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

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FERNALD WORK GROUP

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THURSDAY,
DECEMBER 4, 2014

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The Work Group meeting convened in the London Room of the Cincinnati Airport Marriott Hotel, 2395 Progress Drive, Hebron, Kentucky, at 9:00 a.m., Brad Clawson, Chairman, presiding.

PRESENT:

BRADLEY P. CLAWSON, Chairman
PAUL L. ZIEMER, Member

ALSO PRESENT:

TED KATZ, Designated Federal Official
MATT ARNO, ORAU Team*
RAY BEATTY*
HANS BEHLING, SC&A*
NANCY CHALMERS, ORAU Team*
MARK FISHBURN, ORAU Team*
STU HINNEFELD, DCAS
KAREN KENT, ORAU Team*
JOSH KINMAN, DCAS*
TOM LABONE, ORAU Team*
JENNY LIN, HHS*
MARK ROLFES, DCAS
BOB BARTON, SC&A
JOYCE LIPSZTEIN, SC&A*
MATTHEW SMITH, ORAU Team*
JOHN STIVER, SC&A

*Present via telephone

T-A-B-L-E O-F C-O-N-T-E-N-T-S

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

(9:01 a.m.)

MR. KATZ: Okay. Good morning. This is the Advisory Board on Radiation Worker Health, Fernald Work Group. We're ready to go here.

Just a few notes: The materials for today should be all on the NIOSH website, the agenda and the materials that we're discussing, including the presentation from SC&A. You'll find them on the NIOSH website under the DCAS portion, the Advisory Board, today's meetings. And if you go to today's meetings, those documents should all be there. You can just open them up.

And we're speaking about a site, so, please, everyone, in going through roll call, address conflict of interest as well. And let's begin with that.

I have a note that Mark Griffon,

who’s a Member, will be joining us a little bit late, maybe around 9:30, but let's go with roll call starting with the Board in the room.

(Roll call.)

MR. KATZ: Okay, then. Well, we'll probably, as I said, have Mark Griffon join us a little late. He doesn't have a conflict with respect to what we're addressing at Fernald.

And we're ready. So, please, everyone on the line mute your phones except when you're addressing the group. If you don't have a mute button, press *6 to mute your phone and then press *6 again to take your phone off of mute. Much thanks.

And, Brad, it's your meeting.

CHAIRMAN CLAWSON: Okay. We do have a couple White Papers that were issued by NIOSH, one for the K65 silo -- well, actually two of them, one by Stu Hinnefeld -- both of them by Stu Hinnefeld. There's an addendum. Just want to make sure that people have those before we speak to them.

John, I'll let you take it over from here and we'll start out.

MR. STIVER: Okay. Thanks, Brad. Those of you on Live Meeting, I've got the agenda pulled up and the way I've kind of envisioned this thing going. The first thing I want to talk about is our review of NIOSH's White Paper titled, "Review of Proposed NIOSH Methods for Reconstructing Thorium Doses at Fernald from 1979 to 2006." This is the post-SEC thorium methodology that was released, I believe, back in June of this year.

We finished up our review early in November and delivered it to the Work Group, I believe, on the 17th. It might have been maybe a little later than that.

But, anyway, Bob Barton has got a

presentation on that and we'll probably go ahead and lead off with that. After that discussion is finished, I believe we can go ahead and continue on the issues matrix resolution, of which the K65 silos is one of the open issues, I believe Number 25. And there are about -- I counted up about 11 open issues that we can discuss today.

And so, with that, Bob, you want to take over?

MR. BARTON: Yes, thanks, John. Well, I think it's probably best to give everyone sort of a refresher on what the proposed methods for reconstructing thorium intakes actually are during this period. So I have a couple slides on it, but do you guys want to do a little summary on it? Or I can just go --

MR. HINNEFELD: Well, I'll give just a little historical aspect of the site at this time. In other words, the first SEC at Fernald for all workers extends through '78 now, right? '78?

MR. STIVER: '78.

MR. HINNEFELD: The basis for that mainly being that for the '68 to '79 period was that the in vivo monitoring results for that period were reported in milligrams of thorium and there's just not a consistently convincing way to determine what that means in terms of radioactive constituents. And so that Class was added up through '78.

Now, in '79 -- I think it was '79, or '78, one of those years -- the mobile counter results began to be reported on the constituent daughter product radionuclides that you can count with a gamma counter, with a -- it shows up as a lead-212 and I think there’s a actinium-228 or something. So we feel like those results now are sufficient. We can interpret those results.

We also have access to all of the in vivo counts that were performed, the results of all of the in vivo counts that were performed with the mobile counter. So that's the entirety of the in vivo counts. Later on, we only have in vivo counts for claims. Our paper goes into quite a lot of detail about the construction of, the analysis of the data, the construction of what it looks like. That coworker model is essentially, I believe, done, I think.

At Fernald at this time, '79 was essentially the end of any thorium processing. And from that point forward, thorium existed in storage in warehouses and in some bins and things like that. You call them bins. Sometimes they call them silos. And there was some thorium solution, thorium nitrate solution in large tanks. And so there was not any really routine exposure, internal exposure to thorium, with the possible exception of some overpacking of drums that would deteriorate.

Some of the materials were stored in very -- it was a high-quality product and they were stored in containers that were in good shape decades later. They were in really good shape decades later. Some of the drums, the material had a heavy moisture content and it was kind of corrosive to the drums. So some of the materials corroded the drums and those drums on occasion would have to be re-packed.

At this point, you can't really tell who was engaged in those overpacking operations, and so our approach is to provide some sort of bounding estimate for dose reconstruction during that time. I think maybe Bob's going to -- you're going to cover kind of what the approach is?

MR. BARTON: Sure. I can do that,

yeah.

MR. HINNEFELD: Okay. So, anyway, that's kind of the setting here for what the thorium was.

And then ultimately there were a series of remediation tasks, when the site was in remediation, where these thorium materials had to be removed from storage, placed in suitable containers, if they weren't already, and then dispositioned somewhere. If there was good product material, somebody may have wanted them. Really at the time hardly anybody wanted thorium anymore, so I think the vast majority of it was disposed of as waste in various ways.

A couple of those remediation tasks were subcontracted tasks. Like the disposition of the thorium nitrate was a subcontracted task. And removal of the thorium from Plant 8 -- they're either called silos or bins. Sometimes they'd use one word; sometimes they'd use another. That was a subcontractor task as well. So all those removals, all those remediations, are also described in the paper we wrote about how these materials were removed.

So there's a period of time from about 1988 when the mobile counter was replaced by the fixed counter, the fixed in vivo counter. From that point forward, we don't necessarily have every in vivo result in our records. We have the in vivo results from claims from that point forward.

The in vivo results, all this time, from '79 on, people were not in vivo'd because they were thorium exposed. They were in vivo'd because of potential uranium exposure. But the in vivo counters spit out thorium results anyway. So if there was a thorium intake from one of those overpacking operations, it should show up in that person's in vivo record.

Let's see, what else did I want to say about this? At some point somewhere in the '90s, the site adopted a 100 percent BZ sampling requirement for thorium. And so I believe we chose '95 as the start date when there seemed to be a really robust set of thorium in vivo -- or thorium BZ air monitoring data. From that point forward, the method that we're proposing is to use the BZ sampling record as the record of the exposure.

And in that interim period, our original proposal was to use some fraction of the exposure standard. Again, there's evidence that these projects then were controlled by air sampling. The people who went in wore respiratory protection and so shouldn't have exceeded like 10 percent of the airborne standard during the work.

And then we also, I think, started

to investigate the possibility of do we have enough in vivo data from claim files to build a coworker model. And would that be a suitable model, since we don't have all? We only have claimants. So I think we started some work on that.

Anyway, I think maybe I'll be quiet and let people who know more about it talk more than me.

MR. BARTON: Okay. Thanks, Stu.

We'll go over sort of the proposed methods for dose reconstruction first and then we'll sort of talk about each one in turn.

Again, for those of you have access to Live Meeting, the presentation is up right now. For those of you don't, that presentation is also on the website, as Ted pointed out.

MEMBER ZIEMER: Before you start --

MR. BARTON: Yeah.

MEMBER ZIEMER: -- I was just looking at my emails from Zaida. She didn't send me the connecting thing for this meeting.

MR. KATZ: I'm going to send it to you right now.

CHAIRMAN CLAWSON: Would you send it to me, too, Ted?

MR. KATZ: Yeah.

MR. BARTON: Okay. I guess I'll talk slowly until --

(Laughter)

MR. KATZ: No, that's all right. That’s fine

MR. BARTON: Basically, as Stu sort of mentioned, the --

MEMBER ZIEMER: Excuse me, do you have something different than what's on the meeting papers?

MR. HINNEFELD: No, this is exactly --

(Simultaneous speaking.)

MEMBER ZIEMER: Is it the one that was on the meeting papers?

MR. KATZ: Yes.

MEMBER ZIEMER: On the regular page?

MR. HINNEFELD: On our website.

MEMBER ZIEMER: On the website? So I can pull it up there.

MR. KATZ: Yeah, I'm going to forward it to you. I've just got to do one thing to be able to do that.

MR. BARTON: Okay. Anyway, the DR methods for internal thorium can really be effectively split into three periods. You have the 1979 to 1989 period, which uses the mobile counter in vivo data. Then you have the 1990 to 1994 period where, for unmonitored doses, the proposal is some fraction of the derived air concentration at the time. And then the third period is from 1995 to 2006, where the breathing zone results for workers, which are contained in the HIS-20 and also for claimants in the individual claim files from DoE are contained.

These periods, as Stu kind of mentioned, are sort of delineated by what methods are being employed, whether it was the mobile counter, the derived air concentration of breathing zone, the availability of the data. As Stu mentioned, from 1990 to 1994, when they had the fixed counter, all you really had were claimant records, so we didn't have a full monitored population there with which to really build a coworker model. And then in the later period, you do again have in vivo results, but only for claimants, but you also have this fairly robust breathing zone program, which we will get into.

The methods themselves and how

they're going to be applied are on pages 12 and 13 of NIOSH's White Paper on the approach to reconstructing thorium doses, which we're going to take a look at right now.

So we see with this table -- I said three periods. It's sort of four periods based on whether you have in vivo data for the claimants. But that first line in the table you see here, 1979 to 1994, if in vivo exists, then you're obviously going to use that data for the individual claimant.

From 1979 to 1989, if in vivo doesn't exist for a claimant, that's when we use the coworker model based on the mobile counter results.

Again, in 1990 to 1994, if you don't have in vivo data, then this is where the fraction, the 10 percent of the thorium, its Class W-derived air concentration would be used for unmonitored workers.

And then this final period here, 1995 to 2006, if you have in vivo, you're sort of left with a choice. You can evaluate it. If it was a positive result, you'd either then have to decide whether that was from a previous exposure, possibly from the earlier period in the 1980s. If it's not, if there's evidence that that positive lung burden occurred in the 1995 to 2006 period, you would definitely use that in vivo result, but otherwise the breathing zone data is considered the data of choice to use.

So I'm going to talk a little bit about the selection of what's considered a thorium worker during this first period from 1979 to 1989. And the NIOSH White Paper indicates essentially seven job types. And I'll just read them off here. You have chemical operators, fork truck drivers, laborers, transportation laborers, operations, production workers and maintenance personnel. And we discussed this a bit at the last meeting in September. I'm going to quote Stu here. He's a very quotable guy. To quote, "And we'd be pretty encompassing about that. You figure almost anybody in operations could have done that. Most anybody in maintenance. Transportation could have been involved in it. You have safety and health people. Might have security people there. So you've got to be pretty inclusive."

Now, in the second period, from 1990 to 1994, when it's proposed to use the 10 percent of the derived air concentration values, the selection of workers for which you would assign unmonitored thorium doses is as follows from the White Paper: "From 1990 to 1994, thorium workers with no in vivo results or with pre-job fecal sample results during this employment period are recommended to be assigned a dose." So, essentially, based on the proposed methodology, you have to have that pre-job fecal sample to be considered for an unmonitored thorium dose.

And then unmonitored workers in that third period, from 1995 to 2006, would not be assigned any coworker dose.

Also, the NIOSH White Paper provides methods for calculating thoron exposure to thorium-related activities. And as you can see on this slide here, there's essentially three time periods that were considered and three sort of areas and/or activities that you would consider. And those values are given in working level months per year.

And, again, we're going to get into our review topics on each of these facets. I just want to lay out what the DR methods are that are currently proposed.

As far as the thoron approach, the White Paper doesn't necessarily specify who would be assigned thoron. As you saw on this previous table, they do give an area of the plant. Storage facilities and repackaging are a number of places. And then closure and various storage activities. Again, that's sort of all of the plant. And then you have in 1979, pretty much for the period of interest, the pilot plant.

However, the White Paper does state, and I quote, "The dates and bounding levels of calculated potential exposures represent recorded operational history. However, thorium was present on site for most of its history. For unknown work locations and time periods of concern, dose reconstructors should assume that thoron exposure potential existed as a claimant-favorable assumption and assign thoron doses based on the guidance from the table," which we just say on the previous slide.

So I pretty much take that to mean, if you don't really know where the workers were, and as we know at Fernald it’s very difficult to place workers in specific locations, that the benefit of the doubt goes to the claimant and they would be assigned that thoron dose.

So, next we're going to look at that first period, 1979 to 1989, for which we're using the mobile in vivo data to construct a coworker model. And as you see in front of you, this is a completeness evaluation of that data set, which we actually performed a couple of years ago, but we should go over that here so it's fresh in everybody's mind. What we're looking at is the number of in vivo samples we had per year. As you can see, it's 1979 through 1988, even though this period includes 1989. Essentially, 1989 was extrapolated based on the previous data in the 1980s.

One interesting trend that you can look at here is that from '79 to about 1983 you have between 100 and 200 samples. And then in 1984, it sort of spikes up over 300, then over 400. This was interesting to me because that was a similar trend that we saw in the overall uranium bioassay program and it sort of coincided with the transition from National Lead over to Westinghouse.

One thing we looked at is how these in vivo data broke down by job title. And these job titles that you're looking at here are ranked by the total number of samples available in the data set. And as we can see, 55 percent of the actual in vivo samples that we have are associated with chemical operators, which certainly we would consider to be one of the higher risk job types. And if you look over at the actual magnitude of the results, at the 95th percentile, the chemical operators also had the highest results.

MEMBER ZIEMER: All right. Let me ask you a question here, because I was puzzled on this slide. I thought that these things should add up to 100 percent. What am I missing here? They're way over 100 percent.

MR. BARTON: How -- I apologize.

MEMBER ZIEMER: The percent of the totals. I went through them yesterday.

MR. BARTON: I would assume it's an rounding error, but --

MEMBER ZIEMER: The first two is 80. We got 90 -- about 99 percent --

MR. BARTON: Are we looking at --

MEMBER ZIEMER: -- with the first four. There's another 10 percent. It adds about to about 125 percent.

MR. BARTON: -- 91 percent. We'll have to go back and look at that. There must be some sort of rounding error, but I'm not --

(Simultaneous speaking.)

MEMBER ZIEMER: It looks like more than a rounding error to me.

MR. BARTON: Okay. Well, I think the point is --

MEMBER ZIEMER: Does something include something else there, like --

MR. STIVER: It might have been multiple job types.

MEMBER ZIEMER: I mean, like are mill workers part of construction trades?

MR. STIVER: Yeah, it might be double-counted.

MR. HINNEFELD: I think there are probably some job categories --

MEMBER ZIEMER: Yeah, that's what I was --

MR. BARTON: We might have broken them out, you know, into industrial truck operator and that somehow is still counted as construction trade.

MEMBER ZIEMER: That's what I sort of figured, but it wasn't clear to me why it was --

MR. BARTON: Yeah, and I can certainly get to the bottom of that during the break.

But I guess the point here is that we have a monitoring program that, when you look at the job types that were monitored most frequently, the chemical operators also had the highest results that we observed. And part of that is probably an artifact that the chemical operators would have been involved in the pilot plant operations, which was the final production operation for thorium. And it occurred in 1979. So you would expect that's where your exposure potential would be highest.

I guess what I'd take away from this

slide is that you don't have -- one of the things we always look for with completeness analysis is, does it look like there's a job that had high exposure potential but that was systematically excluded? That's one of the criteria that almost immediately calls a coworker model into question. And I would argue based on what we see here that almost the opposite appears to be true, that the monitoring program was in fact sort of geared toward those higher risk job types.

A big portion of this is the unknown sort of job titles, and that's either because the job title was just not included on the original bioassay card, it was blank or illegible. So that could be a wide spectrum. So just because that's high up on this list doesn't necessarily indicate that you're missing some of those job categories. You could have all sorts of different jobs in there, a full spectrum. You could have ones that were exposed and ones that weren't exposed mixed together. So that's why perhaps the magnitude is not has high as you would expect.

MEMBER ZIEMER: Another question, Bob.

MR. BARTON: Yeah?

MEMBER ZIEMER: This is not directly on the slide, but it has bearing on it. Can you remind me, on thoron, which is another isotope of radon, is the working level month defined in an analogous way to radon-220? And if you don't have equilibrium, how is it defined, the working level month for thoron? Defined in terms of dose or --

MR. STIVER: No, it's in terms of potential alpha energy exposure.

MEMBER ZIEMER: Okay.

MR. STIVER: The main reason being because there is that disequilibrium and that's --

MEMBER ZIEMER: But is the working level month for thoron the same amount of alpha energy as the working level month for radon? That's what I'm trying to get at. Or is it based on activity?

MR. ROLFES: This is Mark. One working level of thoron, I believe, is 7.1 picocuries and 100 percent equilibrium.

MR. HINNEFELD: To your question, yes.

MEMBER ZIEMER: Yeah, it assumes equilibrium, but if you don't have equilibrium, then you just go by total alpha energy? Because a lot of the times you don't have equilibrium. So if you're expressing working level months, are you just saying, okay, if we have the same amount of alpha energy for this ratio, it's still a working level month or --

MR. STIVER: Yeah, I think the

equilibrium ratio may be a little bit different for thoron than it would be by virtue it's a short decay time.

MEMBER ZIEMER: Yeah, different alphas and different --

MR. STIVER: Yeah.

MEMBER ZIEMER: Okay.

MR. HINNEFELD: Yeah, if I recall, working level month measurement is typically an alpha count, a particulate that was counted multiple times, at least two times.

MEMBER ZIEMER: Okay. It didn't matter what the equilibrium was, you just --

MR. HINNEFELD: And what would happen was the extent of difference between the two alpha counts would give you some information about the equilibrium.

MEMBER ZIEMER: Right. You took them --

MR. HINNEFELD: And so there's an

adjustment for that.

MEMBER ZIEMER: -- with a time lapse like you would for radon?

MR. HINNEFELD: Yes.

MEMBER ZIEMER: Okay.

MR. HINNEFELD: And then, as I recall, it's done the same way. And so you get some information. If you're taking actual working level month measurement, then you get information about the disequilibrium from the way you take the sample. If you're taking a radon measurement and saying, well, for this much radon we're going to use a 70 percent equilibrium or a 50 percent equilibrium, I don't know the answer to that one.

DR. LIPSZTEIN: May I --

MEMBER ZIEMER: Joyce will clear it up.

DR. LIPSZTEIN: I think that when calculating that table, NIOSH on the White

Paper assumes some equilibrium fraction.

MEMBER ZIEMER: Thank you, Joyce.

DR. LIPSZTEIN: When you go to Appendix F on the Paper you'll see that some equilibrium fractions were assumed. And actually this was, I think, one of the problems with the numbers that were found.

MEMBER ZIEMER: Right, and the reason the ratios between actinium and lead are so different in a couple cases is what, then?

MR. BARTON: Well, again, these aren't individual workers. These are sort of the 95th percentile of --

MEMBER ZIEMER: It's a distribution --

MR. BARTON: But we're also going to be talking about there's sort of a negative bias between the lead-212 -- and also there's some cases where there might be unsupported radium exposures, which would account for the actinium being significantly higher than the lead-212 result, which won't to be saying a lot, but we'll get into that.

