Centers for Disease Control
National Institute for Occupational Safety and Health
Advisory Board on Radiation and Worker Health
Savannah River Site (SRS) and SEC Issues Work Groups Joint Meeting
Thursday, December 5, 2019

The Work Group convened in the Cincinnati Airport Marriott Frankfurt Room, 2395 Progress Drive, Hebron, Kentucky, at 8:30 a.m., Eastern Time, Henry Anderson and Bradley Clawson, Work Group Chairs, presiding.

Present:

Henry Anderson, SEC Issues Work Group Chair
Bradley P. Clawson, SRS Work Group Chair
Josie Beach, Member
James E. Lockey, Member
Genevieve S. Roessler, Member*
Phillip Schofield, Member*
Paul L. Ziemer, Member

Also Present:

Ted Katz, Designated Federal Official
Matt Arno, ORAU Team*
Bob Barton, SC&A
Liz Brackett, ORAU Team
Ron Buchanan, SC&A*
Grady Calhoun, DCAS
John Cardarelli, DCAS
Nancy Chalmers, ORAU Team
Harry Chmelynski, SC&A*
Paul Demopoulos, ORAU Team*
Joe Fitzgerald, SC&A
Jenny Naylor, HHS*
Joyce Lipsztein, SC&A*
Mike Mahathy, ORAU Team*
Lavon Rutherford, DCAS*
Mutty Sharfi, ORAU Team*
Dan Stempfley, ORAU Team*
Tim Taulbee, DCAS
Chris Tornes, ORAU Team

* Participating by phone

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Proceedings

(8:33 a.m.)

Roll Call/Welcome

Mr. Katz: So, good morning, everyone. This is the Advisory Board on Radiation and Worker Health. It is a joint meeting of the Savannah River Site Work Group and the SEC Issues Work Group, and I'll tell you a little bit more about that in a little bit in the plan for the day.

But, to get started, just a few logistical matters. For people on the line, the materials, the agenda for today, and the materials that can be made available today because they've cleared DOE clearance and so on, are on the NIOSH website under schedule of meetings, today's date. You can go there and you can pull up -- so, many of the documents and background documents, in other words NIOSH documents and the SC&A reviews for the Board, are there and available for you to read through as you might.

For the Board Members that are not here but are joining us remotely, Skype should be up and running. And although there shouldn't be anything shown there yet, I think we will have -- Tim, at least, is planning to put up maybe some of his presentations, if that all works well. So you'll be able to get them there, and you should have gotten them by other means by me directly, as well.

So, roll call. Let's just, I guess, start there. It's easiest.

(Roll call.)

Mr. Katz: Okay. So that takes care of roll call. We did that. So just let me -- although we don't have any members of the public to benefit from this right now, or they're not saying, let me just say a couple things about this somewhat unusual format meeting going forward and then ahead with that.

So, this is a meeting of two Work Groups and in a sense they have sort of interrelated but different tasks. The SEC Issues Work Group is here because it has the charge of reviewing the guidelines for coworker datasets, for use of coworker datasets that the Board has already tentatively approved and wanted to see it demonstrated using a site, and SRS was a handy one, and a complicated, difficult one, probably, but useful for that demonstration. So that is, in a sense, why SEC Issues Work Group is part of this meeting.

And then, of course, SRS is because the models that are being used are for SRS, and SRS is dealing with an SEC petition and those models are relevant to the petition. And they are getting ready to move forward on SRS SEC petition and make some progress on that and get that a disposition. So there's been a lot of work done that's obviously relevant for them.

So, and with that in mind, just let me note, then, how something will work here with respect to the Work Groups. So, we do hope, and we'll see how it works out, that we could possibly have action on the guidelines to complete those, since we've had all this work on SRS now and we have a sense of how those guidelines work. And that would be the SEC Issues Work Group's job to do. So if there's a motion on that, that would come from that Work Group. It would be that Work Group that votes on that motion.

And then, for SRS, this is, as it is on the agenda and also on the agenda for next week, almost immediately, this is an update on the SRS SEC petition. And it's an update because there's a vast amount of work that's been done and it's going to take some doing to get through it, both by the Members -- and we have quite a large membership here of the Board -- but then also the other Members that will be at the meeting tomorrow for them to get sort of -- begin to get their heads around the SRS issues.

So, there isn't expected to be action on the SRS petition at this, or at the meeting next week, but I think the idea is that we have this large meeting to get ourselves well started on that matter. And then we have January, February, March to have more Work Group meetings as we may need them. And they may just be SRS Work Group meetings. They may be joint depending on whether there's a reason to go have more joint meetings. But, so, we have quite a bit of time ahead of us to be able to work through this matter, and we hope have action -- in April we have a Board meeting and we could potentially have action on the petition in April and get this behind us.

So that's sort of the general scheme, and that's about the last I have to say. Except logistically, please, folks on the phone, please keep your phones muted except when you're speaking. And if you don't have a mute phone, *6 will mute your phone and *6 again will take your phone off of mute. But please keep your phones muted and please don't put the call on hold ever. Just hang up and dial back in if you have to, because hold creates problems for everyone.

And let me just check and see -- folks on the phone, can you hear me well? Am I clear?

Participant: Yes, Ted, you're fine.

Mr. Katz: Okay. Good. I just thought --

Member Roessler: I can hear well.

Mr. Katz: Super.

Member Schofield: I can hear fine.

Mr. Katz: Great. So I'll just say for people in the room, then, as this is a long skinny table, we may need to move this phone about a bit because we want people on the line, Board Members and members of the public and other staff that are not here, to be able to hear what's going on, too. We

only have one phone thing. I don't know how much -- how many degrees of freedom we have here. We have some. We have some cord here, so we'll try to move it about. And, people on the line, if at some point you can't hear someone, just pipe up. Let us know and we'll try to remedy that because it's important that people can hear.

And, with that, then I have my two Chairs. If you want to, do you have remarks you want to make to start with?

Co-Chair Clawson: No, that's fine.

Member Beach: I have a question before we get started.

Mr. Katz: Yeah, absolutely.

Member Beach: So, on the coworker modeling, because you made a distinction between the two separate groups.

Mr. Katz: Yeah.

Member Beach: The coworker criteria came out in March and then again in July based on new --

Mr. Katz: In 2015.

Member Beach: -- 2015, based on comments from petitioners. So, where are we as a Work Group? We haven't looked at that. I don't know that --

Mr. Katz: It was really not -- there wasn't substantial change from -- there were some comments that Jim addressed, but they were more about -- descriptive than they were substantive.

Member Beach: But it came out, and I don't think we've ever really had a discussion and --

Mr. Katz: But we had -- the Board already tentatively approved those, so they're really --

Member Beach: Okay. So that was in 2015?

Mr. Katz: There wasn't any substantial change since the Board's approved the guidelines.

Member Beach: Okay. So we have approved it? Okay.

Mr. Katz: So, really, the question now is just, is this demonstration illustrative enough to say these guidelines are functional as guidelines, we can go forward. And the reason that's important is because this is not the only site that needs coworker modeling.

Member Beach: I understand.

Mr. Katz: And there's a whole lot of other stuff that's going to just sit backed up waiting for this to get done. So it is important to get it done.

Member Beach: Okay. So then I was wondering if, before you jump in on your slides, if you would give like the brief history of that, because it's been since 2015, and where we're at with that criteria, if you can --

Dr. Taulbee: I actually have it as part of my presentation.

Mr. Katz: Okay. Great. Super. No, that's for that question. And, Brad, you'd said you --

Co-Chair Clawson: I'm good with it. I'd just like to start in with it and go from there.

Mr. Katz: Yeah. Henry?

Co-Chair Anderson: Yeah, I'm fine as well. And it would be nice if we could change and rather be tentative have it be an accepted and not a draft.

Mr. Katz: Yes.

Co-Chair Anderson: It's been draft for almost 10 years now. So that is my intention here, is to have the Committee look at it and say we're comfortable with it and we can just report back to the Board.

Mr. Katz: Okay. So it sounds like -- Tim, are you the first up?

Dr. Taulbee: I am. And it works.

Mr. Katz: And it's working.

Dr. Taulbee: For now. Can people see it?

Mr. Katz: Right. There are not that many people on Skype, but the people that are on Skype, someone, can you say is it showing the presentation?

(No response.)

Mr. Katz: Maybe no one's looking at Skype. It's possible.

Member Beach: I think Phil was going to be on.

Dr. Buchanan: Yes, it's showing the presentation.

Mr. Katz: Okay. Thank you, Ron. Thanks.

Okay. Go ahead.

Coworker Modeling Guidelines Review Internal Coworker Dosimetry Data for SRS (OTIB-81) by Tim Taulbee

Dr. Taulbee: Okay. Well, thank you all. The title of this talk is the Coworker Model Implementation Guide, and the SRS coworker model is the example from here. And before I get started, I really want to recognize the team that did this work, and that is led by Liz Brackett and Chris Tornes here, that went through and -- Chris was more responsible for Rev. 3 of this example and Liz for Rev. 4. And as Dr. Ziemer noted when he came in, there will be a new person who will be leading coworker next, and that will be Tim Kirkham.

The statistical support on this is Dr. Nancy Chalmers, sitting next to Liz here. And so a lot of this development and the stuff behind the scenes and all is led by her. So, and there's a whole team

that did this type of work, or did this work here.

So -- oh, one other thing. John Cardarelli, sitting to the right of Grady here, is going to be taking over for Savannah River from me. As you know, I've moved to Associate Director for Science now, and having a site as well as doing that job is too much. So I've been double duty for a while. And so John will be taking that over, but Grady wouldn't let me throw him to the wolves on this meeting.

(Laughter.)

Member Beach: Best way to learn.

Co-Chair Clawson: Baptism by fire.

(Laughter.)

Dr. Taulbee: So, but he will be taking over and I gave him a -- him attending here, I said any notes, action items that would come out of here, they're yours. But I will still be involved, just to let you all know.

Mr. Katz: And just to remind people on the phone, please mute your phones. So, *6 for those of you -- there's at least one phone that's not muted. Thanks.

Dr. Taulbee: And I don't know why, but the slide is auto-advancing on me. But it is.

Co-Chair Anderson: Just to keep you on time.

Dr. Taulbee: It is. It is.

(Simultaneous speaking.)

Dr. Taulbee: Okay. So my goal here is, an overview, is to give a little bit of background that you were talking about there, Josie, and to go over the draft criteria for the evaluation and use of coworker datasets. And then go through the SRS coworker model as the example of where we went through and implemented this particular guide. And then wrap up with a summary.

So, background leading to this development. If you go back to 2003, kind of the first original coworker was a bounding approach, and that would be OTIB-1. And we don't use this one anymore, but this is kind of the beginnings of coworker, if you will.

And in 2010, during discussions, there was some concern of coworker models using just the raw bioassay data that they would be dominated by a few individuals. This lead to RPRT-53, which was the one person/one statistic report.

In 2012, there was a series of reports that we did comparing different types or strata of coworkers. And then, in 2013, the Advisory Board, SC&A reviewed those comparison methodology. There were multiple SEC Issues Work Group meetings discussing one person/one statistic, stratification, statistical comparison methodology. And these discussions promulgated the development of the draft criteria for the evaluation of coworker datasets. At that point, around 2014, is when Jim kind of said, okay, let's set the criteria. And so he went back and he developed these.

So, this is kind of the timeline of its development and presentation. June 2014 was Rev. 1. And then it goes all the way to March 1015, which you were talking about earlier, Josie. And then July 6th was Rev. 4.1.1, and that's the information that Ted was indicating was minor cosmetic-type changes. I shouldn't say cosmetic. That's wrong. It was minor changes; didn't change the methodology at all.

Okay. And so at that point is when the SEC Issues Work Group requested a demonstration or a pilot of our implementation of this implementation guide. How do we do this?

And so we did this first with the Savannah River group. And we have three datasets that were readily available -- or I shouldn't say readily. They weren't as close as we thought at the time, let me put it that way, to meet the QA standards that Jim set forth in that draft guide were more difficult, and

so it took us time to go through and do that work.

But in November of 2016 we presented this to the Work Group. And at that time the general consensus was -- and I believe this was from Dr. Melius -- that he wanted to see the full model in order to do an evaluation, or to fully evaluate all of the aspects.

And so we did implement -- completed all the other radionuclides. It took us more time. It took us to March of this year. And this was the first introduction of the full coworker model. And I do have a hard copy of it here, it's 258 pages, so it is huge, the volume of work that Liz and her team did here.

So, let's go over the criteria of what was set forth in there, just to refresh everybody's memory, and then we'll go through the actual coworker.

So, the main elements of it are data adequacy, and this is -- I kind of shortened it into the point of, where the methods correct? Were they doing sensitive enough measurements to actually measure the radionuclides of interest? Is the data complete or reasonable from the standpoint of representative of the work of the people who were unmonitored? And then I've added the word validation. This is the validation of the datasets. Do they contain all of the data that we think they do?

Then the applicability to the unmonitored workers. And here's where we go through a discussion to indicate why we feel that this data applies to the unmonitored workers. And that's then the analysis and application. Then I've got here the time interval of the model data. And this is kind of where, in the past, we've used multiple years sometimes when we had limited data. And in this particular guideline it indicates that we should do a one-year interval with no more than three years without some additional justification. And then the final section of this guide is the evaluation and stratification.

So, let's talk about the data adequacy. This is where we review the sampling methods, the laboratory analysis. And, according to the criteria, consideration should be given to the representativeness of the bioassay collection methods. Were the workers with the highest exposure potential monitored and what was their frequency? Radiochemical recovery. Was this considered or -- the counting efficiency, self-absorption, did they consider this in the measurement methods? And then the reliability of the overall measurement method.

Under data completeness, the draft criteria indicates we should evaluate whether the data are either sufficiently representative or bounding of the exposure potential. And it recommends a minimum of 30 person measurements per year. This is where the one person/one statistic comes into play, that we -- or through the Work Group discussions changed to the time-weighted one person/one statistic methodology. Under data completeness we're to assess temporal trends and gaps. We're to assess the data quality, the accuracy of the data transcription. Those were all things that were discussed in the Work Group meetings. And we are to evaluate the potentially missed data -- or censored data is what I should have said there. And for some of this evaluation they compared to the claimant files and the NOCTS data for completeness.

Okay. Next is the applicability to unmonitored workers, and the criteria sets out a hierarchy in there of routine representative sampling, routine measurement of the highest exposure potential. So, it does actually indicate in there that you don't necessarily have to have representative sampling if you've got measurement of the highest exposed group.

And then the collection of samples after the identification of incidents, if you've got just incident data, as long as your unmonitored population is also represented by those data. And that's the critical

part here. So, representative sample of the exposed population or workers with the highest potential for exposure.

Okay. The next is the analysis and application to the unmonitored population. And here is there sufficient data to construct a representative coworker model? And the criteria recommends the use of 30 workers per interval; however, less data can be used if the data fit a distribution reasonably well. In other words, if you've got 20, 25 workers and you put them on a log probability plot and it's following a straight line, well, do you really have to have 30? Is it really going to change a great deal if you had 10 additional? Probably not. So the 30 is a guideline, which is what this draft guidance is, is just that, a guideline. We try to stick to that 30 as best we can. And, in fact, we will expand the time period first before we will reduce that.

And can the data be reasonably represented by a statistical distribution? We use the time-weighted one person/one statistic. And this is emphasized within the draft criteria, and this was the result of multiple discussions of the SEC Issues Work Group and from that discussion. And this is when multiple bioassay samples are present during a monitoring period -- a year -- for a given individual. It is appropriate to average the values so that a single statistic can be computed for that individual, because in some cases you've got a person who's involved in an incident and they might have 10 bioassay samples. And so, if you only have 30 for the entire population, or let's say 40, and one person has 10 of them and the others each have one, well, then that person is going to be dominating your coworker distribution. So, by using the one person/one statistic you've weighted each person, each coworker equally going across that distribution.

The time-weighted gives some credit for when that exposure occurred. Was it uniform across the year or was it halfway through the year or something

along those lines? And that's where all those discussions with the SEC Issues Work Group came from, and we settled on the time-weighted one person/one statistic.

And this is part of why I believe the SEC Issues Work Group wanted to see this implemented, because these were a lot of concepts that were discussed at the time -- and we would give examples -- but how does this all play out? And so that's what was wanting to be observed, is the implementation.

And, finally, the evaluation of stratification should be conducted. Where there's accurate job categories or descriptions that can be obtained for all workers, there's reason to believe that one job category are is more highly exposed than the others, and there are unmonitored workers in that job category.

And this actually begins to play a role more in some of the external coworker models that we're beginning to try and look at and develop where we've got a high exposure going on, and we go through and we start looking at the job categories. Should this group be pulled out? And we begin to see things like everybody in that group was actually monitored on the external side for neutron exposures, for example. And you go through and even the secretaries in that group were monitored. So, from that standpoint, do you need to stratify? Well, no, everybody in that high group is monitored. They can be moved out of the actual coworker.

I put a note in here that stratification by individual job categories was never our intention, never Jim's intention, nor mine, or anybody's intention from the standpoint of coworker models. We never intended to get down to that fine of a grain of having a coworker model for pipefitters or a coworker model for operators, along those lines. It was more of general categories.

Co-Chair Anderson: Is that text in here?

Dr. Taulbee: It's not.

Co-Chair Anderson: No, I didn't --

Dr. Taulbee: No, it's not. But that's why I put it in here as a note, that it was never our intention whenever Jim wrote this evaluation criteria. He mentions it in here, job categories, but it was never intended to do a coworker model for every job category there is and to evaluate every job category. It was more groups, along those lines. Does that make sense?

Co-Chair Anderson: Yeah, but some of the job categories, that's a group of people. I mean, when you say group, if you're saying this -- I mean, the group can be pipefitters, it could be all construction workers, which has a much broader job classification.

Dr. Taulbee: Right. In the context here -- what I recall Jim meaning, and we're meaning, was construction trades, like you were talking about, would be a group, but not within that group of going much further than that. Now, maybe there is a group in there that we know about or somebody has some knowledge about, but to go through and evaluate every single construction trades category, that wasn't the intention.

Co-Chair Anderson: Not everyone?

Dr. Taulbee: Exactly. It was more of --

Co-Chair Anderson: But you could have some of them, because if there's knowledge that this group did some kind of unusual things where others did not --

Dr. Taulbee: Exactly. Yes. Yes, and --

(Simultaneous speaking.)

Dr. Taulbee: That is right. We did recognize -- the whole purpose of the stratification was along that lines of there could be groups of workers who are

different. And if we had some knowledge of that, evaluate and look at that. That was the intent.

Okay. All right. So, any questions before I go on here with the actual implementation?

(No response.)

Dr. Taulbee: All right. So, with the SRS coworker models, a priori we decided to stratify all construction trades workers versus non-construction trades workers, which is effectively all other workers. Okay? Everybody we could identify as a construction trades worker we put into one group and everybody else went into the other.

As I indicated, there was a lot of discussion about differences in monitoring methods, frequency, the exposure potential, high versus low, duration of the exposures. This was all done during previous Work Group meetings along this line. You can recall that we presented RPRT-53 in the past and there was no real consensus on a quantitative approach. Okay? We showed different comparisons and there wasn't any agreement, really at, that time.

So, instead of getting bogged down into that, that's why NIOSH -- we settled on the qualitative exposure potential differences as the basis for stratification. So this is effectively professional judgment. And the construction trades/non-construction trades centers around, from our standpoint, routine operations and non-routine operations, that that's the actual strata, in a sense. Okay?

And the reason was we found it was difficult to make an argument the exposure potential was similar for these two types of workers. If you think about it from a practical standpoint of a plutonium glove box line, you've got operators who are working on that line, they're putting their hands in the gloves. They're protected from the plutonium due to the glove box line, due to the engineered controls.

They're not wearing respirators. They might have an escape respirator around their neck, but they're not wearing it. They're relying on that engineered control, that glove box, to protect them from the plutonium. That's what we call a routine operation, people who are doing that type of work.

Conversely, we need to do a maintenance job within the glove box. So, the operations folks leave the room, construction trades workers come in, they take the face off of the glove box. They build a hut around it first, obviously. But they're in there with full-face respirators on, their bubble suits, their full-line respirators. So now that barrier of protection between that worker and the plutonium is something PPE. It's more personal protective equipment. It's not an engineered control.

So they're breaching that particular glove box. This is a non-routine operation. This doesn't happen all of the time, but this is the type of work that construction trades workers would be doing. They'd be going into these rooms and doing this type of work. So, this was our basis for the stratification for -- and it's not just pipefitters that would be doing this. It would be sheet metal workers, it would be carpenters. Laborers come in.

We did an interview -- I think Brad might remember that -- down at Savannah River, going into the building trades to talk about the tritium work. And they walked us through taking apart one of these glove boxes. There were 14 different trades that went into that hut in order to do different work, from boilermakers who had to do work with pressure lines -- and so they're all working together at different times here into this hut. Okay? That's the non-routine operations that we're talking about. Does that make sense?

Co-Chair Anderson: Yes.

Dr. Taulbee: Okay. So, previously we tried to quantitatively compare these two strata, construction trades and non-construction trades,

based upon this routine/non-routine work. And this was critiqued by the Advisory Board and SC&A. And Dr. Melius had mentioned at the time that he thought it was going to be generalize on that because there are so many different situations that might change the evaluation of that statistical analysis. And the reasoning why I think he was coming to that is because there's more that needed to be considered than just that quantitative analysis.

So we couldn't come up with a single statistical analysis to be identified a priori, so we left it for professional judgment. Now, here's the reality and the practical side of things. The initial construction trades worker and non-construction trades stratification of the coworker model, that was the hard part. Okay? Before we had been doing small little groups and trying to compare, but when we did this on a global scale, this was the hard part. This was going through all of the individual models and separating construction trades and non-construction trades. Okay?

But once we did this -- and part of why we did this -- is if the SEC Issues Work Group and the SRS Work Group disagreed with our stratification methodology, it's fairly easy for us to put it back together. It's much easier to take two strata and combine than it is to go through and actually stratify them. So, if further stratification is needed, a lot of the work's already done. But if we need to put them back together, that's really fairly easy.

Now, putting them back together could result in a better statistical analysis if the two groups are truly the same; or worse if they're different. So, it could go either way. All right? We don't really know. I will that, from looking at the intake models that we've got, I don't think that they will get worse. The intake models at the end that are in OTIB-81 are quite similar between the two groups.

What remains unclear to us -- and this is based

upon some of the mixed comments we've gotten -- is the recommendation of these respective Work Groups as to whether we need to stratify, as to whether no stratification is needed, whether we should stratify on construction trades and non-construction trades, or now possibly subcontractors or non-subcontractors; meaning people who work for DuPont as only one group and people who didn't work for DuPont in another, at least at SRS.

So we've demonstrated, I think, in this coworker model that we can stratify, that the data is sufficient to do it. The question is whether we should. Do we need to? And please note our preference is to not stratify. It makes it more simple from a dose reconstruction standpoint and there's less professional judgment.

And here is a question that I have for the two respective Work Groups, Dr. Anderson and Mr. Clawson, is the issue of stratification is a dicey one. It's one that is difficult. And we're going to be struggling with that somewhat today. Can we postpone it for the rest of this discussion of the coworker model implementation and address it during the comment resolution component and RPRT-92? Which will be later today and all day tomorrow, or the half-day tomorrow.

And the reason I wanted to do that is for the following. Let's see if we can agree on the elements of the rest of the draft implementation guide and the evaluation methodology that we've implemented on a non-controversial population; say, non-construction trade workers, all the operations workers at Savannah River. Did we take that group, implement the coworker model appropriately, is what I would ask if we could do that. Then we'll get to the stratification issue last. Is that acceptable?

Member Ziemer: Can I ask a question before we decide that? So, at this point, can you identify sort of a primary issue that would be the downside of stratification? Or are there multiple issues that are

of sort of equal weight?

When you say you would rather not stratify, that, in a sense, is because there are some issues on stratification that are sort of downside issues. I think that's sort of inherent in what you're saying. Is there a single downside issue on stratification or are there just a lot of different multiple issues and they'd be different in every case?

Dr. Taulbee: It would be different in every case. And let me give some examples as to why. The biggest one is the time that it takes to go through and identify --

Member Ziemer: That it takes? Okay. Well, that's always a downside, yes.

Dr. Taulbee: And the second one is at Savannah River we have the ability to stratify of construction trades, non-construction trades, subcontractors, et cetera, because we have work history cards for everybody. At other sites we don't necessarily have that.

Member Ziemer: Yeah. See, I would rule out the time issue if, in fact, stratification gave you a better answer. But if it doesn't make much difference, you know, then go with the non-stratification.

Dr. Taulbee: Right. And that's where I want to show toward the end of this presentation. All of the end result of this is going to show that -- or I hope show -- that the coworker model is claimant-favorable in the way that we've applied it and the way that we've developed it, to where when we get into the stratification, how much is this really going to change? And it --

Member Ziemer: Yeah, I mean, that's clear with your final issue, but I'm trying to be clear whether or not you gained anything to speak of by stratifying. Is it like in most cases we don't or occasionally we do?

Dr. Taulbee: Well, this is the first one we've done from the standpoint of trying to stratify.

Member Ziemer: Okay.

Dr. Taulbee: This is the first one that's been done this way.

Member Ziemer: Yeah.

Dr. Taulbee: But I can see at other sites where this is going to be much more difficult, which is part of why our preference is to not do it, because it involves a lot of data capture. And this took us -- this took about a year longer than I thought it was going to take, the development of this coworker model, to be quite honest. I thought we would have this too you all late 2017-type of timeframe, and it wasn't until March of 2019 that we actually got it out. So that's the biggest downside, to answer your question.

Member Beach: So, Tim, I have a question, too. You talked about professional judgment and if you stratify you would be using more professional judgment.

Dr. Taulbee: Yes.

Member Beach: Can you give us a little more on the professional judgment that would be required and how you would be documenting that, because I know we're --

Dr. Taulbee: It would be on the individual dose reconstructions of whether this person met this particular -- the strata for this criteria. And so that's where the dose reconstructor would be looking at this particular claim of, let's say that they were a -- I believe SC&A is going to use an example of general services operator. And so, this person, was it an operator, was it an construction trades worker? And you got to look at more information about that. So that's an example of professional judgment of when we get an individual claim to apply to this.

Member Beach: Okay. And speaking of this, so, when you're stratifying you may be using more professional judgment than if you're not stratifying. And you felt that that was a downside to stratification? Reading your slides that's kind of what I took out of it. Maybe I'm wrong.

Dr. Taulbee: No, I'm not sure I would say that there's more professional judgment in the initial development. I mean, there's going to be some, obviously. The health physicist who's developing the coworker models and developing the strata is going to be -- they're going to have to have some basis and they're going to have to document as to why --

Member Beach: You just mentioned that a couple times and we're kind of sensitive to that professional judgment and trying to track it more, moving forward, than we have.

Dr. Taulbee: Sure. I understand.

Member Beach: So I wanted a little more info.

Dr. Taulbee: Okay.

Member Beach: Thanks.

Co-Chair Clawson: And this is Brad speaking. And so stratification on this, now, you're wanting us to say to just kind of push this to the side right now. This is just for this presentation?

Dr. Taulbee: Yes.

Co-Chair Clawson: Because one of my things was is this part of our issue from the very get-go of how we were going to do this.

Dr. Taulbee: Yes.

Co-Chair Clawson: I have no problem of proceeding on with that, but I'm not agreeing to not -- or to go -- but for this presentation we put this to the side, correct?

Dr. Taulbee: This is just for the other components of that draft criteria that we talked about. Just so that you can see the full coworker model being developed and how we do it without thinking of this strata and that strata.

Co-Chair Anderson: Yeah, I just don't want to push it off --

Dr. Taulbee: No, no, no.

Co-Chair Anderson: -- and then we run out of time for what's really the critical part, is the discussion.

Dr. Taulbee: No, no, no.

Co-Chair Clawson: Because this was one of the issues from the very get-go.

Dr. Taulbee: Yes.

Co-Chair Clawson: So I just want to make sure --

Dr. Taulbee: Yes, absolutely. So, for the next however many slides here, just the operations, the mechanics of us implementing the draft criteria.

Co-Chair Anderson: Because there's always the possibility you can't do a coworker model as opposed to now we've got -- because you're saying it takes too long and it's been years and years. The alternative is it's not feasible --

Dr. Taulbee: That is correct

Co-Chair Anderson: -- to make the people wait for this.

Dr. Taulbee: That is correct.

Co-Chair Anderson: Savannah River is one of these that's been --

Dr. Taulbee: And we very may well run into that from the standpoint of a particular radionuclide for one of the construction trades strata, that we can't implement this guide sufficiently. But what I've

picked here is operations workers, and I'm going to go through the plutonium example, which is where we have lots of data so that you can see the mechanics of how we implemented the coworker model and see if we can gain agreement based upon that. And then we can start looking at the other things under the strata standpoint. Does that make sense?

Co-Chair Clawson: Yes.

Dr. Taulbee: Okay. And as I believe Ted mentioned earlier, another reason why we wanted to do this was we do have several coworker models under development: Idaho National Lab, Fernald. And then there's other sites where we need to update this TWOPOS methodology. So if we can get the draft criteria accepted, finalized, then we can continue work on it. We're working on them now, but it's under draft guidance. And so we'd like to implement this.

It doesn't mean we can be successful in all these cases, to answer what Brad's talking about, but this is the baseline, this is the target.

Okay. The other goal, the final point, is if we need to change something here as I go through it, something that you all don't like in how we implemented it, please let us know now so that we can make that change and move forward.

All right. So, for Savannah River -- and if we just think of the non-construction trades workers here -- OTIB-18 is a bounding approach and it actually takes care of a large number of the claimants who would need a coworker model. At Savannah River, from an operations standpoint, a large number of the workers are monitored. I mean, you're looking at over 80 percent of the actual workforce is being monitored. And so OTIB-18 for those who are not monitored we tend to use as a bounding estimate.

But our goal for a coworker model for SRS is to supplement OTIB-18 as a best estimate for

coworker models. Okay? There are some cases where we need a best estimate. But we never know which radionuclide we need it for, so we developed a coworker model for all the major radionuclides at Savannah River. And that is these nine. The trivalents: americium, curium, californium. At some sites they're called exotics. Savannah River it runs both ways as to whether they're exotic or whether they're production. Tritium, plutonium, uranium fission products, strontium, cobalt-60, cesium-137, neptunium, and thorium. Okay?

So this was what we needed to develop. And that is what is in OTIB-81, the 258 pages, is a model for each of those radionuclides. And just to throw it in, OTIB-81 is a model -- a non-construction trades worker model and a construction trades worker model for each of those radionuclides. Okay. Because we did do that strata. All right. But we're just talking about the non-construction trades right now.

And what you'll find going through OTIB-81 is that the format closely follows the Coworker Implementation Guide, or that draft criteria, of data adequacy where there's a discussion of the personal monitoring, the applicability to unmonitored workers, the bioassay analysis techniques. Under data validation the data completeness and quality, how we interpreted the data and what data we excluded. Okay?

So I'm going to go through -- oh, and this next -- the other steps are statistical analysis, the development of the TWOPOS, time-weighted, one person/one statistic values, and then the intake model. So I plan on taking you through all five of these with plutonium. Okay?

So with plutonium monitoring who is monitored? Bioassay control procedures were started -- we have those documented from 1968 going forward and they identify the types of workers and the frequency of monitoring within the specific areas.

