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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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SAVANNAH RIVER SITE WORK GROUP

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WEDNESDAY
FEBRUARY 26, 2014

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The Work Group convened via teleconference at 10:00 a.m., Bradley P. Clawson, Acting Chairman, presiding.

PRESENT:

BRADLEY P. CLAWSON, Acting Chairman
JAMES E. LOCKEY, Member
PHILLIP SCHOFIELD, Member

ALSO PRESENT:

TED KATZ, Designated Federal Official
JIM NETON, DCAS
TIM TAULBEE, DCAS
DEKEELY HARTSFIELD, HHS
MATT ARNO, ORAU
MIKE MAHATHY, ORAU
ROBERT BARTON, SC&A
HARRY CHMELYNSKI, SC&A
JOE FITZGERALD, SC&A
JOYCE LIPSZTEIN, SC&A
ARJUN MAKHIJANI, SC&A
JOHN STIVER, SC&A
BUCK CAMERON, ATL
DAVID ANDERSON
BOB WARREN

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

(10:05 a.m.)

MR. KATZ: So let's get started. First of all this is the Advisory Board on Radiation Worker Health, Savannah River Site Work Group.

And there is an agenda for today's meeting. It's the same agenda as was for the meeting earlier in February, because we're still going through that agenda. And we could just kick off, where we left off from the last meeting on there.

So there is also a document on the web site that is a Matrix of Issues that has been updated by SC&A so that we're abreast of current status on all the issues that we're working through. And I assume that will be used heavily today.

So let's do roll call. When speaking from specific sites, please speak to conflict of interest when you respond.

(Roll call)

MR. KATZ: Okay then. Brad, you can kick it off. But really, I think it would probably be helpful if NIOSH or ORAU to tee up where we left off.

ACTING CHAIRMAN CLAWSON: Well, yes thanks, that's, like I say this is Brad Clawson. I guess we're going to start up where we left off. And I believe it was in NIOSH's court there. So I'll turn it over to either Tim, or who's going to respond?

Is that correct, or are we waiting, is it SC&A?

DR. TAULBEE: This is Tim. I guess I have a question for you as to how you want to continue on? We could continue on with where we stopped, which was issue or Finding Number 4 from the thorium, our responses on the thorium issues of Addendum Number 3.

But I guess Joe Fitzgerald, he sent out a memo to the Work Group the other evening, that kind of summaries where we are at. And we can kind of I guess in a sense, instead of going

through line by line, kind of --

ACTING CHAIRMAN CLAWSON: Yes.

DR. TAULBEE: -- certain findings together. That seemed to make more sense, that we could discuss instead of going through the line by line of the thorium, and then into 8th Issues Matrix.

So Brad, I'm not sure which way you want to try and respond to this? So it's entirely up to you.

ACTING CHAIRMAN CLAWSON: Well, let me talk with Arjun or how, Arjun, how would you guys like to address this? I know that the memo was sent out on the thorium. Do we want to discuss that right now? Or what would you like to do?

MR. FITZGERALD: Yes, Brad --

DR. MAKHIJANI: Well, Joe is my, the top manager and I think he sent out the memo. Maybe he should respond. I think maybe it might be good to start with the memo that he sent out.

ACTING CHAIRMAN CLAWSON: Okay, that sounds good. I'm sorry, Joe.

DR. MAKHIJANI: I think maybe Joe is most appropriate to respond.

MR. FITZGERALD: Yes, let me clarify just a little bit. Just taking off what Tim said. I thought it would be helpful given the fact we are continuing this several weeks later, to recap a little bit.

And we also as I had indicated two weeks ago, we did not have a chance for Joyce Lipsztein to provide her responses on some, a number of neptunium issues, as a matter of fact.

Because she was, you know she was out of the office during that time. So what I would propose is maybe we could back pedal a little bit. Go back into that neptunium report and start with, what I indicated, is Finding 9.

Kind of where we kind of jumped because we could not address those issues because she wasn't available. And you know, start with item, Finding 9 and go from there.

And she is certainly on the phone and ready to do that.

ACTING CHAIRMAN CLAWSON: Okay. That sounds fine with me. I'm kind of taken by surprise on this, so please forgive me if a little bit cumbersome. Who's going to be running the Live Meeting? Are they going to put up any of these documents so that -- hello?

MR. KATZ: Brad, with respect to documents for a Live Meeting, I mean you have the matrix, you have the memo. The memo, I don't know if it's been put up on the web site yet. But I think it was, not sure whether it's PA cleared yet.

MR. STIVER: I think, this is John Stiver, I can pull that up onto Live Meeting if you want?

MR. KATZ: Yes, if you would that would be great, John, that's what I was going to say. If you just pull the memo up, that would be great.

MR. STIVER: Okay. Just give me a

minute here.

MR. FITZGERALD: Okay. While he's doing that, yes, it did come in late Monday. So I realize this is really going to facilitate for the participants but, more so than anything else.

But Findings 1 through 8, we did spend a fair amount of time in the February 5th meeting. And I think there was some general agreement that many of those issues touched on the, so called, OPOS or statistical issues on comparison of two worker groups, NCW and CTWs.

And I think we could just certainly decide to defer the discussion to that forum. So we're really kind of picking up on some of the first specific neptunium issues that we certainly could address in the Work Group.

And with that, Joyce do you want to start with Number 9, or do you need anything further on that?

DR. LIPSZTEIN: Okay.

MR. STIVER: Before we start, this

is John. Can everybody see the memo?

ACTING CHAIRMAN CLAWSON: Yes, it's just, thank you. I just hate jumping back and forth from these two when we have some of this stuff come up. So appreciate that John.

MR. STIVER: Okay, thank you.

DR. LIPSZTEIN: Should I start?

ACTING CHAIRMAN CLAWSON: Yes, go ahead Joyce.

DR. LIPSZTEIN: Okay. I think I'm fine on Finding Number 9 there was some misunderstanding between SC&A and NIOSH because the response from NIOSH to our finding, didn't answer our questioning.

So NIOSH has justified the use of iodine-131 region to quantify neptunium-237. SC&A doesn't question the use of iodine-131 region to calculate the protactinium-233, which is the daughter of neptunium-237, to calculate neptunium-237 activity in this item.

What we were questioning was the choice of whole body counter geometry to

calculate the exposure, as opposed to chest geometry. Because we have reviewed data from several workers at that time period, which was the early 70s.

And we saw that many of the geometry that was used was chest count instead of the 40cm arc geometry. And specifically neptunium-237 and iodine-131 activities were often were both registered in the chest count geometry.

We agree with NIOSH that in the region that neptunium was quantified in this time period, it's better to quantify it through iodine-131 activities.

We only question this stretcher method, and we saw, and we agree with NIOSH that it is only a question of calibration and the right calibration factor that should be used.

So what we question is, first, why not use chest count geometry as well?

Why, wonder why only counts from chest geometry, why were they discarded, if

they were discarded? Because if they were not, they should be used with another calibration factor.

And so what we, and there are some, also some countings that we did not see what, there was no specification if it was chest geometry that was used, or if it was 40 cm arc geometry. And we would like to know what was done with these, those countings, if there are discarded or not?

So we were questioning the calibration factor that was used and what was done with the chest geometry counts because it's effective to take it away?

We also noted and we are going to discuss this later, that the intake rate that was derived for the 1970 to 1974 intake period was 93.5 dpm per day. While the intake rate immediately before this period, 1968 to 1969, was calculated as 1.79 dpm per day.

So it's a 50 times increase and we don't know why this, there is this huge

difference? Maybe a problem with the calibration factor, maybe it's not the right people that were counted. So there are many hypotheses on that. And we're going to discuss this a little bit later also.

So that's for Finding Number 9. Is there any question on what I said? Sometimes I don't get myself understood very well.

DR. TAULBEE: This is Tim. I don't have any questions, but I can begin a little bit of response or discussion about this if you'd like.

DR. LIPSZTEIN: Okay, please.

DR. TAULBEE: Okay. And I'm going to rely on Matt Arno, a little bit here coming up to give a little better explanation of the in-vivo counts as to what data we used associated with this.

But just to kind of back up a little bit, the reason that we, well there's three, there's two different geometries, three actually.

Three geometries, you've got the 40 cm arc, and then you've got the chest count which was done with phoswich detectors.

DR. LIPSZTEIN: Yes.

DR. TAULBEE: This goes up to about 1974. Around 1975, is when we have a stretcher geometry where a series of sodium iodide detectors were placed in a concave pattern underneath the flat stretcher --

DR. LIPSZTEIN: Yes.

DR. TAULBEE: -- to give a kind of simulated 40 cm arc type of exposure geometry. And there's different calibration factors for both of those.

We were able to pull those out of the log, Jim Watson's lab notebook. So that's the calibration data that we used to do this.

And I guess the other point before I kick off here to Matt, to try and discuss which data we used -- which I believe to be just the 40 cm arc and the stretcher geometries instead of the chest count data -- and that is to address

your comment on the 93.5 dpm from '70 to '74 and that 50 fold increase.

It's not a problem with the calibration factor, it's the change in methodology.

In the 1960s Savannah River, according to DPSOL 193-302, that's the Bioassay Control Procedure, was monitoring folks based upon urine bioassay for neptunium-237.

Since they had not seen any exposures of neptunium that did not have an equal amount of plutonium in them, they stopped doing a large quantity of neptunium bioassay, or urinalysis for the workers.

They went to kind of an incident based monitoring system where if the plutonium was high, and they were in a neptunium area, then they would initiate the neptunium bioassay, which is why you don't have as many neptunium urinalysis results during that time period.

This changed in 1978 when they went

back to building 235, more to a routine neptunium-237 urinalysis program.

So that reason for that 50 fold increase has to do with the change in monitoring, or the change of the MDA method. That's the sole reason for it.

It's not due to in vivo counting calibration factors or anything like that. It's us changing our coworker model from relying on urine bioassay, to in vivo bioassay.

And then when the urine bioassay kicks in again, 1980, well actually '78 time period, we could use it there, but we continued on with in vivo through 1989. So that's the reason for that big jump that you see.

Even the 93.5, again, results in a very claimant-favorable approach. And we knew this. And that was illustrated in the presentation that I gave back on February 5th when showing the urine bioassay data that we have.

And the in vivo measurements being,

far exceeding it, but the bases are still relatively low. So we didn't feel like this was an unreasonably, or we had sufficient accuracy with the dose, with the dose estimate.

So with that Matt, can you touch on a little bit of the data that we used in the coworker model?

DR. ARNO: Yes. One of the issue there, is when we're doing the coworker modeling, we basically have to for any given time period, pick one type of data.

DR. LIPSZTEIN: Yes.

DR. ARNO: Either chest count data, whole body count data, urinalysis data as the case may be.

So in this early 1970s area where we have both. Some chest count results and whole body count results that Joyce was talking about, we can't combine the whole body count data and the chest count data for doing our coworker study modeling.

We have to pick one or the other.

And we chose to pick the whole body count data due to the preponderance of that data, and the fact that there is more of it to use.

As Tim discussed about the MDAs and the change in the intake rate, through all the coworker studies that we've done in all the sites, there's always a strong influence, there's a strong influence on the intake rate based on what the MDA is for the measurements.

Especially when you're dealing with data that has a fair amount of sensoring in it.

But if you change the MDA of, your method in terms of determining an intake, it's going to have a dramatic impact on calculating our intake rate. And that's what we have in this case.

The urinalysis method is much more sensitive so when we don't have that data, we have to rely on something else. The whole body count in this case.

And as Tim said, even though it's a dramatic increase in the intake rate, it still

results in doses which are reasonable and acceptable for the purposes of this project.

DR. LIPSZTEIN: Okay. I understand that. Just some more questions. So all the chest results were discarded when you did the in vivo. Is there any reason why you preferred the 40 cm instead of the chest?

And also on the next period one, there was the stretcher geometry. I know, I just looked at a sample of about 80 workers that we took by random sample. And all of them were measured using chest geometry instead of the stretcher geometry.

So did you have much more stretcher geometry than chest geometry, to just discard the chest geometry data?

DR. ARNO: I think some of those forms get a little bit confusing. Some of those forms report both whole body count measurements and chest count measurements on the same form.

Like one of the forms typically has

the top half of the page is reporting whole body count results, and then the bottom half of the page is recording chest count results.

So you have to be careful in looking at some of those forms. And they changed, even though it's the same basic form, they changed where on the page and whether or not it said whole body in one place, or chest count in another place.

But one of our basic reasons was, there was more whole body count data to use than chest count data. Especially looking at the gamma ray energy regions of interest that we were interested in.

DR. NETON: Matt, this is Jim Neton, can I say something real quick here? I think, I don't think that they routinely quantified neptunium in the chest counts did they?

DR. ARNO: No they did not --

DR. LIPSZTEIN: No.

DR. ARNO: -- routinely continue

doing a lot of --

DR. NETON: And that's the problem Joyce, because I think the region that they used for americium which would have been the closest regions to the 86 keV P4 neptunium, they only integrated like between 48 and 68 keV. And so --

DR. LIPSZTEIN: No, Jim. Jim, just, that's after the 80s, before the 80s they have iodine-131 and chest. They have everything and chest.

DR. ARNO: One other important thing to keep in mind is that neptunium is a Type M material. It clears out of the chest into the whole body relatively quickly. So it's --

DR. LIPSZTEIN: Well, but, you know, if you measure it just after the worker, at least chest is a better measurement than the whole body.

DR. ARNO: Joyce --

DR. LIPSZTEIN: I don't think I even question that, I think it's okay. I'm

just curious why there are so many chest counts, so many chest monitoring that were not used. And they have all the regions on the chest on the earlier countings from 1970 until 1980.

They have everything on chest also. And they have some measurements that are only chest.

DR. ARNO: Okay, chest measurements are typically looking at the lower energy photons, and then you're recent memo that came out, I guess last week, talking about this 86.5 keV photons --

DR. LIPSZTEIN: That's after the 80s. Let's talk first before the 80s, after the 80s is another thing.

DR. ARNO: We're talking --

DR. LIPSZTEIN: Before the 80s because it's just that I found so many chest geometry countings and it's, I don't know.

DR. ARNO: I mean you can find over a hundred, but that's still less than the several thousand that we're dealing with for

whole body counts.

DR. LIPSZTEIN: Okay, maybe I'm just curious because from the '74 to '79 all the workers that I looked at, they all had chest geometry, not stretcher. That's, you know, maybe a coincidence but I got the 80 workers, and all of them had like that. But --

DR. ARNO: Yes, I think --

DR. LIPSZTEIN: -- that was --

DR. ARNO: -- part of that is the point I was making earlier, is that they're reporting chest and whole body counts on the same form. Like some of those forms will say chest count on them, but you'll see that they're reporting results for cesium.

Well they're not reporting cesium in the chest. They're reporting cesium in the whole body. Both types of data are on the same form.

DR. LIPSZTEIN: I saw --

DR. TAULBEE: Yes, if I can follow on there, Matt and Joyce. If you look at a

photo of the stretcher geometry, it becomes pretty clear.

We've got one that we've requested from the site during our recent data captures. And it shows an individual laying on the stretcher geometry, with the sodium iodide detectors beneath them. And then the phoswich detectors are positioned over top of the person's chest. So these 2 counts were done simultaneously.

The form may say chest counting, but as Matt was pointing out there, they're actually a dual count. With the sodium iodide being underneath with the whole body count in that concave shape, as well as the phoswich detectors positioned over their chest.

And they're all reported on the same form.

DR. LIPSZTEIN: Okay. If you go, then you, we were discussing after the 80s. After the 80s, they had, I found just one case because as I told you, we just looked at the

sample of the workers.

So after the 80s, when you go to the chromium, use the chromium to detect the protactinium-233. They had an accident in which they examined the 86.5 measurement of neptunium-237.

So I was wondering if you, at that time, when for sure the phoswich detector was used in the chest. You have both results as you say at that time. The whole body, the 40 cm geometry and you have chest count, if you can use also the 86.5 keV to calculate neptunium-237?

DR. ARNO: If you look at the gamma ray abundance data. You take the 86.5 keV gamma from the neptunium, and then any gammas in that same general area that would come from the protactinium-233. You wind up with a summed abundance that is 14 percent.

DR. LIPSZTEIN: Okay.

DR. ARNO: And that's on a chest count. You have to contrast that with our

whole body counts, have a summed gamma abundance of, I believe it's about 48 percent. So you're looking at a --

DR. LIPSZTEIN: Yes.

DR. ARNO: -- difference in your gamma abundance percentages. And then you factor into that the type M material going to clear to the body much quicker than what's going to remain in the lungs.

And you wind up with a whole body count that you can expect is going to be much more sensitive than the lung counts.

DR. LIPSZTEIN: Yes, but then you have the prebenoff (phonetic) if you leave them that you don't have when you calculate neptunium itself. And it has an almost 12 plus percent.

DR. ARNO: Well regardless of what adjustments may or may not be need to be made --

DR. LIPSZTEIN: That's what we use now, is neptunium, right?

DR. ARNO: -- factor of four, five or six or more that we're going to get by switching to the chest counts.

DR. LIPSZTEIN: Yes, but then you have prebenoff (phonetic) if you leave them off, so that you don't have when you're measuring the neptunium. And if they were measuring americium, they surely can measure neptunium.

DR. ARNO: We'll also run into the issue that the americium measurements are going to be a compounding factor on the neptunium measurements.

Whereas the iodide, the amount of you know, compounding influence of iodine-131 and the whole body counts is expected to be much less due the rarity of the workers actually having significant iodine-131 intake.

DR. LIPSZTEIN: Yes, that's the other question. Why did you use chromium-51 instead of iodine-131 in this?

