U.S. DEPARTMENT OF HEALTH AND HUMAN SERVICES
CENTERS FOR DISEASE CONTROL
NATIONAL INSTITUTE FOR OCCUPATIONAL
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ADVISORY BOARD ON RADIATION AND WORKER HEALTH

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WORK GROUP ON FERNALD

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TUESDAY
APRIL 19, 2011

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The Work Group convened in the Frankfurt 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
ROBERT ALVAREZ, SC&A*
ROBERT ANIGSTEIN, SC&A*
SANDRA BALDRIDGE
ROBERT BARTON, SC&A*
EVERETT "RAY" BEATTY, SR.
MEL CHEW, ORAU Team*
HARRY CHMELYNSKI, SC&A*
LOU DOLL
SAM GLOVER, DCAS*
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
DAVE SUNDIN, DCAS
JOHN STIVER, SC&A
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

9:02 a.m.

MR. KATZ: Good morning everyone. This is the Advisory Board on Radiation and Worker Health, Fernald Work Group. We're going to get started, beginning with roll call, with Board Members in the room, and since we're talking about a specific site, please speak to a conflict of interest too.

CHAIRMAN CLAWSON: Okay. I'm Brad Clawson, Work Group Chair for Fernald. No conflict of interest.

MEMBER SCHOFIELD: Phil Schofield, Work Group Member. No conflict.

MEMBER ZIEMER: Paul Ziemer, Work Group Member, no conflict.

MR. KATZ: And Board Members on the line?

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

MR. KATZ: Thank you, Bob, and we're not expecting Mark today. NIOSH-ORAU team in the room?

MR. ROLFES: Mark Rolfes, health physicist with NIOSH. No conflict for Fernald.

MR. SUNDIN: This is Dave Sundin with NIOSH. No conflict.

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

MR. ALVAREZ: This is Bob Alvarez, SC&A, no conflict.

MR. KATZ: Okay. NIOSH-ORAU team for now, but thanks, Bob.

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

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

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

MR. MORRIS: Robert Morris, health physicist, ORAU team, no conflict.

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

MS. KENT: Karen Kent, ORAU team, no conflict.

MR. KATZ: Thank you, NIOSH-ORAU team. SC&A team in the room?

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

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

MR. KATZ: And SC&A on the line? I've got Bob Alvarez already.

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

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

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

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

MR. KATZ: Okay. Thank you SC&A team. Federal officials in the room?

MS. LIN: Jenny Lin, HHS.

MR. KATZ: And this is Ted Katz, the

Designated Federal Official for the Advisory Board. No conflict. On the line? Any HHS, DOL, DOE?

(No response.)

MR. KATZ: Okay, thank you, and members of the public in the room?

MS. BALDRIDGE: Sandra Baldridge, petitioner.

MR. BEATTY: Ray Beatty, former Fernald worker.

MR. KATZ: Welcome to both of you, and members of the public on the line who want to identify?

(No response.)

MR. KATZ: Very good. That's it. For all callers, let me remind everyone on the line to please mute your phones except when you're speaking with the group, *6, if you don't have a mute button, and then *6 again, to take yourself off of mute.

And Brad, the agenda is yours. The agenda is --

CHAIRMAN CLAWSON: Changed?

MR. KATZ: On the website, but we've changed the order a little bit, to accommodate some staff who have time conflicts.

CHAIRMAN CLAWSON: Okay, I appreciate this. As I said, I'm Barton Clawson. I'm the Work Group Chair for Fernald. Like Ted said earlier, we're going to change the agenda a little bit, to be able to accommodate some people that have some prior commitments. So we're going to start out with recycled uranium.

I'd like to tell Mark we appreciate him getting this out to is, but it was a little bit late, like usual. So we've done the best that we can on this, and we'll respond accordingly, and I'll turn it over to John.

MR. STIVER: Okay. This is John Stiver with SC&A, and I'd like to briefly recap the RU issue from last February 8th meeting. At the end of that meeting, there were several action items that came up, and one published for NIOSH to review, our second report on recycled uranium, and really to address some specific issues that had not been raised previously in our original report.

That was in regard to the site-specific data that we had found, which would tend to indicate that the current defaults NIOSH had been using for their method, were probably not clear about what qualifies as a workers at all times.

So the scene was really to look at this work data that were in the DOE mass balance report, which really especially the Ohio field office report, which really is the fundamental underpinning of a lot of the validation for the default values.

We had one of our associates, Jim Werner, who was directly involved in the production of that document, and has a little bit of knowledge of its strengths, as well as many of its weaknesses. We laid that out in a fairly detailed manner in the RU report, and as Brad mentioned, Mark did provide us a review, and I'd like to talk a little bit about that.

I mean there's good news and there's some bad news too. But I'd say the best thing about is in relation to the first issue, the defaults not being bounding for certain periods of time, NIOSH did acknowledge that it's probably true, based on the additional information provided.

What I was really happy to see is that for the first time there was an acknowledgment that we really need to take into consideration the large amount of variability in the data sets that were provided in the DOE 2000-B report.

They also acknowledged that the use of the arithmetic means that the DOE report had relied on were probably not adequate, given the types and the breadth of the distributions, and the statistical analysis that was actually performed.

So what they did was they went ahead and abandoned the bootstrap means or the arithmetic means, in favor of a log-normal fit to the data sets, and then picked 95th percentiles of that. That's in this table. I believe it's Table 2 on page --

MEMBER ZIEMER: Are you looking at their Table 2?

MR. STIVER: On their, their new paper here. It would be Table 2 on page 16, and you can see that it's very similar to the original table. This had the process subgroups, the 19 process subgroup means, and those bootstrap means are listed parenthetically.

Next to the left of each cell is the, they're at log-normal 95th percentile. So you can look -- really, the most important one here, for dosimetric standpoint, is plutonium, the second column over. You can see there's really about four of these subgroups.

MR. KATZ: John, I'm sorry to interrupt, but I mean I thought you were giving an overview. But I mean typically, DCAS will present its work, and then SC&A will respond.

MR. STIVER: All right. I just -- okay.

MR. KATZ: If you're planning to get to that, that's fine. But otherwise, it would be good to hear from DCAS, since they were working on this report.

MR. STIVER: Okay, okay. I'll just say it in broad brush strokes, then. We were happy with the use of a more realistic distribution. They have addressed the variability in the existing data.

However, we still have concerns that some of the data were not analyzed, and also that the uncertainty, which we feel is quite significant in this data set, in terms of missing data, just the lack of knowledge about what was actually going on and various things like that, you know, process knowledge and terminations.

All those involve a great deal of uncertainty, and that particular aspect was not addressed. Only the variability of the data, on certain parts of the data.

CHAIRMAN CLAWSON: Why don't we turn it over to Mark, then.

MR. STIVER: So Mark.

MR. ROLFES: Yes. I'll just give you a quick overview of what we've done, based upon -- I mean this isn't something that we've been discussing just for a short amount of time. We've been discussing this issue for quite a long bit of time over the past several years.

This response is only our most recent of probably ten different provisions. You know, it's probably been about six back and forths, you know, between NIOSH and ORAU and SC&A. So ultimately, this the culmination of many, you know, back and forth papers.

What we've done basically for this most recent revision of this White Paper, is to reanalyze the data and use the 95th percentiles of the transuranic contaminants in the recycled uranium that was sent to Fernald.

It basically breaks it down by the various process subgroups, we mentioned on page 16 in Table 2, and we've got comparisons of the plutonium in parts per billion uranium at the 95th percentile, in comparison to our previous bootstrap mean analysis results.

The end result of our recalculations increased the plutonium defaults by a factor of four. It increased the neptunium defaults by a factor of three, and it increased the technetium defaults by a factor of two.

So this is what we're proposing to use now for dose reconstruction, for the time period when the high transuranic contaminated materials from the gaseous diffusion plants were sent to Fernald, and that was roughly early to mid-70's forward.

So we feel that this should result, you know, the back and forth discussion, because we're now using the 95th percentile rather than the average or bootstrap mean. That's my brief introduction, and thank you.

MR. STIVER: Okay. Like I said, I was very happy to see that. However, I still keep getting back to the problem with the DOE 2000-B report, and the limitation of that data set. I have some handouts that I printed out actually this morning, and couldn't get it last night or actually early this morning.

MEMBER ZIEMER: Well, before you go forward, I think you're moving beyond this now, right?

MR. STIVER: No. This is related to the same issue here.

MEMBER ZIEMER: Oh, okay, okay. I just wanted to ask about the final factors. So did you sort of look at the averages or were those weighted averages when you got the final factors, the four and the --

MR. ROLFES: Basically, the factor of four increased for plutonium on the uranium mass basis. We're now defaulting to 400 parts per billion.

MEMBER ZIEMER: Right.

MR. ROLFES: And the basis of that is the 95th percentile of the various subprocesses.

MEMBER ZIEMER: All of those?

MR. ROLFES: Correct.

MEMBER ZIEMER: Right, okay. Was that coincidental? I haven't looked at the numbers precisely. It came out 400. Is that a coincidence?

MR. ROLFES: Probably rounded a little bit. We probably rounded up.

MEMBER ZIEMER: I mean what -- yes, okay.

MR. ROLFES: Someone on the phone actually could probably answer that a little bit better than I. Bryce?

MEMBER ZIEMER: Some of these were a little more and some are a little less, it looked like, factor-wise.

MR. RICH: This is Bryce Rich. We stayed with the subgrouping of the processes of the plant, and the 400 represents the maximum of the magnesium fluoride process, which still has two or three percent uranium in it. So the ratioing technique is still valid.

This represents the highest, with the exception of 10A process, which is the gaseous diffusion plant scraps, and primarily the tower ash that came in the highest in the mid-80's. But this represents the maximum values that you would see, with the exception of that one process stream, which was handled in the blending operation for a short period of time, and with additional care.

MEMBER ZIEMER: Okay. So Bryce, it's the -- other than that one then, it's the maximum of all of these. There wasn't any sort of averaging done?

MR. RICH: No this -- because of the fact, Paul, that we -- you cannot identify any given individual with a process for the entire operation. Even on the magnesium fluoride, for example, that operation itself dealt with loading uranium fluoride into the reactors, and that was probably the most, the highest air contaminant job.

But then the magnesium fluoride would be a subprocess to that. So we're just defaulting across the board for every, as John has said, one size fits all.

MEMBER ZIEMER: Got you, got you. Great.

MR. STIVER: Bryce, this is John Stiver. You mentioned the blending operation, when the Paducah tower ash was processed. I was trying to find out, by going through the source documentation, at what step did the blending take place? Was it during the, after it had gone through the, been dissolved in the nitrate? Was it then? Was that the point where the downblending occurred --?

MR. RICH: My understanding, John, is that the stuff that came from primarily Paducah gaseous diffusion plants, but all of the gaseous diffusion plants, they were categorized into chemical-like, and then were prepared, so that they could be blended.

So in a variety of techniques, both in Plant 1 and elsewhere, they would be reduced to a particulate size that would facilitate blending.

Other blending operations generally took place in Plant 4, through a hopper fed operation that allowed them to blend with virgin material, primarily in the early days, and with other materials, to preserve the, not only the enrichment, but to blend down to a value close to the 10 parts per billion that they were working with, and in some cases they were well above, of course.

MR. STIVER: Okay. I just wasn't

sure whether that took place before the dissolution in the refinery. It sounds like it was.

MR. RICH: It was, and if the -- and in some cases, of course, the materials that came were such chemically, they had to adjust it based on what they had. They got magnesium fluoride out of the gaseous diffusion plants also.

So in those cases, they were doing a leach process, and then winding up with a solution that would be blended in a solution, before it was entered into the extraction plant.

MR. STIVER: Okay. Well thanks for clarifying that. There are a couple of issues we still have, and I'd like to pass these handouts out for -- there's not enough to go for everybody. If you're going to share that with Phil. There's one for you, Mark. I believe you guys have one as well.

What I've done is I gathered some of

the summary statistics together for the subprocess groups that came out of the DOE 2000-B report, and in addition to that, I went ahead and took a look at some of the data analyses for the Paducah tower ash.

There was actually two different sets of data, two different analyses, one done by the Paducah plant, and also NLO did their own analysis. The first part is going to take a look at the histograms here.

In Group 8, enriched magnesium fluoride, it would appear that for plutonium, at least, that the log-normal distribution would underestimate the high end. I mean it's not -- I understand that when you use it like that, you're going to, there's going to be a certain amount of acceptance of variation around that pit.

But it would appear that certainly above about 100 parts per billion, you're really starting to -- you see a real increase.

MEMBER ZIEMER: What figure are you

on?

MR. STIVER: I'm on the second page here. The heading is "Sort 8," and that would be Group 8, which is the magnesium fluoride.

MEMBER ZIEMER: Okay.

MR. STIVER: So I have a little bit of a concern --

MR. RICH: John, could I just interrupt and ask a question. You're aware, of course, that Appendix C in the Ohio report has a complete listing of all of the process?

MR. STIVER: Yes, yes. That's a very good data set. It's probably the most comprehensive of the bunch. I think it was like 400 data points.

MR. RICH: Yes, and it lists the individual samples and, you know, the description of the samples.

MR. STIVER: Right. Maybe you could also clarify something. A lot of those are listed at NMC&A. Was that an analytical lab that tested for Fernald or what?

MR. RICH: No. That was the uranium accountability system, I think.

MR. STIVER: Okay, okay. That makes sense. So that would be from any number of sites, and not just -- that wouldn't be site-specific for Fernald.

MEMBER PRESLEY: That's correct, Bryce.

MR. STIVER: Okay, and a couple of pages later for the 10A, this is the Paducah tower ash, and for plutonium here you see it's very, very good. It's a really good fit to the log-normal plot, despite the fact there's only 39 data points there. It does seem to fit fairly well.

But the thing that kind of worries me a little bit, if we can go to the second set of data, or the tables here, the first one is Table 1, Recycle Beads, Paducah Ash," and this is from National Lead of Ohio. You can see over here on the far left-hand column, for 16 different hoppers.

If you just for a minute take a look at T-449, which is, I believe, the fifth from the bottom, and the plutonium on a part per billion uranium mass basis is about 7,000 in this particular assay.

Now you go to the next page, which came out of the DOE 2000-B report from Appendix C, and these are all Group 10A. This is the entire data set that they used, and the first number 2 through 5 are the feed hoppers. So these are actual measurements that were taken by -- GES would have been at Paducah.

Here, we have the number five is the T Hopper 449, and the plutonium assay here is 940. So this calls into question the homogeneity of these samples, and we have two analytical laboratories, which we presume are fairly accurate in their analyses.

Yet, there's practically an order of magnitude of difference in the results in one given hopper of this material. So this kind of an illustration of the type of uncertainty we're concerned with, with the DOE 2000-B report. You guys have done a good job, as far as I can tell, without going into the details.

But it looks like you're addressing the variability in the data sets. But what we're not seeing is any kind of analysis of the uncertainty involved, and this is one of our findings in our report, was that there was no independent review of the data, you know, we were able to go in and do a comparison last night at about midnight, and come up with and see, here's a discrepancy and here's something you can base an uncertainty factor, at least, for a given set of the data.

So posing the question in the homogeneity of the waste streams, the quality of the analytical techniques, all these things that factor into uncertainty. So that would have to be, in any distribution that's going to be used for dose reconstruction, we feel that there needs to be some kind of a robust uncertainty analysis that takes those types of factors into account.

MR. RICH: John, can you take a comment at this point?

MR. STIVER: Certainly.

MR. RICH: If we go back to the description of the stuff that came from Paducah particularly, since they are the source of by far the majority of the RU contaminants, that stuff came in in all sorts of forms, and it had to be prepared. It was not homogeneous when it came, and so the analysis --

I'm not surprised that there's a great deal of variability at all. After it had been worked through so it could be blended, then the process stream had -- it was still had a great deal of variability, and as a matter of fact, I'd just comment as a footnote that we found, similar to what the Working Group found, even to analyze these process streams with a log-normal distribution is problematic, because of the spread in the data.

However, we've defaulted at the very top end of the scale, from a process standpoint, where both of the exposures actually would occur. Thank you.

MR. STIVER: You're welcome. Yes, I understand that. My concern really is the impact on attempting to bound the intakes, because you know, if you have an order of magnitude different than two measurements for one hopper, you've got to wonder if there may have been, you know, three or four other measurements that could have ended up in a factor of two or more higher than the 7,000. So we feel that that type of information needs to be --

MR. RICH: More likely you have ten, with much less activities, compared to five.

MR. STIVER: Right. I guess that could be true, but we just don't know, because we don't have the data to base that on. In this particular case, we have two data points for four of the hoppers, and in the other case we have -- and that also kind of calls into question why that wasn't, the NLO data wasn't used in the 2000-B analysis? The GES data was there, but not the NLO data.

So there was, that's the issue of data completeness there as well. I don't know if that was a decision by the Process Knowledge Team in putting this together. They felt these numbers were better. That still remains a mystery.

So once again, I hate to keep harping back to this, but we really feel that an uncertainty analysis is warranted here.

MR. ROLFES: Well, I don't think -- in the interest of time, I don't think we're going to go back and look at the original data, to develop an uncertainty distribution for the half a million results.

MR. STIVER: Well, I mean maybe not half a million results, but I think you could certainly do some scoping analysis, to get an idea of what types of uncertainty factors you may be dealing with. I mean here's an example where you've got nearly a factor of ten. You have the other situation with the uncertainty in plutonium partitioning into raffinates. Originally, it was thought that 80 percent would go into the raffinates. It turned out only 20 percent did, but that was based on one study, on a single study.

So there's that issue. There's these different types of uncertainties that aren't reflected in the data that we have here in this table. So I guess that's what concerns us.

MR. ROLFES: I was going to say, correct me if I'm wrong, but I believe that those subgroups that are reported in our Table 2 on page 16 here, these individual process subgroups, do account for the different chemical processes and the different movement of materials throughout the Fernald site.

Basically, we've selected the 95th

percentile of each of those results. So there could be a couple of points that exceed the 95th percentile level, but that's still accounted for in the distribution.

MR. STIVER: Yes. I think we're kind of confusing variability and uncertainty here. I think you've done a good job, at least it appears to be. I've done some back of the envelope calculations; I can get pretty close to the numbers that he got.

Based on that data, we're talking about the data that's missing, decisions that were made about whether a data set or a certain data point belongs in Process A or Process B.

So that just throws in another whole level of uncertainty that's going to cause that distribution to drop. So and you know, a corollary to that is that stopping at magnesium fluoride, you know, we're willing to I mean just concede. But in our report, that was one of our main points of contention, was that the workers that ran this production, by virtue of this concentration process through Plant 1 recycling of material back into Plant 5, to reuse in the production bottom liners and also the graphite mold and so forth, that these people were probably one of the most highly exposed groups.

So I'm glad to see that you took care of that particular issue. But then we also have the issue of the complete data set. In my opinion, and I'm certainly not a statistician, and Harry could probably weigh in on this better than I could, but you'd want to look at all the data, and not just, you know, not just rank them and then say okay, we're going to stop at this one because it's the highest. But really kind of combine them all using sort of a more rigorous analysis.

It includes all the data that were available, and also account for uncertainties that were involved. The end result is that you're going to end up with a higher number.

It becomes a philosophical issue at that point. When is a number bounding? Is 400 bounding? Well, you'd think from a standpoint, you know, a practical standpoint, sure.

Who's going to get 400 parts per billion every day for year after year after year after year? Are there a category of workers that this data aren't really bracketing. Maybe there's uncertainties involved, or there may be certainly lower values. There certainly could be higher values as well.

So some sort of an effort to demonstrate that, I think, would go a long way.

MR. ROLFES: Well, when we choose an upper bound value at the 95th percentile, we usually don't assign an uncertainty to that value, because it's a bounding value.

MR. STIVER: Well, --

MR. ROLFES: In addition to that, in

the dose reconstruction process for internal dose, our internal dose, annual dose calculations to the organ, we always default to a GSD of 3. So there's uncertainty built into our dose calculations already. So that's really all I have to add.

MR. STIVER: You'll be putting a GSD of 3 of the 400?

MR. ROLFES: That's on top of the annual dose calculations that we completed in the dose reconstructions.

MR. RICH: This is Bryce Rich again. Could I just make another comment? As we indicated, Process Group 10A defines, and that may be part of what you're referring to right now, John, was the values are higher than the 400.

We elected not to default for the entire plant to that stream that came in as a stream to the plant, because of the fact that the operation of the processing and blending, the operation of it was relatively short-term, and did not exist, you know, did not go on for days and weeks. Any individual hopper would be processed in a relatively short time period. Any individual would be on other standards, uranium streams in the plant.

And plus the fact that they were extraordinarily sensitive to the fact that this stuff was coming in from Paducah, to which they objected in the first place, because it represented a significant additional hazard.

So they were layer protected, and it did not represent a process stream that should be applied to the entire workforce, and I can't see any individual that would be working on that process in that operation, where it would be a legitimate, routine exposure.

But you'll notice 10A is significantly higher than the general bounding of the parameters.

MR. STIVER: Bryce, do you know about how long the blending operation went on?

MR. RICH: In the blending operation, it went on for years, and they handled it -- you know, they didn't have the material to blend with in the first place. In the 70's, they were blending with virgin material, and later on -- but it was kind of hopper by hopper, until they worked it all off.

As a matter of fact, they lost track of several of those hoppers, and in the 80's, they discovered them and counted them, I think, because they had lost track of it because of a mislabeling issue.

So they were mindful of the column associated with that higher level stuff, that came from the gaseous diffusion plant. But it took, you know, they worked it off for a long period of time.

MR. STIVER: Now that particular batch that came in 1980 was the highest contaminated.

MR. RICH: Yes.

MR. STIVER: Do you know how long that took the process? Were any of those hoppers that were missed, were they included in --

MR. RICH: Oh no. Those came in in the 70's and got misplaced.

MR. STIVER: But do you have any idea how long it took the process --

MR. RICH: The relative concentration of those missed metals were in 28 to 30 parts per billion, rather than the thousand part per billion.

MR. STIVER: The reason I ask is that, you know, we went back and looked at the site boundary data. Remember in our originally report, we only had data for 1983.

MR. RICH: That was an excellent work there, John. I appreciate that. Because of the low levels, we just did not make a ratioing there.

MR. STIVER: Thank you. What I've discovered is kind of interesting, when you look at -- I can pass this around. I don't have copies for everybody, unfortunately. But you see that in 1982, you have a plutonium level of about ten parts per billion.

At '83, it gets up to about 200. '84, about 300, '85, you're back down to about 12, and then it kind of stays down again. So --

MR. RICH: And that's typical of how they processed it.

MR. STIVER: So then it looks like you've got that one batch with a real high contaminant, you know, the high ratios, being processed over a period of about -- from '82 to about '84, roughly.

MR. RICH: And any time you put a high concentration into the system like that, it stays with you until it works its way through.

MR. STIVER: Right, and also it tends to corroborate that 1985 baghouse dust sample, which would be --

MR. RICH: Yes, yes, it does.

MR. STIVER: So it's nice to see that this all fits together. But I guess my concern is really that you've got this one particular batch that we know is extremely hot, and for which, you know, a reasonable person or any kind of a coherent health physics program would probably try to control their exposures during downblending.

MR. RICH: And we have records describing the process, the procedure and the process for doing just that, including the air line respirators.

MR. STIVER: My only, my own problem with that is when you go to the 1985 task force report, they really are pretty highly critical of the health physics practices that were in place at the time. They mention respirators just being hung on the wall and not cleaned, you know; individual workers having to volunteer for bioassay if they think they were exposed, things like that.

MR. RICH: John, that's true. There were periods of time when they switched, they initially had the workers clean their own respirators, and they did not stay with that very long. They took over that as a company, to bring them back in. It was a failed process there, but they didn't stay with it long.

MR. STIVER: Well, in my mind it just casts doubt on -- excuse me, go ahead.

MR. RICH: But a lot of the workers do remember that, and quite frankly, you know, it doesn't take a lot of admission to say, you know, it was an awareness of the high level that came in and three Work Groups that were established to make recommendations. They were a little slow in actually initiating a specific bioassay, and I'll just leave it at that.

MR. STIVER: Yes. It appears that finally when Westinghouse came on board in 1986, they really kind of got things in order.

MR. RICH: Well, this was in process before that, but a change of contractor is an excellent time to initiate a lot of changes.

MR. STIVER: Okay, and I guess I'm still a little concerned in the assurances that health physics was adequate to control these people's exposures. You know, there isn't any hard, fast evidence, you know, contrapositive evidence obviously, but there is just not any kind of data available that you could look at and say okay, yes.

It looks like they had a good program in place. These people were trapped. We've got the bioassay results. We've got, you know, breathing zone samples. Anything like that was just not there.

So you're kind of stuck relying on the assurance of well, don't worry, you know. We had it under control and these guys used air line respirators and so forth. But we don't know that.

MR. RICH: John, let me make just a

comment there. You know, the default level approach says that okay, there could have been some deficiencies in relationship to controlling the transuranics as they came in, and significant increased values.

However, what we're saying is that there is no indication that the fundamental and the primary comprehensive program that they had in operation, the air samples and primarily the urine sampling for uranium. What we're proposing is that it is the ratio of the uranium urine program, which was sound, to a bounding default.

So it really doesn't matter if the program was completely adequate or not. We're saying that the bounding ratio to the uranium urine will cover, from a bounding standpoint, and for most of the plant it is enormously --

MR. STIVER: Yes, I understand that. But still, you have the issue of if the health physics controls were not adequate, then omitting this data set is probably not an acceptable thing to do.

MR. RICH: But it was inadequate for a period of time for the transuranics, not for the uranium, and the uranium is the basis for the defaults.

MR. STIVER: I know, but the point being is that the default contamination levels and ratios could be higher, because there could have been people who were actually exposed to this material, despite what's in some of the historic recollections.

So omitting that based on not really, I'd hate to say the word "hearsay," but without any corroborating data --

MR. RICH: Well, there's a lot of hearsay back and forth on both sides. As we examined the history of the plant and the processes, we're convinced that if any exposure to materials that were above this bounding dose on a unique basis, it would be a short period of time, and covered by an exposure to uranium with much less levels of transuranics.

MR. STIVER: Harry, are you on the line still?

DR. CHMELYNSKI: Yes, I'm here.

MR. STIVER: What do you think about -- could you weigh in a little bit on constructing the distribution here? It seems to me that really all the data should be used, if there's any uncertainty at all about the potential for exposure, whether it be short-term or long-term.

Then that data, then, could be used to generate an overall distribution. I don't think you can just outright eliminate the highest data set, based on some recollections and a few quotes from the health physics department, without any kind of corroborating evidence.

I mean how do you -- if you had to do this yourself, how would you construct a distribution from this data set?

DR. CHMELYNSKI: Well I guess to

begin with, I agree with your statement that there's a difference between calculating a distribution on the data you're looking at, and taking the 95th percentile. There's a difference between that and doing an uncertainty analysis.

An uncertainty analysis would question the data you're looking at, and that's the question John Stiver's raising here. If you're going for the 95th percentile, I think it's very suspicious to leave out the highest data set.

I haven't looked at this data in detail to look at those kind of questions. Indeed, nobody was looking at the 95th percentile when I looked at the data. We were comparing the arithmetic means and the log-normal means, et cetera.

But I was only also looking at the data that we had at hand. So I'm a little concerned that I hear now that there's a lot of data we didn't look at.

MR. STIVER: Okay, thanks Harry. John, did you want to say something?

DR. MAURO: Yes. I think I was actually getting ready to pose a question. When we look at this process Subgroup 8, where it said that 342 parts per billion of plutonium, now am I correct that that particular group, that number, does that reflect this dolomite material?

MR. STIVER: Yes.

DR. MAURO: Okay, so in a way what we're saying here is we have some data characterizing the dolomite, a material that we all, I think we all understand the process now. It was looping process, where as time went on, that dolomite might have become enriched more and more.

And there's some data, and obviously we have a certain number of measurements that comprise, that resulted in the, I guess the original geometric mean of 97 and now the 95th percentile of 342.

MR. STIVER: Yes. That original 97 is a bootstrap. I mean that's the arithmetic mean.

DR. MAURO: That's the arithmetic, okay. Now that number, 240, that came from how many samples?

MR. STIVER: That's 400 samples. That's probably one of the most complete data sets they've gotten.

DR. MAURO: Okay, that's important, now and those 400 samples were collected over what time period?

MR. STIVER: You know, it would be a question maybe Jim, do you know about that? What time period those data reflected over? It's not, and the summary report doesn't give you the period over which that was collected, I would assume. That would be one of those questions that you'd have to go back to the source data to answer it.

DR. MAURO: The reason I ask is, you know, I say okay. I've got a large number of samples of dolomite. I consider that hundreds of sample.

MR. STIVER: 402, I think, is the plutonium.

DR. MAURO: 400 samples is a large number of samples, and acknowledging that they represent, let's say someone actually by design, deliberately went in and sampled different batches, different times, different locations, so that they captured the variability in the concentration in that material, and then -- right? And it was designed that way from the beginning.

Then someone comes along and says okay, we're going to pick the upper 95th percentile, and say we believe that it's unlikely that any one individual could have been exposed to more than that for an extended period of time. I would say you're absolutely right. That's the way you do it.

But now what I hear is that well, we're really not quite sure whether the 400 samples is a good representation. That's a big term; I know they often use it. Is it representative of the population? And I guess no one has really, I haven't heard very much of the degree to which we believe those 400 samples are representative of the population.

There's a real but unknown 95th percentile of the concentration of plutonium in the dolomite throughout this facility, throughout the life.

MR. STIVER: Yes. This is just the estimated --

DR. MAURO: And this is some estimate, and what I'm hearing is, and this is really for me. I'm almost speaking to help myself get sorted out in my thinking. Is there a sense that the 400 number, 400 samples did in fact capture the variability of time and space, and therefore is a reasonable upper end value to apply to all workers?

Or is there a reason to believe that, you know, there could have been other campaigns, other time periods, where the levels might have been substantially higher than that. You know, I'm looking. So I'm trying to zero right in on what the essence of the problem is.