For operations there's a lot more detail. For construction trades, it was monitored every one -- one plutonium sample every three years. Okay?

The applicability to the unmonitored workers. The number of workers monitored was relatively constant over time. There weren't any temporal gaps that we noted and the workers with the highest potential for exposure were monitored. Well, how do I know that? And that is looking at this particular table, Attachment C, which I see now you can't look at or can't see very closely, but what you'll see is at the very top, first block is minimal potential. And this is for people working in tritium facilities and 723, the 305-M areas, like 100 areas and like 773-A areas.

For plutonium -- these were non-plutonium areas, by the way -- they were monitored once every three years. If you go down to 221-F and H areas for Savannah River, this would be -- these would be the canyons. And so this is where the depleted uranium targets that had been irradiated were separated, fission products were separated out and the waste was sent to the waste tanks. Uranium product went to the A-Line; the plutonium product went to the B-Line. Okay? So this is the main canyon. High levels of gamma radioactivity in there. People didn't go into canyons. They would draw samples, but they were less exposed than the next group that I'm going to talk about.

And these are the actual plutonium workers. These are the people on 221-HB Line, FB-Line and JB-Lines. These are the plutonium lines. This is where they separated out the plutonium, purified it, made the plutonium buttons. And so this is the final product lines coming off.

There's three other areas there: 235-F, 773, the main research divisions. These workers were monitored for plutonium four times a year. So right now we've got three different frequencies of monitoring. People working on the plutonium lines

were monitored four times a year, people who worked intermittently in the canyon areas where they might draw a sample, where there's lots of fission products associated with plutonium are monitored once a year, and people who are more office-type workers who might go through the areas occasionally are monitored once every three years. Okay? So this is why we feel we know that the highest exposed workers were monitored.

Member Beach: So, Tim, quick question. This says 1976. Do you have '72 through '76 samples or just --

Dr. Taulbee: We have 19 -- we have these tables from 1968 through -- well, when they actually changed the criteria in the 1990s. And they're all here in the appendix, Appendix -- Attachment C of the bioassay, of the coworker model. Okay? So this is why we feel we have a good handle on it.

And if we also think about the whole discussion of the time-weighted OPOS as well, one person/one statistic, here you've got the highest exposed workers four times a year. Their samples are going to dominate, especially if we didn't take it to one person/one statistic, from that standpoint. Okay?

All right. So next we looked at the bioassay analysis techniques. And from 1954 they did a bismuth phosphate lanthanum fluoride coprecipitation. And then in 1959 there's a nitric acid/hydrogen peroxide dissolution and ion exchange. 1966 started the TIOA; because I am not going to try and pronounce that part, liquid extraction. And then in 1981 there's a coprecipitation technique with alpha spectrometry.

We looked at the reporting level. The reporting level or censoring level was 0.1 dpm per day. And I got a bolded note here that this is a reporting level not necessarily the limit of detection or the minimum detectable activity. This is what the site reported. So if a value was less than that on their bioassay card, they wrote less than 0.1. Okay? And here is a snapshot of plutonium logbooks. I believe this is

1980 here. And, gosh, that is hard for you all to see. I apologize.

Mr. Calhoun: You can maximize it. It's pretty easy to see.

Dr. Taulbee: Oh, I do this?

Mr. Calhoun: Yes.

Dr. Taulbee: Awesome. Thank you, Grady.

Mr. Calhoun: Oh, it did that, too? Oh, I wasn't even looking at that. I was just looking at my screen.

(Laughter.)

Mr. Calhoun: That's why I get paid the big -- it worked for me.

Dr. Taulbee: Awesome.

Mr. Calhoun: My work is done here.

(Laughter.)

Dr. Taulbee: Thank you. And what you'll see here is this is a blowup of this area in this upper region right here. And you can see this value here is 0.029, zero point -- is that another 0.029, 0.023, 0.064. They're all less than 0.1. So on the bioassay cards we see less than 0.1, but in the logbooks we see the uncensored results.

Member Lockey: What was the standards then for that, exposure standard for that? Do you know?

Dr. Taulbee: There really --

Member Lockey: Well, wait. In the -- the idea was to keep it at as low a level as possible. So what would have been the guidelines in that time period for internal dose, what were you -- the 1980s?

Ms. Brackett: You mean like the limits for intakes? '90s?

Member Lockey: 1980. '80.

Ms. Brackett: '80 it was 50 percent a maximum permissible body burden.

Member Lockey: '80?

Ms. Brackett: '80. Yes.

Dr. Taulbee: No, 1980.

Member Lockey: 1980.

Ms. Brackett: Nineteen --

Member Lockey: 1980.

Ms. Brackett: 1980. Yes.

Dr. Taulbee: So they used the maximum permissible body --

Ms. Brackett: It would be maximum permissible body burden it was based on then. It wasn't based on a dose. It was based on body burden.

Member Lockey: And what was that? Do you know what that would have been?

Ms. Brackett: The specific value for plutonium? I don't know that off the top of my head because it's different for every --

Member Lockey: Right.

Ms. Brackett: -- nuclide.

Dr. Taulbee: I believe it -- I'm not sure I --

Ms. Brackett: And that would be a body burden, not a urine sample, so --

Dr. Taulbee: Yes.

Ms. Brackett: -- it's dependent on when the intake was. There's -- which is why they converted to dose at some point because it was difficult to --

Dr. Taulbee: To do at that time.

Ms. Brackett: -- to compare this to say external dose, to give you a good value.

Member Lockey: What I'm trying to get a handle on at this level, how do you relate this level to a safety issue?

Ms. Brackett: I mean we don't. I mean we take a result and then make some assumptions and come up with a dose at this point. At that point there would have been a value that the site -- they would have done something to say, okay, 50 percent of a maximum permissible body burden would give me this much in a urine sample. Like at ONRL there was something that called an excretion index, and so that would be related to the body burden. But I don't know that value off the top of my head. But there were --

(Simultaneous speaking.)

Member Ziemer: Well, typically it would be much higher than this.

Dr. Taulbee: Yes, it --

(Simultaneous speaking.)

Member Ziemer: This was like a lower limit of detection or -- but you're saying it really isn't that. It's --

Dr. Taulbee: Exactly. This would be their action level, effectively where they would go and begin to assess the dose for an individual. So if it was less than 0.1, they didn't worry about it.

Member Lockey: So the action levels are set to a factor of 10 to 100 to 1,000 lower than --

(Simultaneous speaking.)

Dr. Taulbee: I believe this is probably at least a factor of 10 lower than what that 50 percent level

is, but I don't know that it's that's translatable.

The other thing I wanted to point out here is if you look over here into the remarks, you can see that -- where the zero had been changed to 0.046. Okay? And what they're doing here is a low-volume correction, because off to the left you'll see the 300 milliliter was their standard volume, and this one here was only 175. So they ended up taking this dpm per disc, converting it up where they would do this -- they did this correction to it and came up with this value. So this was still less than 0.1, and so that's what they reported on the person's bioassay. Okay?

So this will become important for the next discussion, the next part here. Actually I want to make sure that -- yes, coming up is where that's going to become important. Most of the measurements were gross alpha for plutonium. During the 1980s they did report both plutonium-238 and 239. Those were reported separately. Liz's team merged them into a gross alpha, a single gross alpha measurement assuming -- assumed to be 12 percent of 10-year aged plutonium. And this was chosen to be claimant-favorable.

Following along, the draft criteria is talking about data exclusions, what were taken out of the dataset. Well, we took out all of the chelation samples, people who underwent chelation or had an indication of DTPA use, because for one thing that's going to really mess with the biokinetic models that I'm going to be working with, or showing you shortly here. So those samples were removed.

Co-Chair Anderson: Those are likely to be the highest?

Dr. Taulbee: They are, but it -- I mean it depends. It depends. But what ends up happening is the bioassay results become --

Co-Chair Anderson: Yes. No, I understand, but I'm just saying there -- potentially is there a bias, and

do you exclude these -- if you were chelated --

Dr. Taulbee: Right, but if you --

Co-Chair Anderson: -- there was likely a reason more so that --

Dr. Taulbee: But you might also think of it from the standpoint of these are extreme accidents, from that standpoint, extreme exposure scenarios that -- keep in mind the unmonitored worker would be somebody who there's no indication they were ever involved in one, that there was -- that contamination control was --

(Simultaneous speaking.)

Co-Chair Anderson: So it was only when there was an accident that they were chelated?

(Simultaneous speaking.)

Dr. Taulbee: I can't think of a reason you would ever do that.

Co-Chair Clawson: If they chelate somebody, that's a kind of last resort.

Dr. Taulbee: Yes. So it's -- yes, it's not done normally.

(Laughter.)

Co-Chair Anderson: No. No, I know.

Dr. Taulbee: There were some that we didn't have sufficient identifying information, and so those were excluded. Some samples within the datasets were given in units of mass. And these were likely fecal samples, not urine, so again, that changes the bioassay monitoring or the intake modeling methods. So those were excluded as well. And for each radionuclide in OTIB-81 we went through and explained what was excluded, what was in there. So this is where we feel we're following the draft criteria and meeting the intent.

Data validation. And this was one of the things that took a lot more work than what we had initially thought it was going to take. And we used the NOCTS in vitro dataset. And it contained plutonium, uranium, enriched uranium and fission products. And early on, we had to set the criteria of what are our acceptance rates. And so for critical fields we decided on one percent, all other fields five percent. These would be basically transcription errors is what we're looking at here.

And keep in mind that these initial datasets that we used from NOCTS, these were coded to assist the dose reconstructor. They weren't coded for coworker models initially. Okay? And so the dose reconstructor is who does the final QA typically on these data when they're doing the dose reconstruction, but there's times when a dose reconstructor has a lot of external dose. They don't even look at the internal dose because the case is compensable on the external, and so it's never reviewed or looked at, or maybe not even entered. And so we had to go back to these -- to multiple claims and enter the data for the first time. Okay? And so we did that.

Critical fields were determined to be the isotope. Did we get the isotope right? The less-than symbol, which is really just part of the result. Those were the three critical fields that we felt that needed to be correct from this standpoint.

And so in order to do this -- and this is where Nancy and her team came up with how to do this, the procedures to do it, and wrote some reports about it. And because we have a large dataset, we needed to check 4,386 samples. And there were 11 errors. So are these datasets perfect? No. We didn't expect them to be perfect, but this is pretty darn good from that standpoint. And so the actual error rate of the critical field was 0.25 percent with a confidence interval of 0.13 to 0.45. And that confidence interval is a 95th percentile confidence interval.

All other fields: last name, first name, middle initial, payroll ID, date, units, area, there we checked less because we were requiring a five percent error -- or we were allowing up to a five percent error. And here we checked 874 and we found four errors within that.

So we did this for each radionuclide that we did a coworker model on, or actually not for each radionuclide. For each dataset that we used. Each dataset. Okay.

The next step was the time-weighted one person/one statistic methodology. Again, this followed RPRT-53, which is the analysis of stratified coworker datasets. This is where all the background came up. Started out as OPOS, one person/one statistic. We changed it to time-weighted one person/one statistic based upon SC&A's comments. And these time-weighted one person/one statistic data are fitted to a log-normal distribution during this analysis.

Most of the bioassay data is censored. Data is reported as less than some value. In the case of plutonium it's less than that 0.1. Most of it is less than 0.1.

Member Lockey: Most meaning what?

Dr. Taulbee: I'll show you that in just a second. Analysis method that we used to -- for the censored data was we used a multiple imputation, and this is RPRT-96. And we'll talk more about that this afternoon -- or not this afternoon, later this morning when we get to the comment -- our responses to SC&A's comments about multiple imputation. But this is it in a nutshell. Okay? And to answer your question there, Dr. Lockey, this is for 1969 plutonium. There are 892 plutonium bioassays for the non-construction trades workers, the operations folks.

Dr. Chalmers: No, this is all workers.

Dr. Taulbee: This is all --

Dr. Chalmers: All workers.

Dr. Taulbee: Okay. I'm sorry. This is all workers. Okay. Thank you, Nancy.

Eight hundred and ninety-two. And we have 217 that are uncensored. So as you can see, there's a large number that is censored within this dataset.

What we do is for multiple imputation, we'll fit the upper tail of the data; and as you see it fits fairly reasonable, and assume that all of the censored data follows along that line. In the calculation of OPOS -- or TWOPOS, I'm sorry, what we will do is if an individual has all uncensored data -- and now moving to the plot on the right, to where they have no censored data, that's those black dots up there at the top, which you'll see up here. Okay? These are people that don't have any censored data.

People who have less than 50 percent of their data as censored -- in the calculation of OPOS remember some of these workers are monitored four times a year. They might have two of them that are positive and two of them that are not. Okay? And so in that case those two censored values, we would go to the plot on the left and draw samples, less than 0.1, somewhere along that log-normal, and plug them in.

So now let's say they have four samples: two censored; two not. We draw two values from that imputation model and calculate the TWOPOS value for that person. We go to the next person, do the same thing. Some of these people, the yellow dots, have 100 percent censored results. Now, most -- many of those are a single result and we'll grab one value from that left plot to calculate their TWOPOS value.

The plot on the right is run Number 1. We do this over and over and over again imputing these values, imputing the censored values, calculating

the TWOPOS value over and over to where you end up with the plot there on the left and you get this scatter about them. And this is the values that we fit, or this is the TWOPOS plutonium plot for 1969 for non-construction trades workers. These are the parameters that we will use to develop the intake model. Does that make sense?

Member Beach: Yes.

Dr. Taulbee: Okay. I've put 1970 off to the right so that you can see how these look. And as you can see down here at the bottom the spread gets larger, as one would expect. Okay?

All right. So now that we've got these TWOPOS values and we've fit this log-normal distribution to each year, we have -- in this particular case this table I've just taken 1967 through 1970. You'll see that the non-construction trades 50th percentile is 0.036 dpm per day. And by the way, that's coming from right up here, 0.036, and GSD is 3.14, or 3.42. Yes. Okay?

And we -- so off of that plot we pull the 50th percentile and the 84th percentile. And why do we do those two? A) it makes the math really easy, for one, to calculate a geometric standard deviation. Okay? And so for 1969, if you take the 84th, divide it by the 50th, you get 3.14 for our GSD. And this was 296 individuals. Okay? So this is our TWOPOS values. What we'll do next is we'll model the intake based upon somebody being exposed here at the 50th percentile and somebody exposed at the 84th percentile. Okay? All right.

So the next step, once we get these TWOPOS models, these TWOPOS distributions is we model the intake for each of the nine radionuclide categories. We do the 50th and 84th percentiles for each year, and we'll re-look at in a group year. We look at it by solubility type and then we'll look for similarities of time intervals of similar results. And this is where the internal dosimetrist makes their money. This is where they earn their pay, I should

say. There's a lot -- there's professional judgment in here. This is a skill. It is a rare skill in this country, but it is a very valuable one and one that is necessary.

We assume a chronic intake scenario for each time interval to determine the intake. So let me walk you through this.

Time interval Number 1 is those blue dots, those first six years where we're doing this. And this is -- these are those individual TWOPOS values, the 50th percentile of those TWOPOS distribution. That distribution that I showed you previously -- well, it was actually 1969. It's this one right here. But what you have here for this particular year is a whole 'nother plot. You have one of these plots and we're taking the 50th percentile here. Okay? That's what this plot is, or that's what that data point is. That's what that represents.

So we take these six years. We look at this as if this was an individual's bioassay and what intake would they receive of plutonium to get that result, assuming chronic intake. This would be type M plutonium, all right, down here. The line here is what their excretion would be if they started on day one with that intake and stopped in the last data point. This is what their bioassay would look like. Okay?

Now we move in time to the next interval that seems to fit. This is data that seems to be lower than the previous six results, and so we fit a different intake model, or a different intake rate. It's got a different dpm per day intake rate. We do this again, because in this time period starting in about 1967 there appears to be an increase based upon what we see from those TWOPOS results. And so we fit this one. And then in 1971 to 1981 we fit this. Then in the latter years, 1982 to 1900, it's low and it fits this. Okay?

Now put it all together of somebody who's exposed over this whole time period. Oh, I'm sorry. Back up.

Got ahead of myself. Apologize here.

The -- so this is the intake tables that we come up with. These start and end years are the different time intervals I just through with you. The 50th percentile is the 50th percentile intake. So 3.265 dpm per day, that is the intake for each of those that I just showed you going down. A high one, the 67 to 70, is 5.778 dpm per day. We repeated all of these steps.

These plots here were all for the 50th percentile. We did this for the 84th percentile data as well. We went back to that plot, picked off the 84th. There you see the 84th percentiles alongside. And you can see the 84th for that 1967 is a 20. We again calculated the geometric standard deviation of the intake rate because this is -- the intake is what we assign when we do dose reconstruction.

The adjusted geometric standard deviation, if one of these geometric standard deviations is less than three, we increase it to three. That's the minimum that we use based upon guidance from the ICRP for internal dosimetry, which is where you see 2.98. That changed to three. Based upon the 50th percentile and this geometric standard deviation, we calculated the 95th percentile. Okay? So this is how we developed the intake table.

Now if you plug in this intakes -- these intakes for somebody who started in January 1955 and ended in December of 1990, this is what their urine excretion would look like, the green line, summing them all up. If they started in a later time period, say, out here around 1971 time period, yes, this line won't be up here. It will be somewhere encompassing these data points. That's the goal is that that excretion will encompass these data points.

Is it always? No, but it's close. Okay? So our goal is to get an intake to the unmonitored worker, keep in mind. This is an unmonitored worker. We're taking all the monitored workers, putting all of the

bioassay together to come up with an intake model to assign.

This is the 84th percentile that I'm showing you right now. This is what this curve looks like, and you can see it's above those data point as well. That censoring level of 0.1 is right there. So if somebody started in 1955 and went through the end to 1990, of an unmonitored worker, and we assigned this intake rate, if they had in fact been monitored, they would be showing positive bioassay every year from like 1967 or so time period. That's -- it's claimant-favorable built in here, but it's not unreasonable. Okay? We believe this is favorable but sufficiently accurate. It's high, yes, but it's acceptable.

Member Lockey: The green line is what again? That's 84th percentile, right?

Dr. Taulbee: Yes, this is 84th percentile. This would be the -- if you go back up to these plots here, this would be where the standard normal quantile is one. It's this value here.

Member Lockey: Yes.

Dr. Taulbee: Okay?

All right. Now let me get back to where I was. So here's another way of looking at this one: the box plot here, the lower error bar here, is the 5th percentile, 25th percentile, 50th percentile, 75th -- oh, wait a minute. I'm sorry. It's 10th, isn't it?

Dr. Chalmers: No, I think we did 5th and 95th.

Dr. Taulbee: Fifth and ninety-fifth? Okay. And then the 95th percentile. Okay? And so the red dots are the geometric mean of our intake overlaid with all of the TWOPOS values, the TWOPOS distributions. So you can see even our 50th percentile is still high but not unreasonable. This line here is the 84th percentile which I was pointing out. It's just -- it's this plot with taking these individual dots and turning them into the actual box plots that they

represent. Okay?

So we did this -- oh, one of the things I wanted to point out is these are the plutonium intakes from SRS over this time period. These are all of the positive dose assessments that they've done.

This is hopefully to help answer your question, Dr. Lockey, of if you take people who are all positive and they went through and they calculated dose, here's these dose values. And you can see that there's clusters: 1955, around 1960, around 1969-1970, this time period, and out here around 1987. The general trend though is the dose is going down. Most of these doses are between 100 millirem and 10 rem. And these are all of those positive ones that you see up there at the upper tails of these distributions. And this is committed effective dose equivalent. All right?

And by the way, just for -- can you tell when the Cold War ended and production stopped?

(Laughter.)

Dr. Taulbee: When they stopped making plutonium? It's pretty obvious.

So we did that for plutonium. We did this for americium. And here's this plot. For tritium. And tritium is of interest in that this is 100 millirem. So you get beyond 1980 and people aren't even getting 100 millirem.

Uranium. The reason I put all of these on here is this is type F uranium, solubility type, type M and type S. And what you'll notice here, type F, because it clears the body so quickly, the actual curves follow very closely to the actual -- to the bioassay. When you get into the slow solubility category is when you begin to see more of these buildups. The intakes are higher because that's how we end up doing the modeling of the intakes. All right?

This is cesium.

Neptunium. This one looks really interesting, which is why I put it in here. Up to 1970 we have urinalysis data up here. After 1970 we have very little urinalysis data. That's what this is right in here. We got big gaps. What we have is whole body county data. We went through, did the whole TWOPOS methodology that we just discussed using whole body count data and it results in a much higher intake. Whole body counting is not as sensitive as the urinalysis. So what ends up happening in the coworker model is we kind of have a step function happening here, but it's due to a change in the methodology.

I asked ORAU to go ahead and plot the urinalysis that we had here that was limited in this time period to see how does it fall with our model. And you can see the limited urinalysis we have is well below it. And so we believe that this is sufficiently accurate and claimant-favorable.

So to go through the major steps here we've got -- we start with the individual bioassay. We then calculate an individual TWOPOS value, individual -- each person there's a TWOPOS value that's calculated. We substitute that censored data using multiple imputation. We take all the TWOPOS values, we fit a log-normal distribution to them. We then go through the modeling, the 50th percentile, the 84th percentile. That gives us the intake that we assigned and then we apply it to the unmonitored worker.

So in words here it is just that: individual results are averages, so there's a lot of averaging going on here. Bioassay results from the individual worker are averaged into a single time-weighted OPOS. The TWOPOS results are fit to a log-normal distribution, so another type of averaging. The TWOPOS 50th and 84th percentiles are fit in IMBA to develop the intake rates. We look at the uncertainty of that intake rate, at 50th and 84th percentile, calculate a GSD. If it's less than three, we bump it up to three. And so that is how we come up with our intakes to

assign to workers.

Normally the 50th percentile with a full log-normal distribution will be assigned to workers who may be exposed to greater than environmental levels, but less than a typical operations worker. Okay? So these would be your intermittent unmonitored workers going into an area.

If a worker is considered to have a high potential for exposure for whatever reason -- their records are missing, it appears that this person may be at the end -- we can assign an upper bound of this in the 95th percentile on a case-by-case basis. This would be determined by a dose reconstructor. Does a regular log-normal distribution fit, which is most of the cases?

Keep in mind that at least for the operations workers at Savannah River about 85 percent of them have data themselves and we don't even need a coworker model for them. Okay? We use their individual data. This is for people who don't have any monitoring data or a big gap in their data and we're trying to supplement it. That's where this is used.

Member Beach: Have you figured out the highest group of non-data at Savannah River, which group of workers has no monitoring data?

Dr. Taulbee: From the operations side, none. None. I mean there's -- I mean that table that you saw there at the beginning of the people who worked on the B Lines and 235-F, they were being sampled for plutonium four times a year.

Member Beach: How about the subcontractor?

Dr. Taulbee: And that we'll get into with the stratification and the subcontractors. Okay? And this is part of why I wanted to keep it separated, is to go through the method --

Member Beach: Sure.

Dr. Taulbee: -- and see if this method is acceptable. And I think you just pointed out the area of where we need to have a lot of discussion is back into that applicability: were these people monitored? For operations? Yes. And we can demonstrate this. For others, maybe not. Okay?

So in summary, this example coworker model demonstrates how the draft criteria for the evaluation and use of coworker datasets would be implemented. We believe the intent of the draft criteria for evaluation and use of coworker datasets has been met and we believe the coworker models presented are claimant-favorable, claimant-friendly, reasonable and adequately bound potential doses for compensation purposes.

And with that I'll be happy to answer any questions.

Co-Chair Clawson: I think you wore us out.

(Laughter.)

Mr. Calhoun: It's way too early to be worn out, Brad.

Co-Chair Clawson: I know it. Well, actually a lot of the questions are from the other part.

Mr. Calhoun: The next part? Part 2.

Co-Chair Clawson: Yes.

Member Ziemer: Now let me ask SC&A, are you going to comment on this today or you'll do that later?

Mr. Barton: Right off the bat, I don't really have any comments. I mean we just saw this yesterday, but the mechanics of the coworker model have been in place for quite a while. I think the question here is -- and again, the next step of our review of OTIB-81, which parts of it may affect the implementation of the --

(Simultaneous speaking.)

Member Ziemer: Bob, do -- you did your comments before you've seen this, is that correct??

Mr. Barton: Yes.

Member Ziemer: Okay.

Dr. Taulbee: Well, they had OTIB-81.

Member Ziemer: Well, yes.

Dr. Taulbee: And they've written comments on that, which is --

Member Ziemer: Right.

Dr. Taulbee: I think the next phase is for Bob to go through his comments on this --

Member Ziemer: Right.

Dr. Taulbee: -- methodology.

Mr. Barton: And I think we're trying to keep two separate facets. A lot of our comments are specific to SRS, not necessarily how the coworker implementation guideline is -- were implemented. So a lot of our comments are going to be site-specific. A couple of them will apply if you were going to use the implementation guide at other sites, but only a couple of them.

Dr. Taulbee: Multiple imputation is the big one.

Mr. Barton: Yes.

Member Lockey: Let me ask you a question. If I was sitting in a data analysis room and I had this type of exposure data, it's elegant data and I --

Mr. Barton: It's massive amounts of data.

Member Lockey: I mean it's really eloquently presented. This is data that I could use to assign doses to people. Do you have any -- would you have any problems with how the model was designed?

Mr. Barton: No, not with the design, but again there's a lot of questions of representativeness; i.e., monitoring the correct people. And the unmonitored people, do we have information on their exposure such that we can apply this elegant data approach to that group? That's sort of a later discussion.

Member Lockey: That I understand, but in an ideal population this is a good approach to take.

Mr. Barton: I believe so, yes, except for -- and we're going to talk about the multiple imputation.

Co-Chair Anderson: Everybody was monitored.

Member Lockey: Well --

(Simultaneous speaking.)

Member Lockey: -- percent of them. Nobody monitors 100 percent. You can't do it. But if you monitor 50 percent or 60 percent, this is the type of data you would use. That's just reality. So I think from your perspective we have to reach a point where we say, yes, this -- sign off to this or you can use this someplace else. But your question is still a valid question. That's what I'm getting at.

Mr. Barton: Like I say, I think we're -- as we said at the outset we're trying to separate what the Implementation Guide says

and --

Member Lockey: Right, I understand.

Mr. Barton: -- what you'll be using at other sites, and the SRS-specific issues that wouldn't necessarily apply anywhere else.

Co-Chair Anderson: The guidelines are appropriate and they can be --

Member Lockey: That's what I'm saying. Yes.

Co-Chair Anderson: It can be

applied --

Member Lockey: It's been a draft for 10 years.

Co-Chair Anderson: Yes, exactly.

Member Lockey: I think this is the time, not make it a bad thing.

Co-Chair Anderson: Yes.

Member Lockey: That's what I'm saying.

Mr. Fitzgerald: Yes, I guess what I add to comment; this is going back to your line of questioning, I think we're always going to have the question of interpretation and application of the overall model to a specific site.

Member Lockey: Right. Oh, yes.

Mr. Fitzgerald: And actually SRS is offering a microcosm of that, but if we're comfortable -- and I think we are -- with the overall model --

Member Lockey: Yes.

Mr. Fitzgerald: -- we'll probably have that other discussion when it's applying to Idaho. And that's just part of the game.

(Simultaneous speaking.)

Member Lockey: But the general model is a good model. That's what I'm hearing.

Member Ziemer: It's really an elegant model.

Member Lockey: Yes, that's what I said. It's an elegant model.

Member Ziemer: I thought you said good.

(Laughter.)

Mr. Calhoun: Way to go, group.

Member Lockey: I guess good isn't good enough.

(Laughter.)

(Simultaneous speaking.)

Dr. Taulbee: Is there any -- I mean, SC&A is going to talk some about the multiple imputation component of it, but is there any other areas of this where -- that you see that there's something else we need, because I think we've implemented the draft criteria as requested and demonstrated that we can do this.

Member Lockey: One thing I'd like is that there is some judgment, is you don't use it to fit 50 percentile, but you use 95 percentile based on detailed data on that individual. That's what they're saying.

Dr. Taulbee: Eighty-fourth.

Member Lockey: Eighty-fourth percentile.

Ms. Brackett: Ninety-fifth.

Member Lockey: No, 95th.

Dr. Taulbee: On this particular part here, yes, we can.

Member Lockey: You can, sure.

And I think that gets -- that gives the evaluator some wiggle room --

Dr. Taulbee: Yes.

Member Lockey: -- because there's something unique about this particular worker which it has to take under advisement.

Mr. Barton: And as I point out there's sort of almost three tiers. There's the environmental level. That would be someone -- an administrative if they need to enter radiological areas, but they're on the site. There's ambient exposures. And you've got a 50th percentile. Normally we would consider that to be

moderately exposed, for lack of a better term, not necessarily your chemical operators and things like that.

But if you had an operations worker out there who again was either missing their data or wasn't monitored for whatever reason, most likely they were missing their data, then you do have that option of the 95th percentile. And that is applied as a constant, right? I believe.

Member Lockey: I think that's important, yes.

Co-Chair Anderson: And it took a couple -- I mean, it's a massive amount of work.

(Simultaneous speaking.)

Co-Chair Anderson: Well, but I mean a couple of years, so kind of the question is if you get to a -- and this was one of the bigger sites. When you get to some of the smaller sites, now that you have this, are we going to be looking at an additional two years? I mean you're working on a number of these. It's one thing if it's a --

Member Lockey: Trick question.

Co-Chair Anderson: Well, I mean it's a good --

Member Lockey: Trick question.

Co-Chair Anderson: -- design and it's very comprehensive. On the other hand -- and it could be very practical to use when you have lots of unmonitored workers at a -- not lots, but relatively needed as opposed to a smaller site to -- at what point is it inefficient to try to develop a coworker model?

Dr. Taulbee: Yes, and that we don't -- but I mean for us the critical part is trying to get the draft criteria turned into a guide, and then we can start looking at the other ones, other sites and where we would apply this.

We do have what, eight or so sites that -- I'm thinking of the 49 for Super-S.

Ms. Brackett: Right, that's going in.

Dr. Taulbee: Separate.

Ms. Brackett: Yes.

Dr. Taulbee: Right, going ahead without this?

Ms. Brackett: Yes.

Dr. Taulbee: But that have large -- or that have large coworker models that need to be updated TWOPOS.

Ms. Brackett: Oh, yes. More than eight.

Dr. Taulbee: More than eight that need this really bad, and we haven't done it.

Co-Chair Anderson: So the rest of your coworker is going to be --

(Laughter.)

Dr. Taulbee: Beyond ours --

(Laughter.)

Dr. Taulbee: -- to be quite honest.

(Laughter.)

Co-Chair Anderson: Oh, I mean that just -- I mean, having done it, is there -- it looked like there was a lot of hand work. I mean is it -- to the extent more of this could be automated.

Dr. Taulbee: Nancy's done a really good job of automating a lot of this in our -- with having standard codes set up.

Co-Chair Anderson: Yes.

Ms. Brackett: Yes, so we've been working on Idaho since this, and that's much more automated than

the Savannah River data were. So we have made progress on that.

Co-Chair Anderson: So hopefully we get -- in other words, what I'm asking is will it come to us time-intensive and stressing to the staff?

Co-Chair Clawson: But, Henry, this is -- goes to my caveat in the beginning of this, and that is data adequacy.