MEMBER ZIEMER: Okay. We'll get to that. But you highlighted a few here that --

MR. BARTON: These are highlight because they're the only results at the 95th percentile that are actually above the detection limit.

MEMBER ZIEMER: Yeah.

MR. BARTON: So the detection limit for actinium was, I believe, .24 and for lead it was .23, or it might have been reversed.

Okay. So we also take a look at the areas to see where were the people sampled. Now, in this case, a large proportion were sampled in other areas, which is not surprising because you have several areas that wouldn't fit into Plant 1, 2, 3, 4, et cetera.

Interestingly, Plant 5 had

significantly high actinium results. And we kind of asked ourselves why that would be. There's no known processing of thorium throughout the campaign in Plant 5. But also one reason that might be is Building 65, where they stored a lot of the thorium in drums -- and it was actually noted in 1990 how much they were deteriorating and somewhat leaking -- that's right outside of Plant 5. It used to be called the old Plant 5 warehouse. So that might be an artifact of when they went to go get counted via in vivo and it's, you know, where were you? And it's like, well, I was in the old Plant 5 warehouse. So they just scribbled down “Plant 5.”

MR. HINNEFELD: This is Stu. Wait a minute. Building 65 was north of Plant 9, I think.

MR. BARTON: It is referred to the old --

MR. HINNEFELD: It is called the old Plant 5 warehouse on occasion, but 64 and 65 were adjacent to each other. And there was Plant 5. North of that was Plant 9. And then north of that were Buildings 64 and 65.

MEMBER ZIEMER: But Bob is suggesting that someone may have misidentified by calling it old Plant --

MR. HINNEFELD: They may have said they were working in the Plant 5 warehouse, I suppose.

MEMBER ZIEMER: Where they meant old --

MR. BARTON: Yeah, it's a theory, anyway. But also you see down here the pilot plant workers had really the highest overall results, which is not surprising because, as we said, the final campaign in 1979, which is what we're looking at here, happened in the pilot plant. It was a thoria gel operation. That was really the last production activity. So, again, you would sort of expect to see in that last production activity the higher lung burdens.

This next sort of test that we put it to is we wanted to see how frequently workers with positive samples were re-sampled as opposed to the rest of the monitored working population. And what we see here in front of us is essentially we looked at it from three pretty simple metrics: arithmetic average, geometric mean and rank-ordered median. And what you can see here is these sort of bottom two rows are -- well, look at the middle row. If you submitted a positive sample, the average time to the next sample was about 100 days. So, you know, a little over three months. That's at the average. If you start looking at the mean and the median, it's much less than that. It's more like a month.

Now, if your sample was less than the MDA, that number skyrockets to nearly 500 at the average and pretty much close to a year at the geometric mean and median. So you're almost talking a factor of 10 for those two metrics. So it's pretty apparent, based on this analysis, that if you submitted a positive sample, you were put on that schedule to be counted again much faster. It wasn't just a set schedule where it didn't really matter what your result was.

So, again, that's a piece of evidence for us that the data set we're looking at is sort of geared toward the higher exposed workers and, you know, it's not systematically excluding anybody. And really the ones with the higher results were re-sampled a lot quicker. So those are all pretty positive things for us.

Now we're going to go into the

adequacy of these thorium in vivo records. And, Joyce, step in if I get anything wrong here. I know you did most of the work on this particular section.

But the way we see it, there are four many facets in how you interpret the in vivo data. And when we say "adequacy," what we really mean is taking that number we see in the data set and relating it actually back to dose. And the four parts of that are really, number one, the assumption of the triple-separated thorium. And we can get into that if people have questions about that.

Also, to your question, Dr. Ziemer, one of the things was adjusting the lead-212 result for bias, because, as we saw, there's a significant difference in the Ac and Pb results, but also we noted that a lot of the lead results were negative, which just didn't make a lot of sense. So, basically what NIOSH did was they went in and corrected that so that you're not seeing a whole bunch of negative results. We're actually sort of correcting them back to zero for background.

The third facet, we mentioned also the high actinium results in relation to the lead results, is this notion of unsupported radium exposure. And I apologize. You see these two bullet points below 4. Those should actually be underneath 3 because they pretty much describe how that's done. You use the actinium chest burden and you assign it as a radium intake. So you evaluate the actinium burden. Or if it's a missed dose, the MDA divided 2. And you use the radium biokinetic model, and it would be considered Type M.

Okay. The assumption of triple-separated thorium. This was actually discussed a few times in Work Group meetings. And back in our original review of the completeness and adequacy of this 1979 to 1989 data, we actually state, “SC&A agrees that the triple separation hypothesis -- that is, the ratio of thorium-228 to thorium-232 -- equals 0.19. It's claimant favorably for the period 1979 to 1988, and by extrapolation, to 1989 when the lead-212 results are used to calculate the dose.” And SC&A's position remains unchanged on that particular topic.

In a similar fashion, as I said, we noticed that there were too many negative lead-212 results when we were looking at the data set, and we expressed concern in that 2012 report and we state, “most of the thorium-232 progeny results above the MDA are for actinium, and in most cases actinium activities are higher than the lead-212 activities.”

Subsequent to that report, NIOSH calculated an adjustment for that observed bias. It's contained in their most recent

White Paper. And SC&A agrees with the adjustments and how they were calculated. So we have no problem with that.

Now we move on to unsupported radium. SC&A agrees with this method as well, to use the actinium results to calculate radium-228. And these are sort of the samples we saw on the previous page. But to evaluate the actinium chest burden, or if it's a positive chest burden or as a missed dose, the MDA divided by two. And you assign it as the radium-228.

One thing we did note is that this method is really for estimating unsupported radium exposures to monitored workers. What we didn't see is any method to possibly incorporate that into coworker doses. And we don't know if that's something DCAS was planning to do, or I don't know if there's a particular response. We can certainly wait for a formal review of the Paper.

MR. HINNEFELD: Well, see, now we're talking about the period from '79 to '89.

MR. BARTON: Right.

MR. HINNEFELD: Right? And I think that while we have prepared methods for unmonitored workers, I think there's going to be a really small population of unmonitored workers that didn't get any in vivo monitoring during their employment and were in a category where they were likely to be exposed. Because people -- you know, if they were monitoring -- if the in vivo monitor was used to monitor people for potential exposure for uranium, that was what the purpose was, for uranium.

It showed up usually a couple times a year. And they counted everybody who'd had a detectable burden, and so you have the frequent recalculation if they had a detectable burden. The operations people had a particular frequency. The maintenance people had another frequency. And then other people who might go in the process area, like health and safety people, they probably had -- I don't remember exactly, but it may have been a little less frequent.

But almost everybody in the potentially exposed population would have been monitored. So I think there are very few people who were potentially exposed who didn't have an in vivo monitoring. Maybe some claustrophobes or something. Because in vivo, the chamber was really small, the portable counter

MR. BARTON: We did notice “refused to get counted” in a lot of files.

MR. HINNEFELD: Yeah. So you're only going to have a handful of people probably who were potentially exposed who weren’t monitored. And looking at the data, the in vivo data, the radium-228 without associated lead-212 is a fairly uncommon event. So, given the uncommon nature of the in vivo outcome and the small population that's probably going to need to the unmonitored approach, the unmonitored worker approach, we did not propose that we would add the unsupported radium-228 intake for the unmonitored people. We just felt like it would be unlikely those two unlikely events would converge. We can --

MEMBER ZIEMER: But if you had such a case what would you do?

MR. HINNEFELD: Well, we wouldn't know.

MEMBER ZIEMER: Oh.

MR. HINNEFELD: We wouldn't have an in vivo result, so we wouldn't see the high --

MEMBER ZIEMER: Oh, you wouldn't? Okay.

MR. HINNEFELD: -- the 212 and 228.

MR. BARTON: Joyce --

DR. LIPSZTEIN: May I ask you --

MR. BARTON: Go ahead, Joyce.

DR. LIPSZTEIN: I would like to ask one question. Did they understand well that you were going to use all the actinium-228 chest burden to use it as unsupported radium?

MR. HINNEFELD: No, I thought what the decision process was -- and I may be wrong on this, so maybe somebody on the phone might have to correct me -- but I thought what the decision process was, was that there had to be a particular difference, some threshold difference between the actinium-228 and the lead-212 in order to draw that conclusion. I don't remember what it was. And I know Tom LaBone is on the phone and I may have just completely bollocksed that up. Tom would be probably the one who knows better than I.

MR. LABONE: I don't know if that

exists in the procedure or not, the actual instructions to do that.

MR. HINNEFELD: I'm sorry, Tom, I didn't --

MR. KATZ: Tom said he didn't know whether that exists in the procedures to do that.

MR. HINNEFELD: Okay. So, in other words, we may not have actually prepared that.

DR. LIPSZTEIN: Because that's on the Paper. And I thought, wow, that's -- but it's there, how to calculate the dose. And it's there in the procedures, evaluate the actinium-228 chest burden with radium-228 by arithmetic model and assign it as an intake rate of Type-M radium-228. It doesn't say anything about the difference between actinium and lead, or nothing like that. It's just evaluate actinium-228.

MR. HINNEFELD: Well, I think that what we've prepared isn't what I would call a procedure, but rather this is the method that would be utilized in the instance where it's determined it's necessary. And based on what Tom said, I don't know we've actually set a criteria for when is it significantly different. When you look at the in vivo data, there are examples, or at least one example of a case where there is an actinium result that's quite a lot higher than the lead-212 result.

DR. LIPSZTEIN: No, that's exactly what my doubt is, because it doesn't say when there is a significant difference. It just say all actinium-228.

MR. HINNEFELD: I think what the intent was not to -- you know, what we prepared was not intended to be this is a definitive instruction that in every case we will do that, but I think that what we would expect to do is to come up with some sort of criteria. When is the difference between actinium and lead big enough that we feel like it's worth that, you know, doing the unsupported radium intake? Most of these in vivo counts, if they're detectable, they're close to the detection level. And so you're going to have a pretty sizeable level of uncertainty in terms of the result. And so you're going to have a fair amount of separation, I would think, between those two numbers before you would really conclude that you an unsupported radium intake.

So, to answer your question, Joyce, I don't think that we've actually developed criteria for when we would make that decision.

DR. LIPSZTEIN: No, I agree with you that you should analyze the actinium-228. That makes sense when it's significantly greater than the lead. But if you read what is written on the page 13, you'll see that it's written that you are actually evaluating all actinium-228 chest burden.

MR. HINNEFELD: Yeah, I think when we write the procedure for how to do the dose reconstruction we'll make it clear that there's some sort of criteria to select that would cause you to do that, some sort of selection criteria that would cause you to do that.

MR. BARTON: Yeah, I think we're just confused a little bit by the wording, because it almost looked like we were doubling up. You know, we used the actinium here and the lead to do the thorium. So, I mean, obviously that's very claimant-favorable do that, but not very realistic.

All right. To move onto No. 4 -- I don't want to spend too much time on this -- is how you calculate what's known as the OPOS statistic: one person one sample. For those of you who don't know what that is, is what we used to do is called the pooled approach, which is take every sample, fit it to a curve, pick off the 50th and 95th and calculate intakes. One person one sample is we take each worker's samples in a given period, say a year. You average those into one data point and now you have a distribution of workers instead of a distribution of all the samples.

And this is being currently thrown around in the SEC Issues Work Group. And aside from just averaging, it's being proposed whether you actually weight it by some sort time, either the time that happened before that sample or the time after that sample to the next sample. Those are known as post-weighting and pre-weighting. We just wanted to note here that currently NIOSH is using the post-weighting approach. SC&A is recommending the pre-weighted approach. So that might be something that may be changed down the line. It may not. We wanted to make sure the Work Group was aware of that and how you actually calculate the results.

MEMBER ZIEMER: I thought the SEC Work Group agreed on a weighting procedure.

MR. KATZ: They did.

MR. STIVER: Yeah, this is Stiver. Dr. Ziemer is correct. In the last SEC Work Group meeting, I believe Dr. Neton indicated that they wanted to go ahead and use the pre-weighting. So it's something that's been agreed to. It just hasn't been promulgated into a procedure yet.

MR. HINNEFELD: I think this was prepared before that decision was made and so --

MEMBER ZIEMER: Basically it's in abeyance with the SEC Work Group. So it has been agreed upon.

MR. HINNEFELD: I believe it has been, so I think the model will be adjusted.

MR. BARTON: I mean, at the time this was done --

MEMBER ZIEMER: Right, I understand.

MR. BARTON: -- post-weighting was actually in RPRT-53, the revision.

MEMBER ZIEMER: Right.

MR. BARTON: So that might change. So the numbers probably will change. So I just wanted to make the Work Group aware of that.

Okay. Now we're going to talk a little bit about the job types that were identified as thorium workers. And, again, this is from page 16 of the NIOSH White Paper, and it provides this short list of seven job types. I've already read them into the record. They're up here on the slide, so I won't bother to do that again.

But to sort of get a handle on this, SC&A took a look at some claimant files. When

I say "claimant files," I mean the CATI reports, which is the Computer-Aided Telephone Interview; DOE response files, which are the monitoring files provided by DOE; and the Department of Labor case files, which don't usually provide too much more information than you'd find in the CATI and the DOE response, but since it's pretty much the initial application, sometimes there's more information about what sort of job duties were done. And so there is valuable information there.

We only looked at claims with a PoC less than 50 percent, because obviously those are the claims that would ultimately benefit from a coworker model or coworker intakes assigned. And what we basically did is we took that group and we classified them into essentially four categories: Category 1 is not likely to be assigned coworker intakes. These are your administrative personnel. You know, secretaries, people who really didn't enter radiological areas and so it's probably not appropriate to assign thorium exposure potential to them.

Category 2 is essentially those workers, those seven job types that were identified in the NIOSH White Paper as thorium workers. So if they're unmonitored, they're getting the thorium coworker model.

And these next two categories are kind of ones of interest to us. Unknown essentially refers to either there is no job title included in the claimant file, which sometimes CATI interviews are declined or performed with, say, a survivor who wasn't really sure on the exact duties and job title.

Then you sort of have this gray area in category 4 where it's sort of ambiguous. You know, you don't fit into that administrative category, but you don't quite fit into those seven categories delineated in the NIOSH White Paper.

So, on this slide we have essentially the number of claims that fell into each of these categories. As we can see, about a quarter of those claims would be considered purely administrative. A little over 28 percent fit into that -- it says likely, but really based on the current proposed method would definitely be assigned coworker intakes. Unknown was a pretty small grouping. Only six, six-and-a-half percent. And potentially those are the ones we're really interested in.

We're kind of interested in some of the unknowns, too, but it's really tough. A lot of times we glean information about whether they had thorium exposure potential, because when you have an unknown, what they did, you really don't have the information of what types of jobs they would have been doing. So it says here we concentrated on the third and fourth, but really what we're talking about is that fourth category of the short of gray area. They're not on the list of thorium workers as defined by the White Paper. They're not obviously non-radiological workers.

So we examined 20 such claims that fell into categories 3 and 4. Really there were only a couple from category 3. They were mostly from category 4.

MEMBER ZIEMER: How did you select the 20 once you got the categories?

MR. BARTON: Well, what I wanted to do was get a good cross-section of different job types that kind of fell into that gray area, but also have a significant employment period in the '79 to '89 period.

So we have a couple of observations based on that review. The job categories that fell into that sort of gray area were engineers, fire protection technicians, analytical chemists, supervisors; and were they supervisors in an office setting or were they labor foremen, that type of thing? Inventory control. I mean, do we have people out there opening barrels? Clerks usually we would consider administrative, but in this case the person was really out there with a clipboard kind of just like the inventory control person. Laundry we were kind of interested in in case they'd be exposed to thorium-type materials, washing maybe some of the anti-Cs or something like that. So we took at least one laundry worker. And then various types of trade workers that didn't necessarily fit into those seven categories that are in the proposed approach.

So, 13 of those 20 surveyed claims indicated exposure potential to thorium in the CATI report. Now I want to sort of give a little description there, because it's not necessarily that they said, “I was working in the thorium area,” or something like that. What it essentially is, and it's on the second or third page of every CATI report, it's a listing of maybe 25 to 30 contaminants and check boxes. Yes, no, don't know. What form was it in? What type of quantity? And so when I say 13 of the 20, 13 of the 20 had checked thorium for potential for exposure.

One other observation, because several of these workers did work after the time when the mobile in vivo unit was no longer used, was that when the IVEC system, which was the in vivo counter that was directly at Fernald -- it didn't move around or anything -- they were monitored after 1988 in that system, but they were not monitored or were rather sporadically monitored proper to that time.

And what I would take away from that

is, assuming they were pretty much doing the same types of things, that maybe after 1988, as the program is really fine tuning itself, they realized, well, maybe these people should have been monitored, so we're going to monitor them now, but maybe they weren't monitored before.

Another observation we had that 10 of the 20 claimants indicated that their work locations were highly variable. And I have quoted here, "worked all over the site." That's actually a really common phrase that you'll see stated in claimant interviews.

And six of the 20 actually specifically indicated either direct work with thorium or worked in thorium areas, such as Buildings 64 and 65, or involved in the thorium overpack, or a lot of times the quote, "thorium warehouse," which could probably refer to a couple different places.

MR. HINNEFELD: It probably

referred to Plant 1.

I would just like to offer a couple things here. First, we listed seven categories of unmonitored people who would get the coworker approach, which was a way to describe the kinds of people we expect would be there. And at this point, we're not writing exclusive procedures, you know, do this and do that. This is --

MEMBER ZIEMER: Non-restrictive.

MR. HINNEFELD: Yeah. So I think some of these categories; fire protection, assuming that's the fire protection engineer or fire and safety inspector; technicians; supervisors; probably inventory control, would probably be people we would consider potentially exposed in this situation.

Another thing to remember, though, is that during this period the thorium was stored and that the potential for the exposure for uncontained thorium was intermittent and relatively small scale. They had a small scale packaging operation. So things like if it’s a trades worker who worked for a construction -- we're talking about construction trades, not a maintenance trade, because sometimes the same job title shows up in your maintenance organization and in your construction organization.

I don't think there would be a construction exposure to any of these re-packing operations. Once you get into the subcontract remediation activities, there might be -- those who are subcontracted, they might have construction characters in them, or construction trades people. But I think it would only be maintenance trades that would be exposed to the overpacking situation.

Probably the real reason my people who hadn't been monitored previously or sporadically monitored in vivo previously started being monitored with the IVEC counter is because you went from having maybe 12 weeks of availability a year to 52 weeks of availability.

The mobile counter would show up for maybe six weeks at a time, 12 times a week. I think it was about six weeks. And they would count. And the poor guys who ran the in vivo counter worked long shifts because usually there were three shifts of people and they would count people all the time and just get as many people through as you could. And once you had an in vivo counting staff and an in vivo counting facility, and you got 52 weeks of availability, then you would count people who maybe didn't the cut, or didn't make the cut very often previously.

MR. BARTON: Okay. Well, I mean, one of our main concerns here, I guess, from sort of a macro view is that when you create lists of job types to consider for a coworker modeling sort of -- I guess sort of putting the onus on the dose reconstructor to determine whether they fit one of those job categories -- and I think there's more a chance that you could possibly miss someone when you put an actual list of specific jobs to who you're going to assign a coworker intake.

And I think really maybe the better way to do it is to put really the onus on ourselves, the program, to either say, listen, there are reasons why and very specific reasons why we believe that this claimant could not have been exposed to thorium.

And if we don't have that evidence to say, you know, absolutely not, there's no exposure potential for this particular worker, then I think you really have to give them the benefit of the doubt. And I think that's the spirit of what we're really trying to show here, is that you do have the sort of gray area job titles. And I'm not saying we should expand the list of job titles. What I'm saying is, I think philosophically we should be coming from the other angle of not trying to figure out who's included. But really, if we can't prove they should be excluded, then I think they should be assigned coworker intake.