DR. ARNO: When they switched, in

the 80s they switched to a different reporting style and they switched the energy ranges that they were attributing to given radionuclides. The chromium-51 labeled region of interest overlapped the region of protactinium-233 gammas where located.

DR. LIPSZTEIN: Okay.

DR. ARNO: A change in how SRS reported in their delineation of the regions of interest.

DR. LIPSZTEIN: Okay. I still think that maybe the neptunium would be better, like we do now. Nowadays we use neptunium-237 because they don't know they can leave them off neptunium and protactinium, but --

DR. ARNO: Ideally that would be good, but it's very hard to do what we can do these days, with you know, germanium detectors that have very good resolution and sensitivity, and apply those same techniques to historical data gathered with sodium iodides that we --

DR. LIPSZTEIN: No, yes, but --

DR. ARNO: -- play with.

DR. LIPSZTEIN: Anyway, the phoswich I think is -- and going back to that difference in the urine. I saw from one of your slides, I was also not in the other meeting.

I saw in one of those slides, I'm going to Finding 18 and 19 when we are comparing the drop from, when it goes, you have the 50th percentile intake rates for neptunium for '68 to '69 was 1.79 dpm per day.

And it increased 50 times from, in 1970 to 1974 it was 93.5. I agree with you that's the difference between the two methods. I agree with it, and I know it is because of this.

But as the method that was used is not a typical method to have the neptunium activity in the body. And you have for some period of time, you have many urine data after '69.

In the 80s you have urine data. Several years you have a lot of urine data. Can

you do a comparison what you would have for those years, what you got with the whole body counter?

Because we see urine, we know it's neptunium. But with the whole body counter, we never know if it is neptunium or another nuclide and also we have the problem of equilibrium with protactinium.

It's just three years you have a lot of counts with, that you have a lot of urine data. Can you compare them to know how fair we are with this method, with urine data?

DR. ARNO: We have done that comparison and that was, Tim Taulbee presented a plot in the February 5th meeting showing that comparison.

DR. LIPSZTEIN: Yes, that was only, that was claimant-favorable. I want to know, because I mean 50 times to be claimant-favorable is for me --

(Simultaneous speaking.)

DR. LIPSZTEIN: -- is okay, it's

claimant-favorable. I don't know if it is scientifically correct. So I wanted to know when you have the same year, if you can compare those? Give SC&A the data, not just say it's claimant-favorable, so that we know how we stand on?

DR. TAULBEE: This is Tim, Joyce. If I'm understanding what you're asking here, is that we take the neptunium data that we have and we compare that to that person's in vivo data? Is that correct?

DR. LIPSZTEIN: Yes. It's just three years that you have a lot of urine counts and you have whole body counts at the same time.

And I saw in your presentation, you probably have this data ready because it said it's claimant-favorable. I want to know how claimant-favorable it is?

What's the difference between the two? So that we can see where we stand for.

DR. ARNO: We'd obviously have to run those calculations. But in the late 60s,

when we switched from urinalysis to whole body counts, the factor of 50 jump in the calculated intake rate.

But when we get into the 80s, and when we transitioned from the 80s to the 90s, the change in the intake rate is only about 10 percent.

So we're looking at a much lower you know, overestimate if you will, in the 80s compared to what we would -- if we had enough data to do that.

DR. LIPSZTEIN: I understand, no. That's not what I'm talking. For example, in '84 you had a lot of urine samples, what I'm seeing from the, from your, from the slides. And in '82 also there are a lot of samples, and in 1980 you also have a lot of urine samples.

So if you take those three years, give what would be, what was the intake based on those on the urine data, 1980, 1982, and 1984, and compare it with the intake rate that you derived from in vivo.

Because then you have the same year, and then you can, and you know that urine samples is neptunium, and you want to compare it to, with the whole body, so we can say, oh it's 10 times, it's two times, it's only so we know where we stand for.

DR. TAULBEE: This is Tim. This is something we can certainly do. It's going to require us to do some calculations, but we can do that.

DR. LIPSZTEIN: Okay, great. Because I don't have the data, so I can't do it. I don't have the urine data.

DR. NETON: Hey, Tim. This is Jim. Isn't there a potentially better way to do this, using the plutonium to neptunium ratio that you've established?

DR. TAULBEE: Well that is another issue, Jim. But let me finish this right here with Joyce, and then I'll address yours.

DR. NETON: Hey, Tim could you turn up your phone a little? Because I'm having

trouble hearing you.

DR. TAULBEE: I'm sorry. Is this better?

DR. NETON: That's better.

DR. TAULBEE: Okay. I just moved it closer to me, that's all.

If you look at that chart that I put up, Joyce, from my presentation, of the neptunium urine data.

DR. LIPSZTEIN: Yes.

DR. TAULBEE: There’s -- it's going to be this difference in comparison. It's not comparing the intake. It's actually just comparing the urine data.

What we did here, or what Matt did, was he calculated based upon the in vivo takes, the intakes. What the urine concentration would be for a worker in those time periods?

Those are the red dots on that particular plot. The bar charts are the actual urine data that we have, and the, box plots

rather, I'm sorry.

And if you look at it in the time periods you're talking about, 1980, 1982, and 1985. You'll see that the, our, with the exception of 1980, always above the 75th percentile of the data. The actual urine data that we have.

Now we can compare the intakes to give you the field that you're talking about. This factor, I mean on this plot of the urine data based upon the in vivo data, in vivo, and the actual urine data that we've got samples for.

DR. LIPSZTEIN: Yes, but then we know where we stand at those times.

MR. BARTON: This is Bob Barton. Can I ask you a clarifying question here? Do we have a feel for how many of these urine samples of people who were actually monitored via urinalysis for neptunium, that would also be included in the in vivo records that are being proposed to use to reconstruct neptunium?

Because that might be a more direct comparison. I think maybe what was done is we looked at it by year. And grouped the samples together, and put it to a distribution like is normally done.

But you get better information if you can actually look at individual workers, and say well they got monitored both methods.

And if we were going to reconstruct their doses using both methods, you know, how do they stack up with one another? I don't know how possible that is.

If we have a feel for how much overlap there might be? And if that, that type of comparison to me is a little more helpful because you're looking at individual workers who if they submitted urinalysis samples they probably were exposed.

So let's take a look and see based on their records, their in vivo records, and the urinalysis through to calculate their intakes both ways. How does that compare? So I guess

I'd pose the question, is that even a possibility?

DR. TAULBEE: Yes that is a possibility. The only difficulty is on the current NOCTS data, we have so few claims. Where if we could do this on a few number of workers?

We could do it for everybody that we have it for, that's possible. But a lot of the, or some of the neptunium data that we got came out of logbooks and from other sources, where we don't necessarily have an in vivo count associated with them.

Without going back to the site to get more data, which is of course possible but much more time consuming and a much longer time period.

MR. BARTON: I understand, so the urinalysis data covers more than just the claimant population. Whereas the in vivo data is strictly for the claimant population that we have. I understand that.

DR. TAULBEE: Okay. Correct Matt, correct?

DR. NETON: Right.

DR. LIPSZTEIN: I'm curious about Jim's question now.

DR. TAULBEE: Yes, one of things that I indicated during our February 5th meeting, was that there's, the site was using plutonium as the basis.

Kind of for their monitoring to cause the additional neptunium monitoring during this time period of 19, I think it's about 1970 through 1978.

And this is based upon the contaminant of plutonium-238 in the neptunium. And so that is another method of estimating this particular dose. Is to use a ratio off of that methodology.

Chose not to use that because at the time, we didn't have complete data, contamination ratio. And in fact today we've seen the data, we've requested the data, but we

still don't have it in house.

Something else that we could compare, to give you all a feel of what the, I guess what the neptunium exposures would be, would be to look at the plutonium and go off with that ratio.

And the, I guess the true measure in that time period, the early 1970s where we don't have a lot of neptunium bioassay data. Does that answer your question, Jim?

DR. NETON: Yes. Yes, I think it does. I mean if I recall correctly, the plutonium was much more predominant in the mix than the neptunium, right? I mean even under some very conservative circumstances.

That would be one way of bounding these exposures using, you know, the urine data developed, not relying on the in vivo count.

I think what we have here, is we've got a couple approaches. And I don't hear anyone really arguing that none of these approaches are valid. I think we're kind of

arguing about technical details here.

It almost seems to me that this problem's more of Site Profile issue than an SEC issue, but that's just my impression, unless you appear to have another thought.

DR. LIPSZTEIN: Yes, I have problems also with using the protactinium to measure the activity of neptunium. I'm well aware that it's used, but you have to have neptunium in equilibrium with protactinium.

And we don't know about it. And I don't think NIOSH comment on this was appropriate because it was saying about making assumption of, on the time pattern of intake. About assuming a chronic intake during a period of time.

I think this doesn't have anything to do with the time of the measurement and the equilibrium between neptunium to protactinium-233 proportion.

The proportion of protactinium-233 to neptunium-237 is only at the time of the

measurement. Is only related to the age of the neptunium first, and also on how long after the exposure the worker was monitored. So how much has decayed inside the body also.

Doesn't have anything to do with assumption about the intake model that was done after you have the 50 percent, the 84th percentile, the 95th percentile of the log normal distribution of the in-vivo data of all workers.

It has to do with what was the proportion at the time of the measurements. So this an uncertain effect. And also I was not happy with NIOSH response that a GSD of three, or over three would resolve everything.

No, the GSD of three or more than three, has to do with the log normal distribution of all the workers. Nothing to do with individual measurements that is one point in the log-normal distribution.

So one thing is the time of the measurement, and the measurement you get for

one person. And the other thing is the coworker distribution that has to do with the log-normal distribution of all the results of the workers.

So I think the proportion of protactinium-233 to neptunium-237 is also an important point to consider when you have the measurements.

DR. ARNO: The ratio is important, but it's also important to keep in mind you do have to maintain a consistent set of assumptions.

You cannot completely segregate the methodology used to determine equilibrium for the whole body count. And then the methodology to do the intake modeling.

The intake modeling is based off of a chronic intake which is used as a surrogate for either, A, an actual chronic intake, or B, a series of relatively small acute intake, which is another valid and common exposure scenario.

We will never know the length of time between an intake or the point of measurement --

DR. LIPSZTEIN: I'm not --

DR. ARNO: -- regardless of chronic intake, or the age of the neptunium to which the person was exposed. And even if we know the age of the cans the person was working with, the contamination in the lab or along the line, may be from previous runs.

You will never know that information.

DR. LIPSZTEIN: So that's a big point, because if you never know this information, it might have been monitoring someone that was exposed to fresh neptunium-237. And so the protactinium won't reflect what was the neptunium exposure.

DR. ARNO: Even if you can never know the precise number for a specific measurement, it is possible to make some reasonable assumptions about what people would

be exposed to. There was a known minimum decay time between when the neptunium was purified and when the work was done on it.

In the context of a chronic intake, and knowledge about how often people were whole body counted you can make reasonable assumptions, especially in the context of the assumption of a chronic intake.

You're thinking about a huge --

DR. LIPSZTEIN: No, no. I think you are mixing one thing with the other. Forget the chronic intake. So that, the intake is calculated for the 50th percentile count, or with monitoring results from the whole population of workers in that year.

I'm talking about each measurement that is a point in that log-normal distribution. Each measurement if you have one worker, he is measured. He was exposed to freshly monitored neptunium. You are underestimating the neptunium content in the body.

So you have, it is at the time of the measurement. Nothing to do with intake assumptions. It's the amount of neptunium at the time of the measurement that you are measuring for protactinium.

So if you have many workers exposed to freshly neptunium-237 with no protactinium in it, you are underestimating the neptunium quantity in the body.

MR. BARTON: If I could just add on to what Joyce just said. This is Bob Barton. I think you know, as you said Tim, you know at some point you just don't have the information to do it perfectly.

I mean we'd all like to, but I think what Joyce is saying is the assumption on equilibrium we're essentially, the assumptions that have been laid out according to it being in equilibrium at the time of the measurement.

Now we know that's it's probably somewhere in between the freshly separated, and in equilibrium between the Pa-233 and

neptunium.

So I guess, you know, where I come out on this is you sort of stated your assumptions in the response. You know, you said usually there was a 25 day period before irradiated, you know, billets were actually handled and processed.

And then you provided some rational then, well you know, a lot of the exposure would come from the, you know, contamination in the plant.

I guess we would kind of like to see that substantiated a little bit more. I mean, you know, it sounds fairly reasonable. But you know, you stated that it's 25 days, but then 25 days doesn't bring you to equilibrium.

So you know, I guess we'd like to see that rationale flesh out a little bit more with some actual sited references. And steps to really build the case that, you know, since we don't know, equilibrium is going to be the best answer versus some other adjustment factor.

DR. ARNO: The problem here is assuming that this is a measurement made after an acute intake. Our coworker intake model for calculating the intake has to be consistent with how we interpret the bioassay data for determining the log-normal distributions.

And that is based off of a chronic intake. And we were assuming that the bioassay measurement is midway in that chronic intake.

DR. LIPSZTEIN: Yes, but the --

DR. ARNO: We need to keep status --

(Simultaneous speaking.)

DR. NETON: This is Jim --

DR. ARNO: -- consistent, if you don't you're invalidating the way you're doing the analysis.

DR. LIPSZTEIN: Not, I --

DR. MAKHIJANI: That's exactly why --

DR. ARNO: Come on in.

DR. MAKHIJANI: This is Arjun. Could I say something? This is Arjun.

DR. LIPSZTEIN: Yes.

MR. BARTON: Actually, I think Jim was trying to say something first.

DR. NETON: Yes, let me just, I was, can I suggest that, you know we have that plutonium ratio data now. Could we not use that to sort of validate some of, not validate but evaluate the appropriateness of the whole body counts?

DR. LIPSZTEIN: Would be great if you'd do it.

DR. NETON: I don't know, you know, because we know this is a maximum or conservative ratio and then you do a, you look at a whole body count and you say is that consistent with what we, you know, with what we're assigning. Because many of them are going to be based on MDA.

DR. LIPSZTEIN: Yes.

DR. NETON: But we know, no, protactinium there at all. Is that a possibility Tim, or am I off base?

DR. TAULBEE: No, I think it is a possibility. We haven't done that yet because I don't have the, all of the ratio data yet. But we can certainly do that.

DR. NETON: I think that we need to go back and we have this ratio data which can do a lot for us. I think we need to, I would say that NIOSH probably needs to go back and look at that.

And it can either bolster some of these issues, or supplant some of them in certain situations. So I think it's not going to be fruitful here to debate whether protactinium is in equilibrium or not at this point.

DR. LIPSZTEIN: Yes.

DR. NETON: So I think --

MR. FITZGERALD: Just to reaffirm that, I think what you're saying is basically it can be used to both, either validate the current approach or supplant it, if it turns out that there's some issues.

Certainly it offers a more data based --

DR. NETON: Right.

MR. FITZGERALD: -- way of doing the estimate.

DR. NETON: Exactly and Tim pointed out correctly. We didn't have this data until a while ago. And we still don't even have them physically.

We became aware of them and I think there's a lot that can be done with this to address the issues that are being raised here, in my opinion.

DR. MAKHIJANI: Could I say a couple of things? This is Arjun.

ACTING CHAIRMAN CLAWSON: Sure Arjun, go ahead.

DR. MAKHIJANI: Yes, just two things. In regard to the equilibrium question, I think in real life there's going to be a distribution of, you know, protactinium in relation to neptunium, from fresh to fully in

equilibrium.

And we don't know that distribution until you establish what kind of activity has happened, which is going to be quite hard.

I mean actually to, in relation to the time of measurement and the time of exposure and so on. So that's, I think that's a very important thing. So, because it makes a lot of difference to what dose you come up with.

And regarding the point that was just being made, with plutonium and neptunium ratios, I think we do have to establish the dose enough, plutonium contamination in all the exposure situations, or essentially the major exposure situations at least.

That we weren't dealing with near pure neptunium targets as they were being fabricated. And to exclude the idea that there was something close to pure plutonium, that we have the necessary ratios.

I know the difference in, there's a big difference in half-life and specific

activity but still we do need to establish the purity of neptunium and it was not dominant in some situations.

DR. TAULBEE: This is Tim, and this comes from that data that Joe looked at, at the same time we were capturing it there in the vaults.

Where we have virtually month by month contamination, plutonium contamination measurement values for both the HB line and then the oxide coming out the other end, which is effectively purified, cleaned up more than what coming off the frames in the canyon.

We had both ratios of data available to us and so I think that's pretty well established that plutonium is a significant component of this exposure throughout the monitoring time period.

(Simultaneous speaking.)

DR. TAULBEE: I'm sorry, whoever spoke after me.

(Simultaneous speaking.)

DR. NETON: Yes, this is Jim. I mean that's why they decided to stop monitoring for neptunium in the first place. Because they realized that the neptunium was a better indicator of intake than the, that the plutonium was a better indicator of intake than the neptunium.

I think we'll proceed. And I'd like to proceed, as I suggested and that we go back and take a look at that and see what the path forward is for either validating, verifying, or supplanting using some of this plutonium data that we have.

It makes a lot of sense, to me I mean, to me it's a good source of information that we could take advantage of.

MR. FITZGERALD: And for reference sake Arjun, this is in the, and correct me if I'm wrong Jim, it's in the Works Technical Reports. The monthly reports that were generated up through the 80s I believe.

DR. TAULBEE: That's correct. We

only, I believe, we only have data in those reports up through 1983 possibly 1984, but that's 1984 is when they stopped manufacturing the neptunium targets. So they kind of end about the same time.

ACTING CHAIRMAN CLAWSON: So first of all, Ted, I don't know whose taking minutes on this because I have no access to this, but it sounds like NIOSH is going to go back and according to Jim, and we're going to use the plutonium ratio, to be able to look at this, is that correct, Jim?