Co-Chair Anderson: Yes.

Co-Chair Clawson: If we don't have that, then --

Co-Chair Anderson: Yes.

Co-Chair Clawson: -- it's no good. And that's --

Co-Chair Anderson: And that's part of the criteria.

Co-Chair Clawson: And that's what I -- that's one of the things to remember, that when you start getting smaller sites, you were asking and so forth like that, if we do our due diligence up front --

Co-Chair Anderson: Up front.

Co-Chair Clawson: -- and get that taken care of --

Co-Chair Anderson: Yes.

Co-Chair Clawson: -- we may not even be able to use this. And so that's where the SEC comes in.

Mr. Katz: Gen and I don't know if, David Richardson, if you've joined us, and Phil, do you have any comments, questions at this point?

Member Roessler: Yes, this is Gen. Am I off mute?

Mr. Katz: You are. We hear you perfectly.

Member Roessler: Okay. Good. Well, that was a fascinating and very good presentation, Tim. I think I need to know a little more about plutonium biology to understand the bioassay, but I think your

representation was very good.

I will comment on the -- how well we can hear. I could certainly hear Tim very well through the presentation and I can hear Paul and I can hear Bob. And I don't know about anybody else on the line, but I was not able to hear Josie and Brad and Henry very well, particularly Josie, and she -- I know she asked a lot of really good questions, so I'm wondering if maybe people, those people could get a little closer to the mic.

Co-Chair Clawson: Yes, hey, Gen, they pushed us out to the outer edges of the table.

(Laughter.)

Member Lockey: We muted --

(Simultaneous speaking.)

Mr. Katz: Brad, you're just one over from me, but -- no, but, yes, Gen, thank you. That's exactly what we wanted to hear about. I think Josie was talking just at a normal voice level, too, and --

Member Beach: I'll speak up.

Mr. Katz: -- she's at the far end of the table. But so she'll speak up and if need we'll move this down a little.

Member Beach: Gen, can you hear me at this level?

Member Roessler: Yes, that sounds good, Josie.

Member Beach: So I'll just yell at everybody here.

(Simultaneous speaking.)

Member Roessler: -- when one person is talking, it's very clear, but if more than one person is talking at a time, it tends to get all kind of mumbled, from what I hear.

Co-Chair Clawson: Okay.

Member Schofield: I have to agree with Gen. I had a hard time hearing particularly Josie, and Brad would seem to fade in and out.

Co-Chair Clawson: I'm so soft-spoken.

(Laughter.)

Mr. Katz: He's a lightweight, right.

Okay. Thanks, Phil. And, yes, we'll try to do better.

Member Beach: Can you hear this? Is it time for a break?

(Laughter.)

Mr. Katz: So that's the second person who's asked me about a break, so I think it is time for a break. So it's about 10:10 right now. Why don't we take a -- is 10 minutes enough?

Member Beach: Yes.

Mr. Katz: Ten minutes? Why don't we take a 10 -- so at 10:20 we'll come back on. I'll just leave the phone on and put it on mute. (Whereupon, the above-entitled matter went off the record at 10:09 a.m. and resumed at 10:27 a.m.)

Mr. Katz: Okay, sorry for folks on the line, we're a few minutes late, but it's -- always works this way. So we're back on and ready to continue discussions.

Member Schofield: I'm back on, Ted.

Mr. Katz: Great. And, Gen, you're back on?

Member Roessler: I'm on.

Mr. Katz: Super.

Member Lockey: Hey, Gen, can you hear us?

Mr. Katz: Stop that. Go ahead, Tim.

Dr. Taulbee: I just want to make a quick comment,

and that is to thank everyone for letting me put the stratification on hold, and now that hold's off, and so, SC&A, go. I just wanted to -- get through that coworker model without those issues --

Mr. Katz: It was super helpful.

Dr. Taulbee: -- and so now we can go.

SC&A Review of OTIB-81, by Bob Barton

Mr. Barton: Okay. This is Bob Barton with SC&A, and before I get started, can I just ask do I still have Ron, Harry, and Joyce on the line?

Dr. Buchanan: Yes, this is Ron. I'm on the line.

Dr. Lipsztein: Yes.

Dr. Chmelynski: I'm here too, Bob.

Mr. Barton: All right, great. All right, the presentation that we're about to give is actually the same exact presentation that one can find online for you folks on the phone who don't have the Skype connection.

As you can see from this title slide, we had quite the SC&A team working on this project. That's Ron Buchanan, Harry Chmelynski, Rose Gogliotti, and Joyce Lipsztein. We were going to add Joe's name, but it looked too cluttered.

(Laughter.)

So this is the review of OTIB-81 which is the internal coworker model for Savannah River Site, and it's again sort of two-fold. One is how does it fit in with the implementation guidelines that we just discussed. And then there's going to be some actual site-specific issues. Really more site-specific issues than the coworker guidelines as you'll see.

So I'll just go through these really quick just to recap what the purpose of OTIB-81 was. And you see there's a couple quotes up here. But essentially,

it's repeating what Tim's presentation said.

You have workers out there who weren't monitored or their records are missing, and that's why we have coworker models. And the guidelines were developed for that purpose to assure that these coworker models, when we develop them, are going to cover the workers that need to be covered.

So I'm going to skip right ahead to SC&A's review focus. And, again, it's adherence to the principles and guidance in the Draft Criteria for that Evaluation and Use of Coworker Datasets.

And there are basically four main facets, the data adequacy, and that -- basically asking the question does the available data and monitoring methods accurately reflect the exposure that's intended to be reconstructed. The data we have, can we actually use that in a sufficiently accurate sense to reconstruct the exposure.

There's data completeness, and that's essentially how well does the data we have actually represent the worker population, and are there gaps? Is there a group of workers out there who's not essentially covered by the data that we do have.

And that kind of bleeds into the next one which is the evaluation of the monitoring program. This talks about the procedures in place, but also the actual execution of those procedures. You could have a procedure in place, but you really have to go in and check to make sure that it was put in practice and not just a piece of paper.

And then the last item, which is back on the table, as Tim said, is stratification. Is there a subpopulation of workers who had a distinctly different exposure potential than the full group, and do we have the data available to develop a separate exposure profile for that subpopulation?

So, again, one, coworker data adequacy. Again, this is talking about the instrumentation and the

measurement techniques. It's also -- we're going to be talking about the treatment of the censored data, particularly the imputation methods that are being employed currently.

And, again, when we say censored data, that is the biological result itself is just less than some predetermined value. It's not -- it could be anywhere essentially between zero and less than that number.

And, again, it's sort of the same thing as the next bullet. How do we use and interpret data that is less than the minimum detectable activity?

Completeness in this context for SRS, there's actually two different main data sources. One was the claimant data that was used to construct the plutonium, uranium, fission products, and tritium coworker models.

And then the second source was actually laboratory logbook, and that's what's covering the trivalent actinides which is americium, curium, californium. That method also caught thorium and neptunium.

And the reason that it went into the laboratory logbook data is I believe simply because there wasn't enough data in NOCTS to simply use the claimant data. We had to go and try to get the full dataset available from SRS.

Now, the evaluation of the monitoring program, again, that's who was monitored and is there a group out there that maybe wasn't monitored adequately. But that's really going to get addressed later today and possibly into tomorrow with RPRT-92, and that's really focused on the job-specific monitoring at SRS.

More on just what our evaluation focus was, moving to stratification as was talked about briefly before. That a OTIB-81 coworker model was stratified into construction trade workers and essentially all other monitored workers or non-construction-trade

workers.

And what we're really talking about there is the group that's routinely exposed and the group that is in more non-routine exposure scenarios such as maintenance or work inside of a glove box rather than outside of the glove box.

We're going to talk about how we evaluate whether workers are accurately identified with the appropriate strata. In addition to those four main facets of the draft criteria, the TIB-81, the Savannah River coworker model performed a pretty extensive quality assurance assessment which included the completeness of the claims tracking system. So that's the claimant data that was used for several of the individual radionuclide coworker models.

And also the completeness of the logbook data that was used for getting neptunium, the trivalent actinides, and thorium.

We're also going to talk about that QA also looked at the construction worker classification and the construction worker determination QA summaries, so we'll get to that at the end.

Okay, the first item is coworker data adequacy. And our main issue here is bioassay variability. SC&A has expressed concern about that for several years, and it's the observed variability in those transuranic bioassay measurements. That's the americium, curium, californium.

And this has been brought up in several previous SRS Work Group discussions and SRS-related reviews. And the key question here is is the measurement technique sufficiently accurate to reflect the exposure potential it is intended to quantify.

OTIB-81, the SRS coworker model concluded that a small percentage of the identified samples that were unaffected by chelation showed high variability, 4 of

52, and concluded the aliquot variability has an insignificant effect on the overall results.

And now if I could hand it over to Harry Chmelynski who took a look statistically at the variability in those samples in our review, and we'll move right into finding 1. So, Harry, if you're there, could you talk a little bit and gently guide us through how we took a look at this issue.

And, again, what the main issue is for these trivalent actinides, they would take a sample, one single voiding, and they would split it up sometimes as many as 10 times and measure it 10 separate times. Sometimes it was only once; sometimes it was five times. I think the max we saw was 10 times.

And what we observed was that those measurements of the exact same sample had very high variability. As I said, we'll go back one, OTIB-81 took a look at SC&A's previous findings on that subject and concluded that the aliquot variability has an insignificant effect on the overall results.

Member Lockey: Bob, can I ask you a question?

Mr. Barton: Sure.

Member Lockey: The 4 out of 52 it means -- 4 out of 52 were unaffected by chelation, and 4 of those 52 showed high variability. Is that what that figure is?

Mr. Barton: We believe it's much more than that.

Member Lockey: The 4 out of 52, that was meant to mean 4 have high variability of the 52, is that correct?

Mr. Barton: I would turn it over to NIOSH to describe what they did on that subject.

Member Lockey: I'm just questioning the 4 out of 52.

Dr. Taulbee: The 52 was the original I believe; correct me if I'm wrong here, folks. But the 52 were the original ones that were identified by SC&A that had high variability.

Member Lockey: 52 had high variability?

Dr. Taulbee: 52 had high variability. When we excluded all of the chelation folks which we hadn't done in the past, but we did for this coworker model, we were down to 4 that showed high variability that we really can't explain.

Member Lockey: Oh, I see.

Dr. Taulbee: But if there's -- but there were hundreds of results.

Member Lockey: So 52 had high variability, but you take out all chelation, there were 4 that still had high variability that you couldn't explain.

Dr. Taulbee: Yeah.

Member Lockey: I'd looked at that and -- now I understand.

Mr. Barton: And I remind everybody that we're really focused on the highest results there, the results that are well above the MDA where you shouldn't really be seeing that much variability. Now if you're well below the MDA, there might be some noise in there that might cause that.

But we were really focused on those high results because those would be the best to look at to figure this thing out. So, Harry, are you on the line?

Dr. Chmelynski: Yes, I am, Bob. So I've looked at all the data that was in SC&A's original letter report sent to the group in February of 2014. And that report had three tables in its appendix that listed all the readings that were conducted upon -- the multiple readings on each of the samples.

What I did was I took all that data in those three

tables. There were three tables that were divided into low exposure, medium exposure, and high exposure cases.

So I combined all the data together into one table, and I calculated -- I removed everything that had only one observation. In other words, there wasn't any multiple recordings done. Everything that had two or more, I could calculate a coefficient of variation, and I calculated that and the mean for all the samples, and the results are shown on figure 2 which is on page 18 of our report.

And this was a plot of the coefficient of variation versus the mean level for these results that had the multiple aliquot measurements.

And what I see here is that almost all of them are below 100 percent coefficient of variability. I don't know if that's an achievement or not. There are four or five that are above that level which may be the four that survived to be the four out of the 52 that NIOSH reported.

There was a question about how the variability of the DTPA-influenced data compares to the variability of the other ones. And when I look at the figure that I drew, they seem to have the same variability all the way across from low measurements up to high measurements.

So variability in itself isn't a good reason to exclude DTPA data. They do tend to be on the high end, so excluding them does reduce the level of exposure that you get out.

I guess that's about all I can say about this. The question about whether they should -- the DTPA data should be used seems to have already been resolved and that they're not going to be.

That still doesn't explain what the high variability of the other ones are due to. And, in fact, the highest ones are not associated with the DTPA.

Mr. Barton: Okay, thanks, Harry. So that led us to finding 1 in our review, and I'll read that into the record.

Although SC&A recognizes that incident-based sampling involving chelation is not considered in final coworker modeling, the removal of DTPA-influenced samples from consideration in the analysis of high variability observed in trivalent actinides bioassay results has not been justified sufficiently.

Evidence suggests the variation among DTPA and non-DTPA samples is nearly identical. Furthermore, OTIB-81 has not provided any reference to justify the assumption that DTPA causes heterogeneity among a single urinalysis voiding.

So basically what we're saying is the DTPA doesn't affect the variability that we're actually observing in these samples. We're still seeing it, and so while they may not be used in the coworker model, the indications of that variability go back to the counting method and the adequacy of the data.

So while we don't use them in the coworker model, they still do reflect the measurement and whether the data is adequate to actually reflect the exposure that we're trying to reconstruct.

Further on with the trivalent actinides data --

Member Ziemer: Question, Bob. So -- or maybe him, too. Are we talking about the variability in a sample that has been split into --

Mr. Barton: Yes.

Member Ziemer: -- not from sample to sample.

Mr. Barton: No, it's a single sample that's been split.

Member Ziemer: And do you have an established criteria as to -- this is sort of a judgment, but what is an acceptable variability? Like plus or minus some

value?

Dr. Taulbee: I don't think we have established -- an established criteria for acceptance into the coworker model.

Member Ziemer: I'm asking about the word high variability. Bob, do you have a value that -- high -- what does high mean?

Mr. Barton: No, no I don't.

Member Ziemer: So I don't have a feeling for whether we're talking about an order of magnitude, plus or minus 50 percent.

Dr. Taulbee: It was actually, I was just reminded, that was one of our questions back to SC&A, what are they defining as high variability. Because when we looked at the variability, and I've got a plot that I was planning to show, we don't see that this is a major issue when you look at the dataset as a whole, I mean a large --

(Simultaneous speaking.)

Co-Chair Anderson: -- outliers?

Dr. Taulbee: There's always outliers.

Co-Chair Anderson: Yeah, I know. That's what I mean.

(Simultaneous speaking.)

Dr. Taulbee: But if you think how I went through the coworker model and how this would play a role, if you get a few outliers that are showing high variability, when you consider we're taking the 50th percentile and the 84th percentile and then averaging that --

(Simultaneous speaking.)

Member Ziemer: It kind of disappears.

Dr. Taulbee: Exactly.

Member Ziemer: But the other part of it is, I think you suggested, I don't know if you said this specifically, that probably the ones that were influenced by DTPA probably wouldn't have made much difference in the final model anyway. Or did you actually test that?

Dr. Taulbee: I didn't say that, nor did we test that. The problem is the DTPA changes the biokinetic model, and so you can't really use the bioassay.

Member Ziemer: Well, yeah, inherently, but even if you did --

Dr. Taulbee: Even if you did, yes.

Member Ziemer: -- there weren't that many DTPA samples compared to the total, were there? For the --

Dr. Taulbee: There's a fair number. I believe it's -- we've got a table here.

Member Ziemer: Was it a small percent of the total if you're looking at the --

Dr. Taulbee: It's a small percentage of the total.

Member Beach: It was kind of an interesting comparison between the two models, the chelation versus non --

(Simultaneous speaking.)

Dr. Taulbee: But there's a fair number of people that had DTPA from the second one.

Member Ziemer: And then you had --

Mr. Barton: I don't want to lose focus here though because we're not talking about the effect it has on the final coworker model result. What we're talking about is the --

(Simultaneous speaking.)

Member Ziemer: -- about individual?

Mr. Barton: We're talking about the data we start with before we go through the averaging process and all that. The data we start with, is it sufficiently accurate.

When we see variation, even among samples that are high and above the censoring limit or MDA, we're seeing variability among the same bioassay -- before we even get to doing averaging and TWOPOS and all that, all that coworker modeling to get eventually to a coworker intake.

We're still at that first stage where we're evaluating the data. Is the measurement technique actually measuring what we want with sufficient accuracy.

Member Ziemer: That goes back to the original question, what's high variability.

Member Lockey: What was the specificity, sensitivity of the test? Did you go back and look at that timeframe?

Mr. Barton: I don't know offhand what necessarily the magnitude was between the --

Member Lockey: What was the accepted variability during that timeframe for that particular test?

Mr. Barton: Let me pose it to our internal dosimetrist. Joyce, in your opinion, I mean this goes back a number of years, when we looked at that data and we're seeing a number of samples -- again, I don't remember offhand --

Member Lockey: We're talking about split samples, right.

Mr. Barton: Split samples.

Member Lockey: Not one sample analyzed against another, but split samples.

Mr. Barton: It's a single voiding.

Member Lockey: Yes.

Mr. Barton: One person, single-voiding split onto a number of discs and then measured. Ideally, you should be in the same range in values, and that's what these are --

Member Lockey: So what was the acceptable range? That's what we're asking.

Mr. Barton: I'm not sure there is a necessary standard.

Dr. Lipsztein: I think if you take a sample and repeat it, because when you take five aliquots of the same sample, you wouldn't expect a variation more than 10 to 20 percent. And sometimes here you have even double or 60 percent. I don't remember the exact number because this was a long time ago, but we had two problems. Not only this variation, but also the limits of detection for until 90 -- the '90s because they were much lower than the detection limits that were reported by the National Commission of Radiation Protection, the NCRP, that should be taken at the time.

And also we saw the other installations that we were looking at. So we have serious doubts on the validity of those results. One, because they had that high variability; second, because the limits of detection were too low for gross alpha samples.

Member Lockey: It may be that the high variability was due to the very low limit of detection that they thought they could achieve.

Member Ziemer: Well, I think the concern we raised was for higher samples.

Mr. Barton: We were only looking at the higher samples.

Dr. Lipsztein: Yeah. So we had the two things. One, I don't think the limit of detection could be so low because it's different from everything today that was established in other very good laboratories in the world, actually not only in the U.S. but in the

world; and second, because these, the variability was much higher than someone would expect.

For example, if you use other radionuclides that were analyzed at the same chelation, and you have multiple samples of the same urine samples, then they weren't so big. So I'm trying to find out what was the variability, but --

Dr. Taulbee: If I can point people to --

Dr. Lipsztein: -- it's a long time ago.

Dr. Taulbee: -- attachment D of the OTIB-81 is where we did the evaluation of SC&A's previous comment. And here is a plot of the coefficient of variation of all of the samples. Coefficient of variation versus the absolute value of the mean.

And you can see the bulk of the data is below where we consider it an acceptable curve here. Are there counts above it? Sure, there's going to be some. But we feel that the bulk of the americium data --

Mr. Katz: One sec, Joyce, can you mute your phone because you have a lot of background noise coming through your phone.

Dr. Lipsztein: Oh, I'm sorry.

Mr. Katz: No, it's all right. It's all right. I just -- I'm just concerned about the other people on the phone. Thank you very much.

Dr. Taulbee: And so if you look at that attachment D, and -- what page number is that, Dr. Lockey? It's up at the top.

Member Lockey: It's page 153.

Dr. Taulbee: Page 153. The CR plot of the coefficient of variation and what we consider how it changes with the absolute mean value. So obviously as you get to lower values, the coefficient of variation will go up. And --

(Simultaneous speaking.)

Member Ziemer: -- have a chance to look at the finding. I just wanted to get a feel for what the high variability meant, and apparently there's some of the higher samples that have quite a high variability.

Dr. Taulbee: But there's not many higher samples.

Member Ziemer: I understand. I think the question's been --

(Simultaneous speaking.)

Mr. Barton: I would add that the higher samples are the one that you would expect if there's something there and it's above your detection limit, that you're going to have reasonable measurements of it as opposed to lower samples.

(Simultaneous speaking.)

Member Ziemer: Thank you.

Mr. Barton: As Joyce was saying, this is on the next slide which goes into the actual reported MDA for that trivalent actinide data. They report .3 dpm per day. And we compared that with some other standards. It's a factor of 3 less than, as Joyce said, reported by the ICRP as late as 1989.

It's a factor of 3 less than what was at Rocky Flats in 1977, and a factor of 10 less than what was being reported by Los Alamos National Laboratory.

And so what we concluded from that is that such a low MDA is probably only achievable by alpha spectrometry which really wasn't, I don't think, around until the mid '90s, maybe 1995 around there.

Dr. Chalmers: Late '80s.

Mr. Barton: Late '80s, okay. And that's really the time period that we were looking at with this data

because that's the time period that the coworker model was built for.

And so another facet of this data that gives us great concern, so we have two things so far. We have what we feel is, what's the right word, unusual, unusually low dpm. And we just said compared to some other state of the art at the time, we have the variability among the higher samples that are above the detection level where you wouldn't expect it.

And the next thing we're going to talk about is for trivalent actinides, it's unique in that we were talking about data that was below the MDA in the previous presentation. For the trivalents we actually have -- or NIOSH coded the raw data for the trivalent actinides, and what we find is the data is even much more less than that .3 which we already feel is a little unusual compared to the state of the art at the time, and I think the next chart is going to really demonstrate that.

So what we're looking at here, again the two red lines, the top one is the MDA at .3, and then half the MDA there. And what's on the body there is the 50th percentile coworker values based on that raw data.

And as you can see for some years, and again we're only looking at that SEC period, I mean we're below .05, so less than one-sixth of the MDA that we already feel might be too low.

So what we -- I mean we kicked this around a lot as a team. We felt once you're getting that low, even lower than an MDA we already think is too low, are those really meaningful results anymore, or are we just seeing noise in the instrument.

And so that's another one of our concerns, is do those data points that are so far below the MDA, and these aren't imputed, these were reported values, are we just seeing noise in the instrument or are we actually seeing something that can be relatable to dose.

And so that's our major concern we feel just looking at this chart here that we're so below the MDA that we feel the values have actually lost their meaning in relation to dose.

Member Lockey: Do you think the values are real or not?

Mr. Barton: I think that they were measured, recorded by the instrument. Do they actually reflect americium? We don't think so.

Dr. Taulbee: So you're saying that they could not detect an americium intake. Is this what you're saying?

Mr. Barton: At that low of a value.

Dr. Taulbee: Because this data is uncensored, the data that we used. We did not use any of the MDA. We used the raw data coming out of the logbooks, that's what we used.

And from that, whenever there were people who were high, they would then do a dose assessment. And so this method detected when people received an exposure. Okay? That's well-documented.

Now we used all of the data whether it was -- I mean, it's not censored. We used the raw data. So a lot of this is what you would call background. No dose, zero. But this is what we used.

We used the low data here, and they had the ability to detect when an intake occurred. So this is a coworker model that is dominated by --

Member Lockey: This is uncensored data?

Dr. Taulbee: This is uncensored data. It's low, yes. The site controlled the exposures.

Mr. Barton: I understand that as a screening tool, sure. But we're using this data for a dose reconstruction.

Dr. Taulbee: We're using it for dose reconstruction now. When people have positive doses, we're using that in assigning doses.

Ms. Brackett: I would clarify that. We're using it for the coworker models. But individual dose reconstruction, these values are not used.

(Simultaneous speaking.)

Dr. Taulbee: -- we're using an MDA, yes, when we do the dose reconstruction.

Mr. Barton: But, again, should this data actually reflect any sort of meaningful connection to an americium exposure?

Member Lockey: That's like -- I'm not sure I understand your question. If there's no exposure, then you're not going to measure any in urine. I mean, I --

Mr. Barton: But see what our feeling is, as you look at this data, it's so far below what the detection limits were that it's not actually a meaningful measurement anymore. It's just noise, background --

Member Ziemer: You could call it nondetectable.

Member Lockey: Just call it nondetectable. I mean that's what we would do. It's -- you're right, it's so low we don't have to worry about it.

Mr. Barton: That kind of moves us into the next point of what do we do with this data that's so low below the detection limit, how do we deal with that. Do we use it as a real data point? Does this reflect a real exposure potential?

Member Lockey: Well, we deal with it that at -- all the time. If it's below the level of detection we assign a value to it. If one-half of the limit or whatever, we -- yeah, you assign a value to it.

Mr. Barton: This is the third part of our concern with

the americium data. Again, it's the variability. It's the low detection limit which we think is too low -- in relation to the state of the art. And now we have values that are so low that we feel they have actually lost their meaning.

Dr. Taulbee: I would like to go back up to your comment there of the previous one of comparing it to the other sites. Rocky Flats, okay. Savannah River may be americium, so that's where it went. And Los Alamos as well. I mean, Savannah River, this was one of their production items that they made, americium, curium, and californium.

So comparing it to the other sites, well this is way lower than the other sites. It should be. They're the ones who developed the technology --

(Simultaneous speaking.)

Mr. Barton: It's comparable to an MDA that we believe is only achievable with the technology alpha spectrometry --

(Simultaneous speaking.)

Mr. Barton: -- the late '80s or mid-'90s.

Mr. Calhoun: So is the concern that these levels are so low that when you put them into a model it's driving everything down too far?

Mr. Barton: We're concerned that we're using data that doesn't actually reflect exposures to the trivalent actinides.

Dr. Taulbee: Okay. I can't believe that because, I mean, and the reason I say that is because there are people who this method showed they got an intake and they did follow-up, and they assessed dose based upon it. So it clearly works at some level. It clearly works.

Mr. Barton: In that situation, you would have someone that had a value above the censoring limit, right?

Dr. Taulbee: Yes.

(Simultaneous speaking.)

Dr. Taulbee: But it's the same, and they're part of the distribution, and they're part of this whole method.

Mr. Barton: We question those values above the distribution because of the variability we see. Again, the whole question is does this system of measurement, is it sufficiently accurate enough to reflect the exposures we're trying to reconstruct?

Mr. Calhoun: So you're bringing up just the fact that you believe that the whole analysis is invalid?

Mr. Barton: Well not the -- I'm saying -- yeah. The data that we're looking at --

Mr. Calhoun: For all the data.

Mr. Barton: -- the starting point. Well, what we see here -- hey.

Mr. Calhoun: Sorry.

Dr. Taulbee: This is a published paper, a published method in Analytical Chemistry. It's been analyzed. It's been reviewed. I mean we even had Dr. Glover go through it to make sure that the thorium would come through in this particular analysis. I mean, and now you're throwing out the whole analysis. I'm finding that --

Co-Chair Clawson: Questioning it.

Dr. Taulbee: -- incredible.

Member Lockey: I would go back to -- you proved to us that the system is not reflective of what it should be measuring.

Mr. Barton: That's what --

Member Lockey: That's what your --

(Simultaneous speaking.)

Member Lockey: I'm not going to -- I guess NIOSH should go down prove their system is right, and you prove it's not working right, okay. Go back, look at the system that was utilized, your sensitivity, specificity, define it, bring the documentation that is not adequately reflecting what was supposed to be --

Mr. Barton: When you see the measurements is what we're saying.

Member Lockey: I want to look at the technique that was used, and tell me whether it was a valid technique during that timeframe and supply the documentation that goes with that, one way or the other.

Mr. Barton: Understood. Before moving on, Joyce, do you have anything you want to add before we move onto this?

Dr. Lipsztein: I think everything was talked about. But I really, with the practice I have -- for many years in laboratory, I don't believe that the americium method was reliable enough so that we can believe that the data they had on americium was valid.

I think that the limit of detection is very -- that they reported was very low. Even if it was the main guide, still it was gross alpha counting. And I think that when you had the samples -- I'm sorry I didn't -- don't have the data with myself. It's because my CDC computer is not working. So I only have what is available for everybody so I don't have the raw data with me.

But there was a lot of variation on repeated samples which what makes you think there is something wrong with the method so we had two things pointing that you cannot believe on the results. It's the limit of detection and the variability on samples taking -- aliquots taken from the same urine

samples.

So that's it. I don't have anything more to add.

Member Lockey: It's Jim Lockey. I guess as a Board member, I appreciate what you're saying, and I'd like to have you provide a really -- a White Paper review and your position on this with all the appropriate published literature that supports your view one way or the other.

Dr. Lipsztein: I think we had that before, but we can repeat it. It's just that I -- as I told you, my CDC computer is not working so I don't have the data with me.

Mr. Barton: Either way, we need to fully document why we would -- not just on the observations and the data but with sources.

Okay, moving along. And this kind of leads into our next discussion which is the multiple imputation. This goes beyond the raw data that we were just talking about with americium.

This is what we have, a dataset with a large number of censored data. That is a large proportion -- sometimes the vast majority of the data is less than some number. And the question is what do we do with that data in the context of a dose reconstruction.

As Tim outlined in his presentation, TIB-81 adopts a method to infer a numerical result below the MDA, and this is known as multiple imputation.

And, again, we had Harry Chmelynski look at that method of multiple imputation to see how it affects the actual dose reconstruction. When we employed this method which is admittedly heavy on the statistics and far beyond my individual abilities, you know, what are the implications? What is this multiple imputation method doing, and what are the effects on the results that ultimately are calculated from it?

Harry, if you're there, could you talk about the analysis you did related to this multiple imputation method that has been employed in this coworker model?

Dr. Chmelynski: Okay. I began life as a physicist. And what I learned there was all models are wrong. Some are useful within the proper domain, and that sometimes the edge of those domains have been leading to new discoveries in science.

Well, when I became a statistician, I learned in Stat 101 all about regression. And the first thing you learn is it's very risky to extrapolate any regression outside the range of the data.

And the first thing I see when I look at these imputation method, is we're extrapolating all the way down the line. Sometimes we only have 30 percent of the data. But we're going to extrapolate all the way down to zero. And this bothered me right from the start.

Now, how do you usually do regression and prediction? By the way, we started out using the lognormal model because it's a convenient parametric model that gives us a mean and a geometric standard deviation, or a median and a 85th or a 84th or a 95th percentile. All these on the upper end where we have data. It also is useful to find an estimate of the mu and sigma for the lognormal.

Now one of the things that NIOSH never addresses is what are the uncertainties in those estimates of mu and sigma. And when you're only using it to characterize the upper percentiles, I don't have too much of a problem with that because I think we're going to be okay.

However, we're doing something completely different here. Now, we're not using parameter estimates in the same way. Now what we're saying is we're going to predict individual values down in the lower tail of the lognormal where we have no

data, and we're far far below where there is any data in some cases.

I don't know any way of doing that except -- well let's back up. If we're doing real regression, we know what we would do. We would calculate the predictive interval.

And we've all seen these graphs that show the hyperbolic shape of the predictive interval. It gets broader and broader and broader as you get away from where the data is. In fact, sometimes it gets so broad as to be almost infinite for the predictive distribution.

Dr. Taulbee: If I could --

Dr. Chmelynski: I'm sorry, let me finish.

Dr. Taulbee: Yes, sir.

Dr. Chmelynski: Now, what we're doing here is predicting individual values, and what we're saying is, as quoted earlier, we're just going to pick them randomly on that line as you extend the regression line down into the region where there's no data.

That isn't the way you do prediction intervals. As a matter of fact, it ignores uncertainty completely. So now, question.

Dr. Taulbee: Well, it was one of the things that you said of using the data down in that lower region. And what we're doing is we're imputing the values to get to the lower region in order to calculate another value, the time-weighted OPOS value, and those we are not using the lower tails. We are using the 50th and 84th percentile to develop the intake models.