MR. HINNEFELD: Well, I don't disagree with the thought of that, and I think we might be able to prepare something. I think you could write your actual procedures, which we haven't written yet, to tell the dose reconstructor that if you're not going to assign a coworker dose, you need to explain in the dose reconstruction why coworker wasn't assigned, you know, because the person was a secretary, the president of the company's secretary. So we didn't assign coworker dose.

MR. BARTON: No, I agree.

MR. HINNEFELD: Yeah, we could probably do something like that.

Now, it's interesting, you work in the program long enough and you hear everything. Brad, you know that. You hear every side of every question. We are often criticized for not including lists and not being specific. And we are also criticized when we generate lists because you omit people, and what about these other things?

And so I think what we tried to arrive at is a system of, well, here are some things that -- we generally write lists and say you definitely want to do it here. I can understand your point. Maybe you make the dose reconstructor write why they're going to exclude it.

MR. BARTON: Right.

MR. HINNEFELD: So I think that

will be something for the procedure thing, but I don't disagree with the sentiment. I just think that during this period though -- remember, this is a repackaging period, a storage and occasional repackaging period --and I think you would make different judgments about thorium exposure if you were really thinking of -- you are probably able, if you want to go to the problem, of making different judgments about thorium exposure than you would make about uranium exposure during this period, because uranium production was going on all over the place up through '86, something like that.

So you would make different judgments about uranium exposure. But at least you could make different judgments if you wanted to go the effort. So there might be reasons to exclude someone from thorium exposure during this period that would not be a reason to exclude someone from uranium exposure. I just want us all to keep in mind, that what was the status of the thorium out there in the period we're talking about?

MR. BARTON: Sure, and I guess I would add on to that. It's sort of are we trying to create a list of who's included or are we trying to create a philosophy of who will be excluded?

MR. HINNEFELD: Yeah.

MR. BARTON: And I don’t think it's really proper to really delineate specific job titles. I mean, you look at probably the claimant lists for any site and you might have 1,000 workers. You might have 300 different job titles. So creating long lists to be completely prescriptive is just not reasonable, which is why I feel like coming from the other direction to say, we sort of have to prove ourselves that there's no chance that thorium exposure happened in order to not include someone in an unmonitored dose.

And as you say, these operations for thorium are much different than uranium. So, I would say then it's easier to make a case for excluding someone. But, again, I would come at it not as we'll decide who to include, but really you have to prove why you're going to exclude them, which is essentially what you said.

MR. HINNEFELD: Yeah, I think that's decent. I'm just kind of curious about analytical chemists. I would probably exclude analytical chemists for thorium exposure. I wouldn't exclude them from uranium exposure.

MR. BARTON: Yeah, well, I mean, again is that someone out there sampling the drums, or is that someone sitting in a lab?

MR. HINNEFELD: Analytical chemists, as far as I know, either worked in the analytic laboratory in the health safety laboratory. And so I don't think there was any sampling done during this repackaging --

MR. BARTON: But, again, what I don't want to do is add a list of workers.

MR. HINNEFELD: Right.

MR. BARTON: I don't want to have a list of jobs.

MR. HINNEFELD: Yeah. Here is my reason for not including this person.

MR. BARTON: Right. And you could have a situation where they're an analytical chemist because perhaps their survivor said they were an analytical chemist, but really they were out there sampling or something like that. But, again, it's making the case of why you would not assign coworker doses versus making the case for why you should.

MEMBER ZIEMER: This question though is broader than Fernald. It's come up a number of times in the Procedures Work Group. And I certainly agree with SC&A's concern that you don't want it to boil down to a subjective judgment on the part of a single dose reconstructor. You want some consistency across the board so that if five different dose reconstructors had the same case they would arrive at the same conclusion.

There are a lot of situations where it has been helpful to provide a list as an example of the types of jobs, but have the caveat, which is your caveat, that unless you can exclude somebody specifically from a broad category, then they're in. So you end up doing both, because if you completely eliminate the descriptors which are some of those job descriptions, then you could argue that you have interview subjectivity too in the use of the philosophy.

MR. BARTON: Sure.

MEMBER ZIEMER: How do you get away from the subjectivity? So, it's a difficult issue to cover it both ways. I know we just discussed it about a week ago in the Procedures Work Group where SC&A again raised this in another context, and I think it's a good point. We need to be able to assure the consistency of the decision so it doesn't really look subjective.

MR. BARTON: Sure. You're always going to have situations, like you said, five different dose reconstructors. They could look at the same case and, even the way I'm saying it, three of them make a case for why they shouldn't be included and two of them say, well, no, there's a little bit there that we should include. So I understand there --

MEMBER ZIEMER: Well, we sort of, I think, at the Procedures Group kind of reached the point of saying let's give some examples.

We know these are always in, but you can't -- this is not the exclusive list. And you add to it basically what you described. Something like that.

MR. KATZ: You're still going to look at the claims file and see what information is in there, which can modify what you do in a given case.

MEMBER ZIEMER: And you're probably not going to 100 percent eliminate the subjectivity of different dose reconstructors, but you certainly want to minimize it.

CHAIRMAN CLAWSON: Well, and then I guess I kind of look at it a little bit differently being on the Dose Reconstruction Group, because then we get into it and we're seeing somebody excluded from it and we have no explanation why we get into it. And what Stu hit on that I really liked was that then, if somebody is excluded from this, that the dose reconstructor gives a little caveat of why there's --

MEMBER ZIEMER: Yeah, that's what Bob is saying.

CHAIRMAN CLAWSON: Right.

MEMBER ZIEMER: You've got to justify it.

CHAIRMAN CLAWSON: You've got to justify it. And I really like that, because looking at it from our standpoint on that, that gives us a better understanding of the thought process and also the reasoning behind it. Because as you were going through all these, I was looking at the clerks, and the laundry one is the one that really stuck out to me, because, to me, that is the focal point of everything that goes on throughout the whole site. All the coveralls ended up right there, you know, and different protective clothing and stuff that they have. Geez, that's where everything throughout the site, to me, would end up. And I think that's one of the reasons why this started in the issues because most people showed up for uranium who weren't supposed to be.

So I agree wholeheartedly with you. And every site is going to be different, because we even actually brought up certain sites that the person's job was the same for 20 years, but their job title changed four times. So I agree with what you're saying. We just don't know how to get there.

MR. BARTON: Right. Well, I think it's like Dr. Ziemer said, you can add some job titles as illustrative examples as long as the caveat is if you're not going to assign the coworker dose, then there has to be ample justification for why that's not happening. And that is really one of the, I guess, overarching issues that we wanted to bring up today.

I believe we can move on to the 1990 to 1994 period. Monitored worker doses in this period are going to be based on their in vivo results. And as we said at the outset, the unmonitored workers are assigned on based 10 percent of the Class W, which is sort of the middle ground. It stands for weeks. It's sort of like Type M thorium DAC value. And again, as I noted, it was a little confusing in the White Paper because it said in one place that you had to submit that pre-job fecal sample to really be considered for the coworker intakes based on this DAC value.

But, curiously, there were dose reconstruction examples. And it's in the last three pages of that White Paper. It was example 3, which was essentially a made-up worker who worked 1990 to 1994 who had no in vivo monitoring data and was assigned a DAC value, but there was no indication, at least in the example, that a fecal sample was performed. So we were a little confused as to whether that was a stringent guideline. It goes back to the sort of worker assignments that we were talking about in the earlier period where you have some illustrative example jobs and then you sort of have to justify why they weren't being included in the coworker assignment.

Another thing about this that we noticed is we're using the Class W DAC value, whereas the Class Y is about a factor of two higher. And I guess we didn't understand why that choice was made. We feel like consideration should be given to the higher DAC value in calculating those coworker intakes. In fact, it actually says one place in the report, and it's quoted from the 1990 Technical Basis Manual for Fernald -- it says, "ICRP 30" -- and, again, this is from 1990 -- "ICRP

30 has assigned oxides and hydroxides of thorium to inhalation Class Y. All compounds at the FMPC are assigned to inhalation Class Y."

So I guess what we're saying is that, barring a sufficient case of why you wouldn't see that solubility class, we feel you should go with the bounding value. I don't know if there are any specific comments on that at this point.

MR. HINNEFELD: Well, I don't recall. I don't know if anybody on the phone is prepared to talk about it or not. From a control setup, if you're working with thorium and you're not real confident of the solubility class you're going to encounter, or you're going to encounter a mixture of solubility classes, as you set up your controls you would use the lower DAC. Because you'd set up your airborne contamination area at 10 percent of DAC, and you would use the lower DAC if you had questions about the solubility of the material. So it kind of depends on the evidence available. Is that what was done and is that why we chose the lower DAC for this 10 percent intake?

So, now, I assume that once we arrive at whatever intake it's going to be, we'll use our normal method of saying the actual solubility we're going to use in the dose reconstruction is going to depend on the organ because one solubility class would be more favorable for some organs and another class would be more favorable for other organs. So I assume we're going to do that kind of standard practice. So the question though --

MEMBER ZIEMER: If it's lung, we'll use the --

MR. HINNEFELD: Yeah, yeah.

MR. BARTON: But that's after you've arrived at --

MR. HINNEFELD: But that's after

you've arrived at the intake. Exactly. So I understand what you're saying, but I think it has to do with do we have sufficient evidence that the control levels were set at 10 percent of the Class W data, of the lower data? In which case people wouldn't be exposed over that amount.

See, this is getting into 1990 now, so we're getting into fairly recent history. Westinghouse had been there for a number of years. They were still there in 1990, yeah. Westinghouse had been there for a while and had been in place and things were much better controlled than they had been. And so I'm thinking there may be sufficient indication that these work areas, these thorium work areas were controlled sufficiently so that people didn't go into these -- the areas were roped off. People didn't go in unless they were probably monitored and certainly wearing respiratory protection.

So I think we just have to see what the strength of the evidence is. If there's not strong evidence they used the lower DAC, then, sure, I understand what you're saying.

MR. BARTON: There was, I know, a couple of references to the Class W, and when we sort of traced them down, it was related to projects that were kind of started in 1995, as far as we could tell.

And then also sort of anecdotally, when we get to the breathing zone results, which we'll discuss a little bit later, you do see both solubility classes. It's mostly Class W, but there are some under Class Y. And when you get into that methodology, you'll evaluate that breathing zone based on what's listed there. And then as NIOSH does, they assign it based on whatever is higher for the organ.

So I guess we just wanted to see sort

of the case made of why the lower value was used when it does have a factor of two difference in the calculated intake at the end of the day.

Another thing we noticed is that when we were looking at claimant files is there were a lot of workers that suddenly started being monitored, and that could be for the reasons you stated, because now you have a permanent facility onsite.

But, again, we wanted to take a look at some claimants during this period. And, again, we have sort of two criteria. Less than 50 percent POC. And also we kind of added in this caveat that you had to work in this period for at least three months, because we didn't want to be looking at workers who were there for a month and, you know, you don't see monitoring. So that doesn't really tell you anything.

So about 252 claimants fit into those two criteria. Based on that review, 75 percent of the claimants we looked at had in vivo counts during that 1990 to 1994 period. So that leaves 67 claimants who weren't monitored in that period. So we looked at that 67. Forty-five could be considered in those job titles with very little exposure potentially, if any. Again, you have the clerk here, but in this case it was a clerk that was an office clerk, essentially. Secretary, contract administrator, HR representative. I don't want to necessarily read all of these in, but you can see them on the slide there. Very little chance that they would have been exposed to these.

So, now you're down to about 22 workers. And so these are the one we looked at in-depth. And the job titles we see there are laborers, maintenance, painters, iron workers, heavy equipment operators, a technologist, which we weren't really sure exactly how they might be -- they might out there working on instrumentation or something like that. Quality assurance. Again, were they out there sampling drums? Health physicists, obviously, would have been part of that process, or potentially part of that process. And engineers again. So those are the types of job titles we saw out of the 22 we looked at in-depth.

Again, nine out of those 22 worked all over the site, or, you know, all plants and buildings is often what you hear. But also 11 of the 22 actually indicated in their CATI report that their exposure in any sort of radiological area was either intermittent or non-existent.

As a follow-on to that, when you look at their external badging, which was pretty much required to enter a radiological area, again, you have a couple of months during that five-year span where there was a badge or two and then whole periods where there was nothing. They were probably not in areas where exposure potential could have existed for very long.

One claimant actually indicated involvement in the overpacking, however, based on examination of that claim, it probably occurred after 1994. So, even though that person was not monitored in the 1990 to 1994 period, when we examined it, it appeared that that overpacking occurred after 1994. And in fact, we'll talk about it a little bit later. There was extensive breathing zone for thorium over the exact span that was indicated in the CATI report for the overpacking operation.

So, to continue on, like we said, the coworker intakes were based on 10 percent of the DAC value to be applied to workers who submitted thorium fecal samples. What we concluded based on the claimant study, it's pretty unlikely that the unmonitored workers, the workers who didn't actually have in vivo results which you'd use to reconstruct doses, would have actually been in an environment, 10 percent of that derived air concentration, for the entire duration of the relevant employment.

So we feel that that 10 percent reasonably represents a bounding approach to the workers. But also, based on those unmonitored claimants, we do see a few situations where there is opportunity to potentially be exposed to thorium. And so we sort of questioned that criteria that you had to submit a pre-employment fecal sample.

And one of the things that was referenced, I believe, where this sort of came from was a standard operating procedure, but that's the same one that required workers who were, "routinely handling thorium materials submit a fecal sample." It also required that they were in vivo counted at the beginning of the operation, at the end of the operation and at three-month intervals.

So that sort of begs the question, why would you have somebody unmonitored via the in vivo system and not have their fecal sample? Two possible explanations is that their in vivo records were maybe lost or unavailable, in which case the same might be said about the fecal samples. Maybe you have workers who their fecal sample was lost. So that's kind of neither here nor there. Or maybe that operating procedure just wasn't followed as stringently or only followed for workers who are routinely handling thorium, in which case 10 percent of the DAC might not necessarily be appropriate.

MR. HINNEFELD: Okay. Run me back through this again. I think I lost the train there a little bit. The operating procedure you're talking about, or the plan, what was it, for a particular thorium operation?

MR. BARTON: Yes.

MR. HINNEFELD: And the date of that was in this '90 to '94 period?

MR. BARTON: It is referenced. I don't have the date in my notes here, but it was in this period, yes.

MR. HINNEFELD: Okay. And so it said that anyone who's going to regularly handle thorium should have a pre-project fecal, pre-project in vivo.

MR. BARTON: Right.

MR. HINNEFELD: End of project in vivo.

MR. BARTON: In vivo. And then in vivo at three months.

MR. HINNEFELD: At three-month intervals.

MR. BARTON: And then also, if the need arises, additional fecal sampling and in vivo counts as necessary.

MR. HINNEFELD: Okay. And so, given that requirement -- now, what was the next part of your discussion? Why would we have somebody with --

MR. BARTON: Well, the entry criteria for receiving 10 percent of the DAC is that you have a fecal sample, but no in vivo counts.

MEMBER ZIEMER: Which would be somebody who you thought was going to do this work and maybe didn't.

MR. BARTON: That's one possibility. Right.

MEMBER ZIEMER: But you're also asking about what? What about the case where you didn't have the pre-occupational, but --

MR. BARTON: But there's a

potential to be exposed at a level that would be absolutely bounded, in our mind, by 10 percent of the DAC.

MR. HINNEFELD: Okay. So there are two questions: One is, should we really require that fecal sample in order to give somebody 10 percent of the DAC?

MR. BARTON: Or are we at a situation, again, like the previous period, where you have -- it's very useful information to say this is one requirement for if your absolutely routine handling it, but also you sort of have to make the case that they weren't exposed to not include them in that 10 percent DAC subgroup.

MR. HINNEFELD: Okay. All right. Okay.

MR. BARTON: All right. Then for the period three, this is, again, 1995 to 2006. To reconstruct monitor worker exposures we're going to use the available breathing zone samples. We noted that those were contained in HIS-20, but also contained in the individual DOE monitoring files for each claimant.

There is no coworker dose to be assigned, or was proposed to be assigned after 1994. This was, again, first discussed in the September meeting. And, Stu, I'll get to quote you again. "The thorium area would be defined. And if you're going into this, into the thorium radiological area or the airborne, everybody had a BZ with them." And I think you also noted that even when you went into the areas you wore a breathing zone.

MR. HINNEFELD: Yes, I did.

MR. BARTON: So we can take a look at some of the breathing zone samples that we do have. And what we're looking at here is the number of breathing zone samples we have per year. And as you can see, there's obviously a reason why 1995 was the choice to start this, because 1993 and 1994 you have very few samples. Starting in 1995, you're up close to like 1,800, somewhere around there. And it increases in 1996.

You see this little dip here, which was a little curious. I could understand it for '98 and '99 because it seemed like that was more like a characterization. You weren't necessarily overpacking or handling the material. 2000, 2001 there were some significant shipments to NTS of the material, but it wasn't clear whether that had been packed, overpacked earlier and it was just now getting shipped off. So you see that dip. But we still have a significant number of breathing zone samples. And of course it rises from there to a maximum of near 12,000 samples in 2005.

The next chart is very similar

except instead of total number of samples, we're looking at the total number of monitored workers. And, again, it closely mirrors the total number of samples. You sort of have that dip from '98 to 2001. But for many years -- for example, 1966, you have a little over 400 workers who were monitored via this breathing zone for thorium.

I talked before about the claimant who stated that they were involved with the thorium overpack operations. And I wanted to read this from their CATI report, because I think it's very informative as to the conditions that were happening. Again, the claimant specified a pretty exact period of time. Not the actual dates, but the span of time that they were involved with thorium overpacking.

But this is directly from the CATI report. And obviously for Privacy Act reasons a lot of it is redacted. So anywhere it says "redacted," I'm just going to read it in as claimant.

So the claimant worked in the thorium overpack site where the claimant remotely operated a device that would move drums around. The claimant had to dress out and enter the building to get on an electric forklift, went over to the actual boxes they loaded the drums in, the overpacks. The claimant put a lid on the boxes and set them in an area for the chemical operators to clean. Then the rad techs came into survey them. If they were clean, they were sent out to a driver on the 'clean' area on process side and then they were sent to an area to be readied to ship offsite.

In the thorium overpack, the claimant had to wear double sets of cloth coveralls. The claimant had to wear a cloth hood. The claimant always wore a full-face respirator in the thorium overpack area. The claimant had lapel monitoring done when the claim was in the thorium overpack. And the claimant was dressed out in double sets of anti-contamination clothing, or anti-Cs.

So that's a pretty descriptive version of what was happening. And I can say that this would have been sort of in the early 1995, '96, '97 period. And as I said before, there were numerous breathing zone samples identified with this claimant for the exact span that they indicated they were involved in the thorium overpack operation. Looking at those samples, it looks like on average they were probably pulled every six to seven days, but that varied somewhat. Sometimes it would be very two days. Sometimes 10 days or so.

MR. HINNEFELD: Can I just comment on the record? The BZ samples, the record is a weekly compilation of all the BZs that person wore that day.

MR. BARTON: Okay.

MEMBER ZIEMER: Are they counted daily?

MR. HINNEFELD: Yes. Sampled every time. They would have a daily sampler. Whatever their shift was, they would have a daily sampler and it would be analyzed daily. And then their record, though, what's kept in HIS-20, would be the weekly compilation of the samples they wore. So if you see a six-day period, the person probably worked Saturday. They were sampled, for some periods at least, every one of those days, and that's a compilation of the six. If there's only two or three days, it looks like, that means those were the days on that week that they were sampled.

MR. BARTON: And it was evident looking at that that the numerical results themselves reflected sort of the number of days that happened. So at first we looked at it as like, well, these seem kind of sporadic, but when you actually get into the data set, you can see that it's exactly what you described.

