether they agree with us or not on this.

DR. GLOVER: It won't happen here.

MR. STIVER: It's not going to happen? What's going to happen about a week and a half before the meeting?

MR. KATZ: Nothing, I'm sure.

DR. GLOVER: The boss is not here.

MR. KATZ: No, I know, he's not back until the 18th, but the Work Group can report on where this stands. But it can't really be resolved until we get final word from DCAS in terms of how it is going to handle, how it would handle the source term question going forward, whether it is going to revise the TBD in the short term, or whether that's a low priority because there are no cases that are affected, whatever.

Because I think in the very end of the day, even if there are no cases right now that would be affected, at the end of the day, I think DCAS wants to have methods that are valid, that have validity and quality, but obviously it wouldn't be a high priority if there are no cases in the hopper to be affected.

MEMBER ZIEMER: In fact, in view of what Hans told us, it's not clear to me why we had Bob do anything more since then.

MR. STIVER: The first studies

focused on a source term and the last issue that came up in the last meeting was well, given a source term, is the model they are using -- can you use that to bound doses?

MEMBER ZIEMER: Independently.

MR. STIVER: And this is what gave rise to Bob's study. In reality of the two are kind of tied. Okay, basically wait for a DCAS response on --

CHAIRMAN CLAWSON: Wait for a DCAS response on that, and --

DR. BEHLING: Brad, can I make a final comment on this issue? It sounds as if we are not going to get any concessions from NIOSH and if that ends up being the point where we sort of, say, well, we are in a stalemate, it has always been my feeling that we are in an adversarial relationship here, where we say one thing and NIOSH responds by saying the opposite.

However, I think the resolution may have to come from the Work Group that looks at the data much like a jury in a courtroom, and says well, we have listened to both sides, there's no agreement between the two sides, but based on our technical understanding of the issues, we have to come down on one side or the other. The issue may have to be resolved at that point.

MR. KATZ: Hans, there's no question about that. The Board is the last word. It's not SC&A or DCAS has the last word on what the Board thinks, it's the Board. So the Board will ultimately make a judgment on this, and that will result in a recommendation or a lack thereof if it doesn't have a recommendation to make in terms of resolving this TBD issue.

CHAIRMAN CLAWSON: And I'll be right honest, because this Site Profile issue versus an SEC issue, if we can't come to a resolution on it, and that we have an appropriate means to be able to do it, to me it falls in -- that is an SEC issue, though.

MR. KATZ: If it's feasible to do dose reconstruction, it's not an SEC issue. Then it's a question of TBD.

DR. ANIGSTEIN: This is Bob, I have to interject. I have right in front of me, because it is mentioned on page 329 of the transcript, which is Member Griffon.

MR. STIVER: Yes, that's the second --

DR. ANIGSTEIN: I'd like to read this, I think it might be helpful. It's one quick paragraph.

"Member Griffon: What I would like from SC&A is the position, sort of what you just discussed with John, that there might be a difference in our acceptance of the source term, however here's our position on the ability to bound and considering the Pinney -- the approach 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."

Now, that is the position that we have explained -- that yes, we believe that the radon exposures can be bounded.

MR. KATZ: Thank you, Bob, that's exactly what we have discussed here. Thank you.

CHAIRMAN CLAWSON: Okay, should we just keep on plugging here?

MR. STIVER: What I'd like to do is slightly switch up the schedule here. The last two Work Group meetings we have never gotten around to in vivo monitoring for thorium-232.

And it looks like if we continue in the trajectory we are on, that's going to happen again today.

So I'd like to go ahead and let Joyce and Bob talk about the thorium-232 post-1968 in vivo report.

MR. ROLFES: John, if you mind,

before you have them do that, I just wanted to make everybody aware, in case you didn't see your email, that NIOSH did issue a response to SC&A's review of the thorium-232 coworker study.

MR. STIVER: That's right, we did get that.

MR. ROLFES: Okay, and we also proposed a bias correction factor. So both of those, both the responses to your review as well as the bias correction factor have been provided.

MR. STIVER: Absolutely, and we have gathered those and reviewed them.

MR. ROLFES: Okay. And there's also supporting spreadsheets if you'd like to see those as well.

MR. STIVER: Yes, we may very well want those. Okay, so Joyce and Bob, I guess Joyce, your issue is really about the data quality and Bob is going to address whether there is enough granularity to assess doses based on those data, is how I understood it.

So I guess you should go first, Joyce, if you would like to lead out.

DR. LIPSZTEIN: Okay, there was a response from NIOSH that got into the O: drive last week that was a very brief response to our review paper.

And I am going to touch on it while I am describing our problems with the interpretation of data for chest counting of thorium-232.

Okay, one of the biggest problems we have is the uncertainty in the interpretation of data for the period of 1968 to 1978.

These thorium lung burdens are reported in milligrams of thorium in lung. We don't know how the in vivo measurements in this period of time, `68 to `78, were done.

And there are some descriptions in ORAU documents on TKBS-00175 saying that they were most likely based on actinium-229 measurements for thorium-232, but that lead-212 also might have been used to access the thorium-232 and thorium-228.

What happens is that there is no paper or no proof of how those results in milligrams of thorium were calculated or were measured.

If we -- does everybody have our review in hand so that I can refer to the figures in it?

MR. STIVER: I think -- do you guys have that?

CHAIRMAN CLAWSON: I do.

MR. STIVER: It should be in one of the -- it should have been mailed out.

CHAIRMAN CLAWSON: By John.

DR. LIPSZTEIN: Okay, we have, let me just say one thing, that we have taken data from several workers which had body burdens of thorium in chest recorded in milligrams.

Those workers came from a compilation that Bob did. Those results came from a compilation that Bob did.