So, to me, your characterization of saying we're using all this low-end data, we're really not in the final coworker model.

Dr. Chmelynski: Then why did you bother doing this if you're not using it? That doesn't make any sense

to me. You're saying oh, I can predict stuff down here that doesn't make any sense, but it won't make any difference, so it's okay.

Dr. Taulbee: Well, because there's some of the datasets that may be, say, 75 percent censored, okay. So we take all of that data, and we're developing what the underlying distribution is, and that's that lognormal model, okay.

Then when we're taking that, we're substituting the individual values for our TWOPOS calculation. That's how we're using this.

Dr. Chmelynski: Yeah, you're imputing or rather, say, predicting what values should occur down in the lower tail.

Dr. Taulbee: No.

Mr. Barton: Well those values in the lower tail are used --

Dr. Chmelynski: And as you said sometimes you only have 30 percent of the data, so the tail goes from the 70th percentile all the way down to zero. It's a huge tail. It's three times bigger than where you have data.

Now we've all seen those regression models that show you how the predictive interval behaves as you get outside the range of the data. Why doesn't somebody think about that here?

I know why. Because when you do on a list, you don't know what the uncertainty in mu and sigma are.

Dr. Taulbee: In our RPRT-96, we go through the multiple imputation method and we compare it to -- or we compare -- the method was developed using uncensored data, picking censored values, and seeing how we could model this and does it work. Okay? This wasn't done blindly --

Dr. Chmelynski: Which dataset are you talking

about?

Dr. Taulbee: This would be the Y-12 dataset, I believe, and Fernald datasets, that we used to develop this is RPRT-96.

Dr. Chmelynski: And what year were those datasets done in?

Dr. Taulbee: I don't know off the top of my head. But my --

Dr. Chmelynski: Usually what happens -- what I have seen -- the only time I've ever seen the lower tail of a lognormal actually plotted on real data is in more recent stuff where we have good precision and very low MDAs.

I hardly ever have run into a case where I can plot the lower tail of a lognormal.

Dr. Taulbee: Well I would urge you to look at RPRT-96. In fact, I think you guys did in here. I mean I remember -- it's part of my presentation to comment on that. But it's -- yes it's a new method that we've got.

We've been doing multiple imputation on the external side for at least the last four years. And this, well, it's the first coworker model we've done in five years or so, and so we've employed that same methodology here.

Dr. Chmelynski: So you don't agree that the predictive interval is the right answer?

Dr. Chalmers: What predictive interval?

Dr. Taulbee: What predictive interval?

Dr. Chmelynski: The predictive interval for the values you're imputing down there in the lower tail. How uncertain is that line first off? That's the first question.

Then the second question is, now that we know how

uncertain the line is, what if we take individual predictions? Now that's a much broader predictive interval of uncertainty.

Dr. Taulbee: But that's not how we're using this.

Dr. Chmelynski: Let me just finish this discussion and move on to one more topic here. Let's assume that you do this and ignore all the complications that we just talked about, and what do you end up with?

Well, I did a simulation on this, and it's reported on page 30, figure 9 of our review. And what I did here was I looked at -- well, let's say we have a lognormal, and what we're going to do is below some censoring level we're going to predict numbers in the lower tail.

And then what I did was I did it over and over, and I took the mean value of all of them. And here on figure 9, I looked at four different censoring levels. The median is the same always, and everything scales since the horizontal axis is the GSD and the vertical axis is the mean of the imputed values expressed as a percent of the censoring level.

And on each draft there's a line called a censoring level over 2, which gives you a perspective as to where we are here. We're right down in the range below the censoring level and down to zero is where we're talking about.

Now, when the PSE is low, the slope is low, and we impute a lot of values that are up near the censoring level, and you can see that in all these graphs. So on the left when the GSD is 1.5, we get somewhere around 80 or 90 percent of the censoring level as the expected value.

When we get out to GSDs over 5 or 4, depends on which one you're looking at, now we start getting expected values that are down below the half of the censoring level, getting down close to about 20 percent of the censoring level for the high GSDs.

And this, by the way, is just simulated. But what it points out is when you have high GSDs, you end up imputing much lower values down here in these lower tails. Now why is that? Well, that's because of this idea that the lognormal applies both on the upper end and on the lower end.

And if I have some people that are 10 times above the median in this group, now I have to start thinking well there must be some people down 10 times below the median. Well, I don't -- as a physicist, I don't see any sense in that statement.

Why would there be lower ones if now I have higher worker exposures? It just doesn't make sense.

Dr. Taulbee: Okay. My response to that is you have some workers who aren't exposed, okay? In that -- if you actually look at much of the -- or some of the data, you'll see there's negative values in the raw results that we end up truncating to zero.

Dr. Chmelynski: Well, that's exactly my point is you can't fit the lower tail of lognormal because of what you just said.

Dr. Taulbee: But we can make it claimant favorable by moving it up to zero --

Dr. Chmelynski: It's all hypothetical. It's all hypothetical what's going on down there in the lower tail. You make up some numbers, you write them down, say ah, just on the line, good. I don't believe in these made-up numbers.

Member Lockey: I'm sorry, what numbers are being made up?

Dr. Chmelynski: Your imputed number that you're sampling along that line down in the lower tail.

Mr. Barton: It's a non-detectable result that's being --

Co-Chair Anderson: And those bring the mean down --

Dr. Chmelynski: The imputed value for a non-detect, yes, let's leave it at that.

Member Lockey: Okay. So if we go back and say, I guess we can go back and say the analytical technique at the low end of the tail are -- because the analytical technique is valid, those values that were measured are non-valid.

In other words, they don't really reflect what the exposure was because the technique was incorrect. But if we accept that the technique was correct and all those values -- those actually are -- been measured values, then what do we do at that point?

Mr. Barton: I think this is different than the trivalent discussion. In the trivalent discussion, we had raw data points written down in a logbook. What we're talking about now is all we have is a big cohort of less-than-detectable. All it says is less than some number.

And so we're taking that number that's less than, and we're assigning it essentially a simulated real number, and then treating that simulated real number and putting a distribution to it. So these aren't real measurements.

Member Lockey: So you think that's influencing the higher exposure values?

Co-Chair Anderson: No, the median.

Member Lockey: The median? Do you think to a significant degree, you think it's biasing the median?

Mr. Barton: Well, Harry, correct me if I'm wrong, but I think that's what your analysis shows, does it not?

Member Lockey: I'm just trying to understand --

Dr. Chmelynski: I'm not carrying it through at this point to the final step of getting the medians. I just wonder why we want to make up these numbers.

The argument is that it won't affect the median much. I know I've heard that before, but why do we make them up if that's the case?

I guess because we have no other alternative maybe, because you don't like the CO over 2, you don't like the maximum possible mean, you don't like -- I would even take a uniform between zero and CO. Who knows? It could be anything down there.

Dr. Taulbee: But any of those values, any of those methods --

Dr. Chmelynski: All of them are wrong. All of them are wrong.

Dr. Taulbee: Sure, that's my point. Is all of them you're coming up with a value of what you think is down in that lower region. Based upon our review of multiple uncensored datasets, they tend to follow a lognormal distribution, and therefore we are trying to use this to simulate the best possible method that we can, and that's how we came up with this multiple imputation methodology. Okay?

That's why we're doing it this way. We could do it one time, okay. It's multiple imputation. We could impute it one time and go and run with that. But we're trying to make sure we're not biasing around that mean in some unknown way so we do it multiple times, the simulation multiple times to make sure that we're coming up with something that is reasonable.

We then take those values, the 50th and the 84th --

Dr. Chmelynski: That's why these, that's why these --

Mr. Katz: One at a time.

Dr. Chmelynski: That's why these plots are plots of the expected value because you are doing it multiple times. The expected values keep tapering

lower and lower as the exposures get higher and higher. I just don't believe it, I'm sorry.

Now maybe other people who are statisticians will believe the lower tail of the lognormal is something meaningful, but as a physicist, I don't, I can't connect the upper exposures with the lower exposures. It's like those entangled particles that no one can understand in physics.

In this case, why do the lower ones have anything to do with the upper ones? I don't know. But here, we're saying it has to be symmetric.

Dr. Taulbee: Nancy?

Dr. Chalmers: Okay. All I want to say is that we discussed this in RPRT-96 in detail for bioassay results where we had a completely uncensored dataset. We censored it at a certain level, maybe 70 percent censored, you know, whatever you guys were talking about.

And we used this technique to basically show that it's superior to any technique that we've ever used to do coworker. You can compare the multiple imputation technique results to the completely uncensored dataset. If you fit it, you have it. And in those examples, we did have it.

So we did a comparison. It's clearly superior I think if you look at RPRT-96 to all these other methods that we've ever used in the past. This LOD over two they're talking about. The ROS we used to use, all those things.

And this is just a specific use of it, and I think SC&A has already said that we implemented it correctly. They just sort of had an issue with the technique which I think there's references all through RPRT-96 of textbooks.

Review journal articles that say, you know, this is a great thing to do. Dr. Richardson has a paper where they use the imputation technique to do these sorts

of things. And so I think we really need to discuss 96 probably in that setting instead of trying to get into the gory details here. That would be my suggestion.

Mr. Barton: Well, currently we have responses from NIOSH on this issue in the section written on the statistical basis and the effect of multiple imputation.

Harry, I think you maybe got those on Tuesday from us. I don't know if you've really had a chance to really digest it. And if you have any comments or does NIOSH want to explain essentially the rebuttal to what our review says about multiple imputations.

Dr. Taulbee: I actually have a little bit of -- that was part of my kind of response to this, but I wasn't sure whether we wanted to bounce back and forth or wait until Bob was done. Any preference, because I can bring it up now if you want.

Member Beach: Might as well go for it.

Dr. Taulbee: Go for it, all right. Can you give me --

(Pause)

(Off-microphone comments)

Mr. Barton: I guess, Tim, while you're working on that, I guess taking a big picture view. The multiple imputation method isn't actually in the coworker implementation guide. In fact, the implementation guide is silent on the exact treatment of less-than-MDA values.

I was simply pointing out that this multiple imputation method that's contained in RPRT-96, it's actually not in the implementation guide. And the implementation guide doesn't really address what you do with highly sensitive datasets. So, I mean, while this effect would affect all other sites for coworker model, it is not directly related to the implementation guide. But it is how you deal with

less-than-MDA results. So it is universal in that it's sort of complementary to what's in the implementation guide, but it's not necessarily in that, nor is any other method.

Dr. Taulbee: I'm not sure it was completely -- oh, it's on yours, never mind. Well, yeah.

Okay, so I mean, our response to this is multiple imputation is a better, more statistically appropriate method for estimating the sensor data compared to MDA over two. And this is pointed out in RPRT-96.

And I just want to point out that, you know, the program's going to change over time. We've started using MDA over two and other methods, LOD over two for external. And as we get better methods, we plan to continue to use them.

Like I said, it's been well known that we've been using this in the external dosimetry side. And, but both the external dosimetry and the bioassay data are tending to fall log-normal distributions.

RPRT-71 covers this methodology for external dose OTIB -- I'm sorry, that should be RPRT-86, shouldn't it? Oh, RPRT-96, it's a -- oh, I'm sorry, never mind. Pantex. Sorry. RPRT-71 was the external dose methodology using multiple imputation. We implemented this in OTIB-86, which was the Pantex external coworker model.

SC&A reviewed this at that time, but it, the methodology hasn't been critically commented on. They agreed with the coworker model, but didn't see, going back through transcripts and so forth, that there was any major discussion about the imputation method.

But this table here kind of shows the similarities here, the external dose and internal dose, all right. When we're dealing with dose reconstruction, we're using a log-normal -- these would be for the sensor values, the nondetects, when we're physically doing the dose reconstruction.

And we assume a log-normal distribution for the missed does, with a geometric mean of N times the LOD over two and a GSD of 1.52, which comes out to the 95th percentile, is N times the LOD.

On the internal side, we use a triangular distribution with a min of zero, a mode of NDA over two, and max of the MDA. This is for dose reconstruction because we have individual data for that person.

The older method of doing coworker was we would use that LOD over two methodology for external, and then we used a PROC-95 method, which is a ranking method, to estimate the values below the censoring level for the internal.

So we've been effectively imputing or substituting those censored values all along in coworkers models, okay.

Now, the new coworker model for external side, and we're using the multiple imputation, which is very similar to what we're doing here, there we're going through the nondetect values, following the log-normal, and we're substituting them in for an individual worker, and then that gets put in together to develop that coworker model as a whole for that particular year.

What we're proposing is doing the same thing here for the internal. In fact, not proposing, this is what we're doing. But we're doing the imputation, there's an additional step here.

Instead of going directly imputation to dose, we're doing the imputation to the TWOPOS values. The TWOPOS values are then the fitted value that goes into the intake that I went through today. All of these additional steps follow this particular method with the multiple imputation, okay.

So from our standpoint is we intend to continue to use multiple imputation as a primary method for the analysis of censored datasets. We believe it to the be the statistically superior method, as pointed out

in, is it RPRT or OTIB?

Dr. Chalmers: RPRT-71 is the external and RPRT-96 --

Dr. Taulbee: RPRT-96, RPRT-96.

Mr. Barton: Harry, if you're still there, any comments on that explanation?

Dr. Chmelynski: I looked briefly at RPRT-96, although we weren't tasked to look at that report. And I, it has a lot to do with this. And a handful of datasets doesn't really answer the question. We're, but several of those datasets were very recent, actually, too, which also disturbed me.

But going back to the 80s and trying to guess what those numbers are down below the censoring level, however you want to call it, I still think it's a hopeless task. You can make one assumption, like we did, that the log-normal tale is the right answer. There's a lot of other assumptions. None of them are right. That's about all I can say.

Co-Chair Anderson: Just a question for you. I mean, we're dealing with censored data. At what point, if the censored data, the percent of censored data makes your results less -- I mean you're assuming the censored data is a log-normal. But if your actual measured data is at ten percent of all of the samples versus the others, is there a point at which you'd say the dataset is, because of the low levels --

Dr. Taulbee: This is where I'm going to ask our statistician to jump in.

Dr. Chalmers: We've actually developed a log-uniform method that we can use even if it's all censored. And that's probably since 96 was published, I think. So when we revise it, we're going to put that into RPRT-96.

Member Lockey: Say it again now, you're going to

develop what now?

Dr. Chalmers: If all data are censored, we can use a log-uniform that's another distribution, a log-uniform imputation model.

Member Lockey: So the assumption is that nobody had uncensored data; it's all censored.

Dr. Chalmers: Yes, yes.

Co-Chair Anderson: So the --

Member Lockey: I'm sorry?

Co-Chair Anderson: They're assigning values when there was nothing there.

Member Lockey: Well, there's two ways I would look at that. They should have been measuring it when they weren't, or exposure levels were so low that it wasn't worth measuring.

Dr. Chalmers: So basically it's all noise and --

Member Lockey: It's all noise, yeah.

Dr. Chalmers: What you do with it.

Member Lockey: And if it's all noise, you're about, it's about technique. If they should have been measuring when they weren't, then that's ours.

Co-Chair Anderson: Or if the limit of detection is so high --

Dr. Chalmers: But that's a data adequacy --

(Simultaneous speaking.)

Co-Chair Anderson: Not current, but of the older data. It's like here are the --

Member Lockey: Yeah, you have to back and look, you got to look back --

(Simultaneous speaking.)

Co-Chair Anderson: Assumed to be is actually a higher value than what they use. So if the confidence of the laboratory technique --

Member Lockey: Right.

Co-Chair Anderson: If you're confident in the technique, which should have detected things in the range you wanted to detect.

Member Lockey: Right.

Co-Chair Anderson: Then you're assuming the nondetects. But if you want to do the -- it's like you're, if it's a minus number, you --

Dr. Taulbee: We make it zero.

Co-Chair Anderson: Zero to be claimant-favorable. Then MDL over two is even more claimant-favorable. So.

Member Lockey: Then the question is do you start throwing data out or not, you know?

Co-Chair Anderson: Oh, it's --

Member Lockey: I'm always hesitant to throw data out.

Co-Chair Anderson: Well, it's like most of the human data studies for other chemicals, you can't get it published if you try to use something other than the limit of detection over two or the square root of two.

I mean, that's a, you're looking at all the chemical data that the CDC has adopted. They may eventually move to this kind of a model. But at this point, we're telling you this is what, for PCBs, for instance.

Member Lockey: Yeah, and that's usually based on sensitivity lab technique.

Co-Chair Anderson: Yeah, yeah.

Member Lockey: That's what it's based on, how much resources you spend on your lab technique to get down to parts per billion.

Co-Chair Anderson: Yeah.

Member Lockey: It's -- just may not be economically feasible to go to that level, so you do, divide it by two.

Co-Chair Anderson: That's why looking at comparison, if it doesn't make much of a difference, then how you assign those nondetect values with the MDL is so low already. You know, it isn't going to change the ultimate decision, so.

Dr. Taulbee: And that's where I'd like to try and reel, or come back to that initial presentation of how this is used. The censored values are used as imputed values to calculate that TWOPOS, that time-weighted OPOS for each person, and then we fit that to get the 50th and 84th percentile, and that's what we carry forward.

Dr. Chalmers: We fit that multiple times.

Dr. Taulbee: Multiple times.

Dr. Chalmers: One time for each imputation.

Dr. Taulbee: One, exactly. So.

Mr. Barton: Well, I'll say for my own, and again, I admittedly am short on the statistics, but what really caught our eye when we saw this new method was the output from it. You know, we have, all of these censored data say less than one.

And this method is coming up with coworker values that are ten percent of that value. Which to us, you know, seems very low, especially when compared to previous methods.

Now, the next thing -- well, first, Harry, any final thoughts on where we are right now?

Dr. Chmelynski: No, I'm not sure where we are. We seem to be at loggerheads. That's about all I can say. Again, if it really doesn't make a whole lot of difference what numbers you pick -- well, let me take that back.

When you have 70 percent of the data that's down there in the nondetect range, are you still doing this?

Dr. Chalmers: Yes.

Dr. Chmelynski: Yes, okay, no, that doesn't make sense to me. It's way too, too much extrapolation. Now, maybe if, you know, if 20 percent of the data were nondetects, well, you got a fair amount of data to fit your regression line.

And even though it's a phony regression line and we don't know what the uncertainties are, et cetera, it still seems to be okay. But the higher percent of the nondetects was, the worse I like this.

Dr. Taulbee: I guess I would encourage, or you've said you've already looked at RPRT-96. And maybe, I mean, that's where we did this analysis where we censored it at different levels and showed that we are able to predict the uncensored dataset fairly well.

Perfect? No. But for the purposes of this, we feel that this is reasonable in the way we're proposing to go forward.

Mr. Katz: So just a side, just a side note then on this. I think, Harry, you said that SC&A was never tasked with reviewing that report, is that correct?

Dr. Taulbee: That's correct, yes.

Mr. Katz: Okay, so then it seems like it'd be reasonable to have them review it, right?

Dr. Taulbee: Sure.

Mr. Katz: Given this discussion.

Dr. Taulbee: Yeah.

Mr. Katz: Yeah, so why don't we just go ahead and note that for the records, that let's have you review that report since it matters.

Dr. Taulbee: Just a bit of procedural here. Can I ask that the SEC Issues Work Group take that on, or maybe Procedural Work Group?

Mr. Katz: I mean, since this has been, I'm --

Dr. Taulbee: One of the authors on that is Tom LaBone, and so he is conflicted with Savannah River.

Mr. Katz: Right.

Dr. Taulbee: And so --

Mr. Katz: He's conflicted on Savannah River, but he's not, this is a issue of general applicability, so he's not conflicted there. The fact that it's Savannah River data is not going to --

Dr. Taulbee: It's not Savannah River data.

Mr. Katz: Oh, okay. So there --

(Simultaneous speaking.)

Mr. Katz: His conflict has no bearing.

Dr. Taulbee: If the next discussion comes up during an SRS Work Group meeting.

Mr. Katz: No, no, so anyway, that's a generic issue, and that would be handled either by SEC issues, only because they've already started on it, or Procedures, or the two. But anyway, so it wouldn't come under the SRS, right, correct. Good, so it's good to establish that. Right.

But right now, it's going to take them a while to review that. So SC&A too.

Dr. Taulbee: Absolutely.

Member Lockey: This is Jim Lockey. Just maybe I'm missing something here, but let me just raise this issue. If we take the center value and divide it in half, versus doing your imputed modeling value, at these exposure levels, the biological plausibility of an injurious injury is virtually nonexistent.

So from a medical perspective, it makes no difference to me, all right.

Co-Chair Anderson: It's within the rounding error.

Member Lockey: What's that?

Co-Chair Anderson: It's within the rounding error.

(Simultaneous speaking.)

Member Lockey: Plausibility at these exposure levels is any injurious impact. I mean, it's just beyond comprehension.

Dr. Taulbee: Yes.

Mr. Barton: Injurious, I agree because the levels are low. But we're still trying to reconstruct the doses for people, so it's important to know how we're going to handle those sensitive datasets if they get a coworker --

Member Lockey: But that has an impact on how long we're going to allow this to continue, you know. We're dealing with something here that --

Dr. Taulbee: I think due to the averaging and everything that we do going to the intakes, it doesn't really matter.

Well, actually, Bob, if you could give me some data here. I'm jumping ahead, but maybe this will help.

Well, let's continue on, I'm sorry. The thing is that one of the next findings shows that this method will result in some values that are higher. And so we've got some that are showing higher, some that are showing lower.

And they'll end up doing a Probability of Causation analysis that comes out to where it shows about the same. So that's, sorry, that's our interpretation of it. But Bob needs to go through it, not me. Sorry, Bob.

Mr. Barton: The next thing that we're really going to look at, as I said, the thing that really stood out to us was the magnitude of what this method was producing. Again, we're talking about taking a value that's less than some number and what the coworker comes out as is a very small fraction of that number, often ten percent, sometimes lower than ten percent.

So the next question, logical for us and we just, we already discussed finding two, so what does it really mean in the context of a dose reconstruction. So the first thing that we thought of was, well, if it was an individual worker with a less-than result, there's a missed dose methodology.

And that's where you use one half of the dose, triangular distribution from zero to one half to the full dose, based on the MDA. Which is a completely different method than what we're using for essentially the coworker who would be assigned missed dose.

So we have two workers, you have the monitored worker who's getting missed dose, you have the coworker who's getting missed dose, but there's two different methods being employed here.

Dr. Taulbee: And that'll bring you back to the external side of things, how we do the missed dose for dose reconstruction versus the coworker model, which SC&A has already reviewed and said it was okay.

Mr. Barton: You're talking about the imputation and the external.

Dr. Taulbee: Yes.

Mr. Barton: Well, you also said that we didn't specifically critically review that.

Dr. Taulbee: You reviewed the coworker component of it, the coworker model. I mean, all of those findings were closed out.

Mr. Barton: I guess this is the, I mean this is the first time I've seen it because it's the first time it's been used in an internal coworker modeling.

Dr. Taulbee: That's right, that's correct.

Mr. Barton: And I mean, this RPRT-96, right?

Dr. Taulbee: Yes.

Mr. Barton: RPRT-96, I think that came out in January?

Dr. Taulbee: January, yeah, about two months before the coworker did.

Member Ziemer: Could I ask, Bob, are you asking about the fairness of an unmonitored worker ending up with a higher value than the monitored worker?

Mr. Barton: That's what we're about to get to. We did some scoping calculations to see how it --

Member Ziemer: But I think that can often happen.

Mr. Barton: Yeah.

Member Ziemer: Because we're, you're being conservative. Here's one person where I have this actual data and I know what he got. Here's another, I don't know what he got, so I'm sort of giving him a --

Member Lockey: Benefit of the doubt.

Dr. Taulbee: Benefit of the doubt.

Member Ziemer: A benefit of the doubt and he ends up higher. It happens all the time.

Mr. Barton: I guess our point was they're both missed doses. And so --

Member Ziemer: Oh, both missed.

Mr. Barton: Well, they're really both --

(Simultaneous speaking.)

Mr. Barton: Yeah, one has actual values that, yeah.

Member Ziemer: It happens a lot in the external world, and I know that. We've got it at many different sites.

Mr. Barton: I guess if I could offer just my simplistic view of it. In the simplest sense, if coworker modeling was let's get everybody who had monitoring records in a line from smallest to largest, and the person right in the middle at the 50th percentile has a censored value, a value that's less than the MDA, now enter the unmonitored worker and say go find that coworker at the 50th percentile.

So they go to the middle of the line, they shake hands, and they say oh, I guess you're my coworker. Yes, I'm your coworker, I guess we're going to get the same dose reconstruction. And it's not true.

Dr. Taulbee: No, because there's built-in the conservatisms, the unknowns.

Mr. Calhoun: I mean, the whole program is built on being claimant-favorable. That's not even arguable, you know, I mean we got to be claimant-favorable. And it doesn't make sense to me, sorry.

Member Ziemer: Yeah, I know a lot of members of the public think the coworker's the person they work with. And it's not, it's a theoretical person who worked maybe somewhere else or could have worked somewhere else.

Dr. Taulbee: Could have worked somewhere else.

Member Ziemer: So anyway.

Mr. Barton: Well, what we did, and again, the point was, as we're looking at these very low values, the question really is, what's the effect. So we went and we did some scoping analysis where we assumed the coworker-computed values, which again are a small fraction.

And the missed dose for a monitored worker, which is again, evaluated half the MDA, and so the obvious thing to me before we even started out -- can people see this?

(Simultaneous speaking.)

Mr. Barton: All right, so we performed the scoping analysis, because the obvious thing is if you're going to evaluate dose at half the MDA and evaluate the dose at one-tenth of the MDA, the doses are going to be higher for the missed-dose monitored worker, right. And this is what we see here.

And this, these IMBA runs were actually performed by Ron Buchanan and Rose Gogliotti.

Ron, if you're there, you want to walk us through what the results were?

Dr. Buchanan: Yeah, this is Ron. For this discussion, what we did this exercise for was to step back, say does it make a difference, and if so, what are some of the areas, pitfalls we might want to look at.

So what we did was we said, okay -- and there's a lot of parameters go in dose reconstruction. We just tried to get a scoping view of it to get some examples. What we looked at was at two workers working side by side at the same period had the same latent period, and then the same cancer.

And what was the intake ratios, what was the dose ratios, what was the PoCs, which is the final number, of course, we're interested in.

And so we did this for several radionuclides and

several major organs. And we seen that we used the beta, the gamma, and alpha emitters. And so we looked at what the final PoCs would be in the ratios using these two workers.

And we see that we used strontium-90, a beta emitter. We have seen that the missed dose for the person that had records had low, less than MDA, was about two and a half times the dose as the coworker dose.

However, the PoCs was about very similar. Strontium-90 is not a big dose delivered, and so PoCs are fairly low MDA levels. And so they're less than one percent, so not a real good isotope that would give us a lot of information. So we wanted to look at a beta.

We looked at the gamma, at cobalt-60. Again, the missed dose using MDA values is about five times greater than coworker dose. However, the PoCs, the missed dose, led to PoCs about twice that of the coworker dose. However, the PoCs, again, were very much less than one percent. So not a good isotope that gives us much information.

Now, as we move to the alpha emitters, of course, there are bigger dose delivered. So we looked at that, neptunium-237, plutonium-239, and also uranium.

And in this case, we've seen that the missed dose, using MDA values, was about 1.4 times higher than coworker dose. However, the coworker PoCs were slightly larger than the missed dose. And we'll explain why in a little bit.

And the plutonium-239, similar situation, the missed dose was one and a half to about 1.9 times higher than the coworker dose. However, the PoCs for the coworker were slightly greater than the missed dose PoC. Okay, you want to go to the next slide?

Okay, so this observation two was simply that: an

observation. We're saying that we see that in this case, although the intake and the doses were greater, usually for the missed dose because a lot of the values were for Savannah River Site, this is all data taken from RPRT-92, that the PoCs came out relatively the same.

And that is because the missed dose is assigned using the triangular distribution, whereas the coworker dose is assigned a log-normal distribution. And so this distribution difference, even though the intake and the doses were higher for missed dose, this distribution difference about equalizes PoCs.

And we did this, like I say, for strontium, cobalt, neptunium, plutonium, to four or five major organs. However, we did also for uranium, and we did not see the same patterns. Go to the next slide.

We found that when we had used uranium, we found that the intakes were greater for uranium. Bioassays were greater because you had more workers with positive bioassays, had greater intakes. And so you had the PoC for the coworkers, which is about four times higher than the missed-dose approach.

Now, this really probably should be an observation. It's not really a finding, it's more informative.

And so what we wanted to illustrate was and make, you know, everyone aware that using the coworker approach as compared to missed dose sometimes gives the worker a higher dose, the unmonitored worker. Sometimes they get a lower dose and the PoCs sometimes are different. Next slide.

So now we want to make sure and say an important caveat is that we realize there's a lot of things going into dose reconstruction. Our main purpose in this exercise was we had all this discussion we've seen this morning on which to use and what you shouldn't use.

But we found out that the coworker model is usually

assigned as a log-normal. Missed is defined as a triangular distribution, so the PoCs come out similar for the cases and organs that we did.

And we just want to emphasize care needs to be used in using these two different approaches, and that the unmonitored worker isn't shortchanged and doesn't get a lower dose than a coworker. On the other hand, the monitored worker that has records sometimes is penalized for having MDA values reported as -- instead of being assigned a coworker dose.

I think that was the end of my part.

Member Ziemer: I understand what you're saying, Ron. But in most of these cases, we're quibbling about whether you should give somebody one millirem or five millirem. Either way, it doesn't make any difference on the PoC to speak of. And if we're talking about the censored data.

So you know, I think we've already talked in other venues about the issue of what we worry about at the low end of the dose spectrum. Obviously we'd worry about a factor of four if we're up in the hundreds of millirem or up in the rads or rems or sieverts or millisieverts.

But whatever units you want to use, if you're up in high dose, a factor of four is a significant difference. If you're down here in the couple millirem, it doesn't make any difference.

Dr. Taulbee: If I could interject. If you go back to that one plot that I showed of the coworker of once we got done with the intakes and the overlay of the bioassay, that 84th percentile.

If somebody had been exposed to that, this is using the multiple imputation method, if somebody had been exposed to that level for that long, all of their bioassay would then be showing positive. And it would still be a relatively low dose that you're talking about.

So I think we've got enough conservatisms built into the coworker models that they are claimant-favorable, as they need to be. And this method is scientifically superior, RPRT-96 demonstrates that. I know SC&A's going to review it. But I don't see any reason to change what we're doing from that standpoint.

Mr. Barton: And just to add a little perspective, why are we even talking about this? Again, this is the first time we had seen it for an internal coworker model. And again, the output, the output, we looked at the output. It seems so low, especially compared to what you do for missed dose, and that's why we performed the sample calculations.

Now, what's the real, I guess to wrap it up, we think the Board needs to know this kind of information about when, what is going on with these less than MDA results, both in missed monitored dose and for an unmonitored worker.

And that's why we went through this exercise, to really show that, so that we're all informed about how the program works.

Ms. Brackett: I would point out, though, that you know, the analysis showed that the PoCs weren't as different as -- and that is the bottom line, that's the important thing. And keep in mind that when you have assigned a missed dose, it's a triangular distribution.

And coworker is a full distribution. You can't compare them directly, you have to look at the PoC, because you know, you're, it's different in the outcome. So that's an important comparison, right.