MR. HINNEFELD: Yes, you had a daily sampler. Every day they would take it and they counted, like you said, thousands, thousands of BZ samples.

MEMBER ZIEMER: So the last bullet that suggests they were pulled every six to seven days --

MR. BARTON: That's what appeared like in the HIS-20 records.

MR. HINNEFELD: That's the way the record would look because of the way the record was prepared.

MEMBER ZIEMER: Right. Right. The samples were pulled daily.

MR. HINNEFELD: They were daily.

MR. BARTON: Right. No, I didn't want to infer that they only took a breathing zone every six or seven days. I was just --

MEMBER ZIEMER: No, I thought you were suggesting initially that they wore it for seven days and then it was compounded and --

MR. HINNEFELD: The pump wouldn't last that long.

MEMBER ZIEMER: Right. Well, yeah, plus you --

MR. HINNEFELD: The batteries, I mean. The batteries on the pump wouldn't last that long.

MR. BARTON: Okay. In this last section again we're getting back to thoron. And, Joyce, if I have you on the phone, I would like some help if I kind of stumble over myself here.

But essentially what our concerns here were, not necessarily the calculation itself, but how transparent the actual assumptions were in selecting the various values. For instance, Item 1 here is essentially an estimate of source term. And we saw a few different numbers that seemed to contradict each other, and we weren't sure why certain numbers for the source term in metric tons were chosen.

Number two, we talk about the release fraction. And, again, even in that White Paper, it seemed to range from 10 to the minus 3 to 10 to the minus 6, which is factor of three orders of magnitude. And the selected value was I believe somewhere in between there. And it really comes down to, when these thoron calculations were made, we didn't see the justification that you'd like to see to assure that when you select these values, which ultimately go into the calculation to get potential thoron exposures, we feel like you really need to sort of buttress that argument and say why did we select this value, if we selected it, because of this condition? And so that is scientifically justified, but also claimant-favorable.

The other two, the occupancy factor. Again, it selected three months of essential exposure up through 1989. And then it said one month during the final closure. I assume that means 1990 and beyond. Again, we just didn't see necessarily the rationale for selecting those occupancy times.

And then also the specific activity. Joyce, do you want to speak a little bit on this one? Do we still have you?

DR. LIPSZTEIN: Okay. The specific activity of thoron was given assuming exposures occurred six to 12 months after separation and an equilibrium fraction of thorium-228/thorium-232 of .65. And we have some referencing that the equilibrium fraction, for example, for materials in Building 65 was at least .95.

So I think, you know, all those various factors that were chosen to calculate the thorium exposures, they don't have really a scientific justification of saying, oh, we assume this because it was claimant-favorable. They were just taken. And there are many contradictions between the ones that were chosen and the various ones that I cited in the -- even in the same draft and some in the papers that were related to Fernald. So we would like to have this reevaluated and justified.

MR. HINNEFELD: Okay. Of course, we only received the written review, what, a week-and-a-half ago, something like that. So it will take us a little while to go through it and --

MR. BARTON: And we're not necessarily saying that these values that were chosen are wrong. We just wanted to see --

MR. HINNEFELD: The basis for why we did it.

MR. BARTON: The basis, yeah.

CHAIRMAN CLAWSON: Better understand them?

MR. BARTON: Exactly. Exactly. So that we know why we're selecting the different values.

And, Joyce, you also -- this last bullet here about possible handling of radium-228.

DR. LIPSZTEIN: Yeah, this goes back to that maybe misunderstanding that all actinium would be used as a supported radium-228 exposure. I think that we have first to resolve that and then come back. But if they were unsupported radium-228 exposures, then the thorium associated with this radium-228 exposure should be also added.

MR. HINNEFELD: Yeah, I think that really the only mechanism I can think for unsupported radium intake would be a raffinate exposure, because, I mean, it would exist in the waste stream of the thorium purification process. And there was no other source, I don't think, of thorium-228. So I would say there was none that was stored except that it was one of the materials that was pumped out to the waste storage pits. There was lots of uranium raffinate.

MR. BARTON: Okay. And so the final slide here is sort of our main conclusions. The first one is that SC&A feels that dose reconstruction for internal thorium is feasible and can be performed in a claimant-favorable manner. And as I said, we have a few issues that maybe need to be vetted a little bit, such as the selection of the derived air concentration, but that's not something that would render reconstruction of internal thorium infeasible.

And the second bullet here we had a discussion on, and that's we felt that maybe the application of unmonitored coworker doses could be too restrictive, but perhaps that wasn't really the intent. And then, as we talked about, those types of exposures could be illustrated by some job categories, but ultimately the onus is on the program to demonstrate that they shouldn't be assigned thorium exposures versus the onus being that we have to demonstrate that they should be. So, again, it's sort of coming from that other direction.

And then, as we just discussed with the thoron, we'd like to see more scientific justification for the assumed values that were used in those calculations.

So those are really our main conclusions. Any additional questions?

MR. HINNEFELD: I kind of interrupted with mine as we went.

MR. BARTON: What?

MR. HINNEFELD: I interrupted you while we went and asked all my questions.

MR. BARTON: Okay.

(Laughter.)

MR. HINNEFELD: Anybody on the phone? Does anybody on the phone from ORAU want to offer anything? If you don't, that's okay, but do you want to ask questions or pursue anything?

MEMBER ZIEMER: Do you know off the top of your head whether the higher -- if you can go back to the previous -- maybe it's the second-to-the-last slide where I think Joyce was comparing the -- go back one more slide.

Oh, here it is. The equilibrium fraction. Maybe it’s the next one again. Go forward one.

MR. BARTON: This is the next thoron slide.

MEMBER ZIEMER: Oh, here it is. Yeah. Do we know off the top of our heads whether the .65 versus the .95, which would actually be more claimant-favorable? Just off the top of our head, does anybody even -- you haven't looked at it maybe.

MR. HINNEFELD: Haven't really looked at it. Let's see. Well --

MEMBER ZIEMER: I mean, obviously they haven't said why they chose the .65. And that was your point, but I'm wondering do know specifically if that would end up for some reason being more claimant-favorable?

MR. HINNEFELD: If that ratio were higher --

MEMBER ZIEMER: It gives you

more --

MR. HINNEFELD: -- there would be more thoron --

MEMBER ZIEMER: More?

MR. STIVER: You'd have higher 228 concentration, which would be the --

(Simultaneous speaking.)

MEMBER ZIEMER: No. Let's see. If it's close to one -- if it's .95, the two are about equal. If it's .65, then I think you've got more 232, the denominator is larger.

MR. HINNEFELD: Right.

MEMBER ZIEMER: But I don't know how the energies are there, the alphas and so on.

MR. HINNEFELD: I think that this relates to the source term of the thoron, which would be approximated by the activity --

MEMBER ZIEMER: If you don't know off the top of your head, I was just curious.

(Simultaneous speaking.)

MR. HINNEFELD: I think if those two were closer to equilibrium, then you would have more thoron per gram of residue. That's right, isn't it?

DR. LIPSZTEIN: Yeah, you would have more thorium.

MR. HINNEFELD: Yeah, you would have more thoron.

MEMBER ZIEMER: Thoron or --

MR. HINNEFELD: Well --

MEMBER ZIEMER: If they were closer, you would have more 228.

MR. HINNEFELD: But the question is about the thoron source term.

MEMBER ZIEMER: Yeah. Right.

MR. HINNEFELD: And so the closer that ratio is to one, the more thoron you'll have per gram of residue.

MEMBER ZIEMER: Right.

MR. HINNEFELD: And so what the question is, is why did you choose a .65 equilibrium when certainly if the materials had been stored in Building 65 all that time it would seem like it would be different than that. It would be higher than that. So I think that's the question.

MEMBER ZIEMER: Yeah. In that connection, I think Bob had mentioned that there was an issue with whether that was a claimant-favorable assumption. I just wondered if anybody knew. But obviously we don't know right off the top of our heads.

MR. BARTON: It all boils down to again we just -- when you select these values among --

MEMBER ZIEMER: What's the basis, yeah.

MR. BARTON: Yes.

MEMBER ZIEMER: Got you.

MR. KATZ: Just going back, Stu had asked NIOSH ORAU folks on the line whether you had any other clarifications you needed before we close this part of the discussion.

(No response.)

MR. KATZ: No?

MR. HINNEFELD: I have one question before we close. How comfortable is the Work Group with SC&A's main conclusion?

CHAIRMAN CLAWSON: I feel good about it, but the thing is we just -- some clarifying questions. It's never been -- we know what you guys can do. It's just how the process is going to --

MR. HINNEFELD: Okay.

MEMBER ZIEMER: Well, the first bullet I think is fine. I mean, we're all in agreement there, right? The second bullet you're only asking for clarification of that issue that we discussed about restricting the list or --

MR. HINNEFELD: Right. Yeah, that's the one that -- that's the issue that I'm sympathetic to, and I understand --

MEMBER ZIEMER: And I think you've sort of agreed to it.

MR. HINNEFELD: Yeah.

MEMBER ZIEMER: And I think we've sort of agreed to it.

CHAIRMAN CLAWSON: Yes, we have. Well, I have. I'm not speaking for the group.

MEMBER ZIEMER: No, no. And there are some others in the Work Group not here, but conceptually I think we're sort of -- and then the third one is clarification. And once we get that, then we have to decide whether we agree with that.

MR. HINNEFELD: And, I mean, our parameters might change and we might say, okay, good point. We'll change these parameters.

MEMBER ZIEMER: Yeah. So I -- if you were -- who is asking the question?

MR. HINNEFELD: I asked the question about --

(Simultaneous speaking.)

MEMBER ZIEMER: Yeah, I think we're comfortable with what the issues that were raised and --

CHAIRMAN CLAWSON: Right. The one question I did have was one that has come back to me many times, and that's the difference in the tonnage of the thorium that we've had there, because coming from other sites and so forth like this, Fernald actually has become the dumping place for that. And. I mean, I saw six train cars from Hanford. Do we have for sure a tonnage of what was actually there?

MR. BARTON: Well, that would be --

MEMBER ZIEMER: Well, part of the question with the tonnage issue was one of the clarifications, wasn't it?

MR. HINNEFELD: I would think certainly there was -- I don't know we have now. Certainly, Fernald knew how much thorium it -- it was an accountable material like uranium was.

CHAIRMAN CLAWSON: Right. Well, yeah, I was just -- and if you pulled back to that one, I was just looking at the different -- 300 metric tons in the storage site and 450 quoted over 2,000 tons of material. And I just wanted to make sure that we did have -- because it's kind of hard to follow a lot of this sometimes because some of it was coming in and some of it was going out. But I know that in documents that were pulled from Hanford that I was reading it was amazing to me that I was finding Fernald paperwork at Hanford.

MR. HINNEFELD: They got everything.

CHAIRMAN CLAWSON: Well, it was actually the shipments to Fernald of this. And I was looking at well over 800 tons. So that's why that one kind of just sticks out to me. I just wanted to make sure that we -- but also, too, in the same process we could have some tonnage coming in and some going out all through the years.

MR. STIVER: Yeah, there's some kind of mass balance involved --

(Simultaneous speaking.)

CHAIRMAN CLAWSON: Right. Because there's been quite a bit of discussion of how much we really had, and I just wanted to make sure that we put that one to bed, too, even what has been processed through there. Because sometimes they were being repackaged and redone and have gone forward.

MR. HINNEFELD: These numbers are apparently from the document we wrote about the thorium approach. And so I'll just have to take a look and see why there are different numbers. I mean, the easy thing that comes to mind is that one or more of those numbers might be thorium tons, and the other one might be residue. I don't know if that's true or not.

MR. BARTON: That would be part of sort of the justification.

MR. HINNEFELD: Yeah, that's not an explanation. I just made that up. I don't think --

(Laughter.)

(Simultaneous speaking.)

MR. HINNEFELD: Well, I don't mind if I'm quoted as long as I'm quoted saying that I made it up.

(Laughter.)

MEMBER ZIEMER: Bob, a quick question here. As we look back on these slides, the three points on the major assumptions, you list the assumptions, but you also raise some questions in there. I'm not sure those -- did those questions show up in your conclusions?

MR. BARTON: If we could go back to the conclusions, it's that third bullet point that those main parameters for the thoron calculations --

MEMBER ZIEMER: Oh, that was intended to cover these, all of those? Okay.

MR. BARTON: Yeah.

MR. KATZ: Can I suggest a comfort break?

MR. HINNEFELD: I was hoping you would. I was going to suggest it.

MR. KATZ: Okay. How about 10 minutes?

(Whereupon, the above-entitled matter went off the record at 10:36 a.m. and resumed at 10:46 a.m.)

MR. KATZ: Okay. Welcome back, everyone. Folks on the line, I hope we have you, too.

I will just take this opportunity, then, and let me check and see if maybe Mark has joined us. Mark Griffon?

(No response.)

Okay. Not yet?

Do we need to check on anyone else on the line?

Joyce, do we have you back on the line?

(No response.)

Do we have anyone on the line?

MS. LIN: Ted, this is Jenny Lin. I'm here.

MR. KATZ: Okay, good. I just wanted to make sure that we were being heard.

MR. KINMAN: Yes, Ted, this is Josh. I joined the meeting around 9:15 today.

MR. KATZ: Okay, great. Welcome, Josh.

Okay. I don't know if you need Joyce right now. Do you?

MR. STIVER: I don't think we really need her at this particular moment, but she will be back on soon, I would think.

MR. KATZ: Let the record reflect we don't need Joyce now.

(Laughter.)

MR. STIVER: Okay. On this particular issue.

DR. LIPSZTEIN: I'm listening.

(Laughter.)

MR. KATZ: Okay. I didn't mean that personally, Joyce.

MR. BARTON: We knew you were there the whole time.

MR. STIVER: Okay. I guess we can go ahead and get started.

I have shared the list version of the Issues Matrix on Live Meeting. So, you should be able to see we're on TBD Issue No. 4. And I would just like to go through the open issues that we can have a substantive discussion on today. I believe there's about 11 of them.

No. 4, this is one you will find attached. It is the Guidance to the TBD regarding exposures from redrumming of thorium is not well founded and is not claimant-favorable.

And this is one of those legacy issues from a time when thorium intakes would be determined based on air-sampling data. And we decided to keep this thing, this particular issue, open based on a review of the post-SEC Thorium Report.

And, basically, for this period 1990 to 1994, we were kind of concerned that redrumming was going on, repackaging, and that maybe we should take a look at that before we are willing to close this out.

And based on today's discussions, I think we are in agreement that this particular issue can be closed out.

I am trying to bring this down to page 7.

So, we wanted to keep this open and in progress until we some time to discuss this. I think, based on our conclusions and the Work Group acceptance, that we can go ahead and close out TBD Issue No. 4.

Anybody have any comments or questions on it?

CHAIR CLAWSON: I just wanted to make sure what the whole question was. There were a couple of pages there. Just the guidance on the --

MR. STIVER: Well, remember this

was the TBD from 2004. There just wasn't a lot of guidance about the exposures from redrumming and how it would be addressed, and so forth. This is something that we can read down through the text --

CHAIR CLAWSON: Right.

MR. STIVER: -- and read everything into the record. But it is something that has evolved over the time, and we got into the discussion of DWE exposures, our report on that, the determinations of SECs.

And so, really, the only thing that was really outstanding was what was going to be done post-1989, since we have the coworker model from 1979 to 1988, we have the SEC preceding that. And so, we just wanted to make sure we had a chance to evaluate the current guidance in post-SEC thorium and how that is going to be handled.

MEMBER ZIEMER: Question, John.

Do you need to see anything in writing on the issues that were raised today to close this or --

MR. STIVER: I think we probably want to see --

MEMBER ZIEMER: And I am really asking you, I mean, we certainly have kind of an agreement.

MR. STIVER: Yes. I think we probably --

MEMBER ZIEMER: There are some things, some explanations. I am wondering if it is in abeyance versus closed. I'm not sure. This one looks pretty general.

MR. STIVER: Yes, this is one that really wasn't a particular issue regarding dose reconstruction.

MEMBER ZIEMER: Right. It is the guidance that --

MR. STIVER: Yes, it is kind of an

overarching thing. You know, there is very little guidance in the --

MEMBER ZIEMER: Yes, broadly, you are okay with what the guidance is?

MR. STIVER: Yes.

MEMBER ZIEMER: As opposed to some other ones that are more --

MR. STIVER: I think that, because of that, this one could be closed. There's nothing that is going to be coming out in the new TBD that is going to really impinge on this one.

MEMBER ZIEMER: Right.

CHAIR CLAWSON: So, this doesn't come down to who they accepted?

MEMBER ZIEMER: No, that is covered by other ones in there.

MR. STIVER: Yes, that is going to be covered in there.

CHAIR CLAWSON: Okay. That was my

question, because it seemed like this is what we had just gone over earlier.

MEMBER ZIEMER: Well, this is a broad guidance.

MR. STIVER: Yes, this is a broad guidance that is going to be incorporated into TBD 5.

CHAIR CLAWSON: Okay.

MR. STIVER: Like I said, it is kind of a legacy.

MEMBER ZIEMER: I was just asking if you are looking for any new wording.

MR. STIVER: Not particularly.

MR. BARTON: Well, I think NIOSH was going to finalize the approach. As you said, there were some wording in there; it may be confusing. So, I guess it is kind of just a --

MEMBER ZIEMER: Is that part of the guidance?

MR. BARTON: Well, we have several sort of thorium-related findings in here that are all just assumed by the approach we just discussed. So, while we do kind of need to see the final product --

MEMBER ZIEMER: Well, but, see, those could be in abeyance.

MR. BARTON: Yes.

MEMBER ZIEMER: It's this specific one, can this be closed.

MR. STIVER: This particular one I don't feel needs to be in abeyence. There is really nothing that NIOSH is going to do that is going to really impinge on this kind of this kind of --

MEMBER ZIEMER: On this one? If that is the case, I'm okay with it, Brad.

CHAIR CLAWSON: I have no problem with it. Okay.

MR. KATZ: Okay, closed.

MR. STIVER: Okay. Let it be read into the record that TBD Issue 4 is closed.

Now TBD Issue 5 is kind of similar in a way. This, again, was related primarily to the use of DWE data. And this was the notion that the TBD had not evaluated exposures due to thorium fires. Small fires, spills, explosions were commonplace, and it is unlikely that most of the air sampling data that we are compiling will necessarily reflect those radiological incidents.

And again, this was one that we felt, because there is this post-SEC thorium evaluation, that we would want to go ahead and keep it open until such time as we had to discuss this.

Again, this gets to whether it be -- the model is considered bounding for incidents and fires and things of that nature. And I think we have established that the models that NIOSH is putting forth are sufficiently claimant-favorable to cover short-term incidents that took place, based on monitoring data.

If you are looking at the air-sampling data, then you have problems of whether you have a complete set of data that would adequately represent those types of incidents. But, by using bioassay data, you can kind of sidestep that problem.

MR. BARTON: And also, you know, the first part of this is during fires, which really isn't relevant to the period we are talking about.

MR. HINNEFELD: This is in operation. This was an earlier issue.

MR. STIVER: You're looking back in the fifties and sixties.

MR. BARTON: Right, and there were maybe a handful of documented small-scale spills during the early overpacking operations, but they are really well-documented and they had air sampling and everyone was bioassayed.

So, now that we have the coworker model in place, I don't think this is really relevant anymore.

CHAIR CLAWSON: A relevant issue?

MR. STIVER: Yes, this is another one that I don't think revisions to the TBD 5 are going to impinge on closure of this particular issue.

CHAIR CLAWSON: I have no problem.

MEMBER ZIEMER: Yes, I agree, let's close it.

MR. STIVER: Okay. The next one is a little trickier. This is TBD Issue 7, and this gets back to -- I'm just going to read it.

"The TBD is a non-specified method for estimating doses in the raffinate stream."