And we looked at the data, and we could see that the thorium content, there is a monotonic increase of the thorium content along the years, and this could be compatible with the estimation of thorium-230, actinium-228.

Could also, because this thorium was separated, then the actinium-228 would increase in the lung, so this monotonic increase could be characteristic of measuring thorium in the chest through actinium-228.

On the other hand, it could also be that workers were exposed clinically to insoluble forms of thorium and then you would see also an increase of thorium, or could be that they would be exposed in several places to thorium, to increased quantities of thorium, and then you would have the same thing.

So there is a big uncertainty of how this thorium was measured. In the response we got last week from NIOSH, it only says it was measured by lead-212, but there is no proof or nothing or no documentation saying that.

So we still think that there is a lot of uncertainties on this. Also, when we look at the -- just one second -- when we look at the consistence of the data in milligrams of thorium, that were until `78 on, that were measurements done using lead-212, we see that if you look at the results, their results using lead-212 and their results from actinium-228.

So nothing tells us what was used for milligrams to derive the milligrams of thorium in the early times. Then, there was an overlap in the reporting convention that occurred between 1971 -- `78 and 1979.

In those years there were in vivo thorium measurements that were reported as milligrams of thorium and some were reported as nanocuries of lead-212 and nanocuries of actinium-228.

NIOSH suggests a conversion factor of 0.11 nanocuries would correspond to one milligram of thorium, and so we converted the results and then we got -- we compared the results from workers that were measured both in the `78 using milligrams of thorium and workers that were measured at the same time that had results of lead-212 and actinium-228, and there was a large fluctuation of results.

You cannot say either the conversion factor was not correct, or the you know, there is a very big -- we think there is a very big uncertainty on the data that was measured in milligrams of thorium, and we have reported 22 entries of thorium in milligrams and the same activity measured in lead-212 in nanocuries.

So we could have a direct comparison and the ratio of activities runs from minus 6.4 to 13, the ratio of thorium activities is measured two ways.

So we think there is a very big uncertainty on the thorium's activities that were measured in milligrams of thorium.

And we also illustrated this high variability of in vivo measurements recorded as milligrams for thorium then later measurements record as activity of actinium-228 and lead-212 with some graphs that could show this large variability.

We also looked at the MDA of thorium. The MDA of thorium is reported in the Technical Basis Document as six milligrams of thorium.

This is not -- it's an acceptable MDA for the time, but if you look at the data there are many results that are below -- or the majority of results -- are below the six milligrams MDA, and those are reported as positive results.

So this leads also to our conclusion on the large uncertainty of thorium results in milligrams.

And also, we don't have any -- we don't have information on the calibration of the system at that time period. I'm just looking --

MR. STIVER: Should we maybe let NIOSH respond to the issue of the MDA, because that's one that kind of jumps off the page at me as well.

DR. LIPSZTEIN: Okay.

MR. ROLFES: Yes, I think we have documented that in our response of six milligrams. Bob, if you are still out there, I am going to make -- correct me if I am wrong, but let's see here, yes, okay. It is in here.

We basically identified that we used the data as it was reported to us, whether it is above or below the MDA for a coworker model.

So it essentially doesn't matter what the MDA in fact is.

(Simultaneous speakers.)

DR. LIPSZTEIN: May I respond to

it? I am not saying that you don't use the -- that you don't need -- what I am saying is an example of the uncertainty of the data, all of that contributes to saying -- there is a big uncertainty on the significance of the in vivo results.

MR. STIVER: But also, Joyce, if you have got -- the 84th percentile of your distribution is less than the MDA, what does that say about the quality of the data?

DR. LIPSZTEIN: Exactly.

MR. STIVER: I mean, your instrument can't really detect it. It could be giving you any kind of number at that level, and it really has no meaning in terms of an intake or a dose.

I just, I'm trying to get my mind around how that could be used in a coworker model that would have any validity.

MR. ROLFES: That's not true, because any data that is reported to us would be used in the dose reconstruction process, similar to a non-positive uranium sample.

DR. LIPSZTEIN: Yes, but if this isn't really a measurement, that's our problem. If you know which nuclide was used, if it was lead, if was actinium, and how they accounted for the calibrations.

So there are lots of uncertainties if you compare the -- when there were results measured in -- by lead-210 at the same time as results from thorium in milligrams, the difference between the two results is so big that you can accept that one of them is wrong.

And we made a table in our report showing that for 22 individual reports of thorium-232 chest measurements in milligrams and lead-212, for the same in vivo measurements of the chest, and we transform it using the transformation factors that NIOSH has given in the paper.

And we can see that there is no correspondence between the two, and it's not that there is an error on the factors that we have to multiply, because if there was, if it was a problem of the factors that we have to find how to transform the lead-212 results into thorium-232 in milligrams, then there would be a constant error that you would see.

But you see a high situation that results from negative ratios to an order of magnitude ratio.

So this shows us that results in milligrams of thorium-232 are very uncertain, that they probably cannot be used to calculate thorium activity in the lung.

MR. ROLFES: Okay, I think you asked about the MDA and where we got the six milligram level. In our response we pointed out that SRDB 4140 is a paper in the AIHA Journal that lists the minimum level of sensitivity --

DR. LIPSZTEIN: I saw it, and but in this paper they don't say how the calibration was done. But I don't think this six milligrams is a problem. The problem --

because if you analyze the data from all the whole body counts for that time, six milligrams is, you know, an acceptable result. Could be six milligrams.

The problem is that the data in milligrams of thorium-232 has a lot of uncertainties.

MR. ROLFES: Sure, yes. Right. We agree with that. We agree that there are a lot of uncertainties. In the dose reconstruction process, those uncertainties are used to the benefit of the doubt of the claimant for the dose reconstruction.