DR. NETON: Well Brad, I think we're going to look at that and see how that might play out for us. I'm not saying we're going to fully use plutonium ratios but we're going to see what use we can make of it, under what scenarios.

ACTING CHAIRMAN CLAWSON: And I understand. But personally, thanks everybody for giving me a headache right off the bat, at the beginning of trying to follow this, but that

sounds good to me.

But I do have one question and that was back on the Findings, well 1 through 9. One of my questions was, and I understood in there that there was an americium difference back and forth.

But that we couldn't, I got from it, that we could not tell what kind of, if it was a chest count or if it was a genomic issue? Because they were both reported on the same form.

And the only way we'd be able to know what type of process, what they were looking for is by looking at what they were, the detector, or what they were looking for? I didn't understand that.

I thought and maybe I'm just misunderstanding this. But I thought that I heard NIOSH responding that the only way, they were both on the same form.

And you'd have to look at what they were, the radionuclide they were looking for to

be able to determine what, if they were doing it in a genomic, well what, is that correct?

I was trying to, we kind of jumped into 18 and 19, and I just wanted to clarify this back on 1 through 9. Is this correct? That when they were doing these --

DR. TAULBEE: I'll --

ACTING CHAIRMAN CLAWSON: Go ahead, Tim, I'm sorry.

DR. TAULBEE: I'll take a stab at that. From 1974 through the 1980s there was a dual count that was conducted when the person was laying on the stretcher.

There was a phoswich detector over top of their chest, and then there's a series of sodium iodide detectors underneath their body. As they were laying on this bed, this stretcher bed.

And the results are reported on the same form, for both the phoswich, which is considered a chest count. And for the whole body count, which is the remainder of the body

from the sodium iodides underneath them.

When you look at the form, you can look at the top part, and you can tell that these are off of the stretcher geometry.

And then at the bottom you'll see where it says chest count, and you'll see where they're given the x-rays for plutonium, for americium and enriched uranium at times.

Those are the chest counts coming from the phoswich detectors. So when you look at the form as a whole, you can see both counts on them. Does that help, Brad?

DR. LIPSZTEIN: Yes, but sometimes, it's only chest. And sometimes it's only whole body.

DR. TAULBEE: That --

DR. LIPSZTEIN: Sometimes it's only chest.

DR. TAULBEE: Is that post-1975? Is that, because I'm thinking that's pre-1975?

DR. LIPSZTEIN: There are some post-1974 too, but many are between '70 and '74,

but many after '74 until the 80s. And then on the 80s then you have both on the same.

DR. TAULBEE: Now --

DR. LIPSZTEIN: After the 80s then you have both, always. Before the 80s sometimes you have just chest, but as I understood you are not considering the ones that are only chest. Right?

DR. TAULBEE: That is correct. You know, looking at the different forms here. The, let's see, that, okay a 1978 one here. Okay, you know, as Matt was discussing, well now even the 1978 here, there is both chest and the whole body.

And then the 1980 form here, I'm looking at an example, it's both chest and whole body. They're there. They're not labeled explicitly, but if you look at the channels, the regions of interest, the channel count data, I guess the channel numbers in a sense, you can tell whether it's chest or whether it's whole body.

Well we have a report or well it was an internal document that was written that kind of goes through each of these different forms and explains the different regions of interest. And how we calculate the activities.

I think I guess we could form, turn this into a report, if this would help you all understand the data that we're using here. We could certainly do that.

MR. BARTON: This is Bob Barton, you just kind of mentioned a form, I think where Joyce's observations are coming from, were actual, the actual claimant files. So --

DR. LIPSZTEIN: Yes.

MR. BARTON: -- I don't know. I don't know if this is a possibility that maybe you know, they use the same form, but they didn't always fill out you know the counts in each, because maybe they weren't doing both style counting at the same time in every case.

Or you know, I mean if it's, I guess I need clarification was it a standard form you

were just referring to? Or I mean are you looking at actual applications of that form as they appear in the claimant files?

DR. TAULBEE: These are the forms as they appear in the claimant files. Looking at post-1975, it's always an electronic printout.

Now sometimes they've written on the electronic printout, but the printout will have channel data associated with it. If you read the form, the printout, I'm sorry.

But prior to 1975 there is more hand written forms and what you'll see in the NOCTS files is you'll see one page, it'll say whole body counted data, and the next page it might say chest counted data.

In reality, that chest data is on the back of that whole body count data form.

DR. LIPSZTEIN: I agree with that, I saw all of this, but I also saw many reports that were written chest, and some '74, '79 especially. There was, most of the forms I

took the data from NOCTS, from claimant files.

And it was like as I said, was a random number of workers that I looked at. What called the attention to me, is that all of the ones that I looked were written chest on them.

So it was not theoretical, but I think that we are reaching an agreement that you were going to look at this data as compared to the plutonium, right?

DR. TAULBEE: Yes. Could you send me the list of those claimant files that you looked at? And we can certainly take a look at it and then, and instead of trying to talk in the abstract here, we can actually?

DR. LIPSZTEIN: Okay, I'll do it.

DR. TAULBEE: I'll appreciate it.

DR. LIPSZTEIN: I'll put it on the O: drive, okay?

DR. TAULBEE: I'm not sure I have access to your O: drive.

DR. LIPSZTEIN: Oh, okay, I'm

sorry. Okay, I'll see which people of SC&A that you get it.

MR. KATZ: Tim, you have access, it's your O: drive. So you have access to everything SC&A has.

DR. TAULBEE: Okay. Does this appear under the Advisory Board, Radiation Worker Health Directory?

MR. KATZ: Tim, that's where I think she'll put it.

DR. LIPSZTEIN: Yes.

DR. TAULBEE: If you put there under the Savannah River Site SEC, then I should be able to find it.

MR. BARTON: Could I ask an overarching question because I agree with Joyce. I think Jim Neton has given us a really promising path forward with the plutonium ratio comparison.

And I pose this question, if you had, had this data that was just recently captured prior to formulating the current

coworker model, I mean would you have used that plutonium ratio data instead?

I mean do you feel it's a more viable and scientifically defensible way to go than what was currently proposed?

DR. TAULBEE: Yes, I do.

MR. BARTON: Well then, so we actually do --

(Simultaneous speaking.)

DR. NETON: That's why I'm suggesting we go back and look at this and see to what extent it's useful. And it may be the best set of data that we could use.

MR. BARTON: Okay, and then so then I think that's really the path forward, and but I would just reiterate Arjun's caution that we need to make sure that when we're looking at these ratios, that these ratios do capture situations where maybe the neptunium is a little purer, and there might not be as much plutonium there.

And it sounds like Tim said, that,

that's definitely the case. That we have that data, it's solid, it's going to be representative of all the situations we need to cover.

And so I would say, you know, be explicit when you guys do that analysis. But listen, these really are the bounding ratios, and you know, when we look at these bounding ratios we look at this coworker model, you know, however it pans out.

But I think we need to keep focused on that we're not missing any situations where there isn't that plutonium ratio data where there could be a significant exposure to neptunium.

So I would only caution that, but it sounds like a really good plan to me.

DR. TAULBEE: Now let me throw out a little bit of a caution on that, because some of those values of the plutonium ratio are less than. Where the plutonium contamination was less than .05 for example.

So you know, that would be the average for example and they give a minimum and a maximum, and we have an estimate of the number of samples.

So it's not that every single one there is, you know, the average is showing that, you know, the plutonium contamination is .1 or something like that.

There are months, especially you get into the later 70s, where that ratio does begin to decrease a bit. And so it goes into a non, I don't want to say non-detectable type of scenario, but the values that they're reporting were a less than value.

So it does play a little bit back into you know, what Arjun was mentioning of dealing with really pure neptunium if you will, and that was our whole, one of our main reasons for going with the whole body count initially early on. Was we didn't know how that value changed over time.

Right now I just have a general feel

of how it changed, based upon looking at the reports, and the data that we captured.

But we don't have that data in hand to actually have the analysis and to be able to trend it and see what is actually happening with it.

But I do agree this is the way I would, if we had the data early on, we would have gone down that path of the plutonium contamination methodology. And as I clearly was monitoring based upon plutonium because they felt that, that was the most accurate as well.

DR. MAKHIJANI: This is Arjun. Can you hear me?

DR. TAULBEE: Yes.

DR. LIPSZTEIN: Yes.

DR. MAKHIJANI: One thing for this less than, if you have a positive neptunium result above the MDA, and less than plutonium results. This is going to be a big methodological problem.

Of course if most of the measurements are positive for both, then you don't have a big problem. But if you have many measurements of plutonium, less than and neptunium less than.

Or plutonium less than and neptunium positive, this of course would not allow us, you know, would create difficulties. I just want to put that on the record.

DR. TAULBEE: Yes, you're correct there, Arjun. The scenario where this falls apart is where you have a positive neptunium and a negative plutonium. And we do recognize that.

I have not seen that, I'm not saying it doesn't exist, but that's not something that I've seen yet.

And from the cases where I've seen people having exposure, neptunium exposure where they had a nasal smear that came up positive and they did follow up bioassay, then the cases, the case that I was looking at

yesterday for example, the plutonium was on the order of like 5 dpm and the neptunium was non-detected.

So you know, that plays into the opposite role of where this plutonium methodology works and is far more sensitive.

DR. MAKHIJANI: Right, I just want to yes, I just want to raise that flag and then we'll, you know, I'm not disagreeing with anything that's being said in terms of going ahead. And John, you can put me on mute.

MR. FITZGERALD: Jim, Joe. What, I know this data was collected last fall. Is there any sense of when it might be available?

DR. TAULBEE: I had conversation with Savannah River yesterday about this to try and get that data. Because they have not, our notes from our classified vault visit, they haven't even begun reviewing MD2 funding issues.

I'm currently trying to get those bumped up from a priority standpoint to where

they will release some funds to the classification reviewers, so that they can review them, and we can get the data, at least from that standpoint.

I'm hoping in the next few weeks that we can actually get the data in-house from our notes. Which is where we extracted all that data, Joe, if you recall?

MR. FITZGERALD: Yes.

DR. TAULBEE: So that's what I'm hoping for right now.

MR. FITZGERALD: All right. Thanks.

ACTING CHAIRMAN CLAWSON: Well, I guess I'd like somebody to, Joe, possibly you can help me out, let's just summarize our paths forward on this so that we understand what we're doing.

I'm, Jim, I appreciate what you've put out there because I personally think that will help a lot too. But we also need to get, we need to get this data in hand so that the

people evaluating this can be seeing the same information, that all of will be seeing the same stuff.

So what's the path forward on this, Joe? And Jim, I guess, or Tim.

MR. FITZGERALD: Yes, I think you know, Joyce and Matt and everyone else has certainly identified some of the questions that we're having relative to validating the in vivo versus the bioassay.

And I think what Jim has offered is a pathway which would be much more, perhaps efficient, than going back and you know, doing some de novo analyses between the urine and the in vivo, going a little bit further into individual data comparisons.

And in this case, as Tim noted, we collected or observed a lot of very specific Pu/neptunium ratio data that's in the monthly technical reports.

So assuming that data will be available I think that would be the comparison

that would be most useful to do at this point. And I think I like the perspective that once that data is available it can be used to do the kind of validation we're talking about, much more readily and credibly.

And if that validation demonstrates that the approach falls short, then I think what Tim and Jim have said is it would offer an alternative methodology that would be in fact more advantageous in modeling respects than the one that had been used from a couple years back.

So this is responsive I think. And maybe Joyce can help me. I think we've kind of bridged between several findings in the neptunium arena, you know, 9, 10 and 11 at least. Maybe even further.

And this would offer some of the validation that we lack and that we're raising questions about relative to the current approach for using the in vivo, in vitro, the different methodologies and how they compare.

Joyce, I know you covered a lot of

ground. Is this path responsive to at least the issues in 9, 10, 11? I know we kind of moved forward along that line.

DR. LIPSZTEIN: Yes, I do. I think it goes, I think all the issues that I had, it covers it.

MR. FITZGERALD: All right. So Brad, I think this would be the path forward, and the action. We, SC&A as Joyce offered would put the sampling that she did on the individual claimant files up on the O: drive for Tim and his crew.

And NIOSH would await receiving the ratio information from Savannah River, at which point they would, as I understand it, would assess, you know, what would be appropriate in terms of analyses. And then would go forward.

And I don't know Brad, whether the Work Group would want some kind of indication from NIOSH at that point, what that decision would be?

In other words now that the data is

available and they have a chance to look at it, what specific they would do in terms of an analysis of some sort?

With the Board meeting coming up, that might be a useful, you know, feedback, given the fact that the analysis itself might take some time.

DR. MAKHIJANI: This is Arjun. I'd just like to say one more thing. That the issue that we've discussed today relate to the procedure for dose reconstruction. Basically using all worker data, mostly non-construction worker data.

And but you know, we've also raised a number of issues regarding whether than can be used for construction workers, that are quite separate than this. And still need to be resolved.

DR. TAULBEE: And along those lines, this is Tim. And along those lines on this, we're you know, been using the plutonium data we have a lot more plutonium data for

construction trades workers than we have neptunium data for them. So yes --

DR. MAKHIJANI: Maybe this will address that, Tim. I just wanted to remind people that, yes, both those issues do need to be taken into account also before this can be resolved.

DR. TAULBEE: And well I do have one other thing I'd, just, I had two tasks listed down for us. So one is to develop a model based upon the plutonium contamination that Jim was indicating there.

Do we still want to do, because the other task I have is the comparison of the neptunium urine bioassay for the few workers that we do have that for, and their in vivo counts.

Do we still want to do that particular comparison? And illustrate, I guess the order of -- or the increase of using the whole body count in vivo data in our protactinium equilibrium assumption.

I think we still want to do that case, is that correct?

DR. LIPSZTEIN: That would be great if we can. It's just three years that you have enough data, right?

DR. TAULBEE: Yes.

DR. LIPSZTEIN: That'll be great.

DR. TAULBEE: Okay. Those are the only two tasks that I have listed here. Is that everybody's understanding as well?

MR. BARTON: Tim, this Bob Baron. When you mentioned that second task just now, are you talking about comparing individual workers? Is that what we're referring to?

DR. TAULBEE: Actually, yes. In, the few individual workers we have, that was my thoughts. But now that I think about it a little more, of what Joyce just said.

I think she's wanting us to compare the entire distribution as well for in vivo for those years. And the combined bioassay where we have a lot of data as well. So maybe this is

two parts?

MR. BARTON: Okay, I thought that the distribution clearance, and the distributions had already been done. And that was within the February 5th presentation.

DR. TAULBEE: It had not been done for intakes.

MR. BARTON: Okay. I understand.

DR. TAULBEE: We did for the actual urine data, but not actually documenting, showing the intakes. I mean Matt's already done the calculations effectively, it's just we've got to pull from a different place and write it into a report.

MR. FITZGERALD: So you're saying it would be a two part to that?

DR. TAULBEE: There would be two parts to it, but it would all be in one, I think in one report.

MR. FITZGERALD: Right.

ACTING CHAIRMAN CLAWSON: Yes. I guess, this is Brad. Does that sound good to

everybody?

MR. FITZGERALD: Yes.

(Simultaneous speaking.)

MR. FITZGERALD: With the proviso, I think it would be very helpful for the Work Group to know when NIOSH actually receives the data and makes some kind of, and this is what Jim was saying, some kind of decision based on, you know, again the data has not been available yet.

But to review that data and decide, you know, what makes sense as far as analysis. Just sort of a milestone for the Work Group so there's some indication of where this is going.

Because this would pretty much wrap around most of the remaining neptunium issues. Not all of them but most of them.

ACTING CHAIRMAN CLAWSON: Okay. Well does this, I guess the Board Members, does this sound okay with you? Phil?

MEMBER SCHOFIELD: Brad, this is Phil. That sounds like a good idea.

ACTING CHAIRMAN CLAWSON: Okay, well. I guess my question is here, Ted and I want to make sure this gets documented, so I just --

MR. KATZ: Brad?

ACTING CHAIRMAN CLAWSON: Yes.

MR. KATZ: This is Ted. Yes, this isn't a problem. So we can have, you know, Joe, at the, once the meeting is closed you know, tomorrow or the next day or whatever.

When you can get to it, Joe will just send out a brief synopsis of the action items on the table so that everybody is clear about what's coming.

MR. FITZGERALD: Yes, I've been taking notes and I will circulate that and people can check and make sure I got the nuances correctly.

ACTING CHAIRMAN CLAWSON: Okay. This is what I wanted to make clear because I think this is a good clear path. And I'm with you, Joe. I'd like to know when NIOSH finally

does get this data.

Now SC&A also has action items for Joyce to be able to give them their, this, where her information comes from, the files. And so I, with that I think we're pretty well done with these until we get this other information back. Is that correct?

MR. FITZGERALD: That's correct. Tim could you repeat your two part, I mean I got some of it, but not all of it?

DR. TAULBEE: After I get off of mute, sure, sorry. The first part will be comparing the distributions of the intake values that we come up, from the in vivo data for 1980, '82 and '85 I believe.

And we'll compare that to the intake that we get from the urine bioassay distributions for those years where we have sufficient urine bioassay. So that's part A.

Part B, would be to take the few workers we have in NOCTS that we know worked with neptunium and had neptunium bioassay and

compare their in vivo data to their neptunium urinalysis data.

And calculate the, I guess the intake? Is that correct as to what you were looking for there, Bob and Joyce?

MR. BARTON: Yes, this Bob. I think that's pretty much what we're looking at. The only thing I can add, is I don't think you have to necessarily restrict it to only those that have neptunium urinalysis and the whole body counts.

Because you could still offer up some weight of evidence, arguments if you just have the neptunium urine bioassay. You can calculate an intake based on that.