This gets back to the whole idea of raffinates for more process than in plants 2 and 3, and it is kind of two-pronged thing.

One was what we call the use of the radon breath data for transferring the Q-11.

CHAIR CLAWSON: So, this is the raffinates.

MR. STIVER: This is the raffinates. There's kind of two sides to this. One is the dumping of the hot raffinates, Q-11 raffinates, those that came from Mallinckrodt as well as those that were produced onsite into silos 1 and 2, and how do you get a handle on exposures to radium and thorium and some of the daughter products that are contained in the raffinates without some sort of uranium values bioassay.

And NIOSH, over a course of a period of time -- I believe this was in 2008 where they put forth a methodology using the radon breath analysis to get some kind of hook back to these thorium-230 and radium-226 exposures during these operations. And we agreed in previous Work Group discussions that we were fine with that.

The other aspect, though, was situations where you had workers who were exposed, potentially exposed, to raffinate streams that we have been elevating to thorium-230, but that were depleted in radium and uranium. In a situation like this, you couldn't use the radon breath data. We didn't have radon breath data for those people for that matter. Or we were concerned about using urine bioassay because, you know, essentially, there is going to be no uranium in there. You are looking at background levels, if you are able to, in fact, identify those workers who were in that particular facility for that particular time during these operations.

And so, we had talked about this -- oh, gosh, let's see, who produced the paper? I think Joyce did back in 2010 looking at this issue. NIOSH responded by updating Report 52 to address this.

And at the April 2011 Work Group meeting, there was some discussion on this particular issue. We felt that it was probably possible to bound these thorium-230 intakes for these people, in theory, kind of as a general principle. However, there are still some kind of issues out there, I think mainly by virtue of the fact that the guidance or the discussion in Report 52 and our discussion back in that time took place at a time when we felt that this DWE data for the various buildings, the thorium DWE air sampling data, could be used in a way that would allow us to identify particular conditions in a plant for a given period of time. And so, you could, then, identify workers.

If you could identify who was in, say, building 3 on the cold side of the raffinate stream, then you would kind of have the ability to take a look at their bioassay data and, then, assume some particular addition of thorium-230, either based on equilibrium assumption or the ratios from silo 3, which I believe the ratios were at 60 nanocuries per gram of thorium to about 3.5 nanocuries of uranium.

And so, we kind of agreed in theory that that could be done. But, in the meantime, you recall that the SECs for thorium, at least the big one, are based on DOE data, 1954 to 1967, really came about because it was demonstrated that you really couldn't identify who was in a particular plant at a particular time.

And so, we still have some concerns about that. And as you see on page 7 here, we felt that this particular issue at this point was too complex to be put into abeyance without a formal review of what NIOSH puts forth in the TBD 5 revision. And so, we wanted to keep this one open. This also applies to the next finding, Finding No. 8.

MR. HINNEFELD: Okay. Finding No. 7 is quantifying exposures to raffinate materials.

MR. STIVER: Right. It was just basically how were you going to go about doing this.

MR. HINNEFELD: When are we talking about? So, this is going to be after 1978 probably?

MR. STIVER: No, no, this is actually during the fifties and sixties.

MR. HINNEFELD: Well, if it's in the SEC period --

MR. STIVER: Well, it is going to be

for non-SEC cancers, yes.

Now this is another thing. I was wondering who was going to bring this up first. We are looking at thorium-230. Now the SEC, but this is from a separate process stream than the thorium refining and machining and other work in production. It basically comes out of the uranium-238 process stream, but we are still looking at thorium.

Now the SEC doesn't specify thorium-232 or any other isotope. So, this dosage can't be reconstructed from thorium during this period of time. So, this is kind of a wrinkle here.

MEMBER ZIEMER: So, you are looking for what is going to be in Rev 4 on this issue? Is that --

MR. STIVER: This is going to be the latest -- I think it is Rev 1 of the Internal Dose TBD.

Let me just finish. I think the problem we have got here is that, if this were to be determined, the thorium-230 intakes were determined to be part of the SEC, then, for those workers who have non-SEC claims, they are not going to get that dose.

Whereas, otherwise, if it is considered a separate stream, as part of the uranium process stream, even though it is a thorium isotope, it is a daughter product of the U-238 and a different process altogether. If that is kind of taken out of the SEC, then it allows these people to get a more complete dose assessment or higher dose assessment, more claimant-favorable dose.

MEMBER ZIEMER: Have we said we can't do that otherwise?

MR. BARTON: The problem is the language just says thorium, even though --

MR. STIVER: The language in the

SEC says thorium.

MEMBER ZIEMER: Yes. Yes, I see the point you're making.

MR. KATZ: The question is not what the language says, but what the analysis was based on.

MR. STIVER: And what was the intent.

MR. KATZ: Right.

MEMBER ZIEMER: In other words, can you reconstruct that part of it? Is that what you're asking?

MR. HINNEFELD: So, the question is -- now we are talking about the entire operation of the refinery and what was the raffinate exposure. Now the earliest operation of the refinery completed like silo 2 and silo 3, so the K-65 and the whole metal oxide. That was the Q-11 ores, I believe, that generated those two things.

And then, at some point, raffinate started being pumped into pits, you know, slurried into pits. So, there is a question of was there any really potentially internal exposure in the handling mechanism. It seems like it would be modest at best.

But I am trying to think of how -- for those modest or limited number of people who might be exposed, I don't know how you would do it.

MR. STIVER: Yes, I think that is really the hook, the rub here, if you will. How would you identify those people?

MR. HINNEFELD: Well, if you could, how would you do it?

MR. STIVER: If you could, how would you do it? Well, one method I could put out there -- I mean, I don't know if it would be considered sufficiently accurate for dose reconstruction purposes -- but you just assume that, for that period of time that anybody could have possibly been in that facility and they could have been exposed, in which case you would take their urine bioassay and, then, give them an amount of thorium-230 in addition, based on the ratio, that was in that particular material. Now, maybe not grossly but that would certainly overestimate real intakes for most people. But I don't know how you could do it unless you could identify who were the workers in those particular buildings at the time.

MR. ROLFES: It would be pretty gross just because of the low uranium content of the waste pits.

MR. STIVER: Yes. Basically, when they extracted the uranium from the radium, the thorium remains. The amount of thorium is pretty much a constant amount. It was about the same between silos 1, 2, and 3. It is just that the relative abundance compared to what you would use as an indicator in radionuclides is quite a bit different.

MEMBER ZIEMER: If you had somebody currently that didn't meet the 250-day environment or had a kind of presumptive cancer, would you be giving them anything from the raffinate stream at all?

MR. HINNEFELD: No, no.

MEMBER ZIEMER: Because?

MR. HINNEFELD: Well, I don't think we have built in a technique. I mean, the Site Profile doesn't say anything about raffinate exposure. I mean, it was uranium exposure from the bioassay.

MR. STIVER: Well, I think it was a technique that was in development. It is in Report 52, and there were discussions in 2010.

MR. HINNEFELD: How it could be done?

MR. STIVER: Yes, as to whether it could be done, and Mark was involved.

MR. ROLFES: Yes, right. That was primarily, you know, relying upon the DWE data from plant 2/plant 3 to reconstruct thorium-230 exposures. And in our discussion, we found, you know, that since it was a wet process, the air concentrations in those areas were really low. I mean on the order of like 10 or 20 dpm per cubic meter.

And one could assume, you know, a continuous exposure at that concentration to thorium-230, assuming that was the major contribution to what was observed on the air samples. But, then, when the DWE issue came up, there was reason to generate an SEC determination. That sort of left that hanging out there.

MR. STIVER: Yes, it was kind of a situation where --

MR. HINNEFELD: Okay, I see now. That's helpful.

So, if we were to make the assumption that the air-sampling data in plant 2/3 from the review when they were doing the DWE prep data, we said, well, this is going to be mainly -- this is a raffinate exposure, and let's treat this not as a uranium airborne, even though it may have been reported. It was counted as an alpha count --

MEMBER ZIEMER: Right.

MR. HINNEFELD: -- when it was done. Let's say this is a thorium-230 intake, right? Is that what was proposed? And then, say we could assign thorium-230 intakes based on those DWE data for those years.

MR. ROLFES: Correct.

MR. HINNEFELD: And then, that will cover most of the operating period. Well, that DWE data is actually a little older. It goes back to what, around the seventies?

MR. ROLFES: These DWE data cover like the fifties --

MR. HINNEFELD: Fifties, sixties, but they go up through about 1970.

MR. ROLFES: 1968, correct, yes.

MR. HINNEFELD: Okay. So, then, that leaves us -- you know, we could extrapolate those data based on, say, production numbers. I think we might be able to find the refinery production numbers in some historical documents, like historical release documents or something like the throughputs of the various plants.

MR. ROLFES: Yes.

MR. HINNEFELD: Because the DWE data stops at, you say, '68.

MR. STIVER: 1968 is when they basically stopped altogether.

MR. HINNEFELD: Okay. So, the

refinery did operate some after that. It was not a full-time operation, I don't think, after about 1970, but they would run campaigns, a refinery campaign. And then, those operators would move over to plant 4 and they would run that campaign.

So, we could extrapolate that based on sort of a throughput kind of thing for the remainder of the period. And then, the refinery maybe ran once in the eighties. It didn't run --

MEMBER ZIEMER: But what I was asking was, the SEC itself did not include this as something that couldn't be reconstructed based on --

MR. HINNEFELD: It wasn't part of the analysis we did when we --

MEMBER ZIEMER: Right. Right. That's what I'm saying. So, it is fair game to consider it.

MR. HINNEFELD: Yes.

MEMBER ZIEMER: Yes.

MR. STIVER: You can certainly consider it as, you know, a different technique. I mean, we have done that. We have used DWE data for other --

MR. HINNEFELD: So, then, what we need to do is propose something.

MR. STIVER: Right.

MR. ROLFES: The question is, can we use the DWE data, because the DWE data were said not to be good for --

MEMBER ZIEMER: Well, that is sort of what I'm asking, yes. Are they tied together?

MR. HINNEFELD: The DWE data were decided not to be good for thorium exposures at the various plants because the DWE data wasn't really thorium.

MEMBER ZIEMER: No.

MR. HINNEFELD: In this instance, we are going to say, in all likelihood, the plant 2/3 airborne data was thorium-230.

MEMBER ZIEMER: Right.

MR. HINNEFELD: That's what we are going to say.

MR. STIVER: It was probably after because there is certainly depleted uranium.

MR. HINNEFELD: In reality, at the end of plant 2/3 was where they sucked the UO3 out of the reduction pump. So, those samples would have been uranium, and the DWE studies might actually let us exclude those. I don't know. I would have to go back and see how much actual data -- some of those DWE studies gave the job title and job and what result was associated with that job.

And so, it would be called the gulping station. That is what they called it. That is where they sucked the product UO3 out of the final boildown, the denitration pump.

And so, I'm just talking out loud here. You could exclude, if the DWE study was specific enough, you could exclude those air data from your plant 2/3 compilation, consider the rest, you know, for simplicity purposes, or maybe just for simplicity purposes, use 2/3. I mean, what does it matter? Just say, okay, these are thorium-230 samples and we consider that for thorium-230 intakes, because that is where the exposures would have occurred. Since we probably won't know who was there, we would do similar kinds of things. If there is no reason to exclude this person, they will be included, kind of thing.

MR. STIVER: Yes, that is kind of what we are envisioning, something along those lines.

MR. HINNEFELD: Okay. I'm glad there is a transcript of this meeting because

I didn't take any notes when I was just talking.

(Laughter.)

MR. BARTON: Did you make all that up, too?

(Laughter.)

MR. HINNEFELD: Yes, that I really was making up.

(Laughter.)

MR. KATZ: In real-time.

MEMBER ZIEMER: Well, even so, it made sense.

(Laughter.)

MR. KATZ: It made lots of sense. So, there is a path forward there.

MR. HINNEFELD: Like we were talking yesterday, you don't want to cheat anybody out of any non-presumptive cancers out of anything. And if we can do something there, that might be worth doing.

MR. STIVER: Yes, that is something

we can look into for the next get-together that we have.

MR. KATZ: So, this is in progress.

MR. STIVER: Yes, keep it in progress.

MR. BARTON: I guess really the first question was, was that in the SEC --

MEMBER ZIEMER: You know, is it even in progress?

MR. BARTON: And if it is fair game --

MR. KATZ: Yes.

MEMBER ZIEMER: It hasn't really started yet, right?

MR. KATZ: As soon as you discuss it, it is in progress.

MEMBER ZIEMER: It's in progress, okay. Okay.

MR. BARTON: We need formal definitions for those terms.

(Laughter.)

MEMBER ZIEMER: Yes, we have them. Wanda has defined them. So, we use that, right?

MR. STIVER: Okay, we're going to settle on in progress in 7 and 8.

MEMBER ZIEMER: In progress.

MR. KATZ: And 8, is that what you said?

MR. STIVER: Eight is basically related to 7, only it is who is going to get the model --

MR. KATZ: Okay.

MR. STIVER: -- that's going to be applied.

CHAIR CLAWSON: And that we actually kind of just went over?

MR. KATZ: Yes.

MEMBER ZIEMER: It's the same thing, right?

CHAIR CLAWSON: It's the same thing.

MR. ROLFES: They'll be in progress until they are able to develop the model.

MR. STIVER: Okay, that brings us --

MR. ROLFES: Someone on the line just asked if we could all speak up a little bit, that they're having trouble hearing some speakers.

MR. KATZ: Oh, dear.

MR. ROLFES: Thanks.

MEMBER ZIEMER: I've got a microphone over here. Have you got one there, Brad?

CHAIR CLAWSON: Yes.

MR. ROLFES: It was Bob and John. I guess they were having trouble hearing.

MR. HINNEFELD: Is that a phone microphone down there? That's the recorder's microphone.

CHAIR CLAWSON: Okay, this is the microphone we're using, right?

MR. KATZ: I don't know if that microphone is --

MEMBER ZIEMER: The phone microphone is --

MR. KATZ: Yesterday we had three.

MR. HINNEFELD: The phone microphone is this, right?

MR. KATZ: Yes.

MEMBER ZIEMER: It's the flat thing somewhere, or is it?

MR. KATZ: Right. Yesterday we had one down there.

CHAIR CLAWSON: Oh, it's actually down there, Paul.

MEMBER ZIEMER: Shall we move it down.

MR. KATZ: Well, Paul needs to be

heard, too.

MEMBER ZIEMER: Well, I can move it that way.

MR. KATZ: We had three yesterday.

MEMBER ZIEMER: Is there another one?

MR. KATZ: Try that. Well, try that, and we will see if people have problems hearing Paul.

CHAIR CLAWSON: How about that? Can people hear us better?

MR. STIVER: Can you hear me now?

(No response.)

MR. HINNEFELD: Anybody on the phone?

MS. LIN: Yes. Yes, loud and clear.

MS. CHALMERS: Yes, that sounds good.

MR. KATZ: Okay.

MR. STIVER: Okay, the next one, we will move on to -- hang on for just a second. I'm having a slow response on the link here.

MEMBER ZIEMER: Probably 9.

MR. STIVER: Actually, it's Number 10.

MEMBER ZIEMER: Oh, you show 9 as in abeyance. So, that is just sitting there.

MR. STIVER: Yes, these are ones that were --

MEMBER ZIEMER: Yes, it stays, right?

MR. STIVER: Yes. I didn't want to go back over ones that were in abeyance that we don't have any additional information on to move forward.

MR. KATZ: Right.

MR. STIVER: Rather than just kind of restate what have we done in the past.

Let me change the view here where we

can scroll down, kind of jump back and forth.

Okay, the next one that was kind of interesting that came up last time was Finding Number 10. And this is this notion that the radionuclides list, the recycled uranium in the TBD is incomplete. And we talked about this a little bit at the last meeting, this notion of what do you do about americium-241.

And I know Stu at the time had questioned whether it should even be included in recycled uranium at all. I mean, this is a nuclide that was not addressed in the DWE reports on recycled uranium. They looked at plutonium, neptunium-237, and technetium-99 almost exclusively. And so, our review of recycled uranium, which is quite extensive, focused on those three constituents.

I tracked down the source of the mention of americium-241 to the actual TBD 5. I sent an email to Stu about this, and he was going to look into a bit. And I looked into it, too.

It turns out that this may be more important than we figured in the past. We kind of went on the assumption that, if DOE only mentioned americium in passing and never really did any analysis and gathering of data related to it, then there must have been some good reason for it, that it existed in such low levels that it wasn't really worth getting into.

However, when I looked into the production mechanisms for americium, it is basically a serial neutron capture reaction starting with plutonium-239, up to -240, to -241, which then betas at about a 13-day half-life, I believe, if I have got that right, to americium-241, which, then, alpha decays at a 432-year half-life to neptunium-237.

So, what we have is a situation

where americium-241 is the principal mechanism of producing neptunium-237. Neptunium-237 was considered a nuclide of interest in the DOE reports, for which values have been determined in NIOSH's model. But, yet, americium, which is the precursor to neptunium, which actually has dose conversion factors that are about a factor of two higher than neptunium for most organs, isn't included. It also has a very high specific activity, about 3.7 curies per gram.

So, this is kind of the situation where it looks like if you have neptunium and you can't determine that americium may have been extracted from the waste stream before it was shipped to Fernald, you have got a situation where you are going to probably have to reconstruct doses of americium.

And so, this is kind of preliminary, but I guess this would be something for you guys to take a look at. Is there a reason why americium was excluded to begin with in the original DOE documentation? Can we ignore it? If not, how, then, would we go about reconstructing doses?

MR. HINNEFELD: This is Stu.

I question the principal neptunium production avenue that you describe. I don't know that that is the principal neptunium production. I thought the principal neptunium production was a non-fission capture of U-235, because only five out of six captures fission.

MR. STIVER: Yes, there are different mechanisms involved. I talked to our radiochemist about this, and he seemed to think that the plutonium capture is probably the most significant, at least for the weapons-grade materials.

Yes, certainly in U-236 --

MR. HINNEFELD: Yes, U-236 neutron

capture gets you to neptunium-237.

MR. STIVER: It is a very small type of --

MR. HINNEFELD: I guess the information I have received since we exchanged emails was that the americium was more commonly a contaminant in high-enriched recycled uranium; that, and the -- what was the other one we mentioned? -- americium and the -- yes, I guess that is the only one.

But it is primarily a contaminant in the high-enriched uranium recycle rather than the low-enriched uranium recycle. And so, that is why places like Fernald didn't look for it, but a place like a gaseous diffusion plant that was running higher-enrichment materials would have to worry about it.

So, I got like an email explanation of that. So, I think maybe what we have to do -- I just have always thought that -- and I'm not a nuclear chemist -- but I just always have thought that the obvious neptunium-237 production is you've got a lot of uranium in these reactor cores and a lot of uranium-235 in these reactor cores. And so, I think it is five out of six captures result in fission, and that results in U-236, which would have some sort of data capture process and become neptunium-237 in all likelihood.

So, that always just seemed like the likely one because, otherwise, you are having the serial neutron captures to 239 to 240, to 241. They're getting the plutonium-241 in order to get back down.

So, you have a longer chain of serial captures to get through the plutonium chain than you do for the U-236 chain. So, that just seemed more probable to me. But, like I said, I don't know nucleonics, but, presumably, we can do some search on that.

MR. STIVER: Yes, I think that is something we just need to run to ground.

MR. HINNEFELD: Yes.

MR. STIVER: You know, what could be the primary mechanism and what would be expected.

MR. HINNEFELD: And then, I will also check on the -- we will have to also, rather than just get an email message about this, we will have to look at sources and what sources did this email message come from, and what is the source of information that americium-241 was mainly in high-enriched uranium recycle.

MR. STIVER: We might also look at the sources with the neptunium that were used in the reconstruction. I think we used the highest micron percentiles, and that came from one of the source streams, but I don't remember off the top of my head which one it was.