So, I mean, this is essentially coming back to a Site Profile issue, whether or not we should apply this correction factor or that correction factor. It's not necessarily an SEC issue.

DR. LIPSZTEIN: Yes, but that's my problem, there is no way you can correct this. What we have shown on these 22 activities that were calculated using the -- you know, that we had the reported results in milligrams and the reported results in nanocuries of lead-212 that we transformed to nanocuries of thorium-232, using the factors, is that there is a high formulation of the data.

So it's not a problem of having the right transformation factor. It's a problem that you can't do it because the ratios vary so much that you can see that the data on milligrams is not -- you cannot be confident on it, and you cannot derive thorium activities based on those results.

There's a high imprecision in the pre-1979 individual thorium measurements.

DR. GLOVER: So this is a real person, right, Joyce? This is real people data?

DR. LIPSZTEIN: This is 22 real persons.

DR. GLOVER: Okay, and as you know thorium translocates to the bone, so if we look at this over a long term, we are going to use the lung -- whatever they calibrated, they would be looking at the lung, and so it's going to have a lower efficiency compared to what may be in the bone.

And so you could see that the ratio may be widely varying compared to a fresh intake. I mean, you could see the actinium and the lead, which is very low energy. It's 238 KeV compared to a much higher energy gamma rate, that's a more difficult measurement to make.

And so there may be some biases or some bouncing around.

DR. LIPSZTEIN: Yes, but the bouncing is too big. The uncertainty is too much. We don't know anything about those measurements done at that period that thorium was measured in milligrams.

We don't have that much information to validate those data and say, oh those are real measurements.

A lot of evidence saying we don't

know what is the significance of this data.

MEMBER ZIEMER: Whose whole body counter is this?

MR. ROLFES: This came from Mike Wells and they developed incorporation factors, et cetera. There's a document from 1965 which has basically some of the --

MEMBER ZIEMER: Okay, they must have a calibration procedure. I just was wondering about the concern about calibration. It apparently was not made available but they certainly had a calibration procedure.

MR. STIVER: The procedure should be out there and should be available, I would think, somewhere.

CHAIRMAN CLAWSON: I thought we had looked for that once before and we never came --

MR. STIVER: Joyce, your research showed that you couldn't locate any information on calibration, then, for lead-212 system?

DR. LIPSZTEIN: No. No.

MEMBER ZIEMER: Where do they look for that?

DR. LIPSZTEIN: I'm sorry?

MEMBER ZIEMER: Does NIOSH have calibration information?

MR. ROLFES: I'll have to look back. What we can do is look through the Site Research Database for the calibration information. From what I recall we did not find it at the time.

MEMBER ZIEMER: Who was the whole body guy at Oak Ridge at that time? Was it Max Scott?

MR. ROLFES: It may have been. I know that some of the discussion in the report on the mobile in vivo radiation monitoring lab for Y-12, there was some information written up by Hap West back in 1965.

MEMBER PRESLEY: Hey, Mark?

MR. ROLFES: Yes, Bob.

MEMBER PRESLEY: You all get with

Danny Rowan at Y-12, he will probably be able to help you.

MR. ROLFES: Okay. Thank you, Bob.

MEMBER PRESLEY: Danny Rowan. His department. He has been there since Hap West was.

MR. ROLFES: Okay.

MEMBER ZIEMER: I think the calibration issue could be put to bed, I would think.

Then the other part I am trying to understand was that the variability in milligrams detected versus the body burden calculated from that.

MR. STIVER: Yes, that differential between the two methods, I guess there was an overlapping period so they tried to compare those and get -- see if there was reasonable compatibility. You were getting up to the same endpoint and I guess there was --

MEMBER ZIEMER: Now, if they calculate lung burden per -- milligrams per lung burden in the lung, it's a very different ratio than body burden and that is going to depend on time after exposure.

So, I don't know, Joyce? Do you lack the information to address that part of the issue?

DR. LIPSZTEIN: These are lung burdens and they were calculated using lead-212, actinium-228 results and thorium in milligrams, on the years that they overlapped.

MEMBER ZIEMER: So they only looked at the lung burdens. Okay.

DR. LIPSZTEIN: And there is a big difference between the two measurements, and it's not that it's a constant difference then you say, oh, something is wrong with the calibration factor. No, it's not that. It varies widely.

And we don't have enough information, if the thorium, when they were measured in milligrams, if they were measured -- if actinium-228 was measured or if lead-212 was measured.

MEMBER ZIEMER: Okay, and can we -- was there information to tell us, you know, obviously you have to calculate some correction factor for lung wall thickness on this for each individual I think, right?

MR. STIVER: It has to be a specific calculation for every person.

MEMBER ZIEMER: Right. So was that information made available? Is it lung burden per -- let's see, they are calculating milligrams in the lung based on some count.

See, I think that minimum detectable activity is going to vary with the person's size, I would think. You know what I am saying?

MR. STIVER: Yes, the ability to detect a signal would vary depending on the chest wall thickness.

MEMBER ZIEMER: Yes, for a small person --

MR. STIVER: All other things

being equal.

MEMBER ZIEMER: You could detect x milligrams easier than in a heavy person.

MR. STIVER: Yes, that's another concern.

MEMBER ZIEMER: So minimum detectable activities I think could vary quite a bit. I don't have a good feel for this data set. I just have done enough whole body counting to know that those are variables that you'd have to look at.

DR. GLOVER: So would it suffice to say the Board would like us to review the calibration -- the information surrounding the -- maybe we have done that to some degree, Mark.