And then you can go and compare it to what the coworker model would have assigned to these neptunium workers, had they not been monitored. Which is really to me kind of the end game there.

I mean if we have a bunch of neptunium workers, we know were probably the

highest exposed to neptunium and we have some records for them, we can reconstruct their intakes and compare it to what that coworker model says they would have gotten.

And that can go a long ways toward putting a lot of these issues to bed.

DR. TAULBEE: Okay, so you want us to focus on the claims where we have neptunium bioassay data, and then I'd compare them to the current coworker model. Correct?

MR. BARTON: I think that would be beneficial. I don't know how cumbersome that would become if you have you know, if you have a very large subset that had both the urinalysis and whole body counts, you know, maybe we can focus on that.

But I mean I don't think we have to necessarily be restricted by it. I think simply comparing your analysis versus the whole coworker model could offer up some decent evidence as well.

DR. TAULBEE: Okay.

ACTING CHAIRMAN CLAWSON: Okay, well I guess my question is, does anybody need to take a break yet? Or do we want to proceed on?

I'm not hearing that anybody wants a break. I guess we'll go on to Finding 12. Is that the, or is that, that's got the neptunium too, so.

MR. FITZGERALD: Joyce, I know you kind of skipped ahead a little bit. I think we're on 12, but maybe you can advise us on that?

DR. LIPSZTEIN: I think we covered everything. Because we were discussing everything at the same time. So I think we covered everything on neptunium. I don't see anything that we didn't. I think that we covered everything.

MR. BARTON: Yes, this is Bob again. I think really that path forward kind of covered the bases on a lot of these neptunium, maybe even all of these neptunium findings.

DR. LIPSZTEIN: Yes.

MR. BARTON: Insofar as we have to wait to see what that plutonium ratio analysis will bring, but you know, most of these findings are technical concerns that may be obviated by the path forward that we've just chosen.

DR. LIPSZTEIN: Yes, we discussed everything. Because we started discussing and we went all along, all the findings, so.

MR. FITZGERALD: Okay, as far as the other SC&A folks on the phone, before we move on from neptunium to thorium, anything else that needs to be said?

Okay, Brad, I think we can --

DR. MAKHIJANI: Yes. Hi, this is Arjun. I think, I don't know how we kind of threw thorium in there also. I thought we had moving on from neptunium there are quite a lot of thorium specific issues.

I don't know that we discussed them all in preparation for this call. And I didn't see anything from Joyce, going --

DR. LIPSZTEIN: No, no I thought we were finished with neptunium, not thorium.

MR. FITZGERALD: No, we're talking about --

DR. MAKHIJANI: -- there a lot of thorium related issues that are very specific to thorium. And that will not be covered by this ratio approach, because --

DR. LIPSZTEIN: Yes.

DR. MAKHIJANI: -- thorium dose reconstruction is proposed along completely different lines. At least after 1990 or 1989.

MR. FITZGERALD: Arjun, we are about to get into thorium.

DR. MAKHIJANI: Oh, we're about to, sorry. I misunderstood you.

MR. FITZGERALD: Yes. No, I'm just trying to make sure we can close out the neptunium findings.

DR. MAKHIJANI: Oh, okay.

MR. FITZGERALD: But I'm hearing that --

DR. MAKHIJANI: I misunderstood.

MR. FITZGERALD: -- everybody feels satisfied that the path forward pretty much envelopes those issues and we can just move on to thorium now.

DR. MAKHIJANI: Okay, I'm fine. Yes.

MR. FITZGERALD: All right.

MEMBER LOCKEY: Hey, Ted, Ted Katz.

MR. KATZ: Yes.

MEMBER LOCKEY: This is Jim Lockey, I was able to join you now.

MR. KATZ: Okay, thank you, Jim. Thanks for signing up, signing in. And Jim, for the record has no conflict, correct?

MEMBER LOCKEY: That's correct.

MR. KATZ: Okay, carrying on. Yes, I guess Joe you can move into thorium business now.

MR. FITZGERALD: Okay, yes. And Tim was correct that we got as far as I believe through Findings 3. I guess we didn't quite

get into all of Finding 4. We did touch on thoron a little bit.

What I'd like to do because it has been several weeks just for continuity sake. Bob Barton, could you, you were on the February 5th call as well, could you recap where SC&A stands, starting with Finding 1 and 2? Just to make sure that as we move forward that, that doesn't get lost.

MR. BARTON: Sure Joe. Finding 1 essentially regarded the source term for thorium at SRS. Originally in our review of Addendum 3, we felt that maybe the focus had been too narrow, perhaps only focusing the coworker model to assigning doses in the 773-A Building.

Now since then NIOSH has revised OTIB-81 which is their, I guess you call it the, General Coworker Document, which contains the coworker models for you know, uranium and plutonium, as well as the intake values for thorium and neptunium.

And I think that's maybe where to start, with Finding 1 because that's really the name of the game there, as far as who are you going to be assigning these coworker intakes to?

And are you covering the correct locations? And so maybe the best thing to do, I've never used Live Meeting before, but let me see if I can get OTIB-81 up there so that everyone can see it. Let's see.

DR. ARNO: Which revision of OTIB-81 are you trying to post?

MR. BARTON: This would be Revision 2, it was released this past December. Let me see if this works.

Can anybody see a change there? It's just at Table 5-1.

DR. TAULBEE: Yes, I can see it.

MR. BARTON: Okay, so that worked. Okay, so here is essentially in my mind the real road map to how these different coworker models are to be applied. And that includes the

neptunium and thorium coworker models that we're discussing today.

And what you can see here, is in this left column, we have a list of facilities at the SRS site. And then columns 2 through 5 we have different time periods.

Obviously these last three refer to the SEC period so they're kind of more pertinent to today's discussion.

And then the final column you have here are the radionuclides of concern. Essentially the radionuclides that you're going to assign to these different areas.

And obviously those time periods in the middle of this table you see for instance, if you had a dosimeter code in the 1973 to 1990 of 1C through 6C, then you would essentially be assigned to the reactor areas.

And you would be assigned to warrant a tritium and fission products coworker intake.

Now one thing I want to note, that it was very difficult and almost bordering on

impossible for SC&A to actually go in and verify the accuracy of these codes as they're applied simply because they're not really annotated or cited.

So, I mean to do it, during the February 5th meeting it sounded, or my impression was, that this table was essentially a conglomeration of different resources.

Be it operating procedures, other types of reports, interviews perhaps and maybe even just some experience working with the SRS claimant files or documents.

So we can't really comment on whether 1C, you know, does that actually refer to the reactors? I'm sure it does.

But I mean it would be nice if NIOSH could pull together sort of a reference list or you know, annotate these different dosimeter codes so we can kind of see how they arrived at these different assignments.

Because like I said, this kind of is the name of the game of who you're going to

assign these different coworker models to.

So I don't know if that's something that can be pulled together rather quickly, but it would certainly help us in reviewing this table. I don't know if NIOSH really wants to comment on that?

DR. TAULBEE: Yes, this is Tim. We have from the various reports and other health physicist files from the Savannah River, we have compiled a history if you will, of these dosimeter codes over time.

We have a breakdown of them from 1959. We have the breakdown from 1972 into 1973 where they list the old codes and then, the new codes. And then we have additional documents later in time for these codes, from 1977, 1984, and then 1991 time frame change as well.

MR. BARTON: Okay. It would be very helpful to us if you know, could pull together I guess a list of SRDB numbers, or whatever form sort of those references are in.

I assume they're annotated somewhere. That would be very helpful for us if you could pull that together.

But beyond that, a couple of concerns that we noticed with this table. Essentially what we, went in and we just pulled you know, a handful of claimant files.

And said, all right, let's see what these dosimeter codes actually look like in the actual claimant files. How would this table actually apply to the different claimants?

And one of the concerns we have and this may be obviated once we get to look at the sort of the source of all this, is you see there are numerous dosimeter codes for some of these areas.

I mean for example, let's scroll down here a little bit. Central shops, I mean there's a ton of different codes.

So that kind of gave me pause and there's never really, not really a clear pattern in my mind as to why all these different

numbers and letters would refer to the same location?

And beyond that, you'll notice that as you inspect this table, and let's see if I can find a good example here. All right for example let's take the code 5F here.

Code 5F could refer to the unknown facility. F-Area, A-Line, which if you look at F canyon again 5F is there. The 221-F, B-Line and 5F. The plutonium field fabrication and experimental facility, and the 235 Vault.

So that's essentially six different areas that could be assigned based on a similar dosimeter code. And if you look at those six different areas, they all have different sort of mixes of radionuclides that you could assign.

So I guess that's one question we had, is how this table would apply in that kind of situation?

I would assume that in that situation you would essentially apply the full

mix, essentially every radionuclide that appears in those six areas, you have to assume they could have been exposed to. Am I interpreting that correctly?

DR. ARNO: Need to clarify that a little bit, this table is an aide to the dose reconstructor. The dose reconstructor is going to have other information from the claimant's DOE, and DOL files as well as the record of a telephone interview.

So they will use data from all of those sources to assign the individual to a work location. And then base the intakes off that work location.

MR. BARTON: Okay, I understand that. As far as location information goes, one thing that would kind of give me pause, I mean you mentioned the CATI report which is a very useful tool to the dose reconstructor.

But really when I look at this table and to kind of emphasis my point, I'm going to scroll down here to the very bottom where we

have the, not identifiable or unknown facility.

So in a situation where you really can't establish where they are, you essentially are assigning the entire mix of coworker radionuclides.

So when I look at the table what I really see is sort of a, I like to call it a table of exclusion. Because if you can find, you know, establish a worker in a specific location, you know, they get whatever mix of radionuclides is there. But I mean if you can't, you're getting the full mix.

So really what we're saying is if you have a dosimeter code that appears in this table, you might get less of the coworker intakes than if you did not have a dosimeter code that was in this table, or we don't know what the code meant, or there was just no, the dosimeter code was blank.

So a lot of these concerns relate to that uncertainty of how you're going to actually apply this table to a claimant. And

I wanted to say that's an implementation issue.

And what I want to get into next is we did in fact observe gaps in the dosimeter codes in the few claimant files we did look at. This was especially true in the 1973 to 1981 period --

DR. TAULBEE: Bob, can I --

MR. BARTON: -- when the primary source is the HPRED database.

DR. TAULBEE: Bob, can we --

MR. BARTON: -- and from what we observe there just simply aren't dosimeter codes included there. I mean the field is there, but it's blank.

DR. TAULBEE: Bob.

MR. BARTON: Now we also observed this to a -- go ahead.

DR. TAULBEE: Bob, this is Tim. Let's first focus on the first part here because there's some other things I wanted to mention here about this table, to expand upon that. And then we can go into the issue that you have

identified with HPRED, okay? Is that all right?

MR. BARTON: Sure.

DR. TAULBEE: Okay. The use of this table as Matt was pointing out, is just one part of the tools for the dose reconstructor. In many cases for people like working in the F-area, you'll notice that plutonium is listed there for, as part of the mix.

Well most of the people that worked in that particular facility, actually had plutonium bioassay as well. So we wouldn't be applying the coworker model to them.

So one thing to keep in mind, this is a case where we have a person who is monitored. We have a dosimeter code so we know that they worked in, in the case of 5F for example, in that '73 to 1990 time period.

They could have worked in the F Canyon, they could have worked on the FB-Line, they could have worked on 235-F in the vault area, or they could have worked in the 235-PuFF

facility.

If they don't have any bioassay, then yes, we will apply a plutonium coworker model, or uranium coworker model, a neptunium coworker model, an americium, curium, californium, thorium coworker model.

So that's the use of this particular table. Is to help the dose reconstructor if they have no other information about where this person worked. They'll look at that dosimeter code and then assign based upon their particular scenario. So that's how this table is being used.

One other thing I'd like to point out is that the time periods are critical for evaluating this. Because the codes were reused in other time periods and it means a totally different facility.

So you really have to look at the dosimeter code at that particular time period of when that measurement was, to make this determination of which facility, and then which

coworker model. Does that make sense?

MR. BARTON: Sure, I understand. And to point out that example that I gave of one dosimeter code for six different facilities, that's just for a single time period.

I'm not trying to compare dosimeter codes across these different time periods that are established here.

But I guess the second thing is the point was made that there would be other location information that could be used to establish where the worker was.

Well in my experience, the only other really location information that we have is based on internal dosimetry. Now there's the CATI report.

But are we going to start using the CATI report to apply to this table to sort of, and again you'd be using that CATI report to exclude claimants from being assigned certain intakes. You see what I'm saying? I mean --

DR. TAULBEE: Not to exclude on

the, it's more to get a feel for where this person worked and were they exposed to this particular material in that time period?

MR. BARTON: Right, but if you cannot not establish with reasonable accuracy for a certain time period where that person was, it essentially falls into that last bin in the table where you're getting assigned everything.

DR. TAULBEE: That's correct.

MR. BARTON: Right, so I mean this is all about when you have gaps in those dosimetry codes, you know, how do you deal with that uncertainty?

And when you don't quite know where they were. And these instances can span many years, even in the SEC period where you don't know where they were. They would have to fall into that bin.

Now I mean, am I hearing that when you have gaps like that, you would look at a CATI report and use that information to try to place

them into one of these categories?

DR. TAULBEE: Well no, not necessarily. I mean CATI is just one piece. We wouldn't use that exclusively. We would use the whole weight of evidence effectively to place where this person conducted their work.

So there isn't one piece that's used. Unless it's the only piece that we have, in which case, yes, they generally go into that final category.

And this is where it gets into what we call claimant-favorability. Of we've got equal evidence of they either worked in the one area versus another, we don't know. And so we assign a claimant-favorable approach which would be all of the coworker.

MR. BARTON: No, I understand and I completely agree when you don't know, you have to assign all of the coworker.

I guess my point was, based on our very limited, again, you know, hand full, less than 10 claimants we looked at, it looked like

to me on the full weight of evidence.

Now again, I don't think you can look at internal dosimetry files and say, oh well, we're going to use those to fill in gaps for coworker intakes because this is the whole point, they don't have the internal dosimetry files.

The CATI report I don't think can be used to exclude a worker, that's essentially, since, if you don't know where they are, they're getting the full work up.

If you use a CATI report to fit them into one of these categories, what you're essentially doing is going to be taking away an intake from a certain radionuclide based on what Table 5-1 prescribes. You see what I'm saying there?

DR. TAULBEE: I think so, but let me give you an example of let's say a CATI report says that they were a forester. And was working in the outside areas of the site.

We wouldn't necessarily assign the

full coworker model to this particular person. And if we don't have any monitoring data, well they weren't expected to have been monitored.

MR. BARTON: I understand that, that kind of situation. Or if the CATI report said they only worked in an administrative building that was across the street and they never had to enter the plant. That's kind of the special cases.

But I'm talking about when you have radiological workers who were badged, but you see their badging records don't contain those area codes. Well now you're left with, you know, you're left with putting them in the not identifiable column, and giving them the full work up.

And again, our concerns revolve around really the potential for a great many number of workers who need the coworker model applied are going to fall into that bin.

DR. TAULBEE: Okay, this is a perfect segue into your next question, where

you noticed that in HPRED sometimes the area is not listed. One of the things in the memo that was stated here, is that the primary source of external dosimetry information is the HPRED data base.

I don't consider that to be the primary source of dosimetry information. That was a database that's been created based upon quarterly dosimetry reports and logs. And a series of tapes that they've rolled in together.

If you go and look at the quarterly dosimetry reports, the area information is listed there. So even though you found some of these I guess, blanks from the claimant files.

And it's, it may not be in the claimant file, but if you go to the quarterly dosimetry reports, you can find which area they worked in.

I did a case, I helped a dose reconstructor review a case earlier this week where that was the case. The area wasn't

listed there.

We went to the quarterly reports and it was listed there for every single quarter that they were exposed, that particular area. And we were able to identify that.

So if you've identified these gaps, let's work together on them, and I'll show you where these quarterly reports are and how to use them. And you can go through and you can find which areas people worked in.

MR. BARTON: I'm completely with you Tim. I understand the quarterly reports. I would say that in my observation, sometimes that it's the case like you just said. The quarterly reports are all there. They're log books with many other workers, and you could use that to fill in the gaps.

But in some cases all you have in that time period is the HPRED. And we've, that's all that's there currently in the NOCTS database. Now I don't know if you have those additional log books somewhere else?

If they don't appear all the time, in NOCTS, for your claimants, but I no, I did look at the full file. I didn't just look at the first readouts of dosimetry data.

A lot of times those files were missing.

DR. TAULBEE: Yes, we have --

MR. BARTON: Again this is a very limited sampling.

DR. TAULBEE: We do have a complete set of those quarterly reports. Where, what you're seeing in the files where there is additional files from NOCTS are added, is where we went through and did a, oh gee, what's it called? Word identification, I'm missing a term there.

Oh, OCR, Optical Character Recognition, and for the people that we identified who were NOCTS claimants, we pulled out those pages and put them into the claimant files.

There are some of the forms that it

didn't pick up the person's name. The letters were too close together. But we do have the complete set of those forms. So I can show you where they're at in the SRDB, and provide you that information so you can do some of this additional digging if you want as well.

MR. BARTON: Okay, so what I'm hearing is you're saying that, you know, where I saw there were gaps in the actual quarterly log books, that, that information is all in the SRDB.

And so if you had to apply this coworker model, you could easily go in and pull those additional log books to fill in some of these gaps.

DR. TAULBEE: That is correct.

MR. BARTON: Okay, well that certainly alleviates a lot of the missing data, I'll call it, as far as what we witnessed in the claimant files.

One other thing, now let me ask you, it seemed to me that when I was looking even at

the quarterlies, that what's reported there was only if there was a positive dose reported. So I assume the full data set would have those zero records too?