If the answer is that the data we have is the data we have. We do not know how complete it is and how representative it is, how the universe of exposures that real workers may have experienced over this very long time period, we do have a problem, because we are basically saying we have a slice of 400 samples, and intuitively we say geez, that's not bad.

But then you say but wait a minute. We don't know whether that captured the full range of operations that took place, and I haven't heard anything to that effect.

MR. STIVER: And that's why I brought up the issue of the samples in 10A, because here's a situation where you have one particular batch, one hopper of materials.

Two independent measurements, different laboratories come up with a factor of ten difference. So you've got homogeneity in that. You tend to wonder does that carry though in the dolomite, in very different time periods?

I mean the dolomite issue is something that's going to go all the way back to the beginning.

DR. MAURO: Right.

MR. STIVER: Remember, after the 70's and 80's, when this highly contaminated stuff came in, it was still downblended before it ever got to the metal shop. So what you're seeing in the 80's is probably pretty reflective of what was going on before, assuming downblending is effective and they're claiming that it was.

So you have a situation where is that data that were collected primarily in the 1980's and possibly in the 70's, representative of what went before? You could probably make an educated guess that it is. But the question being in my mind, is what's the uncertainty that should be factored into that distribution?

Not just looking at the data, but we know there's missing data. Now you can do that by taking a log-normal and extrapolating it out. But the nice thing about a log-normal distribution is your upper bound is usually the higher, the highest value you can measure.

When you look at the histogram for magnesium fluoride, it takes a big slice, right around 100 parts per billion. So the log-normal is actually under-estimating. So there's a situation where you might want to consider using an empirical distribution. Don't make any assumptions about it.

MR. RICH: This is Bryce again. Could I make another comment, just from a background standpoint? You're right, the primary data in the -- is in the 70's and 80's, mostly in the 80's when the major influx hit. The team that they put together, with the DOE 2000 team, was put together from the best that they had, and they categorized these samples based on a knowledge of where they came from, and how they related.

Now the magnesium fluoride is identified as an enrich uranium magnesium fluoride, which is the highest that you would find. The enriched uranium had the higher levels of recycled uranium. So this stream of magnesium fluoride represents a higher stream.

So the data on, I think it was 11. No, not 11. It was 10A, that identified the material, the samples that were representative and reliable from both Paducah and Fernald, were included in this data set, to describe the incoming activity out of Paducah. Based on the judgment of that team, I can accept that as representative.

The other thing I'd really mention too, if you look at Subgroup 6A, that represents U03 that was straight out of the primary site, out of Hanford, and was unblended. So you can use that stream for the input from the primary site.

So there's a lot of you can do with this data set, when you assume and accept the fact that this DOE team that they put together was not only knowledgeable but had excellent operational background.

MR. STIVER: Jim Werner tended to agree with you on that, and correct me if I'm wrong, Jim, but it said the Process Knowledge Team was probably about the best you can get at the time. So there was probably less uncertainty involved in the assigning data to a given process stream, as there is in -- what are the actual data that you got?

MR. RICH: And I'm personally familiar with that process, as they put that report together.

MR. WERNER: This is Jim Werner. If I could, I just wanted to address a couple of comments, and maybe at the outset, to try to reiterate the compliment, I think, that I am, that the SC&A review is built generally on the very good work in the team and the hard effort that went into that DOE 2000 report.

But in my involvement in reviewing that before it went out, and in some other work, again, I think that notwithstanding that very good work and the terrific team that put it together, it's still, I think, falls short of the mark of the certainty, and the difference between certainty versus variability in the data.

Then just chronologically going back, any of the data from the 70's and 80's, frankly I look at with some skepticism and think that one would really need to look at the pedigree of that data particularly hard. The findings of both the environmental survey that I was involved with as an independent contractor, and of the Tiger Team later on, both found very serious problems in the QA/QC process for many of the sites, including

Fernald, that there really was not a significant amount of confidence in that.

What was particularly problematic, I think, from a management point of view is that there was quarterly reports done by the Oak Ridge Operations office that was supposed to have caught those issues and they didn't. So that really lingered for quite a long time. It doesn't tell you that the data is bad, but it does tell you that there are reliability problems.

This is at a time, remember NLO ran it, and NLO, you know, this was not just a normal switch of a contractor. The NLO contract really was shifted, with prejudice. I mean they just had very serious problems at the site. So it was not just a routine change of contractors and one contractor came in with a better bid offer or a better team or a lower price.

There were pretty serious, widespread problems with NLO, and maybe Bob

Alvarez can get into that in more detail, in switching to Lockheed Martin. Then lastly, you mentioned the Hanford data, and those were among the issues that some of us thought it was appropriate to get into, in terms of variability.

One really can't just defer solely to Hanford sets, if there were really any processes going on there, that if the facilities, anything within the facility they changed over time.

That level of granularity really was not pierced in the DOE 2000-B report, that we got the data that was pulled together, and what people did. I don't know what it says for the heavy lifting. There was a lot of people working very hard to do it.

But at a certain point, many of us did question it and say well, hold it. This is a lot of good data from particular processes, and I don't know if it's appropriate technically to call it anecdotal, but it certainly was by no means what people thought was necessarily representative, that when many of the reviewers asked questions about well, but didn't the process change over time.

Didn't they change the efficiency? Didn't they change the facilities? Didn't they, you know, and all that's, of course, documented in the safety analysis report. The answer is yes, but there was really no time then to go back and start pulling the threads and getting into those details.

So again, we have enormous respect for the hard work that went into it, it simply didn't meet the mark of number one, confidence, and number two, representativeness.

DR. MAURO: Jim, this is John Mauro. Let me ask you a question. I like zeroing in on this Process Subgroup 8. In effect, what we have is 400 samples were collected and measured. I guess we're not quite sure when that was done and does it represent a cross-section, and out of those comes a 95th percentile.

Here we are sitting around the table saying okay, listen. I've got myself 400 numbers that were collected. But you have a sense of the incompleteness. You were there. You worked the problem.

Now in effect what I'm hearing from you is that well, you know, notwithstanding the fact that we have 400 of those dolomite analyses, it's your sense, that I'm hearing, that if you were to go back in time and maybe take another 400 samples some place else at another time, do you believe you'd come up with a 95th percentile that's substantially different?

Do you think it's possible that you could come up with a 95th percentile from another batch of 400 that you just went out there and grabbed again, based on the world you lived in at the time you worked the problem, that would be different, substantially different than this 400 or 342 number?

I guess that's where I'm headed, because what I'm hearing is we've got the data set that we have, that everyone acknowledges that it's incomplete. But it's still a lot of data, but it's incomplete, and there was -- and everyone agrees that the data that we do have shows a lot of variability, which creates a circumstance that can we really be that wrong?

That is, could the real but unknown 95th percentile for dolomite over that time period, or in any given year, another way to look at it, another given year, because they're all people that may have worked there a couple of years. I mean they're assigned an exposure.

Is it possible that at certain locations in certain years, that that person actually experienced something that was double, triple, quadruple that for an entire year, or is that just something that can't be, you know? Because what I'm hearing is the essence of the problem is we could sit around here and discuss should we use 400, 500, 600. But maybe that's not going to solve our problem.

If our sense is that the data are just too incomplete, and our knowledge of what took place is too incomplete, that all we're doing is sort of fishing in the dark, to pick a number that we think we could agree upon, you know.

I think what I'm concerned about is we could work on this problem forever, and given the concerns regarding the completeness or inadequacy of the data, we're never going to come to a place that we could be confident that we've captured it, or do you feel that, and I'll go back to my first question, or do you feel that, you know, if you did take another 400 samples of dolomite with the 95th percentile, it would be much different than the one we're looking at right now?

MR. WERNER: Well John, you've asked a series of questions. I appreciate you rewording one of them, because the question of would it be different, I have no idea whether it would be different, and I wouldn't even begin to speculate about that.

I guess I go back to the methodology, that in real technical operations, that presented, I think perhaps the most significant area of unanalyzed variability, was the reprocessing operations themselves, as we described in the report.

The reprocessing operation changed among the different sites and over time, and the result of that was, you know, from the perspective of where we are now, changes in the plutonium and transuranic concentration. From the perspective of back then, you know, the goal was to maximize the useful fissile materials or other nuclear materials you're extracting out of it.

And I again can't emphasize enough how ingenious the chemical engineer and operations staffs were at coming up with improved methods for producing supergrade plutonium through integration with the reactors and the reprocessing operation, that there was just a lot of changes that went on at the time.

That's the level of detail that 2000-B didn't have time to get into. The turning back the clock wouldn't be to 2000-B. It's not realistic to say you could have done it in the amount of time. Again, our goal wasn't to -- the goal of the report was not, absolutely was not to provide all the details that would support a thorough dose reconstruction.

At the time, the goal of that report was quite different. It was a lower threshold. It was simply to document and necessarily know what their, you know, particularly the percentages, when you talk about uranium, and also was that, you know, kind of order of magnitude, a sufficient problem that would warrant enactment of the worker comp legislation that we're now trying to implement, and was the amount of money such that it would, you know, break the bank. I mean just a bounding analysis.

There were people from both sides of that argument. Some said well, the problem is a minor problem. Well, the report shows it was not a minor problem. Then there were other people who said well gosh, we can't begin to go pay everybody everything, you know. It would just bankrupt the country.

I think the analysis also showed, in bounding analysis, you know, it wasn't everywhere. I think the report was a success, and it went further than that, just the fact that we're even trying to use that data to do dose reconstruction is sort of an extraordinary thing by itself, that such an enormous amount of data was put together in a very short period of time.

So I wouldn't turn back the clock now. I would turn the clock back to, you know, after 2000, for the next several years, and you know, it's just too bad that there wasn't more effort then, while people were still alive frankly, to use some of that detail process knowledge and, you know, where the bodies are buried, so to speak, where the data might lie, to get into the well, when did this process change? Did it really have an effect on the concentration of transuranics, and then get the records for that?

It would kind of go into that next layer of detail. And remember, the other thing that was going on, aside from the legislation being debated on the Hill, was the legal action against Lockheed Martin for the qui tam lawsuit. Somebody wondered earlier, I think John, why didn't we go back and use the NLO data?

Well, you know, there was just a lot of concern about where the barriers existed between Lockheed Martin and NLO from just the data. All of it should have been government property, but sometimes it was just hard piercing the corporate veil to get access to it in that short a period of time again. It was all very time-dependent.

But you know, Lockheed Martin might have used it, those problems were inherited from NLO, and NLO could have said it was, you know, Lockheed Martin's responsibility during the time of new DOE orders, you know. I'm not a lawyer to get into that, but it was sort of an issue at the time that, you know, the simultaneous putting together a report during the legislation.

DR. MAURO: Let me -- I did have another, I want to ask the same question of Bryce, because you see, where I'm coming at, and I'll step out and let you get back in. But clearly, what we have is an opinion, a very well-informed opinion from Jim, that he doesn't believe he would hang his hat on the 400 number, as being a good upper 95th percentile that would capture the range of.

I would like to know what apparently Bryce feels and Mark feels, no. I think that probably is a pretty good number to hang your hat on, what the upper bound is. It's very interesting that we have two separate people who are very versed in the subject.

One would say I think you caught it, and one says I have no idea whether you caught it.

MR. RICH: John and Jim, I've appreciated the comments. But can I make just a different perspective comment? My background, Jim as I started out as at the Idaho chemical processing plant in 1953. So I'm familiar with that process and the process of Hanford and the others.

The data does show from Hanford that there were process improvements. The data show that their record of parts per billion gradually decreased by several parts per billion over the years. The Savannah River plant was built, as an additional technological advance, and their results were in the three parts per billion range.

The chem plant never did put their recycled uranium into the general system. It went straight Y-12, and then wound up in -- it never did make it back into the general system at all. I will say something from my own personal experience, on the way the Working Groups worked at Fernald, and other places, because I was involved in a review capacity in 2000.

The effort of these teams was to default high. They were looking for the highest points in the process. So there was a conscious effort on the part of those teams to identify high levels, the higher levels, and they were competent in identifying samples that were alike in certain processes.

So my personal opinion of the reliability and the value of the processes, and particularly in what we're trying to do in a bounding sense, I think they're good. I think they're adequate, and I guess I'll probably just leave it at that.

MR. STIVER: So Bryce, I'm going to play the NIOSH advocate a little bit here. You know, Jim's brought up a lot of issues about the feed material, you know, the source term coming into the plant being highly variable over time, as well as the space and from different sites and different processes.

But when you start getting into the production plant, the metals productions in Plant 5 at Fernald, doesn't that kind of become a moot point in a way, because materials that came in that were, you know, that were high are going to be downblended.

I assume there's eventually a saturation point with magnesium fluoride, where you can't -- it can't absorb more and more plutonium and strontium and other elements indefinitely. You're going to be kind of -- it's going to be kind of a sigmoid curve and you're going to build up, and eventually you're going to plateau out in a saturation point.

So maybe those data, as John's saying, are these 400 data points, are they representative? Could you have another batch of magnesium fluoride at another period in time, that might be an order of magnitude higher, or two or three times higher, something that would make a big difference in trying to assign a bounding dose?

Or is that going to pretty well be representative of what you'd find in that type of process?

MR. RICH: Let me just give you a line of reasoning here. The activity that came in from Paducah, from a plutonium parts per billion standpoint, was upwards of 4,000, four parts per million. And if you take a look at the process streams in that period of time, we see that the maximum levels in magnesium fluoride, which is high, the ratio is high because the amount of uranium is less.

But that's going to be in the four or five hundred parts per billion. So it's a factor of ten down, of what came into the plant. Now I don't think you can apply that same ratio of decrease to the average plant from the maximum that came in, to the average that came in from Hanford, for example, because they handled that in quite different ways.

The U03 that came in from Hanford, example, had a certain parts per billion, generally in the five parts per billion range. Then it went a number of ways. It was either sweetened by material enriched uranium from the gaseous diffusion plants directly; in other words, it was blended up to a higher enrichment area, and never run through the extraction plant again. It was pure when it came in.

There was a time period, then, when shortly after that, then they took the U03 from the production site at Hanford, and used it directly in the plant, again it did not frequently -- it was not reprocessed through Plants 2 and 3. It did not go through the extraction column, because that was a PUREX plant, and it had come free of contaminants from the other site.

So and because of the fact that it was at five parts per billion, certainly less than ten, then their concern in blending was less than what you had when you received the acknowledged extremely high activity levels from the trash from Paducah.

So as a consequence, you can't do this straight downblending. But you can look at Process Group 6, which is a direct correlation of the activity in the U03, and that went to places like Weldon Spring. I don't know whether those are helpful or not, but I have a lot of confidence in the fact that the records from the production plants are good.

They define, plus the Savannah River. Of course, we had some different, and I would just add one other thing too. As we look at the record, it turns out that there are a number of study groups that were formed in '73, and a number of them specifically directed at -- and the Oak Ridge operations office was involved in all of these, highly coordinated and had numerous meetings on what would come into the plant and whether it was safe or not.

So there was a '73 working group. There was an '85 working group that was appointed and had a program, a specific objective program to examine the material that came in from West Valley, because that was different. That came out of commercial fuel reprocessing.

So the DOE 2000 team had access to

all of those previous reviews. So I would just indicate to you that my opinion is that the objective was to look for the highest levels to bound the problem for the 2000 report. Now we've taken their results and bounded it one step higher.

We've taken the maximum level that you find in a process stream, and use that for every worker that had uranium results.

MR. WERNER: This is Jim Werner, and I appreciate your restatement very well of the sort of background, and then the bottom line, that the idea was to provide a bounding estimate.

But again, with respect to the groups, the people could only work with the resources they had, and for example, the '85 report that pulled together information and went around and surveyed plants at that time.

In fact, they -- you know, as you said, the notion was that, for example, what valley reprocessed commercial fuel and they did do commercial fuel reprocessing, and that was its main mission. But in 1995, of course, we released previously classified documentation, that is DOE did, that indicated that in fact that it's more than commercial fuel.

They received some material from the DOE facility in terms of input, and in terms of the output, the report also revealed previously classified information that would be classified at that time.

West Valley also shipped out some of the extracted plutonium and material that was used in weapons tests out in Nevada, and that was one of the data used in, and later on I will give the details of it, but that show that in fact you could construct an operable fission device with material out of West Valley.

So the report was then necessarily incomplete, and that's why I'm saying that, you know, that people could only do what they had access to at that time, in time to analyze and there really wasn't time to untangle all of that, and it should have been done later, to give you greater confidence in the bounding estimates.

MR. RICH: That's true, Jim, and of course in addition, of course they were doing neptunium separation during a period of time, and they had a number of things going on. What I'm saying is that there was an effort to bound and find the higher doses, and I feel like what we've done is bounding. So that's my personal opinion, based on my own experience.

MR. ROLFES: And that's what the data support. This is Mark Rolfes. The data that we've seen supports that our situation that we're using this proposed approach is a bounding approach. It's the 95th percentile level. We can go on about, you know, whether the data is complete or not all day. We had these discussions. Well, what about the data that we don't have. You know, is there other data sets out there.

To me, based on my quick look at it, it appears that the data -- now correct me please if I'm wrong, Bryce. But it appears that the data in that Group 10A, the magnesium fluoride.

MR. RICH: That's Group 8.

MR. ROLFES: Okay, 8. Okay, looking at my wrong sheet here. The 342 parts per billion in Subgroup 8, was that data collected after the processing of the Paducah tower ash?

MR. RICH: Yes.

MR. ROLFES: Okay. So that is the highest contaminated material ever to come on the Fernald site?

MR. RICH: Yes.

MR. ROLFES: And it subsequently resulted in that 342 parts per billion. We're proposing to default 400 parts per billion. So we are exceeding the highest concentration of enriched magnesium fluoride for all time periods. We know that Fernald never received concentrations of plutonium which exceeded the Paducah tower ash.

DR. MAURO: Great, great. So you just gave the reason why you believe the 342 or the 400 number is probably at the high end, because it happened to be the samples that were taken, and this is a heuristic, after I guess the tower ash.

MR. STIVER: There's a problem with that.

DR. MAURO: Oh yeah?

MR. STIVER: Because even though that would happen during that time period, that material was downblended. So what was actually being produced in the metal was not exceedingly enriched in plutonium, if the downblending was conducted in the way it should have been done.

DR. MAURO: I see.

MR. STIVER: So it's probably representative, and that may be a good thing.

It may be representative of what's going on in the 80's, as well as in earlier times.

DR. MAURO: So the downblending really takes the chief out of the argument you made. See, the downblending is taking place, so that once you do go into the dolomite production process, you're really working with the same material, in other words.

In effect, you don't go into your bomb. That's as you inspect it, to the place where you want it to be. So you're going to start with whatever your spec is for the uranium that you're trying to reduce. Okay. So that really does take a little bit away from the post --

MR. RICH: John, I appreciate those comments, and also, just to add one more thing, they were still bound by keeping their product at a certain level, below ten. On occasions, it was above. But they were blending and operating in such a way that the product would meet the standards.

DR. MAURO: So then the dilemma becomes -- fine. Let's say you're working with -- you're not going to go into the reduction process unless you get your uranium, I guess it's uranium, the green salt.

MR. STIVER: Basically, the U04.

DR. MAURO: You get the U04 to the place where you want it, and then you go with it. And of course, and that's going to contain some level of residual plutonium, let's say, or neptunium. Then that dolomite is used over and over again.

But you're saying at some point they're going to stop using that dolomite. It's exhausted.

MR. STIVER: Well, I think it was about a third or so would go into waste, and then the other would be --

DR. MAURO: Okay.

MR. STIVER: So a certain stream would go for recovery for uranium. Another batch that was evidently no longer usable would be waste, and then about half would then be reused. So you have a little bit of a loss rate each pass, but then you're adding more material in.

So I guess that gets back to my question about, you know, the radiochemistry of magnesium fluoride absorption with transuranics. What does that look like, and when do you reach a saturation point to where if you do, then you probably are not going to see these big excursions from that at some other point.

DR. MAURO: You see, that argues too. It's unlikely that they missed the high end. In other words, I'm really trying to listen to this with an open mind, and the sense that the green salt that went in was under controlled conditions. It was controlled condition.

You got 400 samples of the dolomite. We recognized there's going to be variability in the dolomite depending on its age and the number of cycles it went through. But it sounds like we have some affirmation that says it only went through a certain number of cycles before it was exhausted.

And we're asking ourselves is it possible that we missed the high end with these 400 samples? I mean maybe I'm making it too simple, but it seems to me, I find it hard to believe that you missed the high end.

MR. RICH: John, I appreciate those comments too. Let me just add a couple of things about the magnesium fluoride stream. Obviously, they did an analysis to see if there was enough uranium left that it was above or below the discharge limits. So they reprocessed it with a leach process, and then run it through the extraction columns in Class 2 and 3, if it was worth recovering.

Of course, the enriched uranium, which had the bulk of the -- it had the higher levels of contaminants, was the most costly. So they processed those, and going through, of course, the extraction columns, there were some that came out, we're not real sure whether it was 80 percent or 40 percent.

But nonetheless, it came back and were used. But eventually, it was discharged, and so it did not stay in the system forever. You know, it was a discharge plan based on a number of criteria, one of which being below the discharge limits for uranium, but also other chemical and viability characteristics of recycling.

CHAIRMAN CLAWSON: Basically, this is Brad. You know, we could debate this and we've been debating this, I believe, for four to five years now, and basically we haven't gone anywhere. Mark Griffon wasn't able to be with us, but he sent in an email that I'd like to read to you.

"Fernald Work Group Motion. Brad, unfortunately, I'll be unable to attend the Work Group. I have, however, reviewed the new approach offered for recycled uranium by

NIOSH, and remain concerned about the approach.

"Some of the concerns include No. 1 still remains under excessive. How much data is from Fernald, not other sites such as Hanford?

"No. 2. Some subgroups categories of great interest, incinerator ash, ICP, and tower ash 9 and 10A. Have a small number of samples and a very wide distribution of results. Probably applies mostly for the 70's and 80's.

"No. 3. 1953 to 1960, there is no data. Still appears to be relying on Hanford production specifications, 100 parts per million. Because of these concerns, I would like to make a motion as follows:

"I move that a Class be added for all workers who have had the potential to be exposed to RU for the period from 1953 through 1985. If possible, could you read this motion for consideration by the Work Group at the meeting tomorrow? I may be available periodically during the day tomorrow. You can reach me," and he gives me his email.

Basically, I'd like to second that motion, because we've basically been here for four to five years. We're not going to come to a sense of closure on this. The Work Group like I say, is not the final say, but I think we've got to bring it before the Board.

So I'd like to make a second to this motion that Mark has just made, that we add this Class. Is there any discussion by the Work Group?

MEMBER ZIEMER: Well, it seems to me that motion is premature. We have some other issues that we haven't discussed here, that were brought to us just over the weekend.

CHAIRMAN CLAWSON: This is only recycled uranium. All the other ones that basically came back, the only thing that I saw any kind of movement on is the recycled uranium, which they moved a little bit on.

The radon, K-65 silos, basically it's the same thing that we've had for the last two to three years.

MR. ROLFES: To address some of the things, this is the first time I've heard about Mark's email. I hadn't seen it, but I sent it --

CHAIRMAN CLAWSON: He just sent it to me.

MR. ROLFES: Okay. There were a couple of things that I caught in there. There were some questions about the 1953 to the 1960 time period. Fernald actually did receive some uranium back from the Hanford site during that time period, but it wasn't processed until 1961. So none of the recycled uranium actually was in process at Fernald until after 1961.

I think you said a control level at Hanford of one parts per million?

CHAIRMAN CLAWSON: 100 parts per million.

MR. ROLFES: Okay. It's actually 100 parts per billion.

CHAIRMAN CLAWSON: Per billion, okay.

MR. ROLFES: Regarding the data for Fernald, we actually just asked DOE legacy management about the quantity of data. We asked for some analyses and such from the Fernald site, regarding isotopic analyses for some of the transuranic contaminants, and uranium specifications, isotopic analyses and such, and we got 450 boxes of records back, listed to us as having data responsive to our request on recycled uranium.

So there's certainly no shortage of data, but the way that data's presented, it would take quite a bit of time just to get through the data, and also to link it to specific processes. We'd be basically redoing the exact same thing that DOE completed in 2000, with essentially, I guess the end result being the same.

But what we've done, in the interest of, you know, claims favorability, from the very beginning we started off with a default which was a factor of ten higher than the control level at Fernald of ten parts per billion for plutonium on a uranium mass basis.

We started off with the 100 parts per billion. Because of the higher potential in the later time period to process uranium and concentrate some of those transuranic contaminants. So we, from the beginning, started off with the claimant-favorable approach.

Just because of, you know, the continuing concern from the Work Group, we reanalyzed the data, came up with the 95th percentile, for each subgroup of chemical processing. We're using this new 400 parts per billion, we'll use this to complete dose reconstructions.

Now I can point out that recycled uranium across the board, the concern of the transuranic contaminants for the majority of the organs, it doesn't substantially affect it. SC&A has identified four organs where some of the transuranic contaminants can result in higher internal doses than the uranium itself.

We're aware of that, and typically in the dose reconstruction process, the intakes that we assign typically already exceed and account for those correction factors of three to five. So the dose reconstruction process itself, exclusive of the uranium or excuse me, exclusive of the transuranic contaminants, the uranium intakes in dose alone usually account for the uncertainty from the contaminants.

Let's see. I'm trying to think if there's anything else that I wanted to point out here.

DR. MAURO: I'm sorry, Mark. I have to disagree.

CHAIRMAN CLAWSON: Mark, I

understand that. But I want to come back to something. You say you've got 450 boxes on recycled uranium data, and you haven't used them?

MR. ROLFES: Yes. We haven't collected -- we haven't collected it yet. We had basically been using the DOE 2000 report. We've looked at some of the results, just to see what kind of information is available to us. I haven't seen anything that exceeds our default.

So I'm comfortable with the 400 parts per billion. That's the 95th percentile level, and that's, you know, as good as it's going to get then. You can make, you know, whatever -- we'll continue to discuss it, and you can make your decision. But the science is here.

MR. STIVER: Mark, about these boxes. How long have you had them? Is this something that recently --

MR. ROLFES: We have not collected

these boxes. We had inquired with the Department of Energy legacy management about the data being available, and because of the timeliness issue, we didn't feel that we should go and look through 450 boxes.

MR. STIVER: This is a relatively recent development?

MR. ROLFES: Correct.

MR. ALVAREZ: This is Bob Alvarez. I'm curious. Have you screened the boxes to know their sources and content?

MR. ROLFES: We've done a limited review of some of the -- a limited sampling of some of the information contained in the boxes.

MR. ALVAREZ: Do you know whether or not the boxes contain any sampling data for residual ash and black oxide sent from the gaseous diffusion plants during the cascade improvement and cascade upgrade programs?

MR. ROLFES: I would have to go and look at the data. I couldn't tell you that's a pretty specific request.

MR. ALVAREZ: Well, that's a very important set of information, because it involved the essential removal and decontamination of something on the order of 2,400 converters over a period of a decade, and there's subsequent D&D and recycling of residual contamination in the converters, you know, in the barriers and all the innards of these GDPs, and a substantial amount of this material was sent to Fernald in a manner that appeared to be concurrent with the POOS material.

You would at least intuitively might want to consider that that material might have larger than expected quantities of especially transuranic contamination.

MR. WERNER: Bob, this is Jim. As we've discussed before, you're right. That would be a rich source of data to try to focus on the question at hand. But as I understood it, the 400 boxes, the Fernald-specific material from LM, and as I recall the way the records. There's a DOE order regarding records preservation.

LM is responsible for implementing much of that with regards to old facilities that have been cleaned up. This was in Fernald. So it wouldn't include, for example, from the kind of horizontal records point of view, they're complicated and would come from the GDP in Portsmouth, Paducah and K-25.

Normally, it would include materials that had already gone to NARA, the National Archives and Records Administration. Four hundred boxes is really just a, kind of a slice vertically and a slice horizontally, and I share the concerns that I think somebody just expressed, that yes, this load of data, it would have make sense to go back and examine them.

But boy at this point, there's really -- the 400 boxes will just be open and forgotten.

MR. RICH: This is Bryce Rich. Then there's a section in the 2000 report that talks about the receipts in K-25, and so it's not that, you know, they document the receipts from Paducah as well as -- and Portsmouth.

MR. STIVER: I believe Bryce, that's Subgroup 9. It has a lot of that --

MR. RICH: Yes. Well, it's 9. But it's also an appendix in the 2000 report that specifically documents the material that came from K-25.

MR. STIVER: I think we had been over this in a previous meeting, and there's one subset of that data that didn't make it into the 2000-B report. I think it was a total of about 80 metric tons, and it might have been about 20 that were not accounted for, if my memory serves.

So it gets back to the issue of the uncertainty in the available data. It's something that I really firmly believe, that if we're going to pursue this, that needs to be done.

MEMBER ZIEMER: Well, I have a question for SC&A. Well, maybe it's two questions. Number one, does SC&A still have concerns about the 10A category or subgroup, or were you satisfied that that could be omitted because of its limited --

MR. STIVER: I think for a robust statistical analysis, Harry would agree with me on this, that all the data needs to be reviewed.

MEMBER ZIEMER: Okay. That's one part of it. Now aside from 10A, I guess it was Subgroup 8 was --

MR. KATZ: Can you hold, because we lost the lines.

MEMBER ZIEMER: We lost it? Okay.

MR. KATZ: Hello, everyone on the phone. We have a lightening storm here and it killed our power and killed our line for a second. But we stopped the conversation, so you haven't actually missed anything.

PARTICIPANT: Okay, you're back on now?

MR. KATZ: We're back on now.

PARTICIPANT: Okay, thank you. We were wondering.

MEMBER ZIEMER: Okay, this is Ziemer again. So the question I was asking is two parts. One had to do with Subgroup 8, which seems to be the basis for the 400 parts per billion, and the other had to do with sort of the question of should 10A be included or not. Bryce has indicated that one reason or the rationale for excluding 10A was very limited use of that. Basically, to extend that over all time periods didn't make sense.

