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

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ADVISORY BOARD ON RADIATION AND
WORKER HEALTH

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WORK GROUP ON FERNALD

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WEDNESDAY
SEPTEMBER 3, 2014

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The Work Group convened in the Toronto Room, Cincinnati Airport Marriott, 2395 Progress Drive, Hebron, Kentucky, at 9:00 a.m., Eastern Daylight Time, Bradley P. Clawson, Chairman, presiding.

PRESENT:

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

ALSO PRESENT:

TED KATZ, Designated Federal Official
MATT ARNO, ORAU Team*
BOB BARTON, SC&A
HANS BEHLING, SC&A*
HARRY CHMELYNSKI, SC&A*
LOU DOLL
STU HINNEFELD, DCAS
KARIN JESSEN, ORAU Team*
KAREN KENT, ORAU Team*
TOM LABONE, ORAU Team*
JOYCE LIPSZTEIN, SC&A*
JOHN MAURO, SC&A*
MARK ROLFES, DCAS
JOHN STIVER, SC&A

# 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: Good morning, everyone. This is Advisory Board on Radiation and Worker Health, Fernald Work Group. We're just getting ready to get started here. We are ready to get started here.

So we're going to start with roll call as usual, beginning with the Board Members. A lot of our Board Members I think are going to be on the phone.

We're speaking about a specific site so please speak to conflict of interest as well, everybody, as you register your attendance. Let's begin in the room with Board Members in the room.

(Roll call)

MR. KATZ: Okay, that's it. Then before I turn it over, let me just note the agenda for the meeting, some materials for the meeting, should be posted on the NIOSH website today's date under the Board section, scheduled meetings. And, Brad, it's your meeting.

CHAIRMAN CLAWSON: Thank you very much. Like to welcome everybody today. It's been a long time since this Group's gotten together. I appreciate you taking the time to gather with us today.

With that, I'll turn it over to John Stiver and we'll start out, or did we want NIOSH or, got a couple papers.

MR. STIVER: This is John Stiver. I know NIOSH was tasked to look into the uranium bioassay data for subcontractors during the transitional period in '84-'85 and they produced a paper so it might be good maybe if Stu or Mark could kind of give some highlights on kind of that story.

MR. HINNEFELD: Well, this is Stu and I'll give this a shot and then, Mark, you can correct me when I say something wrong or supplement what I say when I leave something out.

A couple or three Board meetings ago, the Work Group recommended the addition of a Class for subcontractor employees up through

1983 based on the inability to reconstruct internal uranium exposures to that group of people because there was a general lack of bioassay data available for subcontractors for almost all those years.

I mean there were some isolated spots where there was some bioassay data, at least one instance of a subcontractor activity where we had a pretty good description of what they were doing and they were monitored. These people were monitored.

An assessment of intakes from that activity indicated that had those workers not been monitored the coworker model that was available, which was based almost entirely on prime contractor employees, would not have bounded their exposure.

And there was not a lot of confidence that NLO was rigorously identifying subcontractors who would be working in radiological work and, therefore, there may have been other instances of subcontractors doing radiological work who were not monitored

who were exposed similarly to that group of contractors.

And so the decision was made that we didn't think we had, or the Work Group concluded that there wasn't sufficient bioassay data set for subcontractors, through 1983 at least, in order to do dose reconstruction.

Now, there is a data set or there are data from subcontractors, bioassay data from subcontractors from '84 and '85 that are relatively numerous.

We have tables in our latest document that shows how many samples and how many people were monitored in '84 and '85, and so based just on the number of samples available, the original decision was that the Class would be through 1983.

So then as some additional questions were raised in a letter to the Work Group about, you know, some concern that contractors still weren't being appropriately monitored in '84 and '85 and that, you know, throughout NLO's contract or, you know, prime

contract, contractors should be added, should, in fact, be added to the Class for '84 and '85 because NLO's contract ran I think through November of '85 if I'm not mistaken. I think Westinghouse started in December of '85.

So what we've done in the meantime is, you know, and so, anyway, part of the additional analysis that followed on that letter was to point out that in the years '84 and '85 almost all the bioassay samples came from two companies, Rust Engineering and Legge Construction, and that there were quite a lot of other companies that were subcontracted, that had subcontracts during that time. We knew that from other records, that there were quite a lot of other companies.

And so the question was what about these other companies? Could it be that just Legge, you know, happened to be caught by happenstance and that Rust was, you know, included sort of by happenstance or because they were more, you know, regularly at the site?

And so maybe there were other

subcontract activities that should have been monitored that weren't and were heavily exposed as well.

And so our task was to go try to find out what we could about some of these other contracts and what they were doing and whether it sounded like it was a radiological construction work or non-radiological because there was some clean construction going on by that time at Fernald and so we tried that.

We've made data captures to Legacy Management and it appears that the contracts were not retained so we've not been able to find the actual contracts for most of those companies and scopes of, statements of work, scopes of work on contract.

So we couldn't pursue that. You know, so that avenue really didn't pay off, that we could find a scope of work that said such and such company is building the new water treatment plant or whatever they were building. So that avenue of pursuit didn't prove fruitful.

But we did additional analysis, part of which had been at least partially done earlier, of the bioassay data that is available for subcontractors for essentially the bridge period, you know, for '83 which is the last year in the current Class, '84-'85 which are the transition years and then '86 and '87.

And we presented that here in our report. There are tables, Tables 3 through 7 of our report called "Feed Materials Production Center Subcontractor Bioassay Results, 1983 to '87, and Search Results for Scopes of Work, '84 to '85." This is dated August 21st of this year.

We present the data, the bioassay data in Tables 3 through 7 and from that you see that in the years following Westinghouse's takeover of the contract in '86 and '87 we can still see that the majority of the samples came from Rust Engineering or come from unknown, an unknown employer, meaning that this appears to be a subcontractor individual but there was not a company name written on the card. You know,

these data were all collected from bioassay sample cards, sample request and then result cards.

And so it kind of, to our mind, shows a pattern that is similar to '84 and '85, that you have roughly the same number of people monitored, not exactly but the total number of people monitored in these tables and it goes in '84 it was 88, in '85 it was 70, in '86 it was 83, in '87 it was 89. So you have similar numbers of people monitored.

You have, I guess, similar numbers of companies, not the same company year after year except, of course, for Rust Engineering which is there all through.

And so it just appears to us that there doesn't seem to be a particular difference. When Westinghouse took over the contract, there doesn't seem to be any particular, you know, any particular difference in the way contractors were monitored than, say, they were in '84 and '85.

And that's about the extent of the

evidence we could find. Now, Mark, I don't know if you feel like there's more I could say about this or not.

MR. ROLFES: No, I don't have anything to add. That was very good.

MR. HINNEFELD: Okay. So that's about the extent of the evidence we could find which indicate that this '84 and '85 period seemed to have a period, for whatever reason, contractors that seemed to have been identified and monitored, you know, by bioassay.

You know, to conclude, you know, the question then really becomes is that sufficient, to feel like, well, yes, it looks like they were monitoring people appropriately?

And alternatively we haven't found any evidence of a company or of work that was being monitored that would have been more highly exposed than, say, Rust Engineering or these companies that were monitored that would have been more highly exposed than these because if you have contractors who weren't

monitored, I mean, the coworker model's only going to apply to people who were monitored and so you have to have a company that was doing radiological work that would be more heavily exposed than the monitored workers in order for this coworker approach not to bound their exposures.

So from our standpoint, you know, we just don't see the evidence that this data set, which is large enough in '84 and '85, that this data set isn't sufficient for bounding the dose to unmonitored subcontractors during that period.

So, you know, that's the extent. It's not as definitive as we had hoped. We had hoped to find statements of work that said that this company was building a new building someplace but we were not able to find that.

So it's not as definitive as we had hoped to find but it appears to us that there is no particular evidence to believe that it is not a sufficient data set to bound those doses.

MR. STIVER: Okay, thank you.

Thank you, Stu. This is John Stiver. You know, I think our main concern from the last meeting really, and Stu has articulated it pretty well, is that, you know, we had discovered there were probably about 50 different subcontractors during this period yet only a handful, I think 12 of them, predominantly Rust and Legge, were represented in the bioassay data.

And so, you know, looking at these, at a coworker model for subcontractors is a little bit different animal than looking for, say, a coworker model for a set of workers in a building in the plant doing the same type of activity over and over again when you have kind of a homogeneous population.

With the subs you've got almost like a separate population. Every time they come in to do a different job, does that really have any relation to another contractor that comes in and does some other type of job?

And so in our mind it was critical that you have at least a good weight of evidence

argument, if not out and out proof, that, you know, indeed, the potentially exposed groups of companies, contracting companies were, indeed, monitored.

And so I guess that was really the genesis of this data capture which, you know, NIOSH has performed. It would have been nice to find some contract information that specified who did what and when.

You know, looking at the patterns in the data, I got to say I kind of agree with Stu. You've got about the same number of personnel being monitored.

In '86 and '87 when Westinghouse, WINCO came in and took over, you don't see a big change in the pattern, the distribution of samples among the individuals. You still see, with maybe the exception of some of these unknowns, Rust is still predominantly the leader.

There are quite a bit more samples in '87. Now, I don't know if they increased the sampling frequency for whatever reason but the

number of individuals stays the same.

So my takeaway from this is that I'm not really seeing a smoking gun here. You know, I think the criteria for rejecting this data, given the numbers are fairly good and the representation appears to be pretty good, would have to be some kind of a statement backed up by some strong facts that, you know, here's a group that did come in and do some dirty work that weren't monitored.

And I know Bob's went through the claimant file and they haven't found anything that would suggest that and so I know you guys are in kind of a tough position trying to prove a negative. You know, did they or didn't they? You know, we don't really know.

But without any kind of strong evidence to indicate that highly exposed groups were not monitored, I don't see that this remains to be an SEC issue. That's SC&A's position on it. Certainly left it open for debate and I'm sure that the others have different opinions on it.

CHAIRMAN CLAWSON: Well, you know, it looks fairly good and I'm looking over what we've got into.

But one of the things I'd like to do is go on the record of, you know, find that Lou Doll is in the room with us. He just joined us a few minutes ago. I'd like to welcome him to it.

And I don't see a real big increase but, you know, this is kind of a transition period and I've never seen a transition period where we do kind of ramp up but it's looking like that we've got enough results in here to be able to perform what we did. Have you guys been able to look at this paper very close or --

MR. STIVER: It's basically like Stu said. I mean, these are the results, the numerical results.

CHAIRMAN CLAWSON: Right.

MR. STIVER: The conclusions, I think, are pretty much in line right here on Page 9 as to what they discovered.

I think the most important thing is

they didn't find the information on contracts and what was done and by who and when and that was really the thing that we'd like to have had, you know. Unfortunately that's not always the case.

I think something else we need to keep in mind is the, you know, the time frame, and the kind of concerns over health and safety that were evolving during the '80s would kind of, at least in my mind, lead me to think that you would not have a group come in that would, you know, potentially be highly exposed and then just not monitor them.

Now, back in the '50s I could see something like that happening,'50s and '60s. But in the '80s and, you know, transitioning into the '90s with the RadCon Manual and 835 coming on board, it would be kind of hard for me to accept, that that kind of really unlikely event could have taken place without any kind of evidence to support it.

CHAIRMAN CLAWSON: Right. What about the Plant 9 dust collector release? You

were talking about that subcontractor's --

MR. HINNEFELD: Yes, there was a question. The question was raised why did subcontracting sampling all of a sudden go up in late '83, right? Isn't that what we saw?

End of '83 all of a sudden we start seeing from our capture card, urine card, we started seeing a lot more subcontractor data than we had seen before that time.

And the question was raised several meetings ago why did that happen? And I speculated it might have been the Plant 9 dust collector release and I had misremembered the date. I was off by, that didn't occur until the later part of 1984, so clearly that wasn't the reason why.

Couple things, you know, come to mind. First of all, it could be that there were more contractors on site about that time because this was during the period of the Reagan build-up when a lot of money was put into defense programs.

And Fernald for the first time for

probably 15 or 20 years actually got some capital money and was able to build and remodel some things, so there was an influx of money around that time.

Now, I don't know if that was the exact date but it was during Reagan's first term which would have been '81 to January of '85. It was during his first term that he pushed that expansion of nuclear production capability, project production capability.

And some of that money got in Fernald and so there was more work done then than had been done for a long time of a capital nature, you know, building things.

And then I could mention somewhat facetiously because I have no memory of participating in this, but the fall of 1983 was when I went to work in Radiation Safety Department.

Now, I have no memory of ever saying, hey, we should be monitoring these contractors. I'm not saying that. I would think that if I had been asked I would say, yes,

we should be monitoring these contractors but I don't remember that ever happening.

And I was, like, the second health physicist at the time and the first was really junior. The first was right out of school. She'd only been there about a year or so, so in terms of people with actual health physics training background.

So I don't know if, you know, like I said, I have no recollection of ever doing anything like that. It just, I thought the timing was kind of odd when I saw the date.

I think it's the build-up. I think it was the fact that there was more contract work and since it happened in the fall, which would be the beginning of a fiscal year, it seemed to me that that's probably what the likely event was. That's probably when some capital money became available and more subcontractor work actually started happening.

MR. BARTON: Well, I think there's two, really two facets that you laid out pretty nicely.

One was, really the more important one in my mind, was to try to figure out what subcontractors were actually on the site and what they were doing and is there a reason that they might not be showing up in this data set?

We uncovered a list of, you know, 50-something subcontractors who were under contract with NLO at the time. We really don't know what they were doing and if there's a reason they weren't monitored.

There's also the possibility that they were actually subcontractors to Rust, a sub-subcontractor if you will, and so it was really just a naming convention, the reason why we see such a large proportion of these samples going to Rust.

And during the last teleconference back in April, we pretty much discussed, you know, we just really have to do our due diligence on that first pass to try to figure out what information is out there.

And you guys want to data capture and it's just, you know, we're sort of at the

end of that road where we can't really say either way what subcontractors were at the site and what they were doing and if there's a reason why the name of the subcontractors doesn't appear. So that's the first facet and I think that's a very powerful piece of evidence.

The second facet is this comparison of the '84-'85 years to '86 and '87, and I'm going to have to muddy the waters a little bit here.

I'm looking at Table 6. We actually went through that data set and what we're quoting here is 370 total samples. When you do examine the data, 357 of those 370 samples were only for the first six months of 1986.

An additional 13 samples were compiled essentially from the second reference from 1986, so that covers the last six months of the year. So logically we can make the jump that in 1986 your total number of results is probably going to be somewhere around double.

Now, what implication does that

really have, because the effect is likely not going to be as profound for the number of individuals which, in my opinion, is sort of the more important column there.

If you're pretty much looking at the same size population among all these years and if we can accept that when Westinghouse took over that they had a pretty good handle on things, then essentially your pool of monitored workers is fairly consistent.

Now, it's tough to say because without compiling that actual data in the last six months of 1986, you really can't tell what kind of effect it would have.

Like I said, it's likely the total number for trial would be somewhere around double what's quoted there. The number of individuals would likely increase but obviously that's not going to double. You're not going to have a completely different workforce in the last six months. Maybe you add another subcontractor name or two to this.

But I think what John Stiver said

that's really important here is that we don't have any indication or real evidence that there was a group out there that was doing something decidedly different from these other monitored subcontractors, that we're really missing it to where we can't come up with a bounding approach, a bounding coworker model specifically for subcontractors in these years that is going to totally miss the boat.

I mean I think to make a determination, and this is just my opinion, to make a determination that a coworker model fails you have to have that indication that there was groups out there that were doing something completely different and they were completely ignored and that's why we don't see the name of that specific contractor in these tables.

And as John mentioned, we went in and examined some subcontractor claimant files to see if we could see some CATI reports that, you know, talked about incidents or doing specific work such as, you know, working on the

HVAC systems or pulling out decontaminated equipment and we just really came up empty.

And you combine that with the fact that we feel like information, the contracts simply aren't out there. We don't have radiation work permits that would define what it is the subcontractor out there, that we're totally missing with this.

Really it becomes a judgment call as to whether this transitional period when you can see the number of data points picking up and, like I said, I'm guessing it's going to probably double in 1986 and 1987, not that far off from that number.

Oh, also in 1987 I noticed that there was no or there was only one bioassay sample for the months of November and December combined and this is really just the case that those months weren't included in the underlying reference.

And I did look and I honestly couldn't find bioassay cards for those two months. So, I mean, you could probably expect

the number in 1987 to increase slightly as well.

But, again, what I'm looking at is this number of individuals column. I mean, I think if we're going to have about the same number of workers who were monitored, I think that's a good indication that they at least had a handle on which radiological subcontractors they should be looking at.

And, again, this sort of operates under the assumption that in 1986 to 1987 when Westinghouse was there that they were monitoring the right group of workers.

Like I said, it gets a little muddy just because in Table 6 we're essentially only looking at the first six months so we really don't know necessarily what effect that would have.

But I think what we do know is that they were taking more bioassay samples but were they actually taking them from more people?

I think the effect of compiling that data would be significantly less than the total number of samples but we really don't know what

exact effect it would have on this comparison, which is really the second facet.

But in the end, like John said, we don't have a smoking gun. We don't even really have smoke to indicate a fire because we couldn't find any sort of, as John Mauro put it, the rock to stand on to say, you know, we probably have a real problem here.

And if we're going to make a comparison to I guess what we call the gold standard of the Westinghouse years, again, it looks like the actual individual population is very similar and the actual names of the subcontractors are very similar in what was compiled here and they're really almost entirely for Rust Engineering.

And like I said before, one reason could very well be that these other names that we had in that populated list of 50 were actually subcontractors to Rust and so when they entered the bioassay program they just marked them down as Rust.

Now, one question I did have is in

Table 7 we talk about the unknown group and I assume that's because they just didn't have a company name written on the bioassay card.

It says here, there's a footnote, and that the names were compared to the '83 to '86 results but no company could be identified.

So I guess I'm wondering was there, there was obviously a subcontractor identified in the '83-'86 that you were able to match the name to or, I mean, I guess I don't know how you determined those were subcontractors if they weren't marked as such in '87 and then I'm just not sure if they were marked as subcontractors in the prior years and that's how we were able to determine if those unknown worker categories were, in fact, subcontractors.

MR. HINNEFELD: I don't recall. I wonder if someone on the phone can help out with the meaning of that footnote, the double asterisk footnote.

MR. ROLFES: Gene Potter had gone through -- This is Mark. Gene Potter had gone through the records. I don't know if he's on

the phone today. We might be able to send him an email and see if he can get us a response possibly.

MR. HINNEFELD: There were, at least during some period of time, there were a set of badge numbers that were reserved for subcontractors.

And I don't know if this is part of that time period or not but there was a set of badge number, you know, sequence of badge numbers, you know, thousand numbers or so, that were only issued to subcontractors. And so it may have been from that but I don't know if that's how this was done or not.

CHAIRMAN CLAWSON: Help me understand. You were saying that this '86, and this is Brad, urine results is only for the first six months?

MR. BARTON: Plus 13 samples in the second group of six months. Just the compilation appears to have stopped at some point.

I'm not sure if the original,

because this is actually, these are the same numbers that were quoted back in April when we were talking about this and I'm not sure.

When they were compiled for that meeting, the intent wasn't really to make this comparison. I don't believe we had gotten that far.

Now, it's something we discussed at the April meeting. It just, I don't think it ever got expanded to fully pull in the bioassay samples from essentially the second reference.

1986 is split into two different references and I can provide those numbers if people are interested. The first reference was vetted completely and the second reference wasn't, so.

MR. STIVER: The second one contained 13 entries?

MR. BARTON: Well, the file itself was 89,000 pages long and the entries that were pulled were kind of in the first 100 or so and then the compilation just sort of stopped. So the data is there if we want to go fill out this

table.

Now, in 1987 I was not able to find any bioassay points for November and December of that year so those numbers might increase a bit as well for those two months.

It was noted in this paper and is evident when you look at the data that it's true when you get to the colder months there's generally less monitoring going on, probably because there's less construction projects going on.

So there might not be a drastic increase from just adding November and December in 1987 but certainly there'll be a marked increase for 1986 in these totals.

As I said, I feel what's more important is the actual total number of individuals that were monitored, comparison between those and ---

(Simultaneous speaking.)

MR. STIVER: Yes. That's what I'm looking at. I mean for '84, '85, '86 and '87 you've got pretty consistent numbers of

individuals and if there was something problematic that we're missing in Table 6 for the second half of the year, like more individuals being monitored, you'd expect that to carry through to the following year --

MR. BARTON: Right.

MR. STIVER: -- I mean, depending on how steady the workload was at that time, but.

MR. BARTON: I agree. And another very important facet of this was, one of our main concerns was when we looked at the records that we do have for those two years they were for pretty much two subcontractors.

Well, why is that? Let's look in subsequent years and see if, well, all of a sudden maybe we see that there are 30 different subcontractors that are involved in the bioassay monitoring program, and that's just really not the case. When you look at these totals, '86 and '87 was Rust Engineering and then this unknown column.

MR. STIVER: The patterns just

don't change abruptly. I mean, if there was a problem you expect a lot of big players entered in there in a different distribution among them.

MR. BARTON: Only thing it does change is going to be the total number of bioassay results but what we can't really say is that the total number of monitored workers is going to be markedly changed.

CHAIRMAN CLAWSON: Okay, well, a lot of this work that was done for this is in response to Mr. Doll's letter that he sent to us and I'd like to give you an opportunity if you'd like to be able to ask any of the questions. Have you been able to see this paper that we're looking at?

MR. DOLL: Somewhat, I just got it.

CHAIRMAN CLAWSON: Okay.

MR. DOLL: I'm Lou Doll. I wrote a letter. Concerns that I had with the decision that the subcontractors were only included under National Lead of Ohio from 1951 to 1983.

Having worked both under National

Lead of Ohio and Westinghouse, Fluor after that, the differences in how tests and HIS-20 and urinalysis and safety and the oversight was completely different between National Lead of Ohio and Westinghouse.

It raised concerns with me when I read the report that the decision was made on, that the reason they had started doing more urinalysis in 1982 was because they had the bag house at Plant 9 blow up.

If that's the case, and that's what we were basing decisions on as far as ramping up how we test and give urinalysis for the workers, then the basis for that would have been pushed back two years.

So that kind of threw a red flag at me right away. Like I say, after having worked for National Lead of Ohio, they wouldn't do surveys.

Subcontractors, and it's in your report, they wouldn't even, I mean, they called us intermittent workers. They're not going to be here long enough. Don't worry about them.

We don't have to monitor them. We don't have to test, and that was the attitude that they had.

So lawsuit came out on that and part of this was from Fluor when they came in. Accusations against National Lead of Ohio include putting production first, making safety an afterthought, fabricating records on uranium dust emissions, failing to properly record exposure figures for workers when, in fact, they had been exposed, failing to retest workers whose exposure levels exceeded standards, maintaining exposure records for 150 but 60 of the 150 workers failing to tell one worker he had fibrosis of the lungs.

They lost that lawsuit. They were found guilty and they had to set up monitoring programs and that.

And I know that that doesn't give a basis for the records that you have to look at to make a decision on, but it kind of gives a concern for what are these records really as far as like what you're getting. You can only go

make decisions on the records that you can find and what they say.

However, there's been a lot of concerns over the years and a lot of it came out in this lawsuit, that National Lead of Ohio did not keep good records and the records they did keep, were they totally correct? You know, but like I say, that's not you guys' problem. You guys got to deal with what you got to deal with.

One other concern that I had and the gentleman was before you a few, well, I guess it was over a year ago now but he had dose reconstruction done on this thing and he was there the '82, '83, '84, '85, all the way up to 2005.

And we got his report back. It told him that the majority of his radiation exposure was received during employment as a construction engineer according to records received from Department of Labor and information provided in the interview process.

You know, he brought that to my attention. He brought it to this, you know,

group's attention that how could I have gotten more -- and he became a [identifying information redacted] under Fluor. He was a worker under National Lead and Westinghouse.

And when he got this thing back, it just, and I can see why it didn't make sense to him. I mean, if you're out in the field working in all those different things, you got no coverage, you're getting exposures, you don't know what's going on, and we had different partners when we first went down. We didn't even have clearances yet so we didn't know anything.

We worked in buildings down there without respiratory protection, without anything going on but later on, Pilot Plant being one of them, you couldn't even go in the building without a respirator and full dress-out yet we did all the demolition and everything in those buildings with no protection.

I don't know what the exposure records say, but I know what the circumstances

were and that's what gives me concern.

I think that the SEC petition should go through '85, the total time at National Lead. I think it's a nice, clean break.

I do agree that when Westinghouse came, they did a much better job and changed a lot of the things that National Lab did and they were aboveboard and they never had a problem getting taken to court or anything else for any issues that they had.

So, I mean, I don't know if anybody's got any questions for me about any of this stuff.

CHAIRMAN CLAWSON: Lou, this is Brad. You were saying that you were classified as a intermittent worker --

MR. DOLL: Correct.

CHAIRMAN CLAWSON: -- under the construction work. Now, how many years were you actually on the site?

MR. DOLL: '83 to 2004 and there were a couple small breaks in-between. Now, the other fellow, [identifying information

redacted], he was there straight from [identifying information redacted]. He had 23 straight years I think. There were a lot of people there like that.

CHAIRMAN CLAWSON: Well, and this is -- In the interviews I had heard a lot of people say, well, yes, I was a construction worker but the only thing that changed on me was the contractor I was working for. I'd been out there X amount of years straight through.

MR. HINNEFELD: I think that's true and I think that's the real, I mean that's the reason I'm not, I mean Lou is exactly right. I worked for NOL, Westinghouse and Fluor also and he's exactly right.

And the view that construction workers are considered transient and so they're not going to bust any limit so you don't have to worry about, you know, exposure limit, so didn't have to worry about them, that was kind of what was happening.

And so the question now is but when they did start monitoring in '84 and '85 do we

now have enough data to reconstruct those exposures, which appears to me that we do and that's the only issue we're laying out.