DR. TAULBEE: That is correct.

MR. BARTON: Okay, again that's the --

DR. TAULBEE: That is the case of the claim I was working on earlier this week. Is they had zero dose and so that the file actually only showed the last one, that illustrated they had zero dose.

But if you went back to each of the other quarters, in the previous year and a half, you could find the location for all of the zero doses as well.

MR. BARTON: Okay, that's really, really good to know that, that information is out there to fill in those gaps.

I guess the last concern I just want to point out, is what we also noticed. And this wasn't necessarily a specific claimant record,

but that the dosimeter codes in Table 5-1, do not appear to be complete.

In that there are, we identified, you know, over 20 codes that just don't appear, you know, in this table. And I don't know how many more there are, but you know, when you look at this table you see there kind of, you know, sort of put in sequence.

For example the 7 series, I'll call it. So 7A, 7B, 7C in 1973 to 1990, you know, I mean if you were going to fill in sort of the alphabet there, we'd certainly observe some of the ones that weren't included in this table. But again that was only in a limited claimant sampling.

So one concern is that the actual dosimeter codes as you can use them to apply to a certain location don't appear to be complete, and there may be very many, significantly more dosimeter codes that would again fall into that not identifiable bin.

DR. TAULBEE: Can you give me an

example of some of the codes that you've observed that may not fall in there?

MR. BARTON: Sure. Let's see here. Here's the 7 examples. So all I did here was just list them straight. Can everybody see this? See the Excel file?

DR. TAULBEE: Actually I can't.

MR. BARTON: Okay, well I'll guess I'm off of Live Meeting, but anyway for example 7A and 7B were associated with the Central shops in Table 5-1. 7C was not observed in the table at all.

7D and 7E weren't in the Table, but we observed them in the claimant files. 7F and 7G, you know, it goes on like that. That sort of thing.

So I mean it appeared to us based on a very limited sampling that there may be very many codes out there that haven't been established or associated with a particular facility.

I can certainly put out a list of

what we've found so far, but again that's only from a handful. We really don't know quantitatively how many more might be out there.

Did I lose everybody?

ACTING CHAIRMAN CLAWSON: No. This is Brad. I'm just trying to follow you, and I understand where you're going on that, Bob.

And I just want to put one thing out to Tim on this, because, being from the Dose Reconstruction Work Group, I hope that when the dose reconstructor, and I understand that they use several different items here, I hope that he's making it known how this information would come.

Because this would be very, very hard for our side to be able to review something like this because I think he could get information from so many different areas.

DR. TAULBEE: Okay, yes. I guess --

DR. ARNO: Tim, this is Matt, I mean, is there a way for --

DR. TAULBEE: Let me see if I can't pull up one of these TLD badge code location documents to show you what it is that I'm looking at here, Bob. Maybe this will help some.

MR. BARTON: Sure.

DR. TAULBEE: Bear with me here as I try -- actually, Ted, or somebody can you help me in using Live Meeting? How do I select me as the document person?

MR. KATZ: Tim, you don't have to select yourself. You just need to go to content and share something and you'll take over.

DR. TAULBEE: Okay.

DR. ARNO: This process is very similar to what's done in Hanford quite frequently in applying coworker intakes at that facility. Hanford is very similar to Savannah River in terms of the activities going on and

things of that nature.

There is information in the external dosimetry files that provide where a worker was located. It is quite common for workers to be assigned, basically, the coworker intakes for all the radionuclides. Either because the person -- there's good information on why that person is not known, or simply as an overestimate method if the claimant's, you know, not of the verge of going compensable.

This is not that different from what's done at Hanford and other sites.

DR. TAULBEE: Okay, I'm just going to try and share my desktop here for a minute. Can you all see the TLD badge coded location document?

MR. KATZ: Yes, Tim.

DR. TAULBEE: There you will see -- and this is probably where you're seeing other codes, Bob, that you know are not listed there in the table. And you'll notice many of these 7s are from the A Area. Being there's 7-19,

7-20, 7-22 type of scenario. I think this is what you're talking about. And you'll see this is the HP location from the 1977 version that has this.

The previous one, this would be 1959. And off to the left is the HP Area codes listed there. If I scroll down here, you'll see that these are the department codes associated with the April 1977 version.

And, you know, here, if you look at an HP Area code in a claimant file and it says -- oh, let me pull up F, here. If I can find it, 200-F. Yes, the 200-F, the code would be 2F, for example. And then if they were working in, say, 235 Building or PuFF, the department code would be 205.

And so this is the 1977 version. Here is the 1984 version. And you can see that some of these codes, like 9F, for example, aren't listed in our table, I'm sure because it's a firehouse.

And so not all of the codes, you are

correct in your observation, are listed there in the Table 5-1, but we do have what locations these are.

And so this is the 1990 version of the old codes and then the new codes. And you can see there's many places where the dosimeter badge racks were held.

MR. BARTON: Now, when you say badge rack, I envision that's when the dosimeter's dropped off. Correct?

DR. TAULBEE: That is correct. That is where they kept their badges at night.

MR. BARTON: So we're going to assume that that's going to be directly associated with the area of work?

DR. TAULBEE: It is for most places except for Central shops, who could have worked pretty much anywhere. Which is why you'll see in Table 5-1 we account for them being able to tasked out of Central shops and going to the canyons, or going to the reactors.

Which is why in Table 5-1 we have

that kind of a large quantity of things there for construction trades workers who were out of Central shops.

MR. BARTON: Okay, I understand that. I guess a follow-on question to what you stated before, in that you kind of gave the example of the firehouse. I mean, is it NIOSH's position that any code that's not in Table 5-1 is really just a non-radiological area? And so, you know, it would be inappropriate to assign a coworker intake to those workers who were, you know, badged in these areas that don't appear in this table?

DR. TAULBEE: It would be one that I would go and look at more closely as to what they did. In some cases, I'm recalling a case from a few years ago, one person was working out at the A Area. He was working out of the power plant, out of the utility.

When we read his CATI, he talked about going to the reactor areas, to the power plant component of that, and he had some

positive dose.

And so, as a result, we would assign tritium and mixed fission products to that individual. Even though he was badged out of another area.

So this is a case where we're not necessarily excluding somebody because of, you know, them being in a non-radiological area where they might have gotten their badge, or where they kept their badge. But we look at all the information in what is said.

MR. BARTON: Well, in that example you said he was badged out of A Area. What potential radionuclides of concern might have been present in A Area?

I mean, if it's more than tritium and plutonium, you're kind of in trouble there because, again, in that specific case, you know, you'd be assigning only those radionuclides when the only basis for saying he was in an A Area was he said in his CATI report he went to the reactor areas.

Do you see where I'm coming from there?

DR. TAULBEE: A little bit, but I guess I'm looking at kind of the whole weight of the evidence associated with this. And, you know, A Area was a big area, okay?

You've got 773 Area, which is the Technical Laboratory, and there we have, you know, that's one of them listed there in Table 5-1.

But across the street from the M Area, there's a whole series of facility services, if you will, that have dosimetry badges associated with them because they could be dispatched to other areas.

And at that point, we begin to look at other scenarios. And, you know, and looking at the dose of the badge as well. We use that as well. If somebody's got some positive dose and they're saying they went to the reactors, and they were in the reactor building.

MR. BARTON: Well, how do you know

they couldn't have been somewhere in the A Area where they could have received that?

I guess what I'm saying here is when you have that sort of uncertainty, you know, obviously we're limited by the information of how accurately we can place workers.

I guess I'm concerned that certain information, such as the CATI report, or the DOL files, which to my knowledge is largely provided by the claimant as well, that in the face of that uncertainty, I would think you would want to sort of give them the benefit of the doubt and assign, you know, as many of the radionuclides that apply to that area.

In the case that you just gave, if for instance there were areas in the A Area where they assign more than you would assign in the reactor areas. You know, if it were me, I would think you would want to give them the benefit of the doubt, that, hey, listen, our evidence says he could have been somewhere in A Area. His CATI says that he went to the

reactor area, so maybe we don't know when, there may not be any temporal specification there.

I guess, you know, for me, I think you'd have to err on the side of the claimant in the face of that kind of uncertainty and inability to always pin someone down to a certain location.

DR. TAULBEE: I think this is a case of a dose reconstruction type of review. And not really an SEC from this standpoint as to how this goes. This is a general method. There's going to be different cases for every, you know, every single scenario. I can't give you a blanket, yes, we'll always assign plutonium here. I can't do that.

MR. BARTON: No, I understand you can't, but I think, you know, a document like this, which is essentially giving instructions to the dose reconstructor, could tell the dose reconstructor, you know, I mean, it wouldn't have to say every single case, but, you know, when there is doubt or, you know, uncertainty

in where they were, you sort of have to err on the side of, well, they could have been in one of these really nastier areas. And maybe it's appropriate to assign all of the radionuclides to that area.

And, you know, I hear you that that's sort of a dose reconstruction review standpoint. But in my mind this is the sort of thing that should be included as specific instructions to the dose reconstructor and not leave it sort of up in the air for an individual to make that call.

I mean, instructions don't have to be, if you see this code, then, you know, you'll assign this, this, and this. It could be you see this code, then apply it to multiple facilities and give them the benefit of the doubt. Give them whatever mix is the combination of those two facilities.

DR. TAULBEE: I believe we do that. And I don't -- when there is doubt, significant doubt, and we believe this person was exposed

to the different materials, we do assign the full complement there, if we don't know.

But there are scenarios where somebody was a secretary, as I was saying, where we would not assign the full complement even though, you know, there is a dosimeter issue from one of these other administrative or service facility areas.

MR. BARTON: And I certainly wouldn't want to argue that. I know there are situations where you can clearly delineate who was a radiological worker and where they might have gone. And would have been severely limited in which locations, you know, they could have gone. I certainly agree with that example. I'm talking more the uncertainty in a radiological worker.

And I guess it also becomes sort of a judgment call. And you say if there is significant enough uncertainty then you assign the full compliment.

Well, what is significant enough

uncertainty? I mean, is it -- yeah, that comes down to a judgment call. I mean, it seems like the temporal gap issue may be rather moot. I'd certainly like to look at that a little more.

But when you have uncertainty, say, you know, with the dosimeter code, if it turns out we really just don't know what that dosimeter code is, you know, I think then that would fall into sort of that last bin of, you know, if you really can't pin them down, you kind of have to assume they were in the worst areas.

MR. KATZ: Bob, this is Ted. I just think -- I mean, I think you've made your point very clearly. And I think Tim has made his point clearly too. And it is a dose reconstruction issue. But I don't think there's much to be gained by beating this thing to death at this point.

I mean, I think, you know, if there are comments about how their dose reconstruction methods should be -- how

specific they should be, I think that can be addressed further in writing, but, I mean, it's very clear and we're not getting anywhere.

MR. BARTON: Okay, I guess the only thing I'd add to that that could potentially be a SEC issue would be the plausibility aspect of it.

I mean, if it turns out if you did sort of a study and you found, you know, all the workers that require a coworker intake. Well, if there's so much uncertainty where they were, you have to apply, for instance, the thorium to them.

That could potentially be a plausibility issue since we know thorium operation were restricted to a small population. That's the only comment I would make that could still sort of be in that SEC arena.

But I agree the rest of it, as far as instructions to the dose reconstructor, is really I guess a Site Profile issue on the

implementation.

But if it turns out you'd have to apply thorium to, you know, thousands of workers at the site, and, you know, we know that's clearly not plausible.

I mean, one could question that, and of course it would be a judgment call for the Board, but I guess, in an SEC context, that's the only thing I can say about it.

MR. FITZGERALD: I guess, listening to this, I sort of had the same sense that the only way you -- this is almost an empirical thing. The only way you would really know whether the instructions, judgments and the information that was available to the dose reconstructor enabled the assignment based on location would be almost a survey of what's been done already.

I mean, there's no way of knowing if in fact the information's sufficient to make those assignments without just simply knowing, you know, what the experience has been to date.

I mean, Tim, is there any way to know that based on the dose reconstructions that have been done? Whether or not, you know, there's been any problems?

I mean, if it in fact always goes -- or often goes to default, which means you assign everything, then that would raise some questions about the plausibility along the lines of what Bob said.

But if it turns out to be the exception, then I would say it's probably not an issue.

DR. TAULBEE: This is Tim. This is going to sound really interesting -- interesting at least to me -- in my response here. We actually have not used OTIB-81 yet. And the reason is, is the SEC is open still.

And so the way that this is planned to be applied is once the SEC is closed, and if OTIB-81 is agreed to by the Work Group and the Board from that standpoint, then we will implement it. And we'll go back and re-look at

all of the -- we'll do a large Program Evaluation Report and re-look at all SRS claims.

We've not done that yet because we don't know for sure that you all are in an agreement for us to even do dose reconstruction effectively at Savannah River for this whole time period.

The two radionuclides that I can see us going back and making changes to a significant fraction of dose reconstructions are to thorium in the earlier years, and then the thorium from 773-A in the '74 to '89 type of time frame.

And then neptunium as well. Neptunium is probably going to be one of the coworker models we end up applying to a lot of people, especially from the 200-F Area.

Other than that, virtually every other claim that we have for radiological workers, we have monitoring data for them. So we have internal monitoring data.

If you recall back when we did this, the initial presentation on the Evaluation Report, almost 80 percent of the SRS claimants have some internal monitoring data as part of their NOCTS file.

So those bioassay control procedures I was showing you on February 5th were implemented. So we don't need a coworker model, we have their individual plutonium bioassay.

The people in 773-A, from the chemistry department, from the high-level caves, we have americium, curium, californium, thorium data. So we wouldn't be applying this coworker model to them. We will use their data.

MR. FITZGERALD: And Tim, that's helpful because I think, Bob, just to clarify, it sounds like it comes down to whether the operational and the facility-specific location information is sufficient for thorium/neptunium. And of course I know

that's a lot of what the data capture's been about for the last six months.

But doesn't it come down to that? Whether there's sufficient information to know what facilities and to tie those facilities to workers?

I think what you were coming to is, on some of the construction workers where perhaps there is no specific tie in, they would by default get the thorium/neptunium, and if that's a large population then you're raising that question of plausibility.

But it sounds like it comes down to whether the information where the operations were and tying that information to the workers is the bottom line.

MR. BARTON: Yeah, I agree with that. It's an issue of how sufficient is this information for tying workers into individual areas. And when you go to do that, is it plausible that, for example, all these workers were exposed to thorium?

And it sounds like from, Tim's comments, is maybe that would be difficult to do since they haven't actually applied this method yet.

I guess one way to wrap your head around it would be to do sort of a pilot study, a claimant sampling of more than just the handful that we looked at.

And also I'd like to add on to that, Tim's additional information on what records are out there that aren't currently in a NOCTS file goes a great deal to alleviating the concerns we had that you would really just have a lot of workers falling into that you-don't-know-where-they-are bin.

So, I mean, perhaps it's something worth looking into just so, you know, we could see where these extra files are, and that, yes, they are complete. And, yes, if need be we could always pull those records and know where the worker was. That's very important.

And I guess the other side of that

is, I mean, I'd personally like to take a look at some of these records that show what codes apply to which area. And see how some of the ones we've identified that aren't in Table 5-1, you know, what areas did they actually refer to? And if it turns out that, yeah, they referred to non-radiological areas, that's another very powerful piece of evidence.

DR. LIPSZTEIN: May I speak? May I say something? I think that also you say that you have a lot of people with data on thorium, americium and plutonium. It's true you have a lot of data on plutonium and americium or curium. But you don't have the data really on thorium.

What you have is a method where you were trying to apply the results from americium and curium to thorium because you say they were expected together. But you have to know to whom applied those data.

Because there were many people that were not exposed to thorium, but were exposed

to americium and curium. So you have really to know exactly to whom applied those data to thorium.

DR. TAULBEE: Joyce, this is Tim. That is not correct, in that the americium, curium, californium, thorium bioassay, urine bioassay method, is a gross alpha count. Okay? That is what it is. It incorporates all four of those radionuclides.

DR. LIPSZTEIN: I agree with you. Okay.

DR. TAULBEE: I'm sorry?

DR. LIPSZTEIN: I agree with you in that.

DR. TAULBEE: Okay, so now you've got workers in 773-A in the 1972 through 1989 time period that were working in individual labs. Some of them were working with americium. Some were working with curium. Some were working with thorium. Some were working with all three of them. Some were working with plutonium.

And so we're taking that gross alpha count for that worker, that americium-curium-californium-thorium count, and we are estimating the dose based upon what their cancer was, which is the most claimant-favorable to them. What would result in the highest dose?

So we're not trying to distinguish whether they were an americium worker, or a curium worker, or just a thorium worker, or whether they worked with all of them, or plutonium.

We are -- I shouldn't have confused it there with plutonium, but of those count, or of those workers, we're going to just simply assign the one that results in the highest dose that's the most claimant-favorable.

This again gets back to that approach of equal evidence. If we know somebody just worked with americium, then we're going to assign the americium. If we know somebody worked with just thorium, we're going

to assign the thorium. If we don't know, we're going to assign the most claimant-favorable.

DR. LIPSZTEIN: I understand, but the problem is that sometimes you have -- when you use the coworker model and use it for thorium, then you have -- even for one person, if you don't know where the person works, and if you make the assumption that he worked with Type S thorium, for example. And instead he didn't work with thorium but he worked with Type M americium, you wind up calculating a dose that is almost 100 times higher than what he really was. It is claimant-favorable, but is it scientifically okay to do this?

DR. TAULBEE: I think it is from the claimant-favorability standpoint of equal evidence and the unknown. In our federal regulations, we talk about that when there is a benefit of -- or when there is doubt as to what the exposure was, we will give the benefit to the claimant.