Now what I want to ask, this is just a practical question, if 10A were excluded, assume for the moment that it's okay to exclude that, would NIOSH, in using the 400 part per billion value, what you're doing then is based on the uranium information for each individual, or you're going to assign it across the board. How are you going to do dose reconstruction? If people agreed to the 400, what would you do?

MR. ROLFES: Well, what we do in dose reconstruction, for the reconstruction of internal dose from uranium, we would take that individuals on uranium urinalyses, and the uranium urinalyses were reported in units of mass at Fernald. So we would convert the mass units into an activity. We'd multiply that value by 1.4 to account for the urine production rate for the entire day, 1.4 liters per day.

During the time period that this material was processed, we'd defaulted to a two percent enrichment. So we're using a specific activity of two percent enriched uranium to essentially multiply another factor onto the activity being excreted.

We'd take those series of uranium urinalyses over the individual's operational work, say at the individual work from 75 through 85. We would assign an intake for the entire time period, from 1975 through 1985, of the uranium isotope that results in the highest internal dose, and also the solubility Class that results in the highest internal dose to that particular target organ in the dose reconstruction.

So once we've done that, we would add in now 400 parts per billion of plutonium on the uranium mass basis. We would add in 11 parts per million of neptunium on a uranium mass basis, and 20 parts per million of technetium on a uranium mass basis. Then we would have the internal dose.

MEMBER ZIEMER: Right. Now what I was trying to get a feel for is suppose you said okay, during that limited time period, whatever those couple of years were, that people might have been exposed to the values for the 10A group? If you did that, in other words, here's a guy that's worked for 30 years or something, and you have this default value.

Suppose you had a different default value for a very limited amount of time? How would that affect things?

MR. ROLFES: So then you have a two-year period --

MEMBER ZIEMER: Whatever you can identify with 10A, and I also would ask Bryce if that would make sense. I'm not sure how -- because the way you do dose reconstruction when you're talking about it, you take these points, but you're going back from when the bioassay was made, and you're assuming the worse possible intake that could get you to that point.

So there's kind of a smoothing, but it's very much on the high end. If you superimpose higher intake for those few years, it may have almost no effect. I don't know that it would, but I'm sort of thinking about it that way, and John, maybe you could react to that.

But the idea of saying okay, let's

include a higher default value in a certain way, but it doesn't make sense to include it over the whole time period.

MR. KATZ: One second, one second. Someone on the line has a dog barking. Could you please mute your phone -- *6 if you don't have a mute button, but I'm very concerned that other people on the phone won't be able --

MEMBER ZIEMER: I'll translate his remarks.

MR. STIVER: Maybe he's saying something inspiring.

(Laughter.)

MEMBER ZIEMER: Make as much sense as what I'm saying. I don't know.

MR. RICH: Well, this is Bryce. The high level stuff from Paducah was processed sporadically through a period of years, as has been stated, and it was mindful, during all of that period of time, identified as a specific process string.

Personally, I don't think that that is either viable or technically realistic to assign any individual that high levels for that period of time, and particularly everyone on site.

The 400 parts per billion is going to default enormously high, particularly in the case when you do a blending of a container, a few metric tons of prepared waste from Paducah. It only takes an afternoon or less, a few hours.

Then the individual is right back with a uranium fluoride or some other process stream, which is probably a couple of orders of magnitude less ratio. So it doesn't make a lot of sense to me to find a high dose for a period of time, to accommodate the higher levels that are seen in incoming material from Paducah.

MR. STIVER: Bryce, this is John Stiver again. You know, the way I would envision this going, if you have the ideal dose reconstruction methodology, you would have, I'd say, from 53 to 73. 100 parts per billion would probably be reasonable for that period.

You know, once the other materials start coming in, you know, you'd go for 400 or whatever. But I believe that data shouldn't be excluded. But how about, I would propose doing some kind of a weighted average during the period of time during which that material was accessible, and could potentially have resulted in end dose.

I don't know how you would go about doing that statistically, but instead of just defaulting to that highest value for however many years that the 10A group was being processed, you know, have some sort of a weighting factor that would account for it, and would at least give it some recognition later in the reconstruction, in proportion to its contribution to dose.

DR. MAURO: You just jumped -- yes.

Let's get back -- you jumped.

MEMBER ZIEMER: Well, I think what Bryce is saying, that I was thinking about it, that they might be working there for a couple of years. But I think Bryce is saying that whenever they did those runs, they were so limited that to assign it for that year wouldn't make sense if they were working there for an hour.

MR. STIVER: Yes, it may spike for maybe a day.

DR. MAURO: So what I'm hearing is that a --

MEMBER ZIEMER: You see, if you weight it that way, if you weight it -- if you said they worked that year and throw in a few hours, it's almost not going to affect it.

DR. MAURO: So in a way, no, I think I was right. What I was hearing from you is that okay, we're coming up with this concept, and it sounds like I for one buy in on eight on 400, for the reasons we've just discussed, okay.

MEMBER ZIEMER: Yes, I do too. I do too, and I was just worried about the 10A.

DR. MAURO: Now we're going to say -- you know what we're going to do? We're going to give that to everybody, as if everybody was at the 400.

MEMBER ZIEMER: Right, right.

DR. MAURO: And then you asked the question, and this is where I think you were going, and that's where I started to go as soon as you started to move in that direction. Okay. Along comes this other stuff, this nasty stuff. Now right now, they've got 1,732 parts per billion. Sounds like we've got a little debate going on, was that the real 95th percentile or not.

But let's for a moment presume that we went in to grab a lot more data and yes, that holds up pretty good, just for the sake of this. But we also know that it was there, what I'm hearing is it really was, there was no individual that was going to be continuously exposed at this level through the years.

So all of the sudden what happens is that all, when you're assuming, if a person's working there 30 years, 20 years, and you're going to give them 400 parts per billion every year after year, which you know probably is not reality, but you're going to give them that anyway, that more than accounts for the fact that maybe a couple of hours a day this year, and a couple of weeks per year that year, you might have got hit with some of this higher stuff.

So what you're saying is sort in the buffer, that takes care of the uncertainty that lies in the special CIP/CUP. I guess this is the CIP/CUP material?

MEMBER ZIEMER: Well, I wasn't saying it was. I was kind of asking this question, and I think based on Bryce's remarks, it makes me feel pretty confident in the 400, that those brief times probably couldn't impact the distribution, particularly the way they calculate internal dose, where you take the urinalysis value and then you back up say and what have the intake, what would the intake be way back since the last one, to get you where you are here.

MR. STIVER: I have to tell you guys here. I think the 400 for the Group 8 is a pretty solid number. The issue is really, you know, how do you handle those potential sporadic higher exposures? In fact, you're giving this for somebody for a period of 20 years, 10 years, whatever, and then you're also throwing a GSD of 3 on the end result?

MEMBER ZIEMER: No. I thought at first Bryce was saying that it only occurred during a couple of years. I think Bryce, you're saying it may have gone on throughout the period, but only for very limited times. Is that, am I --

MR. RICH: That's correct. Your

understanding is correct.

MEMBER ZIEMER: Yes, thank you.

MR. STIVER: Yes. The two years came from this site boundary data here, that shows a spike in plutonium in '83 and '84, and that's where we got --

MEMBER ZIEMER: Got you, Okay.

DR. MAURO: You know where that leaves us? That leaves us that we trust that that 1,732 is a good 95th percentile. In other words, we just constructed a model for how to simulate, that really everything hangs right now on do we trust. Because it sounds like we do trust the 400, for the reason we discussed.

Are there reasons why we can't trust the 1,732, because there may be other batches out there, other things that were going on that might have missed it?

MR. STIVER: Well, the issue was that was the high degree of uncertainty.

DR. MAURO: All right. That's what

I was --

MR. STIVER: There's very few samples. The one hopper we did look at is an order of magnitude difference in the two measurements in the laboratories. So it's not homogeneous. You've got to wonder, you know, what uncertainty would have to be applied to that. But then to counter that, you have sporadic exposure. You don't have to --

DR. MAURO: Right. See, I think we're zeroing in on the model in our heads about, you know, given the inadequacy of the data, can somehow we live with this and the incompleteness of the data now? But I also hear we have all these boxes that really can help us answer that question. I mean from all specific --

CHAIRMAN CLAWSON: Well, let me jump in on something. We've been at this for four, five years now, and all of a sudden, the boxes pop up. I'll use Mark's reference here when he says "I guess." One thing this compensation program, this isn't something that's 101 or whatever else like this.

For me to be able to find out now that there's 450 boxes of information, is kind of frustrating to me. I thought that we were supposed to start out on this, be able to gain all the information that we basically could on this. To tell you the truth, I was going to call this untimeliness, but Mark beat me to the punch on this.

The other thing is, is you're right. They did sporadically, throughout the years, they had other ash coming in. We don't have an idea for it. This whole thing comes back to that we have been sitting here for four or five years, going around in circles on this whole thing.

The bottom line is yes, we've got some data, yes, it's questionable. The bottom line is this is very questionable, in my mind. So I guess that there's no way that we're going to be able to get to this point, and you know, I appreciate Bryce's comments on this and stuff too, because I basically have worked at the chem plant too, and I'm still dealing with the transuranics that we have out there, and some of those are pretty interestingly quite high.

The issue is to me right now that there has been a motion put out onto the table by Mark.

And my thing is it really upsets me that at the 11th hour, all of the sudden we find 450 boxes. Even if Mark doesn't make this motion, I'm going to make a motion on recycled uranium, bottom line.

MS. BALDRIDGE: Can I interject? This is Sandra. When the Site Profile was initially made for Fernald, you know, in there it says I'm going to jump to the thorium. The records have been destroyed. We don't have any thorium. We have reconstructed data based on the best science available.

Then I present the petition that has

I don't even know how many documents for thorium data, that was all stored in Cincinnati, that they totally missed up until the petition was presented in 2005. Now to find out if they know that there's more information, not the thorium, but something else that they haven't bothered to get?

That's really disturbing. You know, as a person eligible, my mother is 97 years old, and she is fighting day by day for her life to see this resolved, for claims that were submitted in 2001, that now 11, 10 years later there are still boxes of data that apply to these workers, that they haven't bothered to get? You know, I just --

MR. ROLFES: To clarify, the boxes of data are not health and safety data. We have all of the health and safety data available to us from the Department of Energy. That does include plutonium bioassay for the period following the processing of the tower ash. So we have several hundred plutonium bioassays. There's not a lack of data.

We've looked at the plutonium bioassay, and there's no one that had any intakes of significance. You know, if we're talking about an operational period during the 1980's, followed by a bioassay sample in 1986, was the first year that they were bioassaying people for plutonium, if there were significant exposures, you would still be able to detect plutonium in urine.

And I believe out of the several hundred results that we have, there were some which were right at the decision level, or at the minimum technical level of the uranium urinalysis method. Those people were counted in an in vivo counter at PNNL or Hanford, I believe. They were hand-selected, because they had borderline results that were right around, you know, whether or not they could have been exposed to plutonium.

And their lung counts came back as not-positive. They showed no plutonium in their lungs. So there is additional data on top of what we've already discussed, that can be used for dose reconstruction and bounding plutonium intakes.

DR. MAURO: How many people who have plutonium vis-à-vis the chest count? What is the plutonium?

MR. STIVER: I think what you're talking about is after 1986, when they started processing that stuff again, they had a pretty robust set of procedures and processes in place. They did a bioassay to begin with, before working with the POOS. They did it at six month intervals and at the end, and I think they had somewhere over 1,000 workers.

DR. MAURO: 1,000 workers.

MR. STIVER: Now this is after Westinghouse came in and cleaned up house.

DR. MAURO: These are workers that were --

MR. STIVER: This is from about '86 to '89, primarily in Plants 4 and 8. Now this doesn't -- we're not talking about the people -- we also did No. 5. We also did Plant 5. Basically, every place that the stuff was being made. That's why I asked about when it was downblended.

If it had already been previously downblended, it seemed like an awful lot of concern over POOS, unless the downblending wasn't successful and they didn't have the data they needed. But during that period of time, Mark's right. There was a few values that were thought to be positive. They sent them out for chest counts and they came back negative. But this is post-'85, and we're talking about up to '85, when NLO was still in charge.

MEMBER ZIEMER: This is Ziemer again. I agree with Sandra and with Brad on the timing of this issue, and I think it would be a mistake for us to, you know, start digging into another set of boxes and go through, stretch this out. I don't think they're going to be that productive, number one.

Number two, that will add some more years to this process, and I think almost every site we can think of, and I have the same issue with some other sites I'm involved with, there's always going to be something that you didn't find. At some point, you've got to say okay, we have, we've got to make the decision.

This is one. It has gone on for quite a few years. I think we have a lot of data here. I don't support the motion, but I support the idea of going ahead with what we have.

I'm very comfortable with the 400, based on the data set that we have and the values we see for most of the runs, and the idea that this will, this is extremely claimant-favorable on making these levels of assignments to all the workers on the uranium, plutonium and so on.

But I do think it's time we come to a decision point, however we fall down on that. You know, I speak against the motion, but I'm in favor of going ahead.

CHAIRMAN CLAWSON: I understand Paul, and that's your personal opinion.

MEMBER ZIEMER: Yes, sure.

CHAIRMAN CLAWSON: And I basically, I agree 100 percent, because to tell you the truth, until I got this email, I was going to call a time limit on this today, because I made it very clear in our last Work Group meeting that we're basically to the end, and now I hear 450 more boxes.

Basically so what I want to be able to do at this point, and we've still got other topics to be able to talk to and stuff like that. But I've already put a motion onto the table, because the Work Group here is not the final say on it. It's the Board.

MEMBER ZIEMER: Right.

CHAIRMAN CLAWSON: The bottom line

is as a Work Group, we do the preliminary work, putting our findings to the Board and they're going to make the decision. That's also why I asked for all the information from SC&A and NIOSH, that the information be put out on the O: drive so they can review it, which I appreciate you both putting out there.

So basically, I seconded the motion, to be able to take this to the Board at the May meeting.

MR. KATZ: Just to add to the discussion about the motion, the motion, as Mark wrote it, I thought again in '53 or something like that; is that correct?

CHAIRMAN CLAWSON: Yes, '53 to 1985.

MR. KATZ: And then Mark made the point that there was no processing before '61 on --

MR. ROLFES: Correct.

MR. KATZ: You need to at least discuss that matter, because it doesn't make sense to begin in '53. You don't want to go forward with the motion that doesn't have any support to it.

MR. STIVER: I can weigh in on that.

MR. KATZ: Yes. I mean I just think you need to discuss it. I'm not --

MR. STIVER: We had discussed this in previous meetings, and there was about 45 metric tons between, that arrived between '53 and '61, and Mark indicated that it didn't go into processing until that point, but you still have material being handled, you know, until it got to the Plant 1 -- those activities, which would involve some potential exposures.

That's why we felt that '53 was probably a better number to start with than '61. That's been one of the points that got lost, you know, when these bigger issues came up. I know it kind of fell by the way. But the starting point for the period.

MS. LIN: Can we check if Mark on the phone. We're a little concerned with someone who is not participating and then making motions.

MR. KATZ: Mark Griffon, are you on the line?

CHAIRMAN CLAWSON: He was going to try to mail in. He just emailed me this. Actually, he emailed it last night.

MR. STIVER: Yes. He had an eight o'clock plane.

MR. KATZ: I mean if he's not available, you can just call it by -- someone else can make the motion and someone else can second it, to keep processes square.

CHAIRMAN CLAWSON: I'll make, you know. One of my questions, you brought up something, the 1960, the '61 to 1953. Do we have -- and he calls out in this that 1953 to 1960, there's no data, no samples; is that correct? No data for --

MR. ROLFES: No. There's data showing basically -- one of these, and Bryce is probably the best person to explain this, but he had addressed, there were some shipments from Hanford back to Fernald beginning in, I think, 1953, around there, and that material was sent back to Fernald but was not processed until after 1961.

If you take a look at the levels of transuranic contaminants in that material, that was some of the cleanest material that was sent into the Fernald site, which was designated as recycled uranium. I think it was around three or four parts per billion of plutonium on a uranium mass basis.

MR. RICH: That's right, Mark. It was in the five parts per billion range.

MR. ROLFES: Five. Thank you, Bryce.

MEMBER ZIEMER: And if you were doing dose reconstruction for those years, what would you do?

MR. ROLFES: We've already defaulted for all those reconstructions to 100 parts per billion, which is a factor of 20 times higher than the material coming in.

MEMBER ZIEMER: For that period.

MR. RICH: Mark, in our recent table, we default at six.

MR. ROLFES: That's true. We did go back and look at the actual data for the earlier time period, and based upon the analysis of the actual data, we've recommended changing the earlier time period to six. However, we've already completed, you know, 90-something percent of the dose reconstruction for the Fernald site at 100 parts per billion.

MEMBER ZIEMER: Even though there was nothing here that was that high?

MR. ROLFES: That's correct.

CHAIRMAN CLAWSON: Is this the paper?

MR. STIVER: Actually, according to Table 3 here, you're going to do this from '61 through '73. I've got a real issue with that. I mean if we agree, say for the sake of argument we agree with the 400 from '73 to '85. I think that part of that, because of the issue with magnesium fluoride not being influenced by the arrival of highly contaminated materials used for downblending.

I think you're going to have the same problem in the earlier years as you had in the later years. Because you get the highly contaminated materials does not cause the magnesium fluoride issue to increase as much as the downblending. That's part of --

CHAIRMAN CLAWSON: Is this the information that you were saying?

MR. STIVER: This is an NLO report.

CHAIRMAN CLAWSON: NLO?

MR. ROLFES: That's part of it. What's your question regarding that?

CHAIRMAN CLAWSON: No data for plutonium.

MR. ROLFES: I can't quite see here.

MR. STIVER: This is the NLO report that was in, I think it was --

MR. ROLFES: Oh, 1985. This is the 1985 report, correct.

MR. STIVER: Yes. It doesn't show that no plutonium content prior to '65 here.

MR. ROLFES: Right. That's actually representative of the content of plutonium. There wasn't a measurable quantity. I mean we're talking, the first reported quantity here is .019 grams, versus nearly a million kilograms of uranium.

(Simultaneous speaking.)

MR. ROLFES: I'm sorry, I was speaking. I didn't hear you.

MR. STIVER: Oh, I'm sorry. I was wondering if that was because it was below the detection limit or it just wasn't measured?

MR. ROLFES: Well, it appears to me, since there's nothing entered in here. We've got in 1964, there's 780,000 kilograms of uranium that came into the site, and there's no plutonium recorded. The next year, 1965, is 8,174 kilograms of uranium, of they've recorded .019 grams of plutonium.

So we're several orders of magnitude, and the total plutonium and parts per billion for 1965 was two parts per billion of plutonium on a uranium mass basis.

CHAIRMAN CLAWSON: So on the same theory that you're using, let me ask you a question here. Did they sample for plutonium? Are you sure that they sampled, or --

MR. ROLFES: From the very beginning, every shipment that left Hanford was sampled for plutonium, before it was sent to Fernald.

MR. STIVER: My concern here, Mark, was that maybe it's not that there wasn't plutonium, but it just wasn't measured or it wasn't accounted for, that there weren't measurements available, for the summary table put together in '85.

MR. ROLFES: I can't answer the question. I don't know the answer to that.

CHAIRMAN CLAWSON: Well, you know, I

understand your concern for it, so basically I'll make the same motion. I move that we have a Class for SEC to be added for all workers who have the potential exposure to RU for the period of 1953 through 1985. Basically, that's what I'm proposing.

Like I say, when this gets to the Board, we maybe will want to discuss this area more. But that's the motion that I put on the table.

MEMBER SCHOFIELD: I'll second that motion.

CHAIRMAN CLAWSON: So --

MEMBER ZIEMER: Just, you know, I'm going to be opposed to the motion, but the wording. Is it the potential for exposure or --

CHAIRMAN CLAWSON: Yes. Exposure potential --

MEMBER ZIEMER: I mean that's the wording, but is that something that, for example, the Department of Labor would be able to determine potential for that?

CHAIRMAN CLAWSON: Actually, I don't --

MEMBER ZIEMER: How would it be applied in practice?

CHAIRMAN CLAWSON: As far as?

MEMBER ZIEMER: Is it individuals for whom there's --

MR. ROLFES: I honestly can't speak for the Department of Labor as to how --

CHAIRMAN CLAWSON: I think this is the whole thing we've always got into. How are you going to be able to take people and put them into one of the things? One of the things that I find interesting is the site boundaries.

MEMBER ZIEMER: Well, I guess I'm asking, is it everybody on site? Does everybody have --

CHAIRMAN CLAWSON: That's what I would propose.

MR. STIVER: On site during that

period of time.

MR. KATZ: And I think that can get sorted out.

MEMBER ZIEMER: Yes, they'll sort it out. I guess, yes.

MR. KATZ: When there's discussion about this at the Board level, because this won't be the place to define a Class, if there's a Class to be added.

CHAIRMAN CLAWSON: I guess, do you want to -- I don't know how we do this, if it's a roll call or --

MR. KATZ: Do we have -- Bob Presley, are you still on the line?

MEMBER PRESLEY: I'm here.

MR. KATZ: Oh, and Mark Griffon, let me just check, are you still on the line? I mean are you on the line?

(No response.)

MR. KATZ: Okay. Do you need to a vote?

CHAIRMAN CLAWSON: I'm going to take

the vote, I guess, by a show of hands. Who supports this motion?

MEMBER PRESLEY: I'd like to hear the motion again, please.

CHAIRMAN CLAWSON: Okay. I move that a Class, that an SEC Class be added for all workers who had the potential to be exposed to RU, from the period of time from 1953 through 1985. So Robert, how is your vote? Mr. Presley?

MEMBER PRESLEY: Yes, I'm here.

MR. KATZ: Well, so why don't we start in the room?

CHAIRMAN CLAWSON: Okay.

MR. KATZ: Brad?

CHAIRMAN CLAWSON: Yes.

MR. KATZ: Phil?

MEMBER SCHOFIELD: Yes.

MR. KATZ: Paul?

MEMBER ZIEMER: No.

MR. KATZ: And Bob?

MEMBER PRESLEY: No.

MR. KATZ: Okay. It's 2-2.

CHAIRMAN CLAWSON: 2-2.

MR. KATZ: It's a split vote.

CHAIRMAN CLAWSON: Split vote, and do you want to call and get Mark's, or do you want me to email him?

MR. KATZ: Jenny, is there --

MS. LIN: Well, how do you guys usually handle it, because I don't how you collect the votes.

MR. KATZ: Sorry?

MS. LIN: I don't know if we ever --

MR. KATZ: So with Work Groups though, we don't typically collect votes from absentee Members after the fact. We haven't done it on other Work Groups.

CHAIRMAN CLAWSON: So basically we've got a split vote, but as the Work Group chair, I want to be able to bring it forward before the Board in the May meeting.

MR. KATZ: That's fine, that's fine. We don't need --

MEMBER ZIEMER: I think you just report the vote, and Mark will be there --

MR. KATZ: Right, exactly.

MEMBER ZIEMER: And he can indicate his position on it, so it will be clear. It's going to be --

MR. KATZ: I mean the problem for any absentee Member is that they've missed the discussion. So they'll get a recap of that.

MEMBER ZIEMER: The years were '51 through --

CHAIRMAN CLAWSON: '53 to '85.

(Simultaneous speaking.)

MEMBER ZIEMER: '53 to '85.

CHAIRMAN CLAWSON: So Mark, you needed some of this changed? You wanted uranium first. Is there another one that you needed?

MR. KATZ: How about a comfort break?

CHAIRMAN CLAWSON: No way. I'm going to hold you guys here until --

(Simultaneous speaking.)

MR. ROLFES: Yes. If there's any discussion of the raffinates --

MR. STIVER: That was Issue 4, the radon raffinates.

MR. ROLFES: Correct, yes.

CHAIRMAN CLAWSON: So those are -- when do you want to discuss it, next?

MR. ROLFES: Correct.

CHAIRMAN CLAWSON: Okay.

MR. ROLFES: We'll do that.

MR. KATZ: Okay. So we're taking a 15 minute comfort break, and we'll be back at -- what time is it now?

MEMBER ZIEMER: It's 11:10.

MR. KATZ: 11:45, 11:30. You said what time?

MEMBER ZIEMER: It's ten after.

MR. KATZ: Oh, I'm sorry, 11:25. Sorry, sorry. 11:25, we'll be back. I'm just putting the phone on mute.

(Whereupon, the above-entitled

matter went off the record at 11:10 a.m. and resumed at 11:28 a.m.)

MR. KATZ: Are you ready Brad?

CHAIRMAN CLAWSON: Yes.

MR. KATZ: Are you ready Paul?

MR. STIVER: Yes.

MR. KATZ: Okay. So this is the Fernald Work Group. We're just reconvening after a short break, and we've been through an RU issue and moving on. Brad.

CHAIRMAN CLAWSON: Okay. I'm going to turn this over to John, but the next one that we want to discuss is out of sequence on the agenda, and that was thorium.

MR. STIVER: Yes. This was Issue No. 4, the radon breath data for adequacy in reconstructing doses using inhalation of radium-226 and thorium-230. This is one we have prepared a response in May of 2010. This entailed the review of the NIOSH White Paper on thorium-230 and other associated radionuclides.

The action item, I believe from the last meeting, was for you guys prepare a response on that.

MR. ROLFES: I believe our response is contained within Report 52, which is the consolidated report on internal dose topics at the Fernald site. That was sent out on Friday of last week, I believe. Well, I think we basically have fine-tuned our results of basically the notable things in this report.

SC&A, one of the questions we got previously was about the use of radon breath data to estimate the radium body burden and associated radionuclides from Silos 1 and 2, and we have basically put our radon breath data together and developed essentially a coworker intake model to, based upon the bioassay data, to reconstruct exposures to the raffinate materials.

This approach actually, based upon the bioassay data, we went back and compared the bioassay data approach to the approach in our Site Profile, that we originally had written back in 2003 or 2004. It turns out that the use of the bioassay data, I don't have the report pulled out right in front of me at this second, but I believe the Site Profile approach was about a factor of five higher than the actual bioassay data had indicated.

I don't know if we still have ORAU on the phone, possibly to point out any other updates maybe that we've made. I'm not sure if Bryce or Bob are out there possibly.

MR. RICH: I'm on the line.

MR. ROLFES: Okay. Bryce, have I captured everything that we've put together in Report 52 correctly?

MR. RICH: You know, just to review just a little bit, they had the first part, they discharged the raffinates directly into the silo through a mixing and transfer station. They took air samples and they determined that they would take radon breath samples, which they sent to Rochester.

Comparing the maximum breath samples, as you indicated, I think, to the radon samples, the results came out reasonably close. So there were some other issues associated with handling pitchblende ores in the Fernald site itself, which had radium in quantity, of course, and so we added to that the thorium, the possible thorium-230, and again defaulted high. So that's the basis for that write up.

MR. STIVER: I think I wasn't involved directly in this particular item. I have some listings through our findings, that maybe you guys can address. Category 1, we have four different categories of workers here, I believe. Category 1 were areas where uranium-238, thorium-230 and uranium-226 were present.

For example, in the pilot plant, a Plant 1 sampling, Plant 2 and 3, processing of uranium ores and so forth. For that one, the finding was that reconstructed thorium intakes are valid for workers who did not perform the job or spend time in the raffinate areas of the plant or the silo areas, where exposure to uranium was negligible.

So the idea being that you'd have to be continuously exposed to uranium, in order for this method to be valid. If you were in the raffinate area, where the uranium had already been separated, you can get a potential thorium intake that would not be accounted for, and this is similar to what we're doing with the recycled uranium. We've added it back into a uranium bioassay value.

So if a worker is miscategorized with respect to their location, the thorium body burden could be significantly unaddressed. Category 2 type exposures. This is the raffinate area located in Plant 3. Radium will be present in some but not all, and I believe you guys -- hang on just a second here. I think that the problem here was that raffinates were supposedly in a contained, in a piping system, and the NIOSH position was that that would not be a source of exposure.

But evidently, there were some documents that indicated there were leaks from these pipes in areas that could have constituted a source of exposure. So we had a problem with that. Let's see.

Category 3, I believe, was silo areas 1 and 2, where thorium and radium-226 were present for a short period. Okay. So this is where you have from 1953 to '58, you have radon breath data. The White Paper does not make any reference to how to calculate the thorium or radium-226 doses to workers in jobs, involving other jobs related to silos 1 and 2 besides the transfer of 13,000 drums of raffinate.

So I guess it's a completeness issue, of how doses would be calculated, for personnel who weren't working on those particular tasks for which they were monitoring at the time.

MR. ROLFES: Our response, I guess basically, is we've developed a coworker intake model to be used, based upon the breath data that we've collected, assembled and analyzed. We've got the intake levels documented here in this report.

DR. MAURO: When I looked at that, my question was, you know, we're comfortable with the radon breath analysis as a way of getting body burdens for uranium-226 and for the thorium-230, when you have to the two together, you know, in equilibrium.

And we're also comfortable with the fact that there probably are, probably some workers then you say clearly -- of course, the ones you have the radon breath analysis data, you use it. The question is are there other workers that may have been involved in these types of activities, where you don't have radon breath analysis, and in effect you're going to have to use these data as a coworker model.

I assume there are some workers that you're going to have to assign a body burden of radium and thorium, that might have been exposed to this material, but were not, did not have radon breath analysis. You have that problem, that is, knowing who you're going to put into that box.

MR. ROLFES: That's very possible, and you can also identify the individuals who had the highest exposures, because of their recorded gamma doses in those early years, dealing with the K-65 materials. So yes, if there's an individual that does not have a radon breath sample during that time period and has a high gamma dose, that would point us to, you know, that particular claim.

DR. MAURO: That will be a trigger to bring in the coworker model.

MR. ROLFES: Exactly.

DR. MAURO: And if you go with a

coworker model for something like this, where you do have the radon breath analysis, are you going to go with the full distribution or the upper end?