CHAIRMAN CLAWSON: Right.

MR. HINNEFELD: But everything Lou said is right. I had one question. Do you remember when they finished demolition in Pilot Plant to put the new 64 in? Do you remember when that, because I sure don't. I know it was going on in the early '80s.

MR. DOLL: Let's see. Let's see here. Just a second here. Got to find the right one.

MR. HINNEFELD: Because I mean that's kind of, to me, the classic example of a poorly controlled radiological work, you know, radiological construction work, that that was not a good place, that was not a good activity and I don't think it was controlled very well but I don't know when --

MR. DOLL: Okay, Pilot Plant Building 13. Originally hired for 60- to 90-day job to do the demolition of the existing

uranium enrichment process. That was the first job in --

MR. HINNEFELD: '82.

MR. DOLL: -- '83, late '82, early '83.

MR. HINNEFELD: Into '83, okay. Okay, so that's already in the Class.

MR. DOLL: Well, it went through '85 because we had to go back in there when it was running when the coal traps didn't work.

MR. HINNEFELD: Yes. It was more, actually I was just thinking of the original demolition.

MR. DOLL: No, there was a second demolition.

MR. HINNEFELD: Because?

MR. DOLL: On the wet side because we had to go in and tear out the existing stuff on the wet side to put the refrigeration skid in. So we had to do a complete demo in '85 to get that out to put the refrigeration because coal traps wouldn't work.

MR. HINNEFELD: Okay, all right.

MR. DOLL: What we originally put it in they had a problem with the off-gas and the off-gas is HF. So they put it through. They had some different functions, the piping and stuff, so we had to do some stuff.

Finally we got it running and then it couldn't handle the HF through the coal traps so we had then to shut it down. Went in, demoed the, and that was the bad side. You know what was on the wet side of the, that was the right-hand side of the building.

MR. HINNEFELD: Yes, looking south, it's all the piping and stuff.

MR. DOLL: Yes, there was all kinds of stuff left in there, including thorium and everything else, and we went in there and demoed that. There was still stuff in the lines because we ended up having a problem.

One of the things was in the lines that they told us was clean, was caustic. The reason we found out was because our boots started, you know, the leather on the boots started bubbling from that.

So, I mean, out of six lines that we demoed that were in there, three of them had material, you know, liquid materials and stuff and we didn't know what it was. We weren't told. We were just told to get this stuff out and there was no protection at that time.

You know, they told us to take break over in this other, the little room next door. We found out that it was hotter or as hot as the other building were and that's where we were eating lunch at. You know, it was in kind of a maintenance shop. They have a saw in there. They would cut --

MR. HINNEFELD: Yes, what they call the Pilot Plant warehouse across the street?

MR. DOLL: Right next door.

MR. HINNEFELD: Yes, around the corner there or was it kind of --

(Simultaneous speaking)

MR. DOLL: It was right there on the left.

MR. HINNEFELD: Okay.

MR. DOLL: And then Rust trailers

were right beyond that.

MR. HINNEFELD: Okay. Yes.

MR. DOLL: It was a small block building. It wasn't that big.

MR. HINNEFELD: Okay. I think I'm thinking of something else.

MR. DOLL: It had a saw inside and stuff like that but they used it for maintenance. Well, we come to find out later that they would cut the material in there. That's what the saw was for.

Well, that's when -- Then they said, well, construction's a funny duck. They want you to take your break and your stuff in your area. They don't want you moseying off anywhere else for your break --

MR. HINNEFELD: That's right.

MR. DOLL: -- in the morning and stuff.

MR. HINNEFELD: Don't want to lose control, don't want you wandering around.

MR. DOLL: And when we would take break in the morning and that, they'd tell us

to take our coffee and stuff with us and go over here, take a break in this building. So, I mean, you know, you're just thinking everything's cool.

We find out later, I mean, this is in later years like you say, when they completely boarded off 13 later on till they did the demolition on that in 2004 I think and nobody was allowed in and out of it without complete respiratory control yet we did demolition, everything else in there as a first job with no oversight.

I just, I know you guys are looking at what you got and I don't have a problem with that. I mean you guys got to make decisions based upon, but I do have concerns with what was there and the contractor and what I know we were put through and the way we were treated.

I mean, when you go back to the books for 3161, 3162, they did -- 3162 was the medical part of it, 3161 was who was what worker and what were they entitled to --

MR. HINNEFELD: Yes, who ordered

---

(Simultaneous speaking.)

MR. DOLL: Right. And when I went in there, the actual verbiage in 3161 was intermittent workers. That's what the government and the contractors considered construction, was intermittent workers because there was a basis for what you're allowed to get or whatever within this process and that was the tack that they took as far as, like, subcontractors were concerned.

MR. HINNEFELD: And under some circumstances --

MR. DOLL: Expendable was another one.

MR. HINNEFELD: -- if you're going to build a building -- You're going to always need to build buildings and so you would expect the people that build your building to kind of building your building and go away.

But in this instance the same workers, as you say, stayed with either one contract -- And once they got clearance, they

were gold. Whatever contractor was going to be working out there, they would hire the guy that was in there.

(Simultaneous speaking)

MR. DOLL: So, you know, but like I say, between that and, well, I read the thing with the dates and stuff but also --

MR. HINNEFELD: Yes, got that.

MR. DOLL: -- the one individual got back his thing here from Department of Labor. It said that he got more --

MR. HINNEFELD: I know how that happened.

MR. DOLL: I mean it doesn't make common sense.

MR. HINNEFELD: I know how that happened but it's embarrassing, so. It's a --

MR. DOLL: Well, you understand my concerns then about --

MR. HINNEFELD: Yes.

MR. DOLL: Now, you know, I look at -- You say embarrassing. Well, what I'm looking at is these are people filing their

claims and you're saying are getting the best treatment possible and this comes up. So that raises concern. Is this the only one or --

MR. HINNEFELD: Well, they used the job title they have for him which was his last job title and they put that in the essentially boilerplate section of the dose reconstruction.

And when you do stuff like that, if you're not really careful it really hurts the credibility of the product and that's what happened here. We know how that happened.

CHAIRMAN CLAWSON: Well, we've discussed this many times, that when people, you go to the last job and last place that they worked.

MR. HINNEFELD: Yes. The last job title they had is likely the one that's in the database. That's likely the one in the database.

CHAIRMAN CLAWSON: Well, I'd like to open this up to any of the other Board Members on the phone if they have any questions that

they'd like to ask.

MR. KATZ: Paul and Mark.

MR. HINNEFELD: And Phil.

CHAIRMAN CLAWSON: Phil.

MR. KATZ: Phil, right. Everyone. Do we still have you on the line? Maybe you're on mute.

CHAIRMAN CLAWSON: Probably muted.

MR. KATZ: Do we have anyone on the line?

MR. HINNEFELD: Is anyone on the phone?

(Simultaneous speaking)

MR. KATZ: The phone shows that it's -- We have our connection so I know there are people on the line. We're not hearing anyone on the line.

MR. HINNEFELD: Karin or Matt, can you say something?

MR. ARNO: I'm still here and everything.

MR. HINNEFELD: We thought we'd lost our phone connection.

MR. KATZ: So do we have Paul or Mark or Phil still on the line?

MEMBER SCHOFIELD: Ted, can you hear me now?

MR. KATZ: Yes, we hear you now perfectly.

MEMBER SCHOFIELD: Okay. Yes, I was on mute.

MR. KATZ: Oh, I'm sorry.

MEMBER SCHOFIELD: I've got one question on this. Did they use a representative person from, say, some of the small contractors? They would take an escort or something and that person's bioassay was supposed to be representative of the people that he or she was escorting?

MR. HINNEFELD: Phil, I don't have any recollection of that. I don't think that was done. I think if a work activity was determined to be monitored, then people there would be monitored. I don't think that was done, I don't remember that was done. Lou seems to be puzzled as well. He doesn't --

MR. DOLL: The escort, if you're talking about when the porters would go out and work in the plant if they didn't have, in the early days when they didn't have clearances that would have been a guard.

MR. HINNEFELD: Yes, so there wouldn't be any way to associate with that Work Group so they wouldn't have done that I don't think.

MR. ROLFES: This is Mark Rolfes and I did hear back from Gene about Bob Barton's earlier question.

And Gene responded back that the unknowns were Type 50 bioassay samples with no annual routine samples which was the typical pattern that we saw for subcontractors. Some might have also had "sub" written on the card without a company name.

He said another feature was that they didn't have a normal employee number like the NLO employee numbers did. They might have had a different two-number prefix but --

MR. HINNEFELD: Yes, sometimes

they used a two-number prefix and a dash. That was subcontractor. For some period of time there was a, this may be more back with NLO, that there was a period, there was a range of badge numbers that were only assigned to subcontractors.

MR. DOLL: The badges were set up the first two numbers, an 01 or an 02 or an 03, was the craft. And then the second number, which would have then started like 001, 002, 003 or 210, that was the number of the individual as they came into the plant.

So you could almost get a straight line on down as to who got there at what time. You don't have -- I got some dates at my office. But that's how the badge numbers worked as far as Rust Engineering was concerned. First number was the craft. Second number was the individual's number.

MR. HINNEFELD: Okay, thank you.

MR. ROLFES: And then he also added that the meaning of the double asterisk in the footnote from Table 7 was that it was meant to

mean that Gene had looked for the individual names in the 1983 to 1986 time period to see if there were company names in the other years but he couldn't find them in other years.

MR. BARTON: That makes a lot more sense.

CHAIRMAN CLAWSON: Well, I'd like to tell NIOSH we appreciate what they've brought to us on this because this, you know, based on the information we have this is what we have to be able to go with that, you know, they've done due diligence that we have asked them to be able to do.

And, in my eyes, we don't see anything that a coworker wouldn't be able to -- Now, this is only to be used if there's no monitoring data, correct?

MR. HINNEFELD: It's uranium. It's interim uranium only and it is only if there's no monitoring data.

CHAIRMAN CLAWSON: Okay. Well, without, you know, like you guys said, without a smoking gun there's not much that we can do

with this.

But we have evaluated and, yes, it's a little cool in here, done due diligence on this so if there's any more that you had a question on or that we want to clarify on this, this one basically can be closed.

MR. STIVER: I have nothing to add to it.

CHAIRMAN CLAWSON: Okay. What have we got next on the agenda there that we want to go to that? I know that I read a fairly lengthy paper on thorium.

MR. STIVER: Yes. Next on the agenda, back in, I believe it was late June, NIOSH produced a White Paper on thorium internal dose assessment methodology in the post-SEC period and then kind of a companion document to that was released a couple of weeks ago which was the in vivo coworker model. It's kind of a subset of this overall methodology.

And we've been tasked to do a thorough, complete review of it which is getting underway. Anticipate we should have

it completed probably mid to late October.

So at this point, if you guys would like to kind of talk about it, maybe give us the 10,000-foot overview. NIOSH could do that.

I know Bob has a few questions and so do I. We could maybe use that as a way to sort of focus our review going forward.

MR. HINNEFELD: Okay, this is Stu Hinnefeld again. I will give this a shot. I believe the Work Group and then the Board have, the Work Group has recommended and the Board has recommended that SEC Class be added through '78 at Fernald for thorium exposure, internal thorium exposure.

The method for monitoring -- Thanks, Lou. The method for monitoring was proposed to be in vivo monitoring. Well, actually the early, from '54 through '67 the method was daily weighted average air sampling was the proposed method originally and from '68 and later it was in vivo monitoring until we get into, like, '95.

And so the Board and the Work Group

both concluded that the daily weighted average data was insufficient for thorium and that you couldn't reliably interpret in vivo monitoring results in terms of milligrams, in units of milligrams of thorium.

And so Class has been added up through '78 and so we have evaluated what techniques are available after 1978 for assessing thorium internal exposure and so that's what this paper lays out.

This paper also lays out a bit of thorium history at Fernald and how the thorium was handled so let's start with that part. So we're not going to talk about anything earlier than '78 since that's all been decided already.

And then one of the aspects, you know, while Fernald did, in fact, process and produce thorium products for a portion of its history, that all stopped in about 1979. I think their last thorium processing occurred in 1979.

And then from '80 and forward, it was largely storage and then disposition, in

other words getting rid of the stuff.

Now, part of storage, though, was to improve the storage because storage containers were not durable enough because some of the materials were aggressive toward storage containers, shall we say, and corroded the containers.

And so periodically some sets of material would have to be redrummed so there were periodic redrumming operations from '80 until disposition.

So that's really the opportunity for thorium exposure, would be that kind of activity up until the remediation work started in the '80s that this paper describes, the thorium remediation work. Remediation means just, you know, disposition.

There were a handful of task orders. When you talk about thorium work, there were a handful of task orders after 1979 up through maybe '85 or so, not very many.

But those appear to be small amounts to a particular customer and I believe what was

going on there was they were taking material out of storage.

Some of the stuff was good-quality product, thorium oxide that had been made for shipping or for their thorium reactor but had never been sent. It was good quality and well packaged. Those containers held up fine.

And so part of it was getting those containers, you know, shipping a little bit of material to this, kind of this customer or a little bit to that customer, so that seemed to be what those handful of task orders was acting on.

So we're mainly interested in then, you know, can we address exposures between '79 and forward when they were maybe repackaging and then once we get into the disposal activities.

So there is a table in this paper that sort of lays out chronologically the exposure assessment options we have or the approach we have.

And so without going in minute

detail about the information in the paper because everyone else can read it probably better than I can, we'll go if we will to, it's in the summary section and I don't see a table number on here but it seems to be, it's on Page 12 of 147. I didn't print all the appendices so I don't have all 147 pages.

But there are, there is here then, "Thorium doses are recommended to be assigned as follows." This is at the bottom of Page 12 and we start a table that shows chronologically the approach that we intend to use.

So from '79 and through, certainly through '87 or '88 -- I forget when the mobile unit stopped. Do you remember?

MR. ROLFES: '88 I believe. Then they switched over to the IVEC facility.

MR. HINNEFELD: Okay. From '79 then through '88 there are in vivo results from the mobile monitoring facility that include thorium results that are printed, that are reported in units of activity for actinium-228 and lead-212, so we have a number we can

interpret.

After that period, in vivo was done in a fixed facility and a thorium intake or thorium burden would have been identified in the fixed facility as well and reported.

Now, that system did a peak search and would identify what radionuclides were there. The mobile unit always gave you a result on lead-212 and actinium-228 and uranium and so on based on a calculation of certain areas, certain areas of the spectrum.

So you always got a result on the mobile counter. You wouldn't necessarily see a specific thorium result on a fixed counter unless it's identified to be there.

MR. STIVER: Excuse me, Stu.

MR. HINNEFELD: Yes.

MR. STIVER: When did you say the peak system or the fixed system came online? In 1990 and --

MR. HINNEFELD: Think it was '89.

MR. STIVER: Or '89?

MR. ROLFES: Yes, I said '89 I

think.

MR. HINNEFELD: So '88 or '89 is when it came on.

MR. STIVER: Just checking. How long was that system in use after that?

MR. HINNEFELD: Well, they shut it off -- Before I left it was shut down I think.

MR. ROLFES: I used it in 2001. I know it was still going then.

MR. HINNEFELD: Yes, somewhere between 2001 and 2003 they turned it off.

MR. STIVER: All the way up to the demolition phase and so forth?

MR. HINNEFELD: Yes, I mean, the building was, yes, health and safety building was torn down. The in vivo facility was, actually to a good extent I think it outlived the health and safety building. It was almost sort of a little appendage on it but I think it outlived the health and safety building by a little bit.

So for individuals then who have in vivo data, and that's a lot of people because

anybody who got in vivo'ed in the mobile counter or anybody who got in vivo'ed is going to have an in vivo result.

We intend to use the in vivo data and missed doses and things like that if they are a job category that could have been involved in the repackaging.

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 could have safety and health people. Might have security people there.

So you've got to be pretty inclusive about the kinds of people that you would include in that. Even though it's only probably a small group of people who actually did the overpacking, we don't want to miss someone who should be included. So we would include in those, those people who might have been involved in some sort of exposure.

And, in fact, this period then

extends into the remediation period as well but people who might have been exposed, they will get, if they have in vivo data they will a missed dose. And this goes through '94. I'll explain that in a little bit.

If you don't have in vivo data, then from '79 through '89, which is I guess the mobile period, that's when we have all, for the mobile period we have all the bioassay results that were done because they were kept in log books, in a log book or essentially a book of results. And so all the in vivo results for anybody, regardless of whether they're a claimant or not, we have those.

After 1990 when you go to the FITS system, we only would have the in vivo results for claimants. We don't have the comprehensive list of in vivo data, so the coworker model then is intended to address the years of the mobile monitoring when we have all the in vivo data.

For the years '90 to '94 when we no longer have all the in vivo data, all we have

is claimant, you know, data from the claimants, we're proposing to use the control level that was exercised.

And our document makes several references to reports that were done during these activities, repackaging activities that were going on in '90 to '94 and the kinds of controls that were imposed and, you know, including when respiratory protection would be required.

And so our proposal is to use for this '90 to '94 period 10 percent of the derived air concentration for thorium-232 which was the control level in multiple, you know, multiple things that were written there.

And so this would have been, '90 to '94 would have been the end of Westinghouse, the last couple years of Westinghouse and then moving on into Fernald, into Fluor which I think started in '92 I think.

And then for the, and then Fluor instituted a 100 percent BZ air sampling regimen for thorium work while they were there.

But it appears to me that that wasn't fully in effect until '95 even though Fluor got there in, like, '92. The 100 percent BZ, we haven't found that it's completely 100 percent implemented until '95.

So from '95 until 2006, which was site closure, everyone who worked around thorium, every person wore a BZ sampler and we do have that BZ sampling database, all the data from that. So we would propose to use the BZ sampling database for individuals from '95 to 2006.

Now, we also have in vivo data from there so, you know, in this case if we have positive data from in vivo it would trump negative data from BZ and vice versa, I mean, a negative in vivo, if you've got less in vivo, then you use the BZ data for the person.

So those are the proposed, you know, that's the various methods we're proposing for the various time periods post '78 based on the data available and the information we've gathered to date.

And then the paper goes on in some length to describe, you know, the various approaches and then there were some pretty voluminous appendices about how the data would be used.

MR. STIVER: I guess the thing that kind of jumped out at me was that 1990 to '94.

One of the questions I had was, you know, whether or not enough data to, you know, fill the coworker model or just extend what we already had and then explained that fairly well. I guess the data just weren't --

MR. HINNEFELD: Well, we'll have, I mean, if we had all the in vivo data, we would continue to use the in vivo coworker but we only have claimant.

MR. STIVER: But only have it for claimants, yes.

MR. HINNEFELD: All we have is claimant data from the in vivo for them. Ironically there probably is an electronic record someplace of all that but, of all that in vivo data because it was done on its own,

had its own MicroVAX that, you know, ran the system and I think they recorded it all.

MR. STIVER: What became of it after that?

MR. HINNEFELD: What became of that MicroVAX and the data that was in that, that's the question.

MR. STIVER: For that period where you propose to use 10 percent of the DAC, did you go into the future years or, not future years but, you know, '95 and beyond and kind of do a verification based on breathing zone data that you do have, whether that would, in fact, 10 percent of the DAC would be bounding, assuming that nothing had changed, you know, from '90 through '95 and beyond.

MR. HINNEFELD: Well, I don't --

MR. STIVER: Kind of a verification.

MR. HINNEFELD: Yes, I don't remember offhand and I don't know if anybody on the phone can comment about that or not. I don't remember that being done. I don't know

if anyone on the phone who was more engaged in this product can remember that or not.

MR. STIVER: I guess as kind of a follow-on to that there's got to be quite a few workers from '95 and beyond who would extend to earlier years too so, you know, it might be useful for identifying who was who and what they might have done and so forth.

MR. HINNEFELD: Okay. I'm not sure I understand.

MR. STIVER: Well, I mean, let's say you have data for workers who were identified from '95. You know, they're claimants obviously. You can go back and look at their records and see, you know, were they also in that earlier period.

You might be able to kind of build a, not really a coworker model but just to kind of get an idea of how many would also extend into the earlier years when you have to use the DAC as opposed to the actual data.

MR. HINNEFELD: Okay, so how many people from the '95 and later --

MR. STIVER: Yes, were actually still in --

MR. HINNEFELD: -- were also working '90-'94?

MR. STIVER: Yes, still in that kind of a gap period of four years where you don't really have data for everybody. I don't know. I'm just going to cut, you know, cut off here. I don't know if that would really be useful in any way other than to kind of identify what proportion of workers, you know, would still have follow-on of monitoring activity later on or that it might be possible to find earlier data.

MR. HINNEFELD: I don't know. I mean, there is some BZ data before '95 but it didn't seem to be comprehensive until '95.

MR. STIVER: Yes, yes. And then anything that was in HIS-20, basically that's going to be your only source for the breathing zone samples.

MR. HINNEFELD: I believe that's HIS-20. I think it's the database --

MR. STIVER: Yes. Yes, there isn't any other --

MR. HINNEFELD: BZ.

MR. STIVER: -- source you could go look for to maybe, to run it to ground and --

MR. ROLFES: Independent of references, you know, handwritten references in the Site Research Database that we have gone through. We've used HIS-20 as our comprehensive source.

MR. STIVER: One kind of overarching question I guess is I see in a lot of these thorium White Papers that have been going on, exchanging over the course of several years now, I guess, you know, your contractor, ORAU, always mentions that this would be applied to thorium workers, you know.

MR. ROLFES: John, sorry to interrupt you. I was asked if you could speak up a little bit.

MR. STIVER: Oh okay, sorry.

MR. ROLFES: I think we're having trouble hearing you on the phone.

MR. STIVER: Not quite close enough to the mic here. Let's see, where was I?

MR. KATZ: Overarching question.

MR. STIVER: Oh yes, yes. A lot of your papers have identified we're going to apply this towards thorium workers and, you know, our research has shown that prior to about 1994 I guess when some of this new information came along, this really job-identifying information is kind of sparse to say the least.

And so, you know, the two SECs that were based on thorium really give it to everybody because, you know, it's just impossible to say who was, you know, exposed at what time in what building and so forth.

So I see that kind of logic is kind of being carried through in this paper, so I'm just kind of curious. Do you guys have other sources of information you'd be able to find that identify job categories prior to 1994?

MR. HINNEFELD: Well, I mean, there are --

MR. STIVER: Anything new I guess

that we haven't looked at before?

MR. HINNEFELD: There was a fair amount of thorium work done by subcontract. If you read the paper, there's Project 1, 2 and 3.

MR. STIVER: Yes.

MR. HINNEFELD: Project 1 was done by IT Corporation, which was removal of the thorium from silos within Plant A. Project, or now it wasn't 2 or 3. It was the neutralization of the UNH. The Pilot Plant was done by Chem-Nuclear. And so, I mean, those are separate, distinct categories of people we know who do that.

There's some information here about a list of job titles of people who were trained I think for one of the thorium projects, you know, the kinds of people who were involved in that.

But I really, I don't know that we're ever going to find, like, names that we can say this person specifically went in and, at least not with the data available.

MR. STIVER: So you're saying that

the three projects, all three of them used subs for the entire amount of work?

MR. HINNEFELD: No, no, no.

MR. STIVER: They were separate?

MR. HINNEFELD: No, thorium overpack was in-house.

MR. STIVER: Project 3 was the --

MR. HINNEFELD: Project 3 was in-house.

MR. STIVER: Okay.

MR. HINNEFELD: Project 1 was bins and silos and I forget what -- Oh, Project 2 was the outside storage. Yes, that was in-house.

MR. STIVER: Okay, that was in-house as well.

MR. HINNEFELD: Yes.

MR. BARTON: Seemed like from your description and reading the paper there's kind of a list of pretty broad job categories. Those would be, at least being proposed to be applied up through 1994 or just for the in vivo period through '89, because it seems like once you get to 1995 you're kind of saying that

they're pretty much defined by the fact that they have breathing zone.

MR. HINNEFELD: Yes, current breathing zones. From '95 forward they're defined by having breathing zone air sampler for thorium.

MR. BARTON: Right. So you're essentially saying there's no coworker model after 1994?

MR. HINNEFELD: Correct.

MR. BARTON: Right, okay. I guess another question I had about that with the breathing zone specifically and I haven't been able to dive into the references yet but, I mean, when we say that breathing zone is provided for all thorium workers, I mean, are we talking, you know, the main handlers of it?

But what about, like, you know, sort of ancillary workers that might have been in close vicinity, like a security guard or something like that? I mean, would they have to also been included in the breathing zone?

I mean, is there a possibility that

you'd have workers who do have exposure potential but maybe weren't considered thorium workers for the purposes of breathing zone?

MR. HINNEFELD: Well, by this time, by '94, things were pretty controlled. You know, Fluor had been there a while and they brought a lot of rigor to these things, even more so than Westinghouse.

MR. BARTON: So pretty much if you were in the vicinity of a project, you were going to have a breathing --

MR. HINNEFELD: A project, you know, a thorium work area would, you know, the thorium area would be defined.

MR. BARTON: And anyone entering that --

MR. HINNEFELD: And if you're going into this, into the thorium radiological area or the airborne, you know, potential airborne area, everybody had a BZ with them.

You know, I went in. When I would go in to do an observation, you know, I was some pencil-pushing manager, I wore a BZ. That's

what I was. I didn't do any real work.

MR. STIVER: So you didn't have to worry about, like, janitors and staff?

MR. HINNEFELD: If they went in, they wore BZ.

MR. STIVER: You're pretty confident that --

MR. HINNEFELD: Yes.

MR. STIVER: -- anybody who went in that area had --

MR. HINNEFELD: You went into that area, you wore a BZ.

MR. STIVER: And all that data is captured?

MR. HINNEFELD: It is all in HIS-20.

MR. STIVER: It seemed like a pretty high bar to set, that we have no unmonitored workers during this period of time.