But you might narrow down your

search as to the americium that would be more associated with higher concentrations from particular waste streams as opposed to other --

MR. HINNEFELD: I'm sorry, where are we going here now? I'm lost.

MR. STIVER: I was saying that, if you look at the review we did and the particular waste streams that were used to determine the bounding value for neptunium-237, it might help to kind of narrow down your search as to what americium content might have been associated with that.

MR. BARTON: Part of the reason this piqued our interest was that it was listed as a primary contaminant of concern in the original TBD. So, maybe there is good reason in the next iteration to remove that for --

MR. STIVER: Yes. If it turns out it is not, then --

MR. HINNEFELD: Yes. Yes, right.

MR. BARTON: But there is also, I know we came across at least one document while Fluor Fernald was running the site. And it is called Handling Uranium Containing Other Radiological Constituents. And I don't want to read the whole quote, but it said, essentially, recycled uranium can contain trace quantities of plutonium-238, 239, and 240, americium-241, and neptunium-237. These isotopes can have significant internal dose contributions for relatively small activity concentrations.

So, that is sort of the reason we just, you know --

MR. STIVER: I might add --

MR. BARTON: -- might establish that we don't need to take a look at it or perhaps --

MR. STIVER: I might also add to that the DOE 2004 report on recycled uranium doesn't mention that, and it is basically not saying probable line, those particular nuclides. But that never addresses it in terms of accountability.

MR. KATZ: So, we'll put this in progress, too?

CHAIR CLAWSON: Yes, but help me clarify something on this because we have been talking about a lot of isotopes. This is part of the raffinate stream?

MR. STIVER: No, this is not raffinate.

MR. HINNEFELD: No, recycled uranium.

CHAIR CLAWSON: Recycled uranium, okay.

MR. STIVER: We've reached the RU now.

CHAIR CLAWSON: Okay, that's what I wanted to --

MR. STIVER: Remember the long discussions we had about whether 200 or 400 parts per billion plutonium were going to be bounding. And so, there were three principal nuclides, plutonium, neptunium and technitium-99, for which we have bounding values for different periods of time now.

MEMBER ZIEMER: But your original finding does mention the raffinates. Your finding does. We're talking about 10 here, right?

MR. STIVER: Yes. Finding 10 is really related to recycled uranium.

MEMBER ZIEMER: Yes, I understand, but it says, furthermore, the concentrations of trace radionuclides in the raffinates --

MR. STIVER: You know, where this comes from is some of the materials that contain these were -- actually, some of the ones that had the highest values were the reduction pot liners. Remember the magnesium fluoride, which is going to be concentrate neptunium and to some extent plutonium. I believe strontium is another one. I'm trying to think of all of them.

But the point being is that some of these raffinate products would find their way as sources of these materials --

MEMBER ZIEMER: Right, right.

MR. STIVER: -- through the production mechanism.

MEMBER ZIEMER: Right.

MR. HINNEFELD: I think the bounding values take that into account, the higher --

MEMBER ZIEMER: The higher, yes.

MR. HINNEFELD: The bounding values are quite high compared to what you would normally see.

MEMBER ZIEMER: Yes.

MR. STIVER: I think it was like we settled on 10,000 parts per billion or --

MR. HINNEFELD: I forget what it actually was, the numbers that we arrived at, but they are much higher than you would see in production uranium.

MR. STIVER: Yes.

CHAIR CLAWSON: But the radionuclides or the nuclides that were of concern in this section -- because I thought we had talked about most of this is americium-241?

MR. HINNEFELD: Yes, right.

MR. STIVER: Just to determine A) is it really something to be concerned with and, if so, how might we go about accounting for it?

CHAIR CLAWSON: Okay. I want to be clear because a lot of these sites are kind of running together. I know that we did talk about neptunium, but that is more at Hanford.

What is the half-life of

americium-241?

MR. STIVER: Four hundred and thirty-two years. So, it is important from a dosimetric standpoint.

CHAIR CLAWSON: I understand. That clarifies it.

MEMBER ZIEMER: Longer than the life of the Work Group.

(Laughter.)

Is the ball in NIOSH's court then?

MR. HINNEFELD: Yes. Yes.

MEMBER ZIEMER: Okay.

MR. HINNEFELD: I think the ball is in our court, is to provide the backing to our statement --

MEMBER ZIEMER: Yes.

MR. HINNEFELD: -- that this was a high-enriched uranium issue and, then, also, see what we can find out about the production mechanism for neptunium-237. I had never thought of it as a decay product of americium-241. It would never be by itself without americium --

MEMBER ZIEMER: Yes.

MR. HINNEFELD: -- unless you purposely extracted one or the other.

MR. STIVER: I know that is a production mechanism for using it for this type of research, and so forth, is to produce at a reactor now, to the extent that that happened in production after this point. And so, it is up for debate.

Okay, now we jump ahead, 25. This is something that is near and dear to the heart of Hans Behling. And this is about the radon releases from the K-65 silos.

And now, Stu, you produced a couple of responses.

MR. HINNEFELD: Yes. I will start.

MR. STIVER: So, you might want to start out with those.

MR. HINNEFELD: Yes, I will start out with my life since the last Work Group meeting.

(Laughter.)

Well, not my life, but some of the stuff I've done. Sometimes it felt that way.

At the last Work Group meeting I decided, look, we have this question. We have what I'll call the SC&A method of estimating radon emissions, largely prior to 1979, when the silos were, we call it sealed, is usually the term that is talking about. What they did was blanked off what had been an open gooseneck port that had been open to the atmosphere, and they also put gaskets and flanges on some of the other openings, some of the other penetrations that they gasketed up and, certainly, sealed it more than it had been before.

There was an -- SC&A has proposed a method that differs probably by at least an order of magnitude in terms of annual radon releases than the method that was performed for ATSDR by a company called Radiological Assessment Corporation, or RAC. And so, I will, for the benefit of the court reporter, I will frequently use the term RAC during this conversation probably, and I am referred to R-A-C, Radiological Assessment Corporation.

SC&A's approach, SC&A looked at a set of sampling data which were in, they were reproduced in the RAC report. These were data collected in 1991, samples taken from the K-65 materials, residues themselves in the K-65 materials, and the relative activities reported in those samples for radium-226 and lead-210.

Lead-210 is a decay product of radon. It is the one that has -- it essentially stops the short-lived decay daughter chain. You know, you have several short-lived decay products that we typically call the radon progeny, and then, it gets to lead-210 with a 22-year half-life. And we don't have a short-lived half for that sort of thing.

Lead-210 is radioactive. It decays by a beta minus to bismuth-210, which is another beta minus decay to polonium-210, which is alpha down to stable 2 lead, so stable lead-210. So, we are all the way down at the end of the radium decay chain here.

Now in the reported activities for lead-210 and radium-226 there is a significant discrepancy from that sampling between those relative activities. Now, if you had a perfectly-sealed container, the logic of the SC&A approach is that, if this was perfectly sealed or even very well sealed, those numbers would be either the same, if it was perfectly sealed, or close to each other because radon has a short half-life. And so, you would reach an equilibrium. If it were a tightly-sealed container, you would reach an equilibrium pretty quickly, well, as quickly as the lead-210 grew in.

And so, you would think those would be relatively close to each other in terms of activity. And they're not. The way to explain for this deficit in activity is that the radon escaped. And so, it wasn't there to generate the lead-210.

There is a second piece of information that supports SC&A's argument. That is direct radiation survey measurements that were taken on the top of the silo at various times in its history. Now the relevant times for our discussion right now are measurements that were taken before 1979, before the silos were sealed; measurements that were taken after the silo was sealed, typically, even right up to 1987, and then, measurements that were taken in 1987 after the operation of a recently installed radon treatment system, which was designed to remove radon from the headspace of the silos, pump it out through charcoal and absorb it on a charcoal filter. So that you have a direct radiation reading now with essentially the silos devoid of radon gas.

SC&A compared the dose rate readings after the radon treatment system operation to the dose rate readings prior to sealing, and they said these numbers look like the same to us. It looks like there was no radon being retained in the silos when they were unsealed. Whereas, after they were sealed, it did build up. There was radon being retained in there.

So, based on that, they said it appears that this deficit between lead-210 and radium-226, this activity deficit is because the radon left the residues. Once it left the residues, it wasn't retained in the domes. And so, it was released to the environment. So, in simple terms, that is how SC&A arrived at their conclusion.

Now there are some complications about you don't know the starting ratio of lead-210 and radium. That complicates matters. SC&A chose a sort of middle of the road. It doesn't maximize or minimize. You would maximize release if you assume they were placed at equilibrium, and you would minimize the release if you assume there was no lead-210 at the original placement.

And then, it also I think important to note that the sampling was done in 1991, 12 years after the silos were sealed. And therefore, you would have some radon. You know, we know some radon was certainly retained in the headspace. So, that would tend to indicate that the lead-210 was probably even lower than what SC&A's calculated estimate was. You know, the lead-210 was even lower at the start. And so, the radon emissions up until 1979 were probably even higher than what was in the actual paper, the 2008 paper that was delivered. So, that is kind of their technique.

Radiological Assessment Corporation had that same sampling data. You know, they had it in their report, and they did not elect that method. They said that they did use the direct radiation measurements from the tops of the silos though, but they didn't start with the pre-1979 emission rate. RAC started with the 1979-to-1987 period and said, during this time, we have radon concentration measurements from the silo headspace.

So, we have an estimate of what the

radon concentration was in the air. And we can estimate a release rate based on thermal expansion of the air because it was pretty well observed that during this period, as the air warmed-up, the emissions were higher. The air concentrations, the radon concentrations measured in the air close to the silos was higher on warm days in the afternoon, when it was hottest.

And so, there were also temperature readings inside the silo that they made a correlation with the outdoor temperatures. And they said, well, based on this, we would calculate that in a year you would have this daily thermal expansion based on how much the temperature changed on that day, and then, that is how much radon you would pump out. And then, you would also have some radon that would diffuse through the concrete dome. And they used a classic radon diffusion calculation with some known and some assumed properties of the concrete and the measured concentrations that they had here.

So, they generated that release rate and said, well, from this release rate from the silos, you know, radon released from the silos, and this known concentration, you have a classic equilibrium differential equation here and the amount of radon diffusing from the residues into the headspace has to equal the amount of radon being removed from the headspace into the atmosphere.

And so, based on that, they arrived at, well, their release rate was not based on the diffusion, but the release rate was based on the concentration, the thermal pumping and dispersion.

Now the issue we run into when we are trying to -- I was trying to reconcile this. I said, how can you explain both of these approaches? You know, what could be going on that explains both?

And I reached the conclusion that you can't. You can't reconcile that lead-210 and radium deficit with what I would consider the known behavior of radon in residues. After RAC had an estimate from 1987, or from 1979 to 1987, during the period when it was sealed, after they had that estimate, they also said, well, now we know how much the airborne concentration was in 1987. We've got these dose rates before the RTS was run. We have the dose rate measurements after the RTS was run, and we have these dose rate measurements from before the silos were sealed. They said, we should be able to develop a sort of dose rate per radon concentration factor based on using those external measurements.

So, when they compared the post-radon treatment system measurements to the pre-sealing measurements, they said, well, these aren't exactly equal; there is a difference here, and that difference is about 10 times less. It is maybe on the order of 20 millirem per hour. The difference between the post-RTS sampling and the 1987 pre-RTS pumping is about 200 millirem an hour. And I am speaking for medians here. They actually did Monte Carlo calculations to compare distributions of the measurements. And so, the concentration must have been about 10 times lower in the headspace before 1987 than it was after -- or before 1979 than it was after 1979, when it was sealed.

So, that is what RAC was. They looked at the same external monitoring data and said the pre-1979 and the post-RTS operation time are not quite the same; there is a difference there. And so, that is how they arrived at their estimate of concentration in the air.

And then they arrived at a differential diffusion of radon, but it is the diffusion of the radon out of the silo materials into the headspace, is the key question here. And that is the key. That is where the two mechanisms just cannot, in my mind, you cannot reconcile.

Whereas, SC&A's method has, as they wrote in 2008, roughly 60 percent of the material, of the radon being generated in the residues leaving the residues and entering the headspace. And they say it could be more than that.

The Radiological Assessment Corporation estimate puts the amount, the fraction of radon that would diffuse from the residues into the headspace more on the order of 5 percent as opposed to 60 percent. And so, that accounts for essentially your factor of 10 difference in residue.

The RAC also felt like pre-sealing the domes retained the radon very poorly, something like 93 percent of the radon that entered the headspace left. So, they didn't feel like the silos, before sealing in 1979, they didn't feel like they were effective in retaining the radon, just as SC&A doesn't feel they are effective.

The key element comes down to how much radon diffused from the residue materials into the headspace. And so, I can't explain the deficit between lead-210 and radium-226. You know, anything I would say would be rank speculation. I can't.

But, on the other hand, in its report, you know, RAC's report is 150-200 pages long. The appendix in their 1995 report where they talk about how they did their calculations, it is well over 100 pages long.

I think it is close to 200 pages long.

And they speak at great length about how does radon behave in things, in materials. Now a lot of these parameters were not measured in the actual K-65 residues themselves, like radon diffusion length, emanation fraction. Those are the two key ones. Those were never measured in the K-65 silos that we've been able to find.

MEMBER ZIEMER: Well, we discussed a lot of this before.

MR. HINNEFELD: Yes.

MEMBER ZIEMER: And I know Hans did a pretty careful analysis. I am trying to remember the amounts of the residue, and they were pretty thick.

MR. HINNEFELD: They were about 20-feet deep.

MEMBER ZIEMER: Twenty-feet deep. And it is intuitively hard for me to see that

60 percent of that inventory would reach the headspace unless the material is very loose, and no one has done a diffusion measurement. We don't know diffusion length.

I mean, a lot of that, you start out saying that half of it is going to go the other way, Number 1.

MR. HINNEFELD: Right.

MEMBER ZIEMER: So, it is hard to see how you would get more than 50 percent to start with. And then, if the distances are enough, a lot of the decay occurs before it ever gets out.

And so, I don't recall what the assumptions were. There have to be some assumptions about, you know, if the stuff is pretty solid, it makes a difference, versus things where there is like chimney effects like you have in the Pennsylvania Reading Prong where somebody house, you know, the Watras house, was it, that had all the radon, but where things can come up by some sort of a chimney effect.

MR. HINNEFELD: Well, I think that is part of SC&A's approach, is there may have been a chimney effect on the silos.

MR. STIVER: Yes, Hans has actually prepared sort of a final --

DR. BEHLING: Yes.

MR. STIVER: Maybe, Hans, this would be a good time for you to jump in.

DR. BEHLING: Yes. I am hoping to be able to get a chance to counter some of these issues.

MEMBER ZIEMER: No, I'm asking because I don't remember from before. I know you had some good arguments for it, and I just couldn't remember that.

DR. BEHLING: Okay. Well, if I have a chance to give my presentation, hopefully, I can clarify some of those issues.

MR. STIVER: Okay, the floor is yours, Hans.

DR. BEHLING: Okay. Let me just briefly, again -- I think Stu did a very nice job about summarizing some of the things, but there are a couple of areas that I tend to disagree with.

Let me just start out by saying that the SC&A model relies principally on two sets of empirical measurements, measurements that I will take at face value because I have no other choice but to.

In addition to two sets of empirical data measurements, SC&A's estimate also had to rely on one particular assumption. And that is, what was the starting disequilibrium between the radium-226 and the lead-210? And so, what I want to do is identify really the empirical measurements that were used in presenting our model and explain how they were used, and then, also, briefly explain the one assumption that had to be incorporated.

And what I want to do is to describe -- and I think Stu already mentioned it -- there are two phases to this explanation. What were the releases of radon from the waste package inside the silo that escaped from the waste package, but not necessarily into the environment? And the second stage of the explanation is, what happened to the radon that did escape the waste package that may have been in the headspace and was subsequently released to the environment? So, those are two aspects of our model that I will explain in short order.

And what is really important now is also to understand what are the principal players. And I think Stu already mentioned the two major players for this assessment are, obviously, radium-226, which has a half-life of

1622 years, meaning that over the period during which this material was first harvested at the Belgian Congo and the time it was in place in the silos and retained in the silos are relatively brief periods which, by and large, did not really significantly affect the quantity of radium that was, then, obviously, the source term for radon-222.

Conversely, the second player in this whole issue is lead-210, and lead-210, as Stu already mentioned, is near the bottom of the decay chain, but follows the radon-222 radionuclide which has only a 3.8-day half-life. But, at 22 years of half-life, it will, obviously, have a variability in terms of what the starting point might have been, as I will explain.

So, let me talk about, when radon-222 is released, whether it is in the ore or while it is in the silos, it has, if it escapes, the impact of not contributing to any more of additional lead-210 that you will find.

So, let's start off at, what are the potential options for the one assumption that we had to really make in our calculation? If you start out with the fact that uranium ores are usually mined from deep mined strata, you have to also come to the conclusion that at the time of the mining the ore that now contains all of both uraniums, 238, 234, the radium-226, the radon-222, and the lead-210 are likely to be in full equilibrium. In other words, if you were to take a sample at the time that the ore was harvested, you would end up looking probably at a ratio between radium-226 and lead-210 that is probably close to unity, meaning that very little radon escaped, especially if the ore was mined at a deep strata.

So, in essence, we would start out with the simple assumption that, if we were to somehow or other take ore when it is immediately mined and extract the uranium and establish raffinates that are close to time of harvesting of the ore, we would start out with a raffinate that would have an equilibrium value between radium-226 and lead-210 that would essentially approach unity.

But this was not the case here. So, let's try to figure out what would be a reasonable starting point in terms of the disequilibrium between these two players, radium-226 and lead-210. Let's remember that the ore, the Belgian Congo ore, was assumed to be mined in 1944. And then, these raffinates were generated both at Mallinckrodt and at Fernald, and they were placed in the silos as early as 1953 and as late as 1958.

If they had been placed in 1953, that is nine years removed from the time they were first harvested, and if during that period of time, the full nine years, 100 percent of all radon-222 had escaped from the ore, you would still end up with a starting equilibrium fraction of 0.75. In other words, the lead-210 would have the activity of approximately 75 percent of that of radium-226.

If, in fact, the time period between harvesting and emplacement in silos 1 and 2 was at the far end of the spectrum time period of 14 years, the starting point for the ratio between lead-210 and radium-226 would still be .64. And that is assuming that we start out with an equilibrium fraction of near unity for those two radionuclides and, also, that during this nine- to fourteen-year time period all 100 percent of the radon would escape.

Now there have been discussions that perhaps the ore that was emplaced in there had initially been forwarded to the United States with the assumption that they would be returned because they contained certain precious metals, including radium-226 and perhaps lead, that would be separated.

But I did a very intense survey of available data, and I am sure that NIOSH did, too. There is no documentation that that extraction of the precious ores was ever conducted. And if they had been done, the radium would have also been removed along with the lead, so that we would basically have another variable that we couldn't explain. But there is no justification to believe that that was ever done.

Now the only other factor that could potentially create something of a distortion between the two indicated radionuclides could be the actual extraction of uranium during this very process. And we do know that in the initial steps in the processing of raw ore, it involves mechanical crushing, the grinding in order to produce uniform-sized particles, and then, also, the treatment with either an acid- or an alkaline-based leaching process.

We don't know what that could have possibly been done, but on the assumption that it probably wouldn't have affected them very much, we are still stuck with understanding that an equilibrium fraction at the time these materials were placed into silos 1 and 2 could have been as high as .7, depending on the timeframe of either nine years or fourteen years. And that is strictly based on the fact that lead-210 has a half-life of 22 years.

So, what we ended up doing is looking at the actual empirical measurements, as Stu had mentioned earlier. And those measurements were taken in 1991 where they went in there and at various levels within the waste package, they retrieved samples randomly and decided to assess those particular materials for their current levels of lead-210 in relationship to radium-226.