MR. ROLFES: I can't recall if we put the 50th and 95th percentiles into this document. Let me see if I can pull up the page here, and --

DR. MAURO: That's more a Site Profile --

MR. ROLFES: That's a Site Profile decision.

DR. MAURO: Right, okay.

MR. STIVER: I guess the other issue is situations where you have people who are working in the, predominantly with the process, the hot and cold raffinates that have been processed and extracted for the uranyl hydrides or nitrates, excuse me. And then say you have a situation where you have thorium that wasn't extracted, but yet the radium is also depleted, as well as the uranium. So you really can't use the radon breath sample for that particular category of workers.

So in other words, the question of how would you go about doing that?

MR. ROLFES: Well, what we've done, we looked back, if you recall, at the daily weighted exposure reports, in the area of the plant that had calcined thorium-230 raffinates that were depleted of the radium-226. That material was lifted via airline to Silo 3.

There is still uranium available in that material. It's a very low percentage. It's about five percent uranium that's still within that material, which you know, it doesn't preclude us from using a ratio for an individual on an appropriate basis, where we have an indication of thorium-230 exposure regarding an incident, for example.

We can also, you know, develop a ratio. We can use that ratio to apply a thorium-230 intake, based upon their uranium intake. But separate from that, if you take a look at the actual daily weighted exposure data in the area of the plant, where the thorium-230 raffinate would have been processed, that is one of the, you know, cleanest areas in the facility. The average air concentrations are very low and typical of ground concentrations around the site.

DR. MAURO: I've got two questions along those strategies, and I wasn't aware of those strategies. So the first strategy is that along with the thorium-230 that's been sort of separated, there is some small amount of U-238, that in theory, since most people have bioassay for uranium-238, you could say, you could develop a ratio.

I suspect you might find yourself in a situation where it's below the limit of detection. That is, you don't see any uranium in the urine?

MR. ROLFES: Correct.

DR. MAURO: You would then -- you would say, you would just, I guess, assign a default value for, let's say, one-half the MDL or whatever, and then use the ratio on top of that?

MR. ROLFES: That's correct. Even if it's a non-positive uranium urinalysis, we can still assign a missed intake. All it's going to do is drive up the internal dose.

DR. MAURO: I hear you. I just wanted -- and the last thing was the air sample. You're saying that you do have breathing zone samples for the workers that might have been exposed to this situation?

MR. ROLFES: If you recall, there's, I think, around 170 evaluated exposure reports from the beginning of operations in 1953 roughly, up until 1967. So that area was one of the areas that was sampled in the valuation of air concentrations that we used to prepare those --

DR. MAURO: So that puts you in a position where, okay, you have a breathing zone sample that's been counted. It could be uranium, thorium-232 or thorium-230, right? So you're saying that if there's reason to believe, it's possible that thorium-230, which might very well give you the worst, highest intake, the highest dose, you're saying you would go with that approach.

So somehow that breathing zone sample, I'm not sure of that. But you're saying somehow the breathing zone sample, where you've got, I guess, a gross alpha analysis is a hook, as to what the thorium-230 might have been for those workers? Is that what I'm hearing?

MR. ROLFES: That's correct. I mean there's nothing that would preclude us from using a BZ or a GA area sample, air sample excuse me, that was counted. For gross alpha, we can interpret that, you know, if it's in an area where thorium-230 or thorium-232 were present, you know, we could use whichever is the bounding radionuclide.

MR. STIVER: It's going to be

analogous whether the thorium-232 --

DR. MAURO: Yes, got it.

MS. BALDRIDGE: I have a question. This is Sandra. When I was reading the ATSDR, I don't know if I've pronounced it right.

MR. ROLFES: ATSDR.

MS. BALDRIDGE: Right, report on thorium, it says that -- in the area of 90 percent of it goes to the gastrointestinal system.

So when you're measuring what's coming through in urinalysis, how does that account for particulates or whatever, that do not pass through the gastrointestinal system, but in fact become lodged or deposited because of a condition in the bowel or so forth? How is that exposure accounted for in the dose reconstruction process?

MR. ROLFES: Well, regarding thorium dose reconstruction, for the earlier years, we're not using urinalysis data to interpret thorium exposures. We're actually using the air monitoring data to assign thorium intakes. So that would be something to consider, and it's considered in the biokinetic modeling that we use for dose reconstructions.

These are models that are developed by international committees, and they're contained within a computer code that we use to do intake calculations and internal dose calculations. It's called the integrated modules for bioassay analysis. That is something -- biokinetic modeling is built into this program, and that is something that is considered in the dose reconstruction process.

MS. BALDRIDGE: So you're relying on the reliability of the air monitoring for thorium?

MR. ROLFES: For the earlier years, that is correct. For the most recent era, post-1968, we're using the in vivo counts that were done.

CHAIRMAN CLAWSON: How many in vivo counts did we have?

MR. ROLFES: Tens of thousands. I couldn't put a number on it at this time, but it was conducted from, you know, 1968 forward. I don't know if anybody on the line. I don't know, maybe Bob Morris.

Oh actually, you know, I take that back. I think we may have summarized the number if in vivo counts in one of our previous responses. Let me pull it up here. Let's see, "Thorium In Vivo Coworker Study for the Fernald Site," from back in 2008.

Let me see here if I can pull up some numbers. Well, the way it's reported here, I couldn't really add it up. We've got it broken down, specific to thorium. We've got the samples broken down for thorium-232, and then a couple of thorium daughters or progeny, which are lead-212 and actinium-228.

This is discussed in our Fernald in vivo coworker study here. If you want to move on to that or discuss it?

MR. STIVER: Getting back to the

DWEs, I've seen that you would now calculate the exposure amount based on the 50th or an average value, but using the methods of Davis and Strom.

MR. ROLFES: I'll have to take a look. Bob, do you happen to know the answer for the specific area of Fernald, where we have the DWEs, where thorium-232 or thorium-230, excuse me, would have been one of the controlling radionuclides? Have we documented this in our most recent Report 52 here?

MR. MORRIS: I don't know the answer off the top of my head. Billy, have you read that recently?

MR. RICH: This is Bryce. I think we indicated that thorium-230 exposure would be added to, and since we assume an equilibrium with the uranium. So in the front end of it, operation end, including the sampling operation and Plant 8, any uranium results would have an equilibrium concentration of thorium-230 added.

Since the DWE samples at the tail end of the process were essentially zero, then we said that that would not -- in a raffinate condition, exposures would be so low that that would not affect the very conservative addition of thorium-230 to the uranium.

MR. ROLFES: Thank you, Bryce.

CHAIRMAN CLAWSON: So is this different than what you just mentioned to us? I thought --

MR. STIVER: Yes. I was wondering whether you had used the same approach that you did for --

MR. ROLFES: According to what Bryce just said, no. So we didn't look at the data from the area where thorium-230 was the controlling radionuclide, and I didn't know if we were going to add a separate intake there. But based upon the review of the data, it was essentially indicated that there was no exposure potential.

MR. STIVER: I guess that's the

other issue here. In the raffinate areas, Finding No. 8 concerning the airborne dust loading of thorium-230, and the raffinate areas were substantially higher than assumed by NIOSH, and thus the method of dose calculation of thorium-230 should be available for dose reconstructors in those areas.

A corollary to that was that, questioning the veracity of the DWE data, documents here. It was Wing and Halcomb in 1958, and they show that from the period 1955 to 1958, the air sampling of the hot raffinate, when combined with the raffinate areas, was only GA sampling. There were no breathing zone samples at all.

So the problem there, DWEs being low because they weren't sampling the --

MR. ROLFES: Well, keep in mind, though, for that time period, we're using the radon breath data to estimate intakes for the hot raffinate area.

MR. STIVER: But in the combined

raffinate area, you have the depletion of radium.

MR. ROLFES: Correct. However, it didn't specify the cold raffinate area there in your report.

MR. STIVER: Oh, the same for the situation where we have the cold raffinate. I mean we're relying on DWEs, but they're based only on general air samples. Could be a little problem there too.

MR. ROLFES: That's, you know, another method of interpreting the data. So I mean it's another correction factor, as to whether --

MR. STIVER: Seems like more of a Site Profile issue in any case. Let's see. Okay. Here's one I can see. Finding 11 is kind of related to the Silo 3 area again, and its concern is regarding the thorium-230 exposure to people who are involved in Silo 3, basically after the raffinate extraction separations.

You're having about 138,000 cubic feet of raffinate, and you had concentrates from a variety of uranium mills in the U.S. and abroad, of course. It was stored as fully oxidized fine powder, in contrast to the K-65 drills in Silo 1 and 2, that were about 30 percent moisture.

So we're concerned about the potential scenario of enhanced inhalation capability, or enhanced airborne concentration of this particulate material, as opposed to the other raffinates that were in Silos 1 and 2, if there's any accounting for that, any type of correction.

MR. ROLFES: I think I addressed that earlier on, about Silo 3 material that was air lifted via an air line, enclosed air line after it was calcined to Silo 3. You know, in the event of a case-specific release, there have, you know, if there is an indication that an individual was exposed to that material, there's nothing that would preclude us from applying an intake of thorium-230, based upon the quantity of uranium contained within that silo.

On a mass basis, there's about five percent uranium still within the Silo 3. So we can develop a ratio based upon case-specific information, if needed, to make sure that our dose estimate is claimant-favorable.

CHAIRMAN CLAWSON: So you say from the individual, to show the science of this, what would be, I guess I'm trying to figure out who you're going to pick out of this?

MR. ROLFES: An incident report.

MR. STIVER: There just happened to be some sort of a breach in the containment system?

MR. ROLFES: Correct.

MS. BALDRIDGE: I mean we already know that all the incidents weren't always reported, because a lot of times the worker didn't realize it was an incident, and the way they monitored with the urinalysis and the mind set to keep the cost down, they sample monitored people within an incident group.

The case of -- there was about one of the documents talked about the fire. Well, and they also talk in the National Lead documents how they would go in and maybe pick five people out of 20 to monitor.

Well, the other 15 people don't have those incidents listed in their worker records. So how do you assign based on when it appears that someone may have had an exposure? We already know their recordkeeping was atrocious.

MR. ROLFES: But those allegations haven't been supported in my review as a health physicist. So I disagree with you, unfortunately. I'd be happy to make any effort to explain the quantity and availability of records from the Fernald site. I've spent, you know, the past eight years responsible for the Fernald site and several others.

Based upon my review, I mean we've got, you know, this is one of the facilities where we have the most data and, you know, as far as if there's uncertainty as to whether an individual is involved in such an incident, we would assume that they were in that incident, and we would give them dose credit.

If there's uncertainty involved in the dose reconstruction process, the claimant-favorable assumption is made to use that uncertainty to the benefit of the claimant.

CHAIRMAN CLAWSON: You know, that brings up a question. How many years did you work there?

MR. BEATTY: Fifteen.

CHAIRMAN CLAWSON: What do you feel on what was just said?

MR. BEATTY: This is Ray Beatty. I do have a comment with regards to what Sandra brought to your attention about records. Specifically Mark, and I'll bring another side of that issue to your attention. It goes back to something known as *Day v. NLO*, a federal lawsuit that was filed on behalf of all workers at Fernald, from 1953 through 1985, NLO days.

Out of that lawsuit was born a medical monitoring program. Some compensation was given to the workers. In her petition, she uses actual exhibits from the lawsuit as support documentation. I would think that those would speak for themselves as well.

Also, one other thing, and this goes out of the petition cohort era, on past '85 or even '89. I've got a NIOSH report given the Board, the Federal Advisory Board. It was conducted by the NIOSH organization. It basically had four questions they wanted answered.

By looking at records at other sites, Fernald being one, Mound, Rocky Flats, Savannah River, Hanford, Oak Ridge, Idaho National Engineering and Environmental Lab. Four questions, and this is remediation workers now, talking about recordkeeping and accurate records.

Can remediation workers be identified? Are adequate exposure, work history, medical data available for remediation workers? Can individual workers be linked to their exposure and medical data?

With current knowledge and understanding as described in this report, can epidemiological exposure assessment or hazard surveillance studies of remediation workers and the technologies they employ be conducted now or in the foreseeable future?

You read the report, the short answer to all these in report findings is no. Some remediation workers that have worked at DOE sites cannot be identified. Accurate and complete exposure, work history, medical record data are not available for this population.

The individual workers cannot consistently be linked to their exposure and medical data. This is NIOSH report.

MR. ROLFES: Correct.

MR. BEATTY: One other thing I want to bring up, and I think it's been mentioned a couple of times, are the radon studies. A lot of people want to talk about K-65 radon releases, which they were, it was bad.

But the Q11 silos too needs to be mentioned here. I plan on attending the May meeting, and bringing some documentation with me, including a copy of this report for all Board Members again.

This is important, and the *Day v. NLO*, you can look that up yourself. That's pretty easy to find. It's on the website, and those records should speak for themselves. Thank you for allowing me to comment.

MR. ROLFES: Thank you Ray, and regarding the *Day v. NLO* trial, it wasn't a trial, excuse me. I'm very aware of that, and I have seen records that have been presented to us in the petition, as well as much of the information and sources of information.

I've seen, we've gone through quite a bit of effort to address those specific issues that were identified by the plaintiffs. We've responded over the past several years in the Work Group meetings. The transcripts of our responses are available to the specific issues on our website, excuse me.

We've gone and discussed many of the plaintiff's exhibits that Sandra has presented to us. I wanted to point out also that this *Day v. NLO* never went to court. It was settled out of court, so there was really no cross-examination of the data. It was sort of a one-sided story at that point, and a settlement was made.

But I thank you for your comments, and one other thing. I am familiar with the report somewhat that you have presented to us. I'm aware that it was written by NIOSH. It has been previously identified to us. I don't recall any of the -- they were basically looking into an epidemiologic study, I think, at the time.

I don't know. Dave, do you -- are you familiar with the report? Okay. If there's something specific in there that you would like for us to respond to, we'd be happy to do that. I am aware of what the report says, and I need to go back and look at the report, in order to respond to you and your concerns.

I did want to point out that the remediation efforts took place after basically the site was shut down, and right now for the SEC discussions that we're having, the remediation effort is outside of the SEC proposed time period. It's separate right now.

But it is an important thing to look at for dose reconstructions, for remediation time periods post-1989. So but thank you.

MS. BALDRIDGE: I'd like to make one comment. What it does show is the importance that they put on the records. Whether they kept them, whether they disposed of them, whether they didn't keep them, whatever. They weren't deemed important enough to set aside, to make sure they were accurate, to make sure they were available, to make sure they were usable. That's the point.

MR. ROLFES: Thank you, Sandra, and regarding the records review that I've conducted over the past several years for the Fernald site, the information that is required for dose reconstruction has been available to NIOSH.

Our previous efforts with the Work Group, including even going back to the original hard copy data for bioassay information, and comparing that to the electronic database from which data is extracted for us to use for dose reconstruction.

I think both SC&A and the Work Group Members would agree with us, that we found that the data are valid and essentially complete for the development of a coworker intake model for uranium.

CHAIRMAN CLAWSON: You know, we have done work on the HIS-20 database. If what Mark says is correct, that what we've checked is good. But kind of go back to the bottom line too, is it's only as good as what was put in there.

One of the questions is, and it always comes to every site, is recordkeeping. Then the findings of the Tiger Team report. Well, they were mainly hitting on one Tiger Team report was when they came out to check Fernald, basically their recordkeeping was in question too. This is when the new contractor came in.

People can surmise what they want from it, but when the new contractor came in, the whole RadCon program took a totally different change and went from there. Granted, it took a few years to get there, but it did take a drastic change.

MEMBER ZIEMER: What's the bottom line on Issue 4?

MR. STIVER: Well, our main concern was whether they were able to bound doses for the category of workers who were -- may have been exposed to thorium-230, with depleted levels of U-238 and radium-226, where you couldn't use the radon breath data and you couldn't use urine bioassay.

And what Mark said about using the DWE data to bracket dose to start with. Correct Mark?

MR. ROLFES: Correct.

DR. MAURO: Or the bioassay data.

MR. STIVER: Correct.

DR. MAURO: So you have --

MR. STIVER: Well, the bioassay, it probably wouldn't work. I mean you could be. It would be --

DR. MAURO: Well, we'd make --

MR. STIVER: This would be a sub-MDL

type thing --

DR. MAURO: Well, as long as you could say I could put an upper bound on the ratio of the U-238 to thorium-230 in the source, you probably, you know, if you're comfortable with being able to do that. In principle, it could be done.

MR. ROLFES: Correct.

CHAIRMAN CLAWSON: But my understanding was if it was below the urinalysis data, then we would have to have an incident data report to be able to --

MR. STIVER: If there was an incident where there could have been a, you know, an accidental exposure, some kind of an event that took place. But in general, if there was just a sub-MDL kind of a situation, well then they would just do a missed dose calculation.

MR. ROLFES: I guess I should clarify that just a little bit because we don't necessarily have to fully rely on an incident report to identify a person who's potentially exposed. We've addressed this issue. We've identified specific areas at the Fernald plant where thorium-230 would be the controlling radionuclide.

So if an individual was working in that plant during that time period, we would apply the thorium-230 intake.

CHAIRMAN CLAWSON: So then that takes placing that person in that area?

MR. ROLFES: That's -- not necessarily.

CHAIRMAN CLAWSON: Mark, I don't mean to interrupt you, but I guess I'm looking at this from a common standpoint of how you're going to give this to people, and it comes back to the old thing that we've always been battling on this. How are you going to place a person in that area?

And as we've heard from the workers and everything else like that, and especially construction workers or whatever, they were everywhere. I would clearly have a hard time of how you're going to implement this.

MR. ROLFES: When there's uncertainty as to the work location, NIOSH chooses the location across the entire site which would result in the most claimant-favorable dose outcome for them. So if there's any uncertainty as to whether that person worked in that area, we would choose that area for a dose reconstruction, unless there are records that show that they were not in that area.

CHAIRMAN CLAWSON: And the dose reconstruction people would understand this? Being on the Dose Reconstruction Group, you know, that's just kind of at some of the points we're finding too is what pushes the dose reconstructor to do these things? I guess that's kind of where I have my heartache, of how, where we're going with this one.

You know, I just have a hard time of

how we're going to put it to the people, especially with Fernald because the people went everywhere.

MR. ROLFES: I just said we would choose the area of the site that resulted in the highest dose. I mean that's -- there's -- you can't get any better than that.

MR. STIVER: It's kind of a one-size-fits-all approach where you just take a bounding situation.

CHAIRMAN CLAWSON: So everybody's going to get it?

MR. ROLFES: If there's nothing that says they were not exposed to that, then they would be assigned a thorium-230 intake. Let me make sure we clarify something here because we're talking about a very, you know, very limited, small fraction of most workers' exposures. The driving exposures at the Fernald plant were typically uranium, followed by thorium, and then some of the other lesser radionuclides, some of the other, you know, raffinate materials.

When we complete a dose reconstruction, you know, for a lung cancer, for example, it's usually, you know, 90-something percent of the lung cancers at the Fernald site have been compensated. So it doesn't matter about thorium exposures. We don't need to assign thorium-230 exposures if the uranium alone makes it go over 50 percent.

So that's an under-estimate. We don't consider all sources of exposure in the dose reconstruction if a portion of the individual's exposures create a Probability of Causation greater than 50 percent. If the Probability of Causation is less than 50 percent, we give every benefit of the doubt.

We assign a bounding over-estimating dose to make sure that we've considered any and all sources of radiation exposure of significance that could potentially make an outcome difference in the case.

So when we say we do an over-estimate, we assign worst case scenarios and assumptions in a dose reconstruction process to make sure that the benefit of the doubt is given to the claimant if we have to turn that claim down.

MR. DOLL: Brad?

CHAIRMAN CLAWSON: Yes.

MR. DOLL: The gentleman that was here last month instead of me, he filed and he went through NIOSH, and he met with them and they had the conversations about what he thought his exposures and stuff were. I think he submitted a letter of response back from NIOSH.

And there's no records on him. He was down there from 1982. I got there in '83. We worked in all those buildings, 2, 3, the full nine yards. Wherever other people didn't want to go, we kind of found our way in there, and sometimes two and three times a day to different places. That was just our job.

In that letter, he was told that he

got more dose from 1993 on than he did when he was -- as a superintendent, than he did when he was working in the field in all these buildings as a pipefitter. Now if you're doing this, I just -- maybe I'm just not understanding the process, where you're assigning dose to somebody at the worst case scenario.

I just have a problem with how can his dose be less, as a superintendent for Fluor, after everything's been put in position, sitting in a trailer or walking around, versus him working inside these buildings getting exposures. I wish you could explain that to me.

MR. ROLFES: Well, I would really like to. Unfortunately, I can't discuss an individual claim's information openly.

MR. DOLL: Well, I understand. But just make it John Doe.

MR. ROLFES: Well, there are some specific things. One would have to take a look at the data contained for each claim file. I have heard many concerns about in general, availability of data for individuals, and specific to subcontractors.

That is something that we heard first. It wasn't something at the original SEC petition that we received regarding subcontractors specifically.

But that is something that was presented to us in the Working Group process, I believe for the first time back in January of last year, in 2010. So that is something that we have been looking into. We've been looking into hard copy records specific to subcontractors to see if there might be data missing.

We haven't produced a finalized report yet, but we are actively looking into that. But I've heard many concerns about the not having data available or only having, you know, bits and pieces of data for specific claimants. I haven't looked at the numbers specific to Fernald in probably a year, but I think there's around 1,200 or so claimants that have filed with the Department of Labor that require a dose reconstruction for the Fernald site.

Out of those 1,200 people, I think we identified just under 100 people that didn't have bioassay data in their files. So what we did at that point, we developed, using the HIS-20 data, we developed a coworker intake model for uranium, to assign uranium exposures to people who did not have monitoring data, but had an exposure potential.

So the concern about subcontractors specific to that approach was identified. I believe, Ray, you might have identified that at the previous or maybe two Working Group meetings ago. That's something we're looking at.

We had pointed out that the HIS-20 database did not appear to contain subcontractor bioassay results. That's true for the earlier years, but they are in there for post-December 1985. So that is one thing that we've been able to find in our closer look at this.

Once again, I did want to say that it is something that we're looking into.

MR. DOLL: You just made a comment, post-1985. But I mean what you're saying is National Lead walked in the door and had all this set up? Or how long did it take them to get to that point?

MR. ROLFES: This is only specific to subcontractors. We do have hard copy data for subcontractors, which is not in the HIS-20 database, which I should specify. So that's one of the things that we're going back to look at to see if we've got all of the hard copy subcontractor bioassay data, hard copy sheets. The HIS-20 database, that was something that was developed in the more recent era.

There was a previous database where all of the uranium bioassay data was entered electronically. Then prior to that, it was only in hard copy. But we found that the hard copy data, in our review and evaluation of that data that went into HIS-20, we found that it was actually relatively complete, and both SC&A, the Work Group Members and NIOSH have agreed that there's nothing that would invalidate its use for dose reconstruction.

MR. DOLL: Was that hard data of construction workers, or was that all workers?

MR. ROLFES: We have both hard copy data for subcontractors, construction workers and all full-time employees at the site, and partial. So it's not, you know, the first thing when they would request a urine sample from a worker, they would actually give you a urine sample request card, where you'd have to, you know, go to report and provide a urine sample at a given date and time at the site.

So that request card would have your

name, who you worked for, and where you were to report, and then also would have the analysis results. That was the way it was in the earlier days. But then subsequently, that information was all entered into electronic databases, and we have reviewed those databases and compared the database information to the hard copy data.

MEMBER SCHOFIELD: How often did people give urinalysis?

MR. KATZ: Before, Phil, can I ask, can you identify yourself? I'm sorry, but you --

MR. DOLL: Lou Doll. I've been here --

MR. KATZ: No, I know you've been here, but you weren't here when we started. So for the record.

MR. DOLL: Okay, I'm sorry.

MR. KATZ: Thank you.

MR. ROLFES: Phil. Phil had a question. As far as the number of urinalyses, some people may have given them once a year. Some people might have given them several times a day. It all depends upon, you know, if there's an incident, for example, and the individual has a high, you know, above 50 micrograms per liter, for example, they would resample that individual to make sure that it was a valid result and check to see if it was decreasing at all. So it's all, you know, based upon the previous result, the potential for exposure and such. So it varied, depending upon --

MR. STIVER: All right. I don't really have any other questions about Issue No. 4. Joyce, are you on the phone?

DR. MAURO: Apparently, she wasn't able to --

DR. LIPSZTEIN: Yes, I'm here.

DR. MAURO: Oh, she is.

MR. STIVER: She was able to get on. I got an email from her. I thought I told you.

Was there anything that -- I know you were the principle author of the Rev. 7 review about the radon breath data. Was there anything else that you'd like to add to the discussion?

DR. LIPSZTEIN: No, no.

MR. STIVER: Okay, okay. In that case, I have nothing else really to say about Issue No. 4.

MR. KATZ: So can I just ask is there sort of a bottom line for the Work Group. I mean if you're going to be reporting out to the Board on issues, is this an issue you're going to address in your report out to the Board?

CHAIRMAN CLAWSON: Well, I think we've -- yes, we're going to be addressing where we got to on it. We're going to basically go over how we've gotten to this point and what we've done. But that's basically the bottom line.

MEMBER ZIEMER: Well, but let me

ask. I think I heard you saying that you agree that you can bound this data set?

MR. STIVER: Yes, the one category that we were concerned about would be those who didn't have adequate radon concentrations or radium concentrations. So in that case, you know, they would default to the bioassay if they had a missed dose for chronic exposure or to the DWE data.

DR. MAURO: Yes, they had a tractable situation. There's the issue of if it's incident-driven, that doesn't raise a question about yes, you can. Once you've identified a person that you think might have been exposed to thorium-230, what we just heard is that you have two strategies that in theory would allow you to get a hook on the intake.

The issue always is well, who are those people, and when are you going to assign it, and that's certainly -- and the response we heard was that that most likely would have occurred under an unusual circumstance, which would be part of a record of transients. Am I correct? Am I characterizing this fairly?

That is -- or you would assign it broadly for people who even might have been exposed?

MR. ROLFES: We have some details on the specific areas of the Fernald site where thorium-230 intakes could have occurred, and if there's any doubt as to whether that individual worked in that plant, then we would assume in a worst case dose reconstruction that they were in that plant.

DR. MAURO: And SC&A's position, you can't do more than that.

MR. STIVER: Yes, I have no problems with that since they were using a bounding approach. So I think we can close that one out.

CHAIRMAN CLAWSON: How about -- looking at the time right now, let's --

MEMBER ZIEMER: 12:15.

CHAIRMAN CLAWSON: 12:15.

MR. STIVER: We have, you know, the thorium issue and in vivo thorium coming up. That's probably going to be a relatively big one, so it might be better to --

CHAIRMAN CLAWSON: Take a break at this time?

MR. STIVER: Take a break.

MR. KATZ: A lunch break?

MR. STIVER: Yes.

DR. MAURO: And Joyce will be available for that?

DR. LIPSZTEIN: Yes. I'm here.

DR. MAURO: Yes, good. We'll be talking about the chest count. I presume that's what we're referring to, the post-'69 chest count data. I know that you were very close to that.

MR. STIVER: Actually, very much so, and I heard Bob Barton -- or if you're looking particularly at the mass specs on that issue. So about an hour from now, I guess we will be

--

MR. KATZ: About 1:20, we'll reconvene. Thank you, everyone on the line, for hanging in with us, and we'll be back around 1:20.

(Whereupon, at 12:17 p.m., the above-entitled matter went off the record and resumed at 1:23 p.m..)

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

1:23 p.m.

MR. KATZ: Okay. Good afternoon. This is the Advisory Board on Radiation and Worker Health, Fernald Work Group, and we are reconvening after a lunch break. Let me just check on the line for our Board Member. Do we have Bob Presley again?

MEMBER PRESLEY: Yes.

MR. KATZ: Great. Welcome back, Bob. We're glad you could make it. I know you had an interruption at some point. Do we have any other Board Members?

(No response.)

MR. KATZ: Okay, then, Brad, carry forward.

CHAIRMAN CLAWSON: Okay. Now we just got done with the thorium, is that correct, and that was Issue 4.

MR. STIVER: Actually, we finished Issue 4, which was the radon breath data, and now we're going to start with 6B, which is the thorium in vivo.

MR. ROLFES: John, I'm sorry. Before we get into it, I wanted to add something. You can continue and I can come back in a second, but I just wanted to --

CHAIRMAN CLAWSON: Okay, go ahead.

MR. ROLFES: While we were away on lunch, I looked for the report, the NIOSH report that you had referenced, Ray, and I was able to pull that up. I knew I was familiar with it to some extent, and I knew I had recalled the report. But I couldn't exactly, you know, put all the pieces together in my head.

But I was able to find a copy of the report, and in the NIOSH Summary of Findings, this is available on the cdc.gov website under NIOSH. In the NIOSH Summary of Findings, in the Fernald edition, there were four findings reported here.

The first finding was "Some remediation workers who have worked at DOE sites cannot be identified," and it basically says that "complete rosters of current and former remediation workers do not currently exist. Reconstruction of rosters from multiple data sources at the site is labor-intense and it may exclude some groups of workers."

The second point was the one I think that is most important to the discussion here, and it says "Accurate and complete exposure, work history, and medical records data are not available for this population." It goes on to say "Although radiation exposure records appear to be complete, decentralized responsibility for chemical exposure assessment and other records has led to gaps in exposure, work history, and medical data."

So the shortcomings in the records appear to be speaking towards the chemical exposure aspect, rather than the radiation exposure records. I just wanted to point that out because that is something that we have followed up on quite a bit, and it is an important issue, and I'm glad you did bring that up.

But I wanted to insert that clarification, so thank you. Oh, one other thing. It was for -- I apologize -- it was for, I think, to determine whether or not an epidemiologic study could be conducted for the remediation work force.