Mr. Barton: Right, and that was actually one of our observations, that, while the doses can be very different, the missed monitored dose can be much higher. It can be a factor of five higher, I think, for when we evaluated cobalt. A factor of five higher on the dose, yet the log-normal --

Dr. Taulbee: But what I think Liz is coming up with is you're saying it's a factor of five higher, but you're comparing the two geometric, the two central tendencies, the 50th percentiles. And you're comparing two different distributions that don't compare that way.

Mr. Barton: That was the observation that we said. It's what's really driving the PoC actually isn't so much the magnitude of the dose, it's about what statistical distribution you can put on it.

And so for the coworker model we're using a log-normal with the GSD minimum of three. But whereas for the actual monitored worker, it's a triangular distribution.

Dr. Taulbee: Right.

Mr. Barton: So even though the doses are higher for the monitored worker, the effect on the PoC is really driven by that statistical distribution. And that's something that we wanted the Board to understand.

Dr. Taulbee: Okay.

Member Ziemer: Did Ron say you were changing that finding to an observation, or was that --

Mr. Barton: I think he did say, yeah, this is even as we were preparing for this meeting. It really fits in more as, again, with the intention of informing the Board so that they have all the information possible about these less-than-MDA results are being interpreted. It is really more of an observation.

Dr. Buchanan: Yes, I agree that this is mainly informative for us. But then when we -- we knew the question was going to come up, well how does this impact the end results of PoC. And so that's the reason we did the exercise and the reason we wanted to present it today is that it's informative observations, so I think that finding should be an observation.

Co-Chair Anderson: I guess I think one solution around this would be, when you say coworker, most of the workers think about individuals near them. This is really what you're doing is the rest of the workforce.

So it's really a workforce general exposure assessment versus coworker, because you're including a lot of workers that the person you're reconstructing has no idea who they are or where they are.

As opposed to, I want to know, he's sitting next to me, we have the same exposure down that end of the room. It's different, so it could be just a terminology issue might be one way to think about it.

Dr. Chalmers: I think in other fields they call it a co-exposure model.

Co-Chair Anderson: Yeah, exactly.

Dr. Chalmers: They were exposed the same or assigned sort of the same.

Co-Chair Anderson: There's a lot of confusion in the workforce that we're really not doing -- we're doing coworker. So co-exposure, you might want to think about where the missed dose is a different. You're taking into account the data you already have on an individual.

Mr. Barton: Right.

Co-Chair Anderson: And that since they already, most of them will have measured.

Dr. Taulbee: And we can certainly change the names of this is that helps the public.

Co-Chair Anderson: Going back to when we have public comment, you know, you're going to hear much the same when in fact the understanding is different.

Member Ziemer: No, we've heard that a number of times where someone says they used a coworker model, and I checked with so and so, and they never interviewed him.

Co-Chair Anderson: Yeah, exactly. So co-exposure, I think that's a, right.

Mr. Barton: And we understand that. And really, again, the point was if we're sitting here asking this question about the new imputation method coming back with a lot lower bioassay results than what was used for missed dose before, it's like, well, I thought we used half.

Well, it's like now we're down around ten percent. If we're asking the question, somebody out there might be asking the question. So the Board really needs to know the mechanisms of what's happening for any effect.

So we can move on?

Mr. Katz: Yeah.

Mr. Barton: All right, moving on to completeness.

Mr. Katz: Can I just, well, before you move on, though, just let me just get a sense what, how much time do we have left. Is this another half an hour?

Mr. Barton: I think this is going to go a lot quicker because, based on the responses we got from NIOSH, I am not prepared to fully respond on some of these issues, especially related to the --

Dr. Taulbee: I think the next few are when we get to stratification I think that's a good --

Mr. Katz: Because I just want to know when we should take our break, that's all. But that's good.

Mr. Barton: Yeah, I think that this is fine. I guess we're going to follow the format of we'll provide our finding and then respond.

Dr. Taulbee: Sure.

Mr. Barton: Because that seems like, it's been working. Okay, so this is moving on to data completeness. And one thing we noticed is of course several of the coworker models for SRS are based on claimant data, not the full logbook data. And basically what happened is NIOSH used the first of 4,000 claims.

And the reason for that is you have to put a cutoff somewhere and then start analyzing the data. You can't just keep, you know, refining the numbers every time you get new claims. And so the cutoff date was, you know, based on when the files were transmitted by DOE sometime in the fall of 2001.

Since that time, so we started with 4,000 claims to evaluate. Since that time, there have been another 2,000 clams submitted that could be used to augment this coworker model. So essentially it's a 50% increase. Now, why would you do that?

Well, notice that for at least uranium and cesium, data had to be combined for multiple years because you didn't have enough workers to evaluate. So that's why we brought this up that you have additional claims out there.

And as it's written in OTIB-81, it's already in electronic format. So we -- do you want to modify that? That is what it says in OTIB-81, easily retrievable electronic format.

Dr. Taulbee: There's easily retrievable and then getting through the QA verification and completeness testing is two different things. Actually, it's the same thing, but getting through that step took us a lot longer than we thought.

Mr. Barton: Okay.

Dr. Taulbee: To clean up the data and part of the reason is, as I tried to allude to earlier, not every dose reconstruction will go through and do a heavy

QA on internal data that's been coded, because it may not be needed for the dose reconstruction. Okay.

So on those particular cases, we go back and we look more closely at the data to make sure that it's been coded correctly along those lines. Because we don't do double blind entry up front. It's a single entry by the data entry clerks, it goes to the dose reconstructor.

They're responsible for looking at that individual data whenever it goes before they do an intake model or anything with that data. They're the ones that are responsible for that.

So yes, it's been coded. It has not been massaged or looked at by anybody else at that point, and that's what takes us a little longer to do. So yes, it is readily available, but it's not oh, let's just go do it. It's not that.

Mr. Barton: Not that simple, okay.

Dr. Taulbee: No. By the way, going through this, we learned that, okay.

Mr. Barton: Well, I mean, and this chart just shows what we're plotting here is the monitored claims from the -- so the top line is the original dataset. And it's basically the percentage of monitored claims that you could add if you had added this data.

And obviously once you get to the SEC period, it starts climbing up to where those additional data that represent a little over, or about 45 percent, somewhere below 40 percent of what the available internal monitoring you could have for analysis.

So that was the point of that one. There's more data out there. I guess it sounds like it's much more of an effort than simply plugging and chugging.

Dr. Taulbee: It is more of an effort. And but the

thing that I want to try and emphasize is if you think back to the intake modeling that I did, or not I did, Matt Arno is the one who did that work, and Liz's team checking it and all. We end up taking groups of years.

So yes, it would fill in some of those years where we've grouped, but is there a big change within that time period? Generally not in these particular coworker models. So is adding this additional 2,000 data points, is that going to move the mean in the 84th percentile? I don't think so, I don't think it'll move it hardly at all.

And so to do this additional --

Mr. Barton: You can't be quite sure, though, right?

Dr. Taulbee: When we have two, three hundred data points, I mean.

Mr. Barton: Yeah, part of it is --

Dr. Taulbee: You add another hundred data points, unless they're all at one end or the other end, it's not going to move much. If it's evenly distributed, there's no reason it's going to move.

Mr. Barton: One comment on that is there's a similar chart to this one in the report that shows the actual employment periods. And obviously these later claims are going to be in the SEC period more so than in -- or in the post '72 period more so than in the earlier.

So the addition of their data might have more of an effect on the actual SEC period. I don't know that, but you wouldn't know until you do it.

Dr. Taulbee: Right. I --

Mr. Barton: I guess the other, you know, just kind of thinking through this too, you'd also have to stratify them, right.

Dr. Taulbee: Yeah.

Mr. Barton: So it would be a lot of work. Well, anyway, the point was there's a lot more data out there to fill in some of these years where we had to group, because we didn't have the 30 workers recommended in the implementation guide. But the implementation guide does also say that if you have a reasonably consistent distribution, then it's okay.

Now, this is the completeness of those trivalent logbook data. And again, it's an observation. And we notice that especially 1982, or 1980-1982, there were significantly lower, I believe it was around 70 percent of the data we have in hand was reflected in what, the totals that were put forth by the Health Physics Department.

In other words, Health Physics said they put out 100 bioassays in that year, but yet we only have 70. And so this goes to the completeness issue.

And the observation here is, you know, if you have these years where it appears you might be missing some of the data points, it's sort of important to discuss if there was any changes in those specific years that would make those operations important from the fact that you might be missing data. And that's what, that's the observation in here.

We had an update because we thought, and we wrote in the report that perhaps you can taper on americium source terms would address those years and any changes. But from our or my reading, it doesn't look like it does that.

Dr. Taulbee: The one thing I'd like to point out is that year-by-year comparisons are really difficult to try and do, because you've got some samples that were collected in one year and they're analyzed in the following year.

It's not always together. And if you look over the entire time period of like 1963 to 1987, there are 18,293 samples for americium. Okay, so almost 18,000 samples, 18,293. Over the same period, what they reported in those summaries that you're

looking at is 18,153.

So over this large period, there's a difference of 140 samples. So does this really matter?

Mr. Barton: I'm not all that comfortable just averaging it over that kind of a period and saying, well, overall we had it. Because you want to see the temporal changes in it. And if there's a period where data might be incomplete around, I mean, I'm looking at 70 percent.

Dr. Taulbee: You're saying that, how did they make that bioassay summary table? What data did they use, do you know?

Mr. Barton: Are you talking about the source documents?

Dr. Taulbee: Yeah. What were they counting?

Mr. Barton: I assume the number of bioassays they issued.

Dr. Taulbee: See, we don't know, you assume. We don't know that either. We don't know if it's the number the lab counted, we don't know if it's the number the Health Physics Group submitted or requested. We don't know what that number is.

Mr. Barton: Okay.

Dr. Taulbee: So to try and say it's definite in this year, you can't. You got to look at over a larger period of time. Is there a bunch of data missing? The answer is no.

Mr. Barton: Well, you have some years in here that are 180 percent.

Dr. Taulbee: Exactly, because you don't know what that, that source document, that summary, when those were counted, okay. So because we have the logbook, so we know what the bottle date was. We know when the worker left the bioassay sample. That's listed in the logbook.

But we don't know how Health Physics or the people generating that bioassay summary table, when they were counting what samples or what their criteria was.

Mr. Barton: So if we can't make the comparison, how do we judge completeness? How do we judge if we have --

Dr. Taulbee: Look at it over a larger period. Does it look like a large number of samples are missing? No.

Mr. Barton: Well, you know, the theory that some were taken in December and then measured in the next year, that would apply to each successive year, you know what I mean?

Dr. Taulbee: Maybe, maybe not. Not necessarily.

Co-Chair Clawson: So what are we doing? Are we just rolling the dice?

Dr. Taulbee: We have 18,293 americium samples.

Co-Chair Clawson: I understand that, that's wonderful. But we also have a responsibility too. And this is where our completeness and stuff starts to come into a lot of this. I know a lot of this, we're doing the best job we can. But there are some areas that I think we could do a little better.

Dr. Taulbee: I don't see where we're missing a large quantity of the trivalent samples. I just, I'm not seeing that.

Mr. Barton: I would add that this, the data --

Dr. Taulbee: I mean, does SC&A have evidence that we were missing?

Mr. Barton: Well, we were actually just comparing the percentages that were provided in OTIB-81 as part of the completeness analysis.

Dr. Taulbee: Okay.

Mr. Barton: That data comes right out of it.

Dr. Taulbee: Right, and we're looking at it from, we looked at it from a larger perspective of when were they counting these samples. And our comparison --

Mr. Calhoun: It's less than one percent.

Dr. Taulbee: Yeah.

Mr. Calhoun: It's less than point one percent.

Mr. Barton: So basically what you're saying is we can -- over what, how many years? I mean, 20 or 30?

Mr. Calhoun: Exactly.

Mr. Barton: But there's fluctuations in there. But basically what you're saying is we can't actually use this data that's presented in OTIB-81.

Dr. Taulbee: No, I'm saying you could use it. But you're saying that there was a large number of data missing, and I don't see it. I don't see it missing.

Mr. Barton: Again, let's go back. This observation's --

Co-Chair Anderson: Is it assigned to the right year, though?

Dr. Taulbee: It may not be assigned to the right year. But is it significant?

Co-Chair Anderson: -- retired in '83. Their sample may have been analyzed in '85, but it's somebody that's no longer there.

Member Ziemer: But does it matter?

Dr. Taulbee: Right, but does it matter? No. I mean, because when we're doing the coworker, we're actually averaging an intake time period.

Member Lockey: So if you had 200 samples overall, and you're missing, it doesn't matter if you 18,000.

Dr. Taulbee: If you're assigning the same dose every year.

Member Lockey: That's right.

Co-Chair Anderson: The worker, coworker. But if they're only there for one year over your period.

Dr. Taulbee: But we would be assigning that coworker model over that time, to that year.

Member Lockey: You're still going to assign the level.

Dr. Taulbee: I mean, most of these intakes, I mean, we rarely do an intake for one year.

Co-Chair Anderson: I understand, I'm just --

Dr. Taulbee: Averages.

Mr. Barton: And what does this observation actually say for a path forward? All we wanted to hear was a description of the americium operations. If it's not a gap, it's not a gap.

But if it is a gap, it would be nice to know what was going on at the site. And if it was no different, then the exposure potential's no different. It doesn't matter if, you know, that 70 percent is 70 percent or 80 percent.

Dr. Taulbee: Where was that, by the way?

Member Ziemer: Well, I think you're asking them to clarify if it looks like a gap and is not a gap, why isn't it a gap?

Mr. Barton: Or what were the operations going on in those years that can make it, you know, that it really doesn't matter. Because the surrounding years are fine.

(Off-the-record comments.)

Dr. Taulbee: And you're looking at the Table 4-1?

Mr. Barton: It doesn't say here, but I --

Dr. Taulbee: It said 1985.

Mr. Barton: And that is years from 1963-1987.

Dr. Taulbee: In 1985, the number of, the summary says they only did 244 samples, and the logbooks we have 435 samples. So we still have a large quantity of data. That's not a small percentage there being monitored. To our knowledge, nothing changed from an operations standpoint in those time periods.

Mr. Barton: Yeah.

Dr. Taulbee: I can say I've seen from the logbooks where there will be time periods where equipment went down and they held onto samples for three, four, five months, and then pushed through a whole bunch. And so is that the case here? I don't know.

Mr. Katz: One sec, one sec. There's someone on the line who's not muted. Can you press *6 to mute your phone, or a mute button if you have that? We're hearing you, and that means other people on the line trying to listen are hearing you, too.

Mr. Barton: And again, this is just to add information for where if there's a perception of a gap, can we do something to obligate that --

Co-Chair Anderson: You could look at that peak in '85. Pull some of those and see when were they collected versus when were they analyzed. Because they may have had a problem, and then that would explain why there appears to be a gap. That they did a catch-up period because inspectors were coming in.

Dr. Taulbee: Are you wanting, so you're wanting to us --

Co-Chair Anderson: Well, I'm just saying you wouldn't have to do much, but that would explain it. I mean, three months isn't going to get you from

'83 to '85. We just said there were 240 extras. Something going on in '85 that was a catch-up or the samples came from somebody else.

Member Lockey: Well how does '85 figure?

Co-Chair Anderson: Yeah, I mean --

(Simultaneous speaking.)

Co-Chair Anderson: Clearly they did the analyses, so --

(Simultaneous speaking.)

Member Ziemer: What determined the 100 percent value? Is that where you have one-for-one samples collected?

Dr. Taulbee: Yeah.

Member Ziemer: Versus samples --

Dr. Taulbee: Versus what they reported. But see, the bioassay summaries, every month at Savannah River, they would go through and report from the bioassay lab how many samples they analyzed.

But we're not sure, we're not 100 percent sure that it's the number of samples analyzed, the number that was requested. It's all under the Health Physics Department. And so that's what that first column is.

Member Ziemer: If they did split samples, does that count as two?

Dr. Taulbee: We don't know, or were they recounts in addition to?

Member Ziemer: We don't know that.

Dr. Taulbee: We don't know that. So I mean, the logbook values are the real values, because that's what we have. I mean, we collected all of those logbooks. So we have those, those are complete. It's more of how did they report those summaries, and --

Member Lockey: That was a waste of time.

Dr. Taulbee: Yeah.

Ms. Brackett: The next slide shows the graph, right? So.

Member Lockey: That's what I'm referring to.

Ms. Brackett: And that's somewhat misleading when you first look at it, because the bottom line is 60 percent, not zero. So the lowest value is 70 percent, it's not ten percent. It's not what it would appear to be.

Dr. Taulbee: And so yeah. And so my guess is is that there is, and this is a guess, 1980, 1982 type of timeframe, those larger time periods where there's less samples, got caught up in '83, probably the 80s into '83 or '84. And it just carried over, and they got them all completed there in 1985.

Member Ziemer: 1963 you don't know what's --

(Simultaneous speaking.)

Dr. Taulbee: Sixty-three is 19 samples.

Member Ziemer: You're looking at percentages.

Dr. Taulbee: Exactly. So you know, 19 samples there, and then 1985 is 435.

Ms. Brackett: And I got a note that says we know that they got behind in '82 and '83, and that in '84 and '85, they caught up. So that's why.

Mr. Barton: That's exactly the information that --

Co-Chair Anderson: Yeah, that's, we're looking at nothing.

Dr. Taulbee: So based upon that, can we close this one? We haven't tried to close any observations or findings or anything yet.

Member Ziemer: No, even these, it says 90-some

percent.

Member Lockey: Where's that note?

Ms. Brackett: This is just I got a message from someone, so I'm asking if we have documentation. I don't know for sure what, how they know that, so.

Member Lockey: There's documentation that backs it up.

Co-Chair Anderson: That's exactly what we're looking for.

Member Lockey: We can just look at a couple of the lab's reports, and then we'll say whether that collection, when it was analyzed. That's probably what somebody did.

Member Ziemer: Well, she just said they were behind in '83 and four. And both of those, well.

Mr. Barton: The next item is stratification, so.

Mr. Katz: Okay, that's good because I sure could use a break. Are we okay at this point? We're getting to stratification next. Are we okay taking a break now lunch?

Consolidate your thinking about what's been said so far. And then for lunch, this place is super slow, this place here. So, and then if you're going to go somewhere else, it's going to take time to get there and get back. But so I think we need an hour.

Mr. Calhoun: So Ted brought lunch for all of us is what he's talking --

Mr. Katz: I did, I brought lunch for everyone.

(Simultaneous speaking.)

Mr. Katz: So it's about 12:20 now, so let's break for an hour. And folks on the phone, if we're not on right at an hour, just be patient, we'll be back.

Dr. Lipsztein: May I speak one second before we

close, because I can't be here after lunch. I was asked to do a memo on the americium uncertainties and measurements. But we did it already. There is a memo from SC&A from February 2014, so I don't see why I should do something again, the same thing.

Dr. Taulbee: This is when you asked for a more detailed analysis of why you felt that the americium?

Dr. Lipsztein: Yeah, but we did this in February 2004.

So please look at it, and then if you still want something that I do, I'll do it. Okay?

Mr. Katz: Okay, Bob, can you shoot that to me so I can, or shoot it directly to actually Jim Lockey. And copy, just copy everyone else. Thank you for following up on that, Joyce.

And okay, so let's break. And we'll, back in at approximately 1:20.

(Whereupon, the above-entitled matter went off the record at 12:19 p.m. and resumed at 1:29 p.m.)

Mr. Katz: All right, off we go.

Mr. Barton: All right, we're picking up with the sort of last facet here, which is coworker stratification. And then there's going to be one last slide about quality assurance.

Okay, so we're going to go through, like I said, the rest of the coworker stratification slides from SC&A's review of OTIB-81. And then there's a very brief slide about the quality assurance review that NIOSH did that we looked at. And then we'll have the comment/resolution on the stratification.

So start off with Observation 4. And that's that OTIB-81 does now provide a statistical comparison of the two stratified groups as prescribed in the Coworker Implementation Guide. The various

coworker models were stratified based on the a priori assumption that the exposure potential between construction trade workers and non-construction trade workers was different. And as it sort of says in the observation, this is in contradiction to the coworker criteria where it says once the dataset has been stratified based on job category, a statistical analysis should be conducted to determine if the two datasets should be modeled separately.

The next observation, and this is another one that's sort of just pointing towards later discussions at RPRT-92, and it's that SC&A believes the quantitative assessment of available job plans, rather than the qualitative basis that was in OTIB-81, is appropriate to determine that prime contractor and subcontractor construction trade workers are part of the same exposure strata. Such an assessment has been performed by NIOSH and a report of their findings has recently been issued. And it says this issue was discussed in RPRT-92. And also the issue of stratification comparison between prime contractors and subcontractors is contained in a separate White Paper, "Savannah River Site Plutonium Construction Trade Worker Stratification Refinement." SC&A has subsequently reviewed both documents.

Finding 5, this is specific to the job title "machinist". In OTIB-81, a machinist is listed as a non-construction trade worker. We noticed that its classification in at least one other document, which is the Program Evaluation Report 14, that job title of "machinist" was classified as a construction trade worker. So sort of a -- somewhat of a discrepancy there that we need to sort out.

And Finding 6, this is where we kind of sat down and said what can we do as far as, you know, some sort of analysis to sort of get our heads around, you know, not only how difficult it is to determine who's a construction trade worker and non-construction trade worker -- and what we're really talking about

is routinely exposed versus non-routinely exposed. SC&A took two alternative sources and compared them to the designations provided in the spreadsheet files that NIOSH provided us, along with the White Paper.

Now, this was a targeted review going into some detail in the actual review paper about what exactly we're looking for. And the result of that was that we found that 9 percent of the entries appeared to be in conflict. And when I say it was a focused or targeted evaluation or sampling -- and sampling may not be the correct word here -- but we looked specifically at what I would call the gray area job titles, where it's not obvious whether the exposure potential would be routine or non-routine. You know, if you have a chemical operator who is an outside routinely exposed worker, non-construction trade worker. You know, welder is a construction trade worker. Now you have these sort of three categories in the middle up here on this slide. And it's really the line managers and the foremen. Because you could be a foreman who's in his office all day and walks through the job site just to check on his crew. It could be the one up in the gallery watching. Or it could be a foreman of a small crew actually down doing hands-on work. That's the sort of grey area I'm talking about.

The assistants and helpers were also were also classified in OTIB-81 as non-construction workers. And if you think about the title of an assistant or a helper, you could be a lab assistant or something along that lines, or you could be a pipefitter assistant. Same thing with a helper. You know, you could be a chemical operator helper or you could be a carpenter helper. And so it's tough when you have these gray areas, which bin do you really put them in? Because if we're going to assume that there's different exposure potential, if they're in the wrong group, this could sort of muddy the results.

And the last one is operator. Now, operator we really generally consider with the chemical operator

category. They're out there doing routine work in production. Specific to Savannah River, what we found is that a lot of times these operators were actually classified as general service operators. Which a lot of times we interpret it as more of a sort of entry level position that could mean really anything. It could be a janitor, which would probably be considered a non-construction worker. But it also be a truck driver, packaging and driving waste to the burial grounds. It also could be just a laborer category, which is generally considered a construction worker.

So when we took a look at those, we found that was actually less than 10 percent, surprisingly, that in those gray areas we found evidence that maybe they were in the wrong bin, so to speak. Now, again, the sort of question that I try to focus in on is, okay, so what? Is there any sort of path forward on this? And that leads us to Observation 6.

And Observation 6 reads, "SC&A acknowledges that there are inherent difficulties in correctly associating individual workers with the correct CTW/non-CTW strata. This is particularly true for job titles that could potentially be included in either stratum. SC&A suggests a scoping analysis in which such borderline job titles" -- and, again, we're talking about assistants, helpers, you know, the general service operators, and positions like foreman where they could be doing either type of exposure. Those are sort of the gray areas we're talking about.

What if -- and I feel like this is not that difficult -- what if we just pulled them out, re-ran the R code, and let's take a look at the two distributions again and see what effect it actually had on it. Because if these gray area jobs aren't changing anything, then it's not an issue. However, if we suddenly see -- we take these gray area jobs out and suddenly there's a significant difference, we may have to perform a more rigorous approach to classifying these gray area job titles.

Dr. Taulbee: Can I ask a question?

Mr. Barton: Sure.

Dr. Taulbee: What would you consider a significant difference in the sensitivity analysis? What is significant?

Mr. Barton: Well, that would certainly have to be worked out. I mean, I'm not going to come up with the entire plan. That would have to be -- as you said in the response, it would have to be worked out between both parties, what are the results that we're looking for and what they mean.

Dr. Taulbee: Well, yeah, and that's where I'm getting at with this. You know, this is something that can be done. It's going to take significant time to do so, to --

Mr. Barton: Just to pull those jobs --

Dr. Taulbee: Well, to work out the details of how you're going to do this sensitivity analysis. And from our position, especially along -- the goal here is, do we have a significant misclassification issue? Yes or no? I mean, that's the goal. And when we looked at our -- we did a misclassification evaluation through our QA analysis. We looked at the datasets. And, well, may I share?

Mr. Barton: Sure.

Dr. Taulbee: Okay.

Mr. Katz: Do you need the cable?

Dr. Taulbee: Yeah. By the way if I said "I", it's not I, it's we.

Mr. Katz: No, you said we. You said we.

(Laughter.)

Dr. Taulbee: Okay, all right. Let's see here. Okay, when we did misclassification evaluation, we did

probability sampling to quantify the misclassification rate of the coworker models. And for the SRS in vivo dataset, in vitro dataset, the neptunium log books, and the tritium. Those are the dataset sizes, the number of fields checked, and the number of errors, along with classification error rate with 95th percent confidence internal. And all of these are less than 5 percent, which was our acceptance criteria.

So, we looked at a targeted group. And you came up with a central point estimate of 9 percent. And your targeted group was the gray area. We looked at the entire dataset as to misclassification and we're less than 5 percent. Is this something that we should be doing a sensitivity analysis further on? I mean, we don't really see the value of doing this.

You know, none of the other -- I mean, SC&A presented the general service operators, supervisors, foremens as examples where they could be on either side. And we agree that that's there. None of these are listed in OCAS-PER-14. We looked at CPWR document, Bingham as well for the Oak Ridge area. They don't list, like, machinists in that particular category.

We use this information to develop our master occupational table for all operators as to how we categorize the people. And we went through and we reviewed the seven examples presented by SC&A in Table 17 and we found no discrepancies in the original CTW versus non-CTW designation.

So, kind of our questions to you is, because of this, I mean, is this really a finding or is this an observation from this standpoint? I don't know what the -- what's the conclusion that SC&A has here that our coworker model is invalid or not appropriate?

Mr. Barton: Well, we performed the analysis and this is what we found. I think there's a lot of attention being put on the fact that I put 9 percent in the finding. And it wasn't meant to address the quality assurance criteria. In fact, I never

mentioned the quality assurance criteria. You know, when we're going to go and test to see if there are any problem areas, we're not going to recreate the wheel and do the same thing you guys did. We're going to go and look at those job titles and see, oh, yeah, some of them fall into Column A and some of them fall into Column B.

Dr. Taulbee: Okay.

Mr. Barton: We get to that point and then we say, okay, what's the conclusion? And that's Observation 6 where we're saying, well, something we could do that I thought would be pretty simple is just pull out those job titles and then run the code and then we'll see what the difference is. And it may very likely turn out that there are no differences. And then, in which case, we're done. You know?

Dr. Taulbee: And like I said, this can be done. I mean, it's not that it's impossible to do. It's just, you know, we don't really see the value that it's added. We already know from our evaluation of the job titles we have less than 5 percent misclassification rate. I mean, do you want more details than that? I mean --

Co-Chair Anderson: Well, it sort of depends on your approaching it as a population.

Dr. Taulbee: Yes.

Co-Chair Anderson: The program deals with individuals. So if you happen to be an individual in one of these groups -- if you're in one or the other for a coworker model, it might make a difference. So I think that's why the focus on gray area things from an individual standpoint, as you want to know which dataset was used in your individual case. And, I mean, that would be the way I would look at it. Now, overall, yes, you want to do that. And data-wise, since most of the workers not in a gray area, you can say, well, we're just -- good luck to you guys. You know?

Dr. Taulbee: No, no, no, that's not what we're saying. I believe what you're talking about, Dr. Anderson, is, from an individual dose reconstruction we're assigning, you absolutely need to go and look as to which category they are, whether they are construction trades or non-construction trades in this particular case. But I think in the development of the coworker model we know we have less than a 5 percent misclassification rate amongst these gray areas.

So whether they're in one versus the other does not really change those two intake models. And I don't believe that it does. Absolutely, what you're talking about, when we go to apply it, yes, that matters a great deal. And that's where I think the Dose Reconstruction Subcommittee review would catch that, was it being applied properly? But in the development of the coworker, I don't see where this is going to change the two groups significantly. Does that make sense?

Co-Chair Anderson: I was just thinking you -- you're saying that an individual then can be categorized differently in different datasets. So you've made a determination that this person -- this job title is amiss. And when your risk assessor goes to do it, my assumption is they would see that this person's already been classified as XYZ.

(Simultaneous speaking.)

Mr. Katz: No, they wouldn't see that. They wouldn't see that.

Co-Chair Anderson: Okay.

Member Lockey: Tim, you said something just now and I want to understand. You said less than -- when you did your analysis, there was less than 5 percent misclassification in gray area. Is that what you said?

Dr. Taulbee: No, not in the gray area. Overall.

Member Lockey: You said gray area, I just wanted to be --

Dr. Taulbee: I apologize, no.

Co-Chair Anderson: Total.

Member Lockey: Just to clarify because I think --

Dr. Taulbee: Yes. This is our actual misclassification rates amongst -- for the datasets that you've got there. So, I mean, you can see that they passed each of them, the 95th percentile, was less than 5 percent. And I mean, SC&A can go through and they can find, there are errors. I mean, there are some that when we developed the model, they're not perfect. Now, when we went through and did this, we corrected those errors, obviously.

Member Lockey: If you took the gray areas out of this, this is probably going to drop to less than 1 percent, I suspect.

Dr. Taulbee: It's going to drop, I would think so, yes.

Co-Chair Anderson: But, again, there's a probability sample of 28,000. You looked at 800.

Dr. Taulbee: Yes.

Co-Chair Anderson: So you're --

Dr. Taulbee: But randomly looked at 800.

Co-Chair Anderson: Yeah.

Dr. Taulbee: Randomly.

Co-Chair Anderson: Okay.

Mr. Barton: I guess one of the things that I wanted to check on this during lunch -- at least in the in vitro dataset -- and I can point you to exactly where there is. These are in the files that were provided to SC&A. And the name of the file is "SRS Combined In Vitro Data 91818 with construction tradeworkers."

And it has a column in it that says "OTIB-81, Rev 4, construction trade worker." And then it has the job title.

Okay, so we're looking at these two columns here. And if you look at -- we're looking at assistant, and this is the classification, assistant. No, no, no, no, no, no, no, no. I mean, is this --

Member Ziemer: What does the "no" mean in this case?

Mr. Barton: Not a construction trades worker.

Member Ziemer: Got it.

Dr. Taulbee: Some of them could be construction trades in there.

Mr. Barton: And then the same thing for helper and then foreman. They're all classified as non.

Dr. Taulbee: When you look at the dataset as a whole, less than 5 percent is misclassified.