MEMBER ZIEMER: Well, I was just asking if we -- if calibration is an issue, that should be, if it's the Y-12 stuff, surely they calibrated it so --

MR. ROLFES: I'm looking back at our response here and SRDB 32612 has calibrations. It basically indicates that calibrations could be tailored to a specific individual when intake conditions were known for a specific type of material, for example the document contains some calibration information for a specific individual who appears to have been involved in an intake at Erwin, Tennessee, at separate facilities.

MR. STIVER: 32612, huh?

MR. ROLFES: 32612.

MR. STIVER: Say, Bob Barton, could you pull that one down at some point?

MR. BARTON: Yes, what was that number again?

MR. STIVER: 32612.

MR. BARTON: Got it.

MR. ROLFES: I don't know if Bob Morris and Bryce Rich are still on the line, if they have anything to add on what we have stated previously here.

MR. RICH: I'm still on and I don't.

MR. ROLFES: Okay. Thank you.

MR. BARTON: This is Bob. I don't have too much more to say except that when you would inspect a population of unexposed workers, you would get a lot of variation in that and half of the numbers would be below zero.

Now I am not suggesting that we have got unexposed workers here, it's just that there is, for a marginally exposed population of workers, a lot of variation in the data set.

So I guess I am not quite as surprised at that as others seem to be.

MR. STIVER: Like I say, after sampling an old distribution, you would expect a -- I guess the other question is, if so many of the data are beneath the detection limit, that's another issue.

Typically, what we would do in cleaning up a data set would be to look at all the LOD values and maybe assign them some nominal value; I think typically what you guys use is about half the LOD.

MR. BARTON: Well, that is typically done in the coworker modeling process.

DR. LIPSZTEIN: But I think that the problem here is not the MDA. I think the problem here is that we cannot, we don't know what those results in milligrams signify. There are many workers that were measured and had significant results and some that were below the detection limit. There is -- but the results varied a lot, even for the same worker.

For example, we had one worker that had 40 milligrams of thorium and then 40 days after, he was measured again and had 0.5, so what could this be? This could be a contamination of his clothes, yes.

But there is no explanation. You don't know. And there are many workers with this problem, so we also would suppose that if they were monitored by Y-12 monitoring, one of the procedures is to take out the contaminated clothes and all that.

But there is a lot of variation and it goes up and down and up again. So these contributed to the uncertainty of the data on this time period.

MR. STIVER: Joyce, what kind of information would you like to see that might help us to reduce the uncertainty in these measurements given that --

DR. LIPSZTEIN: I don't know.

MR. STIVER: -- fresh information would be available.

DR. LIPSZTEIN: I don't know. Is there any explanation why a thorium measurement would go down 10 times -- 100 times and then go up again? I don't know, unless it was not well measured. That's my point. I think that during this period of time, the uncertainties in the measurements are so high that you cannot use them to calculate the thorium dose.

MR. ROLFES: Well, maybe we could start with looking at the individual that you just cited that had a 40 milligram lung burden of thorium that dropped down to 0.5 milligrams 40 days later.

That might help us to understand what some of the contributing factors to those measurements, whether they in fact were caused by a surface contamination on the individual's clothing, you know, we would have to look. Maybe that individual had an in vivo count during the shift that he was working for example, and had some contamination on him, which would have over-estimated the lung burden if it was on the surface of his skin or clothing.

It could be that there -- we would have to take a look at a specific case like that to determine what the reason for that observed result was.

MEMBER ZIEMER: Well, I can tell

you this, the whole body counting people do not want to put contaminated people in the -- and they normally don't let them wear work clothes, number one --

DR. LIPSZTEIN: Yes.

MEMBER ZIEMER: And it's possible there could be surface, skin contamination that they missed, I suppose. But I mean, in our place we always had people shower and --

MR. STIVER: Yes, you would think you would have some kind of a protocol in place.

MEMBER ZIEMER: We put clean gowns on them.

MR. ROLFES: Usually at the beginning of the shift.

MEMBER ZIEMER: Right.

DR. LIPSZTEIN: I think that whatever you analyze, this thorium data, the variability is so high on a measurement basis of this same individual, that if you are used to work with thorium exposure you see that these are not credible data.

CHAIRMAN CLAWSON: Paul, also too, whenever we have got a positive one like that, they always recounted it.

MEMBER ZIEMER: Well, that, plus you would do follow-ups very soon after. But anyway, Mark is going to take a look at it.

MR. STIVER: Pre-1979, could be a significant issue regarding the ability to reconstruct the doses.

MR. ROLFES: So if you guys at SC&A or Joyce could provide that information to us, then we will take a look at it and prepare a response.

MR. STIVER: Okay. Joyce, could you get that data together and forward it on to Mark Rolfes?

DR. LIPSZTEIN: Okay.

MR. KATZ: So that's an action item for SC&A and for DCAS.

MR. STIVER: And also you had to look into this issue of uncertainty and the calibration information that may be out there.

Joyce, do you have more discussion on the data quality or does that pretty well sum it up for us?

DR. LIPSZTEIN: I think those were the most important conclusions that we had, so given the time I think maybe Bob could --

MR. STIVER: Let's let Bob Barton come on then and talk about the applicability and completeness of the data set.

MR. BARTON: Okay. Thanks, John. I guess in the interest of expediency I will try to give the patented John Mauro 30-second sound bite. And the real issue we see is that with the exception of 1968, we have not found sufficient information that would give us confidence that we can identify who the thorium workers were in the period of interest.

In addition to that, we feel that the in vivo monitoring program didn't target thorium workers for counting and this is based on the response from NIOSH, it looks like they would agree with that premise.

We also provide evidence that it looks like the thorium workers had a higher exposure potential for thorium, so then the question becomes, if you are going to create a thorium coworker model, is it going to be bounding for those workers who handled and were in thorium production campaign?