MR. HINNEFELD: I'm pretty sure there are not. I mean, it was controlled. The area was controlled, you know, to the point of having manned, you know, manned patrol and so I'm pretty sure that anybody who went into the

thorium area from '95 on had a BZ sampler.

MR. STIVER: Now, back to Project 1 and 2, I know that IT did the Project 1 in '89. Were they also doing the D&D of Plant 8 silo, did they also do all of the, do it from start to finish?

MR. HINNEFELD: Yes, I believe, IT did that whole thing.

MR. STIVER: Okay, all right. Those kind of questions, whether there were somebody else or some of the in-plant workers might have done the D&D but it was all contracted out then?

MR. HINNEFELD: Well, I don't really remember. The paper reports that that was all part of Project 1, of the silos, the bins, not Plant 8 itself. You know, Plant 8 was still there when that project was done.

MR. BARTON: So I might have heard the answer and it just passed right through one ear and out the other. I'm trying to get a handle on how we're assigning the proposed coworker intakes. Like I said, there's a list

of workers that, and one of them is, you know, operations, you know what I mean, pretty broad category.

It seems like what you're actually saying is that unless you were a secretary or something like that, an administrative position, then you wouldn't even have come close to these sites of operations so it's not appropriate to apply coworker intakes. I mean, is that essentially what we're saying or, I mean.

MR. HINNEFELD: Yes. I think it's going to be a pretty wide net because, you know, to avoid excluding people that should be included.

MR. BARTON: It almost seems like it would have been better to just go from the other direction and say everybody gets it unless you were clearly an administrative worker, that kind of thing, because I mean --

MR. HINNEFELD: Well, I think that's probably, I mean, we put some examples of jobs here that, and the jobs we listed were

jobs that were identified I think by the training roster, right?

But I think in actuality the approach will be unless this person was clearly administrative or cafeteria worker or, you know, someone who clearly is not going to be in a process area, unless it's somebody like that, they're going to be in.

MR. BARTON: For the 1990 to 1994 period where proposing using the percentage of the DAC, I assume we're not using in vivo results because we simply don't have them for the entire work force. We only have claimant results?

MR. HINNEFELD: We only have claimant results.

MR. BARTON: Do we have an idea of maybe how many claimant results we actually have to, I mean, I'm not sure. I mean, I don't think it would be the first time that you actually built a coworker model based on claimant data.

MR. HINNEFELD: I mean, I suppose

that could be feasible. Mark, could you make a note of that?

MR. ROLFES: Yes.

MR. HINNEFELD: I mean I think that's something we could try. We'd have to look for claimants who have employment in those years and we'd have to open each file to see we have in vivo results.

MR. BARTON: It would be interesting to compare for the claimants who have the monitoring results, compare what those intakes would be versus the 10 percent DAC value, I mean, 10 percent is a little above what --

MR. HINNEFELD: Realistically I think most monitored, most people with potential exposure were probably monitored. Now, they were probably monitored for the purpose of potential uranium exposure but if they found thorium, I mean, in vivo it would pop out, so.

MR. BARTON: And the actual --

MR. HINNEFELD: I would think most,

you know, occupationally exposed people who worked in that period are probably going to have at least one in vivo count in a four-year period if they were there the whole time. I would think they'd have maybe more than one.

MR. BARTON: And just the all worker data for that period is just not available? Like nobody knows where it is or, I mean, is it possible that that could be obtained?

MR. HINNEFELD: I think we've looked for that already. I think we've looked for it and have not been able to come up with it.

My concern is it's an electronic record, whatever data storage the MicroVAX was using, and I don't know that it ever got translated into a paper record.

MR. STIVER: The claimant data would be available for us to review then?

MR. HINNEFELD: Claimant data, sure. Yes, claimant data would be in the claim file.

MR. STIVER: And how about the breathing zone data? We'd certainly like to take a look at that.

MR. HINNEFELD: Breathing zone data is in his claim. We should be able to get that.

MR. STIVER: Get to that?

MR. HINNEFELD: Have you seen that?

MR. STIVER: Yes.

MR. HINNEFELD: Okay.

MR. ROLFES: I'm just looking at a document from the Site Research Database. It looks like ORAUT had taken a look at 248 uncensored lead-212 chest count results that were collected between 1998 and May of 2002.

So we've got at least 248 results that we can use possibly to, you know, calculate lung burdens and compare those to the derived air concentration.

MR. BARTON: That's a different period of time though.

MR. STIVER: Yes, it's a little bit later.

MR. HINNEFELD: Yes, that's later. That is 2002, we are looking '90 to '94.

(Simultaneous speaking)

MR. STIVER: Yes, there's some way you can do some sort of proof of principle to, you know, demonstrate that 10 percent of the DAC would be validated, you know, even looking at, you know, later data.

I'm assuming that you look at DAC, extrapolate, you know, assuming exposure potential would be changed or even, better yet, you could find some of the claimant data and use that.

MR. BARTON: Yes.

MR. HINNEFELD: You know, I think there are probably references to describe when respiratory protection was required on some of that work in '90 to '94 because, I mean, we're talking about pretty mature programs in the 1990s.

MR. BARTON: In looking at the proposed coworker for the in vivo data 1989 and prior, I notice that it did calculate intakes

at the 95th percentile which, you know, would be used as a constant, but there weren't really any instructions as to when that kind of a intake would be applicable, I mean --

MR. HINNEFELD: Well, we didn't go to that degree of detail in, you know, establishing at this point, you know, establishing here are the techniques, essentially what we proposed.

And there are some, granted, there are some decision criteria that have to be bandied about, you know, when you assign the coworker, you know, so on and so forth.

But it has to be, you know, this is sort of a demonstration that we believe that dose reconstruction is feasible and that we understand that there would be some additional essentially Site Profile questions. How will these approaches be applied?

MR. BARTON: That sort of has larger implications for the SEC Work Group, in going through with the implementation of that. I was just curious if you had any ideas or

thoughts on how that might apply in this case, but that's farther down the road.

MR. HINNEFELD: Which, ideas and thoughts in terms of --

MR. BARTON: The application of, say, the 95th percentile to a given worker versus the GM and the GSD and you're actually calculating the POC.

MR. HINNEFELD: I guess I'm not --

MR. STIVER: It's something comes up in a lot of settings outside of Fernald basically. You know, what's the guidance to does reconstructors, kind of up to them, you know, using their own knowledge and experience. This guy deserves a GM or he was highly exposed or 50th percentile or a constant.

So something like that would go into a final coworker model. It would be the next step down the road.

MR. HINNEFELD: Right, right.

MALE PARTICIPANT: Is there any other question?

MR. BARTON: This one is kind of

specific so maybe this one is for someone on the phone.

I was just curious. They used a post-weighting OPOS calculation just for the 1989 and prior period. I was just wondering because when you look at the data set there's a lot of very negative numbers that are in there and I was just curious if those were adjusted at all because I didn't see any mention of it.

When you have, like, a result of, like, 212 with minus 40 nanocurie, you know. I don't if that was removed or if it was adjusted to 0 or, you know, how these, because, you know, over 95 percent of the observed data for that period is essentially below what we believe to be the MDA.

So there's a lot of results that are kind of in that gray area and I'm just curious if those were treated in any manner to adjust them or if you sort of took them at face value and plugged them into the OPOS calculation and --

MR. HINNEFELD: Well, Dr. Neton and

I had a discussion about this following the last SEC Issues Work Group and it's pretty clear that if you're doing a weighted OPOS that a negative result can't really be treated as a negative because essentially you're subtracting exposure for some period of time if you treat it as a negative.

So I don't know if we came to a resolution about how it would be treated. It would have to be adjusted either to 0 or to a limited detached inner half of MDA or something.

There's some sort of adjustment has to be done because you can't leave it as a 0. If you're going to be a time, it's got to be time weighted.

MR. STIVER: A lot of the graphs that you present, I mean, first of all, the data was adjusted for bias in actinium in the lab.

MR. HINNEFELD: Yes.

MR. STIVER: And then the plot, basically just so that the slope of the line to the null distribution, the normal distribution

for the sub-MDL data will go to 0 so it's going to be your geometric mean or your mean for that data. So I didn't have any problem with that.

I guess the other aspect of this using Report 44 -- This kind of is related to OPOS I guess. The data above the MDL are going to be used as they have been --

MR. HINNEFELD: Yes.

MR. STIVER: Whether it is going to be a weighted program or not, I guess your question was really what are you going to do with, you know, the old data --

MR. BARTON: Yes. Well, I was just, like, it's not evident immediately upon inspection of report if it necessarily adjusted, like Stu was saying, whether you treat it as --

MR. STIVER: Will it be used at all or, you know, there was the idea that a maximum possible mean, at one point some of our earlier discussions, the SEC --

MR. HINNEFELD: Yes, maximum possible mean would have adjusted it to, and a

negative result would have been adjusted to, like, the minimum detectable or something, right. I don't remember exactly. I don't know if we actually reached a final decision as part of the SEC Work Group.

MR. STIVER: Yes, this is all kind of ongoing at that point.

MR. HINNEFELD: So it's wrapped up in that and I don't know that we've really reached a final decision on it but Jim and I did talk about it and agreed that if you're time weighting each sample it makes no sense to include them as a negative.

You can't include a negative because you're essentially subtracting exposures, whatever period of time that sample represents, and that doesn't seem to make any, that doesn't make any sense.

MR. BARTON: Yes, because, I mean, I'm looking at Table 2 of the coworker study and even at the 84th percentile they're all, for every year that we're looking at here, they're all essentially half the MDA.

MR. HINNEFELD: Of course, recall, this, to me, it is not all that surprising because we're coming to years of '79 through '88 when the thorium for the most part was sitting in warehouses.

And these in vivo results came out because people were being monitored for uranium and this thorium result popped out. So it's not surprising that there's that not really much --

MR. BARTON: Right, there was exposure because we did observe samples taken from workers in that time period after production had ceased that were positive, whereas they also had samples in 1979 when production was still going on that were not positive so, you know --

MR. HINNEFELD: There were examples, you know, being exposed.

MR. STIVER: There were some exposed personnel in that area.

MR. BARTON: So these could be low because the workers actually involved in

thorium were just a smaller population of the overall monitored population, not that they necessarily, the exposure potential was that low. It could be just an artifact of a smaller number of workers actually involved who could have had the exposure potential.

MR. HINNEFELD: Well, then the question, you know, so of the people who could have been thorium exposed, you know, we have some here who were actually monitored.

The coworker would be applied if we don't have in vivo result for some reason and in all likelihood in the job titles that we're talking about there's probably not going to be very many people to actually get this coworker model because most of the people are going to have an in vivo result if they were potentially exposed.

There were some, you know, claustrophobes who couldn't tolerate being in a mobile counter, you know, and maybe some people for some reason or another weren't there particularly long but most of the people who

were potentially exposed, you're going to have an in vivo result back there. You're going to have in vivo monitoring.

MR. STIVER: Do you have any other question or --

MR. BARTON: I don't.

MR. STIVER: Anybody on the line? Maybe John Mauro or Joyce have some questions about the post-SEC thorium?

DR. MAURO: Yes, I'm here listening in.

CHAIRMAN CLAWSON: Was that you, John?

DR. MAURO: Yes, I'm here listening.

CHAIRMAN CLAWSON: Okay, did John have anything to add?

DR. LIPSZTEIN: This is Joyce. I have a very technical question. My --

MR. KATZ: Joyce, can you speak up? Let me turn up the volume here too, but okay.

DR. LIPSZTEIN: Can you hear me?

MR. KATZ: Yes, that's much better.

Thanks.

DR. LIPSZTEIN: Okay. I have one technical question about the use of lead-212. How is NIOSH going to assign the time of exposure in relation to the time of preparation of thorium, because mostly -- I don't know if this is too technical and we shouldn't discuss in our Working Group meeting. I basically agree with everything that NIOSH is doing on lead-212.

Actually we already sent, had a White Paper. SC&A had a White Paper saying the same thing so we agree on mostly everything and we agree that the pattern is not exactly SEC issue. Is probably a CDC issue on how to use the lead-212.

And I couldn't understand from the draft paper, the next draft paper that NIOSH gave to us. How did time of lead, of measurement is going to relate to the time after separation, because after one year after separation, the actinium and the lead-212 should be the same. Most of the measurements

after they were corrected for bias they are the same.

So I don't know what this model is unfavorable, if it is used one year after separation and then we would expect lead and actinium to be the same or if NIOSH is going to use another time before one year after separation and use actinium as -- for its rating. So I think this has to be clarified.

As for the coworker model, I have a thing that I would like to ask.

MR. HINNEFELD: This is Stu. Could we --

DR. LIPSZTEIN: It's very difficult to ask.

MR. HINNEFELD: -- break before we get into this?

MR. KATZ: So, Joyce --

DR. LIPSZTEIN: Yes.

MR. KATZ: Stu just asked if we could take a brief --

MR. HINNEFELD: Take a brief break before we --

MR. KATZ: -- comfort break before we started out on this topic.

MR. HINNEFELD: Yes, before we get into discussion on this topic?

MR. KATZ: Sure. So let's take a, is ten minutes enough, 15 minutes --

MR. HINNEFELD: Ten.

MR. KATZ: Ten minutes? So let's just, the line will stay on. I'm just going to mute the line but it's 10:30 so 10:40 Eastern Time we'll pick up again.

(Whereupon, the above-entitled matter went off the record at 10:30 a.m. and resumed at 10:42 a.m.)

MR. KATZ: So we're back. Let me just check and see if we have our Board Members back on the line with us and Joyce too, so --

MEMBER GRIFFON: Griffon on the line.

MR. KATZ: Great, Mark. And, Paul, are you back on the line? And you, Phil? Might be on mute. Paul, Phil, are you on the line?

MEMBER ZIEMER: I'm on the line. This is Ziemer.

MR. KATZ: Great. How about you, Phil? And about Joyce, Joyce, are you on the line? Joyce? Maybe you're on mute. Well, we need Joyce to get going here. Joyce?

MR. BARTON: I know she was out of power so she may have turned off her cell phone just for ten minutes to come back on so she didn't waste the battery.

MR. KATZ: Joyce, are you on the line? Wonder if we can pop her an email to check with her.

MR. BARTON: Power's out, so.

MR. KATZ: Oh, right.

MR. STIVER: Actually she was able to connect onto their email.

MR. KATZ: Oh, really?

MR. BARTON: I don't know if she's using Gmail on her phone or what.

MR. KATZ: Joyce, are you back on the line?

MR. HINNEFELD: Now I feel bad for

asking for a break.

CHAIRMAN CLAWSON: What about the other Board Members? Were they there?

MR. KATZ: Yes, except for Phil. I haven't heard from Phil.

CHAIRMAN CLAWSON: Okay. While we're waiting for Joyce, I've got a question for Stu. I'm just trying to understand something about Fernald and this is the thorium storage facility. Where was this?

MR. HINNEFELD: There were a few. Buildings 64 and 65 which are sometimes referred to as the thorium warehouse and the old Plant 5 warehouse were on the north. They were northeast on the property, kind of removed from areas where people typically work.

There was a thorium warehouse over by Plant 1. I forget the building number right now, might be 66 or 67, that left -- my recollection was that was mainly where the nice-quality stuff was stored. Stuff they made for medicine for the most part.

And then there was some thorium.

At least for some period of time there was thorium stored in what's called the Pilot Plant warehouse which was on the southwestern part.

These all were kind of on the outer rim of the production area from the northeast, out. You know, the main production area really went through Plant 9 which was the northeastern-most plant and then you still have the next block up was the thorium warehouses.

And then going to the northwest you had Plant 1, which was the most northwestern plant and across the street was the Plant 1 warehouse.

And then the Pilot Plant was really even a little more south. You know, you think of this kind of square production area. The Pilot Plant really was kind of down, over here and down, and then the Pilot Plant warehouse was, I want to say west of it. Must have been west of it. It's getting hard to remember all this stuff.

CHAIRMAN CLAWSON: Right. Well, the reason why I was wondering this is because

at numerous other sites, Hanford in particular in some ways, actually Fernald became the thorium --

MR. HINNEFELD: Yes, the thorium repository, yes.

CHAIRMAN CLAWSON: Repository because I was sitting there looking at Hanford and I saw train cars of --

MR. ROLFES: Tetrahydrate, TNT?

CHAIRMAN CLAWSON: Right. Being shipped out and stuff like that and I was wondering how and where it went. I guess I was visualizing in my mind that these warehouses weren't really all that big. I thought they were just kind of fairly small but to be able to do a lot of this it looks like they were fairly large buildings.

MR. HINNEFELD: Well, you remember when they were shipping back the TNT from, I mean, that would have been dissolved by Pilot Plant? They run through the Pilot Plant?

MR. ROLFES: That would have been, yes,'60s time period, '70s, early '70s when

they were shipping. I think there were 30-something train carloads that had gone from Hanford back to Fernald because that was one of the issues that we had discussed with the contamination levels of U-233 in the thorium --

MR. HINNEFELD: Oh. Well, in that case, that was what was then dissolved and that's what was stored, is thorium nitrate. They placed the thorium nitrate in the Pilot Plant.

CHAIRMAN CLAWSON: Okay, that's, you know --

MR. HINNEFELD: So I don't know when it came in. I don't know where they staged it because this, you know, in the '60s and so on, that's well before my time so I don't know when these train cars came in. I don't know where they offloaded and staged it or anything like that. But in terms of the actual, you know, processing of it, they --

CHAIRMAN CLAWSON: That's what went through kind of --

MR. HINNEFELD: That would have

gone in the Pilot Plant and if it had the, if it's the stuff of U-233, that was the thorium nitrate that was stored at the Pilot Plant, you know, the liquid, that was project whatever.

No, it's not one of the projects. It's what Chem Nuclear took care of many years later because that was stored there.

The entire time I worked, you know, almost the entire time I worked there, there was this thorium nitrate and we had U-233 on the nuclear materials inventory. It was only the U-233 that was a contaminant in the --

(Simultaneous speaking)

MR. STIVER: Right. Savannah River, I mean, just stayed on the tracks for 20 years.

CHAIRMAN CLAWSON: Yes, because in the Site Profile stuff, I read of these train cars and stuff like that, of having, breaking down, having redrumming runs and so forth like that. There were so many different ones like this --

MR. HINNEFELD: There might be a

little thorium nitrate spread between here and Hanford along rail lines as far as I know. Or highways. Yes, or highways.

(Laughter)

CHAIRMAN CLAWSON: Okay, well, I thank you. I was just trying to figure out. I was trying to just make a mental picture of it because trying to, all the different buildings and --

MR. HINNEFELD: 65 was pretty big. 64 was not quite as a big.

CHAIRMAN CLAWSON: Yes, we'll talk about the others --

MR. HINNEFELD: If you want to talk at lunch or offline, we can talk, sure.

MR. KATZ: Let's check and see. Do we have Joyce back on the line?

DR. LIPSZTEIN: Yes, I am.

MR. KATZ: Oh, great. And do we also have Phil back on the line? I think I heard him cough or something.

MEMBER SCHOFIELD: Yes, you do.

MR. KATZ: Okay, great. We're

ready to go then. Go ahead.

MR. HINNEFELD: Yes, Joyce, I'm sorry I interrupted you but if you could start up again, I would like to address this entirety because I just wasn't going to last very long without a break.

DR. LIPSZTEIN: Okay. Maybe it's better if you look at Page 109 of your draft, the nice document that you sent to us.

MR. HINNEFELD: Is this strictly the coworker?

DR. LIPSZTEIN: No, it's not the coworker.

MR. HINNEFELD: Okay, so it's the other one. Okay, I didn't think coworker --

DR. LIPSZTEIN: It's the other one. Page 109.

MR. HINNEFELD: Okay, I'm at 109 now.

DR. LIPSZTEIN: Okay. So you see there is Figure 6?

MR. HINNEFELD: Yes.

DR. LIPSZTEIN: And then there is a

paragraph just below Figure 6. And the last sentence says, because thorium separation activities ceased at Fernald in 1979, a time post separation of over a year is most likely the case.

MR. HINNEFELD: Yes.

DR. LIPSZTEIN: We agree with that. It's a technical thing of how you calculate it from lead and actinium because all this paper says, that intakes are going to be calculated from lead-212 and the actinium is going to be considered, assumed to be from unsupported radium, which is okay also. It should be unsupported radium maybe. I don't know.

But, anyway, as you go from '79 to '89 you have more than four year. You know, you have ten years after '79, so it's ten years after separation. So there are some years where actinium and lead-212 are going to be the same amount, predicted to be the same amount.

And if you consider that there was chronic intakes instead of acute intake because Figure 6 is for acute intakes, if you considered

chronic intakes after one year after separation, so would be after '80, the activities of lead-212 and actinium-228 would be predicted to be equal.

And in this document also you have shown that after correction for bias, most of the weight of actinium and lead can be considered like equal activities.

So my question is, are you going to deal differently with this first five years on how to calculate the activities from lead? Are you going to consider acute intakes? Are you going to consider chronic intakes? Suppose you just have one result for that worker?

MR. HINNEFELD: Well, I'm going to ask if Tom --

DR. LIPSZTEIN: So this is very technical.

MR. HINNEFELD: Yes, yes. I understand. I'm going to ask if Tom LaBone has been contacted to get on the phone.

MR. LaBONE: This is Tom LaBone. I'm here.

MR. HINNEFELD: Okay, Tom, I believe this is the part of the conversation that you took your time off from vacation for. Is that right?

MR. LaBONE: Yes, yes.

MR. HINNEFELD: Okay. Do you want to respond?

MR. LaBONE: Tom LaBone, ORAU team. I have no conflicts with Fernald. I did not introduce myself at the beginning.

There's two things here, I guess. The first is that we have a standard mixture for thorium that's triple-separated thorium which will give a bounding intake if you're going to go off of lead-212 and so you don't have to specify the relationship between the chest count date and the date of the separation of the thorium.

And that's described I think in this paper and also there is a new OTIB-76 out which goes into great detail on how to actually do these calculations. It gives guidance to the dose reconstructor.

And in that same document it talks about, again, the situation that Joyce is talking about where you know separation stopped in '79. That gives a table for each year after that, what the, basically the ratio is between the actinium and the thorium.

So you can use the actinium if it's not during the time frame in which separations are taking place, I think, which is the most of the data which will be in after it.

So this is basically to give some flexibility to the dose reconstructor depending upon what information they have, use either lead or actinium. So I don't know if that addresses the question, but I can go on from there if you need some more detail.

DR. LIPSZTEIN: Okay. No, that's okay. And I think we're done. I don't have anything, you know, major except for some details on how to calculate things.

And another thing that I would like to ask for the coworker data, I don't know if it's -- in the past when we reviewed coworker

data, there was a special file in the O: drive that could see which data was used in the coworker model.

Now we don't and so in other files to review the coworker model without knowing exactly which data were used because some of, you know, even if we have the data from, all the in vivo data from somewhere not used for one reason or another.

And so for us to review the coworker model would be much easier if, as before, we had the raw data that were used in the model.

So I don't know if it's possible but if it is possible to again put on the O: drive as before the data that were used for calculation, would make our task much easier to review it.

MR. LaBONE: Yes, what we did, I think, starting with this coworker model is what we're doing is we have the original data set and then any changes that are made to the data set are done with a script using our programming language.

And so what's on the O: drive will be the original data, the script which makes any changes to the data, the script that actually does the OPOS, the script that does the things to come up with the 50th and 84th percentiles.

And then also the intake calculations, which could not be done with IMBA this time, also had to be done using a script because IMBA will not calculate given, for example, lead-212 chest burden, it won't give you a thorium intake. It does not have the ability to do that.

And so, anyway, that's one little package that's in a zip file and you can download that and if you're not familiar with ours you can probably follow it or get somebody to help you.

But anyway, it should be completely reproducible and you won't have to be juggling Excel spreadsheets and trying to figure out how things were done.

So it should be much clearer than it has been in the past and we can get you, I'm

sure, that zip file so you can go through it yourself and see what you think about what we did.

DR. LIPSZTEIN: Okay. Actually the most important thing is to have the data that you have used but I'm using other softwares besides IMBA so I don't have this problem with going back to trying to search it too but I don't have the data that you used, so.

MR. LaBONE: What software are you using to do that?

MR. BARTON: Just to clarify --

DR. LIPSZTEIN: 8, the one from Vastalle.

MR. LaBONE: Oh, Louis, okay. Okay.

MR. BARTON: Joyce, if I might, I think what you're saying is we do have the original Excel file compilation of all the in vivo results. I think what you're interested in seeing is which signals were removed.

DR. LIPSZTEIN: On the coworker model they say they didn't use some of the data

so it's better to have exactly what data they used instead of --

MR. BARTON: There's an outline of generalities of which data was removed in the coworker model but without seeing exactly which data points were used --

DR. LIPSZTEIN: Yes, exactly. So if we have the exact data that were used would be, you know, much easier for us to review the work.

MR. LaBONE: Okay. I think this will give you what you're looking for so if there's something that's not there I'm sure we can get it for you but --

DR. LIPSZTEIN: Okay, thank you.

DR. MAURO: This is John Mauro. When Joyce, can everyone hear me okay?

MR. KATZ: Yes. Thanks, John.

DR. MAURO: I have more of a conceptual, simple question. I'm envisioning a worker who is exposed to both freshly separated thorium, so there wouldn't be any or very much progeny or any progeny potentially

from chest count.

And he's also simultaneously working with somewhat aged thorium where you would have the ingrowth of the, certainly the lead-212, paucity of lead-212, and perhaps a little bit of actinium.

So if he's exposed to, like, two different kinds, freshly separated and some aged, my question is, not to get into the technical of it, but you're saying that you do have algorithms that could tease that out and figure out what the thorium body burden would be when you --

MR. LaBONE: The answer is no. I don't know of any way of going that way. What we do is that with this triple-separated thorium has been proposed as being bounding no matter what the mixture is and so you don't have to know what the mixture was or the time since separation.