MR. STIVER: Okay. We've pretty much laid Issue 4 to rest, and we go on to the issue of thorium-232 intakes. We have already been through Issue 6A, which was the use of DWE data.

We feel that's fairly well resolved, and we're going to push that one back to after 6B, which by virtue of its position in line, is in the last three meetings, has never been discussed to any level of detail, and we want to go ahead and make sure that we have a chance to address that one in the level of detail that it deserves.

Basically in 1969, I believe, '69 through '89, the site acquired the mobile and in vitro -- or in vivo laboratory, and were able to do chest count data, which they then used to assess intakes of uranium, thorium, and -- but mainly uranium and thorium. But in this particular case, we're interested in thorium-232 exposures that may have taken place during this time period.

There's really two periods of interest here. It was 1969 to '79, and 1979 to 1989. 1979, a different technique was introduced for assaying the thorium-232, and from '69 to '79, basically they used, reported the data in basically in units of milligrams thorium.

We believe that was based on the actinium-228 activity. There were some problems with that, which Joyce Lipsztein will discuss in a minute. There were issues related to that and also related to the choice of the minimum detectable amount and its relationship to the distribution of chest count data.

Post-1979, they used a different technique, which basically measured the lead-212 from thorium-228, and it was felt that was probably a more robust measure. It's less subject to problems associated with disequilibrium between thorium-232 and its daughter products.

However, there still remain significant issues regarding the minimum detectable amount, MDA, and also its relationship to the distribution. The area of overlap between the two measures indicate there may be some discontinuity there. So, Joyce, are you on the phone now?

DR. LIPSZTEIN: Yes, I am on the phone.

MR. STIVER: Okay. Would you like to go ahead and take it from here?

DR. LIPSZTEIN: Yes, yes. I'll discuss the technical parts of the measurements, and then the statistical part I think Bob Barton will come along, right?

MR. BARTON: That sounds good, Joyce.

DR. LIPSZTEIN: I'm sorry? Okay. So I'm discussing the years of chest count regarding thorium-232, and basically they were chest counts made available from '68 until 1988. But from 1968 to 1978, the lung burden was reported as thorium mass, milligrams of thorium-232, in nearly all cases.

After 1978, during 1979 to 1988, thorium lung burden was reported of actinium-228 and lead-212. So we have different aspects because one of them we don't know, we don't really know how this thorium mass was really measured.

MR. ROLFES: Was that it, Joyce? Excuse me? Joyce?

DR. LIPSZTEIN: Yes, yes. I'm here. I'm just pulling out my notes.

MR. ROLFES: Okay.

DR. LIPSZTEIN: So let's first focus on the period of '68 to '78, when the thorium lung burdens were recorded as milligrams of thorium. We don't know how this milligrams of thorium were acquired, how people measured, because thorium itself, I think everybody knows, but thorium-232 itself cannot be measured by in vivo counts.

So you have to rely on the measurements of the daughter nuclides, and they could have been measured through actinium-228, which when thorium is in equilibrium with the daughters, is the at best look like, to measure and to associate with a dose of thorium-232 because you don't have to pass through the radium emission, which it will disperse like the lead-212.

The problem is that we don't know when the thorium was separated from the daughters. So the measurement through actinium-228 might under-estimate a lot the thorium lung burden. This issue was not solved. There is no proof, nothing, of how the thorium in milligrams were reported.

We have just some figures of in vivo data for some Fernald workers, and of the measures in milligrams. We can see that there is a curve that is an increasing activity of thorium.

This could be consistent either with the measurements of actinium-228 because it will be increased in activity, the actinium-228 would increase in activity in the lungs, after the intake of thorium that has been chemically separated from the daughter nuclides.

Or it could also be the result of someone that was in a chronic intake because the person would be chronically exposed. So he would have an increasing level of exposure to thorium over time. So there is a lot of uncertainty on this data, on milligrams of thorium, because we don't know how to interpret it.

If there is a consistent, there is a thorium intake and then the exposure of thorium with the increase over time, or if we are seeing this increasing in milligrams because it would be we're measuring actinium-228, and there were an increased activity of actinium-228.

There was a brief response from NIOSH from this comment, saying that the workers were measured through lead-212. But we don't know why and how this conclusion was reached. Also because on the data, after '78, both results are given, actinium and lead-212. There are some documents after '78 that say that they use both data to calculate the thorium in the lungs.

So I think this data has a lot of uncertainty to really be used to determine data. The other thing is that NIOSH cites that there is a consistency between the data after 1979 and the data before '79. When the data were -- when you had the measurements in nanocuries of lead-212. Can you follow me?

MR. STIVER: Yes.

DR. LIPSZTEIN: Okay. We have measured -- we have the data from 22 individuals that were measured, that had both measurements, a measurement in mass and a measurement in lead-212, 1979, because we had the both measurements.

If you calculate the activity of thorium by -- you have the resulting milligram. You calculate the activity in thorium using the conversion factor that NIOSH uses of, to convert it to a nanocurie of thorium-232.

Then you have the nanocuries of lead-212, and you transform it in the equivalent activity of thorium from this measurement of lead-212. Then you had ratios of activity that varied widely. For example, from minus 76.82 to 12.8.

So I have, you know, the same person you calculate the thorium activity nanocurie by the milligram result and by the activity that was measured from lead-212.

The ratio of the two types of activity that should be the same. They vary. Like for example, I have .10, then 1.71, then 4.27, then 12.8, and like -- and goes on and on. So we cannot really rely on those activities in milligrams.

The other thing is that we have seen that for some of the workers -- so another issue. Some of the workers that have the in vivo measurements recorded in milligrams of thorium, they have implausible large changes from inhaled thorium over brief periods of time.

What happens is with the biokinetic of thorium will predict that it will stay for a long time in the lung. So you cannot have from one, the measurements taken one month after, a very big change of thorium in lungs. So for example, I have -- I'll just cite one example.

I have one that was measured 10.2 milligrams of thorium in March, for example, and then 40 days later it was .2. You cannot have that variation. It doesn't match the biokinetics of thorium. And as a result of the last Working Group meeting, I was asked to furnish some data where I found this large difference, that it's not -- it does not comply with the biokinetic of thorium.

So we sent a memo. It's just a memo. It's nothing to be added to our review, but just showing number of cases where this happened, where the biokinetic of thorium doesn't match with the measurement results. It was a large variation in measurement results in a small amount of time.

Then the other issue, still on the thorium in milligrams, is that we have -- we were given by NIOSH an MDA of six milligrams for thorium-232. There is no explanation on how this minimum detection activity was derived, nor which nuclide was used to derive the minimum detection activity, or how was the counting time, if there was a counting time -- a standard counting time.

The problem is that when we looked at the table of in vivo counting, we see that about 84 percent of all the -- in all years, except for '68. 84 percent of all measurements are below six milligrams of the MDA. I know that the MDA is not used for a coworker model.

But the problem is not that; it's that if you have a six milligram minimum detection activity, you couldn't have reported as positive results, 84 percent of the data that we have.

So there is a lot of uncertainty on this data on milligrams, reported on milligrams of thorium-232. I don't think they were solved in a convincing way so that we can use them to calculate Dose for the workers.

Now if we could see it, we have the data for the period from '79 to '89. We also have a lot of uncertainties. First, we have data also. 84 percent of the results are below the minimum detection level for lead-212. Now I'm talking about the period where we have actinium-228 and lead-210 results, and NIOSH used the lead-212 results to calculate the thorium lung burden, which is correct of using lead-212 instead of actinium-228.

But even so, we have a lot of uncertainties on these measurements of lead-212. The MDA, the minimum detection activity for lead-212, is about .9 nanocuries. That's what was reported. But when we see the results that are below the minimum detection activity, then that's 84 percent of the results are below the minimum detection activity.

So how would they report so many positive results if the minimum detection activity was really .5 nanocuries. Here, I have another thing, because when one of the questions that we asked NIOSH and we had a fast response from them, with how long did they count the people, the workers.

The answer was that they counted about 20 minutes in the model whole body counter. I work a lot with thorium, and I don't think you can achieve a minimum detection limit of .5 nanocuries with 20 minutes counting in a model whole body counter. I think it's too low.

Generally to have this detection limit, we would have to count the persons in a shielded room for at least 60 minutes. So, you know, there are some uncertainties maybe, but a lot of uncertainties of how this minimum detection activity was calculated and if it was calculated using the same time as the worker was monitored.

So I think there are a lot of uncertainties. Thorium is a very difficult nuclide to measure, and very difficult nuclide to measure in lung also. The other thing is that it's assumed that lead-210 -- 212, I'm sorry, lead-212 is in equilibrium with -- there was an equilibrium assumed for lead-212 and thorium-232, which was .711, which was the mid-point of a theoretical range.

This is correct for the thorium in air. The problem is that the daughter nuclides of thorium-232 don't behave in the same way in the lungs. They don't have the same kinetics of thorium-232, and there might be a big uncertainty on this.

If you assumed the same equilibrium that you have in errors -- on the source -- what's happening in the lungs, then you might infer of errors that might even go to two times or more for when you convert the activity to thorium-232.

MR. STIVER: Joyce, could you mention a little bit about the magnitude of those uncertainties?

DR. LIPSZTEIN: Those uncertainties would be, from calculating the dose from base, that lead-212 and all the daughters, not only lead-212, but you have radon here and you have radium, and you have actinium, everything on the lung, if you assume that they have the same behavior, the daughters have the same behavior as thorium-232, you might incur errors that goes from two to ten times, depending on the solubility of thorium.

If it is, for example, thorium nitrate, you can incur to ten times errors. If it is dioxide, then it's about two to three times.

MR. ROLFES: That's for internal dose calculations, the same order of magnitude's actually pretty good. So if we're, you know, talking about a factor of two or a factor of ten, we're in the right ballpark, I mean, in my opinion.

DR. LIPSZTEIN: I don't know. If you calculate a dose that is ten times higher?

MR. ROLFES: If you calculate a dose that's ten times higher --

DR. LIPSZTEIN: The uncertainty --

I'm sorry. Ten times lower because it would have, you know, the daughters would have lived long. So I think this is a big problem with thorium measurements every place, and there is no real solution to this, unless to say that the uncertainty is high and put a very, very high uncertainty on this.

Thorium is really very difficult to measure, and the results that we have, that we can work with, are very difficult because we don't know exactly how much was, and we don't have urine data. At the same time, we don't have fecal data. At the same time, you know, to reduce those uncertainties. You just have the lung activity.

To rely on the lung activity of lead-212 to thorium-232, it's a big, big uncertainty.

MR. ROLFES: I wanted to make a clarification, Joyce. We actually do have some urinalysis data that was basically collected and analyzed for thorium using neutron activation analysis. That was done in around 1965. There's also some thoron lung breath studies that were done in the earlier time period as well, as well as an off site whole body counting or lung counting of Fernald employees.

Basically, you've identified many uncertainties, and I believe we've responded to them previously. I've got a response here from January 19th of 2011, and also another response, where we have addressed your issues that you have presented to us.

So basically, I think you're just summarizing what we've already discussed at the last Work Group meeting.

DR. LIPSZTEIN: Yes, but we didn't really discuss the answers to that, and some of them are discussed here. I don't believe it is, you know, answered in a satisfying way.

MR. ROLFES: Okay. At the last Work Group meeting, you had indicated that you had an individual who had a thorium lung burden of

40 milligrams that had dropped down to .5 milligrams within 30 or 40 days.

DR. LIPSZTEIN: Right.

MR. ROLFES: And we did subsequently receive your memo regarding the, you know, large variations in the measurements over time. I didn't see any measurements that were as high as 40 milligrams. The highest I saw on your report was 10-1/2 milligrams, that had dropped down to a half a milligram.

DR. LIPSZTEIN: I didn't pick all the data. I just put some so that you can see.

MR. ROLFES: Well, that was one of the focuses of last Working Group meeting, that you were going to provide that individual --

DR. LIPSZTEIN: Yes, yes, and I did. I think I did a lot of, you know, I provided you with a lot of individuals, so that you can see how this dropped.

MR. STIVER: Mark, this is a

representative data set for 30 different workers. I think the point here wasn't to really worry about the magnitude of any particular number, but just to show the difference in time and how that doesn't comport with biokinetics.

MR. ROLFES: Okay.

MR. STIVER: Just to illustrate the uncertainties.

MR. ROLFES: I just wanted to make sure that we're not talking about somebody -- I mean what she said is a 40 milligram lung burden that dropped to .5. So we're talking --

DR. LIPSZTEIN: Yes, but you had already that data. That's why I didn't put it again.

MR. ROLFES: I'm sorry?

DR. LIPSZTEIN: And I, you know, I didn't analyze the whole set of data because it would take a long time, and I don't think that was the purpose of it. So I took some data and I thought, well, I have, I read the sensitive number. You can see how it varies.

MR. ROLFES: I did look at your summarization, and the highest result that I recall seeing in the summary in the memo was a 10-1/2 milligram thorium lung burden, which dropped down to about .5 milligrams, which was below the limit of detection at the time, the six milligram limit of detection.

Something that would drop from a 10-1/2 milligram lung burden down to less than the limit of detection of six micrograms sounds -- doesn't sound abnormal to me. It sounds like a normal excretion pattern for something that's moderately soluble. You know, there were certainly some short-term thorium processing campaigns at the Fernald site, that may have --

DR. LIPSZTEIN: No. Thorium never can be soluble. Thorium either is Type M or Type S.

MR. ROLFES: That's correct, and I

said Type M, moderately soluble.

DR. LIPSZTEIN: Okay. I don't think, you know, you can't go -- if you do the biokinetics of it, you'll see that those results are not possible.

DR. GLOVER: So this is Sam Glover. One brief explanation, one is this external examination. Obviously, somebody who's externally contaminated, if they come back in, because obviously we -- I don't know what all the full history of this person's exposure. Usually, they'd reassess, perhaps, at that level. So external contamination can account for that.

Also the large particles being cleared from the upper respiratory tract can also account for a rapid clearance.

DR. LIPSZTEIN: It depends on how -- okay.

DR. GLOVER: Yes. I'm just saying it's not a complete impossibility.

DR. LIPSZTEIN: No, no, no. Okay, I

agree that external contamination could account for it. But I was told at the last Working Group meeting that the external contamination was not possible because they wanted to make sure that that's true. A whole body count is they want to make sure that the people is clean, has clean clothes and okay.

As for the large particle, yes it could, but the people would have to have been measured immediately after they left work, to account for the large particle that would be excreted in the feces.

So that's why with thorium, if you want to count the lung, you have to have the excrete measurements at the same time, to really have something near to reliable interpretation of monitoring results for thorium.

MR. STIVER: Sam, I might also add that you see this so frequently that, you know, it's something that would be kind of an off-normal event, like a contamination event or, you know, large particle inhalation. You wouldn't expect to see the same pattern in so many different workers for the same data, type of data.

So I think the issue of the high level of uncertainty is certainly a valid one. Mark does have a point. A lot of them are, you know, definitely below the detection limit. So you know, you're looking at a situation where you have a probability anywhere from zero to the detection limit of about -- where basically you're getting numbers out of the detector that just are really meaningless in terms of an actual intake.

But I think probably the more important issue is this idea that the equilibrium ratio can vary so much based on those actual studies that were conducted. I mean you know, theoretically if you're in a closed system, you know, the lowest ratio of .42, I mean of 228 or excuse me, lead-212, starts to come back up again, is probably valid.

But you know, as Joyce brought up, you know, you've got a Group 2 element of the radium, which is giving rise then to the thorons. There could be some migration, you know, out of the immediate area. Even though the thoron only has a half life of about a minute, there can be some migration that could account for these high amounts of variation in the equilibrium ratios.

So the fact that you can under-estimate dose by factors of five or ten, I think, is a pretty serious thing that needs to be addressed.

MR. ROLFES: Well, what we've got right now, you had mentioned lower. You had identified the range of correction factors, basically, based on ratios of lead-212 to actinium-228 -- the thorium-232 activity, excuse me. We've got a midpoint right now of .71, which falls in between .42 and 1. We can always adjust that based upon, you know, information.

If there's evidence that supports, you know, adjusting the correction factor to this or that. But keep in mind how the NIOSH dose reconstruction process works. We're talking about a factor of two or five. You know, for internal doses, that's pretty good. When NIOSH receives that information, rather than just assume, you know, many of these thorium campaigns were short duration.

Some of them did last, you know, a couple of years in duration. But if you take a look, we've got a short duration project and a thorium lung measurement following it, and for us to interpret that data, rather than focus on only assuming that there was an exposure that occurred for two weeks for that campaign of thorium, we would take that lung result and use that result to assume that they were chronically exposed, you know, back to the previous lung count, if there is one in that case.

So if we have a two-week project but we're assuming a full year of exposure, we're talking about 52 weeks rather than two weeks. So that factor is 25 times greater. The actual intake, you know, is going to be 25 times greater than --

MR. STIVER: I understand, you know, the approach of using claimant-favorable assumptions in reconstruction, as I hope, you know, achieving a bounding value to where you don't have to deal with uncertainties to the same extent.

But I think in this case, you're looking at uncertainties that I think would have to be factored into the model, either through a higher GSE or some kind of a -- some combination thereof.

MR. ROLFES: Right, we agree. I think we agree on that, and it's just what the correction factor is.

MR. STIVER: Yes. It's just a

matter of determining what it's going to be.

MR. ROLFES: We agree on that completely, and we've said that, you know, we can certainly adjust the correction factor we proposed. Let's see. Well, that's not specific to this correction factor, but we have made some bias adjustments to the -- this is another portion. I know Joyce presented a lot of, you know, different areas without really given us the opportunity to respond to each of the issues.

One of the responses here that we have made corrections to the in vivo count biases, and I can read that if you'd like. This is out on the page where I've also -- or O: drive, excuse me, for the Advisory Board. It's been sent out, dated January 19th, 2011.

It says "In Finding 8 of their June 2010 report on in vivo chest count data, FMPC, SC&A identified an apparent negative bias in the FMPC in vivo chest count data for lead-212 that was used in the proposed FMPC in vivo coworker model. NIOSH agrees and will make a bias adjustment to the coworker model for thorium for the period of 1978 through 1988.

"Inspection of the data used in the coworker model reveals that nearly 75 percent of the lead-212 data were reported as less than zero. Only the lead-212 data were used in the proposed coworker model. In an unexposed population, one would expect half of the results to be less than zero and the other half to be greater than zero.

"Given this information, combined with the assumption that monitored individuals had some potential for intakes, there is clearly a negative bias in the data set. A bias adjustment to the coworker model will be accomplished based on data collected in 1978 and '79.

"In those years, data were reported for both thorium and lead-212 lung burdens. The median thorium lung burden was .22 nanocuries, and the median lead-212 lung burden, which is taken to be equal to the thorium lung burden, was zero nanocuries.

"The lead-212 lung burden for years 1978 through 1988 will be increased by .22 nanocuries, and the subsequent intake rates will be revised for the in vivo coworker model prior to its use in formal publications."

MR. STIVER: I read that response. Now in my mind, that is related more to the issue of the elevated background for possible site irradiation from bone and that kind of thing. That's a different issue altogether --

MR. ROLFES: Correct.

MR. STIVER: -- this particular factor. But I think this factor, the equilibrium ratio for daughter products seems to be accounted for as well.

DR. LIPSZTEIN: Yes, and another thing is that we cannot mix the data after '78 and the data before '78. The data before '78 were at the time -- results are in milligrams. We don't know how this data was acquired.

I didn't have any satisfactory answer or document telling me how this data was acquired. If you look at all the conversion factors, they have so much uncertainties that we cannot rely on them. We have a resulting milligrams of thorium. We don't know how it was done.

The second part of this is the results of lead-212 being used to calculate the thorium-232 activity in the lung. Then we have a problem that is common even nowadays, when you rely on the daughters to calculate the dose to thorium-232.

It's a problem. It's not resolved in general. It can only be resolved when you have additional bioassay data. But that's a different problem from the first one. The first one, it's so uncertain that we don't know anything.

So we cannot assume something like put some error factor to be on the safe side. But you just don't know where this data comes from and how it was calculated and everything else.

MR. ROLFES: Well, we prepared responses to each of these issues. We've sent them to the Advisory Board Work Group, and what I was trying to get us back to is there were a couple of action items at our last Work Group meeting.

The one thing that we were asked to do was to contact Y-12 regarding -- we were given a specific individual's name to contact at Y-12 to see if we could obtain any additional information on calibrations and operations of the mobile in vivo radiation monitoring laboratory at Fernald.

We've done this, and I received an email just late last night saying that the information has been sent to us. I haven't had the opportunity to review that. Our team hasn't had the opportunity to review that, but we certainly want to take a look at that information to see if there's anything that can help to, you know, further support our position one way or the other.

As far as uncertainties, I mean if there's uncertainties that we can characterize and quantify, then we would use those uncertainties to the benefit of the doubt of the claimants. Let's see. I think what we had captured, back to the data from Y-12, we had gotten roughly 300 pages, which also had some Fernald-specific information in it.

At this point, I'd also like to ask Bob Morris to chime in, to see if he has anything that he might be able to add to the discussion on thorium in vivo counts.

MR. MORRIS: Thank you, Mark. My only addition would be to say that, you know, we have papers that were produced by Hap West and his crew at Y-12, 1965 time frame, where there was an installed chest counter using nine-inch diameter, four inch thick sodium iodide crystals above and below a worker who was lying down.

We know that that is the same geometry that went into the mobile in vivo laboratory counting, and we actually have a pretty good handle, at this point, on how that installed system at Y-12 was calibrated and how the data were interpreted from there.

We're hopeful that this information that we've now been able to locate at Y-12 that is specific to the mobile counter will validate our belief that the calibration for the mobile lab was similar or identical to the one that was installed at Y-12. If that's the case, then I think that we've got a pretty good method in mind that will bring us to the ability to define the uncertainties and specify exactly how the calculations were accomplished.

If not, we'll just find out what is in that data now that it's been obtained and reviewed by a classification officer. So I think the information is just now becoming available to us from Y-12 on that mobile calibration.

MR. ROLFES: Thank you, Bob.

MR. STIVER: Mark, when you have a reading for a particular individual at less than six milligrams for this period, you didn't then default to one-half the MDA and provide a chronic exposure like you would for any of that --

MR. ROLFES: That's correct. So yes. I mean even if you have an individual with a positive result, we would consider that positive result and any values reported below the limit of detection --

MR. STIVER: They still get a --

MR. ROLFES: And we would assign either a full intake based upon a positive result. If they didn't have any positive results, we would still calculate a missed intake, which could have resulted at a level that didn't deposit enough thorium in the lungs to result in a positive whole body counts.

MR. STIVER: Their value would be a -- default to one-half the detection limit?

MR. ROLFES: That's typically the way we --

MR. STIVER: That's what I thought. I just wanted to make sure that this would still apply in a particular situation.

MR. ROLFES: We wouldn't take that, you know, for example, the value that Joyce had pointed out as .5 milligrams, we wouldn't use that. We would use half of the limit of detection of six milligrams, and so default to three milligrams for a missed intake.

MR. STIVER: Well, Joyce, I noticed that some of the data are very extremely high values, and, you know, given the uncertainties that are involved, is there a particular number?

I don't know. I just kind of put you on the spot. But I mean is there a value that you feel would be high enough to where the uncertainties that exist might -- would have some confidence that that would be a bounding value?

DR. LIPSZTEIN: I don't know because I don't know what's spurious, what is external communication, how it was derived. So we don't know. My problem is that I really don't know. One thing is the data after '78, which we are using the lead-212 results. The other thing is the data before '78. So before '78, there's so much uncertainty.

If you calculate, you know, even when you have in '78, you have the two results of lead-212 and in milligrams of thorium, we saw that the variation in calculating the thorium lung burden by the two measurement results, they vary so much, almost 100 times, you know, the thorium to thorium ratio, calculating one way and calculating second way.

So I think we don't know anything about that thorium in milligrams. When you come to the period after '78, when you have really the lead-212 results, even if you don't know how they calculated at the time, now we have a lead-212 results. Then we can, you know, say well, the uncertainty because of the daughters could be as high as and apply the uncertainty on the counting measurement because they were only counted for 20 minutes, can be as high as -- but we have, you know, we know where we stand for. We are doing all the calculations based on lead-212 results. But on the period before that, we don't know. We don't know what they used. I read some papers after '78, that were after '78, where they were using both actinium and lead-212.

MR. STIVER: It appears, at least from the Technical Basis Documents that you quoted in the report that pre-'78, they were relying pretty much on actinium-228.

DR. LIPSZTEIN: Yes, I saw that.

MR. STIVER: That seems to be extremely problematic, I would think.

DR. LIPSZTEIN: Yes, yes. Then

NIOSH modified its answer, so I don't know where we stand for it. But anyway, you know, it's a lack of information. Without knowing that, we don't know what these results mean.

MR. ROLFES: I think we pointed out that lead-212 was used for the earlier years, not actinium-228.

MEMBER ZIEMER: As well as the later years.

MR. STIVER: So it's for both periods? I thought it was only for the post-'78.

DR. LIPSZTEIN: Yes. Well, that was post, that lead-212 was used. On the answer, we had a response to SC&A comments, saying that the lead-212 was used. And that's it. There was no other, you know, document or anything like that, saying why.

I personally had read some documents that were posted on the O: drive, where they used after '78. There is no information on before '78, saying that to calculate the activity of thorium, they would use both actinium-228 and lead-212. I think that's true because they have both nuclides listed and we have a measurement value for actinium and for lead-212.

Now then, after '78, the actinium-228 result is not used, was not used by NIOSH, and only the lead-212 result was used, which is the correct thing to do. But when we have the results in milligrams, if people used both nuclides, you know, there is an error here, and we don't know what they did and what they have done.

So what I'm trying to say is that if we can quantify an uncertainty, it's only after '78, not before.

MR. MORRIS: Mark, this is Bob Morris.

MR. ROLFES: Yes, Bob.

MR. MORRIS: We do know that in 1965, according to the paper by West and others, that lead-212 and actinium-228 were considered. There were two regions -- three regions of interest that were defined, along with a control region of interest for each one, that was used to define the lung burden.

We also know that, from the documents that we have on file, that they were able to make better assessments of the lung burden if they knew the operational history of the counting, of the person being counted and the material they were exposed to.

So we know that there was the capability and really the desire to talk to the health physicist that was assigned to the area in order to understand the kinds of material that were being used and the potential for disequilibrium.

Now having said that, I'll just repeat what I said before, is we hope to find in the documents that have just now been released by Y-12, the information specific to the calibration used at Fernald in the early -- in the first ten years of use of the mobile in vivo radiation monitoring lab.

DR. MAURO: This is John. Did the breathing zone data that was collected pre-'69 continue past '69?

MR. MORRIS: No, it didn't. That really was -- I'm putting two and two together to try to figure this out. But it appears to me that when the mobile in vivo radiation monitoring laboratory became available, that daily weighted exposure efforts went down drastically, the effort that went into that.

So we actually see a clear break point in time where the DWE data dwindles to zero and then we've got this, a campaign that started in 1968 to count every thorium worker of record that was on site. That was the first plan of use of the in vivo laboratory when it came in 1968 was to go back and capture thorium workers.

Then going forward, they had the laboratory on site every 6 to 12 months.

MR. STIVER: Okay. You know, this

is John Stiver. Given the importance of this calibration information, it would be great if you guys could provide that to us when you get it. I'd really like to see that here. It seems like everything hangs on the validity of this MDA value and the calibration methods.

MR. MORRIS: I don't think the MDA value is that important.

MR. STIVER: Not the MDA, excuse me, but basically the uncertainties involved in the type of calibrations that were done, whether it was actinium, lead. From what you said about the West article though, it sounds like they had a pretty robust system here.

MR. MORRIS: Yes. The West article in 1965 was several on the topic.

MR. STIVER: That's 1965, so that sounds like good news, as far as being able to reconstruct the doses, at least at this point, without having seen the information.

MR. MORRIS: The West article has been in our data set all along.

MR. STIVER: You have the SRDB reference for it by any chance?

MR. MORRIS: Yes.

MR. ROLFES: One of the documents, I don't have the Site Research Database number here, but it's a Y-12 document, Health Physics Considerations Associated With Thorium Processing, Union Carbide Corporation, Nuclear Division, Report No. Y-KB-53 C.M. West, 3/25/65, and it's part of our previous responses to SC&A's review. This is also one of the sources where it cites the 20 minute count in the mobile in vivo lab.

MR. STIVER: I think I might actually have that one.

MR. ROLFES: But that document also, I believe, is the same one by Hap West. He has quite a bit of discussion about the disequilibrium and corrections to equilibrium factors for thorium-232 progeny.

MR. STIVER: Bob Barton, are you out there?

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

MR. STIVER: Yes. Could you see if you can find that on the SRDB at some point?

MR. BARTON: Sure. Can I have someone repeat that number?

(Simultaneous speaking.)

MR. ROLFES: Search for West.

DR. LIPSZTEIN: From the documents that I read also, even if they were after the period '68 to '78, they confirm that both actinium-228 and lead-212 were measured, and they would, you know, use both data to get into the activity of thorium.

MEMBER ZIEMER: They probably developed some sort of ratios of those two, to get at it. But could I ask a question? I'm trying to understand fully Joyce's issue. So and Mark, maybe you can help me understand the process here.

So now let's say they get a lead-212 count. In reality, you've got to go back with the biokinetic model to the previous intake time, right? That will be different. It will be different for the lead than the thorium. I think that's --

DR. LIPSZTEIN: That's for the period where we have the lead-212 results, yes.

MEMBER ZIEMER: Right, right.

DR. LIPSZTEIN: The lead-212, you know, the amount of lead-212 in the lung will under-estimate the amount of thorium-232 in lung, because of the different behaviors, the daughters, radium and radon will disperse from the lung more fast than --

MEMBER ZIEMER: Right. But you can use a specific biokinetic model for each of those. So I think in principle, you can do that. But you still have the issue of the starting ratio, I guess, of what those were when they came in.

DR. LIPSZTEIN: Yes, right.

MEMBER ZIEMER: But on a given person, you're able to track --

MR. STIVER: You have multiple data points.