Mr. Barton: I understand what you're saying. And when I did my analysis, I wasn't trying to recreate that quality assurance. Again, I wanted -- it is focused. It is targeted. And when we saw something like that, that's what prompted us to go in and look at those files and say, well, you know, how many -- are these really all just non-construction workers? And I can go down to helper --

Member Ziemer: So where did you put those, then?

Co-Chair Anderson: They all went into non.

Mr. Barton: Non-construction workers. See, now we're on boilermakers --

(Simultaneous speaking.)

Dr. Taulbee: Carpenters are all yes.

Mr. Barton: No, I understand. I understand. I'm just saying this is why we took a closer look at those,

because all of the assistants and all of the helpers were classified as non in this in vitro database. And had the most recent timestamp on there.

Dr. Taulbee: Okay. So, again, I say what is the conclusion of this? You found there's approximately 10 percent, 9 percent, whatever, which you would consider in the wrong category.

Mr. Barton: The solution I put forth, or potential solution, was to take these out and re-run the R code and take a look. That was all that -- that was Observation 6.

Member Ziemer: And do the distribution without these --

Mr. Barton: Exactly.

Member Ziemer: -- and see if it made any difference either way.

Mr. Barton: Yes, either way.

Member Lockey: Do you think that's worth it with their sensitivity analysis at 3 percent or less?

Mr. Barton: Well, I guess the point is, I figured it wasn't going to take a lot of work to do. But it seems the answers that I keep getting is that it's going to take a whole lot more than I thought.

Dr. Taulbee: Well, it's going to take more work than you thought, I think. But, I mean, I went through the steps here earlier. What this would do would be we'd go back and we'd have to redo all of the TWOPOS values. We could separate them into the two groups, or take them out rather. And then you're going through and you're analyzing the TWOPOS values, doing those fits again. Then you're coming up with the 50th and 84th percentile. You're redoing all of the intake calculations.

Mr. Barton: I don't think you'd have to do the intake calculations.

Dr. Taulbee: Why not?

Mr. Barton: Because you can just take a look at the relative magnitude of the annual TWOPOS values.

Dr. Taulbee: But that's not what we use for the final model, for the final coworker model -- coexposure model. We're using the intake values. It all gets rolled in together at the end here. And that's where I'm asking about, you know, what are your parameters for the sensitivity? What is a large difference? Because you're going to get different. I mean, but how much is too much at that point?

Co-Chair Anderson: Well, you had said 5 percent.

Dr. Taulbee: Well, I had said 5 percent -- our misclassification rate is less than 5 percent.

Co-Chair Anderson: Yeah, but that's your target. It could've have been more than 5 percent --

Dr. Taulbee: If it's more than 5 percent --

(Simultaneous speaking.)

Co-Chair Anderson: -- and then you would've stratified it and looked to see where the excess was. And the excess is in --

Dr. Taulbee: Sure.

Co-Chair Anderson: -- 9 percent of this group, calling them all non.

Dr. Taulbee: If it had been more than 5 percent, we would have done more investigation to try and find out what was causing it and where it was. Absolutely, we would have done that.

Dr. Chalmers: Well, and even if we would have noticed a systematic pattern in the errors we found, we would have tried to do something.

Dr. Taulbee: You're right, because we did do that on some of them.

Dr. Chalmers: Yeah.

Dr. Taulbee: We found the systematic problem.

Member Lockey: So, a priori, you had a 5 percent figure set?

Dr. Taulbee: Yes.

Member Lockey: Okay, so, a priori, use that 5 percent figure. It's found 3 percent or less, so you just move one. That's basically --

Dr. Taulbee: That's what we did, yes. Member Lockey: So we need -- I guess from you we need --

Mr. Barton: We keep talking about the quality assurance criteria and the 5 percent. What I'm talking about is the effect on the actual distribution of bioassay values. When you take these out, how is it different? Is it even different? You know, it's very likely, because it's a gray area and some should be construction trade workers and some shouldn't be, it might just wash itself out.

Member Ziemer: Well, is that the equivalent of mislabeling, in a sense?

(simultaneous speaking)

Dr. Taulbee: That's my interpretation, but maybe it's not. I don't know.

Member Ziemer: Is the 5 percent --

Dr. Taulbee: Our 5 percent is, were they categorized properly? And our standard from that standpoint in going back is, you know, you're looking at a bioassay table here. We go through and we pull that particular worker --

Member Ziemer: If you pulled one of these, would it show up as misclassified?

Dr. Taulbee: Yes, and we have some that do.

Member Ziemer: Some do and some don't.

Dr. Taulbee: Right. Most of them -- 95 -- well, less than 5, more like 97 percent are correct. And we looked at 847 of those. So, yes, maybe some of the gray areas can go either way.

Co-Chair Anderson: What was the total count for the gray areas?

Dr. Taulbee: How many did you look at?

Mr. Barton: I think it was 14,000, somewhere around there, data points.

Dr. Taulbee: You had 14,000 gray area data points?

Mr. Barton: That I evaluated -- they weren't part of that 9 percent, but the total was about 14,000.

Dr. Taulbee: Okay, what was the part -- what was in for your 9 percent figure?

Mr. Barton: Nine percent of that. Hold on.

Co-Chair Anderson: Because that's kind of getting to, if you're in a worker group title class that's proportionally small, but they are disproportionally higher miscalculation rate, it will be across-the-board missed when they've got 28,000 and there's all these different worker groups. So your sampling frame -- I mean, not that it -- I'm just saying --

Member Ziemer: So you're not saying the gray area is 9 percent. You're saying --

Mr. Barton: No, when I looked specifically at those gray area jobs. And you can see up here, the first one is conflict with both NOCTS and employment history, conflict with employment history only, conflict with NOCTS only, conflict with both. And the adjusted versus the unadjusted. Unadjusted counts each conflict as its own thing. The adjusted takes into account that you only have one conflict as actually two of them agree, one of them doesn't. Because they have three independent sources. OTIB-81 was one source. Then there was the NOCTS files, which can be the CATI, it can be DOL -

- the whole file. And then there's the work history cards, which actually have a lot of temporal information.

I guess the way we approached it is, how we do something to take another look at the issue of stratification? Now, I don't think 9 percent is a huge issue. But the question was, if it's easy to do, is it worth doing? Now, it may not make a difference. It may make a difference, in which case maybe we have to take a harder look at those classifications for those specific workers because those are the toughest one to make a call.

Co-Chair Anderson: I mean, the assumption would be, on the other one, misclassification is a random event.

Dr. Taulbee: Right.

Co-Chair Anderson: This would suggest, given these -- it's not particularly random unless you were to look at it and -- it's almost nine times the rate of your overall one.

Dr. Taulbee: Well, it's nine times the rate, but it's still pretty low. And that's the --

Co-Chair Anderson: Low in the eye of the beholder.

Dr. Taulbee: But the application of it, I don't think that's a problem. I mean, I think we look at enough details when we go to apply this coworker model.

Mr. Katz: Hold on a second. Excuse me. Someone on the line, is there a line open? I can hear talking. Can you mute your phone? Press *6 to mute your phone if you don't have a mute button.

Go ahead.

Dr. Taulbee: But, I mean, from this standpoint, we're looking at population-type data to develop the coworker model. Okay? So, I mean, we can look at this. It's going to take a lot of time -- not a lot. I mean, at least six months, I mean, if you think

about it, at least. Mr. Barton: I was thinking a few days. Well, you just go through and you delete -- I mean, if we're going to do full intake modeling, I understand that. I was thinking a more rudimentary, just calculate the TWOPOS with those groups not included.

Dr. Taulbee: No. No, we'd have to --

Mr. Barton: Okay, it sounded like we had an R code that had kind of streamlined the process. So that's sort of where I had gotten that idea from. But if it's six months then certainly I'd --

(simultaneous speaking)

Dr. Taulbee: -- and get it reviewed and get it to you all for you guys to comment on. I mean, we're looking at at least something like that for the sensitivity analysis. And we've got to agree on the parameters first. How different is too different? And is this worth it?

Member Ziemer: I'm still confused. Maybe not a surprise. Bob, on your chart where you listed all the aides and there were non --

Mr. Barton: This right here?

Member Ziemer: Yeah, if you scroll up to the no's. The assistants, let's say, for example, you're not saying that that's a misclassification.

Mr. Barton: This isn't my spreadsheet. This is provided by NIOSH.

Member Ziemer: Yeah. But you wouldn't say that's a misclassification because it says no, necessarily.

Co-Chair Anderson: No, nine percent of them.

Member Ziemer: Is that what we're talking about?

(simultaneous speaking)

Member Ziemer: Regardless of where it is.

Co-Chair Anderson: Well, I mean, it's a systematic -- I mean, you've got to do that. I mean, you could use your random assignment like you do with the -- but that's not what you want to do.

(Simultaneous speaking.)

Member Ziemer: So presumably 9 percent of these no's might be misclassified. And 9 percent of everything else might be. So that's the 9 percent. Right?

(Simultaneous speaking,.)

Mr. Katz: Focus on those gray job titles.

Member Ziemer: I just want to make sure we're not saying these are all misclassified.

Dr. Taulbee: No. I mean if you go down -- what is this dataset? This is the in vivo one?

Mr. Barton: That's the in vitro model.

Dr. Taulbee: In vitro? How many -- This has got like 200,000 --

Mr. Barton: I think it's 100,000. I don't know why it's not filling the full screen. Yeah, just over 100,000.

Dr. Taulbee: Okay, so 100,000 data points in there. And there are a few job categories that SC&A has pointed out that, when they went to look at them, they found that nine out of ten are okay and one of them may not be, may not have been classified properly, of these job categories that are in the gray area. And what we did was we looked at the whole dataset, all of them, and came up with what is that misclassification rate across all 100,000.

Member Lockey: How many are in the gray area?

Mr. Barton: I don't know that offhand.

Member Lockey: How many in total?

Dr. Taulbee: 100,000.

Member Lockey: 100,000 total?

Dr. Taulbee: Yes.

Member Lockey: And do you have any idea how many are in the gray area?

Mr. Barton: Well, I looked at about 14,000.

Co-Chair Anderson: Was it 13,000 and something?

Mr. Barton: Well, it's 13,000 was the one with no conflicts. But the total I looked at was a little closer to 14,000 or 15,000. So, say 15 percent just for the sake of argument.

Member Lockey: So 15,000?

Mr. Barton: Yeah.

Member Lockey: So 15,000 in gray area, total 100,000, 9 percent and 15,000 is -- 1,500 over 100,000?

Mr. Barton: Yeah.

Member Lockey: That's 1.5 percent.

Co-Chair Anderson: Of the overall group, yeah.

Member Lockey: Yeah, I don't think it's worth --

Mr. Barton: Again, what I wanted to do was present some options --

(simultaneous speaking)

Member Lockey: Well, if it's going to take six months --

Mr. Barton: I agree with that.

Member Lockey: I think the chance of you getting something that's going to be significant is remote in comparison.

Mr. Barton: And I would add that the ones I wrote there is not all the workers that fell into those three gray categories.

Member Lockey: I understand. Well, I think you did the right thing. I mean, when we get job histories and we get -- we have gray areas. And it's hard for us sometimes to classify them into one group or another and it's a struggle. But, yeah, since there are 100,000 people and this is 15,000, 9 percent of that -- that's pretty good for gray area data.

Co-Chair Clawson: Why are we using NOCTS on this again, NOCTS data? Is there a comparison between or is this -- I'm just trying to understand why we're using this.

Dr. Taulbee: NOCTS data, primarily because it's the most readily available dataset we have that's electronic now. We do have all of the plutonium data from the site in log books. It's hard copy. In order to use it all, we would have to code it all. And you're looking at multi-year project. We rely on OTIB-75 where we had compared electronic datasets to the NOCTS dataset. And we found that there was no statistical difference between the two. And so we use that methodology to use the NOCTS data as a random sampling, basically, of the total population that's out there. That's our basis.

Co-Chair Clawson: Okay. Because I know we're just -- this is just dealing with plutonium. Correct?

Dr. Taulbee: No.

Mr. Barton: Plutonium, uranium, fission products. Everything except for trivalents, thorium, and neptunium.

Dr. Taulbee: The trivalents and the neptunium -- well, the trivalents we coded all of that data several years ago because the NOCTS data was limited. So we went to the log books and coded all 18,000 exotic radionuclides -- the trivalents, all 18,000 americium results.

Co-Chair Clawson: Okay, thank you.

Mr. Barton: So, that was the last on stratification. It sounds like it's way too much of a bother to yield a meaningful result.

Dr. Taulbee: Can we consider that observation, then, closed?

Mr. Katz: That's up to the Work Group.

Dr. Taulbee: Yeah, that's what I'm asking the Work Group, the Board.

Member Lockey: My perception is yes, that's fine. Jim Lockey.

Co-Chair Clawson: I do. With one caveat, though, because I'm looking at this from another standpoint, from the dose reconstruction. Because I hear so many times, well, their classification was this, so they wouldn't have that type of a radioactive dose. We use classifications for a lot of different things. Now, I'm not saying that in this sense, it is that way. But you guys do use classification and their job class in dose reconstruction.

Dr. Taulbee: Yes, sir. We do.

Co-Chair Clawson: That's where I have problems with this. I'm not saying with what Bob has been saying. I just want to go on record. We use classification for a lot in there. And so we better make sure that it is the best we can. I do agree with Dr. Lockey, and if the other Board Members, that with this one, it is not. But this is one of the positions where I want to make sure we realize that when we use a classification like that, it does affect people. And right down to the level of their doses.

Member Lockey: Do we need to hear from the other --

Mr. Katz: Yeah, we do need to hear from the other Work Group Members.

Member Ziemer: Is this an SRS vote or is this --

Co-Chair Clawson: This is an SRS issue, I believe.

Dr. Taulbee: Well, this is a coworker.

Co-Chair Clawson: Okay.

Dr. Taulbee: This is under the OTIB-81.

Member Beach: Well, technically, the issue doesn't go away. It's the second suggestion, or SC&A's suggestion that goes away. But the acknowledgment of the classification is still an issue. Correct?

It's kind of a two-part observation. I mean it's an observation, of course. But it's a two-part. So, yeah, the second part goes away because you're not going to do it. It would be too much. But the issue still stays.

Member Ziemer: We're talking about the SRS stratification, though, right?

Mr. Katz: So, just to clarify, when you close an observation, what you're saying is we're done dealing with this, there's nothing more to do with this. The alternative is to have a path forward to do some more work.

Member Beach: Right, but --

Mr. Katz: That's all we're talking about.

Member Beach: I understand that part, but this is kind of a two-part deal where it doesn't go away, just any work to do anything about it. That part goes away. So what do we do with the beginning of the issue is not classifying workers correctly? Or what does that mean going forward? Is it still an issue that some workers may be classified incorrectly, which would therefore create a problem for them?

Dr. Taulbee: Okay, it's not a problem for them

individually because that's handled under dose reconstructions.

Member Beach: Okay.

Dr. Taulbee: Here the misclassification is within the total population. And in this particular case, as I've got up here, this is Finding 6. There's Finding 6, which to follow the example that my colleague, Megan, used a few weeks ago on Lawrence Berkeley of related issues. This is related to Finding 5, Observations 4, 5, and 6. So, Observation 6 is the recommendation to do a further sensitivity analysis.

Member Beach: Right.

Dr. Taulbee: Okay. So that's how these are tied together. But under this particular issue of where they looked at the targeted group, this is actually a finding that was listed. And this is along the lines of the 9.14 percent of this targeted group. And what we did for our analysis was a probability sampling, not a targeted sampling. And we looked at it and we came up with less than 5 percent.

So we don't feel that this is a significant issue with regards to these datasets, that our classification rate is sufficiently low as to our coworker models are valid.

Mr. Katz: So if you're closing it, what you're saying is -- if you're closing it, you're saying we agree there may be some differences, there may be some misclassification, but it doesn't have enough of a bearing on the coworker model to destroy the model, in effect, to make that model invalid.

And so you can just close it. And the observation would be, if you didn't want to close it and you wanted them to do more work before you close it, then you'd go down that route. And six months later you'd get that information to consider in whether you close it or not. So that's sort of what's on the table.

Member Beach: Okay.

Dr. Taulbee: Thank you.

Member Ziemer: Well, the specific finding here is Savannah River. I know the general model applies to everybody, but this observation is very specific.

Mr. Barton: It's only at Savannah River.

Co-Chair Clawson: You're using Savannah River data, but this is -- this is for you guys on that end of the table.

(Simultaneous speaking.)

Co-Chair Clawson: That's where some of the confusion is coming on this.

Mr. Katz: Okay. So, if you want, I mean, Henry's group can make a recommendation to close these with respect to the coworker model guidelines, that this doesn't show any inherent flaw in the coworker model guidelines. Your Work Group can make a vote as to whether this is -- again, for the Savannah River model that we're discussing -- whether this is an inherent problem that needs to be solved for the model, in which case you have to go down that six-month path. If you want, you can do that separately. You can do it together in this case, because we all understand what's going on with this.

Mr. Calhoun: But classification for individual DRs could be a different issue all together.

Mr. Katz: That's totally separate. That's not the issue because, again, they used their actual data.

Member Lockey: Henry, why don't you make a motion about your group?

Co-Chair Anderson: I'm just thinking about it. And, I mean, I think there is systematic misclassification for some of the job categories. Would you agree to that?

Member Ziemer: Systematic?

Co-Chair Anderson: Yeah, I mean, they're all said to be non.

Member Lockey: No, I don't agree with that. There's no systematic misclassification, no.

Co-Chair Anderson: But all of those job classifications that were mentioned, if you look at that list, they're all given to be non-construction workers.

Mr. Katz: But what Bob's finding is, is that there's a higher rate of misclassification as a result of that for that group.

Co-Chair Anderson: Yes, exactly.

Dr. Taulbee: It's not all of them.

Co-Chair Anderson: No, it's not.

Member Lockey: It's ten percent --

Co-Chair Anderson: No, but I mean -- but that's -- it's systematic because you coded them all in. You could have taken any of the others and coded them.

Dr. Taulbee: That's not -- let me show you a different example here. And this goes to the machinists, but this is a reasonable example of what we found in going through it. Well, maybe not reasonable. I hate to use general terms here. But, you know, there is 39 machinists here. Nineteen were private contractor, 12 were subcontractors. Okay? When we went through, there were 18 that were assigned to CTWs in going through their jobs. Two were assigned -- so, in this population of 31 machinists, they weren't all assigned to CTW. And they weren't all assigned to non-construction trades. All right? Eighteen of them were previously assigned to construction trades due to other job titles that they had in there that would say something like machinist/millwright or machinist/maintenance mechanic. And so they got

properly categorized.

So it's not all one or another is what I'm trying to communicate to you. The table that you're looking at just had the assistant on there. But we're using additional information to come up with that. That's kind of a condensed number.

Co-Chair Anderson: So you're saying there are some assistants in there that are coded correctly?

Dr. Taulbee: Yes. Absolutely, yes. Some of them, like if it's an assistant pipefitter, yeah.

Co-Chair Anderson: I mean those that were just one word, assistant.

Dr. Taulbee: Those are the ones that could be different. We'd have to go look at other details. What I believe SC&A did was they went and looked at those other details. And they found that nine out of ten of them are correct and one out of the ten was not.

Dr. Chalmers: And I think the list of occupations we were looking at were very generic bins that the subject matter experts had me put them in. Because that came along with the S-node (phonetic) that was from the master occupation table. And those bins were more generic.

Co-Chair Anderson: Okay.

Member Lockey: Ninety percent were classified correctly.

Dr. Taulbee: Of the hard ones.

Co-Chair Anderson: Yeah, but it's different than the overall group. I mean, the whole thing is, will this be used for the individual job classifications at any point?

Mr. Katz: No.

Dr. Taulbee: No.

Ms. Brackett: They don't need to be classified individually because they have bioassay results. By definition, if they're in the coworker study, they have their own bioassay results. That's what would get used in their individual dose reconstruction. It doesn't matter what category they fall into, they have bioassay --

Co-Chair Anderson: But you're not going to ever use this to look at if you have a large group with a job classification, and now you're going to do a coworker model that's specific to a subset of the workers. See what I'm saying now? If you'll do that and you were going to say, oh, that's something --

Dr. Taulbee: I see where you're going, okay.

Co-Chair Anderson: -- that it's going to be used for. And then you could potentially, if you did the assistants or the whatever, they wouldn't meet your 5 percent criteria.

Dr. Taulbee: Yes. I see what you're saying, yes. If we were to further sub-stratify, which we do not have plans to do, then I see what you're saying that, yes, they could end up from that standpoint.

Co-Chair Anderson: Yeah.

Dr. Taulbee: But, I mean, we don't have plans to go now to --

Co-Chair Anderson: As far as the impact on the overall --

Dr. Taulbee: Right.

Co-Chair Anderson: -- N for those. I mean, the alternative would be --

Dr. Taulbee: Yes.

Co-Chair Anderson: -- to just as was mentioned, take them out all together.

Dr. Taulbee: If we were to do a coworker model for

assistants at the Savannah River Site along these lines and pull all of those people out and look at them, they would fail at misclassification rate.

Co-Chair Anderson: Okay. That was --

Dr. Taulbee: So your criteria would work if we were to do that.

Member Lockey: Via the 5 percent figure.

Dr. Taulbee: Yes, they would fail that 5 percent figure.

Co-Chair Anderson: Right. I mean, that was my --

Dr. Taulbee: Okay.

Co-Chair Anderson: I'm just worried that when you start breaking groups up into small numbers and then bury them in a large group, the large group will always overwhelm.

Member Lockey: It will bury them.

Co-Chair Anderson: Unless it's random error, in which case it ought to be the same in all of them. And this would suggest it's grayer than a random error -- or misclassification, not necessarily an error. So does anyone out of the committee want to make a motion? The Chair shouldn't make a motion.

Mr. Katz: That's true.

Member Beach: Yeah, good idea.

Mr. Katz: Proper Robert's Rules.

Member Ziemer: Can you suggest what motion you'd like the members to make as you call for the motion?

(Laughter.)

Mr. Katz: Well, I can't make a motion, but I can tell you.

Member Ziemer: Right.

Mr. Katz: I mean, the motion that you guys are talking about making is to close the observation. Meaning you're not recommending they do this further work. And the associated finding is not what we call fatal for your coworker modeling guideline.

Member Ziemer: That was exactly what I was thinking.

Mr. Katz: That would be the motion that you might be making.

Member Ziemer: I move we close this observation for the, for our Work Group. Are you okay with that?

Member Lockey: Was that a combined motion?

Member Ziemer: Why don't we just approve it?

Mr. Katz: Or we can do it combined.

(Simultaneous speaking.)

Mr. Katz: We can do it either way. So if you want to make it one simple for everybody, that's fine. I think everybody --

Member Beach: Do you supposed Gen might have any comments? She's on the coworker model.

Mr. Katz: Yeah. Once we get the motion -- yeah, so Gen, are you there?

Member Roessler: I'm here.

Mr. Katz: Do you have any comments about this discussion about --

Member Roessler: I want to -- it's been a little difficult hearing. I think somebody's not muted. But I want to make sure I know which observation you're talking about.

Mr. Katz: So it's Observation 6 --

Member Beach: Page 24.

Member Roessler: Okay. So the idea is that, if this is closed, then SC&A's -- our suggestion for a scoping analysis would not be done.

Mr. Katz: Correct.

Member Schofield: Yes.

Member Roessler: Okay.

Mr. Katz: Correct.

Member Roessler: So I understand. I'm ready to vote when you get ready.

Mr. Katz: And Phil, are you okay too?

Member Schofield: Yes, I am.

Mr. Katz: Okay. Yeah, loud and clear. And then --

Co-Chair Anderson: Not doing the scoping, close out 6.

Mr. Katz: Close out 6, yeah. Yeah. Now, the only clarification that I need -- are we closing the associated findings too? Because the findings, you're not going to do anything with otherwise. You're basically inherently saying that the finding is not fatal for --

Co-Chair Anderson: Right. Yes, that's correct.

Mr. Katz: Okay, so then you're closing Finding -- Someone help me with this.

Member Beach: Five, six, and Observation Six.

Mr. Katz: Okay. Okay, so that is the motion. Someone needs to make it because I can't.

Member Ziemer: I made the motion that we close that.

Mr. Katz: Okay. Paul put forward the motion to

close Observation 6 and Findings 5 and 6.

Member Lockey: I second.

Mr. Katz: And Jim seconds. And why don't we -- anyone opposed? Does anyone oppose?

Co-Chair Anderson: On the coworker model.

Mr. Katz: Right, right. Well, in either. No, either. Is anyone opposed to this motion?

Member Lockey: I'm not sure they can vote on our motion.

(simultaneous speaking.)

Mr. Katz: It's a combined motion.

Member Lockey: Okay, that's fine.

Mr. Katz: No one's opposed?

Co-Chair Clawson: You could have run this whole process and had it done by now.

Mr. Katz: Okay, I don't hear anything on the phone, so it's passed. So those two findings and that observation are closed. Thank you.

Mr. Barton: We just had one last slide here. Harry, are you still on the phone with us?

Dr. Chmelynski: Yes, I'm still here.

Mr. Barton: Okay, the last slide here is really about the quality assurance assessment that you looked at. And it was Chapter 6, I believe, in our -- and I'll read the Observation and perhaps you can comment on it. But it's pretty straightforward.

The results shown in Attachment A of OTIB-81 demonstrate a high degree of confidence that the acceptable error rates are within the goals established for each test. However, this conclusion is dependent on the assumption that payroll ID issues identified would not affect the resulting

coworker distribution. That's Observation 7.

So Harry, if you want to give a brief overview of what you saw there, and maybe talk about the payroll ID issue that was identified that may or may not have an effect?

Dr. Chmelynski: Attachment A to OTIB-81 includes the results of a whole list of QA tests that were performed essentially to verify the data that's being used for the coworker model. This included the NOCTS datasets, both in vivo and in vitro, and the lab logbook data for americium, neptunium and some others, and also some quality assurance, which has already been talked about on the construction worker classification problems. So that serves as a good introduction to what this whole Attachment A is about.

There's 13 sections in it, and each section has perhaps sometimes more than one test being done because some of them have both the critical fields and the all fields being down separately. Also some of them were done and then done again for various reasons. And what we end up with is a list of about 25 different QA studies that were done. And these are all summarized in our report in one of the tables. And right now, I lost the page that tells me my table number. But it's in Section, what is it, about 6?

Mr. Barton: Yeah, Section 6. I'm looking.

Member Lockey: Section 6?

Dr. Chmelynski: Yeah. I lost the page that has the top of the table, so I don't see the table.

Mr. Barton: I think it's Table 18.

Dr. Chmelynski: Is that it? Okay.

Mr. Barton: Table 18 and 19.

Member Lockey: Getting there.

Dr. Chmelynski: Right. The first table essentially shows what the 13 sections contain. The second table is the breakdown of all the different tests that were done in this quality assurance program. And to make it more complicated, there's various methodologies used for the tests. Some of them started out before the final methodology was obtained. And therefore, there was a Round 1 and a Round 2 for some of these tests.

At any rate, I looked over all the tests that were done. And I agree with this idea that the critical fields should have a stricter standard of 1 percent allowable error rate as opposed to the noncritical fields, which was a 5 percent now.

Up till now, we've been talking about the 5 percent kind of study. They also did a 1 percent critical field study on several of these analyses. And the acceptable error rates in each case, whether it was a 5 percent or a 1 percent, they did manage to come within those errors in almost every case. But that took some time eliminating some errors that were found from consideration in order to get the final number. And in particular, the largest type there. And this was very prevalent. And in one case, it was at least half of the data. The error was identified as a payroll ID error.

And the notes that I read in that attachment indicate that most of these payroll ID error matches occurred because one of the documents would have a prefix on the payroll ID such as a T-dash or a 1-dash or a 0-dash. And on the other document, they would not have that prefix. So the computer would flag them as errors. But yet it was a clear that these two were matched; it's just that they were entered differently in terms of the payroll ID number.

So once they eliminated all of those payroll ID errors and a few other types of errors of similar ilk, they managed to get within the allowable error rate bounds. And in some cases, this involved, you know thousands of lines of data being checked. And I

looked at one here, thousands of lines of tritium data, 14 errors. Result of somewhere around a third of a percent, which is really good.

I tried to summarize all this in a graph at the end of the section. And what I did was I took all of the 25 cases -- I'm sorry, the 13 cases when the final analysis was done. And I used those error rates and I tried to combine all the different datasets into one picture. And what I found out was that we have a 99 percent confidence that the error rates are less than 1 percent. Which is pretty amazing because that includes some of the 5 percent target goal studies too in that simulation.

So I'm pretty happy with all these results in that QA section. And I'm pretty confident that the data we're using is what we want to be using. I guess that's it.

Dr. Taulbee: If I could explain a little bit of the payroll issue. As Harry mentioned, some people would have a -- well, all of the DuPont people had a four-digit payroll ID number. But sometimes whenever they would enter it into the bioassay logbooks or something along that lines, there is a row. And Row 1 would be your salaried DuPont people. Row 2 is your technical people. Row 4 was your construction trades people.

In Row 1, sometimes they would put 1-dash and the four-digit number. Sometimes they would put T-dash for the technical area and that four-digit number. And other times they would just enter that four-digit number. It's all to the same person. But if in the analysis, it was different than what was in the source document, it got flagged as a potential error even though it didn't have any impact carrying forward because we were using that four-digit number. And so that's, that difference of the payroll numbers. One of the main differences, I think.

Co-Chair Anderson: Good picking. Easy solution.

Dr. Taulbee: Yes.

Mr. Katz: Next.

Mr. Barton: That's it. I mean we can go over the --

(Simultaneous speaking.)

Co-Chair Anderson: So do you want to tell us that observation analysis? I would think so.

Mr. Katz: Yeah. Well, it's --

Co-Chair Anderson: Do we need to for an observation?

Mr. Katz: We don't really need to close that out given the nature of it.

Co-Chair Anderson: Yeah. There was an explanation?

Mr. Katz: Yes, an explanation doesn't have an action.

Co-Chair Anderson: No.

Member Lockey: Moving right along.

Mr. Barton: I mean I have a conclusion slide just reiterating what we already talked about.

Mr. Katz: Okay, so we probably don't need to do it again?

Mr. Barton: I don't think so.

Dr. Taulbee: Can we go back to the first findings to find out where we have to -- as to whether we have an action item or not?

Mr. Barton: Oh well, okay.

Mr. Katz: Well, I know you had responses to some of them.

Mr. Barton: Oh sure, okay. Let me see if I can share my window.

Member Lockey: Finding Number 1 is the bioassay

variability.

Dr. Taulbee: That's correct. I just want to make sure who has which.

Okay. So back on the bioassay variability, I believe that's an SC&A action or not.

Mr. Barton: I think the variability was we were going to get that 2014 report that Joyce had written up. And then we'll take a look at that in the context of the discussion today --

Dr. Taulbee: Okay.

Mr. Barton: -- of the discussion of the methods.

Dr. Taulbee: Okay, just so that people are aware, that report that Joyce wrote, we did a response to it. And that response is in Attachment D of OTIB-81.

Mr. Barton: We responded to the response.

Dr. Taulbee: Attachment D of OTIB-81.

Mr. Barton: I think the question was where the genesis of that came from. So what's in Attachment D is actually addressed in the OTIB report which was released all at the same time. The variation is the same whether it's a DTPA sample or not.