To do what you're saying, you'd have to really kind of know what, you know, how many separations and what the time frame of those

separations was and a lot of times we're not going to know that.

So it's a problem of having unknown mixtures we had to deal with and our proposed solution to that was this triple-separated thorium which we discussed in a number of different papers. As far as we can see, it will give a bounding estimate using that to calculate the thorium intake.

DR. MAURO: Okay. Yes, my main question is was that, you do have the wherewithal to come to grips with that circumstance. I didn't want to get into details of --

MR. LaBONE: Yes. The information we'll use but if you have information, you can refine it and make it more accurate so, again, most times it's tough to figure out what it was they were exposed to.

DR. MAURO: Okay, no. Thank you. That's all. I just wanted to know that that has been, is a subject that you looked at and you feel that your current protocol has a way to

deal with that in a reasonable way.

MR. LaBONE: Yes, I believe so.

DR. MAURO: Okay, thank you.

MR. STIVER: I guess at this point, we're ready to move on to the issues matrix unless anyone has any more questions about the thorium paper.

CHAIRMAN CLAWSON: In just listening to him, I want to make sure that I'm understanding because John's comment was going to that. By going to this triple separation, they're actually saying that the unknown is taken out of it? Is that kind of like the worst-case scenario?

MR. HINNEFELD: Yes.

CHAIRMAN CLAWSON: Okay. I just wanted to make sure that I was understanding how that is going because I was looking at the graphs here and stuff like that and that's kind of what I've got the feeling of so I just wanted to make sure of that, so okay.

MEMBER ZIEMER: Brad, just a question. This is Ziemer.

CHAIRMAN CLAWSON: Paul, go ahead.

MEMBER ZIEMER: Yes, this is just a procedural question. It's really directed to SC&A. Is there a plan then to have some official, an official review of this?

You've had these preliminary questions and it sounds like you're in a fairly good place. Is there going to be a formal review of this that will spell out some additional issues or you're not closing this, are you?

MR. STIVER: No. This is John Stiver, Paul. We have been tasked to do a complete, thorough review.

MEMBER ZIEMER: Right, that's what I thought.

MR. STIVER: Yes.

MEMBER ZIEMER: So you're just raising the initial questions then?

MR. STIVER: Yes, we're just trying to focus in on certain issues today.

MEMBER ZIEMER: Yes, got you.

MR. STIVER: Going to help shape

our strategy.

MEMBER ZIEMER: Okay, thank you.

MR. STIVER: Okay, should we move on?

CHAIRMAN CLAWSON: Yes.

MR. STIVER: This is Stiver again. Those of you who have Live Meeting, I've pulled up the Fernald Site Profile Issues Matrix, Revision 2, which was just delivered over the weekend.

And you'll recall that the Site Profile Review was delivered back in November of 2006. Shortly after that, we began, we were tasked to do the SEC Evaluation Report Review and so a great deal of the Site Profile findings were tabled pending resolution of the SEC.

And so a lot of these findings that we have that are being carried on the books are about eight years old. Some of them are still pertinent. Others are no longer really relevant because of developments in the program over the past eight years that have kind of rendered them moot.

In addition to that, a lot of the questions that we have in the, the carryover questions, both related to the former five SEC categories as well as these Site Profile, the 33 Site Profile findings, are related to internal exposures and some related to thorium in the post-SEC period and others related to recycled uranium and some other aspects.

And, you know, obviously until the thorium post-SEC methodology is reviewed and any findings resolved, NIOSH won't be able to put out the TBD revision for internal dose.

So a lot of these are kind of being held in abeyance until such time as we'll be able to take a look at the final TBD revision and take it on from there.

So there's probably about 20 issues that we can look at today. Last time, back in April, we closed out six that were related to the, remember the DWE approach to thorium intake modeling which was the basis for the largest of the thorium-based SECs.

But I'm going to go ahead, if

everybody can see this, I'm going to work my way down and we can just kind of go through them.

The ones that are in abeyance I'll just briefly mention or that are closed. And here we are. I won't bother with the closed findings.

This is Finding 1. This is all related to the thorium DWE. Finding 2, Finding 3.

And Finding 4, this is related to thorium in the post-SEC period and you can see back in April of 2010 we had mentioned that NIOSH's response kind of opened the door for a new time period and new methodologies which we have not reviewed.

And you can see our latest response in red bold font is that we would recommend keeping this finding open pending our formal review of the NIOSH White Paper and so that will be a theme we will see often today.

TBD Issue Number 5, this is another related to thorium fires again. This post-SEC period comes into concern for us and we

recommend keeping that open until we have a chance to take a look at the, we do our review and see how it's incorporated into the new TBD.

Let's see. Let's see. Six was closed. Seven, this is another one related to internal doses from raffinate streams, from ore processing in Plant 2/3. This, I believe, became SEC Issue Number 4. Some of these findings were kind of wrapped together. Again, we recommend keeping this in abeyance. We basically are in agreement with NIOSH's proposed methodology.

As you can see down here in Column 3, the bottom of the page, detailed discussion of SEC Issue 4 took place at the April 2011 Work Group meeting where SC&A agreed that NIOSH's methods were bounding and sufficiently accurate.

This then needs to be incorporated into TBD 5 and we recommend keeping this in abeyance until such time as that happens.

Moving on down here. Finding 8 is also related to raffinates. Refers back to

Finding 7. Once again, the recommendation is stay in abeyance.

Okay, Number 9, this is related to trace contaminants and recycled uranium and the NIOSH response was that Report 52 incorporates the latest thinking on recycled uranium and that is going to be incorporated into TBD 5.

However, we noted that Report 52, April 2011, does not reflect agreed-upon constituent levels from Work Group discussions on February 9th, 2012.

And you can see our citation of a White Paper entitled, SC&A's Response to NIOSH's Subgroup 10A Impact Analysis. And our concern is that while we reached agreement on the approach for plutonium, technetium and neptunium, that methodology has not been incorporated into the TBD so, again, we recommend keeping this in abeyance.

Moving down, Issue Number 10. Now, this is something that has never really come up in the Fernald discussions and the finding states that, the radionuclide list for Ru in the

TBD is incomplete. And that's really the part that we're concerned with here and this relates to americium-241 and thorium isotopes.

Now, these have never been discussed in the Fernald Work Group setting. However, for the sake of completeness, we feel that they should be and that those approaches for dealing with the other nuclides that were not addressed of the others, aside from the three main ones, should somehow be incorporated into the methodology for assessing dose from recycled uranium.

And so we recommend keeping this open. You know, obviously we'll need to take a look at the revised TBD but at some point between now and then this would have to be addressed by NIOSH.

MR. HINNEFELD: This is Stu. Just a question here. Do you have, like, I'm not as familiar with this as maybe I should be, do you have, like, source documents that identify occurrences of thorium isotopes in americium and recycled uranium?

MR. STIVER: Yes. I know when we first looked at this, oh gosh, way back, 2010, 2009/2010 time frame, we were looking at some documents that showed levels of various isotopes and I think it was, yes, at the bag house or some of the dust collectors.

And there were -- I'm trying to remember everything. Oh, there was definitely some thorium in there, cesium-137, or some other isotopes. I don't recall seeing actinium per se, but there are source documents that I would have to go back and dig up in order find that.

MR. HINNEFELD: Are they, like, referenced in a report that you find here?

MR. STIVER: This is something that we would have to probably handle on a technical call since we're -- let me pull up the Site Profile Review and I can go bring up that finding and look at the exact wording. Let's see here.

MR. HINNEFELD: I'm just asking because I know during the hunt for, you know,

recycled uranium and contaminants in that, it was always plutonium, neptunium and technetium. I mean that was always, those were always the ones that were considered potentially significant after --

MR. STIVER: Yes, remember part of the problem with thorium was that there was so much residual thorium from processing. It was not related to recycled uranium. It was really hard to try to separate the two out.

MR. HINNEFELD: It would not be addressed by the thorium approaches that we're dealing with?

MR. STIVER: It would be. That's my sense because I don't think it's possible to tease out, I'm speaking off the cuff here, tease out the component before recycled uranium compared to what was residual in the facilities from contamination and processing.

MR. HINNEFELD: Right.

MR. STIVER: There's just no way to tell because we don't have constituent levels identified in the recycled materials that were

sent in.

MR. HINNEFELD: Yes, I mean, there's no, there's no obvious mechanism for recycled uranium to have thorium in it I guess. Might be able to find one.

MR. STIVER: Well, there's thorium-230. You know, that kind of cleared up.

MR. HINNEFELD: Well, these are --

MR. STIVER: And actinium, we have the same problem with that that you saw with neptunium during the, you know, breakout and metal reduction process. There might be some accumulation into that, into the mag-fluoride.

MR. HINNEFELD: Okay, I'm still losing the mechanism for actinium -- actinium is below thorium-231 in decay chain. I mean, and you really --

MR. BARTON: Let me pull up --

MR. HINNEFELD: Once you start making uranium products, there's really no thorium there anymore.

MR. BARTON: Yes. Let me see if I

can get to the finding here, pull it up. I'm having a hard time with this. Let me see if I can find these.

MR. STIVER: I think Bob's going to go ahead and look for that then we can just kind of move on.

MR. HINNEFELD: I mean, you may have, you may have reports where you reference your sources. I'm just not --

MR. STIVER: I apologize. This is eight years ago and it's been off my radar scope for a long time.

MR. HINNEFELD: Yes. My recollection was that, you know, you looked for, you know, plutonium, neptunium and technetium because for the first year or so they had to worry a little bit about ruthenium because ruthenium would come over too but --

(Simultaneous speaking)

MR. STIVER: -- decay away pretty quickly.

MR. HINNEFELD: So anything that's been through the separation at Hanford a year

or more ago, it's those three things that were looked at.

Now, it's not, I just don't recall ever having to worry about americium or the other things. I mean, if it's thorium that's left over from thorium processing, I would think the thorium model is acceptable, it would deal with that.

MR. BARTON: Okay, let me try. Let me share this.

MR. STIVER: Page 54.

MR. BARTON: Yes. Okay, here we are, Page 53, and here's the finding in its entirety. Radionuclide list for Ru TBD is incomplete. Other radionuclides such as americium or thorium isotopes are mentioned but no data are provided. May be of considerable significance. The raffinates tend to accumulate in plutonium and other trace contaminants including thorium-230.

So this is something was just never run to ground I think and carried on. I would say rather than try to resolve it right now in

real time, we could go take a look back at some of the source documentation from this and then maybe have a technical call, so.

MR. HINNEFELD: Okay, this is Finding 10?

MR. BARTON: This is Finding Number 10.

MR. HINNEFELD: Your Site Profile Review?

MR. BARTON: Yes.

CHAIRMAN CLAWSON: So who's actually got the ball on this? Is it --

MR. STIVER: This will be, so we need to go back and do some --

CHAIRMAN CLAWSON: SC&A, okay.

MR. STIVER: -- some archeological digging.

CHAIRMAN CLAWSON: I know that we discussed it but we kind of --

MR. STIVER: Well, we never really focused in on some of these other nuclides.

MR. HINNEFELD: So this is a specific raffinate stream from the recycled

process? Is that what we're talking about?

MR. STIVER: You know --

MR. HINNEFELD: Because it sounds like if you're talking about thorium-230 it being concentrated relative to uranium isotopes, that occurred in a raffinate.

MR. STIVER: Let's see. Let's continue.

MR. HINNEFELD: -- taking uranium out of your stock and sending it this way, what's left over is the other stuff. And so, and I was thinking in the discussion of this, that we had kind of addressed it. You know, we've adopted a relatively high ratio of plutonium and neptunium compared to production products that were observed with the expectation that the overall exposure would be bounded by the ratio --

MR. STIVER: See, a lot of these former issues are kind of rolled into one here, one being the concentration of the mag-fluoride which we've already addressed.

But the other was this idea of

neptunium. Excuse me, americium-241, thorium isotopes. I'll have to go back and review our, the kind of development of the logic that went into making that finding.

MR. HINNEFELD: Okay, and I recognize that, you know, in a raffinate stream you've taken the uranium out --

MR. STIVER: Oh, yes, the raffinate stream is going to be a different situation.

MR. HINNEFELD: But my understanding was and it's been a long time since we talked about this, I thought that the numbers that were adopted for the contaminants, recycled contaminants were considerably higher than what's typically seen in the production uranium strains.

MR. STIVER: Yes, they were. Remember, it was the --

MR. HINNEFELD: And so there was sort of this expectation that that number will bound the --

MR. STIVER: Yes, for plutonium it was from that Group 10A --

MR. HINNEFELD: Yes.

MR. STIVER: -- and the really highly contaminated stuff from the gaseous diffusion plants. The question was who handled this material and when and we went through several White Paper exchanges on that before we finally came to a conclusion.

MR. HINNEFELD: Yes, and the raffinate exposure opportunities are pretty limited compared to the uranium product exposure opportunities so those factors we thought and the selection of that high ratio I thought would take care of this. Now, maybe I read too much into that.

MR. STIVER: Yes. You know, let us go back and do some research on this and then, you know, if it becomes evident there are some issues still I will set up a technical call to deal with it.

MR. HINNEFELD: All right. Okay.

MR. STIVER: So we'll take that as an action.

CHAIRMAN CLAWSON: What did they

call that that come from Paducah?

MR. STIVER: POOS.

CHAIRMAN CLAWSON: POOS, that's what it was.

MR. STIVER: Plutonium out of specification.

MR. HINNEFELD: Well, that was everything that was above. You know, POOS and the feed plant ash was what they most commonly referred to --

(Simultaneous speaking)

MR. STIVER: Yes, feed plant ash was the worst.

MR. STIVER: Okay. Let's see. Get back here, get back to the other content. I'm trying to do this with a touch mouse pad which is really not my favorite way to do things.

Okay, here we go, Finding 11. Okay, this is the suggested approach for Ru dosage. Estimation, the TBD is claimant-favorable for many workers but not for all. And this is something that we have run to

ground. The new methodology I think addresses that.

The response to Finding 9 is that we keep it in abeyance until such time as we can review the TBD so I don't think we have any problems with that.

Uranium enrichment, this is something that was decided at the last meeting. Closure was recommended and it was, indeed, closed.

The next one is kind of an interesting one. This is Finding 13 and this gets way back to a time before you guys had really developed many coworker models and this was about female employees.

Actually 13 and 21 are related. They're two aspects of the same finding. 21 relates to the external extremity, you know, shallow dose, external dose, whereas Finding 13 is related more to internal intakes.

And this whole idea was that you had female laundry workers and they're handling highly contaminated clothing and so there's

some potential for intake and so our concern was, how are you going to go about assessing that?

Now, the response from NIOSH is related to the external component which is Finding 21. Wait a second. I just lost it. Let me go back up a notch.

But our position is this finding predated the internal dose coworker models that anyone used for unmonitored workers and we recommend closure on this one. It's kind of an artifact of a previous time. Anybody else have any comments on that particular one? No.

CHAIRMAN CLAWSON: This one actually came up because we had the female [identifying information redacted].

MR. STIVER: Yes, we talked about it at the last meeting at the teleconference and the idea was we'd take a look at the TBD and see whether or not these changes had, in fact, been implemented. And our position is, yes, they have been. We've taken a look.

Now, the TBD. Now, this, a little

bit of a wrinkle here is that the discussion that NIOSH put out here is related to the external dose component but the internal dose component is also covered by the existence of coworker models so this is no longer an issue.

CHAIRMAN CLAWSON: And this gets back to the thing of this wouldn't be considered a clerical worker or anything else like that because the other part of this with the female [identifying information redacted], they were not considered, they -- she had a result that came back high and she was not one of them that was really being monitored. It was in her yearly --

MR. STIVER: Yes, this would be for unmonitored female workers. That's what I think was the genesis of the whole problem.

CHAIRMAN CLAWSON: Okay, that's what I want to make sure.

MR. STIVER: Okay. So we can go ahead and let me write that in here.

MR. KATZ: Do you want to hear from your other Board Members before you close this?

MR. STIVER: Somebody else out there who wants to speak up?

CHAIRMAN CLAWSON: Phil or Paul or Mark.

MEMBER ZIEMER: This is Ziemer. I don't have any comments other than I think this takes care of everything. The current status I think takes care of the issue so in abeyance seems to me to be appropriate.

MR. KATZ: This one's --

CHAIRMAN CLAWSON: This one we're actually looking at closing because we feel that it's been covered.

MEMBER ZIEMER: I thought it was in abeyance simply because you're waiting to see the final --

MR. STIVER: Yes, if I could kind of step in. This is Stiver. It was recommended at the April 15th teleconference to be put in abeyance so that's what the current status is over on the far right-hand column.

MEMBER ZIEMER: Right, right.

MR. STIVER: And you see under the

red font under A29, this is we recommend going ahead and closing it out based on the research we've done since then.

MEMBER ZIEMER: No, I think it's appropriate to close it if we can close it now.

CHAIRMAN CLAWSON: Okay.

MEMBER SCHOFIELD: This is Phil. I agree with that, let's go ahead and close it.

MR. STIVER: Okay, I'll go ahead and indicate that it's closed.

CHAIRMAN CLAWSON: Mark, I heard you in the background.

MEMBER GRIFFON: Yes, I just said I agree with that, to close it is fine.

CHAIRMAN CLAWSON: Okay, thank you.

MR. STIVER: Okay, let me worry about details later. Let's see, 14, this is closed at the last meeting. Fifteen, this relates to ingestion doses as outlined in the TIB-9 methodology being incorporated into the internal dose TBD, which as you'll see in a lot of these findings we're recommending abeyance

until such time as we review it. I don't think there's any bone of contention there.

Sixteen, these are some findings related to a shallow dose and our response did not fit nicely into a little box on the matrix so we added an attachment that had a more detailed description on some of these.

And once you see it you'll know who wrote it, let me get down here. I had asked John Mauro to take a look at this. I believe he's on the phone right now.

DR. MAURO: Okay, I'm off mute. Yes, I --

MR. STIVER: And this related to TIB-17 and the external dose, the extremity dose methodology and the implementation of TIB-17 and so forth, if you would give us like maybe your 30-second sound bite on this.

DR. MAURO: Yes. Could you scroll -- I reviewed nine of them so I have to get my bearings a little bit. Some of them I recommend closing and some of them I recommend keeping open.

If you look to the bottom of this write-up on the paginate, I just wanted to get a quick read again. I know that all of these OTIB-17 issues have been thoroughly reviewed and I cite in this write-up the history briefly of where this was addressed and also discussed during meetings.

I actually gave the page number of one of our meetings, relatively recent, and I believe that all issues related to this matter of these direct deposition have been resolved and the documentation for that resolution exists on Pages 42 to 52 of the February 13, 2014, minutes of the Procedures Subcommittee.

And so on that basis and after reviewing that write-up, and because this is an overarching issue, it applies not only here but many places and I believe it now resolved, it has been resolved across the Board and the basis for that resolution, our original is, well, we have original paper on it that's cited here in this appendix and then the discussion and the agreements made are cited here in the minutes,

not the minutes, the transcript of that meeting, so we recommend closing this issue here.

MR. STIVER: Yes, John, I would like to add that, you know, these issues have been formally closed out in the Procedures Subcommittee meeting.

DR. MAURO: Yes.

MR. STIVER: And so this also applies to Issue 18, which is just virtually identical to 16. So unless there's any objections, I will go ahead and close this out.

CHAIRMAN CLAWSON: Any of the Board Members on the phone have any questions or --

MEMBER SCHOFIELD: Not at this time.

CHAIRMAN CLAWSON: Okay.

MEMBER ZIEMER: No, I agree, close 16 and 18. Those two are the ones you're talking about, John Mauro, right?

DR. MAURO: Yes, that's correct, and they would be closed generically.

MEMBER ZIEMER: Yes.

DR. MAURO: And I think that's, it's an important matter because I think we find this in many locations, including Fernald, and having the resolution of this is achieved.

CHAIRMAN CLAWSON: Not hearing any more discussion, I will close it.

MR. STIVER: Okay, I'm just typing it in our response here.

CHAIRMAN CLAWSON: Okay.

MR. STIVER: Let's go back to 17. If there's enough room here for it. And, let's see here, 17, now, John, this is another one you looked at, this is about extremity dose and --

DR. MAURO: Right. I --

MR. STIVER: -- this is one where we're kind of working this through the INL Site Profile Review and some of the work that you guys are doing on extremity dose.

DR. MAURO: Right.

MR. STIVER: So let me pull up the response, the detailed response here. Yes.

DR. MAURO: Yes, this is, the issue of, I'll give it a 30-second sound bite so you

understand where we, why we're keeping it open, or recommending keeping it open.

There are procedures for dealing with extremity doses that have been reviewed and approved where you, well the person could either be wearing a finger or a wrist dosimeter or you could establish a relationship between the dose, let's say, that was withheld and the dose to the skin and if they're not wearing, to the skin on the hands.

Now what came up on INL and it might have applicability here, I think it's worthy of a little bit of discussion, is that we're finding that at least on INL there are 62, so far, counts of individuals with skin cancer on the extremities, namely the hands and the forearms.

So at one time we felt that this was not a major issue or an important issue because you just don't get cancer of the hand, so, of the extremities, but we're seeing skin cancer.

So our concern is that how are you going to calculate, estimate the dose, the beta

dose to skin on the extremities for workers that you suspect, now if you understand the situation, we're working in a situation where the film badge open window, you know, the standard method of estimating the dose to skin under OTIB-17 really can't be applied to the hands if the person is working under a set of circumstances where the dose to the hands could be, especially the beta dose, could be uniquely different than what's being let out on the film badge.

Now there's one with regard to uranium very often what's done is you go with this 240 mR per hour direct contact, total dose, to the skin.

So if you're in a circumstance where you feel you had a worker that might have had direct contact with uranium, that's one way to place an upper bound on the exposure rate to the skin when in contact with uranium.

But this whole subject as applied to workers who have cancer of the hand, the skin on the hand, seems to me that it's still an issue

that we want to keep open to hear how that's going to be dealt with.

We're looking at it right now on INL and seeing exactly what was done for those, you know, we had this collection of 62 cases, we're looking at all of them to see how the dose to the skin of the hand was derived for those workers because they did have that cancer and see what those protocols seem to do, a scientifically sound, claimant-favorable.

My recommendation is let's wait until we see what happens there before we close this and we're not far away from that. We have a draft report that I'll have in my hands probably by the end of today where all that's put together and I think what we find there and how we come out on that will have a bearing here.

So I'd like to hold off a little bit until we have a chance to look at, finish up on INL work.

MR. STIVER: Okay, thanks, John. I might add that Finding 19 is very closely related to 17 and it has, let me go down a bit

farther.

MEMBER GRIFFON: John, this is Mark Griffon, maybe I didn't hear you, the 62 cases are they from Fernald or --

DR. MAURO: No. They're INL.

MEMBER GRIFFON: Oh, they're INL, okay, that's what I couldn't hear.

DR. MAURO: Well it turns out that, of course, inevitably this, you know, we just happen to be working that problem and we're almost done with it and it would have applicability.

MEMBER GRIFFON: Okay, thanks. Thanks.

MEMBER SCHOFIELD: This is Phil. I got a quick question, John. Are you assuming leaded or unleaded gloves for the workers or what?

DR. MAURO: If we have knowledge, see here's the situation, if the person has a skin cancer on the hands, but we have affirmative evidence that he could not have gotten any exposure because he was wearing

adequately protective gloves from the beta exposure.

You know, then, you know, that's a good question. Do we have that information and if we do have that information, do we just say well because of the wearing of the gloves that do provide, you know, will stop the betas of interest.

Well, does that mean that we're taking credit, credit will be taken for that and that's a reasonable thing to do except that as you know very often when it comes to respiratory protection, no credit is given to that.

So I don't know if we had that conversation yet with NIOSH, and please remind if we have, but when a person does have cancer of the skin of the hands and he had a job where he was, you know, his hands were in close proximity to a beta source, but you do have reason to believe that his skin was protected because of the gloves or of the glove box and the handling was such that it would not allow that exposure to occur.

Am I correct that you would assume zero exposure under those circumstances?

MR. BARTON: John, this is --

DR. MAURO: By the way this is one things we're looking for and looking at when we look at these cases at INL.

MR. BARTON: Yes, John, this is Bob Barton and as you know I was looking at a lot of those dose reconstructions for INL and it was pretty much standard practice. If the cancer was on the forearm or the hands, I don't believe I came across any cases where any sort of protection factor was used.

Now if it was upper arm, shoulder, something like that, you know, there would be an attenuation factor of somewhere in the 80 percent range.

But I think it's, I mean we haven't specifically looked at Fernald dose reconstructions for extremities. I know we put together a list of how many we found, but I probably assume and, Mark, maybe you can weigh in on this, I mean usually there's no assumption

of wearing lead gloves or anything of that nature.

MR. ROLFES: Yes, definitely not at Fernald. I mean leather gloves possibly, but there were always issues, too, with contamination of the leather gloves early on.

In the 1950's they had concerns about reusing them just because of the materials getting ingrained in the gloves and delivering dose to people's hands, but, yes, comparing apples and oranges, talking about lead gloves and glove boxes at places like INL versus a place like Fernald where it's completely different.

MR. STIVER: Yes, I think one thing we need to keep in mind is this is, you're looking at an overarching issue when it was identified, you know, eight years ago for Fernald, but certainly being addressed in other venues as well.

And that's the reason why, you know, INL is under way right now, this is going to be a good vehicle by which we can kind of look at

then really try to address the whole idea of the beta dose changes and geometry factors and so forth that might impinge on these extremity doses.

And that's kind of what we're looking at in finding 19, which is kind of the other side to 17, which John had just discussed, and basically the same response is given by NIOSH.

The TIB-13 and geometric exposure is another consideration for external dose reconstructions at uranium facilities is being used to correct the geometry, first, kind of a, I don't know I'd say an off-normal or situations like in this particular finding we're talking about thorium handling where the, you know, the beta and gamma dose contributions are a bit different than it would be for uranium handling.