And that is the perfect example that

you just gave. Yes, somebody worked with americium, we didn't know that, they could have worked with thorium, they could have worked with curium.

We went ahead and assumed, based upon their cancer and which radionuclide concentrates there, we assigned the highest and most claimant-favorable. We gave that benefit of the doubt of the dose estimate to that worker.

If we have evidence that says they didn't work with thorium, they worked with americium, we'll use the americium. But that changes, all things being equal, giving the benefit of the doubt.

DR. LIPSZTEIN: So you think that assigning a dose 100 times higher than he really had, it's okay?

DR. TAULBEE: I do think it's okay, if we don't know whether they worked with thorium or americium. Yes. If I can't tell whether they worked with one or the other, then

yes. I'm okay with assigning that higher dose.

DR. LIPSZTEIN: Yeah, because you know that americium is tight. It's like, for example, if you assign for the same urine excretion, it's not only a 100 times, more than a 100 times.

The same excretion results for americium would result in a lung dose of .25 rem. If you assigned that to thorium Type S, it's 80 rem. So that's a huge difference. I don't think --

(Simultaneous speaking.)

DR. LIPSZTEIN: -- it's scientifically. Yes, okay. I'm hearing.

DR. NETON: I can give you multiple examples of where this type of issue resides in our program. I mean, we, for example, if a person is monitored for 20 years for plutonium and never had a positive plutonium result, we're going to assume that person was exposed and they were excreting uranium, or plutonium, equal to one half the MDA for their entire

career.

I mean, and the fact may be that the person never inhaled one atom of plutonium. So, you know, I understand what you're saying, that these values can vary, but the fact is if you don't know, you don't know.

I mean, you have to make some type of an assumption. And when we do, we err on the side of the claimant.

(Simultaneous speaking.)

DR. LIPSZTEIN: I've been working with thorium for a long time and I know that if a worker is exposed to a Type S thorium, it's very, very, very difficult to see something in urine.

So if you have something in urine, of course with the extraction method of the time, of course not with using the other methods that we use today. But using gross alpha, or even alpha spectrometry, you have to have a very, very, very high exposure to thorium Type S to see something in urine.

(Simultaneous speaking.)

DR. LIPSZTEIN: So, no, what I'm saying is that if you assign those -- if you see something in urine and you assign it to Type S thorium exposure, it's going to be unrealistically high because it's very, very, very rare that someone exposed to thorium Type S you would see something in urine using alpha, total alpha or alpha spectrometry.

(Simultaneous speaking.)

DR. LIPSZTEIN: But I'll accept what you --

DR. NETON: And Tim has done the calculations and they're not that different than what you see with other actinides for Type S.

DR. LIPSZTEIN: Oh, yes.

DR. NETON: The actinides are higher, for example --

DR. LIPSZTEIN: Like plutonium, but not americium Type M. So you'll see a lot more in urine. But if you are okay with this,

I think it's scientifically, for me, that I work with thorium for my whole life, so I think through --

(Simultaneous speaking.)

DR. LIPSZTEIN: -- and it's very weird to have a high dose of thorium, but anyway, it's okay. I understand what you are saying.

MR. FITZGERALD: Yes, this is Jim, let me interject. I think this is one of those issues that I referenced in my little note a couple days ago, that we -- and I'll defer to Brad and the Work Group, but perhaps we owe NIOSH and the Work Group a somewhat more detailed treatment of this. Just to make sure that, you know, these questions are unpacked a little bit more.

And maybe as part of that, as Bob was pointing out, propose a path forward, whether it be a little bit of a sampling or whatever for the Work Group to consider.

I don't think we're going to resolve

anything today on this issue, but I think we do owe that to the Work Group. Is that reasonable, Brad, Ted?

ACTING CHAIRMAN CLAWSON: I think so. I actually have a more specific -- I'd like to see something in writing myself, actually, so that NIOSH has something to be able to respond to, which is correct, and what our exact issues are.

We can debate this for hours if we wanted, but I'd rather get on to some other things. If we're not going to be -- I can see both sides on this. And I think that would be positive to do, Joe, thanks.

MR. FITZGERALD: And I think, as NIOSH has pointed out, we ought to take a look at the standard practice at other sites, like Hanford, and compare that with the methodology here.

Because I think, in fact, if it's standard operating procedure, or standard practice, then, you know, then we ought to be

fully aware of that in this context. Bob, is that reasonable?

MR. BARTON: Yeah, Joe, that sounds good to me. I'd also add that, you know, coming into today I had significantly more concerns. Now Tim has presented some information, such as the additional data sources, which will, you know, greatly go to alleviate that.

And I'd like to work with Tim to sort of see if we can fill in some of these gaps. And, you know, just resolve that, yeah, we're comfortable that we can place people where they should be for the purposes of assigning coworker.

And I think taking a closer look at the claimants is the way to do that.

MR. FITZGERALD: So, Brad, I would --

DR. MAKHIJANI: This is Arjun. Could I say a couple of things? Hello, can you hear me?

ACTING CHAIRMAN CLAWSON: Yes,

Arjun. Yes, Arjun, go ahead.

DR. MAKHIJANI: Just a couple of things. I wanted to respond to a comment that Jim Neton made earlier about plutonium and using MDA over two.

Presumably people were monitored for plutonium because they were in plutonium areas. And using MDA over two when you get a less than detectable result seems a very reasonable thing to do, because there is nowhere else to place that. You could use a distribution, but more or less the same thing.

I think this thorium, using americium for thorium when you get more than two orders of magnitude difference in the dose is a completely different thing. And as I understand the situation, the dose estimates have to be scientifically reasonable.

And if you don't know and are simply assigning thorium instead of americium and you have an uncertainty of more than two orders of magnitude, I think this is -- I mean, maybe this

is a question that the SEC Work Group should take up, as to whether a two orders of magnitude or a factor of 300 uncertainty is a reasonable thing to do.

DR. ARNO: This two orders of magnitude is very common at other sites as well. Especially when you're dealing with gross alpha data. And when you're dealing with any nuclides that have solubility that vary from F to S, two order of magnitude difference in doses.

DR. LIPSZTEIN: This time you are going to much more than two orders of magnitude, it's from .25 to 80. And between Type M and Type S is about 40 times thorium.

But if you go to americium, then it's -- because it's 80, then two for Type M, and then .25 for americium, the dose's difference.

My biggest problem is that I don't really believe that data above the detection limit could be from thorium without people

knowing that the worker was really heavily exposed to thorium Type S.

But that's a problem, that's a problem with thorium monitoring. It's in every place, that's the same problem.

DR. ARNO: Yes, I mean, if you compare a Type F plutonium dose to the --

DR. LIPSZTEIN: No, Type M and Type S. No, there is no thorium.

DR. ARNO: But you said two orders of magnitude is unusual, and I'm saying it's not. When you go from plutonium Type F or M, all the way up to Type Super S, you can get three orders of magnitude in the difference in the dose assigned.

Especially when you start talking about the lungs or the thoracic lymph nodes. I mean, the way we apply claimant-favorability in this project results in many orders of magnitude difference in dose depending on what assumptions you make regarding the radionuclide that was in the intake and what

solubility you assign.

MEMBER LOCKEY: Hi, this is Jim Lockey. You know, I think, as a Board Member, our direction is to be claimant-favorable, and the approaches that are being taken are very claimant-friendly and -favorable. I don't have any problems with that. We're not doing a scientific study. These aren't being used to design an epidemiology study, this is health outcomes.

Our directions is to provide a matrix to be claimant-friendly. And I think the approach is appropriate.

ACTING CHAIRMAN CLAWSON: Well, Joe, I'm going to -- and thanks, Dr. Lockey, for your input. I think this whole thing comes down to it is claimant-favorable as long as you can put the information in there and the people can be assigned to the right areas.

But there's some underlying questions here, and as Joe has stated earlier, I believe it would be beneficial for us to be

able to put these issues down in writing so that we know exactly where we're at on it and proceed on from there.

Is there any issue with proceeding on like that? Because I don't think we're going to be able to get this resolved today.

MEMBER LOCKEY: Yeah, I agree, we should put down something in writing so we can look at it again so that we know what the issues are.

ACTING CHAIRMAN CLAWSON: Does that sound all right with -- oh, I'm sorry, I stepped on somebody. Go ahead.

MR. KATZ: Oh no. I was stepping on you, Brad. I'm sorry. I was just going to suggest -- that all sounds good. I was just going to suggest, it's 12:30 now and maybe this is a good place to take a lunch break?

ACTING CHAIRMAN CLAWSON: Oh, I was going to stop for breakfast, but okay. Yeah, if that's all right with everybody, why don't we do that and we'll show back up at 1:30?

MR. KATZ: Yeah, that sounds great. I mean, we can do it shorter if everybody wants to do it shorter, that's fine too. Whatever is good for everyone else is fine with me. Everybody fine with an hour break? Anybody want to shorten that?

DR. TAULBEE: An hour sounds good to me.

MR. KATZ: Okay. Very good, then we'll reconvene at 1:30?

ACTING CHAIRMAN CLAWSON: Okay. Ted, just a quick question. Are you in your office, or would you be by your cell phone?

MR. KATZ: Yeah, cell phone. You want to give me a ring?

ACTING CHAIRMAN CLAWSON: Yeah, I'll call you in just a minute here, okay?

MR. KATZ: Okay, thanks.

ACTING CHAIRMAN CLAWSON: Okay, we'll see everybody back at 1:30. Thank you.

MR. KATZ: Thanks, everybody.

(Whereupon, the above-entitled

matter went off the record at 12:29 p.m. and resumed at 1:33 p.m.)

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

(1:33 P.M.)

MR. KATZ: Okay. So we're ready and we can carry on from where we left off. Joe, maybe you want to lead the way?

MR. FITZGERALD: Yeah, let me recap. I mean, I think we certainly have chewed on this quite a bit. So I think it lends

itself to SC&A providing more specific details in written form on where we're coming from on some of these plausibility questions. And this is, again, directed at Finding 1 on the thorium.

And we were asked to look at 5.1, Table 5-1, in more detail on the February 5th meeting. And so I think, from what we've discussed, it looks like we need to do a little more analysis, and I know Bob wants to look at some of these quarterly reports that Tim was referring to.

And so we need to do more on this, and we'll get something back to the Work Group hopefully within the next several weeks just to put this in more specific form. And maybe identify sort of a path forward to resolving the question, at least as far as any necessary review. So that's what we'd would offer on this first thorium item.

ACTING CHAIRMAN CLAWSON: Does that sound good with -- this is Brad. Does that sound good with the Board Members and also with

NIOSH?

MEMBER LOCKEY: Jim Lockey, that sounds great.

ACTING CHAIRMAN CLAWSON: Okay.

MR. FITZGERALD: I'll write this up very specifically in my notes I'll be circulating by tomorrow.

ACTING CHAIRMAN CLAWSON: Okay. Well, we can proceed on to the next one. I'm just trying to bring up media here so that I can see what the next one is.

Joe, why don't we go -- you've probably got the papers right in front of you. Why don't you go ahead and just go on to the next issue?

MR. FITZGERALD: Well, I think Finding 2, I think NIOSH's response or concerns on thorium after -- what was it, 1970? No, I'm sorry, after 1990, certainly what was provided was, to us, a new approach based on using --

MEMBER SCHOFIELD: Hi, Brad. I'm up there.

MR. FITZGERALD: Hello?

ACTING CHAIRMAN CLAWSON: Okay. Thanks Phil. Go, go ahead.

MR. FITZGERALD: Okay, using a derived air concentration value of two times ten to the minus 13. And using that as a basis for looking at dose estimation after 1990.

And I think what was in the NIOSH response, and Tim can clarify, that's going to be treated perhaps in more detail in a follow up review or report or OTIB or something.

And so what we have right now is what was in the response that was given, but not much more in the way of details or references and what have you.

So I guess that sort of puts us in a position of, if there is going to be a follow up OTIB or report, we probably would want to look at that, rather than try to do a review at this point. We really don't have many of the specifics or the references or anything else.

DR. TAULBEE: This is Tim. That's

correct. I mean, our approach was to first present this to the Work Group and kind of get some feedback as to what questions you have so that we could kind of flesh those out a little more in the reports.

And so we kind of got a little bit of feedback during that February 5th meeting, so we'll draft up a report here of what our approach is and we'll get that out to you all so that you all can chew on it.

Basically it's going to take a lot of this same information that I walked through, kind of the weight of evidence approach that I went through on February 5th, and just formalize, document it, in a sense.

There won't be any new information from that standpoint, that wasn't in the presentation. But it will be more in a written form, so that you can, I guess, if you have questions or develop comments from that standpoint. Does that sound okay to you, Joe, Brad, other Members of the Group?

ACTING CHAIRMAN CLAWSON: That's fine.

MEMBER LOCKEY: Yeah.

MEMBER SCHOFIELD: That's good.

ACTING CHAIRMAN CLAWSON: This is Brad. That sounds good. Mark, did I hear you come on the phone?

(No response.)

ACTING CHAIRMAN CLAWSON: Okay. Yes, that sounds good to us, Tim. And I agree with what you had to say and we'll proceed on from that one.

MR. FITZGERALD: Unless Bob has something else to offer, Bob Barton, I would go to Finding 3, which I think we touched on as well at the --

MR. BARTON: I agree, Joe, I think we have to wait and sort of see the full package before we can comment on the new approach to the post-1990.

MR. FITZGERALD: Yeah, we did have some discussion last time, as we did with

Finding 3. Finding 3, quite frankly, and we had some original issues as to whether or not all the incidents had been identified, and were they in fact available in databases.

And NIOSH has done additional review and has pretty much confirmed this is pretty much what is available. We have not found any evidence of additional information, so that's, you know -- without doing additional site data capture, I guess we would ask the Work Group, how do you want to proceed?

I mean, as far as whether all of the available incidents information had been identified, we don't dispute what NIOSH has done. The original question was whether it was complete enough or not. And that's kind of where we're at, at this point. So I guess we would defer that to the Work Group.

ACTING CHAIRMAN CLAWSON: Well, Joe, help me understand this, I'm trying to think back to when we got into this. It was that they didn't have enough data for this? Or

just run through the issue again. I'm trying to picture what we have here.

MR. FITZGERALD: Well, the question was whether or not, you know, the full body of information available for incidents, as reported to Savannah River that might have involved thorium, whether it was in fact encompassed in the review that NIOSH had done for the Site Profile, and then the SEC.

And in light of further data capture and the discussion we had -- well, actually, the NIOSH response to this particular finding, I guess we've just come to the conclusion we can't identify any additional sources.

And, you know, certainly the data capture's been fairly complete on the point. So what we're saying is without doing any further review, we think, you know, this is it. I mean, I think this is all the information we have at this point.

ACTING CHAIRMAN CLAWSON: Okay. And, Joe, part of the issue on this was getting

you guys into Savannah River and being able to look at some of these documents, wasn't it? This was part of the issue of the problem with Savannah River Site of not being able to get into the data.

And since that time, you have made a trip down to Savannah River, and also has Tim, is that correct?

MR. FITZGERALD: Yeah, we've been a couple times and we've interviewed a number of workers. And that included any instances of unreported incidents. You know, we're relying on the database, the so-called Special Hazards Investigations Database. That's what's referenced in the Addendum 3.

And our question was, how complete is that? There was some evidence that it did not in fact encompass all the kinds of events at the site. It’s just the major ones.

And what we wanted to do was establish whether there were other incidents that may have not got into that database that

had occurred involving thorium.

Now, you know, it's like proving a negative. We have not found anything that suggested that, to date. And we've looked at least twice now onsite.

ACTING CHAIRMAN CLAWSON: Well, you know. Go ahead, I'm sorry.

DR. TAULBEE: This is Tim. If I could just add, this kind of goes back to that discussion that I gave on February 5th of there's a tiered structure to the incident reporting.

The Special Hazards Investigations, and then there was the Facility-Specific Incident Reports which are DPSP reports, or DPST reports. And then there's the Health Physics Logbooks. And then inside the Monthly Works Technical Reports are incidents listed as well.

But the main source from the dose reconstruction standpoint, for us, for incidents, is in the claimant files. And we've

cross-checked to where people who were involved in some of these incidents, at all three stages of that, that I just listed there: Special Hazards, Facility, and then the Health Physics, you see that information within their records, within their individual files within NOCTS.

And so this is -- you know, we went through them, we've looked for thorium incidents, and frankly we really haven't found any in this time period. Mostly because, in the 1972 to 1989 time period, you're dealing with very small quantities of thorium, and there just haven't been any known incidents to this.

MR. FITZGERALD: Okay. And just, again, we did want to talk with some of the identified thorium workers, former thorium workers, going back into the 70s just to more or less validate that. People who actually handled thorium, whether there were events that may or may not have found their way into any of these databases and what not. And we found no

evidence of that. So I just wanted to make sure that was clear to the Work Group.

ACTING CHAIRMAN CLAWSON: Well, you know, we can only do so much. I don't know if I can act for Mark or anything else on this like the Chair, but as far as I can see, if we can't find any more data on this, there is nothing more that we can do.

We've done our due diligence to this. And we've uncovered every stone that we can. So I'd basically say that this one would be closed. Other Board Members can voice their concerns.

MEMBER LOCKEY: Jim Lockey. I would agree with you. You've done everything you can do to find additional data. It's not there, so there's nothing left to do. I think we can close it.

ACTING CHAIRMAN CLAWSON: Okay.

MEMBER SCHOFIELD: I agree, Brad. I think we should close it. I mean, they're just beating a dead horse if they keep looking more.

ACTING CHAIRMAN CLAWSON: Okay, well, with that said, I'd say that this one is closed. And we can continue on to the next one unless anybody, NIOSH or SC&A, has any other issues with that.