Dr. Taulbee: And that's why I wanted to circle back to this one because I don't think that Joyce's response is answering Dr. Lockey's question. But I don't know that for sure. That's up to you all to decide. Our response to Joyce's paper is in OTIB-81. We copied it in there as an evaluation of variability and our conclusions associated with it.

What I was hearing earlier today was that you were feeling that the combination of variability and the MDA levels, which we're not using for americium, call into question the entire method. And that's where --

Member Lockey: That's what I heard.

Dr. Taulbee: And if that's an issue --

Mr. Barton: Was there another slide in that -- that sort of conclusion on this?

Dr. Taulbee: No.

Mr. Barton: No.

Dr. Taulbee: I mean --

Mr. Barton: I thought the stance was pretty much that, once you start averaging --

Dr. Taulbee: Oh, that is our next slide is that because of the averaging that ends up happening between the analytical results, the individual bioassay being converted into a Time-Weighted One Person One Statistic, the fitting of that data, the use of the 50th and 84th percentile, overall this variability just gets averaged into a reasonable model that we use. But further is some of your discussion on the actual analytical method not being appropriate and not being adequate. And that's actually a different issue.

Mr. Barton: Well, I think when we look at that data, we see certainly troubling observations in the variability. And when I see that, well, it averages out, that doesn't quite ring true because -- I'm going to take this right out of the Implementation Guide. This is on Page 3. Prior to this as in prior to all these steps, it is necessary to establish that the available internal or external monitoring measurements were technically capable of evaluating the monitored workers' exposure impact. If the techniques used to monitor exposed workers were inadequate, they clearly cannot be used to assess exposures for unmonitored workers.

Now further on, on Page 5, the quality of the available data also needs to be considered. This would include a review of appropriate collection and analysis of blank samples. When paired measurements are available, the precision between

measurements should be examined. If widely different results from the same aliquot are observed, the effect this might have on the usefulness of the data should be considered.

When I read that, I don't think we're just going to try to figure out if it changes the end result. And it's prior to this -- that's how the first sentence starts. Prior to all these averaging -- because I agree. I mean through all -- you know, you average all these aliquots and you might have two samples in a day. You average those. You get the TWOPOS result. You put the TWOPOS result in the distribution of all the workers in that year. And then you pick an intake regime. And then you, you know, draw your intake line through that, then I agree. You know, the variability at the very beginning, the front-end problem is going to get washed out. But the whole point was when you start with this, you have to make sure that the data is adequate. Our concern is that observed variability.

Co-Chair Anderson: Let me kind of cut to the chase here. My view on what our coworker model committee asked for is an example of, can it be applied, and does it cover all the issues? Not, do we agree with how it was done on a specific site? I would say that has to be a -- I mean your --

Dr. Taulbee: Right.

Co-Chair Anderson: We can go round and round on has this been done appropriately as opposed to is the methods that we as a group decided ought to be applied, it can be applied.

Dr. Taulbee: Right.

Co-Chair Anderson: How you interpret that, you're never going to have the methods particularly to find the detail of, is it appropriate in this case or that case, I think. So my sense is rather than go through some of these issues or in our group saying -- I mean one, I didn't see anything you did until your presentation. So I haven't had a lot of time to think

about that. I did have the time to look at, is the graph that we have been working with -- is that appropriate? Does it include all of the issues that we'd like to have addressed when you say is this a coworker model that can be used?

So I guess what I would look for our group is to say my sense is that it can be utilized. We don't need to go back and redraft it to include all sorts of little caveat things that may come up under every possible circumstance.

It's been an interesting discussion on the modeling and all of that, but it can be done. Which model is really dependent upon the site-specific issues that I don't think our group has delved into over the years like Savannah River has done. So that would be my recommendation so we can move on to let Savannah River deal with it.

Member Lockey: Do you want to make that a motion?

Mr. Katz: I mean he's the Chair. He's the Chair, so one of you guys put the motion.

Member Ziemer: Well, this is a site-specific issue here. That's what you're saying.

Co-Chair Anderson: Yeah, yeah.

Member Ziemer: The analysis of those samples. And I think the concern on the variability has to do with the higher points toward the end, toward the tail. Is that not the case?

Dr. Taulbee: That was the ones that they primarily --

Member Ziemer: You're not concerned about the real low-dose values.

Mr. Barton: We looked at the high values.

Member Ziemer: Yeah.

Mr. Barton: We did not look specifically for variation.

Member Ziemer: Right. So it's an SRS specific kind of issue.

Mr. Barton: That's correct.

Member Lockey: But in relationship to the coworker modeling --

(Simultaneous speaking.)

Member Ziemer: I agree with that.

(Simultaneous speaking.)

Co-Chair Anderson: It can be done. There are tools to do it. Which tool on a site-specific basis, that's really specific to the -- you know, to the --

Dr. Taulbee: Okay.

Path Forward on Coworker Guidelines

Co-Chair Anderson: We aren't going to reject the coworker model --

Dr. Taulbee: Methodology.

Co-Chair Anderson: -- methodology simply if we decide that, well, the data isn't good enough at Savannah River. It just means --

(Simultaneous speaking.)

Co-Chair Anderson: -- and we picked the site that had a beaucoup amount of data.

Mr. Katz: So then the issue shifts to -- so there's nothing for SEC Issues Work Group to do about this.

Co-Chair Anderson: Right.

Mr. Katz: There's nothing to close here for you guys because this doesn't have a bearing on you being able to endorse --

Co-Chair Anderson: Right.

Mr. Katz: -- or not endorse the draft guidelines.

Co-Chair Anderson: Yeah.

Mr. Katz: So it falls to the SRS Work Group as to what the action is forward on this matter. Because that's where --

(Simultaneous speaking.)

Co-Chair Anderson: -- I want to be able to close out that this draft had been there and NIOSH had been working with for some time. It is a valid and appropriate tool to use.

Mr. Katz: Let me finish.

Co-Chair Anderson: But for SRS, you don't have to resolve this now.

Co-Chair Clawson: No, we don't.

Co-Chair Anderson: Right.

Male Participant: Because the data --

Member Lockey: But we need a path forward.

Mr. Katz: We need a path forward. But they may not be ready to even speak to a path forward immediately, in which case, you know, it will get addressed in the next Work Group meeting. It's just up to SRS Work Group as to what you think -- if you can get your heads around this -- what the path forward or what the matter is. How you want to deal with this. That's all I'm saying. I'm not saying you have to solve it now, but obviously it's on the table for you.

Mr. Barton: If I may, the mystery of this again, has gone back many years. And the most recent response is from NIOSH -- Harry, if you're still on the phone, I don't know how much you've gotten to take a look at that. And if you feel there's anything

for us to do to respond to the response, that might be a potential path forward. Because again, NIOSH responded to the section in our OTIB-81 review concerning the variability in the most recent response to the response.

Member Lockey: I'm still confused. I'm not confused. I'd just like to --

Co-Chair Clawson: Yeah, you are.

Member Lockey: Yeah, I am. My wife says I'm confused all the time. But in relationship to -- are there still just -- is there one issue or are there two issues? Because one issue is was the test being utilized at Savannah River, a technically reliable test? I think that's what -- you inferred that it was not.

Mr. Barton: Well, I think that might have been a little strong.

Member Lockey: Then would you -- then pull that back.

Mr. Barton: Yeah. I think what we're looking for is the data that we're going to base the coworker model on: is it sufficiently accurate and adequate to use -- to enter the coworker process? And at the very front end of this process, we look at the data that we have and we see concerning variability.

Member Lockey: Okay. So then we get to the variability issue, which NIOSH has already written a report on -- or Joyce has written a report on and you responded. Is that correct?

Dr. Taulbee: Yes, that's correct.

Member Lockey: So I think I would recommend, Brad, that both those be sent out to committee members.

Dr. Taulbee: Okay. Our response is to OTIB-81.

Member Lockey: Right, but --

Mr. Barton: I'm talking about what you responded to on Monday.

Co-Chair Clawson: But this is part of the confusion we're getting into because we're looking at OTIB-81, which is coworker, but we're using Savannah River data to be able to do it. And right now, to tell you the truth, I don't want to deal with this in the confusion. I would rather deal with it as Savannah River when we get to that point. And I'm sure we're going to review that in the Savannah River evaluation.

Member Lockey: Okay.

Co-Chair Clawson: Because to tell you the truth --

Member Lockey: So this is an OTIB-81 issue?

Co-Chair Clawson: Yes, that's what we're discussing this morning till now.

(Simultaneous speaking.)

Co-Chair Clawson: I know. But this is what I'm saying. This is part of our confusion. And I want to be able to deal with that on the Savannah River issue.

Mr. Barton: The implementation guide itself allows for this type of analysis to happen. I think that's the point from the coworker methodology point here. And that process is going on with Savannah River currently. So as far as the implementation guide is concerned, there's already the provision in there that we need to look for this, you know, type of thing -- this type of analysis --

Member Lockey: Right.

Mr. Barton: -- for each site that we, you know --

Member Lockey: And the variability issue has already been addressed. So I need to update myself on that.

Dr. Taulbee: We included it in this OTIB-81 -- our analysis.

Member Lockey: Okay.

Mr. Barton: And SC&A has the response to that in their review of OTIB-81. And then there's a second response to that.

Mr. Katz: So this goes on the agenda for the next SRS Work Group meeting following up on this.

Co-Chair Clawson: Right.

Mr. Katz: Okay. So let's capture it well that way, so we can -- So it will be on the agenda for the next SRS Work Group meeting, whether it's in January or February or whenever, to move forward on this.

Mr. Fitzgerald: Well, we have -- we have NIOSH's response to the 0992 response. And we have our response to NIOSH's response on 0081 OTIB. So you have this.

Mr. Katz: Yes.

Mr. Fitzgerald: It all happened in the last couple of weeks. Am I right?

Mr. Katz: Yes.

Mr. Fitzgerald: So you know, in a sense, yeah, we have all that for the next Work Group meeting.

Mr. Katz: Yeah, it's all -- it's all on the table.

(Simultaneous speaking.)

Mr. Fitzgerald: And the only thing that really may be a pertubation is that today we had some discussion which led to closure as we went on a few on those items. That's what I think you're trying to disposition. What are those few items that we can take off the table at the next Work Group meeting?

Dr. Taulbee: Yes, trying to reduce the scope. Trying to get things done.

Member Lockey: I wanted to walk back the invalid technique.

Mr. Katz: Okay. Okay, good, good. So that's Finding 1.

Dr. Taulbee: Finding 1. Finding 2 is the multiple imputation, and this is related to Finding 3 and Observations 1 and 2. Do we have as NIOSH any action items -- things that we are to follow up on?

Mr. Barton: I think the action was actually ours to take a closer look at RPRT-96.

Dr. Taulbee: Okay.

Mr. Barton: And the imputation method that came out in January.

Dr. Taulbee: Okay. So the action then is SC&A and RPRT-96.

Member Lockey: What's the action? I'm sorry.

Mr. Katz: SC&A is going to look at RPRT-96.

Dr. Taulbee: Which details the method.

Member Lockey: RPRT-96 detailing method.

Dr. Taulbee: Yeah. And that actually falls to Finding 2 and 3 really.

Dr. Chalmers: Have you looked at 71?

Dr. Taulbee: Yeah, they haven't looked at 71, but we might. It's the external one. It's simpler. And they have already approved the Pantex coworker model -- external coworker model, which dealt with it or used it. But if you want to review 71, it seems appropriate.

Mr. Katz: Okay, so that's --

Dr. Taulbee: It's your call.

Mr. Katz: So that's associated with Finding 2?

Member Beach: Yes.

Mr. Katz: So RPRT --

Dr. Taulbee: RPRT-71.

Mr. Katz: RPRT-71. So I guess what we're saying is, SC&A, take that under advisement how you reviewed that in reviewing this. Right?

Dr. Taulbee: Yes.

Mr. Katz: Does that make sense?

Dr. Taulbee: Yeah.

Mr. Katz: Okay.

Dr. Taulbee: Excellent. Okay, I think we only have one more to go through here, and that is Finding 4, claimant cutoff of adding more data. And this is the additional 2000 claimants since we cut off our coworker models for data analysis.

Co-Chair Anderson: Is the 2000 just for Savannah River?

Dr. Taulbee: Yes.

Co-Chair Anderson: Okay.

Member Lockey: Because there's going to be a lot more total.

Dr. Taulbee: There will be more tomorrow.

Co-Chair Anderson: Oh no, I mean not just for Savannah, but --

(Simultaneous speaking.)

Co-Chair Anderson: -- other coworker models. Are you using the same 2011 cutoff date?

Dr. Taulbee: No, no, not at all. Not at all.

Co-Chair Anderson: Okay, good.

Member Beach: Bob brought up a good point on this because you had to combine certain data points because you didn't have enough. So that would be a reason to move forward and add the other 2,000 or some variable amount of those.

Dr. Taulbee: I mean, if that's what the Work Groups want, we can do that. But you are looking at another year.

Mr. Fitzgerald: That was essentially the trade-off. It was just the level of effort necessary to --

Dr. Taulbee: Absolutely.

Mr. Fitzgerald: -- give you that additional advantage.

(Simultaneous speaking.)

Mr. Fitzgerald: That's kind of a NIOSH -- I mean it's the classic balance that I think the agency has always has to come to. Is it worth it? And I'm not sure we can answer that.

Mr. Katz: Well, I mean the Work Group -- if the Work Group has a perspective on that, I mean honestly if that has a bearing on your position on how good the coworker model is, then of course you're stuck with --

(Simultaneous speaking.)

Mr. Fitzgerald: Yeah, but the part of the equation I think the Work Group would have difficulty with is the part that NIOSH has, which is the level of effort and the issues surrounding that, that would be necessary to --

Mr. Katz: Right.

Mr. Fitzgerald: -- generate it. And that would then go with the advantage of doing so. And I think that would be a piece of input that Tim would provide that, you know, is it worth it given that effort? And you know, both the numerator and the

denominator, is it worth it? And how much time will it take? I think intuitively, you're saying it's going to take a lot, a year at least.

Dr. Taulbee: Yeah.

Member Ziemer: Well Tim already also spoke to the outcome as not having a great impact, in your opinion, at least.

Dr. Taulbee: That is my opinion, yes. That is correct. In my opinion, it's not going to have --

Member Ziemer: If we anticipated a substantial change in the impact, then it might be worth stretching it out. But if it's just going to delay closing some issues and getting the claims adjudicated, then it's not worth it.

Mr. Fitzgerald: Yeah. And I think that trade-off is one that happens all the time. And I think NIOSH is in the position to advise the Work Group as to whether that's the case or not. And I think your advice then would be -- wouldn't justify the effort.

Dr. Taulbee: And that is correct. That is smart advice. And I'm trying to pull up the actual uranium ones to show this -- it's OTIB-81.

Co-Chair Clawson: And see, from the Savannah River Work Group's side, we can take care of that quite rapidly. And I believe it's called an --

Co-Chair Anderson: SEC.

Co-Chair Clawson: -- SEC, yeah.

(Simultaneous speaking.)

Mr. Fitzgerald: But you know, this balancing would be for every single site. I mean this is not -- this is a generic question. And we're going to see the same question played out at other sites. And that's the perspective, I think the Board will need to hear is to, you know, how many of these data points do you need? And beyond which, it's not going to give

you any return for the investment. And I think that's something that only NIOSH can answer.

Dr. Taulbee: If you look at the uranium for Savannah River here that I've got pulled up -- this is M type uranium. And you can see that four construction trade workers out 1980 through 1990. We combined some years. But you don't see a huge movement in that data.

Dr. Chalmers: They're two-year intervals, I think --

Dr. Taulbee: Yeah.

Co-Chair Clawson: -- all the way through.

Member Lockey: It's two-year intervals?

Dr. Taulbee: Yeah.

Member Lockey: Okay. So that fits within the guidelines. Okay.

Mr. Katz: So, I mean this is a matter you can close now because I guess you're not going to get more information about it.

Dr. Taulbee: Yeah. I mean we are recommending not to pursue it. If you feel it should be pursued, let us know and we will.

Member Lockey: I move that we close.

Mr. Katz: Well, this isn't --

(Simultaneous speaking.)

Mr. Katz: You're on the right side of me, it's confusing.

Member Beach: So Ted, it's real easy, the outside is not. The inside is.

Mr. Katz: Oh, I didn't notice that.

Co-Chair Clawson: This being said, this is why we have not proceeded any further with this a long

time ago because we did not see the benefit of it. But in their evaluation of the coworker data, this came out of it. So I don't see any benefit from it because we're not going to get any more.

Member Lockey: It's just going to delay it. And a year from now, it will be delayed again.

Co-Chair Clawson: We've already delayed a lot.

Mr. Katz: Phil, are you there?

Member Schofield: I have to agree that I just can't see waiting for more data points. I think we have sufficient to go ahead and close it at this time.

Mr. Katz: Okay, that's three. And Dr. Richardson is not on the line. Right? So that three out of four is enough, though. That's the quorum of the Work Group. So Finding 4, we're closing. Not worth the push.

Co-Chair Anderson: At this time.

(Simultaneous speaking.)

Dr. Taulbee: And we're closing Finding 5 and 6, which were the misclassifications.

Dr. Taulbee: So we are good then. We have a path forward.

Mr. Katz: Good work, everybody.

Co-Chair Clawson: Tim, I told you years ago how you could take care of that. And you just don't want to listen.

Mr. Katz: All right, now where are we?

Dr. Taulbee: Do you want to take a break while trying to figure out how to get that connected?

Mr. Katz: Well, do people need a break right now?

Member Beach: Sure.

Dr. Taulbee: Because it's going to take a reboot.

Mr. Katz: I'm sure everybody's happy for a break. So ten minutes, what do we need?

Dr. Taulbee: That's fine.

Mr. Katz: You guys have a ten-minute comfort break. Folks on the line, ten-minute comfort break. And I'm just going to put the phone on mute, but I'll take it off.

(Whereupon, the foregoing matter went off the record at 2:51 p.m. and resumed at 3:08 p.m.)

Mr. Katz: Are we ready to move on? Okay. Okay. We're back online. Let me just check and see. Sorry we're a little bit slow. Gen, are you on there?

Member Roessler: I'm on.

Mr. Katz: Great. And how about you, Phil? Are you there?

Member Schofield: I'm on, Ted.

Mr. Katz: Great. Thanks, Phil. And is David Richardson on by any chance?

No, I don't think we have him today. Okay.

Dr. Taulbee: Are people able to see the presentation right now?

Mr. Katz: Can you, folks on Skype, can you see the presentation?

(Simultaneous speaking.)

Mr. Katz: Yeah, okay, Gen just said yes, so --

Dr. Taulbee: Okay, good. All right. This next presentation is where we're delving into the details of stratification and in this particular case it's an evaluation of the bioassay data for subcontracted construction trades workers at the Savannah River Site.

And before I get going here I've really got to acknowledge the team that did this, and this would be the ORAU team. Mike Mahathy was the lead on this particular effort. It took way more effort than what we initially thought. And this was from the table.

I think initially I had mentioned to Brad and the other Work Group members that I probably could get this done in nine months. That was not the case. This took way longer than that and -- just so that people are aware of that. But Mike and his team did a fantastic job.

I want to go through some background of how we got to here and then go through the work permit sampling plan and then get into the evaluation.

And I'm doing this in reverse order for a reason, okay? And I'll get to that from the background standpoint and then do some conclusions. And then what I'm going to talk a little bit about at the end is an evaluation timeline.

So the background, first of all, as I mentioned this morning, coworker model use. We developed coworker models, or co-exposure models I should call them now, and we will. By the way, we will start changing our documentation.

(Simultaneous speaking.)

Co-Chair Anderson: From a public perception --

Dr. Taulbee: Yes.

Co-Chair Anderson: -- that's one accomplishment we can pass.

(Simultaneous speaking.)

Member Ziemer: A major accomplishment.

Mr. Calhoun: It's too easy now to do it.

(Laughter.)

(Simultaneous speaking.)

Dr. Taulbee: And so we developed co-exposure models because we recognized that some workers were not monitored, okay? For a co-exposure model to be valid it needs to have a representative sample. A representative sample is all that's really needed for it to be valid. Okay?

If all exposed workers were monitored for every radionuclide, there'd be no need for a co-exposure model. So coming into this you've got to recognize that 100 percent, if we got to 100 percent, well, there's no need for the co-exposure model. All right?

So one of the issues that came out of the December 2017 Advisory Board meeting was one of SC&A's conclusions, and it was SC&A concludes that the bioassay data for -- or for construction trades worker subcontractors specifically, and construction trades workers generally is demonstrably incomplete for 1989 to 1998 and likely before that time period and does not satisfy the criteria set forth in NIOSH's draft criteria for the evaluation and use of coworker datasets.

Okay, now I added some emphasis here because the time period that we started here to address was this '89 to '98 time period. Now, through further discussions we started going back, so let's try and address that initial issue that we handled or that was left at that Advisory Board meeting.

At that time we indicated that we disagreed. We believed that 90 to 87 percent direct monitoring for subcontractors is not demonstrably incomplete and does satisfy the criteria.

We went on and did an additional analysis that you talked about during the last SRS Work Group meeting indicating subcontractors were monitored, and that evaluation indicated that 91 percent of them who were claimants from '91 to '97 have some form of internal monitoring data. Okay?

Savannah River used a Defense in Depth approach for the radiological control program with the intention to prevent non-tritium intakes. And the reason I say non-tritium there is they actually did allow some small intakes of tritium. They didn't go to an extreme level of trying to put everybody in bubble suits every time they were around tritium.

And as you saw from the previous coworker models those doses are really low, okay? So but for all others they had, what they had is a zero policy, zero intake policy. They had engineered controls. They had glove boxes. They had procedural controls. They used PPE.

They did surveillance to verify these were working. They did air monitoring. They did facility contamination surveys. They did personal contamination surveys, and they did routine and job-specific bioassay, okay? So this is all part of the radiological control program.

Mr. Fitzgerald: Tim, just this is a clarification question. All right. We're still talking in the context of '89 and beyond, right, or beyond '89?

Dr. Taulbee: Yes.

Mr. Fitzgerald: Okay. So this description of Defense in Depth is really the so-called Westinghouse era. I just want to make sure I understand the context of this.

Dr. Taulbee: This -- yes. This is --

Mr. Fitzgerald: This is the --

Dr. Taulbee: -- is specifically for post '89, yes.

Mr. Fitzgerald: Post '89, okay. Thank you.

Dr. Taulbee: Now, engineered controls have been there all along.

Mr. Fitzgerald: Right.

Dr. Taulbee: Okay?

Mr. Fitzgerald: Right.

Dr. Taulbee: When they first started doing --

(Simultaneous speaking.)

Mr. Fitzgerald: Yeah, I'm talking about the whole package is Defense in Depth and all --

Dr. Taulbee: Yes.

Mr. Fitzgerald: -- that. That describes the Westinghouse program as we know it.

Dr. Taulbee: Evidence of this though appear back into the DuPont.

Mr. Fitzgerald: I agree.

Dr. Taulbee: Okay.

Mr. Fitzgerald: Okay.

Dr. Taulbee: Because there is air monitoring. There's facility contamination --

Mr. Fitzgerald: Right, right.

Dr. Taulbee: -- personnel contamination. It's all --

Mr. Fitzgerald: Right, okay.

Dr. Taulbee: Okay, a good clarification. Routine and job-specific bioassay, in this time period of '89, '90 there's really no practical difference between the two, okay? It's used to verify the effectiveness and procedures of the procedural and engineering controls.

It's also used to trigger for-cause or special bioassay, okay? If somebody comes up on a positive on a routine sample, they go and do an investigation.

And it's requested from workers who have a

reasonable potential for intakes but who SRS was confident did not have intakes in excess of two percent of the annual limit. So it's not that they didn't have any intakes, it's that they would be in excess of two percent of the annual limit. Okay?

And these are slides that I presented to the Board back in December of 2017, this beginning part, these first three or four slides, just to recap everybody's memory.

Westinghouse further stated that workers themselves were the last line of defense in the workplace indicator program, which is the reason why a confirmatory program for workers was conducted.

Okay. Here is how in the 1990s how Savannah River did -- radiation work control and bioassay monitoring. A worker had to attend Rad Worker II training. After their training they were issued a radiation qualification card, and there's an example of one that I've got up there.

A worker told then go to sign in on an RWP. The worker would check the bioassay codes on his radiation qualification badge against the RWP requirements or the area for the bioassay. Okay?

And what this is, if you look here, you'll see the bioassay codes right here, Pu-02, Eu-02 and Sr-01. Pu-02 meant plutonium, twice per year. EU is enriched uranium twice per year. Sr-01 meant strontium-90 once per year.

If you can go back to, and, Josie, if you remember that frequency diagram chart that I had in the coworker one where it specified the frequency of bioassay by area that people were required to do. So DuPont did have procedures back then.

But when you get into the Westinghouse area it was done on an individual basis like this. So those went away, and this was the controlling method as to whether somebody would be monitored or not.

And there was a determination as to whether this person versus that person should be, and it depended upon which area they were going into. Okay?

Mr. Fitzgerald: So just one clarification on this one here. You say 1990s.

Dr. Taulbee: Yes.

Mr. Fitzgerald: But, you know, I think we all understand that Westinghouse put the program in place, I mean, in terms of RQB and the whole --

Dr. Taulbee: This actually precedes Westinghouse.

Mr. Fitzgerald: Okay. So you're saying this actually goes back before 1990?

Dr. Taulbee: Yes, they started using the radiation qualification badges before then.

Member Beach: 2/16/90 is the date on the corner there.

Mr. Fitzgerald: But the --

Dr. Taulbee: Yeah.

Mr. Fitzgerald: -- so you're also saying that they'd had an active RWP program before '89?

Dr. Taulbee: No. At that time it was depending upon which area they were going to be going into as to whether they had -- what their bioassay criteria was and the monitoring frequency.

Mr. Fitzgerald: So in terms of who gets --

Dr. Taulbee: It would be specified by the job permits at that time.

Mr. Fitzgerald: The work control and bioassay monitoring, the RQB, may have predated Westinghouse, but the other elements may or may not have?

Dr. Taulbee: That is correct.

Mr. Fitzgerald: Okay.

Dr. Taulbee: The job plan components of it.

Mr. Fitzgerald: Okay, thank you.

Dr. Taulbee: Okay? And so a worker goes in -- a worker goes to conduct -- goes and conducts their work. And then the worker leaves a bioassay based on either the routine schedule -- if they're on a routine schedule for plutonium they did not have to leave a job-specific bioassay.

If a worker, say two workers went in, one was a sheet metal worker, one was a laborer going into a same area to do a job, and they come out and the laborer -- it's a plutonium area, and the laborer's on a routine bioassay, he doesn't have to leave a job-specific.

The sheet metal worker wasn't on routine plutonium bioassay. They were supposed to leave the specific bioassay, job-specific bioassay. That was how this is to work, same work being conducted. Okay?

Routine versus job-specific bioassay, most of the workers, 95 percent according to the site, were on a routine bioassay -- monitoring method.

SC&A has postulated that subcontractors were primarily on job-specific bioassay. And this is from the November 2017 Work Group meeting. The question of how complete is complete enough for coworker development can only be answered in the context of coworker guidelines and stratification assumptions that have been validated.

They guide what datasets can be legitimately applied. However, 79 percent incompleteness strains credulity. And so what SC&A implied is that only 21 percent of the subcontractors were monitored.

Mr. Fitzgerald: Okay. Before you leave this one, it's

not so much our implication, as you know. This was a Westinghouse self-assessment of its own job-specific bioassay program in 1997 that found only 21 percent of the bioassays were submitted.

And of course this was self-reported to DOE, who turned around and cited Westinghouse under the Price-Anderson Act and fined them. And that was a compelling reason for what we have done since then.

So we're not implying that only 21 percent of the contractors were monitored across all years. We said back then that with the self-assessment finding of 21 percent completeness it's a compelling reason to examine going backwards whether or not completeness was an issue for preceding years.

And that then led to looking at a number of different sources of documents to see whether or not one can establish the level of completeness for job-specific bioassays in preceding years, which has proven difficult because of lack of records.

But as far as applying 21 percent across-the-board we did not. That was a finding by Westinghouse that we, in fact, said, you know, had a compelling basis for looking at the question of completeness.

And the issue of strains credulity comes in when we're talking about completeness because I think there was a lot of dialogue about how complete is complete.

And I think our point is quite apart from all of the, you know, rationales for that question, the fact that Westinghouse confirmed they had 21 percent for that year, I think, is compelling enough to look at completeness.

That's not a borderline issue or a gray area. You're talking about a year where, you know, 79 percent of the job-specific bioassays were not submitted.

So that, it -- just a little more context on that. You

know, we're not -- we did not invent the number. We did not suggest that number applied year by year. We were saying the fact that that was a finding that led to an enforcement action and led to resampling by Westinghouse for every one of those that were missing certainly suggests that one needs to look at completeness for other years to see whether or not it's an issue.

Dr. Taulbee: Okay

Mr. Fitzgerald: That was the context of how that came about.

Dr. Taulbee: Well, I understand. Okay, from what you're saying is that this should be investigated further, which is what we've done. That's what generated this report.

But in the context here, this is

basically presenting that our datasets are incomplete and that using them strains credulity. These are your words, not ours, not DOE's.

Mr. Fitzgerald: Yeah, yes, but, you know, the context, don't miss the context because I think a lot of times in the presentations I've seen in the past there's a quote pulled out without any explanation or qualifiers.

And it suggests that we're doing or saying something is prohibited, dose reconstruction is prohibited. That was used once. That this applies to the regular, routine bioassay program. It does not.

This is explicitly on the question of RWP-directed job-specific bioassays and using a finding that the contractor generated. And we treat it as a red flag. And we're saying in terms of how complete is complete, the fact that they came up with 79 percent incompleteness would certainly compel one to look at this further, to look at whether or not it has implications for the coworker model. And that's the context we've raised this.

And we've raised it now for two years.

Dr. Taulbee: Yes, sir?

Member Ziemer: Well, Joe, can you remind us what year did the event actually occur? Were they looking at a particular year, the 21 percent?

Dr. Taulbee: 1997.

Mr. Fitzgerald: That was 1997.

Member Ziemer: '97, okay.

Mr. Fitzgerald: And, you know, they didn't look at the previous years because, again, they didn't think it was cost-effective to go back and actually look at some of those records.

But nonetheless, that one year -- that one year -- and they had, you know, there was indications there was an issue --

Member Ziemer: Yeah.

Mr. Fitzgerald: -- and they had done some initial sampling before that and found not quite this dramatic an incompleteness but still found some incompleteness.

And they, at DOE's prodding, this is the local field office, went ahead and did a 100 percent sampling. And this is the only 100 percent sampling on this issue that's been done, quite frankly, because it was a contemporary.

They had all the records, which we don't obviously now, and this is what they came up with. And it was dramatic enough that DOE took action and even went so far as in 1998 did a DOE-wide moratorium on enforcement actions on this subject by itself, the bioassay programs.

Dr. Taulbee: Okay.

Mr. Fitzgerald: So it was a pretty compelling issue.

Member Ziemer: But it wouldn't be surprising if the subsequent years were much better then.

Mr. Fitzgerald: Well, it --

(Simultaneous speaking.)

Mr. Fitzgerald: Yeah, it certainly -- it was certainly an attention-grabber.

Member Ziemer: Yeah.