MEMBER ZIEMER: You've got multiple data points. So you at least have that for the lead.

DR. LIPSZTEIN: Yes.

MEMBER ZIEMER: And in principle, then, you can track the other back by a different kinetic model to some start time. But you still -- I think those parts, it seems to me in principle, you can handle it. I guess my concern is the initial ratio when they first enter the body, and that would be the only uncertainty that I see.

I think you can handle the rest, and the counting uncertainty is very straightforward. That's simple accounting statistic. So that --

MR. STIVER: Yes. That's --

MEMBER ZIEMER: That's just, you know, the count rate and the total. So I think the uncertainty that I'm worried about is that initial ratio, and I'm trying to understand how you handled that.

MR. ROLFES: Then we're right in the middle because --

MEMBER ZIEMER: The calibration might help you on that.

MR. ROLFES: It could, it could.

DR. LIPSZTEIN: The problem, I don't know if I'm interrupting someone.

MEMBER ZIEMER: No, go ahead.

DR. LIPSZTEIN: The problem with the lung biokinetics is there is not a well-established model, you know, to go back. We just know that there are a lot of uncertainties. So the only way to really get rid of those uncertainties is by measuring feces and lung at the same time, and if you have -- it's impossible to have had thorium measured in urine at that time, because there was only with some mass spec -- data from today.

But if you have feces data, you

could compare with the amount in lead, and then come to a reasonable conclusion of what is in the lungs. But that's a real problem. Trying to switch to is very, very difficult, until nowadays.

MEMBER ZIEMER: Well, yes. Joyce, even if you had that data, I think if there's another compartment in between the final excretion, you still may not know the rate at which it leaves the lung because it may go to another compartment.

Yes. Well, okay, but I think that calibration data will be very important.

DR. LIPSZTEIN: Oh yes, especially you know, for the '68 to '78, we really don't know anything, how they did, how did they account for both the actinium-228 and the lead-212.

MR. ROLFES: So well, without reviewing the data, I can report back to the Work Group after this meeting as to what is contained in the new information we've received. Hopefully, we can move on from there. So that's, I guess, the action item that I'll report.

CHAIRMAN CLAWSON: How much, in the early years, how much thorium went through Fernald? You know, you mentioned small little campaigns, one or two days here and one or two days there.

MR. ROLFES: Right, right. Let me, yes. I've got to pull out my time line here.

MR. STIVER: Yes. Bob Morris put together a time line back in 2008, and it's got a nice little graph in the -- this little. It shows the amounts, the plants and the process that took place.

MEMBER ZIEMER: Where is that?

MR. STIVER: This is on the SRDB if you want to take a look at it.

CHAIRMAN CLAWSON: The reason why I brought that up is because when you had mentioned small little runs here and there, I found in the 1960s railroad cars, not --

railroad cars, five railroad cars.

MR. ROLFES: Sure. Thorium nitrate tetrahydrate is probably what you're referring to.

CHAIRMAN CLAWSON: Being sent up there, and it surprised me, and this is a Hanford document.

MR. STIVER: Yes. You look at the values here. There's metric tons here.

DR. LIPSZTEIN: The dose per unit intake for thorium is very high. So it's very problematic because even if there are small quantities of thorium inhaled, the dose is very high. It's comparable to the problems with plutonium. Thorium is one of the worst elements.

MR. BARTON: This is Bob Barton. Just for everyone's benefit that time line that Bob Morris put together. It's on the O: drive in the AV document review folder under Fernald, and the title is Thorium Time Line With AA, and it's dated 2/29/08. That shows a nice table with the plants and the years and some information on how much was processed.

CHAIRMAN CLAWSON: When we mentioned earlier, it surprised me to see this type of tonnage, and that kind of took me by surprise on that. But didn't Fernald actually become the nation's --

MR. ROLFES: That's what I was going say. In 1972, Fernald was designated as the thorium repository for DOE. So essentially any unused thorium was sent to Fernald for storage.

MR. MORRIS: If I recall -- this is Bob Morris, excuse me. Brad, if I recall correctly, that thorium nitrate in the rail cars was received but not processed. I mean they didn't actually purify it.

That was one of the things that our interviews revealed is that the chemical engineer involved said, you know, it was a real thing of contention whether they should actually purify that big source of thorium when it came in or not. And I think, if I recall correctly, that they didn't.

MR. STIVER: That was Kispert?

MR. MORRIS: Well, I'll leave that alone for now. But we do have, in our interviews with some of the chemical engineers associated with the site, that that was --if I recall it correctly, that that was not purified material at the Fernald site. It was brought in and then disposed, if I recall. Mark, do you remember it?

MR. ROLFES: I don't recall. I remember seeing a shipment of roughly 33 rail cars, railroad car loads of thorium nitrate tetrahydrate coming into the Fernald site, and I don't remember the fate of that specific material. I do remember, you know, seeing that, and it didn't seem as a surprise to me since Fernald was in fact designated as the thorium repository.

MR. MORRIS: Well, sorry to divert that, but we do have information specific to that example you brought up, Brad.

MR. ROLFES: Yes.

MS. BALDRIDGE: Can I make a comment?

CHAIRMAN CLAWSON: Yes.

MS. BALDRIDGE: They were designated the national repository in '72. But there's a document in the petition that states in the 50s, they were asked to start stockpiling the thorium.

So you're looking at from '72 back to '60 is 12 years, and then back into the 50s, possibly another three years. So we're looking at at least 15 years that they were stockpiling, before they were designated the repository.

MR. ROLFES: You're right. We're not trying to say that there was no thorium on site prior to 1972. That's not at all what we're pointing out because we do recognize that within -- across the entire United States, we wanted to start stockpiling thorium because of its interests in the nuclear fuel cycle.

You know, they were trying to purchase it from like GSA and from some private industries who were extracting, you know, heavy metals from different ores. And you know, we're not trying to say that there was no thorium at Fernald.

MS. BALDRIDGE: But the fact that it was there, it was a hazard, based on the condition of the containers that it was kept in, and it was a bad enough hazard that there were documents where it actually burned through concrete floors and pads that it was stored on.

MR. STIVER: I think that was the issue of the drum deterioration and some of the oxides and some fires and things like that would take place from time to time.

MR. ROLFES: I don't disagree with that either. I mean I understand.

MR. STIVER: We brought that up in

our thorium paper.

CHAIRMAN CLAWSON: Well, the only reason I brought that up, Mark, is because you were talking about small campaigns, you know, of two weeks here and two weeks there. I read through these documents and also some Hanford documents that were discussing this. They were talking about the degradation of all the drums and what they were going to do.

They had campaigns to recapture this because the drums were deteriorating and falling to pieces. They had redrumming operations, and they were having to get it into a form that would actually hold up in the drums.

MR. ROLFES: Right. They had to stabilize them.

CHAIRMAN CLAWSON: And that was my question because I kept hearing you refer to they had a little thorium run here and a little thorium run there. I'm sitting there looking at 460 tons.

MR. STIVER: Yes, particularly when you compare it to the uranium that was processed, it's fairly small.

CHAIRMAN CLAWSON: It's a large volume.

MR. STIVER: It's a huge amount, but not compared to a lot of the other materials --

MR. ROLFES: A very dense material. If you compare it to water, you're talking a difference of, you know, 19 grams per cc, versus one gram per cc. So much more dense material than we're typically used to. It's very dense material. So thorium, uranium as well.

MR. STIVER: I guess the issue here is the high uncertainty and how to deal with that. I think we've done some of -- about the equilibrium ratio, that we might consider looking at some kind of a correction factor that would account for the increase in the GSDR.

CHAIRMAN CLAWSON: So the action item for this is actually we'd like to see the data that they just got.

MR. STIVER: The Y-12 calibration data.

CHAIRMAN CLAWSON: Now does this need to go through any kind of a process? It's already been cleared, or can they just send that over, as is?

MR. ROLFES: We can send the data that we've received and the data that we have as is, or we can identify it. It might be, you know, quantity-prohibitive. It could be several hundred documents.

MR. STIVER: Is it something that can be posted?

MR. ROLFES: I can post whatever you like.

CHAIRMAN CLAWSON: Well, just so that you guys can see this.

MR. STIVER: Once you get as far as the Y-12 calibration data, I'd certainly like to see that.

MR. ROLFES: Okay, all right.

MR. STIVER: So let's see. The second general comment was related to the -- not the data quality, but assuming the quality is acceptable, is there a sufficient quantity of data to characterize exposures for the categories of workers and buildings at the various times? The same kind of a problem that we saw with the DWE data.

Bob Barton has looked at this pretty intensively. So, Bob, I'd like to go ahead and turn it over to you.

MR. BARTON: Thank you, John. Like he said, what we did is we wanted to go ahead, dive into this database and see, you know, what groups of workers were monitored, what exposure potential there was to these groups and whether, you know, you find a group of workers who had a high exposure potential, but maybe wasn't really monitored as much as some other groups, which could throw off your distributions and ultimately skew the coworker model so that it's not quite claimant-favorable anymore.

I'd like say, before we kind of dive into this whole thing, from where I'm sitting, and I think my SC&A colleagues would agree with this, this seems like a tractable problem. But we felt it didn't really get enough time last time in the meeting. You know, we only really had a few minutes to quickly go over and discuss it.

So I'd like to go into a little bit more detail of what kind of analysis SC&A performed and what implications we can derive from that.

So as a starting point, kind of intuitively we said all right. We want to try to see if there's a group of workers out there who had high exposure potential but maybe wasn't monitored frequently, or you know, the monitoring program was not targeted towards that group of workers.

We started with workers who had actually handled the thorium, and for lack of a better term, I'll refer to them as the thorium workers, even though we know that's not a real job title, that sometimes the workers moved around from job to job.

So we went in, and the first piece of material that we found that identified thorium workers was a memo by Bob Starkey at the very end of 1967, which basically listed 51 workers who were involved in thorium operations. The purpose of that memo was, when they wanted to start in vivo counting in 1968, they specifically wanted to look at this group of people.

That information came from the interview that I believe actually involved Mr. Starkey, in which that was really deemed the intent of this list, which is a very valuable piece of evidence because we actually could link people who worked with thorium with a specific year, and then look at how many of them were monitored and what the results of that monitoring were.

So as it turns out, in 1968, there were these 51 workers, and about a little over half of them were monitored that year. The first thing we did was okay, let's take those monitored workers who were identified with thorium operations. Let's just do a simple rank order of the in vivo counts that they had in 1968, and let's compare it with the rest of the workers in that year.

You rank order it and you take a look at it and you see, okay, not that surprisingly, the workers who were identified with thorium operations had higher lung burdens than the rest of the overall population.

So that's good. We have information from 1968. We can show that at least half the workers were monitored. They had a higher exposure potential, but at least we know who they were.

The second piece of evidence we found became a little more problematic. This consisted of a series of in vivo log sheets covering 26 workers. In the top right corner of the log sheets, it's handwritten in either thorium or former thorium worker. Now we don't exactly know when these labels were applied, what work period they were applied to, when these workers might have been involved in thorium operations.

What I can tell you is that of the 26, 17 of them were labeled as former thorium workers, and the rest, nine workers, were labeled as thorium workers. Now like I said, we don't know.

Those labels could have been applied the first year they were counted, in 1968. They could have been applied anywhere in their employment period. They could have been applied at the end.

One piece of evidence that I would point to that might suggest that they were applied at the very start of counting is that of those workers who were labeled as "former thorium workers," and again there were 17 of them, 16 were also contained in the Starkey memo.

So that kind of piece of evidence would kind of suggest that okay, if they were listed as thorium workers at the end of 1967, presumably so they were counted in 1968. But if they were listed as former thorium workers, how do we know that they continued operations, you know, throughout the rest of their employment history?

So you're kind of left with then ten workers who are not or who weren't in the Starkey memo, nine of which are listed as thorium workers. So again, it gets more problematic as you get out of 1968 because we just don't have information on which workers handled thorium, when they handled it, whether they were counted.

So there's a lot of, and I hate to

use this word, because it's already confused a lot of people today, but there's a lot of uncertainty about whether you're covering the right people with this monitoring program. In fact, when you look at the data, every thorium count is accompanied by a uranium count. So it almost appears as if thorium was just counted along with uranium as sort of a complementary thing.

But there doesn't seem to be any indication that they tailored the monitoring program for thorium, to specifically look at high exposure jobs in the thorium operations. So basically what we did at that point, we said, okay. Don't have a lot of information here for us to say these workers were working with thorium here. Here are their lung counts. Let's compare them.

So we kind of took, made some broad assumptions and said all right. We have 51 workers in the Starkey memo, and 26 in these in vivo log sheets, and of course there's the overlap I mentioned, with the former thorium workers. You end up with 60 total workers that we can say at one time or another, they were labeled as a thorium worker.

Let's take a look at their doses, and in particular we're looking at the production period, which only lasted until about 1979.

So let's just take all of these, let's assume that these people who were labeled at one time or another, worked with thorium the entire time, let's take their doses, let's compare them to the rest of the workers, and lo and behold again, you find that this group of 60 has a higher exposure potential, as evidenced by their lung burdens.

That's, again, you rank order the data; very simple, quick, and in almost every single percentile of value, you find that the people who are labeled as thorium workers, even though we don't know if they were actually working with thorium at what point during their career, they still had the higher exposure potential.

By making that assumption that they worked -- through their whole career, you kind of see well, that's -- they're trying to look at the premise that thorium workers had a higher exposure potential. By saying that they were always working with thorium, you're almost diluting the results.

But even with that taken into account, you still see that those that were labeled as thorium workers had a higher lung burden.

So that's kind of the meat of it. You have evidence that thorium workers were not targeted. We don't know who they are, but there's certainly some evidence presented in the paper to suggest that they weren't specifically looking, when monitoring for thorium, at thorium workers, which is not that surprising, and in NIOSH's most recent response, they said they didn't specifically look for thorium workers, as we posited, but rather they looked at chemical operators.

It was also posited at the last meeting that chemical operators are actually the Bounding worker class. So it doesn't really matter whether you can identify who worked with thorium or not because the data has a lot of measurements for chemical operators. So that is something that we took and we're going to take a look at and see again, what can the data tell us about this.

And, John Stiver, were you able to print out those charts?

MR. STIVER: No, I wasn't able to, but I can direct --

MR. BARTON: Okay. It would be easier to have that kind of visual aid to look at.

MR. STIVER: Yes. It would be under the O: drive under Stiver, Fernald WG 110419, and you'll see the little subfolders there by issue. So go to Issue 6B, Thorium Intakes

From Chest Count Data, and you have ChemicalOperatorChartsnew.docx.

MR. MORRIS: Bob, this is Bob Morris. May I ask a question please?

MR. BARTON: Sure. Go ahead, Bob.

MR. MORRIS: Doesn't this, just on the face of it, the fact that you can actually find a positive correlation between thorium workers and these lung count data, suggest that in fact counts, the lung counts are valuable and can be used to gather information about workers and that the uncertainties are not so large that the data's not useful?

MR. BARTON: Well, as I tried to make clear, I don't personally feel that this is an intractable problem. We feel that there is probably a way that you can assuredly bound doses to the unmonitored thorium workers because as, again, the evidence suggests even in the year when they were explicitly listed, with the intention of monitoring them, you still only got about half of them.

MR. MORRIS: Right.

MR. BARTON: And you couldn't find them in later years. There are some very limited -- and, again, all this analysis is based on assuming that these workers, who most of them are only associated with 1968 or being former thorium workers, which we really don't have a definition for what that entails; but there's some indication that it just means they were part of the 1968 crew.

The question is what happens in the later years of production when you really don't have any information on who was handling it, whether they were monitored, and whether you captured the highest exposure potential.

The fact that when we make this kind of broad assumption, that even though a lot of them were probably only involved with thorium for part of their employment, and you're diluting the essentially exposure potential from thorium operations by assuming that even when they were just working with uranium, they're still thorium workers, you still get that sort of bounding nature for thorium workers versus the all-worker population.

Second, I hope we can look at, I'll look at a comparison we also did with chemical operators.

MR. MORRIS: Well, I think my point in making that question was to just show that without regard to whether we were reporting in milligrams of thorium or lead-212, there is still a useful set of data there that can actually demonstrate that thorium workers got more thorium exposure.

MR. BARTON: I absolutely agree with that, Bob, and one of things John Stiver mentioned at the outset was this type of analysis was completely aside from any quality issues brought up by Joyce. We took the data and just assumed it was all fine, and took it at face value and performed this analysis.

MR. MORRIS: So I think I would sum up at this point to say now we're not talking about whether the data are useful; it's about what the correction factors that are applied to the data would be?

MR. BARTON: I personally would agree with that. I don't know if anybody else on the SC&A team has any additional comments.

MR. STIVER: This is John Stiver. I think the issue we have here, this is real analogous to the HIS-20 construction worker subpopulation issue, and that is do you have a homogeneous population within all these workers who were monitored for thorium?

Is there a subset that's up at the high end of the distribution, so when you take the complete distribution, you try to pick off the 84th percentile or whatever, the 90th percentile.

That particular subset is being under-represented by that value to where it's really not a bounding intake for that subset of highly exposed workers. I think that's the issue here. You know, the graphs, the cumulative distribution functions that Bob has put together, demonstrate that, yes, there is a subpopulation of highly exposed workers.

Whether they're actually labeled as thorium workers or if they had previously worked with thorium and then at the time they were entered into a system, they may have moved on to another job, but still retained a significant lung burden, that's another possibility.

But I think these graphs do show that there is a subset of more highly exposed workers that need to be addressed in the coworker model.

MEMBER ZIEMER: What was the paper reference on that again, John?

MR. STIVER: Oh, for that particular graph?

MEMBER ZIEMER: You gave us a reference a minute ago. I was going to pick it up in the O: drive.

MR. STIVER: Oh, yes. Did you get

to the right folder? It's under Stiver.

MEMBER ZIEMER: That's what I was -- oh, a folder called Stiver?

MR. STIVER: O:, Stiver, and then under that, Fernald WG 110419.

MR. ROLFES: So this is something that we haven't seen before. This is the first I've seen it in your folder. It wasn't emailed to us prior to the Work Group meeting.

MR. STIVER: This is one of the things that we just put together, you know, kind of like you guys were doing at the last --

MR. ROLFES: I really can't comment on anything. I haven't seen it, you know.

MR. STIVER: Well, we don't expect an immediate response. This is just to demonstrate that, you know, there is an issue here. We talked about it briefly at the last meetings, and we really didn't have time to explore it in the detail that was warranted. Were you able to find it, Paul?

MEMBER ZIEMER: No, I don't find the Stiver folder.

MR. MORRIS: I think not under AV Document Review, it would be just under the O: drive.

MR. STIVER: I could help you find it.

MR. BARTON: Are these the same graphs that are in the June 28th, 2010 report?

MR. MORRIS: No, Bob. These are really in response to the brief discussion we were able to have at the last Work Group meeting.

I quickly glossed over what we had done, essentially saying that we thought there was a subgroup of thorium workers out there who were under-represented, but had a high exposure potential. It was suggested that it didn't really matter whether you knew who the thorium workers were. They were the chemical operators, and the chemical operators are all well represented in the database.

MR. MORRIS: Okay, thank you.

MR. KATZ: John, would you just, after the fact, after the meeting, if you would just, through Nancy, send that document formally out to the Work Group. Then they'll get it by email at some point.

MR. STIVER: Okay, okay.

MR. KATZ: Thank you.

MR. STIVER: So, Bob Barton, we probably want to kind of format it and put it into a formal presentation, with maybe some discussion of what's going on.

MR. BARTON: Sure, John.

MR. STIVER: Okay, and then get it to the Nancy, and we'll get it to the Work Group. That will be an action item for us.

MR. BARTON: Okay. Do people have the charts open in front of them? We can quickly go through them just to see what the data kind of says about chemical operators versus thorium workers. What we did is I guess I'll just give a little background, is, again, what we did at first was we said all right, we have a group of 60 who were involved in thorium operations at some time or another.

Again, we're going to take all of their records, and then we're going to compare them against all of the chemical operators that are in the database because fortunately, in a lot of cases, job titles were provided, so that the comparison wasn't too difficult.

Also, as a first step, we separated out those chemical operators who were not part of the 60 thorium workers we had identified, just to see how those people who were identified as chemical operators, but never had any indication of thorium work, which again, the indicators are very limiting post-1968.

One could say that you only really have your 51 from the Starkey, and then you have some overlap, and then you only have nine workers who are identified as thorium workers on their in vivo count.

Everybody else was a former thorium worker, and everyone pretty much got counted in 1968. So if we kind of make the jump and say that label was likely applied when they first started counting, you know, you're entering their name, their badge number, and they specifically wanted to look at thorium workers, it seems likely that's when the label was probably applied.

As an aside, while everybody's kind of getting this document open, another approach we took to try to get a handle on this was we took Bob Morris's time line, in which it shows what building or what plants and what years thorium was produced and said okay, there's information in the in vivo records that gives the plant number for workers.

So aside from whether they had the label of thorium worker or not, we'll just look at the plants that thorium was processed in. Now I don't think, to my knowledge these plants ever exclusively processed thorium. So you can't just assume that the records for say Plant 4 or Plant 1 in 1968 reflected thorium work.

But we took a look at it, and what we found is there's certainly no bias towards these plants, as far as thorium monitoring was concerned. So again, that was another piece of evidence for why it appears the monitoring program was not centered on thorium operations per se, but rather probably the larger operations involving uranium.

But just to add a caveat to that, Mark Rolfes aptly pointed out in his first response from NIOSH, because the mobile laboratory was not on site at all times, it's quite possible that even though the record indicates the workers in a plant that produces thorium, or maybe the record indicates it was in another plant, it doesn't necessarily mean that he wasn't involved in thorium operations.

It's just that when they were

scheduled to be counted, they happen to be in that plant. So that connection is a little tenuous, but it was worth checking out to see, again, the weight of evidence argument, but see what the data tells us.

What it seems to tell us is that the group of thorium workers and, again, this isn't a job title. These are workers who handled thorium, had the higher exposure potential, and it doesn't appear that the monitoring program was ever centered on those workers, with the exception of possibly 1968, when it was explicitly stated and a memo was put out listing workers for the purposes of counting them.

MR. STIVER: Bob, this is John Stiver again. Back up just a little bit. Now you said that for the records you're looking at, you have a particular worker, and it identifies a building, and we're saying we don't know whether that's the building they worked in or that was the building where the assay was conducted?

MR. BARTON: That's correct, or if you changed buildings, perhaps that building that went on the in vivo record we -- there's just no --

MR. STIVER: Okay. So that's not necessarily --

MR. BARTON: -- definite connection between the building number listed on the in vivo record and a time frame that they actually worked in that building. You can certainly assume that they were counted in closer proximity to the building number listed on the in vivo record, and that's certainly something we looked at, and that's how you sort of get that, again, a weight of evidence argument that says it really didn't look like the buildings that were processing thorium were being focused on by the thorium monitoring. It really just appears as though the thorium counts were incidental to uranium.

MR. STIVER: Does everybody have the

graphical file open? Would you like to just kind of walk through each of the figures and talk about them just for a minute? Because everybody, I think, has the file opened now.

MR. BARTON: Okay, good. So the very first figure is kind of what I was talking about. We have a group of 60 thorium workers, and those are represented by the blue line, and, again, those 60 are just they were involved in thorium operations at some point.

Then the red line there is all of your chemical operators, which are going to include some of those 60 thorium workers who are also chemical operators. Then the final line there is the green curve, which are thorium workers or, excuse me, chemical operators who were not part of the 60 who were ever identified with thorium operations.

Then you can see the two groups of chemical operators are very close. But when you include those chemical operators from the 60 thorium workers, it becomes rather limiting. But even more poignant is the thorium workers themselves, that blue line, which is clearly below both chemical operator groups.

Now if we scroll down to the second page, if everybody's ready, there was just another test where we pulled out all the chemical operators, and, again, this will include some of those 60 thorium workers, and we just compare it to the all-worker average.

The two cumulative functions essentially overlap each other. So it appears that chemical operators aren't really a bounding job category, but rather they could almost be the normal exposure pattern for all the other workers. Which is not entirely surprising. When you look at the records for thorium lung burdens, again milligrams thorium, chemical operators constitute almost 40 percent of the counts that were taken.

The remainder of the 60 samples also mirror the chemical operator lung burden almost exactly. You see the two curves, but they're almost right on top of each other.

MR. STIVER: Essentially the same population.

MR. BARTON: Right.

MR. ROLFES: John, excuse me. Bob on the phone, my apologies, Bob Barton. One thing I wanted to point out is possibly, you know, if you look at the individuals' lung count data, it identifies them as someone other than a chemical operator.

The one shortcoming that could be there is if someone was a chemical operator but, you know, received a promotion and became something other than a chemical operator, after you know, that particular job.

So I mean we've got to be cautious about using job titles to quantify exposure potential.

MR. BARTON: That's a good point, Mark, but I will say that this job title data was essentially on a measurement by measurement basis. They list the job title, as well as the plant, and the count results, the date, all in one line on these in vivo count sheets. So the job title should reflect the actual data point.

MR. STIVER: That could be a problem in later years, though, when you don't have the identifiers.

MR. BARTON: It's actually more of a problem in the earlier years, when the first counts in 1968. A lot of times they didn't list the job title, and in fact, in a second we're going to look at the 1968-only data, and for that, a lot of job titles I had to identify, based on that Starkey memo.

So a lot of times they wouldn't have a job title specified in the actual in vivo record, but from the Starkey memo, I knew they were a chemical operator or a machine tool operator or whatever it may have been.

MR. STIVER: So this is Figure 3 we're looking at now?

MR. BARTON: Yes. If we scroll down to Figure 3, this is looking only at the 1968 data, which is a little more valuable, because again, we have a direct correlation between year and who was designated as a thorium operator.

MR. STIVER: And it's a pretty sizeable difference there, isn't it?

MR. BARTON: Well, some of the percentiles you can see sort of at the lower percentiles, they're a little bit closer, you know. Then you get into the 20th, up to about the 65th, 70th, there's some distance before the other chemical operators kind of merge with them, and then again in the 90th percentile, they kind of merge away again.

MR. STIVER: Yes. The 90th percentile for all workers is correlating with about the 60th percentile for the chemical operators identified in the memo. A pretty sizeable difference. Then Figure 4 is --

MR. BARTON: This is, what

essentially I did here, because I felt that Figure 3 might be confusing, since you're looking at it's not all of the Starkey workers. It's only the chemical operators in the Starkey memo, versus the other chemical operators in 1968.

The reason I did this is I did not want to include all chemical operators in 1968, because of what I was just speaking to. A lot of the job titles were not there, so in order to do an all chemical worker category, I would have had to add in all the Starkey workers, which would have, you know, really muddied things up, because then you're going to have a lot more overlap.

What I did was I just compared the chemical workers from each data set, and you kind of see there's not that many data points for the other chemical operators. But if I added in all of the Starkey ones, they probably would have overlapped a lot more, which is kind of counterproductive to what we're trying to get at.

So just to show that if I include all of the workers in the Starkey memo and do the same plot, and again only compared to the chemical operators who are not identified as thorium workers in that year, it's essentially the same distribution. So that's really what I was trying to show there, just so it didn't really raise eyebrows as to, you know, what you're looking at.

MR. STIVER: Okay. Well, that was very instructive, Bob, and we're certainly seeing that there is a more highly exposed population that you've identified here. I think that has implications for the coworker model, as it's applied.

You know, we need to obviously, NIOSH needs to look at this, so we'll go ahead and do our formal review, and send it through the right channels and deliver it to the Board.

MR. BARTON: John, if I could make

one other suggestion? One other sort of contention that was put forward is that workers were chosen because of their exposure potential. I just wanted to ask NIOSH if that contention was based on worker interviews, or was it documentation regarding the bioassay program? Or if you came to that conclusion from the data itself.

MR. MORRIS: This is Bob Morris. We have that documented in interviews.

MR. BARTON: Okay. Oh, I mean that's good. I would say it might be instructive to look at the data and see, because that might be true for uranium monitoring, if the monitoring program is really geared towards uranium.

It might be instructive to look at the thorium records and say all right, who had the most frequent monitoring? Did they really have the higher lung burdens, and something like that might go a long way towards telling us, you know, what kind of a problem we have here.

MR. MORRIS: Perhaps for those of us on the phone who didn't get to see this stuff, if you could like sum up real quickly what you think -- John just said this could be a real problem, and you said it's tractable. I actually had trouble following, and I was hoping you could like tell us what you think the approach is that you have envisioned?

MR. STIVER: Is this Bob Morris?

MR. MORRIS: Yes.

MR. STIVER: Yes. This is John Stiver. I didn't mean to imply that it was a real problem in terms of like an intractable problem. I think it means is that there has to be some adjustment to the coworker model at some point, I think, to account for this subpopulation that we're dealing with. But I think it's an tractable problem.

MR. BARTON: And there is some information provided as to how much the exposure potential increased. I don't know if we can maybe extrapolate that 1968 operational data to other years, or maybe you could have a location-specific modification. I mean it's really not our place to say how this problem can be dealt with.

I guess the point of saying was it don't seem like it's, you know, the killing stroke here, you know, the end game. It just seems like it's something that really needs to be addressed, and if the coworker model isn't modified, you probably need to provide some rationale for why you think it's going to bound doses for this group of workers.

Because down the line, you're going to come up with a situation where you have an unmonitored thorium worker only. You're not going to know that they're a thorium worker, and if you kind of employ the coworker model just as is, that's probably not going to be claimant-favorable to that worker.

MR. MORRIS: Okay. Well, we already know that we have an adjustment to make in the coworker model, based on the bias of lead-212 that SC&A identified and we agreed with.

MR. BARTON: Right, and this is really a separate issue. Like I said, this is completely independent of the quality issues that Joyce raised. This is simply who was monitored and is there a worker population out there that could be underestimated, and how do you account for that worker population, which evidence suggests had unmonitored workers, who this coworker model is certainly going to apply to.