And we once again recommended that this issue be kept open pending our investigations at INL because that's really where we're kind of getting a handle on how the

beta dose components are going to be adjusted for a dosimeter reading to account for geometric correction.

And so once again we recommend keeping this open until such time as we have an opportunity to finalize the INL studies.

CHAIRMAN CLAWSON: This is Brad. I don't have any problem with that, but I want to have it clarified. Now this is between the badge and badge reading and what?

MR. STIVER: This would be the source to the badge reading. If you have a badge you're wearing on your chest for example and you're working in a glove box or you're handling uranium or thorium materials and you're getting beta dose to your extremities, your fingers, your hands, forearms, what not.

CHAIRMAN CLAWSON: Okay.

MR. STIVER: How then would you adjust that film badge reading that was worn on the chest to account for what the exposure was actually, you're actually experiencing on the extremity and that's really what this is all

about.

CHAIRMAN CLAWSON: Okay.

MR. STIVER: Now TIB-13 addresses correction factors for photon exposure, for gamma exposures, but not for beta exposures for electrons, and so the INL work is kind of, you know, we said INL is kind of the vehicle by which we're kind of examining this overarching issue.

CHAIRMAN CLAWSON: Okay. Now I know. I just wanted to have a better idea of what we were looking at on this because -- okay, any of the other Board Members have any problems with keeping these open or any questions? Not hearing any, we'll continue on.

MR. STIVER: Okay.

CHAIRMAN CLAWSON: You know, one thing that comes to me though is when we're working in a contaminated area like this they have us bag our TLDs so that we can keep it on the outer part of that and that's the --

MR. STIVER: You know, I think that was one of the things that was discussed in TIB-13, in the Procedures Subcommittee

discussions on TIB-13 was, you know, how would you account for bagging and so forth and --

CHAIRMAN CLAWSON: Okay.

MR. STIVER: -- other attenuating materials between the source and the film badge.

CHAIRMAN CLAWSON: Okay.

MR. STIVER: And that would obviously be a lot more important for beta exposure, but, yes, that is.

CHAIRMAN CLAWSON: Right.

MR. STIVER: Is Hans Behling still on the phone?

DR. BEHLING: Yes, I am.

MR. STIVER: Hans, would you like to talk about 20, Issue 20?

DR. BEHLING: Okay. Let's see, this particular finding was correction factors used in the initial period of use of TLD at Fernald and I find it to be appropriate.

And if I recall what was initially criticized about that was the timing of the use of the Panasonic 802 for TLD badge. Initially

it was identified as being introduced in 1985.

The second issue was the correction factor associated with the change in the algorithm that initially involved an algorithm developed on fatal nitrous back in '82 and would subsequently found to be inadequate in addressing issues related to the beta component that was essentially an algorithm established later on.

Those two, looking at the revised version of TBD involving Chapter 6, have been corrected, so those two issues that were initially identified are now essentially resolved.

There was, however, a third one which was not necessarily addressed and that was the issue of correction factors associated with contamination found on the badges and that involved a time frame that involved somewhere around 1985 when Westinghouse identified this problem and corrected film badge you got on your TLD badges that were subject to degradation of contamination and what they in essence did was

to use a series of badges, contaminated them with known quantities of activity and then assess the response due to the contamination alone.

Now to establish correction factors you need, not only to understand what the contribution of doses based on the activity of contamination level, but also the time frame.

So what they in essence did was to do the following, they had received some calibration codes that said so much activity on a badge will introduce an incremental dose rate that will be assigned to the badge that would be fraudulently assumed as occupational exposure.

However, another component is the time duration during which the contamination sits on a badge and here is where I get some questions raised and the model that NIOSH used to affect that correction factor was to do the following.

If you have let's say contamination of 1000 dpm for a badge of material that might

contribute to the dose you have to also understand how long was that contamination there.

And what was done was to, in essence, identify the date of the, issue of the badge and the date of the readout and then if, let's assume it was exactly one month and you issued a badge to the person on the first of the month and at the end of the month, approximately 30, 31 days, you treat the badge and you read it out.

So you have obviously an unknown and that is when was the badge contaminated and the assumption here is the amount of, what's the use of it, where it point, and assume that that contamination that you observe at the time that the badge was turned in for readout was approximately halfway, so 15 days.

When you do that apparently it was found that some badges using that model would actually end up with a negative value, meaning that the person had not zero exposure, but less than zero exposure, which would suggest that

the assumption was that perhaps the badge was contaminated on day one and therefore the actual duration of the contaminate adding dose to the TLD was in fact 30 or 31 days as opposed to 15 days and that would account for the negative values.

I looked at this particular issue and realized that well this is basically the problem that we face on many other issues, whether it's the LOD over 2 for admit dose on the dosimeter or TLD or in the case of a bioassay when we take MDA over 2.

And I realized that this is an issue that cannot really be resolved. You have to accept the fact that when you do this particular type of presumption that there will be instances where you will obviously subtract more than what's necessary, in other cases you give more.

And so this limitation of the system and it's why I'm concerned it's part of the way we do business here at NIOSH and I don't see any resolution to it.

So as far as I'm concerned the Finding Number 20 should be resolved and as far as I'm concerned there's no need to continue this discussion.

MR. STIVER: Brad?

DR. BEHLING: Any questions?

(Simultaneous speaking)

DR. BEHLING: How many?

CHAIRMAN CLAWSON: I have a lot of questions, Hans, but I understand the gist of where you're going. I don't see anything else but to --

MR. STIVER: I think it needs to be closed out.

CHAIRMAN CLAWSON: -- close it, so other Board Members any problems with closing this one?

MEMBER ZIEMER: No, I agree. I think it's the only thing you can do. It's a reasonable approach.

CHAIRMAN CLAWSON: Correct.

DR. BEHLING: And I would assume that no one's ever going to be assigned a

negative number anyway, so what in essence, you would be shortchanged.

What, in instance where you have negative numbers that would suggest that the person wasn't exposed to anything and the subtraction ends up giving you the negative number and, in essence, you would be essentially shortchanged 50 percent of that value that has been subtracted much like when you get, when you have a film badge and the LOD is 40 millirem the truth is the person could've had 39 and in other words he could've been shortchanged 14 millirem.

On the other hand it could've been that the person really didn't have any in which case he had the benefit of getting the assignment of 20 millirem. That's just the way the system works and I think we just have to accept that.

CHAIRMAN CLAWSON: Okay. Thank you, Hans. Any other Board Members have anything? If not we'll go ahead and close that one.

MR. STIVER: Okay. Twenty-one was the other side of the story regarding the female employees' exposures to external sources and we went ahead and took a look, this is one that we put in abeyance back on April 15th based on our review of the external dose TBD.

And we did take a look at that and confirm that the NIOSH statements that missed dose is no longer used to assign unmonitored external dose and that the 500 millirem upper bound dose methodology has been removed.

We also note that Section 6.6.2 of the TBD -- those refer to OTIB-17 and so for the same reasons that we discussed earlier regarding Issues 16 and 18 and also Number 13, we recommend that this issue be closed.

CHAIRMAN CLAWSON: Any of the Board Members have an objection to that or any questions?

MEMBER ZIEMER: No objection, I agree. Ziemer.

CHAIRMAN CLAWSON: Okay. Not hearing any more, we'll go ahead and close that.

MR. STIVER: Okay. Now the next three I believe are related to atmospheric fugitive emissions and intermittent-type exposures in limited areas and I had asked John Mauro to look into this because he has done a lot of work in this regard.

Again, a lot of it related to INL. So, John, I've got Finding 22 up here.

DR. MAURO: Yes.

MR. STIVER: This is a source term for atmospheric uranium emissions is significantly underestimated.

DR. MAURO: Yes, let's scroll down, I'm reading it just to refresh my memory again on this Finding 22 in the Appendix.

MR. STIVER: Okay.

DR. MAURO: I see you have it on the screen.

MR. STIVER: Let me make it a little bigger for you here.

DR. MAURO: And that goes on, I just want to get my bearings again. Okay, let's just take a look. Okay.

MR. STIVER: It just kind of lays out the background.

DR. MAURO: Yes, the history of this thing.

MR. STIVER: Okay. Right here is the gist of it right there.

DR. MAURO: Yes, gist, right. Bear with me a minute.

MR. STIVER: Okay.

DR. MAURO: Okay. Ah, yes, the Clark issue, I got it, okay, thank you.

MR. STIVER: Okay.

DR. MAURO: I just needed to remember the essence of the issue. Originally the source terms were estimated. There was a number of studies on emissions and this goes way back to findings back in maybe 2005 where the source term that was provided, it was defended in the write-up and there was a, but a question came up, did you look at the work done by this fellow Clark, et al, 1989, the citation, and at that time that was not reviewed.

And the possibility that his work

could've shown that the emissions, airborne emissions of uranium may have been higher than what was used.

I checked Clark and the work there and also the current write-up for environmental exposures and what source terms were used and it turns out that the source term that is currently used for uranium emissions from stacks, some big number, 300, right there it is 308,000, is substantially larger than the estimate that Clark made.

So we concluded that the issue as originally raised there is no issue here because the source term that NIOSH is using from its source documents where it references, is higher than what Clark estimated, so we're recommending this issue to be closed.

MR. STIVER: Now, John, you have a nice, interesting postscript of this finding, too, I mean kind of a whole other, a notion of using the uranium bioassay coworker models instead of atmospheric dispersion model and to assign doses of this sort.

DR. MAURO: Yes.

MR. STIVER: But anyway, it's kind of interesting and I know that this kind of impinges on the radon doses that we'll be discussing, too, is that, well, radon's kind of a different animal, obviously, but, you know, this isn't -- let's separate the radon issue from this uranium issue. All I'm saying here is that, you know, right now we have a circumstance where most workers, the vast majority of workers at Fernald have bioassay data and if they worked for a National Lab as a prime contractor, you know, we have the data, we have the wherewithal to reconstruct the doses since most of them have bioassay data, and for those few that don't, you could build a coworker model with the exception, of course, for subcontractors.

Now we have a circumstance here that says well, what about outdoors where you have, perhaps you have workers outdoors where you want to reconstruct their doses if they don't have bioassay data and what I'm saying here is

that when you look at the current, I call it the 2014 version of the Site Profile, there's a very well developed description of, given the source term of this 310,000 kilograms total, I think they have it by year and perhaps by building.

In my opinion, given that source term and given that you have meteorological data, you should be in a position to reconstruct doses if you have to resort to the atmospheric transport model and its associated atmospheric dispersion factor as inhalation.

The tools are there to do that if it comes to that, but where you do have a worker that does not have bioassay data and if you need to go to this protocol, I believe the protocol, as laid out in the current version of the Site Profile, the 2014 version, you know, it's scientifically sound and claimant-favorable.

So this was not an issue that was specifically raised here, you know, in this whatever number we're on right now, but I thought I would just point that out because I can see someone asking that question and from

looking at the, just from reading the Site Profile and looking at this section on environmental, I felt the section was strong.

The source term was good, that was the original concern, but not only is the source term good but the protocols for how they would go about dealing with reconstructing those outdoor exposures can be done.

CHAIRMAN CLAWSON: Thank you, John.

DR. MAURO: For uranium. This question of radon is going to be a different one that we'll talk about later.

CHAIRMAN CLAWSON: I understand. Thank you, John. Board members on the phone, any questions?

MEMBER ZIEMER: Not at this time.

CHAIRMAN CLAWSON: Okay.

MEMBER SCHOFIELD: No questions, sounds good.

CHAIRMAN CLAWSON: Okay.

MR. STIVER: Close?

CHAIRMAN CLAWSON: It's closed

then. Looking at the time on this I think we ought to break for lunch.

MR. STIVER: It's probably a good break point.

CHAIRMAN CLAWSON: And we'll continue this up at --

MR. KATZ: An hour?

CHAIRMAN CLAWSON: In an hour, one o'clock.

MR. KATZ: One o'clock.

MR. STIVER: No objections.

CHAIRMAN CLAWSON: No objections?

MR. KATZ: So on break till one. Thank you everybody and we'll reconnect the phone then.

CHAIRMAN CLAWSON: Thank you.

(Whereupon, the above-entitled matter went off the record at 11:59 a.m. and resumed at 1:04 p.m.)

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

(1:05 p.m.)

MR. KATZ: We're just waiting for Stu to join us, but everyone else is in the room. Let me just check on line while we're waiting. Do I have Paul and Mark, and Phil?

MEMBER ZIEMER: Yes, Ted, this is Paul. And before we move in to -- I have to [identifying information redacted] at 1:30 p.m. So, I'll have to bail out early.

MR. KATZ: Okay, 1:30 p.m. And, Phil, are you on too? Did I hear you? I think I heard Mark, right?

MEMBER GRIFFON: Yes, Ted, I'm here.

MR. KATZ: Great. Phil, are you on? Okay. Well, we're waiting for Stu anyway.

(Off microphone comments)

MR. KATZ: Okay, we have Stu back. I think we're ready to go. Let me just check again. Phil, are you on the line? Okay, not Phil yet. But we've got, Stu, we've got Paul

on the line, as well as Mark.

MR. HINNEFELD: Okay.

CHAIRMAN CLAWSON: Where are we at? We're at 23?

MR. STIVER: Yes. We finished up 22, and we're just starting Item 23. Let me know when you guys are ready.

CHAIRMAN CLAWSON: Okay. I'm ready.

MR. STIVER: Okay. This is another one related to environmental monitoring. Actually, excuse me, environmental dose calculations using Gaussian atmospheric dispersion modeling

And the finding, you know, back in 2006 was that the TBD has not adequately considered various aspects of internal environmental dose, including applicability of the Gaussian model, episodic releases and particle size.

NIOSH's response was that the environmental TBD revisions do indeed use a standard annualized Gaussian model, including

assumptions regarding atmospheric, I'm just going to read the entirety of it here, stability, and that it is claimant favorable. In addition, short term episodic releases are modeled using the puff modeling, so the continuous release model.

And they also have factored into account for a respirable fraction of particles. And we had recommended closure, based on our review done a couple of weeks ago. And the reason being, we did take a look at the environmental TBD.

Basically Table 4.6 of the 2014 cycle file provides examples of six significant episodic releases. These occurred over a period of about, of less than a day.

And these were the ones that, some that were of concern to us in our original review. Equation 4.7, the atmospheric diffusion equation, is going to be used to model dispersion factors for these releases.

It was taken from Slade 1968, recognized as one of the seminal documents on

atomic energy and meteorology.

So, the bottom line is that the model does specifically take into consideration the things we're concerned about, wind speed, direction, stability, class, the time of the release and use conservative parameter values.

And so, in summary, we believe that the TBD revision is fully responsive to our original concerns. And we recommend closing this finding out.

CHAIRMAN CLAWSON: Board Members have any comments or questions on this before we proceed on?

MEMBER ZIEMER: I concur. This is Ziemer.

CHAIRMAN CLAWSON: Okay.

MEMBER SCHOFIELD: Yes, I'm okay with that.

CHAIRMAN CLAWSON: Okay.

MR. HINNEFELD: Okay.

CHAIRMAN CLAWSON: Phil, are you with us yet? Well, I don't see a problem with

that. So, we'll go ahead --

MR. STIVER: Okay.

CHAIRMAN CLAWSON: -- and close that out.

MR. STIVER: Twenty-four. This finding states diffuse emissions of uranium and thorium may have produced significant internal exposure for some personnel.

This topic had not been previously discussed in any of the 17 Work Group meetings that have transpired since then. We recommend keeping this finding open as a topic of discussion.

NIOSH responds that, basically the same response they had to Finding 22. They basically say, the stack effluence for the operational period identified releases of thorium, uranium emerged from building exhaust waste pits, UF6 releases from storage containers, and six specifically identified off normal events. But they're not aware of any other significant additional sources.

And I had John Mauro look at this.

And he went through Section, a perfect Section 5.7.3 of the original review. And our concern was that at the time the Site Profile addressed the diffuse emissions from the waste pits, but not potentially important sources of deferred diffuse emissions, many of which were described in our review at the time.

Some of those are listed here on our response, on Page 39. There's four in particular. One was the outside Williams Mill, Breaking Salt at outside mill, shoveling onto conveyor belt, the conveyor at the outside mill, and the changing drums at the outside mill.

And each of these list general air breathing zones, air concentrations in terms of maximum allowable concentrations. And so, we felt that the TBD might benefit from taking a look at some of these kind of off normal events that we identified.

We realize that they're separate from episodic releases, because they're highly localized ground level releases that can't be

easily modeled. So, we proposed doing an upper bound estimate, localized airborne concentrations. Example being the bulleted items that we have.

And of course, this is only going to apply to workers who are not on a routine bioassay program for the radionuclides of interest, and that are not covered by the SEC. Let me take a look, read down here a little bit further. Okay.

Now, as far as being able to place people at particular locations regarding, in relation to these short term releases, and so forth, we realize that they might have the granularity in the days that they actually do this. However, we thought that we should leave this open and give NIOSH a chance to respond, and maybe, you know, take a look into it themselves, and come back with a proposal of their own.

MR. ROLFES: For uranium exposures, I mean, any exposure to an individual that's performing such a task, their

uranium urinalysis results will obviously reflect any routine exposures or intermittent exposures that they might have had in these higher air concentration areas. We're not going to be reconstructing thorium intakes because of the SEC.

MR. STIVER: Right.

MR. ROLFES: I don't know how much more we can do on something such as this.

MR. STIVER: Yes. You know, our response to Finding 22 is kind of along those lines too, you know, that we feel that the coworker model is the way to go in doing this. So, because we have the coworker model at that time, and haven't even begun, we haven't even discovered the data.

CHAIRMAN CLAWSON: Will this one basically be tied into the coworker data? I mean --

MR. STIVER: Yes, basically.

CHAIRMAN CLAWSON: -- coworker model?

MR. STIVER: It's kind of, you

know, off normal occurrences that, you know, at the time would have had to have been modeled. But this is at the time before you had the, had developed the uranium coworker model.

And, you know, we put that kind of addendum to Finding 22, that we recommend that, you know, in a situation like this that, you know, the preferred course would be to go ahead and just use the coworker model, because of the uncertainties considered in trying to model this kind of exposure.

CHAIRMAN CLAWSON: Well, this would actually be tied in with the --

MR. STIVER: Yes. These are all kind of combined, 23 through 24 are similar. So the bottom line is I think we can probably go ahead and close this one out.

CHAIRMAN CLAWSON: Pending the coworker model evaluation? Is that --

MR. STIVER: Oh, the coworker model. Well, we're talking about uranium coworker model, which is already under discussion.

CHAIRMAN CLAWSON: Okay.

MR. STIVER: Except for the, obviously the subcontractors.

MR. HINNEFELD: So then, your recommendation is that this can be closed because of the coworker model? Because that --

MR. STIVER: No --

MR. HINNEFELD: -- was my thought. I mean --

MR. STIVER: Yes. I mean, the coworker model's really going to be the whole standard for this --

MR. HINNEFELD: These were episodic --

MR. STIVER: -- kind of thing.

MR. HINNEFELD: -- exposures of people working with radiological materials. And so, they're either going to be monitored, or they'd be covered by the coworker.

MR. STIVER: Yes, yes.

MR. HINNEFELD: The coworker approach.

MR. STIVER: Yes. I think, you

know, at the time, you know, step back eight years and, you know, we are faced with having to model, is kind of --

MR. HINNEFELD: Okay. All right.

CHAIRMAN CLAWSON: So, your recommendation is to close, correct?

MR. STIVER: Yes, close.

CHAIRMAN CLAWSON: Other Board Members, any questions on that?

MEMBER ZIEMER: No. This is Ziemer. I just wanted to clarify, John Mauro, are you okay on that?

MR. STIVER: Yes, I had talked to John earlier. He's at another meeting right now.

MEMBER ZIEMER: Oh, okay.

MR. STIVER: So he can't jump in. But he's all right with that.

MEMBER ZIEMER: Yes, good. Yes. It makes sense to me, yes. I'm good.

CHAIRMAN CLAWSON: Mark?

MEMBER GRIFFON: Yes, the same, I'm good. I'm good on it.

CHAIRMAN CLAWSON: Okay. Phil, are you there, or on mute? So, okay. Well we'll close that one then.

MR. STIVER: Okay. The next is related to radon modeling from the K-65 silos. Actually, 25 and 26 kind of subsume into SEC Issue 5, which had been the topic of a lot of discussion several years ago.

And Hans Behling is prepared to talk about this. He did the lion's share of our work. Produced, I believe, three White Papers that were exchanged with NIOSH, back in 2011. So, Hans, are you on board here?

DR. BEHLING: Yes, I am.

MR. STIVER: Okay.

DR. BEHLING: Let me just briefly recount what the issue in Finding 25 was originally. It was, again, based on the questions of radon modeling. But it really addressed the, kind of the two values, and fully accepted the actual release rates of 5,000 to 6,000 curies from the K-65 silos.

So, at this point I think it was John

Stiver had mentioned we want to really focus on another issue that relates to this issue. And, as a matter of record, I just want to say, I was not involved in the original findings that involved the TBD review.

But back in 2007 I was asked to review the SEC Petition and NIOSH's Evaluation Report. And I looked at a number of issues. Among those was the radon releases from the K-65 silos, which mostly involved data that was reported by the Radiation Assessment Corporation. Refer to RAC Report of 1995.

And I came up with a very, very different conclusion. And I just wanted to just briefly review some of the things that I reported in behalf of my review of the SEC Petition.

And what I looked at was, in essence, the data that was presented by the RAC Report. And I just wanted to make a comment here, that it's not my opinion, but by and large reflects what was reported in the RAC Report.

And I wanted to state very privately

that the estimates that were presented by the RAC 1955 Report were based purely on the model for which the most basic model parameters, that is, the diffusion coefficient and the radon emanation fraction were really unknown. And they were really based on various assumptions that really could not be confirmed.

And the principled assumption was that the radon that was released from the weight package that represented the disequilibrium between radon 226 and lead 210 was really radon that escaped from the waste package into the head space, decayed in the head space with very little being released.

And the serious deficiency of the RAC model is that the head space radon would, in fact, be mostly retained within the silo, and ignored the whole issue of many of the penetrations that were subject to radon leakage, as well as the Venturi Effect, which I then discussed in a couple of the White Papers.

As a result of that initial review

of the data that I had available to myself, I came to the conclusion that the radon releases from the K-65 silos in combination would release approximately 100,000 curies per year, which is almost a factor of 20-fold difference from the 5,000 to 6,000 curies that were projected to be released, based on the RAC model.

And let me just briefly go over what that -- or what my model really entails. I looked at the release of radon based on the disequilibrium back in 1991. This is now almost 40 years after the waste package was introduced into silos.

And for Silo Number 1 the radon 226 activity was based on 525 picocuries per gram. And the lead 210 activity in Silo 1 was 194,000 picocuries. And that, by and large, translates to a ratio between lead 210 and radon 226 of 0.37.

That would suggest that in the absence of a full equilibrium you had something in the order of 63 percent of the radon 222

leaving the waste package. And very similar values were identified for Silo 2, 417,000 picocuries for radon 226 and 160 picocuries per gram for lead 210. And again, the disequilibrium there was suggestive of approximately 62 percent of the radon leaving the waste package.

Now, the question is, where did it go? And this is where I essentially came to the understanding that it is probably all vented to the atmosphere. Also in that calculation I concluded, on the basis of disequilibrium, that approximately 88,000 curies per year were vented from Silo 1, and about 23,400 curies per year were vented by Silo 2.

And as a result of my modeling of releases, there was significant question about whether or not I was right or wrong. And I was asked to write a White Paper that was issued back in 2008. And that White Paper pretty much explained a lot of the issues that I was not willing to put into the original report.

Also, because there was a lot of

additional information I was able to assemble. And to put my claim of these kinds of annual releases from the K-65 silos. When that White Paper was reviewed by NIOSH they did not really discredit anything, in terms of technical.

But, they by and large, in October of 2008, concluded that the numbers were not subject to technical criticism. But dismissed my White Paper on the basis that that report had not been subject to a National Academy of Science review. And therefore, they would stick with the RAC model that, in fact, was reviewed.

I believe during that very meeting, I think it was Dr. Ziemer who said, well then, let's find out what the National Academy of Science has to say about the 1995 RAC model, and if in fact that concurs with NIOSH's assumption that that should be the model we should go with.

Well, that was the genesis of my second White Paper, which regurgitated pretty much what I said in the first. But also added a significant amount of information that, among

other things, discredited the notion that the National Academy of Science in fact endorsed the 1995 RAC model.

And, not to belabor that issue, I also added a lot of additional information. I point to the RAC model and the excessively type information that was cited in the RAC model. And, granted, you can read the two White Papers, because there's an awful lot of information.

But let me get down to the real issues that, at this point, supports my contention. And on that issue I would hope that, John, you can introduce the Table J-19 from Appendix J of the RAC 1995 Report. I included that as Exhibit 5 in my 2010 White Paper.

MR. STIVER: The 2010 White Paper?

DR. BEHLING: Yes.

MR. STIVER: Okay. Let me pull that up.

DR. BEHLING: Table J-19 from Appendix -- It's Exhibit Number 5 on Page 19 of the development section.

MR. STIVER: Let me find it here. Okay. Okay, Hans, I'm on Page 19. Can you see that?

DR. BEHLING: Well, actually that's not the one I have. I'm not sure whether that is the --

MR. STIVER: Okay. Hang on to this. It may not be the one.

DR. BEHLING: That may be, that's the 2008 White Paper.

MR. STIVER: Let me check. This is called "The Second White Paper".

DR. BEHLING: Okay. But --

MR. STIVER: Yes, this --

DR. BEHLING: Let me see. Hold on. It's the table that, let me see, maybe I used the wrong -- I introduced that table twice, both in the White Paper, as well as, the first White Paper as well as the second paper.

MR. STIVER: Okay. Table J-19, Exhibit 5. Let's go down here.