So that's pretty much the summary of our position in this second tour of the report.

MR. ROLFES: Okay, the first time the mobile in vivo counter came on site was in 1968 and they actually did prepare a memo listing, I don't recall the number of individuals who had been involved in previous thorium operations, but they did in fact prepare a list of thorium workers that had not been monitored.

And their intent with the bringing the mobile in vivo radiation monitoring laboratory to the Fernald site for the first time, was to take a look to determine whether there were significant lung burdens or a fraction of a lung burden in the Fernald work force who had previously been exposed to thorium.

MR. BARTON: If I could stop you right there, I did say with the exception of 1968, because that was one of the two pieces of information we found that actually identified thorium workers. The memo came out December 26th, 1967.

So one of the things we did is we assumed all right, those workers were still thorium workers in 1968, how many of them were actually counted?

Turns out it was just over half of them and when you look at the actual numbers of those individuals, half of the 51 workers who were counted, they showed higher concentrations of thorium than the whole worker population for that year in 1968.

So that's one of those pieces of evidence that they had a higher exposure potential, and past 1968 it becomes very difficult to try to figure out which workers were actually involved in these operations.

And I don't know if that's a problem that can be got around, if there's an argument that can be made that after those years they had the same exposure patterns as the general population.

But from some of the analysis we did in that second section, it shows that the ones that we suspect were thorium workers in the later years had a higher exposure potential.

MR. ROLFES: Yes, the people that had the highest exposure potential were typically the chemical operators and there wasn't any kind of bias to select them specifically to look for a uranium exposure or a thorium exposure.

The individuals who were in that

highest exposure class would have been the ones that were counted first and those were the typical -- you know, we can, we have got various methods of assessing a potential for exposure to a worker and the chemical operators are among the highest exposed individuals, and they were the ones that were typically most frequently counted.

Now there were also some occurrences and incidents on the site in between the trips that the mobile in vivo counter made to Fernald, and if an individual was exposed to an incident in between, well, if he had an incident, prior to the mobile in vivo count -- mobile in vivo unit coming to count employees, that individual would have been also among the individuals who would have been counted first.

Other individuals that would have been counted were those that had a high count on the previous trip of the mobile in vivo unit.

MR. BARTON: I understand what you are saying, but irregardless of what their job title was, I am saying that if we have any evidence that they were a thorium worker, and we pull those files, we put them in a simple rank order, because then it would be all worker population, which presumably is still the chemical operators as well, it shows that the thorium workers, at least based on the limited analysis we were able to perform, have a higher potential than the general population.

So it would seem like from an SEC context, when you are forming a thorium coworker model, you should have to be able to identify or at least prove that those thorium workers who are not monitored, which I think we agree that there is certainly a portion of that class who wasn't monitored, are they going to have doses that are bounded?

MR. ROLFES: Okay, well, I guess it comes down to whether we have a representative set from the individuals who were monitored.

MR. STIVER: This is John Stiver. It looks real similar to the problem we had at Savannah River with the construction worker data set versus the rest of the workers, and so I guess the issue is, do you have enough personnel identified as thorium workers or who you are relatively sure are at later periods to where you could build that kind of a distribution?

And from what Bob is saying, there is some serious doubt as to whether you can even identify those workers.

MR. ROLFES: That's why we have created the coworker intake model, to assign to workers, to give them the benefit of the doubt that they were exposed even if we have no indication that they were.

MR. STIVER: But I guess the problem is that if that real subset have higher intakes, that's going to get smeared out if they aren't identified, if they are just lumped in with all the rest of the workers.

MR. MORRIS: This is Bob Morris. The chemical operators really were the focus of the lung counting operations and that was done without bias to what their job assignment was.

So at any rate, you have got a random distribution of the worst case exposures.

I guess my question would be how, as the model is currently constituted, if you don't know someone worked with thorium, how do you know that this model will bound the doses to an unmonitored thorium worker?

MR. BARTON: Because the thorium workers were the chemical operators.

MR. STIVER: There are other categories other than the chemical operators, though, that had high exposure potential.

MR. ROLFES: Like who?

MR. STIVER: I am just asking.

MR. ROLFES: Well, you made a statement.

MR. STIVER: It was more of a question really: are there other categories that potentially could have been exposed? I know --

MR. ROLFES: That's why --

(Simultaneous speakers.)

MR. STIVER: We had the issue of the metal production workers had a very high potential for exposure to airborne contaminants.

MR. BARTON: If there are unmonitored thorium workers, I mean, I understand that maybe all the thorium workers were chemical operators, but if you have a significant portion of that subset who had a higher exposure potential, who are not factoring into this distribution, how are you going to account for that?

MR. MORRIS: Well, we have got

interviews from people who planned the counting operation and we have got documented memos, I am sure you have read them too, that say there was no bias into that process of selecting after the first year.

MR. BARTON: Well, irregardless of the first year, which again, only a little over 50 percent of those guys in the Starkey memo were actually counted that year.

Irregardless of that, you are saying that it wasn't biased towards thorium workers, what I am saying is the thorium workers had a higher potential.

So how does this unbiased monitor account for that bias?

MR. MORRIS: There was no bias involved with it. It was chosen only to focus on the workers who have high exposure potential.

MR. BARTON: So if you had an unmonitored thorium worker, how does this model apply to them?

MR. MORRIS: Well, these are the same people.

MR. BARTON: But I -- are you saying that all of the thorium workers were monitored?

DR. GLOVER: I guess that was my question.

MR. MORRIS: There's no doubt the thorium workers were part of the general work force that they were monitoring.

DR. GLOVER: The premise of the coworker model --

MR. BARTON: I'm not saying you can't find the numbers to bound their doses but I mean, if you are just going to take the whole work force, even though we know this subset of workers at a higher potential, that would seem to be an issue.