Hearing none, I'd -- did somebody go ahead or was somebody trying to speak?

(No response.)

ACTING CHAIRMAN CLAWSON: Okay, with that said, we'll continue on, Joe.

MR. FITZGERALD: I think this is about where we left off on the 5th, which was a discussion on thoron. And I don't -- you know, we sort of segued into a couple other subjects at the same time. So I'm not sure we actually did close out the discussion.

So I want to just open it up to my colleagues, particularly Bob Barton. Is there anything more? I mean, you certainly have the response on thoron, particularly as it relates to the tank farm.

But is there any other questions?

I think we had some clarifying questions regarding whether all the sources of thoron were accounted for.

MR. BARTON: Yeah, Joe. This is Bob Barton. Essentially what we were looking for with this finding was to see if NIOSH, you know, what their plan was. If they intended to address the issue of -- sort of the thoron source term leaking form areas of the site that had significant thorium storage areas.

And one of them that was identified and NIOSH mentions is the Tank Number 15. I think another one might be the Tank Number 12, but, you know, I really don't think we got into it much.

I'd kind of like to hear, because I don't recall actually hearing it, if NIOSH intends to -- what, if anything, they intend to do about potential exposure to thoron?

So I guess I would turn that back to NIOSH to kind of have them present their position and then we can go from there.

DR. TAULBEE: This is Tim. Our general position is that, with regard to the tank farms and the venting of the thoron coming out of those, Savannah River has done an analysis of what those thoron concentrations are.

So people who were working in the tank farms, that would be something that we would just take what those doses were that they measured coming from that, and assign it.

For the other areas, in particular 773-A, there is lots of air sample data in the post-1990 time period. Well, actually, even all the way through there is initial count data, there's 24-hour count data, six-hour count data, 24-hour count data where we could go through and estimate what the thoron concentration is, or a component of that radon, of that total radon, if you will.

But I guess my question to you all would be for that time period where we're dealing with only, you know, 100, maybe 150

milligrams of thorium in the building of 773-A, is this something that you want us to go back and look at from the Advisory Board, I guess, the Work Group Members here?

It's something we could do. It would involve capturing more data from that earlier time period of '72 to '89. It's certainly doable, but again we just don't think the source term is really large. Certainly, the source term coming out of the tanks would be bounding for that area. At least in my opinion.

But to prove that would take effort. And we can do that, but it certainly would just take time and effort. So I'd like to get feedback from Work Group Members as to whether that's something you want us to pursue.

MR. BARTON: This is Bob Barton. Maybe I can add -- our original concern really was related to those tank farms. And can I ask, though, you'd mentioned sort of the survey --

DR. MAKHIJANI: Bob, this is Arjun.

Could I just add one more thing?

MR. BARTON: Please.

DR. MAKHIJANI: I think we had mentioned the storage areas of thorium, so I think Tim is right about that. And it might be useful to have at least some check on whether the tank farm thoron measurements are bounding or not.

MR. BARTON: Yes, Arjun. Yeah, I agree. And that's exactly where I was going to. I think the intent of our original finding was really related to those tanks: was it a significant source term of thorium that produced thoron?

And I guess I'd kind of ask Tim. You had mentioned that, you know, you have survey data. Are you referring to the reports from the mid-90s? Because I do remember seeing those references. They did some survey work around definitely the Tank 15, and also Tank 12, between 1995 and 1997, to sort of you know characterize it.

And actually in those reports they talk about some modifications that could be made and whether you should have workers in the area when they are actually purging those tanks. And I guess I'm asking is that the resource that you're looking to use to bound the thoron potential at those tanks?

DR. TAULBEE: That is correct, yes.

MR. BARTON: Okay. And I guess then my only question would be, is there any reason to think that that would not be appropriate to use for the earlier parts of the SEC?

Because, like I said, I think those analyses were done in the mid-90s. And I don't know if there were any modifications to the way they would do the purgings. Or the way they had, you know, the stack height or whatever it is that would make the earlier period different. Or if there is actual survey data out by those tanks that sort of verifies the larger project that was done in the mid-90s. I

guess that would be my only question.

DR. MAKHIJANI: If I might add something to that. I know in the 80s sometime maybe they changed their maintenance procedures about the tanks. Maybe late 80s. So they may have changed their ventilation and maintenance procedures. I'm not 100 percent sure about that. But I think that happened there. So this point may be fairly material. And it would be useful to compare this source term with wherever the maximum storage of thorium was, as a check.

DR. TAULBEE: Well, the amount of thorium that's in those waste tanks far exceeds any other storage area onsite.

And correct me if I'm wrong on that, Mike or Matt, but I'm believing it's somewhere around 30,000 kilograms or something like that?

The next closest source would be the RBOF Building, which is the spent fuel, and there the thorium is stored under water, and it's sealed in fuel elements. So there is no

potential for exposure to thoron there.

And so then you're dropping from 30,000 to 8,000 kilograms of thorium in the RBOF. And the other area would be the 773 where you're all the way down to 150 kilograms.

So clearly the tanks would be, in my thought, the largest source term of thorium that would be available for an exposure. As we're looking at the other --

(Simultaneous speaking.)

DR. MAKHIJANI: -- attenuated by the liquid in the tanks?

DR. TAULBEE: Well, that's a hard question to answer, but yeah --

DR. MAKHIJANI: That's sort of a little bit what's behind my concern here, is that we need a little bit of a demonstration that these source terms are, you know, that the right source term in being used. And that the right periods are --

DR. TAULBEE: I mean, if we're going to go through that type of effort, I would

just go back to the original air samples that are in the building and use that.

I mean, that's the actual data, and so the actual exposure would be dropped tremendously. So if you want us to do, you know, an evaluation of it being bounding, it's just as easy to go get the original data and come up with another model for the buildings, which would be a --

DR. MAKHIJANI You know, it's not for us to say, but I'm just raising a point of scientific correctness here.

ACTING CHAIRMAN CLAWSON: This is Brad. One of my questions is -- this is for SC&A. Have we reviewed the data and this information -- I guess, Bob, this more directed towards you or Arjun. Have we reviewed the data on this and evaluated? Have we taken a look at it for its accuracy and so forth?

MR. BARTON: We have not directly evaluated it as far as comparing the air sampling and potential thorium areas which is

the tank farms.

That was sort of where my question was directed towards NIOSH as to it feels like we're kind of hanging our hat on the analysis done in 1995 and again in 1997, which identified that with the current configuration, they didn't want workers up on the catwalks around those tanks when they were being purged.

As much -- because of the topographical data. Tank 12H was kind of surrounded by a berm and a little bit lower and -- so, I guess, you know, it’s just sort of a new issue as far us discussing it. Because we really didn't get to it last meeting.

And it sounds like NIOSH's position is to use the data from the tank farms to bound thoron exposures sitewide. Am I correct in that assumption?

DR. TAULBEE: Yes, I mean, that was my approach to it. But, again, if that isn't reasonable, we can always go get the data and analyze it.

MR. BARTON: Okay, I mean, that's maybe a question for discussion. I mean, logically I would think that those tank farms, as you purge them, would represent the bounding source term. But that argument certainly needs to be buttressed by more than my opinion.

And the other facet with that was, you know, the reports that we're kind of referring to occurred in the mid-90s. And I'm not saying the source term was appreciably different but perhaps the actual configuration around the tanks where those catwalks were or the size of the stack, might have been different.

So I think that would be a line of investigation that would be worth taking as well. Just to make sure that when we're using sort of, for lack of a better word, surrogate data from the later years to apply to potential thoron exposures in the earlier years, to assure that we're not missing something that was materially different in those earlier

years.

DR. TAULBEE: This is Tim. Just want to follow up on that. That thorium had been in the tanks since the late 1960s up through about 1971 -- about 1971 was the last bit that was sent to those tanks following the U-233 campaigns.

So it's been in there the entire time period of evaluation. It hasn't changed.

MR. BARTON: Well, sure. I said that. I don't think the source term materially changed. But did sort of the exposure configuration? Because what we see even in the 1997 report when they, you know, did a fairly extensive review of the potential for a thoron problem, they gave recommendations on how they should either change the stack height or limit worker access to the catwalks around the different tanks. I believe it was like 9 through 13.

And so, you know, they were looking at improvements then. And do we have any

evidence? I don't know that there is any that, you know, improvements were made even earlier that would sort of make it difficult to use that mid-1990s data as a bounding source of exposure.

DR. TAULBEE: I don't have any evidence of changes, but that doesn't mean they didn't occur. If you want us to go to that type of level to go back to drawings and see if there was changes to the stack height or the venting procedures and so forth, it's just as easy for us to go get data from the site within the buildings of interest that handled thorium as well. Mainly 773, and just recalculate a new thorium model or a new thorium dose based on those.

MR. BARTON: Sure. I was actually more referring to changes in exposure potential to the actual tank farm workers who were out there when purges were occurring. Not necessarily whether that data would properly bound exposures to people who were inside the

plant. But actually to worker who might have been up around those catwalks or, you know, in and around the purge section of the site when it was happening. I guess that was our --

(Simultaneous speaking.)

MR. BARTON: -- that was my point anyway.

DR. TAULBEE: -- farms as well.

ACTING CHAIRMAN CLAWSON: What was that, Tim? I didn't hear that.

DR. TAULBEE: There's air sample data for the tank farms as well.

MR. BARTON: Okay, so that goes back to the earlier 70s as well? And we can look at that and use that.

DR. TAULBEE: We could, but we have to go capture it first.

MR. BARTON: I see. Well, that's kind of where we're at on that. I mean, we have a good characterization of those tanks where the majority of thorium was.

Again, in the mid-90s, the source

term probably didn't change very much, but maybe the configuration changed. You know, with the current information, we can't really go much further than that.

ACTING CHAIRMAN CLAWSON: Well, this is Brad. I understand where we're at on this issue but part of my problem is that I'm having trouble following where we're at.

NIOSH has put out a way that they figure they'll be able to do it. And to be able to justify it, we've got to be able to evaluate the data.

But what I hear Tim saying is, is, well, if we're going to do that, then we're going to change the process of how we're going to do it.

My question is, do we have a established method right now that NIOSH has proposed to us to be able to do? And that's correct, isn't it, Tim?

DR. TAULBEE: Well, we're going to be using the concentrations from these ventings

as a bounding approach. And that was our approach.

But SC&A is bring up all these other issues that it seems like you want us to go and try and track down. And if that's the case, I mean, I feel that the ventings are by far the most significant exposure to it.

But, I mean, that's just my professional opinion. If you want me to prove that, then why don't we just go and capture the actual air sample data and demonstrate what the actual concentration was?

So, yes, we would be changing our approach if we have to go and try to justify our current approach as bounding. I think it's bounding just based upon my experience, but others might disagree with that.

ACTING CHAIRMAN CLAWSON: Well, and understand, we're -- and I guess I'm looking at this from a Board Member, Tim. I'm not ever questioning your professional judgment or anything else like that.

But to put some of the data out there, I want to make sure that we have all the data that we have used, and that we can actually justify and prove to anybody looking in from the outside of it, that this is bounding, this is the best that we've got. And from SC&A's standpoint, that they have justified all of the information and the data, and we've run this to ground. This is just part of the process that we're in.

And I guess my conundrum here is on one hand you've got something set out here for us. But if we're going to make you justify this information, then you're going to change it. That's where I've got my issue at.

I guess what I would like from -- you're going to have to justify it no matter what. We're going to have to make sure that this is the proper information that we've got, that we've got adequate information to be able to make this kind of a judgment. So my question to you is, we're going to have to do that, so

you're thinking of changing the process and just go pull all the sample data from the -- the air sample data from the stacks? Correct?

DR. TAULBEE: No. Yes and no.

ACTING CHAIRMAN CLAWSON: Oh, okay.

DR. TAULBEE: The final part there, pulling it from the stacks, no. There is air sample data not from the stacks that is there in the workplace area. And they were doing other sampling and other tasks.

When they did those other tasks, for the workers standing on top of the tanks, they would take an initial count for radon-thoron. And they would do a measurement of that. That is the data I'm talking about going to get.

The only reason I'm suggesting to do that and change our approach, which I understand you're concerned with, is that frankly I believe it's easier and faster to go get that data than it is to try to demonstrate

that there weren't any of these other changes that changed that plume, venting from the 1990s measurements. I simply think it's an efficiency standpoint.

ACTING CHAIRMAN CLAWSON: And I understand that. And I guess from -- I know, as a Board Member, where I'm at now, I would tell you that, well, okay, then we need to be able to look at this data. And we need to be able make sure.

Because I don't want to be put up in front of the people and say, well, how come, how can you just take this as this. We need to be able to run this in.

So if you're going to pull this data, this is my understanding, and forgive me, I didn't mean to misrepresent you there. I just kept hearing the stack and the air sample data and stuff like that and probably got confused. Then basically this comes down to this is back into NIOSH's court. And they are going to proceed with a different path forward and

they'll get back with the Board on this.

DR. TAULBEE: Okay, I agree to that. Thank you.

ACTING CHAIRMAN CLAWSON: Any of the other Board Members feel any differently? And feel that they need -- Jim?

MEMBER LOCKEY: Jim Lockey. What's going to be the NIOSH path forward, then, so I understand?

DR. TAULBEE: Our path forward would be for us to go to the site and take a representative sampling of air samples over time from the tank farm area and evaluate those initial counts, the six-hour counts, the 24-hour counts, and come up with what the thoron component of the total radon would be. And that would be the dose we would assign to the workers in that area.

MEMBER LOCKEY: You're going to go back to the site and actually do air sampling?

DR. TAULBEE: Actually get air sampling data from the 1970s up through the

1990s.

MEMBER LOCKEY: So then you're going to retrieve that data, okay.

DR. TAULBEE: Yes, we'll do a sampling. We won't capture it all. Because it's hundreds of thousands of pages.

ACTING CHAIRMAN CLAWSON: Tim, is this electronic or is this all paper?

DR. TAULBEE: It's a mix.

ACTING CHAIRMAN CLAWSON: It's a mix.

DR. TAULBEE: Yeah. Well, it's not electronically available from a database standpoint. But some of it is available PDF-wise to where Joe can look at it now and I can look at it now.

And then others we'll actually have to pull some boxes, especially back in the earlier 70s through probably the early 80s.

ACTING CHAIRMAN CLAWSON: Okay. Dr. Lockey, does that answer your question?

MEMBER LOCKEY: It did, thank you.

ACTING CHAIRMAN CLAWSON: What's your feelings on it, Jim? You know --

MEMBER LOCKEY: What are my feelings about it?

ACTING CHAIRMAN CLAWSON: Yeah, do you agree with me that basically we've got to, if this is what we're going to do, we've got to review the data and go from there.

MEMBER LOCKEY: I think looking at the data is always the best approach on anything. I don't know enough about it to say what the probability of what SC&A is saying in regard to perhaps a exposure level that's not represented by the bounding limits that NIOSH is already using.

I don't know that probability. Whether it's a ten percent probability or 50 percent probability. Not knowing that, I always say go look at the data, and the data will tell you where to go.

ACTING CHAIRMAN CLAWSON: Okay. Phil?

MEMBER SCHOFIELD: No, I agree that we've got to get all of that data for them to look at to verify what the model is.

ACTING CHAIRMAN CLAWSON: Right. Well, Bob, is this going to -- you know, you're going to have to review this, you or Joe, or any of them. Is this a good enough path forward? You think this will satisfy the issues that you've raised?

MR. BARTON: Yeah, Brad, I think that is probably the best path to go with. I mean, just as a general philosophical point, I think anytime you're going to use situations from a later time period and apply them beforehand, you kind of have to have some connection to say that conditions were sufficiently the same, that it's fine to go ahead and use, you know, the mid-90s evaluation for the earlier years.

And, to me, what I'm hearing, I mean, I think this is probably the best way to do it. I mean, as Tim said, just from an

efficiency standpoint, I mean, you can go in and look at structural drawings to see what the height of the catwalks was.

But, I mean, even then, you know, what's the connection to how that affected the exposure potential? I think getting the air sampling and comparing it to what NIOSH is proposing for the thoron issue, is the way to go.

And there hasn't been a formal, necessarily, write-up on -- or has there? I mean, maybe Tim can remind me, is there an official write-up on how NIOSH currently proposes using that mid-90s evaluation to bound the thoron exposure?

DR. TAULBEE: I'm actually not sure. I'd have to go back to ER Addendum 3 and dig out what it was that we said about the thoron there. I couldn't answer that off the top of my head. Sorry.

ACTING CHAIRMAN CLAWSON: Okay, well, then with that said, then, this one will

fall to NIOSH, and we'll proceed on. Joe?

MR. FITZGERALD: Yeah, I think going from -- that was Finding 4. Really, the vast majority of the next, I guess almost 15 findings, from 5 to 23, had to do with OPOS-related, or NCW versus CTW comparisons.

Which, I think, in the last Work Group meeting, I think it was everybody's consensus that that would better deferred to the SEC Work Group review of that report, which by the way was just issued. SC&A did send that out. I guess it was Friday, late last week.

And we did make a comment, though, that quite apart from the question of applying OPOS, we do have some very fundamental concerns over the comparison between the NCW and the construction trade worker groups in terms of data accuracy standpoint.

Though OPOS is the methodology, but I think we still have very much site-specific concerns about whether that's feasible or not. So I just want to throw that in.

But for this discussion, for this Work Group, I would recommend we probably move beyond those findings.

Seventeen, we did touch upon last time, which was a question of whether chelation samples were included. And I think there was agreement. And NIOSH's response in the discussion was that they would not be. Did I get correct?

DR. ARNO: I guess just two things there. Within the bioassay records it's noted that DTPA had been administered. Those have already been removed. But we did not go back and look at the other information about who was given chelation therapy to exclude records on that basis yet.