DR. BEHLING: In the first White Paper, John, it's on Page 10. And if you have

the 2008 White Paper, it's on Page 10.

MR. STIVER: I'm trying to find it here. According to your Table of Contents, it's on Page 19. Okay. I've got to go down. I'm not -- I was looking on Page 19 of the PDF. Oh, here we are. Here we are. Okay, Hans, we're on it.

DR. BEHLING: Yes, okay. This is really the crux of everything. And it's relatively easy to understand. And it comes, of all things, from the RAC 1995 Report. So, it is something that I can't understand why the people from the RAC group, who compiled these data, didn't realize that they were essentially in conflict with their own data.

So, let me explain what's in that report. First of all, if you look at the top it has two sets of data that, one, involves, prior to the sealing of the silo openings that pre-date 1980, okay.

So, this is the first sets of data. And what these data represent on dose rates, standing on top of the silos, Silos 1 and Silo

2. And the reason in 1980 they felt compelled to seal the dome, there was a huge six inch gooseneck opening, and huge numbers of fissures that was just barely able to release anything that was in the head space.

And as a result of the high dose rates that made it unacceptable for workers to be up there for doing anything, they decide to seal the domes in 1980. And so, we have dose rates that pre-date 1980 and post-date 1980.

So, let's go look at the data. The very first entries was in April 1964. It was Silo Number 1. And the contact reading on that, at that time, was 75 millirem, okay. In March of '72, eight years later, again there was a contact reading of 75 millirem.

However, the footnote under which silo it is, it says it's NF. It doesn't indicate which one. But it could have been 1 or 2. But their, at that time the dose rate reading on top of the dome was 30, and so forth. Again, it's a very, very low dose rate reading.

On the fourth entry, identified by

May 1973, again Silo 1. So you have a nine year time interval. The contact reading on top of Silo 1 was 65 to 90 millirem. The entry below that now is in May of '73. And this involves Silo Number 2. And the dose rate at that time was between 70 and 75 millirem, okay.

So we have, by and large, data that involved the dose rates on top of Silos 1 and 2, pre 1980, that suggest dose rates somewhere between 60 or 75 millirem per hour, with the dose rate on top of either one of those silos.

And then you have in late 1979 a strong effort to seal all the cracks, remove that six inch gooseneck, and everything else that might have potentially allowed the radon to escape.

And what would happen, as you would expect, if the radon in fact that had emanated from the waste package into the head space, you would obviously see a rapid and significant increase in the dose rates, based on the presence of the radon that was now captured in the head space, the radon daughters.

And so, when you go now to the second half, actually not quite the second half, where it says, after sealing silo openings, you have April 1980, Silo 1, the contact reading went to 250 millirem per hour. In other words, more than a three-fold increase from the previous readings that date back to April '64 or May of 1973, okay.

Below that you have again, in April 1990 Silo 2, a contact reading that at that time after the resealing and then the repair was done, raises the dose rate on top of the domes to 200 to 250 millirem.

So we have, in essence, approximately a three-fold increase between the time frame prior to 1980, post 1980, in terms of the dose rate on top of the dome. And that had to be, obviously, due to the fact that the radon that was previously released into the environment had now entered the head space. And basically, at least for the most part, was retained in the head space, and gave rise to the increase in dose rate.

So now, let's go down towards the bottom. I'm trying to quickly look and see when this issue came to pass. But, there was a time when again, the radon had to be released, based on the issue of concerns. And let me just briefly, quickly find out where that is.

Okay. When you look at the bottom four entries that occurred in 1987, at the very top, on top of the four, last one, you have November 1987. You have, again, on Silo, on top of Silo 1 a contact reading that was 160 to 208 millirem. And again, that's considered a baseline.

Right below that, the same date, top of Silo 1, there was another contact reading that resulted in a dose rate of 35.5 to 68 millirem. However, that occurred after the operation of the radon treatment system, as you see on the right hand side, for an average value of about 65 millirem, okay.

This is very important, okay. The next two entries involve, again, November 1987. But this one now is on top of Silo Number 2. And

you have, in the second to the last entry, a measured dose rate of 221 to 250 millirem, with an average of 232 millirem per hour.

Below that is the same location, but it is now after the radon treatment system was operated, that reduced the dose rate from 60 to 76, for an average of 68. And what I want to point out here now is that, before the radon treatment system goes into being, it by and large mimics the dose rates that were measured early in 1980 after the sealing of the dome.

On the other hand, if you look at the radon treatment system readings that reduced it by more than three-fold, you have almost the identical dose rates that pre-date 1980, without the radon treatment.

And let me just tell you what the radon treatment, how it was used. The radon treatment system was operated on one silo at a time, with a flow rate of about 1,000 cubic feet per minute. And was operated until radiation levels on top of the silo dome surface stopped decreasing. And that basically said, we

eliminated all of the radon and their short-lived daughters.

And when you see this data, there is no, there's an inescapable understanding that the pre 1980 dose rates on top of the dome reflect a situation where the radon is not collected in the head space, but was directly vented out.

And based on my calculation that turns out to be about, between Silo 1 and 2, about 100,000 curies per year, and not 5,000 to 6,000 as modeled by the RAC Committee, using various questionable parameters.

And on that basis I stand my ground in saying that the release rates that have been modeled into the environmental radon releases for the silos 65 1 and 2, are probably a factor of 20-fold off.

And like I said, if you want to understand something, you have to understand one thing. When there is a disequilibrium that somehow or other cannot account for somewhere around 67 percent of the radon that would have

been held in the waste package, along with its short-lived daughters, and you say, where can it go? Where can it go?

And the answer is, it can go in one of two things. It can diffuse to the periphery, and even be released from the site of the silo. Or it can migrate, as it most likely would, to the head space.

But if it's in the head space and it stays there, it's held there, you would see, in essence, a dose rate pre-1980 that would have been the same post-1980. But it wasn't. And the difference being, it can only be accounted for by release of the radon from the waste package into the environment.

CHAIRMAN CLAWSON: Hans, this is Brad. Let me just make sure I understand what you're telling me into this. For what you're saying is, before they sealed up the K-65 silos, and the radon was able to escape, that these figures are off, correct?

DR. BEHLING: That is correct.

CHAIRMAN CLAWSON: This is the RAC

Report. And I understand this. And I just want to make sure that that was where we were at on it. Because, once they sealed that up, then the corrective action was to turn a fan onto it, and pull all this out of the head space, correct?

DR. BEHLING: Well, I don't know. When they pulled it out of the head space, they might have actually filtered it through a charcoal filter, meaning that they might have reduced the radon releases into the environment by capturing it in a charcoal filter, along with the short-lived daughters.

So, I'm not saying that post 1980 the release of the radon by way of the radon treatment system would have been vented into the environment.

CHAIRMAN CLAWSON: Right, yes. That's true. I understand what you're saying on that one.

DR. BEHLING: My contention, Brad, is that prior to 1980 the RAC model, which identifies 5,000 to 6,000 curies per year is not

correct.

CHAIRMAN CLAWSON: I understand. Mark or Paul, do you have any questions on this?

MEMBER GRIFFON: Well, I'm actually curious if NIOSH has a response to Hans' assertion.

CHAIRMAN CLAWSON: Okay.

MR. ROLFES: We discussed -- This is Mark Rolfes, Mark. And we discussed this quite a bit back, you know, six years ago. And I know Hans and I yelled at each other. My opinion is that we shouldn't be using external dose rate measurements to characterize the quantities of radon gas being released from the head space.

We both issued White Papers, you know, supporting our own opinions. And we had developed a best estimate approach that indicated that the 5,000 to 6,000 curies being released per year by the RAC study was actually much less than that, by a factor of approximately ten.

Hans' White Paper, based upon the

external dose rate measurements conducted on the outside of the silos, you know, he believes that it's a factor of 20 higher than the RAC study. So, we've got, you know, one paper in the middle, a better estimate that's much below, and another estimate that's much above.

MR. HINNEFELD: Well, let's find out some things here. You know, we had some institutional interest in, you know, the RAC Report was also adopted essentially by the Pinney Report, right?

MR. ROLFES: Correct.

MR. HINNEFELD: I mean, they're essentially the same number. And so --

DR. BEHLING: Yes, that's correct.

MR. HINNEFELD: And Penny was a NIOSH sponsored report. So there was some sort of institutional interest, you know, in the Penny report. Now, having said that, you know, we, for the purpose of this program, the purpose to make sure we're bounding, we have departed from the way NIOSH has done things in other fashions, in other arenas, because of the

nature of this being a compensation program as opposed to those other programs.

So, I think there's some food for thought here. Hans, I wanted to ask you just real briefly, if you can explain briefly, how did you arrive at your estimate of the radon emission of 80,000 and 20,000 curies per year?

I assume this is for the period from the time the silo was built up until the openings were closed, or sealed up in like '79. Or that would be one release rate.

DR. BEHLING: Okay.

MR. HINNEFELD: That's the release rate. Okay.

DR. BEHLING: I assumed that the disequilibrium that was measured in 1991 was probably disequilibrium that had existed pretty much throughout the time frame when the 13,000 drums were being emptied into Silo 1 and 2. And there's no reason not to.

It's reasonably conservative, if not just reasonably intuitive to conclude that this disequilibrium existed, okay. And the

fact, when I hear you say, oh, dose rate, well, the dose rate is not due to radon. But if the radon, short-lived radon daughters can escape, I must certainly have to conclude that radon as a gas will equally escape.

So, I will not buy on the issue that the dose rate measurements in itself serve as an indicator for something that involves radon, and it may not be correct.

My conclusion is this, if anything, the short-lived radon daughters would, if they escape they certainly will allow the radon as a gas to escape, okay. Because, as short-lived daughters they may even attach themselves to a dust particle, and stick to the inside wall where they're stuck to decay.

And that apparently does not seem to be the case when you look at those dose rate measurements that I just showed you. So, if the dose rates are not an indicator of the fate of radon, then I don't know what is. Because I do stick by my guns in saying, if the dose rate's reduced, that means the radon also

escaped.

MR. HINNEFELD: Okay. So then, back to my original question about the 100,000, or the 80,000 and 20,000 release rate, that's based on the total amount of radium in the silos and --

DR. BEHLING: Yes. And I adjusted it even for -- Because when the radon treatment system went into effect, they must have taken measurements and said that there is still a three percent retention of short-lived daughters. So, I even adjusted for that.

And I did this, again, on my White Paper on Page 14. This is my November 2008 White Paper, on Page 14 and 15. I go through the actual calculations that gave rise to my understanding that the number of curies that's released from Silo 1 and 2 were about 82,000 and 23,000 respectively, curies per year from each of those. And total is somewhere around 100 and some odd, 110,000 curies.

I will go somewhere above 100,000 curies per year, is my best estimate as to what

escaped from the head space into the environment during those years prior to 1980.

MR. HINNEFELD: And that's basically, mainly on the disequilibrium between the radium and lead 212 in the 1991 samples?

DR. BEHLING: Yes. And how do you account for that?

MR. HINNEFELD: Yes, yes.

DR. BEHLING: If you start, and understand that this waste was introduced in the early '50s, and these disequilibrium values were measured in 1991 and again in 1992. Now, there was nearly 40 years of time lapse between the time that the raffinate wastes were introduced in Silos 1 and 2. And of course, lead 210 has a 22 year half-life.

So one could say, okay, there was some ingrowth made. But the truth is, I don't believe there's any reason to believe that this disequilibrium did not also exist beforehand. Because if the radon can escape from the silos, it most likely can easily escape from the

packaged drums.

So, it's possible that this disequilibrium basically existed beforehand, and continued unabated throughout that 40 year time frame, that period while it was packaged in the silos. And those are my, you can go through my White Paper.

And I had to make a couple of basic assumptions. But they were very reasonable. And so, it is, it might be based on the disequilibrium and the need to identify the fate of the radon that is accountable by the disequilibrium, and where does it go?

If it's in the head space, as was always, or I was told it all decayed in the head space. But the data I've just shown to you in Exhibit 5 seems to contradict that assumption, that it does not decay in the head space, but it was rather vented out. Because the dose rates before and after 1982 rose sharply by a factor of three.

But when the radon treatment system was kicked in, it basically reduced the dose

rate levels prior to 1980, meaning that the radon has to have been escaping during that time frame.

MR. HINNEFELD: Okay. Well, I'm going to think about --

DR. BEHLING: This is a clear cut case.

MR. HINNEFELD: And we have been thinking about thorium and coworker bioassay for the last, you know, year on this. And I haven't really picked this back up. But I would like to go re-read Hans' and our proposals, if that's okay with the Work Group?

CHAIRMAN CLAWSON: Yes, that's, well, that's been one we've been dealing with for a long time.

MR. HINNEFELD: And realistically, this is a matter of, what's the number? This isn't, can you do it? This is what's the number.

So, this is a site profile issue. But let's wrap the thing up while we're talking about it.

MR. STIVER: Right.

MR. HINNEFELD: And so, there's some, you know, there's some compelling arguments, you know. Hans made some compelling arguments. There are complications that probably perturbed us more than we would like.

But I would like to actually take some time to just read through the whole of both our argument, our estimate, Hans' estimate, and see what we can say about is there something we can come up with here that seems acceptable in both realms?

Six years ago I probably was a proponent of, you know, NIOSH has endorsed this number in the Penny Report, we should stay with that, you know. I think I probably was a proponent of that six years ago. I don't know that we need to do that anymore.

I mean, we've, like I've said, for a compensation program you do certain things that maybe you, you know, certainly the health effects reconstructors didn't do. So, I think

we can take a look at this. And I would like the opportunity to form a judgment. And then I'll have additional discussions.

If it comes down to it, we can have a technical call, and just kind of go through this. But I'd like to reserve this one for myself, since I can talk about the site.

CHAIRMAN CLAWSON: Okay. Any thoughts to --

MR. ROLFES: Since you were on site at this time period, do you ever recall having 2,000 or 3,000 curies of radon being released from the silo on any given day?

MR. HINNEFELD: Well, we didn't measure radon directly. I mean, there were radon monitoring stations that were passive. So, you would collect the monitoring device and get integrated exposure for the exposure for their deployment period.

And there were some radon, I mean, periodically, you know, very infrequently there would be radon fluence metrics with charcoal canisters, where you would then take

fluence rate through, you know, that much area of the dome. And again, that's passive. You'd leave it in place, you'd collect it, and you'd estimate the fluence rate for this deployment period.

I remember that it was significantly different if there was a crack under your canister, as opposed to a solid piece of concrete, it was way different, which doesn't, which is not surprising.

But, you know, the numbers, I don't know that at the time that we were estimating emission rates from these concentration values that were measured. Or, there may have been an emission rate estimated from the fluence rates.

But I don't know exactly how they'd do that. Because, how much area does your crack take up, you know? Or, I guess you could do a length of crack thing. I don't remember if that was done at all. So, I don't know. Three thousand curies a day seems like a lot. But I'd have to go back and do some reading.

MR. ROLFES: I think we discussed

some of these issues. I know on the internal dose, Report 52, Internal Dosimetry Issues. I think for Fernald, I mean, this has been something that we've discussed quite a bit. So, there are some measurements, like you said. And some employees from Mound, someone by the name of Jenkins, I believe, had come down and done some charcoal studies.

MR. HINNEFELD: Yes. They did some charcoal studies. And I think Mound deployed the first passive monitors on PERMs, passive monitor, radon monitor.

MR. STIVER: Yes, on PERMs.

MR. HINNEFELD: And so, I don't remember actually a daily emission rate being calculated.

MR. ARNO: I've got a question. Were these silos maintained at atmospheric pressure? Or were they negative --

MR. HINNEFELD: No they were --

MR. ARNO: -- on the outside?

MR. HINNEFELD: They were atmospheric.

MR. ARNO: Okay.

DR. BEHLING: And this is a comment. That is really the whole basis by which the RAC model operated. It in essence said that there was a diurnal variation in ambient temperature, which would then under, reduce temperature or elevated temperature, cause the head space in the silo to exhale a certain amount.

So, it was based on diffusion constants and certain pressure differentials, diurnal pressure differentials, based on the heating effect, solar heating effect of the silos. And those are very questionable models. And if you read my White Paper, I go into great detail in analyzing what they did, and some the deficiencies in their assumption.

MR. HINNEFELD: Well, the diurnal variation is, that in fact was true. The radon level was always higher at night than it was in the daytime.

DR. BEHLING: Yes. And, of course, I would expect that, to a large extent

that whatever was released might have been released slightly higher during those time periods. But I also make a major issue out of the Venturi effect.

When you look at the dome, and it's a round, semi hemispherical shape, when you have a passing wind over it you have what's called the Venturi effect, that by and large leaves the entire head space very quickly.

And they did not, they understood that. But in one of their discussion points in 1995 RAC model they said, we will not account for that. And I believe that's really a dominant means by which the head space was vented.

MR. HINNEFELD: Well, it's, I think the physics of it is really complicated. I think you're right, the Venturi, I'm not doubting Venturi effect would contribute. I'm confident there was this, I mean, there's just too much observation of various kinds of data to say that this diurnal pumping didn't occur. And I'm pretty confident that did occur.

But, I think that, well again, I'd just like to go back and carefully read the arguments, our arguments and your arguments, and see if I can't sort this out a little bit.

Because I, it was pretty clear when radon treatment systems that were ran, whether it was just makeshift one that they were running that's referred to here, or the permanent one that was running before the remediation of the silos. When you turn those on you did significantly decrease the direct exposure reading from those domes, from the silos. That's a fact.

So, since you do that, you have to conclude that some fraction of the radiation dose you're reading on the surface is due to head space radon decay problem.

DR. BEHLING: Well, I would question that. Because obviously the waste package in itself contained significant amounts. So that when you turn on the radon system you exhaust all the head space, that 60 to 70 millirem per hour, which is the same as

the dose rate readings before 1980, was all likely due to radioactivity in the waste package that still obviously penetrated the dome, and gave you those approximately 70 millirem per hour dose rate readings. And so --

MR. HINNEFELD: Yes. I thought that's what I said.

DR. BEHLING: -- we're not talking about residual. I just believe that when you ventilate just about everything out, using the radon treatment system. And you're left with approximately 70 millirem of residual baseline radioactive dose rates, I mean, dose rates. That dose rate reflects a contamination that was in the waste package, not in the head space.

MR. HINNEFELD: I thought that's what I said.

MR. STIVER: Yes, yes. Hans, I think that is what Stu said. I think it might have just been some --

MR. HINNEFELD: I may have not said it very well. The other issue about, you know,

two-thirds of the, or the lead 212 being only a third of the total of the radium total, and therefore, two-thirds of the radon that were generated had to leave, that assumes that two-thirds of the radon had to get out of the residues into the head space.

Because it has to be from the head space. I mean, there's no doubt that, you know, some fraction, I don't know what fraction. But you've got these residues that were what, 18 feet deep, roughly?

MR. STIVER: Ninety something percent water.

MR. HINNEFELD: Yes. And were wet. That it's a, you know, a pretty good fraction of the radium is not going to leave the residue just physically. And we don't really know what the diffusion constant, you know, what Hans said. You don't really know what the diffusion constants are.

But just as a practical matter, you really expect 60, you know, two-thirds or 63 percent of the radon to even get out of the

residue into the head space, which then raises a question, why the hell did the analytical results turn out the way they did.

DR. BEHLING: Well, I can only say, with the disequilibrium between radon 226 and lead 210. And those are empirical measurements --

MR. HINNEFELD: Yes.

DR. BEHLING: -- done in 1991 and '92.

MR. HINNEFELD: Yes, I know.

DR. BEHLING: And they both show that disequilibrium. So, if the radon did not escape the waste package, how do you account for the 63 millirem of lead 210? I don't see how you can draw any other conclusion that that disequilibrium is so strong that the release and removal of radon 222.

MR. ROLFES: Hans, this is Mark. One other thing I think we discussed is, you know, my speculation that they could have done some attempts to recover lead 210 out of the materials for the production of polonium, you

know.

So that could have created a disequilibrium I guess, prior to the materials being loaded into the silos at Fernald, when the material came from Mallinckrodt. This would have fit in the time period that Monsanto was doing research with the production of polonium 210 as well, late '40s. But I was --

DR. BEHLING: Well, I will dismiss that issue too. Because of this, these materials were loaded into the silos in the '50s, okay, early '50s. This disequilibrium measurement was discovered in the early '90s. That's a 40 year difference. That's almost a two-fold time frame of the half-life of lead 210.

In other words, you would have had a reestablishment of equilibrium with no radon escaping of approximately 75 percent. And so, I'm not going to accept that as an explanation, that they may have removed all the lead 210. It wouldn't add up just on that basis. If they removed 100 percent of the lead 210, you would

have had an ingrowth of up 75 percent at the time that these measurements were taken, if no radon escaped.

MEMBER SCHOFIELD: This is Phil. Why would they have even bothered removing the lead if they were just going to store it in these silos?

MR. HINNEFELD: That would have been done, Phil, that would have been done at Mallinckrodt, because Mallinckrodt was experimenting with production of polonium for, I guess for initiators, right. So they were trying to extract things from the raffinate. And they may have extracted the lead 212 in there, as what they, in their work there. You know, trying --

MEMBER SCHOFIELD: Oh, okay.

MR. HINNEFELD: -- to reclaim some materials from the raffinate. That would have been, that happened at Mallinckrodt before these materials came to Fernald.

CHAIRMAN CLAWSON: Well, you know, I think we could discuss this a lot more. But

bottom line is, as you've asked, you want to be able to look at this in a little bit closer detail. Plus, we're looking at it a little bit different than what we were previously. We were looking at this as an SEC. This is a Site Profile issue, and when we get to it --

So, my suggestion, and Board Members, and if anybody has any disagreement, is that we give NIOSH an opportunity to review this. And I don't know if it will come in a White Paper form. I'm sure it probably will. But, reevaluate this, and then we'll get back with it. Is that okay with the Board Members?

MEMBER GRIFFON: Yes. That sounds great.

CHAIRMAN CLAWSON: Is that okay with --

MEMBER SCHOFIELD: It's a reasonable approach.

CHAIRMAN CLAWSON: Okay. Is that all right with you too, Hans?

DR. BEHLING: Well, I guess I'm waiting. I just hope one more time that

they'll look at my report, and assess it technically. And not dismiss it because it wasn't reviewed by the National Academy of Science.

MR. HINNEFELD: Well, crap. You want me to assess it technically? Oh, man.

MR. STIVER: Yes, here we go, yes.

CHAIRMAN CLAWSON: Okay. I appreciate that, Hans. So, we'll --

DR. BEHLING: I'm being cynical, I admit. I'm used to waiting.

MR. HINNEFELD: That's okay. I was being a smartass.

MR. STIVER: Stu, do you have any idea about the time frame we'll be looking at here?

MR. HINNEFELD: Well, right now I'm really interested in it. And so, I could do it, you know --

MR. STIVER: Strike while the iron's hot?

MR. HINNEFELD: -- without too much -- I'd have to read these things.

CHAIRMAN CLAWSON: Maybe you'd take this off on your vacation, and --

MR. HINNEFELD: I just took my vacation. And believe me, my wife would not let me take this on my vacation.

MR. STIVER: Yes.

MR. HINNEFELD: Well, beyond just reading Hans' papers and our papers, certainly we're going to have some internal discussions on this as well. So, I would think, I can shoot for a couple of months, maybe.

MR. KATZ: Which is when we're planning for our next meeting.

MR. STIVER: That's about the time for our next meeting anyway, yes, about two months.

MR. HINNEFELD: Okay. We're planning a meeting in a couple of months. Well, if we're going to plan for a meeting in a couple of months, then that gives me something to shoot for, to try and get something out in advance of a next meeting, in order to at least have something to talk about.

MR. KATZ: Yes.

MR. HINNEFELD: Give me something to shoot for.

MR. KATZ: Because SC&A will complete their coworker model review at the same time. So we're shooting for, I think November, early December, right?

MR. STIVER: Yes. That sounds about right.

MR. STIVER: Ballpark.

CHAIRMAN CLAWSON: Yes.

MR. STIVER: Brad should be back from his exploits by then.

CHAIRMAN CLAWSON: Yes. I'm spending my birthday here with you guys.

MR. STIVER: Some fish stories. Some great fish stories.

MR. KATZ: Good stories though.

MR. STIVER: New ones.

CHAIRMAN CLAWSON: Okay. There, you put that stipulation on me. Oh, I see. Okay. Well, we'll look forward to that.

MEMBER GRIFFON: Watch it, Brad,

you're getting to be an old man.

CHAIRMAN CLAWSON: Yes, I know it. I know it. Hit the big 5-7 today. Well --

MR. STIVER: You're remarkably well preserved for the big 5-7 I would say.

CHAIRMAN CLAWSON: Yes. So, we'll wait for that. And you want to proceed on, or --

MR. STIVER: Could we take a five minute break here?

CHAIRMAN CLAWSON: I was going to suggest that, but I thought I was the only wimp. We're going to take a ten minute comfort break if we could, if that's all right with everybody? Not hearing any objections.

MR. KATZ: So, it's 2:02 p.m. right now on my computer. Ten minutes.

(Whereupon, the above-entitled matter went off the record at 2:02 p.m. and resumed at 2:13 p.m.)

MR. KATZ: Okay. All right, we're back. We're even --

MR. STIVER: Everybody back and in

the --

MR. KATZ: -- a minute early. Uncharacteristic. Is, I think Paul we probably lost. He was going to leave about a half an hour ago. But we have you back on line, Phil?

MEMBER SCHOFIELD: Yes, you do.

MR. KATZ: Great. And Mark? Mark are you back on --

MEMBER GRIFFON: I'm here.

MR. KATZ: Oh, great. Super. Okay. Let's carry on.

MR. STIVER: Do we have John Mauro back?

DR. MAURO: Yes, I rejoined you.

MR. STIVER: Oh, okay. The next one was Issue Number 27. This is one that John looked into as well, and provided a detailed response, which I'm going to give to you right now.