MR. ROLFES: I guess the important part of this is that the data are out there and available for us to analyze and come up with a correction factor, if need be.

I don't want to state that we need one without seeing the actual report that you're going to send to us. But you know, that's the important part, that the data are available to us, and it's just a matter of, you know, looking at the data to determine whether a correction factor is needed.

MR. STIVER: And we'll get on that.

MS. BALDRIDGE: I have a question. Will you use this same type of data for those who did have lung burdens with the thorium work, to apply to those prior to '68, that you don't have any data on? Or is there another way you're going to assign dose to prior than '68 people?

MR. ROLFES: Right, and that's probably the next issue that we're going to discuss, about pre-1968 thorium intakes. Those are based upon daily weighted exposure reports conducted throughout the Fernald site, from 1953 or '54 until, right up until '67-'68 time period.

What we're referring to here in this discussion is 1968 through 1988 time period.

MS. BALDRIDGE: And you would also expect those to show the same type of exposure potential, as those who were given the, had the lung monitoring?

MR. ROLFES: We wouldn't say that they had no exposure. Based upon the air monitoring data that we've looked at, that's what we're going to discuss up next here. The air monitoring data has been taken from various operations and places in the plant, and we've developed an approach basically to assign thorium intakes, by assuming that an individual was present in that area.

What we're going to do for the early time period, we've got a series of air monitoring results for the buildings that were involved in processing thorium, and what we've done, basically we're using the highest result for that building for that year, to assign intakes to people.

We're not considering any reduction in the exposure potential based upon respiratory protection, or based upon, you know, some of the airborne data. For some of the airborne thorium, there's a bunch of different particle sizes. We're assuming it's all respirable.

So any activity that's in the air that's measurable, we're assuming the worker was exposed to at the full concentration, and at the highest value in that building for that year.

MR. KATZ: Before we go into this, can we have a comfort break?

CHAIRMAN CLAWSON: Yes. I was just going to say --

MR. STIVER: That's probably a good idea.

MR. KATZ: So ten minutes? Is that good enough. A ten minute comfort break for everyone on the phone as well. So what time is it now?

MR. STIVER: Three o'clock.

MR. KATZ: Okay. So about ten past three, a little bit after. Thanks. I'm just putting the phone on mute.

(Whereupon, the above-entitled matter went off the record at 3:02 p.m. and resumed at 3:15 p.m.)

MR. KATZ: So we are back. This is the Advisory Board on Radiation Worker Health. We just took a short break, Fernald Work Group, and let me check to see. Do we have any Board Members? Do we have Bob on the line?

MEMBER PRESLEY: I'm still here for a little while.

MR. KATZ: Great. Thanks, Bob. And off we go, wherever we are.

CHAIRMAN CLAWSON: No, go ahead.

MR. STIVER: The next thing we'd like to talk about is Issue 6A, which is the pre-1968 thorium-232 intake estimates, based on DWE data, basically the breathing zone and general air sampling that was conducted. There have been several White Paper exchanges. The last, I believe we produced a review of Revision 2 of the NIOSH, the White Paper on DWE usage back in November.

Then they released Revision 3, which

basically took care of a lot of the concerns that we had. So now, we really don't feel that this is an SEC issue anymore. It's really a Site Profile issue, the basis being a memo or a paper put out by Davis and Strom in 2008, where they went back and looked at, I think it was five different uranium processing sites, a thorium processing site, and a radium processing site where radon was at issue.

They did fairly elaborate Monte Carlo simulations of the DWE data. They used the discrete data. They did log-normal fits to the data, and really with the ultimate goal of determining, you know, what is the uncertainty associated with these measurements, and how do we use that in a dose reconstruction environment.

The most recent version of the NIOSH White Paper basically is pretty much in line with recommendations by Davis and Strom. There are only a couple of issues in our latest review. There were nine findings, but really the ones that count, in terms of a valid modeling approach, I think, was this issue of a data validation.

Davis and Strom found a lot of errors. Not lots of them, but there were a lot of insignificant errors. But there were some which they called blunders, that were pretty significant. I think the very significant ones were up to a factor of ten, and this was due to mathematical errors, data transcription errors, putting in the wrong time for a particular task, things along those lines.

Now those guys, for whatever reason, had the advantage, and their DWE reports had the raw data with them. So they were able to go through and just look at the raw data and do their own analysis. One of the first things they did was a data purification effort, and this is where they discovered these types of errors.

Now I'm not sure that the Fernald

DWE data have that raw data associated with them. I know I've seen some of it, but it's certainly not contained in the reports themselves. So it may be problematic to go through and review, and conduct any kind of a search for a meaningful representative sample of that data, to do any kind of a, you know, validation exercise.

However, we feel that, you know, the potential for these large underestimates, and in some cases overestimates, it would warrant at least some type of preliminary attempt to scope that, the feasibility of doing that, to see whether, you know, those data are available. If so, what kind of a sample size would be statistically valid in order to --

Certainly with respect to the sample, everything single the DWE report would get through, and a representative sample would at least give some kind of an estimate of the frequency, or if it's even an issue at all at Fernald.

The other issue was that -- I believe it was for the pilot plant. You had a series of different steps, depending on the quality of data. It's kind of a hierarchy of methods, and the first being that when you've got good DWE data for a plant for a given year, you take the highest DWE for the entire plant and assign it to everybody.

On top of that, you get a GSD-85. So that was definitely claimant-favorable. I don't think there was any problem with that. That was recommended by Davis and Strom. It's an acceptable approach.

The other situation is when you don't have data for a given year, but you do have it in adjacent years for a given building, right, and you can use that as a -- not really as surrogate data, because it is from the same facility, but it's coming from a different operation and process. So that data could be used as well, with the same type of approach.

The third situation is where you just don't have data for a period of time. You have might have some unweighted air sampling data. Davis and Strom looked at this, and they came to the conclusion that even the average value of an unweighted distribution is, I think it was higher than all the three of their 63 worker categories. So just taking the average value will get you to a bounding, certainly a 95th percentile in most cases, based on the data that they reviewed.

But the problem we had with the new NIOSH methodology is they're not going to go beyond that. They're going to do the 95th percentile. When you do that, I mean you're in a situation now where, you know, certainly you're bounding, but you get into the issue of is this really plausible.

I think it was the pilot plant, 1967, when you went into that. We would recommend possibly reconsidering and using the

50th percentile in situations like those. Actually, I found in a line here where they talk about that exact issue in the Davis and Strom memo. I think it's, what page are we on here, page 159. It's a Health Physics Journal article.

They say, clearly, the site average is a biased estimated for exposure, that can be used in making compensation decisions when it's required to be favorable to a claimant. So they -- they also say that using a distribution for all samples from a plant without tying weighting or assignment to specific jobs does not produce DWA or GSD that's representative of any individual worker at that site."

So the idea of one is enough or not. We feel that probably the 50th percentile would be probably more defensible in that particular situation. Those are really the only kinds of issues we have with the DWE approach.

MR. ROLFES: If we had previously stated that we would use the 95th percentile to bound unmonitored workers' exposures, then that's what we're going to do. So we don't want to reduce doses.

MR. STIVER: The only point being is you're kind of bumping up against the notion of plausibility.

MR. ROLFES: If we're basically coming out on the high side, that's fine with me. I have no concerns, because it would be claimant-favorable. I don't want to go back, you know. We've never really defined, you know, "sufficiently accurate," and when we do a dose reconstruction, if the dose value's high, that's okay.

So that's -- I'm fine with the 95th percentile. If you would specifically like us to use the 50th, then we can certainly consider that.

MEMBER ZIEMER: Have you already done a number of these at the 95th?

MR. ROLFES: This method currently has not been used for a dose reconstruction, because it's still a draft method, and the current approach that we use for thorium dose reconstruction is based on the Site Profile, where we were assigning for every year, a 30 nanocurie intake of thorium-232 and a 30 nanocurie intake of thorium-228, I believe.

We've been assigning thorium intakes, but for certain years and certain operations, that intake is actually going to be lower than we've had in the TBD. For other years, it's going to be higher. So it's a draft method that we haven't put into operation yet, I guess.

MEMBER ZIEMER: There are some philosophical issues that I think are behind this. On the question of what's plausible versus bounding, and obviously where the fine bright line is is always a question.

But we've had at least a case or two where the bounding approach for some inhalation situations led to some proposed air loadings that were essentially impossible to defend scientifically.

Sure, they were bounding, and so you'd say yes, they're very claimant-favorable. But they were so, you couldn't put that much material in the air physically, so then you're erring on the side of saying it's not scientifically defensible.

And I think, maybe the suggestion here is along that line, I wasn't quite clear why you felt they were --

MR. STIVER: Well, my point being is like we just brought up. For example, the highest DWE was for a Plant 9 production plant, in 1955 and that's for a welder's helper. The DWE for that group is 690 MACs. That much above the limit, or it should be the limit, which at that time was 70 dpm per cubic meter.

If you look at the -- go through all that air sampling data, you have breathing zone samples for really dirty jobs that are very short duration, cleaning out a reduction --

MEMBER ZIEMER: Right, right.

MR. STIVER: And you might have, you know, millions of dpm per cubic meter in that operation, but only for five or ten minutes.

MEMBER ZIEMER: Yes. You couldn't stay in that kind of environment.

MR. STIVER: You couldn't stay in that kind of an environment. Then when I listed it on a paper, and that particular highest measure with seven samples was literally a million dpms.

But they're highly variable, and so if this is approaching the 95th percentile of the unweighted air distribution, we know the highest distributions are usually associated with this sort of, short duration task.

So if you take that and assign that to everybody in the plant, it's just not -- it's not a physiologically tenable position to take. That's my point. You know, it might not reach that kind of a physiological limitation or plausibility limitation for another plant with lower air concentration. But that approach, taken to its extreme --

MEMBER ZIEMER: Well, it's sort of like breathing snootful of smoke and you can do that for a few seconds, but you can't do it on a sustained basis. In fact, people take, at some point they take avoidance measures to get out of there. And I don't know if we're there, but I think that's the question you're raising.

MR. STIVER: Yes, and it's the issue of when you push so far up against the realm of impossibility that you don't want --

MEMBER ZIEMER: And I was just -- the reason I asked if you had used it, because if you're already used it on a number of individuals, then it's harder to back away from. If you're still considering it, you might at least look at that and say all right, it's sure bounding, but is it scientifically not tenable.

MR. STIVER: Well, you know, as kind of an aside, we had a reviewer a few years back kind of claim that for some of these detonations at the Nevada Test Site, we should be using a resuspension factor of 10 to the minus 5th. When you go ahead and put that much dust up in the air, see if you can even breathe or even walk into it and stay upright, not suffocate.

MEMBER ZIEMER: Right, right.

MR. STIVER: So yes, you butt up against believability.

MR. ROLFES: To maybe address it from a more broad scale from the dose reconstruction process, if you take a look at the cases that we've completed, you know, some of the early Fernald claims were completed with one of these types of scenarios, the OTIB-2 method, using an intake of 28 radionuclides on the first day of employment is a worst case, implausible scenario that was applied to ensure that we did not underestimate someone's dose.

MEMBER ZIEMER: Right.

MR. ROLFES: Now for cases, for example, for a lung cancer, we might not even need to consider thorium, and typically don't even need to consider thorium intakes, just because the uranium intakes alone are of sufficient magnitude to complete the case.

You know, when you get down towards the best estimate, we might actually have to do a best estimate of the individual's actual uranium bioassay data, interpret, you know, that data using claimant-favorable assumptions, look at all sources of information in there.

So I don't readily know of any best estimates off the top of my head for Fernald, and that's where we get down to, you know, the nitty-gritty. It's probably a handful of cases, where the best estimate dose reconstruction approach would come into play, and the issues that we've been discussing over the past several years really don't generically apply to our overestimating dose reconstructions or underestimating dose reconstructions.

They're really focused down narrow on a very, very small portion of the Fernald claimants that we have to do dose reconstructions for, and it's the best estimates. I'm comfortable with the information that we use in those best estimates, to make sure that we still have a very claimant-favorable dose reconstruction approach, to make sure that the doses that we've calculated are not underestimated, even when it's a best estimate.

To get down to, you know, whether it's the 50th or the 95th percentile in this case, all in all it's probably not going to make a difference for any but a possible small very, very small fraction of a percent of cases, for which we complete dose reconstructions.

MR. STIVER: Well, that would be our suggestion, you know. It's certainly not going to be a show-stopper.

MR. ROLFES: We can keep it in mind. So thank you. Keep that in mind.

MR. STIVER: Okay. That's really all I had to say about the thorium-232 DWE issue. Is Bob Anigstein still on the phone?

DR. ANIGSTEIN: Yes, I am.

MR. STIVER: Bob has done some work, and since Hans has become otherwise occupied, Bob has kind of taken over some of the issues regarding the K-65 silo radon emissions. Now we don't feel that this is an SEC issue, but it's been a topic of contention and debate for several years.

Bob has done the analysis the last time around, back in February presented a review of the dispersion model that was used by NIOSH. But Bob, if you'd like to go ahead and go and get started.

DR. ANIGSTEIN: Okay.

MR. MORRIS: This is Bob Morris. I have a question, please.

DR. ANIGSTEIN: Sure.

MR. MORRIS: Didn't you say at our last meeting, you said that K-65 emissions were not going to be considered an SEC issue?

MEMBER ZIEMER: Yes. He just repeated that, right.

MR. STIVER: Yes. There's still some, based on the paper that Mark put out on Friday, there's still a lot of these issues that apparently, if that is the current standpoint of NIOSH, that we feel that need to be discussed at that time on this.

I believe there was an action item that came out of the last meeting, to review the cases that might have been impacted and potentially consider the possibility of rescinding that guidance.

So that's kind of where we stand.

We felt it was important to at least -- Bob has just come up with a slightly different approach to looking at the source term deficit issue, and I think that it would be good for him to be able to at least share that with us.

MR. MORRIS: But still my question stands. Is this an SEC issue now?

MR. STIVER: I think it's probably to be considered more of a Site Profile issue at this point.

MR. MORRIS: Because on the agenda, it's listed as an SEC issue.

MR. STIVER: I think it's one of those issues that started as an SEC issue, and has kind of --

MR. KATZ: It comes from the SEC Petition, but that wasn't meant to imply that these are all still SEC issues.

MR. MORRIS: Okay, thank you.

MR. KATZ: Sorry about that.

DR. ANIGSTEIN: Okay. I don't know if anyone has had a chance to look at the short memo that was sent out yesterday afternoon. It was in response to the NIOSH report, which I don't know when it was officially put on the O: drive. I became aware of it late Friday, so there wasn't a heck of a lot of time to go over. I went over the report, but you know, it's hard doing background research.

But I probably would like to start off with something that would clarify, that sort of coalesced in my mind, as I was doing this over the last couple of days. There's been a lot of back and forth with NIOSH and NIOSH's contractors and us, about what's referred to the RAC report, the RAC model.

RAC stands for the Radiological Assessment Corporation, and that it was, had been worked on. It was the subject of several successive reports.

Just for background, RAC was originally contracted by CDC to do an offsite risk assessment for people living in the vicinity of Fernald, and one of the contributors to their dose was radon, emitted from a number of courses, but again, the main source was the K-65 silos.

Now everything that they did was starting with data from -- there were some measurements of radon in the 80's, the late 70's and 80's, at least it was in the 80's, and going into the 90's, and then Dr. Susan Pinney received a contract, originally submitted from NIOSH actually, to do a study on the effect of radon and cigarette smoking or a combination.

She and her coworkers used some of the RAC model or used the RAC model to estimate the radon concentrations in various buildings on the Fernald site. Now the main critique is all of the information comes from after the domes were sealed in 1979. There was no data, no radon measurements, no measurements inside the dome prior to that.

So consequently, what they

essentially did, and by the way, the model is not easy to understand, and not that I'm trying to be, make excuses. But the National Academy of Science, the committee, the National Research Council, that was engaged, hired by CDC to do a peer review of it, had difficulty understanding this model, and at first they did not understand it. Then the RAC people gave them some clarification and they said "Oh, okay. Now we get it."

But the way I understand the model, and I'm probably skipping something that I'm simplifying, is they both, they took a measurement of the external exposure just outside the dome using gamma radiation monitors, and also simultaneously took a gas sample from inside the dome. The dome is the air space covering the silos.

They made a relationship between the two and said okay, and then they said okay. They realized that some of that radiation will be coming from the radon daughter product, the short life inside the dome, and some of it will also be coming from the K-65, the solid K-65 material underneath.

So they looked at the radiation measurements that had been made earlier, when the dome was deliberately exhausted. They had a radon removal system that ran just briefly. So it was to reduce the radiation readings, so that workers could do some work, applying -- sealing the dome, and applying foam to the outside, specifically to reduce radon emissions.

So they said okay, here's the reading that's due to the material, basically the background reading, due to everything except the radon in the air space. Here, the reading with the radon in the air space was the differences due to the radon, and they made some.

Then they said we'll calculate. We know how much radon there is now with the dome sealed. We'll calculate the escape rates, because of course it's not totally sealed. So we'll calculate the escape rate, and of course we know what the decay rate is.

From those two factors, and I think assuming 100 percent equilibrium with its daughter product, we'll calculate the rate at which the K-65 material generates radon. Okay. That's a straightforward and is a very simple model. It doesn't require anything, any assumptions about diffusion rates or emanation coefficients from the radium.

In fact, it doesn't even require knowledge of the radium. It just says radon is coming in, radon is leaving. Here's how much is in there; we can calculate this number.

The problem with that in my mind is that it neglects the fact that radon generally, in a large matrix, 20 feet deep of this raffinate material that could be compared analogous to soil, the natural soil. Radon in soil does not move by diffusion. It moves by convection.

Only in conditions where there are no, there's no air movement and there are no pressure differences and no temperature differences, will diffusion be the dominant mechanism. But the air convection has been shown to the be the dominant mechanism in bringing radon into the basements of homes and modeling how to use a diffusion model has been spectacularly unsuccessful in predicting the concentration of radon in home basements.

There is no reason to believe it will be better here. The reason our numbers are such a magnitude, an order of magnitude difference than the RAC numbers for the emissions prior to the time the domes were sealed, is -- I mean we're basing it entirely on the deficit in the lead-210.

Whereas if you have radium generally in radon, and the radon decays in place and does not move, then you should have secular equilibrium between lead-210 and radium-226.

The lead-210 activity concentration actually will be a couple of percent higher than the radium secular equilibrium condition.

In reality we found that depending on -- we did two assumptions. One is let's say that when they put the waste in place about 1953, let's look at silos. We looked at Silo 1 and 2, but the researchers talk about one silo. And then the measurements were made in 1999, I believe, or the sampling was done.

True, there were only nine samples taken throughout the large volume of raffinate. If we were to, depending on whether we assume that there was equilibrium between lead-210 and radium, meaning that prior to the time the weights were in place, there was no radon escaping, or and therefore the lead-210 started to decrease or decay and was not being replaced because radon was escaping.

Or we assume that the radium had somehow been stripped of the lead-210 and radon, that is, prior history was exposed in such a way that all of the radon escaped, both of which are unrealistic but extreme, but the reality is in between. Some radon would have escaped before; some radon would have been retained.

In either case, we get a factor of magnitude, an order of magnitude, more radon having been released during this pre-sealing period than is in the model. The reason, the mechanism by which this can be explained is that you had very different conditions. You had this dome, which had this six-inch gooseneck pipe that went up, made 180 degree turn and was pointing down.

Now the wind blowing in a horizontal direction, that this would maximize the Venturi effect. It's a large opening that's tangential to the wind. So the wind is going across it, just like the wind going across the chimney of a home is known to increase the radon concentration, due to again, the Venturi effect or sometimes called in more everyday terms, the chimney effect.

The wind actually sucks the radon, sucks the air out of the home, creates a partial vacuum. It's like a high decrease of pressure, and if the windows and doors, it's the wintertime and all the windows and doors are closed, it increases the air flow from the soil through cracks in the basement into the basement and carries the radon along with it.

So that has nothing to do with the radon diffusion. It's just the air movement. The radon escapes from the soil matrix into the pores and gets carried into the house. The same thing would have happened here. The Venturi effect would have caused a decrease in the pressure of the dome.

The material, the air in these pore spaces would move up, will be presumably replaced, because again, there will be cracks inside of the dome, probably perhaps even in the bottom of the dome, and you would essentially have air moving through the dome and pulling the radon up at a much faster rate than would account by diffusion. Diffusion is a very, very slow process.

So now we did not model this, because we have no idea of what the magnitude of these effects are quantitatively. This would explain it and quantitatively, we find much of a 30 -- we find that, depending on which hypothesis we use about the initial conditions, we either had 36 percent as much lead-210 as expected, or at the opposite end, 51 percent, 52 percent.

This is right within the range, incidentally. So if we assume about 60 percent, this is right within the range of reasonable emanation coefficients. Obviously, the radon, the escape of the radon cannot be higher than the emanation coefficient, because it stays in the particle.

But for uranium ores, and this is the closest -- I'm not saying this is uranium ore, but it's a raffinate and no one has made a study of this, it can be as high as 58 percent radium-emitting.

So our position is that since this deficit in the lead-210 cannot be explained away, and there is no mechanism that has been proposed to explain why it would leave other than radon emissions, the only claimant-favorable assumption is that in fact the radon escaped.

Now there are possible other explanations, but they can't be proven. The explanation that the radon decayed inside the dome or in the passage through the thickness of the walls or the foam, again, in the earlier days, is not plausible, because that would mean that the radon was held up for many days and it has over three-day half life, it would take a couple of half lives.

It would have to be perhaps a week before the radon escapes, and even the RAC study concedes that there was very little hold up of the radon before the domes were sealed. So that it escaped as it was being evolved, and there was a low concentration in the air space.

So the explanation that the lead-210 faded out on the inside of the dome or in the -- or on the surface of the raffinate or in the material of the wall itself, again, is not plausible, would not answer that.

So I don't think I'll go through and answer point by point the comments in the NIOSH report, because I'm not sure everyone online has even read that report, which only came out on Friday. So I think I would just like to end with this general overview, and if anyone has any questions or objections, I can discuss it in more detail.

MR. KATZ: Thank you, Bob.

MR. STIVER: Bob, thanks a lot, and I don't know if anybody's had a chance to look at the paper. It was just released the other day, but it's definitely a nice adjunct to follow on to what Hans has produced in the previous two White Papers. I think it helps bolster our position about the reason for the higher escape rate and our calculations.

MEMBER ZIEMER: I have a couple of quick questions for Bob. Bob, this is Ziemer. For your source term, are you using the total activity in the silo?

DR. ANIGSTEIN: Yes.

MEMBER ZIEMER: Okay. Now of course you're still going to have the issue of how much of that radon really becomes available, even with a chimney effect. When radium decays, you actually get a recoil. The radon ion or atom recoils, but it's not necessarily available in the air spaces in the matrix.

DR. ANIGSTEIN: I know that.

MEMBER ZIEMER: So do we -- are you assuming that there's some fraction of that that's available to actually be sucked out?

DR. ANIGSTEIN: Yes. Again, it's a qualitative argument, because I'm not -- we don't know otherwise that we'll be doing this detailed model, which can't be done. Not only that we're constrained by not doing it; there's just not enough data on the emanation coefficient, on the porosity and on the diffusion, I wouldn't even consider it. And again, on the pressure differences.

So I'm simply saying that we observe reliably with a, that the ratio of the radon of nine measurements in dome, Silo No. 1, we observed a -- depending on that we assume that the lead-210 was already in equilibrium with the radium-226 at the time of the emplacement, or that it was totally absent.

The reality is there will be some fraction of this, and we either get a ratio of 35.7 percent or 51.6 percent, meaning 60, like going the other way around, either 64 percent or 48 percent or somewhere in between, of the lead-210 that was expected and it somehow escaped, and of the nine samples that we took, there was variation in the samples.

But the ratios showed less variation. There was only -- the ratios showed a coefficient of variation of less than 20 percent. So I think that that's very indicative for every single sample, depending on which assumption we made. There would be no -- the full equilibrium with the lead-210 or zero lead-210 presence in 1953.

In every single sample, there was a deficit of the lead-210, and the variation of the ratio is only 20 percent. So I think that given the accuracy of the data, that's a very strong argument that the deficit is real.

The only other arguments would be made that maybe there was some inherent bias in the assays, that somehow the radium was overestimated and/or the lead-210 was underestimated. Of course, we have no way of knowing that.

But presumably, data was published in the RAC reports, so they must feel that it must be sufficiently valid for them to have reproduced it. But I admit, it's a high amount, and it assumes a relatively high, but not implausible coefficient of emanation, which you were talking about, the fact that it comes out, which has been --

There is this data collection handbook put out by the Argonne people, their environmental group, headed by Charles Yu, and it reports from the literature that the emanation coefficient in crushed, wet crushed uranium ore could be as high as 58 percent. Does that answer your question?

MEMBER ZIEMER: Yes. Where does that leave us, though, with respect to --

DR. ANIGSTEIN: Well, that it could be that the -- there was a separate, they did a separate calculation. There was a very early report, which somehow got lost in the shuffle, and did not get actually issued. At that time, based on this, I think there was -- the only uncertainty -- I won't say the only uncertainty; the only, I mean the data was based on published data.

The one item that we weren't sure about was actually the mass of the -- we knew what the concentrations were in picocuries per gram. We weren't quite sure how many grams there were. But based on one assumption about the mass, which may be subject to future revision.

That over the period, the total deficit was between 1.9 million and 2.5 million curies of radon, total over the period from 1953 to 1991. This is about, this is an order of magnitude higher than the model used by NIOSH.

MR. STIVER: Bob, you mentioned in your paper also that it's got a misconception, it appeared on the part of NIOSH, that the equilibrium activity would be the upper limit, as opposed to the instantaneous release rate. I think you demonstrated how that misconception could give rise to the differential as well.

DR. ANIGSTEIN: Perhaps. I'm talking about cumulative releases, not about how much radon is out there at any one time, but about the -- you take the instantaneous rates, so many curies per second, and you simply integrate.

MR. STIVER: You said that's over a period of years.

DR. ANIGSTEIN: Yes, yes. I believe -- now in the RAC model, which NIOSH is using, that is how they describe it. They talk about cumulative, cumulative releases, not about the amount out there. They talk about curies per year released.

MR. STIVER: I was referring to the report that was posted on Friday.

DR. ANIGSTEIN: Yes, right, and my response to it was I thought that that particular point at paragraph five was just, you know, some -- that it really didn't belong there. I thought it was just a, you know. It's mathematically correct, but I don't think it's -- I think it's not what we're talking about here.

MR. STIVER: It's not describing the process --

DR. ANIGSTEIN: Right.

MEMBER ZIEMER: Are you assuming some -- you're assuming some decay during the transit time, even though it's short. What about played out?

DR. ANIGSTEIN: I'm not, you see I'm not modeling it. I'm not, everything what I said before was a qualitative argument that explains the difference between the calculation, which is a bit of an estimate, based on the deficit of the lead-210, and why, and then I was just trying to make, you know, trying to show a possible explanation of why this is so different from the RAC model.

I was simply saying that if you based it on diffusion, because the RAC model also goes into diffusion and that's what the NIOSH report goes into, and this report, the back up. It goes into making some assumptions about diffusion coefficients and emanation coefficients, and here you have these 20-foot deep pile of raffinate, and the radon is coming out by diffusion.

Well again, that's a very slow process, and most of it would have decayed if you assume that as the mechanism. The vast majority, particularly coming out from the lower reaches of the soil, of the raffinate, would have decayed before it ever escaped. If it decayed, where is the lead-210?

Once it decays to its own product, the bismuth and the whole succession of these short-lived products, these are not volatile. They don't move anywhere. They don't move by diffusion. They should remain. And yet the analysis did not find it.

So even for the analysis, they pointed out there is variation from spot to spot, even if it's not that bad. A 30 percent coefficient of variation isn't that terrible.

But if you look at the ratios for each individual sample, there's much less variation. Meaning when the radium is high, the lead-210 is high. When radium is lower, the lead-210 is lower.

But in each and every case, the lead-210 is lower than what you would predict on the basis of no radon escaping.

MEMBER ZIEMER: I guess I'm going to have to think about that, because I think diffusion really describes what radon is available in the air spaces to be removed. The rest of it is stuck there. So all that happens, I'm thinking off the top of my head.

I may be thinking wrong, but all that happens, if you bring a suction, is you pull that available radon that is in the air spaces, you pull it out. The rest of it's not available to pull out. See what I'm saying?

DR. ANIGSTEIN: Okay, into the -- excuse me.

MEMBER ZIEMER: The diffusion

coefficient basically describes the fraction of the radon that's available for being removed. So I don't think you can dismiss the diffusion coefficient. Well, give that some thought. I may be understanding it wrong, Bob, but my understanding of diffusion, it really describes --

DR. ANIGSTEIN: Who's speaking?

MEMBER ZIEMER: This is Ziemer.

DR. ANIGSTEIN: Oh, okay. I'm sorry. I lost you for a second.

MEMBER ZIEMER: And I'll just tell you. I've done a lot of studies on radon diffusion, and really --

(Simultaneous speaking.)

DR. ANIGSTEIN: Okay, now I --

MEMBER ZIEMER: And really, it really talks about the fraction that's available to even come out.

DR. ANIGSTEIN: I think that you said it right the first time. What happens is, let me just give you my understanding of it. What happens, my understanding is what happens is that the recoil, when the alpha is emitted, and you get small particles, the recoil kicks the radon out of the crane.

MEMBER ZIEMER: Some of the radon.

DR. ANIGSTEIN: Well again, some fraction of it, and it could be anything from --

MEMBER ZIEMER: A lot of it's not available in the air space.

(Simultaneous speaking.)

DR. ANIGSTEIN: Up to 58 percent.

MEMBER ZIEMER: Right.

DR. ANIGSTEIN: When they talk about the diffusion coefficient, it's not the movement of the radon within the grain, within the matrix. It's the movement of the radon through the air space, through the pores.

MEMBER ZIEMER: Well, my only point is that I don't think that 5,000 curies is available to get out.