In 1991, in silo 1, that ratio was down to 0.37, and in silo Number 2, it was 0.38. So, it was essentially equal. In other words, if at that point you can trust your measurements, the absence of this equilibrium that we now observe would suggest that perhaps as much as 62 percent may have escaped the waste package and perhaps entered the headspace.

Then, again, in 1993, a second set of measurements were taken. In silo Number 1, the disequilibrium was defined at 0.42, not much different from the earlier version of 0.37. So, that has probably been a statistical error of those two measurements. For silo 2, on the other hand, this disequilibrium of 0.38 had changed to 0.72.

In selecting which one I was going to use, I decided to be claimant-unfavorable by using the data that was generated in 1993 that says the disequilibrium in silo Number 1 was 0.42, but for silo 2 it was 0.72. And so, I intentionally used those two values as my starting point for saying what quantities of radon may have been released from the waste package into possibly the headspace, but not necessarily into the environment.

And so, if I look at those two latter datasets of 1993, my assessment would have been that about 58 percent of the radon that was generated in the waste package in silo 1 left the waste package, and for silo 2, 28 percent left the waste package. And that is strictly assuming that these disequilibrium values are legitimate and that our starting point was using the same disequilibrium as we observed in 1993.

And as I said, I believe that assumption about a starting point being equal to what it was 1993 and projected backwards to the time of emplacement is a very unconservative and claimant-unfavorable starting assumption. And so, on that basis, I was able to calculate the total quantity of radon that was released from the waste package into the headspace. And I cite those numbers in our calculation, and those numbers represent somewhere around -- let's see here -- 90,000 curies for silo 1 and about 24,000 curies for silo 2.

So, at this point, the argument in the past has been, well, whatever radon left the waste package, but now entered the headspace, in all likelihood most of it or the majority of it decayed in the headspace. And that became the second phase of our investigation.

And the second set of empirical data, then, became really the data that Stu referred to earlier as being measurements that were taken on top of the silo in earlier years. And I am talking about the years that are identified in one of the exhibits that were included in my 2008 White Paper.

And in April of 1964, again in May 1973, and again in July 1973, a series of dose rate measurements were taken on top of silos 1 and 2. And at that time, the average dose rate -- I don't want to give each of the numbers -- but they averaged approximately around 70 to 75 millirem per hour.

Now one can conclude that those dose rate measurements taken on top of the silos were perhaps the combined dose rate contributed from the radium that was still left in the waste package below, as well as the presence of radon and their short-lived daughters in the headspace. And as we all know, there are some short-lived radiation emissions from the short-lived daughters that are gamma emitters.

And so, they would contribute if, in fact, radon was a major component of that dose rate that was measured on top of the silos. The short-lived daughters of the radon that had accumulated in the headspace would be a contributor.

And so, we have dose rates on top of the silos that were measured prior to the sealing of the dome in the sixties and early seventies that would suggest that the dose rates on average on top of silos 1 and 2 was around 75 millirem per hour.

In June of 1979, there was a significant effort put forth to seal the dome caps in order to prevent the radon being released into the environment. As Stu had mentioned, there was a gooseneck, a 6-inch gooseneck that openly allowed the air in the headspace to enter the atmosphere outside.

In addition, there was a whole series of manholes that did not have a seal.

And also, there were serious, serious cracks that also allowed the release of any gases that may have accumulated in the headspace into the environment. And so, in the process of ceiling them up, they eliminated any open, direct openings, as well as also sealed many of the cracks.

And then, in 1987, measurements were taken on top of the dome. We, obviously, realized that the dose rate on top of the dome had now gone from approximately 70 to 75 millirem in silo Number 2 all the way up to 250 millirem per hour, and in silo 1, around up to 200-and-some-odd millirem per hour.

And it was realized that a person who might work on the top of the silos over a period of eight hours would be exposed to well over a rem and a half. And so, there was the reason to introduce the radon treatment system.

And the radon treatment system did

one thing. It was operated for several hours at a time until the reduction in dose rates ceased to come down any further. And it was assumed that that period of time, usually in a matter of hours, had removed 97 percent of the radon gases and, along with the radon gases, all of the short-lived daughters.

And then, if you look at the dose rate measurements following that radon treatment system, the dose rates from over 200 millirem per hour were reduced to levels that actually look very close, if not identical, to the dose rates that were measured prior to 1979, before the domes were sealed.

And that can give you only one understanding. And that is, that change in dose rate in the post-radiation treatment system were reduced to pre-1979 or pre-1980 dose rate levels on top. It means one thing, that all of the radon prior to the sealing of the domes had, in fact, escaped from the dome airspace, underneath the dome airspace.

And what it means is that the releases were probably promoted by a large effect by what I had introduced in my description as a Venturi effect. And the Venturi effect has not only the ability to void the airspace, the headspace, in the dome, but by pressure differential -- and this is what Dr. Ziemer mentioned beforehand -- had, obviously, augmented the rate by which the radon in the waste package that the RAC people had estimated were only being released by passive diffusion, had been greatly accelerated.

And that is something that Dr. Ziemer had just mentioned beforehand. When you operate a house that is at constant equilibrium with the outside ambient pressure, you will have very little radon emanating into the house. It is when the house is relatively sealed, meaning that there is a roof on the house and there are various devices that are operating inside a house, such as bathroom ventilation or a wood-burning stove that has a chimney or other effects, as you all know, when you stand in front of a door that is not necessarily a good seal in the winter months when the house is probably sealed, you will see a constant flow of air into the house. That means the house is operating under negative pressure to the outside barometric pressure.

And I believe this is the very issue that defines the silos. When you have a steady flow of air over a curved surface, such as a dome, you have something similar to what provides lift in an airplane at the leading edge. An airplane that is pulled forward by a propeller or jet engine produces a flow of air over the curved wing that, then, lifts a very, very heavy airplane into the air.

And I believe it is that particular effect that was very critical in the understanding of how radon that was produced in the silo waste was allowed to emanate into the headspace and, also, by the same Venturi effect, was then released into the environment.

And on that basis, using two empirical sets of measurement that talked about the disparity of the disequilibrium that was observed in the silos, and I use the 1993, which is a very unconservative and non-claimant-favorable assumption as a starting point for saying what was released potentially from the waste into the headspace. And then, using the empirical dose rate measurements on top of the silo prior to 1980, when the dome was sealed, and then, following the use of the radon treatment system, and realizing that those dose rates now were essentially identical, meaning that whatever accumulated in the headspace was vented out, on those two assumptions, I came to the conclusion that the radon releases from silos 1 and 2 at approximately the 110 to 120 thousand curies per year were, in fact, about twenty-fold higher than the radon release estimates, as generated by the RAC committee.

And that is basically my model. I have explained it the best I can. And putting trust in the empirical measurements and, also, consciously selecting a starting disequilibrium that is not claimant-favorable, and I have no other reason to believe that that is the real number that I believe was released from these two silos.

If anyone has any comments or questions, I would --

MEMBER ZIEMER: Yes. Hans, this is Ziemer. I have a couple of questions just for clarity.

I think you said that you assumed that during the venting that all of the radon and the daughters would have been removed from the headspace. And I am wondering about the issue of plate-out of daughters. It is notorious in other circumstances. Is that an issue you have looked at?

And then, my second question has to do with whether or not you or NIOSH or anybody independently calculated what the contribution to the surface doses would have been, knowing the inventory of radium in the waste and using first principles to calculate, you know, using distance plus absorption to calculate what you would expect to be the dose rates from the waste itself.

Were either of those looked at, and can you help or clarify your thoughts on that?

DR. BEHLING: Well, okay, I don't think I have to really calculate it because, Dr.

Ziemer --

MEMBER ZIEMER: Well, I know there's measurements. I was --

DR. BEHLING: We know that -- let me explain. Let me give you what my feeling is on this.

We know that radon-222 has a half-life of 3.8 days. Okay? And as a gas, it remains in gas; it will not, obviously, decay.

So, when the radon treatment system is operating for in excess of three hours, and in the process the dose rates go from around 200 millirem to 70-75 millirem again, which equals the pre-1980 dose rate in the unmodified domes, you have to draw the following conclusion: if you remove radon-222 and it is basically gone, and if that time period involves three hours, the longest-lived radionuclide that follows among the short-lived daughters is only a few minutes, 20 minutes. And that means that they will be plated out. They have decayed.

And so, what, in fact, you are looking at is strictly, once again, the dominant contribution in the post-radon treatment system that comes from radium-226 in the waste package and perhaps the 3 percent that they all said, obviously, while you are running the system, you are constantly drawing in new radon-222. And they accepted the fact that maybe 3 percent of the radon-222 still remained even after prolonged hours of the radon treatment system, which I took into consideration.

And for that reason, I think question Number 1 goes by the wayside. When you evacuate radon, and that system has been operating for three hours, those short-lived daughters are gone.

And this is one of the things that we always used to do when we looked at environmental samples or when I was in the nuclear utilities, allowed that sample, that air sample you collected, to decay for at least three or four hours to eliminate any short-lived radon daughters as a contributing false positive.

And so, I think I can reasonably answer your question Number 1.

MEMBER ZIEMER: Yes, I agree with that part of it. I was thinking of the lead-210.

DR. BEHLING: Well, listen, I don't know if that is really a significant contributor to the dose.

MEMBER ZIEMER: Yes, I don't, either. I don't recall exactly what its decay scheme looks like. Are there any gammas or x-rays from that?

MR. STIVER: I don't know if there are, but it is primarily a beta emitter.

MEMBER ZIEMER: Yes, okay. Good. Certainly, over time the lead-210 would build up in there, because there is going to be plate-out of those short-lived ones, and whether they contribute over time to the dose rate, I wasn't sure.

The other part of it, I was looking for an independent, you know, do the calculational methods compared to the direct readings.

DR. BEHLING: Well, Paul, I did not -- I calculated, obviously, in deriving my estimate of total quantities of radium-226 as a way of calculating what I would expect, therefore, the production of radon-222 to have been.

MEMBER ZIEMER: Right, right.

DR. BEHLING: But, then, again, I would have to look at -- this, obviously, has to be done by a computer that would, then, say, okay, on the basis of total curie content and the distribution in this waste package, what might be the dose rate exclusively confined to radium-226 standing on top of the silo? I have not done that.

MEMBER ZIEMER: No. Yes.

DR. BEHLING: But I think on the --

MEMBER ZIEMER: No, I wasn't saying you should. I just wondered if anyone had done it, if NIOSH or anyone, just as kind of an independent cross-calibration of how the actual measurements compare with what you would expect from the source term.

MR. HINNEFELD: Yes, if I'm not mistaken, RAC in one of their reports did something like --

MEMBER ZIEMER: Oh.

MR. HINNEFELD: -- I don't think it was an MCNP run, but some sort of calculation of dose rates and expected, you know, with the material in the residues, they kind of got the dose rate roughly that they measured --

MEMBER ZIEMER: Okay.

MR. HINNEFELD: -- on the dome.

MEMBER ZIEMER: Okay. Thank you.

MR. HINNEFELD: I think, but I can't find it right now.

DR. BEHLING: But, Stu, I do want to come back to your comment that you made in your presentation with regard to the assumption about the starting disequilibrium. You said that my estimate would be somewhere in the middle. It's not. I believe I intentionally erred on the opposite side, on unconservative and non-claimant-friendly assumptions that would potentially lead to doses or release rates that are actually less than what I calculate.

And so, when I defaulted to an assumption that the disequilibrium that was identified in 1993 had existed at the time of emplacement, I believe those numbers would prove to be in all likelihood an underestimate, and therefore, unfavorable to the claimants.

And so, this pretty much explains the logic that I used and the method that I used and the numbers that I used to arrive at my numbers. And I will stand by them.

MR. HINNEFELD: Yes, I know. I don't argue with that. What I meant when I said that was that the placement condition that would maximize the release, you know, the calculation of the release, would be if the lead-210 and the radium-226 were in equilibrium at placement. If that were the assumption, then the release estimate would be maximized. If the assumption of placement was that there was no lead-210 present, then that assumption would minimize the release.

DR. BEHLING: Absolutely.

Absolutely.

MR. HINNEFELD: Yes. You didn't choose either of those. You chose 40 percent.

DR. BEHLING: No, but there is no reason to, but, Stu, there is no reason to believe that there was no lead there because, as I had mentioned, I started out -- my basic feeling was this: if you start out with the assumption that at the time that this ore was harvested, in all likelihood the ratio between radium-226 and lead-210 was probably close to unity, because there is no reason to assume that a significant or major part of the radon had escaped during this time interval, it was probably there since the time the earth was created. And if it is a deep stratum, the potential release of radon that would disrupt this equilibrium was probably minimal.

So, what you started out with, it is probably at the time that the ore was produced with an equilibrium ratio that probably came close to unity. The only thing that now has to be accounted for are the nine- to fourteen-year time intervals between the time the material was harvested and the time period when the raffinates were in place in the silos.

And there, I gave you a calculation that says let us proceed with a very unconservative assumption that during that nine to fourteen years all of the radon escaped. You would still end up with an equilibrium fraction of approximately .72 and 6-something that I mentioned to you, which is higher than the assumed disequilibrium that I chose to use that equals the disequilibrium fraction that was measured in 1993. So, I was, again, very unconservative and non-claimant-favorable in my assumption.

MR. HINNEFELD: Yes, I understand. I wasn't intending to argue.

(Laughter.)

I didn't disagree with what you said.

MEMBER ZIEMER: I have another question, though. Stu, this may be for you. So, you said in your report the diffusion rate into the headspace of the order of 60 percent is inconsistent with the behavior of radon. I think that is sort of what we were talking about before.

MR. HINNEFELD: Well, that is basic for access.

MEMBER ZIEMER: Yes. But you did say you proposed to use the 95th percentile. In effect, what does that mean in terms of what that would look like relative to that 60-percent figure?

MR. HINNEFELD: Well, the 95 percentile estimate in RAC's report would add about 50 percent to our proposed release. It goes from about 6,000 to about 10,000 curies a year. Isn't that right? Something like that.

And so, it would still come nowhere near, because, as Hans said, our original proposal or our Site Profile proposes 6,000, roughly 6 or 7 thousand curies a year pre-1979.

MEMBER ZIEMER: Yes.

MR. HINNEFELD: SC&A's report is at 100. And realistically, if you used 40 percent equilibrium, which was seen in both silos in the 1979 sampling, if you used that, their estimate would be higher than that.

MEMBER ZIEMER: Yes.

MR. HINNEFELD: It would be more on the order of 180,000 curies per year.

And so, you are talking about a factor of 20 or 30 difference --

MEMBER ZIEMER: Okay.

MR. HINNEFELD: -- between what is in our Site Profile and what the SC&A estimate would be, and it is going to the 95th percentile; the RAC report wouldn't come close to bridging the gap.

MEMBER ZIEMER: Okay. Now, as a practical matter, recognizing there is still this substantial difference between these two views, in terms of workers and where they are located and what the impact of dose is, can you give us some feeling for the practical outcomes, let's say, from the current SC&A view versus the other? Are we talking about large? This is sort of this issue of what's a significant difference in terms of how it impacts the PoC, for example. Because we don't have -- I'm trying to recall the worker situation here. And who is getting the doses and what are they looking like?

MR. HINNEFELD: Well, the dose is assigned, essentially, to everybody.

MEMBER ZIEMER: Yes, I know, but

what kind of doses are we assigning here?

MR. HINNEFELD: Oh, gosh, it is in our Site Profile.

MR. ROLFES: In the earlier time period we are talking about more sizable doses. I don't recall the maximum values, but I want to say it was pretty hefty, you know, exceeding what some of the uranium miners would have received, is essentially what we are going to be assigning for the earlier time period, when the Q-11 ore silos were open.

MEMBER ZIEMER: The annual doses are going to be --

MR. HINNEFELD: We can probably look those up, but I think it would take us a little bit. It might be better to try to do that at lunchtime.

MEMBER ZIEMER: Well, remember when we were talking -- I'm talking about the SEC Work Group had looked at this, issues of how big can the error be in your estimation if it is an error in a small dose versus an error in a big dose.

MR. STIVER: Something else which we need to consider is that Hans and the SC&A model is really applicable to the period before June of 1979.

MEMBER ZIEMER: Right, where you already have --

MR. STIVER: We don't seal.

MEMBER ZIEMER: Yes.

MR. STIVER: And remember, we have got an SEC that goes all the way to 1978.

MEMBER ZIEMER: Right.

MR. STIVER: And, you know, radon is only going to affect lung cancer, which is an SEC thing to start with.

MEMBER ZIEMER: Right.

MR. STIVER: So, you have got a very small number of people who are going to be affected.

MEMBER ZIEMER: Right. It is only less than 250 people mainly.

MR. STIVER: The model that is being used is the appropriate model.

MEMBER ZIEMER: Yes. I understand, but I am just trying to get a feel for how much that is contributing, yes.

CHAIR CLAWSON: It's lunchtime right now.

(Laughter.)

MEMBER ZIEMER: Yes.

CHAIR CLAWSON: I think maybe if we can take a little bit of time to be able to digest this --

MEMBER ZIEMER: While digesting food.

(Laughter.)

CHAIR CLAWSON: Yes.

Stu, if you could kind of look at how

this is being implemented, this may help us.

MR. STIVER: If I could jump in, if people don't mind, I mean, the rest of these issues I could get through in about 15 minutes. So, if you don't want to take a lunch break now, we could just go through and close these others out.

I guess the question in my mind is, where do we go from here?

MEMBER ZIEMER: On this one, where do we go on this one?

CHAIR CLAWSON: Well, because I had a couple of questions. If you want to go into that, it is, why was this RAC report actually generated? Because it seems to me that there must have been a very large concern over this to have such a report written.

And I guess I have heard a lot from a lot of the Fernald workers, always the K-65 silos. What stimulated this to be able to happen?

MR. HINNEFELD: Well, actually, RAC was contracted by ATSDR, the Agency for Toxic Substances and Disease, something. It is part of CDC, actually.

MR. KATZ: Yes.

MR. HINNEFELD: And they were doing at the time a series of dose reconstructions to populations around DOE facilities. They did a whole bunch of them. And they did Fernald, was one of the ones they did.

And so, to do that, they estimated total releases from the sites of all radionuclides -- radon is just one piece of that report -- and modeled the dose to the neighbors, okay, not the workers, but to the neighbors.

CHAIR CLAWSON: This is for kind of an environmental --

MR. HINNEFELD: Yes.

CHAIR CLAWSON: Right?

MR. HINNEFELD: Yes. And so, this was done. This was a part of an effort that ATSDR did for a number of DOE facilities. And that is why they did their report.

We have just taken the radon emission rate, which was a part of their report, and said, okay, based on that and their models, what concentrations would you expect around the site? I think our Site Profile file says that the maximum concentration is here. Actually, we used some other information, too. The maximum concentration in this part of the site we are just going to assume that people were exposed on that part of the site and that they are going to get this concentration of radon. Just by working at Fernald, they are going to get this concentration to work from.

Now, in addition to the RAC report, there was an additional study done by some researchers at UC about radon concentrations around the site. They used CR-39, which is a track etch detector. And they taped these track etch detectors on glass windows around the plant.

CHAIR CLAWSON: That is kind of the Pinney Report?

MR. HINNEFELD: That's the Pinney Report. That's the Pinney Report. And that was a technique that had been demonstrated by other researchers, that you can place this track etch on glass that has been around, and you will get an integrated total exposure to radon progeny from the radon that has been etched, the progeny that has been etched into the glass. And then, you count off the decays from what is there.

They used that study and they saw that, man, the highest concentrations they found were not the ones necessarily closest to K-65 silos, although they wouldn't have been terribly far. The highest concentrations they saw were right around plant 1. Or 2. One, yes, right around plant 1.

And they said, well, what happened at plant 1? Well, that's where the Q-11 ores were stored prior to being run through the refinery. Q-11 ores was what gave rise to the K-65 residues that were generated at Fernald.