MR. FITZGERALD: But I think in principle there's agreement that they would be excluded?

DR. TAULBEE: Yes, I believe that's the case.

MR. FITZGERALD: Yes, I think

that's where we came out. But --

DR. NETON: Yeah, this is Jim. We agree with that. Technically, it's not appropriate to use chelation data in a coworker model when you know it.

I mean, if it slips in there because you don't know it, as you indicate in your finding, that we be claimant-favorable. But where we do know it's a chelation person, it should be avoided.

MR. FITZGERALD: So then it just becomes a question of just verifying, implementation more than anything else.

So, Brad, I don't think we have an issue on that. We seem to be in agreement on Number 17.

ACTING CHAIRMAN CLAWSON: Okay, then. With the other Board Members' concurrence, then we could close that one.

MEMBER SCHOFIELD: I have no problem closing it.

MEMBER LOCKEY: Jim Lockey. No

problem.

ACTING CHAIRMAN CLAWSON: Okay.

MR. FITZGERALD: That moves us swiftly forward to Finding 18, which actually Joyce has spent a considerable amount of time, as you probably can see by the attached spreadsheets. And I would not want to pretend I could describe everything that she's done in terms of her analysis. So, Joyce, are you on the phone?

DR. LIPSZTEIN: Yes, I'm on the phone.

MEMBER LOCKEY: On Number 18, which gets to be a little complex, could you I guess slowly take us through, take the Work Group through what the issue is and what we would think the implications are?

DR. LIPSZTEIN: Okay. I'm not a chemist is the first thing I want to say. I'm a physicist, not a chemist. But I looked at the raw results, some americium, curium, which might have thorium in it.

And I examined this raw data and the raw data is, as you can see by the table that we put on the spreadsheet, it comes like several discs.

There were ten disc results. You can see dpm for 1.5 liters and down. They are all different disc results for the same sample. And then you have the report value. Okay?

So I've noticed that in several of the discs the results were very different, one from the other. And I was asking how reliable are those results if they are so different one from the other?

So we got an answer that when you have results that are near the detection limit, you would find a lot of variability. And I understand that. But then I decided to divide the results in parts.

So first I made a table with all results that were greater than three dpm per 1.5 liters. Why greater than three dpm per 1.5 liters? Because the MDA of the method is

reported as .3 dpm per 1.5 liters.

On that range, from greater than 10 MDAs, about 15 percent of the results are the green ones. They are results which I think have a great variability.

One example for that, if you take the third green results, one disc is 53, the other one is 23. And you get an average of 38. Then you have the 4th disc results. You have 8.64. Then you have 6.79. Then you have 2.72. Then you have 15.3.

So you have a big variability between 2.72, which is almost three, to 15. It's five times. So even in that range of results greater than 3 dpm per 1.5 liters, you have 15 percent of the results that has this kind of uncertainty.

Then I went to look at the results that were between one dpm and three dpm per 1.5 liters. So between 3.3 times the minimum detection activity and 10 times the detection activity.

And, of course, as we expected, as you go down you have more variability. So we came up with 26 percent of the samples had what I call very high variability.

Then I have the results that were another table with results from .32 dpm per 1.5 liters to .99 dpm divided by 1.5 liters per day. 1.5 liters is the urine excretion in a day.

So these are results that are -- when the final result is above the detection limits, and between the detection limit and three times the detection limit. And I have 43 percent of the results that have a lot of variability.

So I don't know if we should trust results that have a large variability of results. And what to do with those samples?

Then, as I looked at the table, and you can see, most of the results that are above 3 dpm won't be used anyway, because 90 percent of those results were from DTPA, when the DTPA was given.

I don't know about, you know -- some of the results I don't have information, if DTPA was used or not. But 90 percent of the results we know that DTPA was used.

Then you go to the other range, from 1 dpm to 3 dpm, then you have that 75 percent of those results were from the usage of DTPA. So most of the results were from results that were below .99 dpm per 1.5 liters.

That's where most of the results are going to come to do the coworker model. And they have a lot of uncertainty. So I don't know what is acceptable, what is not acceptable.

But if -- I don't know. I think that if I were in my lab, I would like to know what was happening that you have such high uncertainty on results from the same sample.

And then I went -- I know that it was told here on this conference call that the OTIB-81 was not discussed yet, but I'm looking at results that you used for the urinary excretion, the 50 percentile and 85th

percentile of the urinary excretion rate of americium, which will give us the data for thorium.

And from '72 on, that's the time period that we are looking at, they are all below the detection limits. So they are results with a high range of uncertainty. So I don't know if they can really be used like that, using the number itself that you got.

So I doubt very much those results that are well below the detection limit when you have such an uncertainty on the results. Can you get what I'm talking about?

DR. TAULBEE: This is Tim. I understand what it is you're talking about but I disagree with some of your conclusions.

DR. LIPSZTEIN: Okay.

DR. TAULBEE: Of the --

DR. LIPSZTEIN: So let me hear.

DR. TAULBEE: -- excessive uncertainty. A large number of this uncertainty can be explained by simple counting

statistics. Not all of it. I agree there are some other process things that were going on.

But I, in our response we're trying to point out that one of the major advantages of doing multiple counts on the same sample is you're getting more accurate answer. You get more towards what the true meaning is. And that was the goal here that Savannah River was doing.

DR. LIPSZTEIN: Yes. But would you trust the results like, you are well above the detection limits first. Not the bottom of detection limit. You have 63 and 23, then you have another one that has 8.6, 6.8 --

DR. TAULBEE: But that --

DR. LIPSZTEIN: -- 2.7 and 15.3. You have even some results that they sum a positive result with a negative results to give an average result. That's very strange.

DR. NETON: Tim, this is Jim. I've got a question, or two questions actually. Am I correct in understanding that they would

take a sample and split it and do four separate discs on one sample?

Or are, you know, multiple discs? Kind of unusual.

DR. TAULBEE: I don't know that for sure --

DR. NETON: Are these individually separate counts on the same disc? I don't quite follow what they were doing here.

DR. TAULBEE: My belief is they were counting the same disc multiple times. But Matt can you shed some light on that?

DR. ARNO: No, I can't. It's hard to tell from available records whether they were counting the same thing multiple times. Or whether they actually had separate aliquots from one sample.

DR. NETON: It would be really good to understand that. I think Joyce raises an issue. And I think it deserves us to follow up a little bit further on, I think. The ranges are pretty large.

The other thing that concerns me is the DTPA usage. These were Type M actinides, right? So these were being measured for americium, curium, and thorium came through as well, right?

And Californium. My understanding of exposures, at a facility like Savannah River was, you wouldn't have had a huge number of people chelated for those nuclides. It was a by and large chelation for plutonium. I'm a little --

DR. ARNO: A lot of times that's right. People were chelated to plutonium and americium contamination in the plutonium came out at the same time.

DR. NETON: Yes, that's true. Yes, because I was going to say if it's a chelation for plutonium, it would have been taken out as part of the other procedure.

But it is true that the americium would come through, but it certainly would be a trace amount compared to the plutonium.

I think there's two issues here and I think we need to maybe follow up a little more. I'm not sure what we can do.

But I tend to agree that the variability seems, well I don't understand what's driving the variability, whether it's time and statistics or the chemical procedure itself.

Because if it's multiple aliquots it could be a chemical recovery issue. And that's being reflected in the samples. So I think we need to understand better what's driving that variability before I could even comment. And my --

DR. ARNO: We also need to put this in a little bit of perspective. A lot of these samples that Joyce identified, were for people that either had DTPA, which means they'll be excluded when we revise this data.

Or it was for a person that had some intake, even though they didn't receive DTPA, they were sampled extensively every single day

for a couple weeks. And that data all becomes one result when OPOS evaluation is done.

And even if we go away from the OPOS type process, that would still be a person that would be subject to some sort of exclusion or some sort of averaging of their results.

So even if there is a large variation from disc to disc, we're talking about averaging of that. And then multiple samples and averaging of that.

And when you get into looking at the statistics of, you know, a large number of samples, a large number of counts, the individual variability becomes much less important because you are looking at the average quantity.

DR. NETON: Yes, I realize. I agree with that. I think we do need to dig into this a little more. I was wondering if those individual discs weren't actual individual urine samples? I don't know. Do we know that, Matt?

DR. ARNO: It's very clear that the discs were all from the same urine samples. Just not clear if the same disc was counted multiple times, or if it's multiple discs.

DR. NETON: Well, I think, I think I agree that we need to follow up on this. A little more detail to shed a little more light on it that we can. So I think we'll, our action item there is to dig into this a little more.

I do agree when you go from something like 15 to 50 that does give me a little concern. And I'm not saying it's not appropriate, I just need to understand a little better what's driving that.

MR. BARTON: Yes, Jim, this is Bob Barton. I'm in complete agreement there. This is really one of those things where when we started looking at the data, you just kind of scratch your head and say, well how were you arriving at these seemingly radically different results?

When there may be a very good reason

for that. And you know, it's fine to use these data as is. You know, I agree, I think we need to understand why there's that variation.

DR. NETON: Yes, a lot of the lower ones could be definitely accounted for by TIB-6. But if you have a detection limit of .3 and you're up around 15 dpm, that result should not be quite as variable as is being reported, so. We'll go look at it and get back to the Working Group.

ACTING CHAIRMAN CLAWSON: Okay, that being said. Is, not seeing anymore, that action item falls on NIOSH and we'll look forward to seeing what they have to come back with. Joe.

MR. FITZGERALD: Yes, the remaining set of very similar issues on solubility, 24, I put them together -- and I'm getting a lot of background noise. I don't know if somebody has their conference line open?

Oh, that's much better thank you.

Bob, I'm just trying to characterize our discussions on 24 through 26. I don't think we had any disagreements on how the solubilities were addressed technically.

But some question about what if some validation could be done on the coworker model? I guess my question is has that been subsumed by the earlier agreement to do, for Tim to do such an analysis? I thought the analyses that was pertinent to the analysis that offered.

MR. BARTON: Yes, we actually did discuss that on the 5th with regards to the thorium issue. And then earlier today with regard to neptunium.

And I think it's the same line of discussion, that you have known thorium workers, you really want to go ahead and look and see, you know, A, are they included in your coworker model? At least to some part, some extent.

And these workers if, you know, you

have a list of, you know, 20 or so workers I think was said at the February 5th meeting.

Go ahead and look at their in vivo results if they are in the coworker model. And how do they sort of stack up against the rest of the population?

I mean one result of that might be see, well, hey, I mean they're right in the middle, you know. These thorium workers that we know were thorium workers are sort of subsumed into this larger coworker model.

Alternatively, you could see that they're much lower, or alternatively you could see, wow, their results are way at the top end of the tail. And then maybe we have more of an issue.

So I think and again I believe NIOSH agreed to this at the next meeting, that you know, that if they had a list of however many known thorium workers, they could take those names and look into this database and see where their results stack up.

DR. TAULBEE: This is Tim. That's correct. I had that as an action item from the last meeting, and we have begun to schedule that particular work to be done.

MR. FITZGERALD: To sum up on that one, I thought that, you know, we had looked at the solubility and discussed that. And I thought we didn't really have any differences on the actual technical approach.

But we had that larger question which as Tim just noted, we thought that would be the more fundamental answer to that. And so Bob, I guess is there anything else on that particular set of issues?

MR. BARTON: No, that's really all I had on that. I mean we kind of discussed the use of different solubility types and how that'll affect the calculated doses before in this discussion.

And that was sort of an add-on to the discussion last week of, you know, it would just really be a good idea and a good weight of

evidence argument to say, hey, look, you know, we know there are some thorium workers.

These are the ones that are in our distribution, and these are what the results look like. And in so far as you can tie those results to thorium activities, you know, that might get a little murky.

And there might be some caveats to that, but I think it's an effort that's worth doing that would not necessarily be all that cumbersome.

It would be something, you know, if you only have a handful of workers, you know, it wouldn't be too hard to look up those names in your electronic database and take a look at what their records look like in comparison to the coworker model as a whole. That's really all I had.

MR. FITZGERALD: Yes, I think we're okay on the solubility questions on 23 through, I'm sorry, 24 through 26. And really the commitment that Tim was referring to. With

the bigger issue we thought was the more important pathway to resolution.

So I would leave it to the Work Group on those 3 findings, but we feel comfortable on the solubility issues.

ACTING CHAIRMAN CLAWSON: Joe, this is Brad. So you want to close the solubility of these issues, but these other, they still lap back to what Bob was saying about checking out, you know, checking the data out basically?

MR. FITZGERALD: There is some overarching questions that get to validating the data. And I think what NIOSH has offered as far as neptunium as well as thorium, would be in that direction.

So we would say that would be the pathway to go, and the specific questions we had on solubility, I think were answered on February 5th.

ACTING CHAIRMAN CLAWSON: Okay, so with these, what is it, 23, 24?

MR. FITZGERALD: 24, 25 and 26.

ACTING CHAIRMAN CLAWSON: Okay, on these the solubility question of it is closed though. It got an overarching issue, but the solubility issue has been closed and you're satisfied with it, right?

MR. FITZGERALD: As far as I'm concerned and I think Bob has confirmed that. So I think we're okay on the solubility issues. Joyce did you have anything?

DR. LIPSZTEIN: No.

MR. FITZGERALD: Hello, Joyce.

DR. LIPSZTEIN: Yes, but I think we discussed this already. I'm not completely satisfied but I'll accept it.

MR. FITZGERALD: Okay. So I think --

DR. LIPSZTEIN: Because I think that really when you have insoluble thorium, we won't see anything in the urine. Unless you had a big accident, and you would know that. But it's okay.

ACTING CHAIRMAN CLAWSON: Okay. Any of the Board Members have any objection to closing the solubility issue of this portion of this?

MEMBER LOCKEY: This is Jim Lockey, I'm fine to close this.

MEMBER SCHOFIELD: This is Phil, I'm fine with it.

ACTING CHAIRMAN CLAWSON: Okay with that we'll proceed on. Joe. And somebody thank you for forwarding the information, the Live Meeting. It was kind of back there a few.

MR. FITZGERALD: Well I think we're done because really the balance of what's left, 27 through 32 refer back to the 1990 to 2007 period, which is the approach based on the DAC, you know, the air concentration measurement.

This is the report that Tim briefed out on February 5th, which the Work Group asked NIOSH to go ahead and you know, draft it up for

review. And so these findings relate back to the, you know, those activity levels.

So I would say 27 through 32 refer back to Finding Number 2, which is the application of the DAC hours to 1990 and beyond.

ACTING CHAIRMAN CLAWSON: Okay.

MR. FITZGERALD: So we'll await for that report, that draft report and SC&A would commit to reviewing that and providing any findings and issues back to the Work Group and NIOSH.

DR. MAKHIJANI: This is Arjun. Joe, I didn't quite understand that. This is Arjun. I, these particular findings relate to, you know, the compilation of the data, which hasn't been done.

And we weren't actually able to figure out whether, you know, what the merit of this approach was and the coworker model and how it was going to be assigned.

And that was part of it, the FASTSCAN data and whether they could actually

catch any thorium. And so we're sort of beyond the DAC hour question, I think.

Maybe Joyce can correct me if I'm wrong.

DR. LIPSZTEIN: I think because of DAC, I wasn't on the February 5th Work Group meeting, but I think that because you can't see with the FASTSCAN, that's why there was this new DAC method for assigning thorium dose instead of the whole body counter dose.

DR. MAKHIJANI: Okay, so --

MR. BARTON: Yes, Arjun. This is Bob Barton we're essentially at a spot where we have a new paradigm shift. There's a whole new model on the table that NIOSH is proposing to reconstruct thorium doses in the 1990 and on period.

So we're kind of waiting --

DR. MAKHIJANI: Have we seen the compilation of that data? Not yet I guess?

MR. BARTON: Of the air sampling and such, no. We're sort of waiting on NIOSH

to put that package together.

DR. MAKHIJANI: Okay, sorry. So excuse me about that. Yes, sorry about that.

MR. FITZGERALD: Yes, again I think it was decided it would be better to see those details and to I guess defer any kind of judgment or analysis until we have full --

DR. MAKHIJANI: No, no I stand corrected, Joe.

MR. FITZGERALD: Yes.

DR. MAKHIJANI: John, put me on mute.

MR. FITZGERALD: So Brad, I think that's just about it. And I'll do the best I can to put these notes together as well as the actions. And I'll lean very heavily on my colleagues to help me on some of this.

But make sure it's as detailed as possible and get it back to the Work Group hopefully by COB tomorrow.

ACTING CHAIRMAN CLAWSON: Okay, and Tim you'll do the same with your action

items?

DR. TAULBEE: That's correct, although I'm not sure I'll have it done by tomorrow. But certainly early next week.

DR. NETON: I think, I thought Joe was going to collect them all and then we were going to --

MR. FITZGERALD: I'll volunteer to collect them all and I'll lean on Tim to edit at will. Some of these have nuances. Like I say, I was scribbling as fast as I could but if I missed anything, please feel free to edit this.

It will go back and forth until everybody's satisfied.

DR. NETON: Yes, I think that makes the most sense. We'll be happy to do that.

ACTING CHAIRMAN CLAWSON: Okay, thanks. Is there anything else needs to be brought before the Work Group at this time?

Not hearing any, Ted, I move that we can adjourn.

MR. KATZ: Yes, thank you, Brad for stepping in for Mark for one. And thanks everyone else. I think was incredibly productive and well done. So thanks for everyone's efforts going into this and during the meeting.

ACTING CHAIRMAN CLAWSON: Okay. Everybody have a wonderful day and until we hear or see you next time, bye.

(Whereupon, the meeting in the above-entitled matter was concluded at 2:38 p.m.)

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