DR. GLOVER: The premise of a coworker model is you don't have to measure every high exposure worker, but that you had to have at least measured some. And so it doesn't sound like we have tried to exclude them, they sound like they had an opportunity to be included in the coworker set, so --

MR. STIVER: I guess the question I have, have you captured enough of those high exposures that you haven't biased your distribution to where you might not include personnel who might have been in that subset of thorium workers?

MR. MORRIS: But we did reflect -- the criteria for including a worker in the counting system was on an annualized basis or even more often than that.

So those are the memos in the record of memos and essentially, the intent was to capture people who had the most high exposure.

I guess my question still remains, if you know their -- I would go out there and assume that there are unmonitored thorium workers and I believe that's how NIOSH put in their most recent response, that it's likely that the thorium worker population wasn't completely included, so if you have this population of workers that have a high potential, like John says, it is just going to bias the distributions to where when you apply coworker doses to that unmonitored worker, you are not being claimant-favorable.

MR. BARTON: I have heard the conversation before about what disqualifies a coworker data set and it is it systematically excludes the highly exposed workers. Now there is no reason to think that this data set systematically excludes the highest exposed workers.

MR. MORRIS: I would agree with that. I guess you just can't identify who those more highly exposed workers are; so what do you do with that?

MR. BARTON: Well, again, I'll say I've heard, in the last two weeks have heard John Mauro say this probably twice, the reason you disqualify a coworker model data set is if it systematically excluded the highest exposed workers.

And there's no evidence in this case that that occurred.

MR. MORRIS: But there is really no evidence that it didn't occur either, because we can't identify who those thorium workers were other than real 1968.

MR. BARTON: Well, we got them in 1968 as you know, at least half of them, and we also know that -- I mean, that was retrospectively looking through the historical thorium workers.

Now we got half of those as an early counting group and then systematically after that, there was all workers who were in the high exposure potential group were included. That also included the group that did thorium work.

I don't see how you can come up with this criteria that you have to prove that the people were in the -- who was in that group. We have an unbiased attempt to grab the highest, most exposed workers in the plant, without regard to their thorium exposure.

We know that that group overlapped because of the way that they made assignments into the thorium processing which was a periodic assignment, not a continuous assignment.

And there's no evidence, based on the memos that we do have, that there was exclusivity on this, and in fact the reasons -- there is reason to believe that it was an inclusive monitoring process.

MR. STIVER: Bob Barton, this is John Stiver. If you could kind of restate for me, 1968 you had a group of thorium workers who were monitored and compared to the non-thorium workers, there was definitely higher exposure potential in that sub-population.

MR. BARTON: And actually that included all the workers. That didn't exclude those that were lifted in the Starkey memo and monitored.

MR. STIVER: Okay, and then you also compared it then to their subsequent years, for which the thorium workers were not identified?

And that 1968 thorium workers -- excuse me, let me just -- the 1968 thorium workers, were they also higher than the distributions for later periods for which you can't identify thorium workers?

MR. BARTON: What we did there, John, is there's a second source for trying to determine who is a thorium worker, except it's not really specific to years.

What happens is you have a logbook sheet which lists all the in vivo counts listed for the workers, presumably during their employment, and sometimes in the upper-right corner of that sheet, it would either state "thorium worker" or "former thorium worker."

Now there are only 26 of these

individuals, nine of whom had ticked "thorium worker" and the other 17 said "former thorium worker."

We don't really know how to apply that to a year-by-year basis to compare their specific --

MR. STIVER: So if you just took those ones that you knew were thorium workers and you compared them to all others --

MR. BARTON: At some point I lumped them all together and all their data points were for thorium work and compared that to the all-worker again, and once again you find that those we suspect were thorium workers at some point past 1968, again, they have a higher lung burden than the general population.

MR. STIVER: How about at the upper end of the distribution?

MR. BARTON: There are some. The very top of the distribution, the highest values, were for workers that didn't have the writing up in the upper right corner.

MR. STIVER: Okay, so you could possibly presume that the high end of that overall distribution would capture even the highly exposed thorium workers.

MR. BARTON: Yes, I would think so, I guess our question is, can anything be done for this group of workers who, we have evidence of having a higher dose than the general population but we are going to apply the general population dose to them and that's --

MR. STIVER: I guess that depends on what --

MEMBER ZIEMER: That's not how you use the coworker models. You're not taking the average for the population. You are --

MR. STIVER: You're taking some upper bound of that.

MEMBER ZIEMER: Yes, and that's why you --

(Simultaneous speakers.)

MEMBER ZIEMER: -- representative which includes the higher people, because you don't want it to be biased one way or the other and then you are assuming that all these people are up at the upper bound.

MR. STIVER: You can show that overall distribution, you are looking at the 95th percentile, and you have this subgroup of thorium -- actual thorium workers or suspect thorium workers -- and there you look at the upper bound of that distribution and that is not above the other, I think you are okay.

And you don't really care if you can identify them. It's only when you are looking at the central estimates of those distributions and without regard to the tails, that you might get in trouble, I would think.

MR. BARTON: So what I am hearing is that the higher dose assignment would be made for thorium workers?

MR. STIVER: I would think for those who were suspected of high exposure potential, you would assign a 95^{th} percentile.

MR. BARTON: I'm not sure if that was in the original report. Maybe NIOSH could verify if that is actually in the language there, that those who were suspected of having higher thorium intakes were not -- would be at a higher --

MR. STIVER: Well, that's the issue we had with TIB-78 too, and we had language put in that that would allow for an assignment for workers who were suspected to have had higher exposure potential.