DR. BEHLING: John, this is Hans. Are we skipping Item 26?

MR. STIVER: Actually, 25 and 26,

and SEC Issue 5 were all kind of rolled into the same can, if you will.

DR. BEHLING: I know that, you know, the issue of the K-65 silos do play a part here. But, okay. If you choose to do that, then --

MR. STIVER: Well, hang on just a second. I just lost my file here. Hold on. Yes, Hans, the pitchblende ore on site in Plant 1, I believe if memory serves, that NIOSH has rolled that into their model, you know, into another source for radon release.

And we talked about it at several of the Work Group meetings. But if you've prepared a response to that particular issue you'd like to share with us, that would be fine.

DR. BEHLING: Okay. It's very brief. At least the original Finding 26 really addressed the problems associated with the 211 ores, and the fact that they had not at that time taken full consideration of what the release rates were as defined by Pinney and Horning 2006 and other data.

And I believe that has subsequently been incorporated into the revision of the TBD. Now, again, the only issue here is that, related to what we just talked about regarding the K-65 silos, are part of that. But the essential concerns that were raised back in 2006 have been addressed.

MR. STIVER: Okay. So, in essence, you're recommending closing that one particular aspect of it?

DR. BEHLING: Yes. It's not that the K-65 silo issues that we just mentioned in behalf of Finding Number 25. It's not part of this. But at least the original concern was raised back in 2006, when the Richman SC&A reviewed the TBD. That has been addressed. Except that it did not address the issue of the K-65 silo release quantities.

MR. STIVER: Okay. Well, I guess --

DR. BEHLING: So, if we resolve the K-65 issue, then that component of Finding Number 26 will also be resolved.

MR. STIVER: Right. Okay. So, for that particular aspect of it then we can recommend closure.

DR. BEHLING: Yes.

MR. STIVER: No objections to that?

CHAIRMAN CLAWSON: No. Thank you, Hans.

MR. STIVER: Okay, John, you want to go ahead and go with Issue 27 here?

DR. MAURO: Twenty-seven, okay. Twenty-seven is --

MR. STIVER: This is outdoor diffuse emissions in production areas as a source of external environmental dose. Let me pull up the response here.

DR. MAURO: Yes.

MR. STIVER: Okay. This is related to external environmental dose, aside from that from the K-65 silo.

DR. MAURO: Right.

MR. STIVER: And then Issue 28 looks at the K-65 silos separate from the other sources.

DR. MAURO: Right. We're talking about external exposures, and yes, I have a write up here. And we'll go to the, the bottom line is you have these external exposure contour maps for 1976 to '85. And also a section, there's in the Site Profile. And also Section 4.5.4 presents onsite ambient dose rate estimates from '52 to '75.

MR. STIVER: Right.

DR. MAURO: They provide a protocol to use this information to estimate external exposure. So, in other words, they do it, the new Site Profile does explicitly address, approach the data, and the approach to reconstruct these outdoor exposures.

And a lot of information is there. I read through it all. And my last paragraph, and the attachment basically summarizes my findings regarding that data, and the whole approach, which taken in its entirety it appears the new profile provides the guidance to estimate external exposures outdoors from all sources stored on site. And residual

radioactivity at the site, okay.

And so, we recommend that this issue be closed, with one proviso, that a statement be made in the Site Profile itself regarding, this goes back to the skin exposures, constructed in accordance with OTIB-17, and what we discussed earlier about localized doses.

So, in other words, my takeaway from this is that the techniques, the data and the techniques as described in the Site Profile will allow you to reconstruct external exposures outdoors, certainly photon exposures.

But I didn't see any language that discussed that they will be adopting OTIB-17 protocols, as further elaborated on in the agreements made during the Work Group meeting on this matter of direct contamination. So, that was the only --

Also, I'm recommending we close this. But I think it might be a good idea to have some language in the, at some point in the

process, that says that they will, NIOSH will be using OTIB-17 as interpreted, and as further developed in the agreements made at that last Subcommittee meeting regarding direct deposition.

MR. STIVER: Anybody else have anything to add on that? Or can we go ahead and close that out?

CHAIRMAN CLAWSON: Any other Board Members on the phone have any questions? Not hearing any, we'll close that one.

MEMBER SCHOFIELD: I'm just assuming that NIOSH is agreeable to those conditions that John talked about. Is that correct?

MR. HINNEFELD: I believe so. I'd have to --

MR. ROLFES: I don't know what they are.

MR. HINNEFELD: I'd have to go back and refresh my memory. At this point I don't object to that. If we find that we don't, for some reason we don't think that's right, we'll

let everybody know. But, if you don't hear from us --

DR. MAURO: I can help out a little bit with this.

MR. HINNEFELD: Yes.

DR. MAURO: The idea being, certainly external photon exposures, given the protocol that you've laid out, and the data you have in the contours, and you have, you know, you were going to place a person in theory at some location for some time period.

You could certainly reconstruct external exposure, because of this residual activity that's either in soil, or that's in locations where there's a radiation field being created. And there's information from TLD measurements, for example, of what those fields are.

What's not there is the external exposure to skin, and how that is going to be dealt with, but which has been addressed in other venues, and agreed to. But not specifically here in this Site Profile, as best

I can tell.

So, what my understanding is that your plan will be, that when you encounter a person who has a skin cancer, and you're reconstructing his external doses, and he's outdoors, that, you know, you'll use, of course you'll use the method you describe.

But in addition, if it's a skin cancer you will be taking into consideration the beta dose to the skin that might be associated with direct deposition.

Under the, and there are, there's quite a bit of discussion on under what circumstances that's done, when there's affirmative evidence that yes, there might have been a problem, where there could have been direct deposition on the skin. That needs to be taken into consideration.

And then once you, you know, once that determination is made, and that's a judgment call based on where the guy's located, and the circumstances under which he's been operating. A judgment call is made whether the

direct deposition scenario applies.

And that's all laid out very nicely, and discussed in other documents and other meetings, how you make that judgment. And then once that judgment is made, the procedures for doing that dose reconstructions will localize deposition, are all agreed upon on how that would be done.

And I don't want to get into details about that, because it's been written up in a number of locations. And it has been discussed relatively recently at the Subcommittee meeting.

But certainly take a look at it, see if you're comfortable with all that. But that's the only proviso I make. Sort of like adding a little bit more richness to the section you have right now.

MR. HINNEFELD: Okay. I don't foresee any issue with it.

MR. STIVER: I don't think there's going to be a problem. Okay. I guess we can move on to Issue 28. And this is related to the

external environmental dose for workers near the K-65 silos.

Basically the one element that was taken out of Issue 27, presumably because it was more of a gamma dose issue, and not necessarily related to a dose from deposition from betas, from shallow dose considerations.

The original findings, the TBD is silent on how external doses to workers on the silos were derived. The persons that may have spent time in the area of Fernald containment silos. This is a particular concern for the early years, before additional shielding was provided for the silos. And also a concern for those unmonitored workers who may have taken breaks near the silos.

And NIOSH's response was that the external environmental TBD in Revision 2014 addresses the issue of external environmental dose to persons near the K-65 silos. And we went back and took a closer look. And, much as John described for Issue 27, we found that the

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Hang on just a second. Let me pull this back up. That the TBD provides a very thorough discussion and methodology for calculating the external doses, the environmental doses for personnel in this particular situation.

From 1976 to 2005 the ambient radiation associated with the silos and the production plants is based on TLD measurements that were taken at various locations, both on site and at the fenceline boundary.

And that prior to 1976 there's a modeling of average direct dose rates at the fenceline, based on a combination of historic data from the radiation levels, and the application of measured dose rate values.

And I guess in summary, what we can say is that the Site Profile is, the guidance that's provided there, is certainly adequate to reconstruct external exposures from the silos. And we recommend closing this issue out.

DR. BEHLING: John, this is Hans. I looked at it too. I guess I wasn't sure

whether you were going to respond, or I was going to respond. But I come away with slightly different feelings about that.

Because the Finding Number 28, as it was originally offered back in 2006, really talks about very close proximity to the silos. Not the fenceline, but very close. And if I can look at the actual statement in the current TBD regarding that, it does talk about measurable levels.

And I'll quote here. The measurable level as measured by Juno survey meters in 1963 was interpreted to be an exposure rate of 30 millirem at three feet, one meter from north and south silos, a total of 60 millirem per hour at three feet from the tanks.

Anyways, those numbers don't agree with the values shown in Figure 4-16 for the 1965 silos, where the maximum dose rate is identified somewhere around just slightly above one millirem per hour.

I think what was initially identified in this particular finding, back in

2006, was the fact that when you went very close to the silos themselves, and I guess maybe on the other side of the berm that was ultimately constructed, you would encounter dose rates between 30 and 60 millirem per hour.

Now, I think that's what that particular finding identifies. I was not the person who identified this finding. But I'm trying to respond to the finding the way I read it. And I note that they've made changes in the current TBD.

But the dose rates in Table 4-16 are not necessarily the ones that I think were identified by the original people who wrote the finding on the 26 back in 2006.

Because we're talking about dose rates between 30 and 60 millirem per hour, as he quotes there, for even, maybe especially female employees during the years who were not monitored. It involved those unmonitored workers who may have taken breaks near the silo.

The assumption is, if you had an unmonitored worker who decides to sit in the

shadows of the Silo 1 and 2, he might have been exposed to 30 to 60 millirem per hour, which is -- as I said, I don't know if there's any evidence to that effect. But this is really the crux of the question associated with Finding 26. And it cannot be answered with 4-16.

MR. STIVER: Okay. Would you recommend then that we should keep this one open?

DR. BEHLING: Well, I would be happy to listen to what NIOSH has to say on this, in the sense where hopefully there weren't enough people stupid enough to sit next to the silos, given the fact that they understood that those readings were fairly high.

But, you know, this is what the original finding really requests to give answers to. Whether or not that question is legitimate is another question.

MR. HINNEFELD: Is there any indication that anybody was ever out by the silos and not monitored? I mean, has anybody

ever said that?

MR. ROLFES: I've never seen any indication that people weren't monitored at the site, other than the female employees not being monitored, since they didn't have --

MR. HINNEFELD: Yes. They discriminated against female employees for a while. And they wouldn't let them go out back, anywhere near the radioactive, radiological material. I mean, that's true.

But that doesn't mean that they, you know they didn't badge them, they let them wander around wherever they wanted. I don't know of any time or any circumstance when somebody would be out by the K-65 silos without being monitored.

MR. STIVER: It seems kind of far-fetched that you'd have somebody taking a lunch break next to the silos, but --

MR. HINNEFELD: Who would be there?

MR. STIVER: Yes.

MR. HINNEFELD: I mean, they were, I mean, this was not an administrative area,

this was the waste storage area, you know, out, you know, well within the controlled fence. I don't know. What's the circumstance where you'd have somebody who wasn't monitored in the vicinity of the silos?

MR. STIVER: That's kind of the way I interpret it, as being -- You know, this finding pre-dates my involvement by about four years in the program. But, I think it might have been Arjun who came up with that one.

But, yes, I can't interpret that to mean that the TBD was that silent on, you know, modeling, and that particular source term an entire post, yet, you know, reasonable distances from the silo. It's not for people who would be right next to it. But if that was truly the intent, I just don't see it as being a very likely scenario, certainly.

MR. HINNEFELD: I mean, the --

MR. STIVER: To the extent that the external coworker models are being implemented now too. I mean, you would have to worry about an unmonitored female. And you would get some

sort of either environmental dose or, you know, an external --

MR. HINNEFELD: Yes. I don't, I really don't, you know, I don't understand what the thought process is for the unmonitored people outside, you know. That's what I'm missing.

This is, if you really talking about, you know, being right there by the silos with about 30 mR per hour, well, who's going to be there that's not monitored, you know? If you're worried about the dose rate in the silos out on Willey Road, you know --

MR. STIVER: Yes. Somebody that just --

MR. HINNEFELD: -- off the boundary of the property, that's addressed elsewhere.

MR. BARTON: So really, the situation really is dose rates, or dose estimates would never really be used, right? Because you're either monitored --

MR. HINNEFELD: Okay.

MR. BARTON: Or if you're actually

out by the K-65 silos, and when you say that in your CATI, then you'd get the coworker model, right?

MR. HINNEFELD: Yes. You're going to be monitored --

MR. STIVER: But once again, this kind of pre-dates the coworker model. So that, you know, we're looking back at Zion.

MR. HINNEFELD: Yes. I mean, maybe it comes from that. Maybe it comes from back before there was a coworker model.

MR. STIVER: Yes, yes. I think the problem with this is these languishing for eight years.

MR. HINNEFELD: Yes.

MR. STIVER: The program moves on, and people involved in developing these are no longer involved. And so, what seemed to be an important thing at the time may no longer be very pertinent.

CHAIRMAN CLAWSON: This is only dealing with unmonitored, right? Because, I guess I was kind of looking at a little bit

different of people going out there and taking these rad readings on top of that, that you're --

MR. HINNEFELD: Yes, but they --

CHAIRMAN CLAWSON: But they're all monitored.

MR. HINNEFELD: Monitored.

CHAIRMAN CLAWSON: Right.

MR. STIVER: And your rad safers are being monitored, have their own instrumentation.

CHAIRMAN CLAWSON: How about any of the other Board Members? Are they, have we got any questions on this?

MEMBER SCHOFIELD: Not at this point.

CHAIRMAN CLAWSON: Okay. What do you feel we ought to do, Phil? I don't see where we use this. I think this is kind of a remnant from before.

MR. STIVER: See, the key thing that's open really has much value to it.

CHAIRMAN CLAWSON: I recommend to

close it. Any other Board Members have any issues with that?

MEMBER SCHOFIELD: I can't think of any reason not to at this point.

CHAIRMAN CLAWSON: Okay. Okay, we'll close that then.

MR. STIVER: Okay. Let me make a note to that effect.

MR. STIVER: Now, this is kind of an interesting one, 29. This takes us way back again. Occupational internal exposure radon is estimated based on just two radon data points from 1953. This is an inadequate basis to reconstruct occupational radon dose.

It's clearly not related to radon emanating from the silos. But due to radon progeny and hail during driver unloading as Silos 1 and 2 were being filled. I don't believe this ever made its way into worker discussions outside of some other related issue. I guess the response is what kind of surprised me.

MR. ROLFES: I was looking at the

response too when I saw that earlier. I don't --

MR. STIVER: It says --

MR. ROLFES: -- know how that got in.

MR. STIVER: NIOSH is recommending the 1953 radon exposure be added in the SEC. And that would certainly make the point moot, the finding moot. But I just put that question to NIOSH.

MR. ROLFES: I'm curious how it got in there myself. Because I saw that. And I thought maybe it was something that, you know, just popped in there. But I don't know where that came from.

And I think this pertains to maybe using radon breath data for the estimation of radium body burdens, is this, that we would incorporate that into our approach for reconstructing the progeny from K-65 filling operations.

DR. BEHLING: Can I make a comment here? I believe I have a fairly good

understanding what was meant by --

CHAIRMAN CLAWSON: Okay. Please.

DR. BEHLING: -- Finding Number 29. And I think it is really based on the 13,000 drums that, for which the raffinate was transferred into Silos 1 and 2. And I looked at what he wrote back in 2006.

And the issue that he raised was probably addressed much more extensively in my review of the SEC Evaluation Report later on. And I identified that particular issue as Finding Number 4.2-1. And I just want to go over that.

Because this is the way in which NIOSH modeled the exposure, internal exposure, principally from the transfer of raffinate from the 13,000 drums to the Silos 1 and 2. And I addressed that in my draft report back in 2007.

And if for any reason somebody wants to look at that extensively it's defined on Page 37 to, I guess probably to, let's see, that's containment, 44, all the way to Page 46. There's an attachment to it.

But what I really questioned there was the way in which NIOSH assessed the exposure potential for the transfer of raffinate from drums to silos, using a couple of empirical data points. And then modeling those data points in a way that I did not consider claimant-favorable. In fact, far from it.

If everyone agrees, I can go through the issue, or simply defer the issue to a later time by telling you that this issue was addressed in my finding of Evaluation Report, the SEC Evaluation Report at Finding 4.2-1. Do we have time? If we do, I can go through it now. Or we can postpone it for a later discussion.

MR. STIVER: Well, why don't you go ahead and go through it, Hans? This is also related to SEC Issue 4, which is kind of similar, I believe.

DR. BEHLING: Okay. I think I know what's meant by the two data points. But anyway, let me just quickly go through it. Again, for those who may be taking notes, NIOSH, on Page 37 of my report that assessed the SEC

Petition Evaluation.

And on that page I talk about the key elements of the K-65 dose model, which involves the 13,0000 drums of K-65 waste into Silo 1 and 2, between July '52 and September '58. We're talking about a six-year period.

One of the data points was, involved a small number of record data sheets between '52 and '58 involving air samples, which had a wide range of activity levels to find an alpha activity per cubic meter.

And those values range from less than a MAC to 17,777 dpm per cubic meter, or 268 MAC. And so there are some data there. These air samples consisted both of general air samples, as well as breathing zone samples.

And I, you know, identified some of the parameters that involved the flow rate of the air samples, which was consistently around 0.02 cubic liters per minute, or that translates to 20 cubic liters per minute for both general air and sampling at the time. And the same thing, duration was about one to 30

minutes.

Anyway, so what were the assumptions that NIOSH used? They obviously start out with the assumption that there were 13,000 drums. And one of the key assumptions was that this transfer took place around the clock, in three shifts.

And one of the other key parameters, and it was a very spotty parameter, was that one of the data sheets showed that in one day 80 drums were transferred.

Then they used, by and large, to control the time frame during this exposure, because they have air sampling but they don't know exactly the time frame, they used external dose rates. And this is where I sort of had a problem.

A group of external dose data sheets were available for 22 workers. And they were used as a basis for defining the yearly exposure duration for K-65 airborne contaminants, and include the following. One of the, among those 22 there were, NIOSH chose 13 workers with the

highest doses, ranging from 115 to 500 millirem average per week.

And then the available records show that three of the 13 workers were assigned to K-65 for three weeks. And there were ten other workers who were assigned for six weeks.

The highest recorded weekly external gamma dose among the 13 workers was 1200 millirem per week. So, for the 13 workers, the collective average exposure for all 13 workers was calculated at 312 millirem per week.

So they used these dose rates, external dose rates as a way of gauging how much time was spent there. This is T, here. And we're really talking about understanding what the internal exposure was. And that was now based on external dose rates. And I just mentioned those.

So anyway, going on here, NIOSH did define the collective average external dose of 312 millirem per week for the 13 highest K-65 workers, was used by the model to justify yearly

exposure time to K-65 airborne levels by means of the following assumption.

NIOSH assumes for 1952 the annual external exposure limit for penetrating radiation was five rem. If you want to, please write that down. Because I'm going to get back to it shortly.

NIOSH further assumed that the extent they must have had, not being able to prove that, they must have had the more restrictive administrative dose limit of four rem per year.

So, by dividing the assumed administrative dose limit of four rem per year by 312 millirem per week, NIOSH concluded that K-65 workers would be restricted to a maximum of three months, after which the worker would have to be shifted to a non-radiological work location.

The above derived three month per year exposure duration was further reduced to six weeks, as explained by the following statement on Page 27 of the TBD. And I quote,

from the information derived in the external dose data sheets, and the air monitoring sample sheet, it appears that the transfer could have limited to a period of ten weeks per year with no individual working more than a period of six weeks in a year, in order to control external dose within the regulatory limits.

Now, when you go back and check for the early '50s, the regulatory dose limit was not five rem. And there's no indication that there was administrative dose limit of four rem. In fact, the regulatory dose limit during those years was 15 rem.

So, the use, also the time frame is stacked by the very fact that they used the highest externally exposed workers, okay. And then, using the four rem as a restrictive limit, you're already stacking the cards against those who were not among the highest in terms of exposure.

Secondly, as I've already mentioned, the exposure limits during this six year period, the exposure limits employed by

the AEC was 0.3 rem per week, 3.9 rem for 13 weeks, and 15 rem in a calendar year, which is three times higher than NIOSH's assumed value of five rem. And also, there's no indication that there existed such an administrative dose limit of four rem.

All these numbers, the highest exposure dose rate, and then the assumed regulatory and administrative dose rates, are used to restrict exposure time frame for the workers who were transferring the raffinate into the silos, and their potential exposures to an inhalation one.

So, in summary, I don't believe this is claimant-favorable. I think that there is numerous assumptions here that restrict the time frame based on external dose rates and assumed regulatory and administrative dose rates. So, my feeling is that this issue needs to be looked at.

MR. ROLFES: Hans, I think you might be referring to a really old version of the Site Profile, maybe. And we're not using

external doses as a controlling factor to estimate a worker's internal exposure.

We had proposed using the radon breath samples to estimate radium body burden, and associated radionuclides. We're using bioassay data essentially, to estimate workers' internal exposure from K-65 materials.

The external dose rate I know we discussed, you know, external doses as being one of the controlling factors. But it wasn't something that we are proposing to ratio our internal doses, based upon.

MR. HINNEFELD: Well, there's a question, just based on my own ignorance. Do we have an estimate of radon intake, as they were in these, of 55 rem?

MR. ROLFES: Most of the radon would be inhaled and exhaled. But the radium body burdens were being estimated based upon radon breath samples.

MR. HINNEFELD: Okay. Well, that's a radium body burden. And so --

DR. BEHLING: You know, you mentioned that you don't do this anymore. But I looked at the TBD. And Section 5, which is internal, still identifies those values.

MR. ROLFES: Okay.

DR. BEHLING: If you look at Page 26 of the current version, which is 2004 old. And I assume you haven't changed anything. It still has those numbers.

MR. ROLFES: All the updated things that we've discussed in the Work Group meetings have been incorporated into Report 52. It's titled, it's a White Paper basically discussing internal dosimetry issues at the feed materials production center. So our updated approach is in that document, which is --

DR. BEHLING: Well, as I said, I don't, I'm not familiar with that document. But if you look at Page 26 of the current TBD, from 2004 --

MR. ROLFES: From 2004.

DR. BEHLING: -- Page -- 2004, you will see the exact numbers that I just quoted

to you.

MR. ROLFES: Right. And the TBD hasn't been updated to incorporate the discussions over the past eight years from the Work Group. They've been incorporated into Report 52, and ultimately we'll revise the TBD to incorporate that information, once we have closure on the issues.

I believe that we've come to agreement, as a matter of discussion from the past several Working Group Meetings, that this wasn't an SEC issue. That we all were in agreement that we could estimate radium body burdens using the radon breath data. And I think that's what your issue is.

DR. BEHLING: Yes. Well I identified it as an SEC issue, based on my review of the SEC Petition and your Evaluation Report. And when you do change it, do at least look at my finding 4.2-1.

Because I looked at that model, and I find it very flawed. And so, if you update the internal dose, essentially the TBD

component, I think you should look at that as it currently reads, and versus what I identified as a serious flaw.

MR. ROLFES: Other members of SC&A and the Work Group have looked at our Report 52 though, is my understanding. And we, I believe SC&A has come to agreement with us that the new approach that we're proposing is acceptable.

CHAIRMAN CLAWSON: You know --

DR. BEHLING: Well, I wasn't party to that review process then. I'm only --

MR. STIVER: This, actually, Hans, this is John. That resolution of the radon breath data actually pre-dated my association with Fernald. I think it was during the 2008 deliberations that you guys reached consensus on that.

I know it's been listed as no longer an SEC issue. And it's been tabled to TBD. I can't give you the chapter and verse as to why that took place. But I've gone back to the worker transcripts from that time period. I know, John Mauro, you were kind of heavily

involved in it back then. Do you remember much about this?

DR. MAURO: Well, yes, I do. My recollection is that the radon breath analysis was accepted as a method for reconstructing the body burden of radium in workers involved in I guess this drum transfer activity. So, I recall that issue being resolved.

Now, whether that covers the population of workers we're talking about here, I really, I'm not quite sure what workers. There was also an issue related to thorium intake. And, you know, unfortunately this is, you know, it was a little bit more complicated than just looking at the radium.

In other words, I do agree, I do clearly remember that intakes of, body burdens of radium 226 were modeled using radon breath analysis. And there was considerable amount of data for the workers involved in certain activities where -- and that issue was resolved.

And there is actually a procedure on

how to do that. That procedure was reviewed and finalized, and it's, I think that issue was closed. But if we're talking about other radionuclides that might be at issue here, other than radium 226, that might have been inhaled --

MR. STIVER: I remember the thorium 230 came up in later Work Group discussions.

DR. MAURO: Yes. That's where I'm headed.

MR. STIVER: And we did agree that their method could be used to reconstruct doses. I can't tell you exactly why we agreed without going back and reviewing those transcripts. I think this was in the 2010 time frame, 2010, 2011.

But it has been listed, you know, in our records as having been resolved as an SEC issue. Now, I guess the thing we have to do now is keep it flagged for review when TBD 5 is revised, and the Report 52 methodologies are incorporated.

DR. BEHLING: Just a question to

John Mauro. The document that you say reviews the issue and identifies radon breath analysis for the assessment of radium 226, was that the, by and large involve, did that involve workers who were engaged in the transfer of raffinates of the drums to the silos?

DR. MAURO: Yes. But there were, it was, as John points out it was a little bit more complicated because there were other workers involved, where there was thorium 230, but not necessarily accompanied in a known ratio to radium 226.

The way I recall it, the hook on dealing with this problem was that you had the radon breath analysis, which allowed you to predict the radium body burden. And if you had knowledge on the relative abundance of thorium 230 and radium 226 in these, I guess, containers that were being repackaged and handled, you had a way to get a handle on thorium 230.

However, I remember Arjun pointing out at the time that there was a certain waste stream where you didn't have that known

relationship between the radium 226 and the thorium 230.

MR. STIVER: Okay. I remember this. This was involving the transfer to Silo 3 from Plant 2 and 3.

DR. MAURO: Right.