CHAIR CLAWSON: Okay.

MR. HINNEFELD: So, this high-rating content ore was stored for a period of time in plant 1 silos. And they concluded that that was a high source of radon to that area of the plant. And it drops off very rapidly as you get away from plant 1. So, they concluded that the major contributor to employee exposure certainly would have been in that area of the site and would have been from Q-11.

So, that also is part of the story here, is that there is this study that kind of shows pretty high concentrations right around plant 1, more so than K-65. So, our Site Profile adds, you know -- and when Pinney wrote their report, they already had the RAC report. And so, they could essentially integrate what exposures to workers. And Pinney was interested in exposure to workers, right.

So, they were worried about exposures to workers. And they said, based on the data we have and the RAC emissions report, you know, the RAC estimate, this is what we think radon emissions, radon exposures would have been around the site. They actually had like individual worker histories and people filling out where I worked. So, they would say, for each worker, they could generate an exposure.

We didn't bother to do that. We said, they said the highest exposures were in this quadrant. We are going to give those highest exposures to people who worked at the site, rather than trying to chase people, because don't necessarily have for all our claimants, we don't really have them chased all over.

MEMBER ZIEMER: So, you considered those boundings were --

MR. HINNEFELD: We considered those an estimate of it, yes. And so, if we chose the highest location, we figured we would be bounding people's rate.

MEMBER ZIEMER: Do you know how those numbers compare with what you would get if you used the Behling methodology? Or do we know that?

MR. STIVER: It is scaled by about a factor of 20.

MR. HINNEFELD: Scaled up by a factor of 20.

MEMBER ZIEMER: Still a factor of

20 differential?

MR. HINNEFELD: It probably would. And the reason I say that is because part of the Pinney -- we have a research paper that Pinney's research team wrote. And it appears that they sort of calibrated their track etch detectors based on the RAC estimate and putting detectors at a fairly remote, like an environmental location --

MEMBER ZIEMER: Yes, okay.

MR. HINNEFELD: -- and said, what would RAC predict would have been the integrated exposure here at this remote location? And they in a sense sort of calibrated. I think that is what they did. We do have this paper. And they sort of calibrated their track etch detector based on that. So, it sounds like it would just be a scaling of a factor of 20.

MEMBER ZIEMER: But they are

calibrating a pretty low level, then, if it is --

MR. HINNEFELD: Yes, what they considered background.

MEMBER ZIEMER: Yes. So, then, your error gets big.

MR. STIVER: Yes, you have got your error --

MEMBER ZIEMER: Yes, got you.

MR. STIVER: We have a Gaussian dispersion model.

MEMBER ZIEMER: Yes, yes.

MR. ROLFES: I did want to add a couple of things about the data that we do have available from Pinney. We actually have printouts of each individual's exposure that was assigned to them, based upon the air concentration and the location that they worked; basically, the work location on the site, whether they were working during the day or night. When there was uncertainty, you know, people were placed into higher exposure scenario areas of the site.

So, we have those printouts showing each annual working level exposure value from the Pinney study by Social Security Number. And those are SPEDELite-linked into claimants' files in NOCTS.

Now we have also independently done an update to the environmental TBD. That was just approved earlier in March, I believe, of this year. And we have instructed dose reconstructors to use the Pinney data for radon dose assignment or in dose reconstruction as needed. And, also, if there is a higher value in our TBD for a given year, we have told them to use the higher of the two values, between the Pinney and our environmental TBD.

To summarize the Pinney values, there's a couple of excerpts that I was going to -- let's see -- point out. Let's see.

It says, yearly mean worker exposure attributable to K-65 source term ranged from 1.04 working-level months in 1973 with a range of .003 to 2.16 working-level months, to 0.03 working-level months in 1988, with a range less than .001 working-level months to 0.093 working-level months. Yearly mean exposures to workers in the area of the Q-11 silos ranged from 3.34 working-level months to 10.99 working-level months during the years when the silos served as a radon source.

And then, there is a separate excerpt that -- let's see. We have got some 90th percentile cumulative radon exposure values from the K-65 source. It was 18.06 working-level months and 31.52 working-level months from the Q-11 source.

Without reading the rest of the context, that will give you an idea of the ranges that --

MR. HINNEFELD: What was that excerpted from?

MR. ROLFES: This is from the Radon and Cigarette Smoking Exposure Assessment of Fernald Workers, part of the Pinney Report.

MEMBER ZIEMER: Okay.

MR. ROLFES: And it is in the AB Document Review folder. I can point it out.

DR. BEHLING: Can I make a comment here because I think it is very important for me to also make a comment with regard to the Pinney Report?

I think, early on, and this was the issue of the Pinney Report gave rise to the need for a White Paper that SC&A wrote for 2010, and that was requested by Brad for us to do.

And the statement up to that point in time was that the Pinney Report independently validated the release quantities as measured or estimated by RAC. And that is not the case.

And I brought this up in my email that is a companion document to Stu's White Paper over the last few days, but it is also something that I had written about in my second White Paper dated 2010.

And that is in a quote, and whoever has the email that I submitted a few days ago, there is a quote from the Pinney Report that clearly states that the Pinney Report did not validate the RAC release models from silos 1 and 2, but simply accepted them and, then, coupled that data of 5 to 6 thousand curies per year released with data, from meteorological data for dispersion.

So, they did not validate the numbers. And whatever they came up with in terms of dose estimates to people onsite were essentially nothing more than coupling RAC data to a dispersion model. If the RAC model data of release of radon releases are in error, then so are the Pinney expected doses to workers onsite. Simple as that.

And for anyone who questions this, take a look at what I submitted in my recent report to Stu, my email, where I take a direct quote from the Pinney Report as to what they did and how they used the RAC data. They simply coupled it.

MR. STIVER: Yes, Hans, this is John.

We're all basically in agreement with that. I think the issue really is, what is the proper source term to use? Is it the RAC's source term or is it our source term? And I guess that is really where it is.

I mean, you can model, from that point on, you can use dispersion modeling to get just about any kind of an exposure you want, depending on the type of parameters used, and so forth. But I think the starting point is what really counts here, and that is really what we are trying to focus in on.

CHAIR CLAWSON: I guess I hate to say this, but I can understand what Hans has been saying on this from the start now to the very beginning of it. But the bottom line is this is what Dr. Ziemer has also said, what do we do with this? Because this is part of the SEC time period, correct?

MR. HINNEFELD: All except the last six months.

CHAIR CLAWSON: All except the last six months of it.

Radon is only going to affect lung cancer, if I'm correct.

MR. STIVER: It is.

MR. HINNEFELD: Yes. Theoretically, there could be some dose to other organs, but it is going to be not very much. I mean, there's some that theoretically is distributed through the bloodstream to other organs. Nothing really concentrates radon. There is no organ of interest. It is going to be highest non-metabolic models, which never really gets you much dose, the non. Unlike other cases where we have a skin cancer, a skin dose potential, and you really want to give a fair shake to the non-presumptive cancers because skin is a non-presumptive cancer, the non-presumptive cancers that are going to be affected by internal dose, you just don't get very far, and especially not when you've got highest non-metabolic sort of dose to it. There might be -- I'm not going to say it is zero, but I don't know that it is --

MR. STIVER: It is pretty close to zero.

MR. HINNEFELD: Yes.

MR. STIVER: As far as a fraction of a millirem.

MR. HINNEFELD: Yes. I mean, there's not much there, except in the respiratory tract and they are SEC cancers.

MR. STIVER: I guess the question is, is it prudent to dismiss the model and go with RAC, or whatever, based on the magnitude of the dose that might be involved? Or there is a question of, you know, find a model that has the best science for the particular period in time. Because if we didn't have the SEC, this would still be very much --

MR. HINNEFELD: Well, there's the SC&A report, which I can't refute necessarily. Is it better than the RAC report, which I can't refute?

MEMBER ZIEMER: And I don't think -- I know Hans has mentioned this -- but I don't think the issue is that SC&A's report was not reviewed by the National Academy. That is not the issue.

CHAIR CLAWSON: No, we don't even want to talk about the National Academy.

(Laughter.)

MEMBER ZIEMER: No. No. And I think if the science is good, then what I would be looking for would be NIOSH's reason for disclaiming what Hans has done or else -- you're not obligated to follow a National Academy report necessarily. But, also, it is difficult to ignore it at the same time.

So, if we think there's reason to adopt this other model as being at least a reasonable possibility, is the science as good, or whatever we say, I don't know if you need to look at it anymore or not. You have looked at it.

MR. HINNEFELD: I can't reconcile that.

MEMBER ZIEMER: Yes.

MR. HINNEFELD: I really spent a lot of time on that RAC report and I got to where I think I understood it. I even got to the point where I am pretty sure in one of the tables that should have been at least values for both silos, it had to be realized the value for one silo instead of both silos combined. I got to know it pretty well, well enough that I understood that. I think I understood it pretty well. It makes perfect sense except for the absence of lead-210 in the residues.

You know, this is kind of an oddball suggestion, but we have an option of a triangular distribution with the upper --

DR. BEHLING: Stu, can I weigh-in on this? I hope you will take my statement sincerely.

When I was asked to look at this, that calculation was based on the assumption that no one would be covered under the SEC. If the SEC extends right through the timeframe other than the last six months that might be affected by this, there's no point in doing this.

As was clearly pointed out, the issue of radon exposure only affects the lung dominantly, and that is, obviously, covered as a presumptive cancer. And it is covered, essentially, all the way to the point where I had estimated these higher doses.

So, there is really no point in investing a huge amount of effort in rectifying this problem. If this had been done at a time when the SEC had already been granted for this time period, I probably would have looked the other way and said, what's the point in discussing something that has such little impact?

At this point, I obviously started

this in 2008, was again asked to do it in 2010. And those time periods predate the assignment of the SEC Class.

And at this point, I would probably recommend to ignore my model. And I feel vindicated that at least you have given me the chance to talk about it and not feel that I was an idiot for having proposed this.

(Laughter.)

MR. KATZ: Oh dear, Hans, no one has ever called you an idiot.

MR. STIVER: No one.

CHAIR CLAWSON: No. No, actually, it is a little bit different than that, Hans, because I did request you to do this. And I personally believe that the work that you did is outstanding. And I agree from it. From just my simpleton way of looking at it, it makes sense to me, what you are saying.

But, also, too, the bottom line is

I want to make sure that the model and the product that we give to our customers, which are the claimants, is the best that we can. And I just want to make sure that -- you know, I know it is not going to affect anybody really because the lung cancers have already been taken care of by the SEC. But the bottom line is I also want to do due diligence and make sure that what we do is right.

And I'm kind of in a corner with --

MEMBER ZIEMER: Well, I heard something starting to be proposed. If I understand just from the description, it is you can use both of those points and make a distribution, right?

MR. HINNEFELD: Yes.

MR. STIVER: Something else we need to keep in mind is our model is really only applicable to pre-June of 1979.

MR. HINNEFELD: Yes.

MR. STIVER: So, it is kind of a moot point.

MR. HINNEFELD: Make sure everybody understands here. In terms of radon emissions in the RAC report, there is a period of time where your K-65 residues were being shipped in from Mallinckrodt and sitting on the storage pads in trucks. And RAC has an estimate for that, that release rate.

There is a period of time when the silos were being actively filled. That goes from about 1952 or 1953 up through 1958. They have a release estimate for that and have a set of assumptions.

There is essentially the dormant storage state from 1959 through 1979 or -- 1979 was --

MR. STIVER: Yes, 1979.

MR. HINNEFELD: The storage unsealed part from 1957 to 1979, and then, there is the post-sealing, 1979, and their report only goes to like, I don't know, 1988. Yes, I think it goes to 1988 or something like that. But we have the estimate to continue on.

So, there are various things like that. If we use the RAC value -- and Hans has been most gracious today, and I feel bad that he feels like he wasn't valued. I couldn't find anything wrong with this work. It looked okay to me. I just can't reconcile it with other stuff.

If we stay with the RAC estimates -- and I proposed in the paper that maybe we should use the 95th percentile rather than the median estimate because for modeled exposures that's often what we do. We often use 95th percentiles rather than median exposures.

If we propose that, we would have, then, a consistent basis for those various timeframes and things like that, with just the six-month difference between the end of the SEC and of the high radon release area. If we are okay with that, I think that would be very palatable from our standpoint.

MR. STIVER: Yes, I think that would probably be okay with me.

Hans, would you be willing to accept that?

DR. BEHLING: Yes, I will. As I have said before, the thing that bothered me was the blanket rejection of my data in previous discussions and presentations. And I think what was stated today satisfies my ego at least.

(Laughter.)

CHAIR CLAWSON: Well, we appreciate that.

And, with that, then that is how we will proceed, if that is all right with you, Paul.

MEMBER ZIEMER: That is a good solution, yes.

CHAIR CLAWSON: Okay. Hans, I appreciate what you have done there. It is probably one of the first reports that I have really been able to understand. So, I feel good about it.

(Laughter.)

DR. BEHLING: Well, as I mentioned to you, I always go for the simplest approach.

(Laughter.)

CHAIR CLAWSON: Thanks, Hans. I appreciate that.

DR. BEHLING: When you can reduce something to the simplest methods of explanation, obviously, you usually end up with the best results. And I have to tell you, I do not understand how the RAC people whose data I used, their own data, failed to understand what I was looking at when I looked at their model and came to the conclusion that they did nothing but make one assumption after the other, inclusive of deficiencies that obviously I pointed out in my report. And I have a tough time. Were they that blind to realizing that they had the data and failed to use it? I just don't get it.

MR. KATZ: Let's just leave it there.

MEMBER ZIEMER: So, I am going to use that teaching standard in the future for my students, to make things that even Brad will understand.

(Laughter.)

CHAIR CLAWSON: You know, we could put that into a TBD, so even Brad can understand it.

No, you guys, really, seriously, it was there.

MEMBER ZIEMER: Yes, that is a good

teaching method.

CHAIR CLAWSON: We may not be able to make lunch where we are at and be able to eat some and find food, but I personally would like to finish this off, if everybody is okay with that.

MR. HINNEFELD: I need a break sometime somehow. If we are not going to have lunch, I need to have a comfort break.

MEMBER ZIEMER: I do, too.

CHAIR CLAWSON: Okay.

MR. STIVER: It will only take us about another 15 minutes to go through the rest of it.

CHAIR CLAWSON: Okay. Well, let's go ahead and have a break.

MR. KATZ: Have another 10-minute break?

CHAIR CLAWSON: Yes, and then, we will proceed.

MR. KATZ: Okay. So, at 10 to, we will reconvene.

Thanks, everyone, for hanging in there on the line.

(Whereupon, the above-entitled matter went off the record at 12:40 p.m. and went back on the record at 12:46 p.m.)

MR. KATZ: Okay, we're back and we're about ready to get going.

And I think you just go, right?

MR. STIVER: Okay. What's left now are the old SEC issues, 3, 4, 5, and 6(b). And I think this is going to go pretty quickly.

SEC Issue 3 is about the default concentrations of plutonium, neptunium and other isotopes in recycled uranium. This was the notion that it might not be bounding for some classes of workers and activities built into the time period as well.

I think that aspect has been

resolved for at least the principal three, but we still have this outstanding issue potentially of americium-241. So, I would like to keep that one. Maybe instead of in abeyance, we should go ahead and change that to in progress, just to account for the fact that there is ongoing work here.

MR. KATZ: Yes.

MR. STIVER: Okay, SEC Issue 4, this was the radon breath data, radium-226 and thorium-230. Okay, this is another one; this is very similar. This is the whole idea of the thorium-230, unsupported radium -- or excuse me -- depleted or deficient in radium and uranium. And this is something you guys were going to look into in regards to this is very similar to the issue of 7(a).

So, we just keep that in --

MR. KATZ: In progress.

MR. STIVER: -- in progress as

well.

MEMBER ZIEMER: It was in abeyance, though, before. So, why is it moving out of abeyance?

MR. STIVER: Well, wasn't this the guys were going to have to kind of look at a different approach for the thorium-230 in the plant 2/3, that issue? Because we talked about it in relation to --

MR. ROLFES: It is what Stu had said we could look at the DWE data.

MR. STIVER: Yes, the issue is 7(a). So, this is the same. We could probably just go ahead and actually just close this out because it is no longer an SEC issue. So, we don't really need to keep both of them open. We have the Site Profile.

MEMBER ZIEMER: You have it in the other one.

MR. STIVER: Yes, and it was moved

to the Site Profile.

MR. KATZ: Oh, yes, that's true. Right.

MR. STIVER: Let's go ahead and just close this one.

MEMBER ZIEMER: Yes.

MR. BARTON: Although, are we sure that thorium-230 can be estimated or could we envision a situation where that has to be added as not reconstructable?

MEMBER ZIEMER: We already have the SEC. Why are we looking at a --

MR. BARTON: Yes.

MR. STIVER: Yes. Once it got moved to the Site Profile --

MEMBER ZIEMER: Yes, yes.

MR. STIVER: If it was reopened and there was an issue, I guess just we would have to take all of those doses as well. That's all.

MEMBER ZIEMER: Right. What about

Issue 3? Isn't it the same deal?

MR. STIVER: Issue 3, there is still an outstanding notion of americium-241. If it turned out to be --

MEMBER ZIEMER: That's an SEC issue?

MR. STIVER: Well, if it was a worst-case scenario, there was a dose potential, and there was no way to reconstruct it, then --

MEMBER ZIEMER: Oh, okay. Yes.

MR. STIVER: Just keep that one --

MEMBER ZIEMER: Yes.

MR. STIVER: -- on the books for now.

So, 4 we will go ahead and close.

Five, radon releases. Now this one we should have closed a long time ago because it is captured in Finding 25, which we just reached agreement on. So, go ahead and close

SEC Issue 5.

And that brings to our last one. This is 6(b), and this was the in vivo thorium model from 1979 to 1988. And we have agreed to accept that model.

MR. KATZ: I'm sorry, which issue is this?

MR. STIVER: This is 6(b).

MR. KATZ: 6(b)?

MR. STIVER: This was the second half of the --

MR. KATZ: Yes, thanks.

MR. STIVER: -- thorium-232 in vivo monitoring from 1978 to 1988, and we are keeping that open until such time as we have reviewed the post-SEC thorium. So, we have reached agreement on that. And this one can be closed as well.

And that brings us to the end.

MR. KATZ: Yay. Congratulations.

That's excellent.

MR. STIVER: It's only 12:47.

MR. KATZ: Yes. Okay.

MEMBER ZIEMER: Future plans?

MR. KATZ: Future plans. Oh, yes, timing, I guess, to wrap up.

MR. STIVER: Yes, I guess we probably want to wait until we have a chance for NIOSH to produce TBD 5 revisions and for us to review it.

MR. KATZ: Yes. I'm assuming we don't have a sense right now as to when we would be ready to meet on these things.

MR. HINNEFELD: The same disadvantage I always am; I've got to plug it into the project schedule --

MR. KATZ: Yes, sure.

MR. HINNEFELD: -- and determine what kind of resources we can get to it.

MR. KATZ: Right. So, when you get

to that, if you can send out a note giving a ballpark for when it would be ready.

MR. HINNEFELD: Yes, I will do my darnedest.

MR. KATZ: Right. I mean, there is no rush on that one.

MR. HINNEFELD: I would like to get this done.

(Laughter.)

MR. KATZ: Of course, it would be great to get it behind us. Yes.

MR. HINNEFELD: All my team leaders and my Associate Director for Science are conflicted on this site. So, I would like to get this one done and get the heck out of this business.

(Laughter.)

Let the people who know how to do this better than me do this.

MR. KATZ: You do okay.

Okay. So, Brad, are we adjourned?

CHAIR CLAWSON: Yes, we are.

MR. KATZ: Thank you, everyone on the line.

Have a good rest of your day, and much thanks for all you have contributed today.

Take care.

(Whereupon, the above-entitled matter went off the record at 12:52 p.m.)