As long as you could show that that upper bound of the overall distribution captured the subset, I think you would be okay and it sounds like that is what they have got here.

MR. STIVER: Well, I can't say. Was that language in the original report?

I don't recall that. It might be something to look into. I haven't read the original report in that kind of detail to recall it. It was about a year ago when I read it.

MEMBER ZIEMER: Well, isn't it really covered by the broader issue of how NIOSH uses coworker data? It's not specific to this -- this is the same issue you have everywhere. How are you going to tolerate --

(Simultaneous speakers.)

MR. BARTON: Well, I think that's certainly an argument that we felt should be made for this class of workers, which we have shown that -- well, we can't find any evidence that you would be able to identify them by year so you can't really -- you can't really develop, or take a look at thorium workers versus non-thorium workers because the connection is not made to compare by year.

So I guess our argument was, will there be something done with this co-worker model to address that issue, and what I am hearing is that it will.

MEMBER ZIEMER: Well, in essence

it says that you are treating all the people as if they had the possibility of thorium exposures because that's what the distribution includes.

MR. STIVER: I guess I would like to see that document back and look at the original paper to see if that language is actually in there.

DR. GLOVER: This sounds like it's still in the SC&A's workup, that we don't have a response on that.

MEMBER ZIEMER: But, Mark, isn't that how you apply it?

MR. ROLFES: We have a standard method of applying coworker intakes and that's -- I don't remember the TIB, but one of the people that are responsible for putting together our coworker intake models would be able to answer that.

MR. KATZ: I think I recall it because I have heard it so many times, I mean, for people with high exposure potential, and chem operators always fall in that basket, I believe, they get the 95th percentile, the coworker model. Right?

MR. ROLFES: Yes, correct, if they have no monitoring data and, for example we had to take a look at the specifics of a case, if you had an individual who was routinely monitored for external dose and never had any kind of internal exposure information that would certainly raise a lot of questions with us and certainly would prompt us, if an individual was routinely receiving external doses, it would certainly, you know, make me wonder where is the data. It has got to be there, you know, because every time we look into it, we end up finding it if we don't initially have it.

But if that was the case, if we couldn't find data for that individual, for his internal exposures, then we would certainly use the most claimant-favorable --

MR. STIVER: Give them the upper

threshold, upper 95% percentile?

MR. ROLFES: Yes.

MR. STIVER: Okay, I am inclined to defer on this until I have a chance to actually read up and see if the language is there, at least in the TIB. So maybe we will get back on that particular issue. I don't feel comfortable buying off on it at this point.

MR. KATZ: That's fine.

MR. STIVER: Bob, maybe later in the week we can get together and go over that data set in a little more detail.

MR. BARTON: Sure.

CHAIRMAN CLAWSON: So this one will fall into SC&A --

MR. STIVER: Yes, we will follow on and review that particular issue of the subset of workers being bounded by the upper bound of the overall distribution.

The only remaining issue was the DWE data and we have put out a revision to our paper from November of last year to address the Revision Three of the NIOSH DWE model and there are a few findings there that are -- I don't believe this rises to the level of an SEC at this point, one more Site Profile Issue as to -- there is one issue on data validation and the applicability of the Davis and Strom GSD to Fernald and we feel there should be some kind of a site-specific evaluation of that data set to make sure that Davis and Strom uncertainty is applicable and is bounding at Fernald.

The other issue was this issue of blunders in the original data, and to the extent that that original data is available for Fernald.

There may be some -- a scoping assessment should be done on that to identify the frequency of blunders. That was a big issue for the Davis and Strom paper. There were not that many, I think there was about 11 percent or so, but they could range up to --

on average they were underestimated by a factor of two and some were up to a factor of 10.

So we feel that the GSD of five is probably going to be adequate to capture that, but there should be some site-specific assessment of that data set to identify if that is an issue.

CHAIRMAN CLAWSON: The third issue --

MR. STIVER: The third thing -- there were actually two others that were problematic for us. One was the assignments of a 95th percentile in unrelated air concentrations for a building if DWE data weren't available.

We felt that it would be more plausible to assign actual DWE data from an adjacent year or from the same building in a different year or from another building with some --

MR. MORRIS: Mark, this is Robert

Morris. On that topic, there were some -- especially the pilot plant if I recall the data --

MR. STIVER: Yes, the pilot plant in Table 2 of your report I believe.

MR. MORRIS: Yes, we didn't think that the other applications that were going on in other buildings were close enough to make that assertion.

MR. STIVER: How about the early data for the pilot plant? The process has changed enough to --

MR. MORRIS: Well, they were 15 years earlier, right?

MR. STIVER: I don't recall if it was 15, but with -- did the processes change in the pilot plant?

MR. MORRIS: Well, of course, it was a pilot plant.

MR. STIVER: Yes, but regarding thorium processing.

MR. MORRIS: I guess the issue

there is, if you take that to the extreme, you could look at the, I believe it was Plant 9, where they had the highest DWE was like 600 and some MAC and if you were just to take the highest air concentration, it was like 9,000 MAC, and if you assign that, it would certainly be claimant-favorable but it's completely implausible.

I guess that was the issue we had about using that particular approach.

MR. MORRIS: We noticed that that 95th percentile value was identified in the Strom and Davis paper, and definitely bounding although --

MR. STIVER: And I guess it was a plausibility issue as far as I am concerned.

MR. MORRIS: And so if you were wondering why we chose that line of thinking, that was it. They said they can go back and, if you think it is more appropriate, reduce those numbers.

MR. KATZ: This is Ted. I am going

to interrupt, actually, because we are running out of time and we really, we don't have time to discuss this, we really -- let's just put it on the agenda for the next meeting so it can be properly discussed.

MR. STIVER: They are just minor issues, I think, that are TBD-type issues that need to be resolved.