MR. STIVER: And we went through this in a lot of detail. And it sounded like, I'm going to give a bit, I believe this material used an air lift to bring it over. It was dry material. It was air lifted over to Silo 3. There was general air sample data involved.

And also there's a -- I think the issue is that you couldn't really identify thorium, because the uranium levels were so low that there was a concern that, Arjun argues this, that you wouldn't be able to get a hook back on to the thorium 230 that way.

DR. MAURO: Yes, that's --

(Simultaneous speaking)

MR. STIVER: The methodology that we're going to use was more than adequate to address the ranges of exposure you might expect

to see.

MR. KATZ: So, where do you want to go with this?

MR. STIVER: I recommend that we keep this one in abeyance until we have a chance to look at the TBD revision. And that is SEC Issue 4 as well.

DR. BEHLING: Yes. And my feeling is that if the TBD 5 for Fernald gets revised that they simply then delete it if it's not going to be useful in dose reconstruction. Because right now that model is definitely flawed.

The very numbers that I just cited to you regarding that model on Page 26 and 27 of the TBD needs to be eliminated because we don't use this model.

MR. ROLFES: Right. That will be, Hans. And I think that was prior to the time that we had found the radon breath data, when the TBD was written in 2004.

DR. BEHLING: If that's the case, then I think we can somewhat close this issue

out.

DR. MAURO: Or leave it in abeyance.

CHAIRMAN CLAWSON: Well, I --

DR. MAURO: We're going to do it here I guess --

MR. STIVER: We'll leave this in abeyance until we actually see the TBD --

DR. MAURO: Yes, okay.

MR. STIVER: -- 5 revision.

MR. ROLFES: I was going to say --

DR. MAURO: That's what we usually do.

MR. ROLFES: I was going to say, the one issue, the thorium 230 issue coming from the process plants going to Silo 3 is a slightly different issue than --

MR. STIVER: Yes, yes.

MR. ROLFES: -- estimating radium and associated radionuclide body burdens from radon breath data.

MR. STIVER: Oh, yes, yes.

MR. ROLFES: It's two separate

issues. And --

MR. STIVER: The two issues where they were kind of conflated --

MR. ROLFES: Right, right.

MR. STIVER: -- during the finding.

MR. ROLFES: Just the raffinate issue type discussions. I mean, my opinion is that the Silo 1 and 2 workers that were working on dumping the 13,000 drums into Silos 1 and 2, that we've got an approach that addresses that.

But the thorium 230 issue from plant operations, I know we discussed as part of the issue, just because it was lumped into silo discussions. I think they're two separate issue.

MR. STIVER: Yes, they are. They are.

MR. ROLFES: So, I think the one finding that Hans was relating was more towards Silos 1 and 2, versus the thorium --

MR. STIVER: Yes. That's going to be the revision model that's laid out in Report 52 now.

CHAIRMAN CLAWSON: So, we'll keep this open until the --

MR. STIVER: We'll keep it open until they look in the TBD, and make sure that things were done as agreed.

MR. KATZ: Well, it's in abeyance actually, it looks like.

MR. STIVER: Okay. Let's see, where are we here? Well, a series of ten easy ones coming up here. And, John, these are the ones related to medical dose.

DR. MAURO: Yes.

MR. STIVER: Thirty to 32.

DR. MAURO: Yes. I can address those. Originally, these were one of the issues that always came up. This goes way back, related to, do you use photographic analysis, lumbar spine analysis. You make those assumptions part of the medical X-ray.

And there was some guidance on when you do that, when you don't do that. And it has a function of time, that sort of thing. That goes back to OTIB-6. So this has been, the

issue's related to these types of examinations other than chest, the classic standard of DA chest examination.

There are also issues related to, and this goes back a long way, to retakes, issues related to, was, these being collimated. So, these were all related to the medical examinations.

So, what I did is take a look at the new Site Profile, Revision 1, dated 1/2/2014, recent, to see what they say about all these things now. And there's a very detailed description of the equipment that was used, the procedures that were used.

They addressed the subject of retakes. They addressed the subject of collimation. They addressed the subject of uncertainty. And the equipment that was there as a function of time.

And my takeaway from this is that there is good reason to believe that there was not the equipment there for TFG. If it was it would have been part of this, there would have

been some discussion. Because they went through the different equipment they used. So I think we could -- there was a time when we would automatically assume TFG exposures prior to a certainty in 1970. But I think the evidence, the record that we have here now in the Site Profile, you know, is very, quite detailed. And there's no indication that you would assume that there was some TFG examination going on.

So, I'm agreeing with NIOSH that I don't think these are issues any longer. With the new information that they've uncovered and put into this new Site Profile provides a great deal of evidence that both TFG and lateral, as they called them, I guess, lumbar spine examinations, which could be substantially higher than your classic chest X-ray. There's no reason to believe that those took place.

And so, that's not part of the false assumptions that are used in reconstructing worker doses. And I think the section of this OTIB-17-3 gives you the evidence you need to

feel confident that those types of exposure turn out to be assigned, and that they have taken into consideration issues relating to collimation and issues related to retakes. And I'm recommending that we close this issue.

MR. STIVER: John, as kind of a follow on that, Issue 33 is related to that too. And it states that NIOSH had prematurely concluded that lumbar spine actuaries for laborers and construction workers were not conditions of employment.

DR. MAURO: Okay.

MR. STIVER: And this is something we had left open. NIOSH in their response cited several SRDDs, excuse me, claim file records, to show that those X-rays were performed, having been listed as suspensory, and not as an annual pre-year term.

And so Bob Barton went through, looked at about 30 different claim files, and basically came to the exact same conclusion that NIOSH did.

So, we see no evidence that these

lumbar spine X-rays were ever a condition of employment for categories of workers, the heavy laborers and those type of people. And we also recommend 33 be closed as well.

CHAIRMAN CLAWSON: Okay. Board Members, any objections to closing those?

MEMBER GRIFFON: No. I agree with closing them, Brad.

CHAIRMAN CLAWSON: Thank you, Mark. Phil?

MEMBER SCHOFIELD: Yes. I agree too.

CHAIRMAN CLAWSON: Okay. Thank you. We'll go ahead and close those.

MR. STIVER: All right. Now, we're finally down to the remaining issues that were considered SEC issues. And kind of lumped together a lot of the different findings from Hans' 2007 SEC Evaluation Report.

SEC Issue 3 is related to recycled uranium. We talked about that in relation to a couple of the Site Profile findings earlier on. And once again, we recommend that this one

be kept in abeyance pending our review of the new TBD, to make sure that all the agreed upon levels and time periods are in fact incorporated.

I guess as a corollary to that, we're also going to kind of follow up on this issue, this notion of actinium 220, which, or excuse me, americium 241, and how that made its way into the finding.

SEC Issue 4 was the radon breath data, which we just talked about. We agreed that we're going to keep that one in abeyance as well, pending a review of the revised TBD.

SEC Issue 5 is the radon release from the K-65 silos, which Hans discussed earlier. And we're going to keep that open for discussion for the next Work Group meeting.

And we're finally getting down to the end here. SEC Issue 6D was the use of chest counts throughout thorium 232 exposures in the 1979 to 1989 time frame. And as Joyce mentioned earlier, and we've discussed in several Work Group meetings, we're basically in

agreement with that approach.

However, we want to keep this issue open pending our review of the post SEC thorium models. So that will be a topic of discussion at the next meeting as well.

CHAIRMAN CLAWSON: Right.

MR. STIVER: And finally, this last one is kind of an orphan issue. It's not really related to a lot of the other stuff. This was 4.5-1, the absence of performance standards and quality assurance for personnel and dosimeters. I asked Hans to take a look of this, because it pre-dated my involvement. And, Hans, would you like to talk about that?

DR. BEHLING: Yes. By and large, Finding 4.5-1 that you just identified really reflects something that I extracted from the National Lead of Ohio corporate response to these assumed assessment fact sheet dated September 11, 1981, in which it was acknowledged that there are certain deficiencies. But the report is part of my assessment response to this fact sheet.

And it goes from Page 113 all the way to 118 in my report of my review of the SEC Petition and Evaluation Report. Anyway, just to quickly review a couple of things that were cited in this response to this assessment fact sheet, there were some concerns about the fact that test dosimeters, that is control badges, were not routinely processed along with exposed badges worn by people.

There was an issue involving heat damage from leaving badges in cars where the hot weather was a problem. And however the use of industry responses. However, this has not been a real problem for many years. Leaving badges in desks, cars, et cetera, did not have a significant impact of the overall external dosimetry program.

And then there were also issues involving failure to have a bona fide official training program for the technicians engaged in assessing the badges, and so forth, and so forth. And I don't want to make an issue out of it. In fact, I'm going to conclude that this

should be closed.

But my statement of findings, and I quote, I state the following, although SC&A does not generally question the merits of external dose data, the credibility of external dosimetry data has to be viewed in context with several limitations as described in the document entitled Response to Dosimetry System Fact Sheet, dated September 11, 1981.

And all I wanted to do here is, obviously we can't do anything about this deficiency. But sometimes if we do recognize there were certain limitations, what we can do is perhaps explain the uncertainty by which some of the data has been reported.

Normally, when we talk about the uncertainty of dosimeters, when we start out with the assumption that the only variability of a dosimeter response to a constant radiation field is, in the case of film dosimeters, what type of film was used, was the developmental time a constant set of --

In other words, we never, ever

incorporate uncertainty that involves human errors, such as the failure to use control badges as part of this, or perhaps update the dose response curve for a particular badge of film dosimeters that have potentially been revised in some way or another.

And so, I'm not looking to say anything other than, perhaps in the face of certain uncertainties that we notice it and document it, the option is perhaps in explaining the uncertainty associated with the actual recorded doses.

But beyond that I don't expect to do anything. And at this point I don't think there's really any way which we can rectify these deficiencies. Accept them, and say we close out this issue.

CHAIRMAN CLAWSON: Any other Board Members, any questions?

MEMBER SCHOFIELD: Yes. I've got one quick question. And I'm just kind of backtracking just a second here. Talking about the americium, do they know what kind of

quantities were handled and stored there? Whether it was in the form of an oxide, or a metal?

MR. HINNEFELD: Well, this, there was never any americium, you know, per se, handled at Fernald. The question, or the comment or finding has to do with, was there americium in recycled uranium? In other words, uranium that had been, you know, run through the Hanford PUREX.

And then they reclaim the uranium and send it back. That's what we call recycle. And in that recycled uranium there's always a little plutonium and actinium and technetium. And those were the three radionuclides that we looked for in recycled uranium. In other words, the contaminants concerned.

And the finding was, well, you didn't consider americium. And I don't know that there was any americium there. So, that's the nature of the americium is, was it present as a contaminant in the recycled uranium? Not that we handled, not that any americium was

handled there.

MEMBER SCHOFIELD: Okay. Because, see, if it's just like a contaminate in the recycled uranium then --

MR. HINNEFELD: Well, it might have been.

MEMBER SCHOFIELD: -- it's not the issue that I was thinking of. I was thinking of, you know, were they handling a few gram quantities, were they handling kilograms of it, you know, just what I'm, where I was coming from. So that --

MR. HINNEFELD: No, if it --

MEMBER SCHOFIELD: -- kind of takes care of my concerns.

MR. HINNEFELD: Yes. If it was there, if it was there it would have been as one of the contaminants that came out in recycled uranium. But I don't, I know that it was not one that people were concerned about. I don't think that's true all through the complex. I think all through the complex --

CHAIRMAN CLAWSON: Yes. It was

never --

(Simultaneous speaking)

MR. STIVER: -- in the recycled uranium reports I read from 2000 and so forth, the DOE reports that were mentioned.

MEMBER SCHOFIELD: Okay. That answers my questions.

MEMBER GRIFFON: Hey, Brad, this is Mark.

CHAIRMAN CLAWSON: Yes. Go ahead.

MEMBER GRIFFON: I just have one, going back to SEC 4. And this is the radon breath stuff that Hans was talking about earlier, and it came up again. Is there a time period? I'm trying to remember myself. I don't doubt that we discussed this.

But I'm trying to remember what time period this was, this technique was going to be used over. Or is it limited to that specific operation of, involving the drumming of the material for the silos, or what? Does anyone know that offhand? I can also look back at the report. But, I'm curious.

MR. ROLFES: This is Mark. I'd have to look back in Report 52. I know as far as when we would apply it, the method to estimate the radium body burden. The majority of the data that were collected though were in the 1951-1952 time period.

And I want to say that there might have been around 400 usable radon breath samples. I don't know if we put any additional details about using it up until like, you know, a point when we have documentation showing that, you know.

It was, there were a few occasions, you know, in the, you know, maybe one here and there in the 1960s, where they might have dumped additional materials into the K-65 silo. They'd take up a manhole and dump in a barrel, or dump in a small quantity of materials.

We've seen some bits and pieces of documentation showing that there were, you know, some workers that were involved in doing something of that sort. But I'd have to look back to see what years that approach or coworker

approach would be applied. But the reason --

MEMBER GRIFFON: Okay. Definitely I want to look back at that report. And is there an easy way to find this Report 52? I know I've seen it in the past. Or can it just be sent around by email? Or is it something that can't be distributed?

MR. ROLFES: Yes. It's out on the K: drive. I was going to say, it might be on our website. But I'm not certain that it is yet. It's definitely, I can send you the directory if you'd like, or email it to you.

MR. HINNEFELD: It might be easiest to email it to him.

MEMBER GRIFFON: Yes. If you can email it? I mean, I have the government email, so maybe I can get it that way.

MR. ROLFES: Yes, CSP?

MEMBER GRIFFON: Right.

MR. ROLFES: CSP, okay.

MR. HINNEFELD: Yes. I don't know that the Board can see the entire K: drive.

MEMBER GRIFFON: Okay.

MEMBER SCHOFIELD: Hey, Brad?

CHAIRMAN CLAWSON: Yes.

MEMBER SCHOFIELD: This is Paul. I'm back on the line here.

CHAIRMAN CLAWSON: Well, welcome back.

MEMBER SCHOFIELD: On Hans' last discussions, my understanding is you were concerned, or raised the concern about the size of the uncertainty that's reflected in these other kinds of errors.

But in practice, maybe I'll ask you this, that's either covered by the existing distribution that's used, or is there something else that's going to be done that covers that?

MR. HINNEFELD: Well, Hans' recommendation was that we expand the standard, you know.

MEMBER SCHOFIELD: So, I wasn't sure if you were actually planning to do that, or if it's a different general comment on that.

MR. HINNEFELD: Well, I guess I'd have to look at that response in that 1981, what

the situation was there. I do recall from a couple of years later --

MEMBER SCHOFIELD: Hans was recommending that we close it. But I wasn't --

MR. HINNEFELD: Yes.

MEMBER SCHOFIELD: -- certain if there was anything specific that was going to be done about it.

MR. HINNEFELD: Well, I'd have to, I think I'd have to talk to some folks about what makes sense if we're going to expand it, how far do you expand the uncertainty along the --

MEMBER SCHOFIELD: If you need to. Is it already covered?

MR. HINNEFELD: And there is an --

MEMBER SCHOFIELD: Yes, it's just it's the --

MR. HINNEFELD: I mean --

MEMBER SCHOFIELD: -- kinds of regular uncertainties.

MR. HINNEFELD: Yes. I mean, another think to think about as we go down this road in terms of the reliability and the

dosimetry from a couple of years later than that, around 1983, there was some testing done of various dosimetry systems around the DOE system to determine how they would compare to the upcoming proposed Bell Lab standards, you know.

Because Department of Energy was interesting in publishing these Bell Lab standards, but they wanted to see how people would do ahead of time. Because they didn't want to create a disaster by just plopping these to that.

And so they did a round robin test, or not round robin, they had several DOE processors participating in this testing against that. And Fernald was one of the few sites still using film. This would have been in the early '80s. Most people were on TLDs by then.

And they kind of confounded the expectations by performing really well in that round of testing, their film badge did. So, there's at least another data point from a

couple, a year or two after the '81 event to indicate that Fernald's dosimetry was pretty reliable, you know, for what it was attempting to measure.

So, I'm kind of a mixed emotion about that, you know, on the one side, you know, if you, it really makes no particular, there's no downside really to expanding the uncertainty of the dosimetry reading if there's reason to do that.

I'm just not really 100 percent sure there is, because there is other data about the performance of the dosimetry system from about that same time period, where it would seem that the data was pretty good.

And I know the people who did the dosimetry processing. And I'm sure there was not a formal training program. But these were, I don't want to say old people. They were experienced people who had spent a life of meticulous care in their work.

And despite the fact that there wasn't a formalized documented training

program, it was only like one or two people. And they knew what they were doing.

DR. BEHLING: And let me just add a couple of statements. Because in that review of the dosimetry report I think he responds or Fernald's response was as follows, there were no specific training requirements for the film badge technicians when this program began in 1951.

The technician received on the job training. The technician now performing, i.e., and this is 1981, all film badge process began this work in 1952, and he's been the only technician doing this task since '59. So you're correct, Stu. Obviously this person was not doing it wrong. He'd been doing this work for many years.

But, nevertheless, there was no formal training. And I guess one of the deficiencies was the failure to use control badges with each badge, of worn badges, which is usually standard practice.

I'm not saying that there were real

deficiencies here. But in light of contemporary requirements you would say, well, there's less than what you would normally expect in today's world. And then, I'm not going to recommend anything else beyond that.

I just brought it up, because it happened to be part of the information that I reviewed in behalf of the SEC. And, by the way, there was, in our, on that issue of NIOSH has stated that NIOSH will attempt to make more information available on O: drive for data capture. And they include five documents, 43, 36, 46, 18, 42, 439, 85, 99.

I reviewed those documents. And they have a certain amount of merit. But they really do not address the issues that were cited in this particular finding. I mean, they go back, and they had a comparative review of dosimeters back in the early 1940s, '43, amongst the different laboratories.

And they showed to be fairly consistent in response to a constant radiation field. And that assures that the dosimeters

were fine, operating fine under controlled conditions. But that, those documents that I read and offered, you offered to me to read, really didn't address the specific issues that were identified in Finding 4.2-1.

So, anyway, as I said, I stand by what I said. I don't think you can really do much. If there's anything that could be done is to perhaps widen the uncertainty associated with dosimeters. But I don't think that's doable, and at this point necessary.

CHAIRMAN CLAWSON: So what do we want to do?

MR. STIVER: Having the discussion, to close it out. There's nothing much to be done about it, adjusting the uncertainty.

First of all, you'd have to quantify, you know, what the increment would be applicable. And whether it would be a sum, and how that would affect the outcome, you know, for the individual badge, or for the model based on the badge.

CHAIRMAN CLAWSON: Other Board Members with this last one here? Do any of you have a problem with closing this out? Because I don't know what to do with it. You know, we've been discussing about this. So I guess I just wanted to know what you guys' feeling about this was. Mark, any problem with it, closing it?

MEMBER SCHOFIELD: I think it's kind of moot at this point. Let's just close it out.

CHAIRMAN CLAWSON: Okay. Thanks, Phil.

MEMBER GRIFFON: Yes, I think so too, Brad. This is Mark.

CHAIRMAN CLAWSON: Paul? Not hearing any, we'll go ahead and close that one out.

MR. STIVER: And that was the last of them.

CHAIRMAN CLAWSON: Yee haw.

MR. STIVER: Made it all the way through. So, I really --

CHAIRMAN CLAWSON: No, we --

MR. STIVER: We've got quite a few that are still in the docket.

CHAIRMAN CLAWSON: We've got some still there. But, well --

MR. STIVER: So, next meeting.

MR. KATZ: John, you were worried we wouldn't have enough to talk about today.

MR. STIVER: Yes.

MR. KATZ: We've made it.

MR. STIVER: Yes. I'm still revising my estimates.

MR. HINNEFELD: I thought we'd be here all night.

CHAIRMAN CLAWSON: No, not tonight. We would be --

MR. STIVER: We've spent entire meetings just talking about radon.

CHAIRMAN CLAWSON: Well, we've got to be able to get through this.

MR. KATZ: It's a reminiscent day. Next meeting. We want to hunt for a date already?

MR. STIVER: Late November? Before Thanksgiving weekend? Because I won't be around.

MR. KATZ: Well, John, when do you think you have to get your material cleared, and so on? So, when do you think?

MR. STIVER: Well --

MR. KATZ: You're actually giving everybody --

MR. STIVER: -- we're shooting for --

MR. KATZ: -- time to review it.

MR. STIVER: -- for the post-SEC thorium to have a document ready for DOE clearance, towards the end of October.

MR. KATZ: Okay. Through clearance, finish clearance, or into clearance?

MR. STIVER: No, into clearance, depending on how long --

MR. KATZ: Okay. And how long has it taken them for these Fernald --

MR. STIVER: I usually like to give

them a couple of weeks.

MR. HINNEFELD: Yes. They asked for ten working days, two weeks.

MR. KATZ: Okay. So then end of October you get it to them. That puts us halfway through, or at least a quarter of the way through November, right?

MR. STIVER: Maybe the week after --

MR. KATZ: Plus we have a Board Meeting in November, the 6th and the 7th. So, I would say we wouldn't want to look to schedule before either -- well, there's Thanksgiving week. We don't want to do that.

There's the week of the 17th. If you think that's too early then we should push it to -- I was thinking we have, Brad, we have NTS in December, beginning first week of December. You want to partner these up?

CHAIRMAN CLAWSON: I would.

MR. KATZ: That would help you, right?

CHAIRMAN CLAWSON: Yes, it would.

MR. STIVER: It would be a long trip.

MR. KATZ: So, NTS is December 3rd. What about, and that gives extra leeway for getting these things done. What about the 2nd or the 4th. That's a Tuesday or a Thursday, December 2nd or 4th. Mark, would the, how, do you have anything on your calendar for that week? Mark Griffon?

MEMBER GRIFFON: I just need a second to look.

MR. KATZ: Oh, yes. No, no, I wasn't rushing you. I just wanted to make sure you understood when -- and how about you, Phil, too? And Stu and Mark, does that work for you guys?

MR. HINNEFELD: Works for me. The 4th would be better. But I could do the 2nd.

MR. KATZ: How about you?

MR. ROLFES: I'm sort of at the hands of someone else right now in determining my future schedule here.

MR. HINNEFELD: Your knee, your

surgery thing?

MR. ROLFES: Yes. I'm going to be on crutches at least six weeks I think. So, I don't know.

MR. STIVER: Well, you could still talk.

MR. ROLFES: I can. I can participate by phone.

MR. HINNEFELD: We could let him call in. We wouldn't make him hobble down.

MR. ROLFES: I haven't scheduled anything yet. So I just don't know exactly --

MR. KATZ: Okay.

MR. ROLFES: -- when.

MR. KATZ: Okay.

MR. ROLFES: I haven't spoken with him.

MR. KATZ: So, you said the 4th is better for you, Stu?

MR. HINNEFELD: Yes. I can do the 2nd, though.

MR. KATZ: Okay.

MR. HINNEFELD: Yes.

CHAIRMAN CLAWSON: Let's shoot for the second.

MR. KATZ: Well let's, I just want to hear from Mark.

MEMBER GRIFFON: I'm okay on either of those days.

MR. KATZ: How about you, Paul?

MEMBER SCHOFIELD: I can do the 4th, but not the 2nd. Which date now, the 4th?

MR. KATZ: Yes. How about the 4th, Phil, December 4th?

MEMBER SCHOFIELD: November 4th?

MR. KATZ: No, December 4th, December 4th.

MEMBER SCHOFIELD: December 4th.

MR. KATZ: That's a Thursday.

MEMBER SCHOFIELD: Let me check quick. That may be when I'm in Denver. Take me up there and dissect me.

MR. STIVER: Several operations.

MR. KATZ: That sounds great.

MEMBER SCHOFIELD: Yes, if I'm still alive at that point.

MR. KATZ: That's good. We want you alive. No inert bodies around here. Okay. So, December 4th it is. Fernald.

MR. HINNEFELD: So, Brad, I'll trade you birthdays. That's my birthday.

MR. KATZ: Oh, isn't that awesome.

MEMBER SCHOFIELD: Hey, Brad, I got something to tell you.

MR. HINNEFELD: It's also the day I --

MEMBER SCHOFIELD: I'll always be able to remember your birthday now.

MR. STIVER: The Lord giveth and the Lord taketh away.

CHAIRMAN CLAWSON: Why is that?

MEMBER SCHOFIELD: [Identifying information redacted]

CHAIRMAN CLAWSON: Well, congratulations.

MR. STIVER: Congratulations, Phil.

MEMBER SCHOFIELD: Thanks, all. I'll be able to remember your birthday from now

on.

CHAIRMAN CLAWSON: Well, I hope that's a good thing.

MEMBER SCHOFIELD: I'm sure it will be.

CHAIRMAN CLAWSON: I wouldn't worry about my birthday, I'd remember hers.

MEMBER SCHOFIELD: Well, see, that's kind of like I remember hers, then I remember yours.

CHAIRMAN CLAWSON: Oh, I see. Okay.

MEMBER SCHOFIELD: And if I forget hers, her grandma will remind me.

CHAIRMAN CLAWSON: Oh, okay. Well, anything else that needs to come before the Work Group at this time? If not --

MR. STIVER: I guess, Stu and I, we can kind of email each other about, you know, the coming deliberations, and so forth.

CHAIRMAN CLAWSON: Right.

MR. STIVER: And get all that squared away.

CHAIRMAN CLAWSON: And we'll go from there. If not, I'll take a motion to adjourn.

MEMBER SCHOFIELD: I second that one.

CHAIRMAN CLAWSON: Okay. We're good.

MR. KATZ: Okay. Very good. We're adjourned. And thank you, everybody, for all the hard work that went into this.

MEMBER GRIFFON: Thanks a lot.

MR. KATZ: And today. Take care. Bye, bye.

CHAIRMAN CLAWSON: Thank you, everybody.

(Whereupon, the meeting in the above-entitled matter was adjourned at 3:27 p.m.)