DR. ANIGSTEIN: Well --

MEMBER ZIEMER: You see what I'm saying?

DR. ANIGSTEIN: I hear you, but I --

MEMBER ZIEMER: Yes. Well, think about that.

MR. STIVER: The lead-210 deficit is a real thing --

DR. ANIGSTEIN: Once you have an emanation coefficient of as much as 58 percent, then that amount goes into the air space.

MEMBER ZIEMER: Yes, okay. I'm all up with that.

DR. ANIGSTEIN: And it can either be removed by diffusion through the air or by convection.

MEMBER ZIEMER: Right.

DR. ANIGSTEIN: But it certainly can be the net of that quantity.

MEMBER ZIEMER: Right.

MR. STIVER: And it flows right. When you have an increase or a pressure differential that draws more of it out of the soil, you're not increasing the emanation; you're decreasing the concentration profile in the upper section of that silo. We saw that --

(Simultaneous speaking.)

MEMBER ZIEMER: It's pulling out what's available. It would come out anyway.

MR. STIVER: You could track radon concentration and barometric pressure just track each other perfectly.

MEMBER ZIEMER: Right, right.

MR. STIVER: And when you look at the concentration profile in the soil, you'd see a compensatory decrease in the upper layer.

MEMBER ZIEMER: Right.

MR. STIVER: You use kind of a simplistic wind dimension diffusion rate, and see the slope drop off.

MEMBER ZIEMER: I'm just trying to understand what we're looking at here.

CHAIRMAN CLAWSON: Well, this radon issue has gone on for a long time, and I really don't think it belongs in the SEC discussions. Perhaps it would be best to move it to the Site Profile discussion.

MEMBER ZIEMER: Well, and actually I was looking at my notes from last time, and we had agreed last time that it wasn't an SEC issue, according to my notes.

MR. STIVER: Well, I think it warrants pursuing, but you know, personally, we'd have the practical aspects of what Mark has talked about, how many cases are going to be imposed.

MEMBER SCHOFIELD: Correct me if I'm wrong, but my understanding was even if you went in and sampled the silo, depending on where in that silo you take that sample, it's going to vary.

MEMBER ZIEMER: Oh sure. I'm sure that would be the case.

MR. STIVER: Well, it's not going to

be completely homogeneous.

MEMBER SCHOFIELD: That's what I mean. It's not going to be a homogeneous sample. So if you just take a one graph sample there.

MR. STIVER: Yes. Well, they took multiple samples they describe.

DR. ANIGSTEIN: Well, they took nine. In Silo 1, they took nine samples, but basically they divided the silo into 12 portions, 12 regions. There was north, four compass directions, north, west, south, east, northeast and southwest, because there were four access holes that were located diagonally in the principal compass directions.

So that gave them four, and then they took them at three different depths, which they called A, B and C. So essentially they tried to sample from 12 regions. They didn't catch all of the 12 regions. They ended up with nine samples, but they were all from different regions. So they got nine out of the 12.

So there was, you know, it's certainly not a complete characterization, but you know, it's the best we've got.

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

MR. ROLFES: Yes. I just wrote up on our white board here well, I had the opportunity. I put up the concentrations of radium-226, lead-210 and polonium-210 in both Silos 1 and 2 at the Fernald site. If you take a look at the radium-226 concentrations for Silo 1, it's 477 nanocuries per gram, based on the analyses that were conducted, and 263 nanocuries per gram for Silo 2.

The lead-210 value of 202 nanocuries per gram and the Silo 2 concentration of 190 nanocuries per gram, we're concerned about the lead deficit, the lead-210, and so we've got the 202 and 190. However, the lead's still in there somewhere, for that measurements, you know, calls into question the measurement.

Because the polonium-210, which is the daughter product of lead-210, the polonium-210, which is a 138 day half life, those values of the progeny exceed the lead-210 values. You can't have that. You can't have more progeny than the parent radionuclide.

So clearly, that data, the values for polonium-210 for Silo 1 and 281 nanocuries per gram and 231 nanocuries per gram, to me makes it appear that the lead-210 is in there somewhere and contributing to the concentrations of polonium-210 that are produced within the silos.

DR. ANIGSTEIN: Can I ask a question? How can this be? I agree. I noted it, and also even if you use the polonium, even if you say the lead-210, which I don't concede by the way.

But even if we were to say let's ignore the lead-210 data for a second and just look at the polonium-210, you still have a deficit in regards to radium. Not as much, but it's still.

The average ratio of the polonium-210 to the lead-210 are to be expected. Again, it's not exact. The equilibrium concentration, it will be slightly higher. But not a lot, a few percent higher. It's only 33 percent. So the deficit would now be reduced.

So instead of 50 percent, it would be two-thirds or three-quarters of that. So we're still saying there is a large amount of radon that's unaccounted for, and why there would be a difference between the polonium and the radium, and the lead-210 is hard to explain, because unless there was some water movement, because certainly there's no gaseous phase, or it just may be some systemic error in the measurements.

Polonium-210 is an alpha emitter. Maybe they were measuring alpha activity, whereas lead-210 is a beta emitter. I don't know how they were measuring it.

MR. STIVER: What are the moisture contents in the silo contents? I think they numbered 30 percent.

MR. ROLFES: Roughly 60 percent, I think, for Silos 1 and 2. The method that was used to get the Silo 1 and 2 material in was the slurrying system, where they would dump drums and slurry the material into the silo, decant the water and then recycle that water to slurry additional K-65 materials in. The excess water, I think in there, was decanted after the silos were loaded.

MR. STIVER: Maybe there's the possibility that it was like entrained in the water at some point.

MR. ROLFES: That's very possible.

MR. STIVER: Gravitational settling.

MR. ROLFES: It's also possible that some processing at a different site such as Mallinckrodt removed the lead. So we're never going to answer, be able to answer that, because --

MR. STIVER: You had a homogeneous mix to begin with --

DR. ANIGSTEIN: Like I said, we assumed, the hypothesis I was explaining was we assumed either all the lead was there or none of the lead was there, and even if none of the lead was there, we still, according to one set of calculations, based on the assumptions about the density and about the mass of the raffinate in Silo 1, we still had 19, 1.9 million curies of radon emitted between 1953 and 1991. If we assume that it started out with all the lead in place, it wasn't that different, because the problem is, the reason is we're looking at a period from 1953 and 1991.

We're looking at a period of 48 years, and with the 22 year half life of the lead, whether it's there to begin with or not, it will grow into almost the same amount. So it's not a huge. It's more than two half lives. So whether it's there to begin with or not there to begin with is a smaller effect than might appear at first sight, if you think of over two half lives of ingrowth.

MR. KATZ: So Mark, do we have more response from DCAS at this point to discuss?

MR. ROLFES: Not at this time, since we've just received this latest, we received -- the SC&A report, was it yesterday I think it came over to us, which is a little four-page response to a couple of our points here. Yes. I mean we can certainly take a look at it, and see if we can provide any kind of additional information in response.

MR. STIVER: Okay. Well this -- well, I'm just saying this is part of the question of if it's an SEC issue or basically a Site Profile issue. If we can get our hands around it, then basically it's a Site Profile issue.

MR. KATZ: So it's something that the Work Group will address down the road as a

TBD issue?

CHAIRMAN CLAWSON: Yes.

MR. STIVER: It's a TBD issue, I think. So there's no action item at this point.

MR. KATZ: Well Mark, they are going to have to respond.

MR. STIVER: DCAS is going to have to respond.

MR. KATZ: They're going to have to respond to that.

MR. STIVER: Well, on our list, we have the construction coworker. What were we going to do on that, Mark?

MR. ROLFES: Well, what we had done, basically I think I had alluded to what we'd done previously. We were looking to see if there was any kind of adjustment factor, or if a separate intake approach was warranted for subcontractors. Because in our initial review of the HIS-20 database, we didn't think that any subcontractor data made it into HIS-20.

However, that's not necessarily the case. Beginning in December of 1985, Fernald did actually begin entering subcontractor data into HIS-20. So their information is in HIS-20, and was considered in the coworker intake model for uranium.

However, prior to 1985, subcontractor urinalysis data is in hard copy data. So what we've done is checked with the Department of Energy, to determine whether they have provided all of the hard copy urinalysis data to us. Then we have received a good sampling of it right now, since we had sampled to determine whether we had any concerns with the data being available to us.

We're waiting to hear back. I don't know. Maybe if -- I'm not sure if Mel or Bob or Gene might have anything else to add. But we're essentially waiting to hear back a response from the Department of Energy, as to whether there are additional hard copy files available for us, for subcontractors.

Anybody have any additional updates besides that?

MR. POTTER: Mark, this is Gene Potter calling in, and I think you've summarized it pretty well. We're looking for a response. We asked for subcontractor data initially. We think there's probably more subcontractor data out there that isn't necessarily identified as subcontractor data, because the subcontractors are just mixed in with everybody else. We've also asked for some contracting documents and that sort of thing, to see if we can get a handle of the number of subs that were working on site. That might be kind of a long shot, but we're going for that as well.

MR. ROLFES: Okay. Thank you, Gene.

CHAIRMAN CLAWSON: So basically, we still don't have anything yet. The last Work Group, I thought the two Work Groups, you were on the verge of creating this coworker, constructing a coworker model?

MR. ROLFES: Yes, correct. We've already got OTIB-78, but we've gone back and sampled, you know, portions of subcontractor results, and compared those to the total coworker intake model, based on HIS-20.

We haven't really gotten anything at this time to, you know, form -- we don't have a complete picture yet, and we need some additional data basically to, you know, get the best available picture, I guess, for subcontractors.

CHAIRMAN CLAWSON: So basically we don't have anything as of yet that we can look at?

MR. ROLFES: That's correct. This was a new issue that has been identified during the Work Group discussions. It wasn't something germane to the original SEC Petition. It's been something that we've taken on in our discussions.

It was first identified as an issue to us in, I believe it was in January 2010.

So we haven't been working on this issue quite as long as some of the other issues that were identified in the original Petition.

MR. DOLL: When you say "subcontractor," can you define that for me, what a subcontractor is?

MR. ROLFES: What we're referring to are not full-time employees. I mean this incorporates some of the subcontractors from places like Rust Engineering, some of the smaller. There were a lot of smaller painting operations like painting businesses, some smaller businesses that might have had employees that came onto the site for, you know, for a specific job, for maybe, you know, several months or a couple of years.

It's, you know, not an NLO proper employee, I guess is what I'm referring to.

MR. DOLL: Okay. Now you do know that Rust Engineering was both a prime and a sub during different periods?

MR. ROLFES: Yes.

MR. DOLL: When you say, what they call and what the Department of Energy liked to call those people was intermittent workers, as far as like cold war warriors and the rest of it. They went through this whole process with us, and finally the Department of Energy had to back off of it, because the data that they used was wrong.

They said that well, any of these construction workers was this or that was only there for a short period of time. But going back, I mean, the fellow that was here at the last meeting went in there for Rust Engineering in 1982 and left in 2005, with no break in service.

There were a lot of people during the 80s and the 90s, and even all the way up into 2000. I have 21 years there. I wouldn't exactly say that that was an intermittent worker.

MR. ROLFES: Sure.

MR. DOLL: If you just took people

that came in, and I guess a subcontractor could be a guy that came in the gate, to go over here and work on a high lift as a mechanic and then leave.

MR. ROLFES: Right.

MR. DOLL: You're not going to get, you know, any data out of this. I guess what I'm looking at is just like you defined the thorium workers before, if some of those guys would have had much higher uptakes, you're going to have construction workers in certain areas for certain contractors that had the same thing, Rust Engineering being one of them.

Because they worked on a daily, it was almost like a maintenance-type schedule, whereas the Davis-Bacon Service Contract Act construction in nature, went to Rust Engineering, and then they deployed people out into the field to get that job done. It might take three weeks, it might take a month. You might be in four different buildings in the same day.

But you were there every day, going into these different buildings inside the plant. The first one we went down there for was the pilot plant for the 6 to 4 project, lasted two and a half years. Okay. So we need to get --

MR. ROLFES: Could we clarify that area, because that was a new construction --

MR. DOLL: Building 13.

MR. ROLFES: That was new construction.

MR. DOLL: Well, it was demolition with new construction, because we had go in and tear stuff out, the old stuff. Then we not only did new construction, but we also stayed in there during when they started processing, because we had to make sure that the thing worked while they were making green salt.

There was only two people in there from the plant. One was Paul Savage and there was another guy named Evans. Both of those guys, by the way, died of lung cancer. But those were the two guys, the two operators assigned to the plant.

The rest of it was the construction people who put the process in order. When we first put it together and made the first batch of green salt in there, we had to make a lot of adjustments during the process, one of them being the cold traps didn't work.

So we had to go back in and demo the whole left side of that project, so we could put a big refrigeration skid in there, tear out the, you know, the "what you call it" houses, the bag houses and all the rest of the stuff, in order to put the equipment in.

Also, to do demolition on lines. There was still a handful of stuff, I guess, left over. We didn't know what it was. And then when they finally got the process to work, we still had to go back in there. We were talking earlier about the weigh tank. We had go in and fix welds on the HS system.

So we were constantly in that building over a two and a half year, three year period, until they shut it down, because they didn't need the green salt. They also shut the other project down, which was new since the 4 plant closed, which is now the AWWT.

MEMBER ZIEMER: So some of the subs may look like full-time regular workers, and some --

MR. DOLL: Well not only that, but we got a lot more exposure than a lot of the people that were down there with HIS-20. Now earlier, you made a comment. You said be careful using job titles to assign dose to certain groups of workers, and I think that's very true with a lot of this stuff here.

Now a lot of subs came on site, and they didn't have anything to do with the construction process. You know, some of them weren't -- I mean some of them were out there as surveyors. Anybody that wasn't an employee of National Lead of Ohio worked floor, or whoever the prime was was considered a sub.

So when you go through this process and you start assigning doses and using, which is, I guess my point being with the individual John Doe that we talked about earlier, of having more dose from '93 on than he did prior to working in this building, because he was in that building for 2-1/2 years.

Plus 9. We demoed 9 out to put in a process to make jewels, to make glass, and we had to demo the lines and everything out of there to put the process in. They were still, the lines that we cut out, six-inch, eight-inch lines were still full of product from when they had shut it down in 1970-something. A lot of exposure.

CHAIRMAN CLAWSON: But your comment that John Doe was -- the years that they had no data for him, were up to, what was it?

MR. DOLL: Well, the letter stated that he got -- the letter from NIOSH stated on his dose reconstruction, stated that he got more dose from 1993 until 2005.

CHAIRMAN CLAWSON: Which he was monitored for.

MR. DOLL: Which he was monitored for.

CHAIRMAN CLAWSON: Than the years --

MR. DOLL: Than he did -- now this is a superintendent working in a trailer and stuff, walking around when most of the place was clean, than he did working out in the plant as a pipefitter, during the years 1982 to 1992. Now that may be true, but it doesn't make a lot of sense.

MEMBER ZIEMER: Well, I can see how that could happen in the systems, because if you have dose records, you tend to end up with a lower dose than when you don't, because the assumptions made of maximizing dose --

MR. STIVER: But that's just the

opposite.

MEMBER ZIEMER: No. He said when he worked, didn't work in the restricted areas, he got higher dose.

MR. DOLL: But there are no records.

MEMBER ZIEMER: Yes, and where there's no records, I'm saying that they often tend to assign higher doses than some of the workers, because they use this maximizing. But we can't deal with that specifically, but I could see it happening.

(Simultaneous speaking.)

MR. STIVER: Well, yes. But my understanding of this was that in the years when he was out there working in the field, he had no monitoring data. When he went in as a supervisor inside the trailer, and no longer really worked out in the field, he had monitoring data.

Then in his monitoring, he got more dose from the time he became a supervisor with monitoring data, then he did before when he was actually working in the field.

MS. LIN: Brad, everybody else here, and this is really specific personal information at this point. I know because we're trained, but if we can just --

MEMBER ZIEMER: Yes. We shouldn't talk about that case.

MR. DOLL: Okay. But I guess the bottom line to it was is we didn't have urinalysis, or we did have urinalysis. And when we had the urinalysis, I got urinalysis probably over maybe once a year or something like that, and I'm not sure that they have that.

But mine was only for chemicals, which wouldn't -- I mean if I went in there and okay. We went in. I said before, we had an HF leak. When we go in there, if we had a HF leak, you had to go drop a urine sample. It wasn't about, it wasn't about the uranium or anything else. We had no air monitoring. In fact, you couldn't even get a rad tech down there.

So some of this data that you might have might not even be related to testing the individual workers for uranium or radiological-type exposures.

MR. ROLFES: I don't know if I'm allowed to speak in response to your statement about no urinalyses. I mean I don't know. Can I ask him if I can discuss his specifics? We'd you prefer not to?

MS. LIN: Yes. I would prefer not to.

MR. ROLFES: In general in the past, I guess I'll put it this way, in the past we've had employees that have been concerned that they were not monitored for uranium exposures, and I've pointed out a few people that they can submit a FOIA request, for example, for that information.

We have actually pulled out some information to show them that yes, in fact they were monitored. So those are options for people to determine whether or not they were in fact monitored. They can submit a FOIA request to NIOSH or to DOE, and we can certainly coordinate to provide that information to you in response.

MR. DOLL: I just got my FOIA request back last week. I've got a stack of papers about this big, and I've started to go through them. So I'll make sure that I get that information to you.

MR. ROLFES: Okay. If you have questions, I think you have my number from last meeting.

MR. DOLL: I've got your number.

(Simultaneous speaking.)

MEMBER ZIEMER: He changed his number.

MR. DOLL: Thank you.

CHAIRMAN CLAWSON: Also, last Work Group meeting, there was a draft White Paper by Bob Anigstein, Evaluation of Occupational Environmental Exposure to Radon on Fernald, Environmental Management --

MR. STIVER: It's evaluating the dispersion model, and that one's another one we haven't gotten a response back on.

MR. ROLFES: Okay. The radon approach that we're using is documented in Report 52 here as well.

MR. STIVER: All right, but if we could provide a response to Bob's previous write-up.

MR. ROLFES: We can respond, again, if that's what you would like for us to do. But you know, to try to explain, you know, when we've got the data that indicate that the lead-210 is still in there, because polonium-210 is being produced.

I mean for us to really go back and look at something, and I think we've alluded to this previously. We're using the Pinney study, which considers the radon source term from the K-65 silos, as well as the materials in process from the Q11 ore silos at the

Fernald site.

So we've got individualized exposure estimates based upon the results of the RAC study, the NAS review of the RAC study, and also independent analyses of the uncertainties of the radon releases. Furthermore, we also did contain code calculations, which basically support a lower quantity of radon than what was found in the RAC report releases.

You know, I don't think we can get any closer to, you know, I mean do we --

(Simultaneous speaking.)

MR. ROLFES: --to review something that's been reviewed by the National Academy of Sciences.

CHAIRMAN CLAWSON: Now wait a minute, wait a minute. I don't want to talk about the Academy of Science. You'd better read the report very good. At the very beginning, what it states, that this is not to be used for anything else. That was cursory report. You originally brought that up to us, that we're not going to do the Pinney; we're going to do the National Academy of Science.

After reading that report, read that very carefully, because it does not support. It just went over what it was asked to be, basically do, and it can't be used for anything else. It's just there.

MR. ROLFES: Well, you know, I'm just basically pointing out that, you know, do we really want to call into question, you know, independent universities that have analyzed the data, an independent contractor that's analyzed the data, and you know, do we really want to rework a model that's already been designed as a claimant-favorable model, basically, for historical dose estimates for radon?

MR. STIVER: If we can go back just for a minute to Bob's previous paper, I think it has important insights into the dispersion model for transporting the radon from the silos to areas where the Pinney report calculates, just based on our views on the window data. So anyway, I think it's relevant to the discussion, and I feel it would be good for you guys to be able to respond to that.

But our position has been laid out in Hans Behling's two papers, and Bob's report. I think we have a pretty solid case.

MR. KATZ: I'm so confused, because I thought there's a four-page, right, a four-pager. Is that what we're talking about, or are we talking about a separate --

MR. STIVER: There are actually two different ones. There's the one that Bob just turned in yesterday, which is --

(Simultaneous speaking.)

MR. KATZ: And they want to respond to that. They already said they would respond to that.

MR. STIVER: There was also in a previous report that Bob Anigstein put out --

(Simultaneous speaking.)

MR. KATZ: You see, I thought the

four-pager with a follow-on to the prior report, after a response from DCAS.

MR. STIVER: No, we haven't had a response from DCAS.

MR. KATZ: Okay, got it. Thank you. Now I understand it, at least.

MR. STIVER: Tie up the loose ends here.

MEMBER ZIEMER: Well, their response may be that they're sticking with the model. I think that's what I'm hearing.

MR. STIVER: That's basically what it's been.

CHAIRMAN CLAWSON: That's basically what they're saying, so that's a response to them.

MR. STIVER: So that can go to the, take it to the Site Profile and we'll hash it out there.

MR. ROLFES: Our current response is contained in Report 52. That's the most recent and available response. So that's where we stand at the time still.

MR. STIVER: Okay. We agree to disagree.

CHAIRMAN CLAWSON: So do we have any other --

MR. STIVER: I just want to go through the listed action items that I've got, just to make sure that we have everything documented. As far as I know, we have no action item regarding recycled uranium at this point?

MR. KATZ: That's correct.

MR. STIVER: Okay. Issue 6B, which was the NIOSH had an action item to provide the Y-12 calibration data when that becomes available. Bob Barton is going to produce a formal White Paper.

K-65, we need a NIOSH response. Both of Bob Anigstein's papers, this one and the one that was produced earlier that we mentioned, and I believe that's it, unless --

MR. KATZ: You already mentioned the

calibration data?

MR. STIVER: The calibration data.

MR. KATZ: Right, so that's it.

CHAIRMAN CLAWSON: And now I guess we come back to the construction coworker model. We don't have anything there, was my understanding.

MR. KATZ: So they're working on that, is what I heard.

CHAIRMAN CLAWSON: Yes, last while there.

MR. STIVER: It's still in process.

MR. KATZ: So do you want to talk then, are you through -- you're through all the issues, right?

MR. STIVER: Right.

MR. KATZ: You want to talk a little bit about how you're going to divvy up reporting out to the Board on the first day of the meeting. Thank you.

MR. STIVER: Thanks for contributing.

CHAIRMAN CLAWSON: Well, I guess this is a new experience, so I guess I need to bring forth, you know, where we're at. The biggest thing we've got is recycled uranium, coworker and so forth. I guess just present it, what you --

MR. KATZ: So well there's -- I mean there's been a lot of discussion here -- sure.

MR. STIVER: Well, I talked to John a little earlier, and it might be good for us to give SC&A's position on where things stand from a technical standpoint, and it wouldn't necessarily may or may not influence the Board's decision.

MR. KATZ: Sure, so I mean you can support, but Brad and Paul may want also to present, as sort of either an overview or just on particular points. It's however this Work Group wants to report out.

MEMBER ZIEMER: Well, it seems to me that Brad can report that we have this recommendation from the Work Group. I'm assume it's going to be a 3-2.

MR. KATZ: We hope, yes.

MEMBER ZIEMER: Well, it is. It's a recommendation, but you also in fairness, you tell them that it was a split vote, right.

MR. KATZ: Yes.

MEMBER ZIEMER: And then it seems to me it would make sense for NIOSH to present their approach to the RU issue, and SC&A present their concerns. Now part of this had to do with timeliness also, and Brad, you probably want to speak to that, because I thought, aside from the 400 boxes or whatever that probably we're not going to look at, I thought that both SC&A and NIOSH were pretty close on the other issues.

But and, and we should be aware, going forward, if we go the SEC route, they can't use that for dose reconstruction for the rest of the cancers. That means that all of those people lose a big, big chunk of their dose.

MR. STIVER: That certainly has consequences for the non-compensable --

MEMBER ZIEMER: Right, and the non-compensable --

MS. LIN: Or even people who need a Class Definition generally.

MEMBER ZIEMER: Right. The 250 days are the wrong cancer. They cannot use that method. The recycling uranium is off the board. I think the Board Members need to know that in fairness, because when you make the decision, there's downsides both ways. If you don't go with the SEC, then there's people get left out on compensables. If you go the other way, there's some people that are going to be left out.

But the decision shouldn't be based on that particularly, but --

MR. STIVER: Based on the technical merits of the methodologies.

MR. KATZ: Yes, yes. Let me just, some clarification about timeliness. This is not a -- timeliness is not a criterion that the Board can use. The Board is free to make its decision when it feels like it has enough information to make a decision, make a recommendation. That's what we're talking about.

There is a time limits factor in the regs. That time on this factor has to do with the head of DCAS saying, at some point, enough is -- it's no longer practical to obtain these records and hence, I'm going to sort of call it and say the records are what they are, as we have them now.

But that's really distinct from what we're talking about here. Here, we're talking about at some point a Work Group decides it's investigated the issue enough, and hence, in this sense, with due diligence, it's time to report out. I just want to be clear that that's what we're saying.

We're not saying that because this has taken so long, now we're making the recommendation.

MR. STIVER: Thanks for clarifying that, because I wasn't aware of whether or not that that was a valid criterion for SEC.

MR. KATZ: Yes, it is not.

MEMBER ZIEMER: That we can make.

MR. KATZ: Right.

MEMBER ZIEMER: Or the Board can make?

MR. KATZ: Right, the Board does not --

MR. STIVER: Well, I think basically that the public needs to realize that up front. You know, you said that we need to say into this. Well, we ought to tell them there is no time restraints. We could go on for 15 years, right. We've got a lot of projects that are still ongoing out there.

But the problem is, like you say Ted, which is correct, we haven't moved anywhere. We haven't done anything for quite a while. A little bit here, back in some places. We haven't moved. So you know, great. We can't do it on timeliness, but -- and to your question, Paul, they can't use any data on that.

Is there something in the regulations that says that they can do partial doses too?

MEMBER ZIEMER: Oh, they do partials. They'll do partials, but if we tell them that that is not an acceptable way of doing dose reconstruction for recycled uranium, I mean that's basically what we're saying. We're saying -- they will be saying that we cannot reconstruct recycled uranium doses, I think is what --

MR. KATZ: That's what the Board would say.

MR. STIVER: You would not be able to use that data set and that methodology to reconstruct the doses.

MR. KATZ: That's correct.

(Simultaneous speaking.)

MR. STIVER: The definition, the legal definition is you cannot reconstruct the doses.

MR. ROLFES: That's correct. Keep in mind that sometimes the NIOSH-proposed dose reconstruction methods resulted in a higher number of Probability of Causations greater than 50 percent than the SEC compensations. So you know, that's something to keep in mind.

MR. STIVER: That's the non-compensables and the 250 day. People are not going to meet that criteria. I've seen quite a few of them be left in the lurch as a result of the unforeseen consequences.

MR. KATZ: Yes, but I mean I agree with what Paul said, which is at least Paul, that these decisions should be made on the merits, not on the consequences, at the end of the day.

(Simultaneous speaking.)

MR. STIVER: I think we're making a lot of progress on the RU issue, and personally I think it's possibly tractable. I would hate to see it go down. That's my personal opinion for the record here. I think that the legal approach has some merit.

MR. KATZ: I think part of Brad's sense too is that it's good to engage the Board on this at this point. I mean, as Brad says, having gone around and made incremental progress, but it being slow-going with the Work Group.

MEMBER ZIEMER: This one is -- this is a complex site in a way. I mean in some regards it looks straightforward, but the issues have been complex, and I actually will be surprised if the Board will be willing to actually take action. They may want to postpone, because even the recycled uranium issue is fairly complex. I mean we've been immersed in it, but --

CHAIRMAN CLAWSON: Well, this is what I want to bring to the Board.

MEMBER ZIEMER: But they may want to

hear it and say okay, we need to cogitate on this for another --

MR. STIVER: Before we can make a decision or, you know, resolve these issues.

MS. LIN: We also have potential compensation, which is that the proposed Class, which is not a definition at this point, ranged from 1953 to 1985, and it will be great if the Chair of the Work Group or SEC technical presentation, with applicable discussion as to why that period of time is justified.

CHAIRMAN CLAWSON: Okay.

MR. KATZ: Right. Maybe that's something SC&A can do, just distinguish the time period and rationales as they relate to --

MR. STIVER: There are a lot of different periods.

MR. KATZ: Well, whatever time periods might be substantially different. If there weren't substantial differences, then

you would lump it all together.
MR. STIVER: In your discussion, just as regards to the SEC period or dose reconstruction potential?
MR. KATZ: Yes. Well they're two sides of the same coin.
MR. STIVER: I think one's a little different than the other.
MR. KATZ: Yes.
MR. STIVER: I think mainly what we're looking at is that `53 and `61, right?
CHAIRMAN CLAWSON: Great. Then I guess we'll proceed on with that path, and then we'll go from there. Anything else that needs to be brought up before the Work Group?
MR. STIVER: The meeting's on the 25th?
CHAIRMAN CLAWSON: Well, 24th in St. Louis. This will be the first date.
MR. KATZ: I assume you don't want to schedule another Work Group meeting at this point, given --

CHAIRMAN CLAWSON: I don't think we have any --

MR. KATZ: No, no. Not one before May. I'm just talking about next week. You might as well have the, learn what we learn from the Board meeting.

MR. STIVER: Probably not too far after, then, I think, while it's fresh in everybody's mind.

MR. KATZ: Yes, okay.

CHAIRMAN CLAWSON: You know, I think my biggest thing is, you know, I think how did Sam Glover put it, we've come to a loggerhead on this, and what I think we need to start involving the whole Board in it because this is a complicated site.

MR. STIVER: It also has ramifications beyond Fernald, for other sites that handled recycled uranium.

CHAIRMAN CLAWSON: Right, so okay.

MR. KATZ: So are we adjourned?

CHAIRMAN CLAWSON: We're adjourned.

MR. KATZ: We're adjourned. Thank you everyone for your hard work, as well as everyone on the line. Have a good day.

(Whereupon, at 4:43 p.m., the meeting was adjourned.)