CHAIRMAN CLAWSON: Did SC&A put a new revision of this out?

MR. STIVER: Yes, we sent it out.

MR. KATZ: So there is a response to be developed by DCAS and that is the action item here.

MR. ROLFES: I want to add a caveat. I think we should focus our efforts right now on the SEC issue of most importance, the recycled uranium, I think that is what you would like us to do.

MR. STIVER: Absolutely that is the most --

MR. KATZ: So before we -- before

Sam loses all his brain cells on the table --

(Laughter.)

MR. KATZ: We have a place on the agenda for Fernald Work Group update and we set aside a lot of time for this on the possibility that the Work Group would be ready to make recommendations to the Board.

It is clear that the Work Group is not ready to make recommendations to the Board. Now just what would you like? Would you like to simply report as part of the Work Group updates?

CHAIRMAN CLAWSON: That is what I am going to have to do, just -- I'd like to start getting this before the Board so that they are not blindsided with everything.

MR. KATZ: If you think you have a substantial, say, 20-minute presentation or whatever, we can preserve that session and just shorten it.

If you, for example, want a full half hour to discuss with the Board where we are at this point, as opposed to just moving it out into the Work Group report session.

CHAIRMAN CLAWSON: I'd like a little bit of time to be able to discuss the issues where we are at, and give the Board just a little heads up as we come into it.

It may be shortened a little bit.

MR. KATZ: So it's an hour and a half right now, so you probably would want it at 30 minutes.

CHAIRMAN CLAWSON: Yes, about thirty.

MR. KATZ: Thirty minutes, and then I guess the other Members of the Work Group, it would be probably good to help Brad out just in the -- because I think a lot of it will be informal report out.

MEMBER ZIEMER: But you may went to prepare a little PowerPoint or something and we can review it and play it.

MR. KATZ: Absolutely.

CHAIRMAN CLAWSON: Okay.

MR. KATZ: And SC&A, Brad, if you want SC&A to help you with that, that's absolutely okay.

CHAIRMAN CLAWSON: Either John or -- yes, he said he would line us up so --

(Simultaneous speakers.)

MR. KATZ: I think that would be good. Okay. So just let him know that we have shrunk the session to, how about half an hour or so?

CHAIRMAN CLAWSON: Okay.

MR. ROLFES: We have somebody here, I didn't know if you wanted to --

CHAIRMAN CLAWSON: Right, I wanted to just give him a few minutes. We have got a former Fernald worker here and I know that he had some things that he would like to be able to say.

MR. KATZ: Yes, I have to catch a plane and this meeting can't go without me.

So I guess if we can keep it to two minutes or less that would be great.

MR. HENNEKES: Okay, the reason I am here is I am trying to get some clarification, okay? Let me read a brief statement here.

My name is Dan Hennekes and I worked at Fernald from July 24^{th}, 1982 through June 16^{th}, 2005.

On February 24^{th}, 2009 I was diagnosed with basal cell carcinoma of my neck. The U.S. Department of Labor determined on June 29^{th} 2009 it was at least as likely as not that the exposure to the toxic substance of the feed material production center was a significant factor in aggravating, contributing to, or causing my skin cancer.

Okay, so that was one part of it, so then I went through NIOSH and the dose reconstruction. Well, I got back the preliminary findings and they came back with -- okay, with this statement.

"The majority of Mr. Hennekes' radiation exposure was received during employment as a construction engineer according to records received from the Department of Labor and information provided in the interview process."

So I seen this and of course I said, well, wow, I must have done a poor real job at explaining what I did at Fernald during this time.

So what I did, I made a little work history, okay? And here, I explain here from 7/82 to 6/84 we averaged 55 hours of work. I was working in a pilot plant. Okay? Which was not a whole lot of monitoring going on there, and with the things we did, we did the demolition of the existing systems, we had the red and the black drums and found out a bit later that the red ones had to be geometrically spaced for criticality reasons.

There was no radiological coverage there at the time and basically that is what it was, it was the enrichment process and then we was doing the maintenance and the startup procedures on this process.

And then again, we went in there in 1986 to do a hydrofluoric recovery system, and I'm just saying that -- there's three pages all in, we don't have the time to go through it, but basically we worked in Plant 9, 5, all these different buildings, we were doing construction work in these buildings.

So my point was, does it seem logical or plausible to anyone that I would receive more radiological exposure working as a construction area engineer, construction manager on new projects, or spending from 1982 to 1993 working as a pipe fitter working in and around uranium on a daily basis?

Basically all I want to do is just be able to get that on the record. So that's it in a nutshell.

MR. ROLFES: Thank you very much. Did you provide a copy of that to NIOSH?

MR. HENNEKES: Yes, I did. And in fact I brought you another one.

MR. ROLFES: Thank you.

MR. HENNEKES: Okay. This is what I sent the Department of Labor on that. I don't know if you want a copy of that.

MR. ROLFES: Well, thank you for -- I'll take a look at this.

MR. HENNEKES: Yes, I appreciate it.

MR. ROLFES: You have a copy of my card if you have any questions. I would be happy to talk to you. Thank you for coming in and sitting through this meeting.

MR. KATZ: Yes, we do appreciate it.

MR. HENNEKES: Well, I appreciate you giving me the opportunity, and apologize for putting you through that.

CHAIRMAN CLAWSON: With that said, as usual we are going to send both action items, SC&A if you will send -- and make sure that we are all on the same page with this.

Frankly, with the recycled uranium

I want to make sure that we are on board with which way we are going with this. With that said, we will adjourn.

MR. KATZ: We are adjourned. Thank you, everyone on the line.

(Whereupon, the above-entitled matter was adjourned at 5:32 p.m.)