Mr. Fitzgerald: And it has -- it goes straight to the question of how complete is this segment of bioassays? And that's the reason I think we wanted to flag it as a significant issue. That's kind of a clarification.

Mr. Katz: Okay, before anyone else speaks, someone on the line has -- they have their line open. I can hear their background, and it's not so troubling to folks in the room, but it may be for other people on the line trying to listen. So can you please mute your phone if you're listening? *6 to mute your phone or a mute button. Thanks.

Dr. Taulbee: Okay.

Mr. Katz: Sorry.

Dr. Taulbee: Okay, Mike, did you --

Mr. Mahathy: Yeah, so I have something to add, a little bit of clarification.

Dr. Taulbee: Sure.

Mr. Mahathy: So there was an assessment of 3,200 samples --

Dr. Taulbee: I'm getting ready to go through that, Mike.

(Simultaneous speaking.)

Mr. Mahathy: Okay.

(Laughter.)

Co-Chair Clawson: Hold on, Mike. We'll get to it.

(Simultaneous speaking.)

Mr. Mahathy: -- only -- okay.

Dr. Taulbee: Yeah, the next slide I have up is dealing with those 3,200. Okay.

Mr. Mahathy: All right.

Dr. Taulbee: Okay. During this time period, SRS did a limited assessment of the 3,200 bioassay requirements of all the workers. And here's -- the system that they had set up wasn't working like they thought. This is what was working. This is what they determined was happening. Okay?

And so just to walk you through here is you've got 3,200 workers signing in on RWPs. Ninety-five percent of them go down this path off to the right here of leaving routine samples.

Did all of them leave routine samples? No. Some of them went on to a delinquency tracking system. Those that did ended up down here.

Now, this is saying 95 percent here of these 3,200 made it directly over here. These got dispositioned somehow from that delinquency tracking system. I'm not exactly sure how many of them were completely followed up. That we don't know.

We do know that five percent were not left as required, okay? Ninety-five percent routine bioassay, five percent of these job-specific ones.

Mr. Fitzgerald: What timeframe, Tim?

Dr. Taulbee: This is the 1997.

Mr. Fitzgerald: This is '97, okay.

Dr. Taulbee: This is the fine year. This is the NOV. Okay? So you go down here and worker submits the

required sampling. Some of them did. In this particular case under this 3,200 bioassay sample, 33 percent submitted their samples.

And I don't know why that keeps popping up.

And so they come over here in sample received. And so but 67 percent of them did not, so 67 percent of five percent results in 3.35 percent or about 107 of the 3,200 samples.

Now, this got worse. This was a limited assessment. They did a further assessment later in the year. This is where that 79 percent and the 21 percent comes into play.

But again, it's 21 percent of five percent, 79 percent of five percent. Actually, I shouldn't say that. We believe it to be 79 percent of five percent and 21 percent of five percent.

We don't have these numbers as to what N was when they did that full assessment. We don't. We believe that these ratios held, but we don't know that for a fact.

But the point that I'm trying to bring here is, yes, SRS had a problem. They didn't use job-specific bioassay much. This impact on the coworker models for subcontractors is not what it appears when you just mention job-specific bioassay because a lot of these workers were monitored.

And that you're going to see through the rest of our presentation with the bioassay that we find.

So the unanswered questions from the December Board meeting was what fraction of the subcontractor construction trades workers were monitored? And again, I come up here to his previous one because we don't know for sure that this was really five percent.

But what we're looking for is how many samples are down here?

Co-Chair Anderson: Where did you get the five percent?

Dr. Taulbee: That was in one of the DOE reports. Okay. So --

Mr. Fitzgerald: And this is true for 1997.

(Simultaneous speaking.)

Dr. Taulbee: This is 1997.

Mr. Fitzgerald: -- snapshot. Right, okay.

Dr. Taulbee: -- 1997, so this ratio here could be different in other years. We don't know. So one of our questions is what fraction of the subcontractor construction trades workers were monitored? So what -- oops -- what fraction of them signing in on RWPs requiring bioassay we've got a sample for.

All right? Oops, there we go.

So that was one of the questions. Were subcontractors primarily monitored via job-specific bioassay? Because that's another question of were these job-specific bioassay dominated, and again, this is where, you know, there's some -- there was a question.

We had no idea whether they were dominated by job-specific bioassay that was incomplete or whether they were actually following the same operations-type of ratio of 95 and five percent.

Did the subcontractor monitoring change over time, by area or craft? Did unmonitored subcontractors work side-by-side with monitored subcontractors?

Did people signing in on the RWP, going back up to this one right here, was this decision done? Worker participates in a routine bioassay sampling program? Yes.

If they're both signing in on the RWP, this person's covered, this person's not under -- going down

through this path.

So were they working side-by-side doing the same work? And were these subcontractors monitored for the correct radionuclides? Okay. There's a -- and SC&A raised a valid point of the bioassay moratorium and one of SRS's responses dealt with americium, and they said that, you know, we had some people that weren't signing in or weren't on the proper bioassay, bioassay schedule.

Now, we did an analysis of that, and when they went back and did a full audit of it it turns out that, yes, there were some that were not on the proper schedule, but it was less than -- I believe it was less than five percent, something along those lines, that were on the incorrect bioassay schedule.

So but this is one of the things we could look at by going to RWP. So here's the subcontractor monitoring evaluation for 1990 to 1998. Our three goals were to determine the percentage of subcontractor construction trades workers monitored by year; determine whether the unmonitored subcontractor construction trades workers were represented by monitored subcontractors in the same radiological environment, the same RWP at the same time; determine whether subcontractor construction trades workers were monitored for the radionuclides of concern given the radiological environment on the RWP. Okay.

So we developed a sampling plan. And what we did was randomly select subcontractor radiation workers from the various areas of Savannah River Site, such that an evaluation of the monitored and unmonitored workers could be conducted.

So the first step was to define the sampling frame, and I've got to tell you this -- I learned a ton in doing this about sampling. And Nancy's over here laughing at me because I was very naive in understanding what this entails and how much work is involved in doing so.

But the goal was to focus on the actinide exposure, okay? So we focused on plutonium, uranium, americium, and neptunium work.

We excluded the reactor areas because of the tritium. It wasn't much of a dose. It wasn't really significant from that standpoint.

Going back to DOE's notice or finding in violation back up here to this chart right here, we don't know what fraction here is actually tritium and which is the actinides.

We do know that the tritium doses are so low that it really doesn't play a role, so those were excluded.

We also excluded the standing radiation work permits, the SRWPs. So the routine work was taken out. Okay.

Now, in a box of records you're going to have regular RWPs and standing radiation work permits intermixed, okay? They're not one or the other. They're all together. Okay, typically a folder here and a folder there.

So we went through in developing our inventory, developing our sampling frame. We had estimated pages based upon our inventory, and this is what you get.

Now, when you take out the reactor areas, and we have a table for the reactor areas here, but taking that out and just focusing on the actinide areas, the plutonium areas are F and H.

M area is primarily uranium. A area has uranium, plutonium, americium, neptunium. E and Z area really there is some plutonium but not much from that standpoint. Okay?

So this was the estimated number of pages before we did our sampling. These are the folders containing RWPs with subcontractor construction trades.

When we did our inventory we specifically would pull folders within the areas within these boxes that we pulled. And we went through them to identify was there any subcontractor construction trades on these RWPs. Okay.

And so here are the folders that we would sample from, draw -- and sample from. And you can see there's 245 total, and this was spread across -- I don't remember off the top of my head how many boxes. I don't know, 60 or 70 boxes, something like that?

Dr. Chalmers: It might have been more than that.

Dr. Taulbee: It might have been more than that. Okay. So pardon me here while I take a sip.

So this was our sampling scheme. If you did it proportionally based upon the 245, it would be two RWP months from A area, two from E area, 42 from F area, 30 from H, one from M, one from Z.

Well, we wanted to try and get across all the sites or across all the areas and as best we could. And so we came up with a semi-proportional scheme, and that's what you see there of the 15 to 10, 30, 30, 10, 10.

And this comes from this particular area here where you look at the pages. We had a lot from F and H area, and this is part of how we excluded samples, okay?

And what I mean by that is we excluded all of the tritium bioassay or tritium work, as well as limited it to actinide-type of work. That's going to be the F and H areas primarily and subcontractors.

So what we ended up with is this one and a half times the semi-proportional to try and get to 158 RWP months that I'm going to call it here. And that means -- and let me try and further explain this. The RWPs would be in a folder. The folder could be a half inch thick or four inches thick of pages, okay?

A single RWP might have been written for one day. Could have been written for a six-month job.

And so what we did from the sampling standpoint is we would -- our statistician, Nancy, came through the folder sampling here, told us which folder to go to. And at that point the HPs went through and identified all of the RWPs that had a subcontractor on them. Okay?

And in this particular example, this would be sample 32 within our report, there were six RWPs that had subcontractor construction trades workers on them in that folder that we grabbed.

We would tell her there were six in this particular one. She used a random number generator and told us to go to whichever RWP. In this case it was RWP number six.

We then went to RWP number six, and we looked at how many months does this cover? Like I said, some of them were one month, some of them were six months, and we would feed that back to here.

She was sitting there right across the table like we are right now. And in this case we'd tell her March and April, and she'd tell us okay, go and do the April RWP.

At that point, we'd go and pull all the subcontractor construction trades workers off of that RWP and write their information down to later go and capture their bioassay. So that's how we came up with this random pull that we did.

So we sampled folders and boxes until a minimum number of 158 months was reached and a minimum number of subcontractor construction trades with 766 were satisfied.

Member Lockey: How did you reach those limits --

Dr. Taulbee: Those limits were part of the sampling plan, and that was the --

Member Lockey: Based on?

Dr. Taulbee: There's here in report 92 there were some confidence intervals --

Dr. Chalmers: There was a whole simulation we ran.

Dr. Taulbee: -- the simulation.

Dr. Chalmers: And we had some, you know, some I guess subject matter experts or estimates of how this whole thing would lay out. And so we did a whole simulation and calculated confidence intervals until we got it as narrow as kind of Tim said he wanted it.

And that -- that gave us the 158 months and 766 subcontractor CTW ---

(Simultaneous speaking.)

Member Lockey: Okay, so a statistical model that --

Dr. Chalmers: Right.

Member Lockey: -- to tell you how far you needed to go to make it --

Dr. Chalmers: Basically a sample size calculation --

Member Lockey: Right, got you.

Dr. Chalmers: Nothing as simple as you find in a textbook. This was --

Member Lockey: Sort of like a power calculation.

Dr. Chalmers: Yeah. Little bit. This was ridiculous.

(Laughter.)

Dr. Chalmers: So we had to simulate, you know, simulate things to get it --

Member Lockey: I just was curious. Okay, thank you.

Dr. Chalmers: Yeah.

Dr. Taulbee: Okay. And that's all there in the report as to how that came about.

In total, we got 662 subcontractor construction trade worker RWP month evaluations. Why did we miss 766? Well, because in the capture we did not pay real close attention to the bioassay monitoring. And what I mean by that is the RWPs you can go through and you can see that there are subcontractors on here. After we captured it if you went through and looked at the tasks, in latter years they would break it out as to whether monitoring was required by task. And so if the first task was for operations to go in and, you know, shut things down along those lines, then that would be like task one. Task two would be building a hut. Task three would be these other things.

Only whenever they went in to actually do some breaches would they be requiring bioassay for certain tasks -- or not bioassay, I'm sorry, wearing a respirator. And that was our trigger.

So we captured some RWPs and some people that didn't wear respirators because they didn't need to, inadvertently while we were doing the sampling.

So this is hard. This was hard to do in live time while you're going through here. And hindsight being 20/20, any time we do this in the future you guarantee we're going to be paying way more close attention to this. What this would impact, though, is really our power or our confidence of what these final numbers mean.

If you go back to the coworker match, the coworker use and dataset to criteria for the evaluation, what we call the implementation guide. This is one of the quotes that Jim had put in there, and it was the minimum number of samples should of course be considered when considering the number of workers potentially exposed to airborne source term.

For example, the number of samples that are necessary to be representative of exposures at a

uranium foundry where airborne activity is generally widespread will be greater than the number required for a small glove box operation where six workers were involved in manipulation of plutonium parts.

The latter situation it may be that samples of three out of the six could be used to bound exposures for three who were not monitored. Okay? And that's my emphasis added there. Okay?

If you consider this RWP work of interest to small activity, in many cases this was -- well, most cases this was by far the activity. There would be a few people signing on this RWP.

The evaluation criteria that we used was the same RWP on the same day at the same time. However, time's not exact, and we went by kind of morning and afternoon because some people might sign in 15 minutes later than the other person.

So it's not an exact science here as far as matching. It takes people time to dress up -- or to dress, get in and out. We did not match on craft. Why?

Well, in our opinion, the exposure environment was what the critical component was. The exposure environment can vary depending upon the RWP work being conducted.

As Joe pointed out earlier, context really matters here. Crafts may have similar or different exposure potentials.

So let's look at one of our matches here. This is example one in our Table 4.7. This would be a RWP for 241H. This would be the tank farms. And the work description was deconning the V2 riser for hut tear down.

So earlier, this particular, in this particular month in 1992 they did some work on the V2 riser. And then once that work was done, this RWP was to go in, decon that area and so that the hut could be torn

down.

So they're going to decon it. HP's going to come in then and see how well they did. Here are four construction trades workers that went in to do this work, a laborer, a carpenter went in at around 8:15, came out at 11:00.

A sheet metal worker and a laborer went in at 8:30 and came out at 11:00. The two laborers and the carpenter were monitored for plutonium. The sheet metal worker was not. All of them were monitored for strontium and fission products. Okay?

We believe that this is similar work and meets the criteria. Matching on craft here wasn't necessary. Okay?

We do not believe it's correct to say that the worker was not monitored because they were not monitored for all the radionuclides on the RWP. Because in this case the worker was not monitored for plutonium, one worker was, the sheet metal worker, but dose reconstruction can be conducted for strontium using his personal bioassay.

The coworker model can be used to estimate his plutonium exposure. Okay? Similar work, same radiological environment, three of them monitored, one not. Okay?

So making a criteria to got to have all the radionuclides we think is too stringent. Let's use that.

So we went through and did an evaluation. We looked at the monitoring percentages for radionuclides of concern, plutonium, strontium, uranium, americium, neptunium --

Co-Chair Anderson: So again, why if they were there at the same time, the same environment, well, you could say it was appropriate, the sheet metal worker would not be monitored for plutonium because he --

Dr. Taulbee: No. This is a case where he should have been monitored for plutonium. Okay? But he didn't. He either didn't leave a sample. This is part of that job bioassay sample not being taken, not being left by the worker or the worker not being told to leave one --

Co-Chair Anderson: Yeah.

Dr. Taulbee: -- by HP. But for whatever reason the sheet metal worker was not monitored for plutonium. These other three people were. We would apply coworker model to this sheet metal worker based upon he was monitored construction trades worker with these other three.

They were doing deconning of the V2 riser --

Co-Chair Anderson: Right, right.

Dr. Taulbee: -- for a hut teardown.

Co-Chair Anderson: I understand but I thought you were using this to validate or invalidate the initial table that you had there on the 21 percent or not. That isn't -- they weren't using coworker at that time.

Dr. Taulbee: No.

Co-Chair Anderson: Okay.

Dr. Taulbee: What I'm --

Co-Chair Anderson: That's okay. I just --

Dr. Taulbee: Okay, yeah. No. We're trying to determine what fraction of these subcontractors were monitored at this point.

Co-Chair Anderson: But --

Mr. Fitzgerald: I think there's some --

Co-Chair Anderson: -- you're defining monitored broadly now to include coworkers being monitored, right?

Mr. Fitzgerald: Yeah, I think there's some conflating is what you're saying because I think the exercise itself is to substantiate that there's a sufficient representation of the subcontractor data --

Dr. Taulbee: Yes.

Co-Chair Anderson: Yeah.

Mr. Fitzgerald: -- in the coworker model. So you wouldn't have the coworker model be applied as part of this process now necessarily. It would come later if you were to do dose reconstruction.

Dr. Taulbee: Yeah, if we were to do dose reconstruction.

Co-Chair Anderson: Okay.

Dr. Taulbee: The initial question was are these subcontractor construction trades workers, is their data appearing in the coworker model or are they being left out of it due to this job-specific bioassay issue of them not being monitored? Okay?

And so in this particular case for plutonium we've got 75 percent of this work crew being monitored, 25 percent not.

Co-Chair Anderson: Yeah, okay.

Mr. Fitzgerald: But your match just is -- that's a good point. You're matching percentages when we get to that point and you're looking at an RWP to determine whether or not a worker would have, you know, to identify a worker that would have been on that RWP.

You're just looking for one of the, I believe, one of what's on the RWP, right?

Dr. Taulbee: That's correct. In this particular case --

Mr. Fitzgerald: That's matching the --

(Simultaneous speaking.)

Dr. Taulbee: We matched the center, too. That was our match. We just looked for one additional worker to be on the RWP that was monitored and we called it a match. That this worker worked in an area with a monitored worker.

So coworker model made up of the monitored worker should be able to be applied to the unmonitored worker. Okay?

Okay, similar work, okay. Thanks for that.

We evaluated by year, by area and craft. We considered what we were going to call the effective monitoring based on matched coworkers for specific radionuclides. And I'll explain that a little more here shortly.

Okay. Six hundred and forty-four, and the example I'm going to walk you through, I'm not going to walk you through all the radionuclides that are here in RPRT-92 because it's, I don't know what, 188 pages long.

I'm going to walk you through the plutonium evaluation that we did. Okay? There were 644 subcontractor construction trades workers required plutonium monitoring from 140 RWPs.

Five hundred and sixty-seven of them were monitored for plutonium, or 88 percent of the subcontractor construction trades workers. So this is the population that we are concerned about whether they were monitored and whether they have -- they're sufficiently represented in the datasets such that the coworker model was valid.

Okay. Five hundred and forty-eight were monitored via plutonium urinalysis, 19 via in vivo chest count.

Mean number of days between the RWP and the bioassay is 159 days. Five hundred and one were within one year, 39 additional within two years. That's 540.

Some terminated workers were monitored upon their return which was greater than two years later, so that's what kind of makes up some of this difference. Okay? They didn't leave a termination sample but they came back a few years later and left a sample.

With plutonium, depending upon the solubility type, effectively if you have a bioassay at the end you can bound their dose, okay?

So here's the evaluation by year. And you can see that we did a pretty good job of sampling, I think, across the years. The number of RWPs is pretty similar and here you can see the number of subcontractor construction trades workers that were monitored, which was starting in 1991 there.

Eighty-two bioassays required across 17 RWPs and there were 78 of them monitored. So 95 percent in that particular case had bioassay.

Taking 1994, it's not as good. You have 140 bioassay required across 32 RWPs and 104 of them were monitored. So 74 percent had bioassay.

Now, in this particular case it is if you look at the subcontractors without monitoring data, that's this column here, okay, that were matched working under an RWP with somebody who was monitored. That's what we were talking about here.

So the unmonitored we went through and looked at other construction trades workers, were the other people working with him monitored? And here you can see that in '91, three subcontractors were matched with the 78 that were monitored, bringing this up then to, what, 81 or 99 percent effectively monitored.

Go down here to 1994 and you've got 74 percent. Twenty or 104 subcontractors were monitored, 20 additional could be matched to somebody who was working on an RWP, bringing this up to 89 percent.

So if you look at the direct monitoring over this whole time period, '91 to '98, you have 88 percent with bioassay, direct bioassay monitoring. Forty-seven were matched to people working on RWPs, bringing it to 95 percent. Okay?

This is why we believe, for plutonium, the subcontractors in this time period are adequately represented in what we will -- for a coworker model.

And we looked at this by craft next, and here you have, again, the 644 bioassay spread across boilermakers, carpenters, electricians, insulators, ironworkers, laborers, millwrights, painters, pipefitters.

And as you see, the other category isn't really that big. This is the dominant trades that were doing the work that would require the bioassay. And this is that breakdown.

We don't see any major under representative group. The lowest group is your painters. They might actually have the lowest potential here. So all right.

We looked at it by area across the A, F, H, E and Z areas. E and Z were lower than F and H, which is to be expected. F and H were well-known plutonium areas and so we've got lots of plutonium bioassay in that area. Okay?

And again, we don't see any major underrepresented area, which was one of our goals was to look at all the areas, not just one or two areas.

So we didn't see any significant difference in plutonium monitoring by year, by craft, by area. We conducted this evaluation for all radionuclides of interest in this time period and that's this particular plot.

We've got those year, craft and area breakdowns in the report. You can go through and read those, but within this is the summation of them all for this time

period of 1990 to 1998.

And what you'll see overall is that about 88 percent of the subcontractors have direct bioassay. We could match an additional, what was it, about eight percent -- no, seven percent to people who were monitored. So from an effective monitoring standpoint this would come out to 95 percent.

Member Beach: Tim, it looks like you're pretty light on 1990.

Dr. Taulbee: Yes.

Member Beach: '91 to '98 is it's pretty clear, but '90 is pretty light.

Dr. Taulbee: And I'll get to that. It'll be at the very end, if that's okay?

Member Beach: Okay. Yeah.

Dr. Taulbee: All right. And then I know Joe and Bob are going to go into it in great more detail and I think it's going to be an area of further discussion absolutely.

Co-Chair Anderson: So is the match with a subcontractor coworker?

Dr. Taulbee: Yes.

Co-Chair Anderson: Okay, that's what I thought.

Dr. Taulbee: Yes, it is. Okay?

So most, I mean, subcontractor construction trades workers are considered to be more at risk for being under-monitored. Okay? That was what prompted this particular analysis.

More of your routine construction trades workers who were onsite would be under the routine bioassay. This is a group that may be more transient or for whatever reason might only be signing in under job-specific and how does that

initial finding from DOE play out here?

And again, I'm -- this is based upon kind of three populations, operations workers, prime construction trades workers, subcontractors. And so the fraction of subcontractor workers needing a coworker model to supplement bioassay is rather small. It's on the order of less than 15 percent.

And that is, if you go back up to this one right here and you'll see the total here is 88 percent. For americium it's more than 15 percent. It's 25 percent.

But what you'll see here with the plutonium is that these are people who have bioassay. These are subcontractors, have the bioassay in their file. They don't need a coworker model at all. Okay?

Or I shouldn't say at all because they could have gaps in the time period where they would need a coworker model. But we have bioassay data for these people. We get that when we do a dose reconstruction. Okay.

So if you take a step back and look at a global view, and I'm calling this the 30,000-foot level, of at least one bioassay, okay, of the 662 subcontractor construction trades workers in this time period, 633 had one or more of the required bioassay. Okay?

Rated by the area strata and monitoring percentage, a point estimate is 95 percent, okay, have bioassay.

The bottom line is that in this time period most subcontractors were monitored for internal exposures and have bioassay data. So the coworker model developed from this we feel would be representative in this time period. Okay.

The 29 subcontractors, 4.4 percent that did not have any monitoring data, 19 of them were directly represented by other coworkers. Okay?

Only 10 subcontractor construction trades workers were not represented by a coworker, so there are some. And so DOE going in and fining them for violating procedures, people not doing it, sure. You've got some that were not monitored, okay?

But this is a small percentage of this total that we looked at. We feel the subcontractor data is sufficient.

By the way, of those 10, five either waived the bioassay required and we have documentation of that where they declined to leave a termination sample. And there's, Health Physics has no authority, no police authority to compel somebody so from that standpoint.

So it's really five of 662 were not represented from a coworker model that we can find. Okay.

Whoops. So this is that particular summary. So considering the majority of the subpopulation, the subcontractor construction trades workers, were monitored for each of the radionuclides and they would normally use the full uncertainty distribution of the coworker model for the unmonitored workers.

We feel that, A, the coworker model can be constructed and subcontractors' doses can be estimated.

We also still have that option of the 95th percentile if we feel that somebody was going into areas or there's documentation. They provide it in their CATI and we get it from other ways, their external dose.

There's other pieces that can be evaluated. We can assign the 95th percentile and we consider that to be bounding.

So our conclusion here for the '90 to '98 time period, and that's critical, it's just this time period here that we've been focusing on is we continue to believe the coworker models developed from the workers with monitoring data are sufficient to

estimate the dose to the few workers without monitoring data and supplement the monitoring data for those with incomplete internal monitoring.

Mr. Fitzgerald: Tim, just to go back to the date, I notice you have '90 there.

Dr. Taulbee: Yes.

Mr. Fitzgerald: But the area of your analysis was '91 through '98? Did that switch? Was that purpose -- do you feel you can apply the coworker to '90 as well?

Dr. Taulbee: Yes, I do.

Mr. Fitzgerald: Okay.

Dr. Taulbee: And I will show why I believe that.

Mr. Fitzgerald: Okay.

Dr. Taulbee: Okay?

Co-Chair Anderson: Since this really is results from a sample, did you do the confidence intervals?

Dr. Taulbee: On the individual ones? No, we did not.

Co-Chair Anderson: On the percentages?

Dr. Taulbee: No, we did not.

Co-Chair Anderson: Okay.

Dr. Chalmers: Just the final one.

Dr. Taulbee: Just the final.

Dr. Chalmers: Only the final one.

Co-Chair Anderson: I don't --

Dr. Chalmers: The total percentage. You don't -- that works fine.

Dr. Taulbee: This one.

Dr. Chalmers: Right there.

Dr. Taulbee: Of whether they had any bioassay, not looking at -- because --

Dr. Chalmers: Okay.

Dr. Taulbee: -- in this particular one you could have a worker that was required to have plutonium, strontium and americium, okay? And so what we did here was we said did they have any one of those to be monitored? That's what this number here is.

Co-Chair Anderson: You didn't do it by individual.

Dr. Taulbee: No. No. I'm not sure you can --

Co-Chair Anderson: You didn't collect it.

Dr. Taulbee: I'm not sure we can develop the confidence interval from that. I didn't know.

Co-Chair Anderson: Well, you have, I mean, your sampling design --

Dr. Chalmers: Well, the sampling trials do not --

Dr. Taulbee: For that.

Dr. Chalmers: -- specifically for this --

(Simultaneous speaking.)

Co-Chair Anderson: For any, for any test.

Dr. Chalmers: Yeah.

Co-Chair Anderson: Or for any one of the --

Dr. Taulbee: For any of the bioassays.

Co-Chair Anderson: For any one but not for plutonium.

Dr. Taulbee: That's correct.

Co-Chair Anderson: Okay. The highest proportion was with the plutonium testing.

Dr. Taulbee: It was, yes, but if you go back to the very beginning --

Co-Chair Anderson: Yeah.

Dr. Taulbee: -- of this particular task of the question of --

Co-Chair Anderson: Generally it would have been --

Dr. Taulbee: Yeah, yeah, determine at what percentage of subcontractor construction trades workers were monitored in total. Okay. And this is not --

Co-Chair Anderson: For any of them.

Dr. Taulbee: For any of them and so --

Co-Chair Anderson: But it would all have been probably urinalysis.

Dr. Taulbee: Or in vivo.

Co-Chair Anderson: Yeah.

Dr. Taulbee: Whole body count.

Co-Chair Anderson: Yeah.

Dr. Taulbee: Chest count, sorry. I'm sorry, Liz. I learned that internal dosimetrists consider them differently.

Co-Chair Anderson: Yeah.

Dr. Taulbee: Whole body count chest.

Dr. Chalmers: But they are different.

(Simultaneous speaking.)

Co-Chair Anderson: You can't call it neptunium --

Dr. Taulbee: Yeah, okay. So that's 1990 to 1998 or 1991 to 1998, whichever way you want to consider it at this point.

Okay. Now, let's look at what I'm going to call the late DuPont era of 1980 to 1989. Here we only had job plans available. Okay? The RWP system hadn't been implemented yet so we had job plans and special work permits for A area. That was the only area that we had.

The job plans were the primary source of information. The job plans for other areas we looked for and we looked hard for them.

We contacted the site. We worked with the records folks to try and locate them. These job plans is actually what we believe might have been destroyed based upon the interviews with workers.

Not their bioassay data, not their dosimetry data, but these job plans, okay? For some reason they may not have been considered as radiological records or whatever in the 1989 and '90 time period as DuPont was leaving. Okay. So we don't have them.

We do know that a lot of good, or a majority of the work switched to what they call standard operating lists, is that the correct term, Mike, the DPSOLs? I believe it's standard operating lists where they would have these tests --

Mr. Mahathy: Yes. Yes.

Dr. Taulbee: Okay, thank you. Where they would have the same tasks listed as the RWPs in the breakdown, but instead of sign in and sign out, it was initial off. And the initial we couldn't track back to a worker, so there wasn't a Social Security number like there were on later RWPs or a payroll ID or something along those lines.

They still had some work control, which is what I'm getting at, here from that standpoint. It was different. It wasn't as easy to -- well, and we couldn't tag it back to individual workers other than these particular job plans in A area where they still required them to write down their broker payroll ID

number.

So we could look at A area. The safe work permits is SWP, by the way.

Co-Chair Anderson: Yeah.

Dr. Taulbee: Safe work permits were being phased out after 1972.

Because we realized pretty early on in our sampling frame development that we didn't have these other records, instead of doing a random sampling we did a census, meaning we captured all of them that we could find that had subcontractor construction trades workers. So every single one of them that we could find. All right?

And so here's the breakdown from 1980 to 1989 and you'll see that we don't have anything in 1989. So as Josie pointed out, 1990, 1989, we've got a couple of years here where we don't have any data that we can evaluate in this manner.

Okay. We have a number of job plan pages here, and then here we did a breakout here of DuPont construction trades workers and subcontractor construction trades workers. Okay.

So 610 job plans, 200 of them were DuPont construction trades workers, 11 were subcontractor construction trades workers. Okay, so we have 211 total or 34 percent are actually construction work. The other job plans were operations.

Okay, so these construction jobs were mixed in with all of the other job plans that operations did, where operations would do something unusual going into an area and cleaning it up or something that was non, you know, standard type of work. They documented that the same way in job plans. Okay.

Overall, we have about three percent of the construction work being subcontractors.

Mr. Fitzgerald: Or 773-A, I mean, the A area only.

Dr. Taulbee: Yes.

Mr. Fitzgerald: Okay.

Dr. Taulbee: For 773-A. In total, we came up with 591 subcontractor construction trades worker monitoring evaluations with 219 unique subcontractors on 145 job plans. So if you compare that to our sample down there at the bottom, we had 429 unique subcontractors on 146 RWPs.

And as Joe pointed out, this is just A area, okay? That '80 to '89 is just A area. All right.

We went through the same evaluation method as the '90 to '98, break by craft or by new client, by year, by craft. We didn't do by area because we only have the one area. Okay. We didn't have any data in 1989.

And so here is the breakdown that you get. And in this case you'll see that the percent with bioassay is less, okay? It's only 80 percent.

1980, it's only 50 percent, but look at the numbers. They only had six job plans; three of them were monitored. Okay? Or six, from the job plans only six bioassays were required. Three of them were monitored, three were not, so this is a pretty small sample size, especially compared to '81 through '87. Okay?

So that's one thing to take into account, that 50 percent. That's got a high confidence or --

Member Beach: Tim?

Dr. Taulbee: -- a large consortium.

Member Beach: Tim, how many facilities does this cover?

Dr. Taulbee: This covers one.

Member Beach: Just one.

Dr. Taulbee: Just one a year, okay? We went through the crafts, same thing. And again, by the way, the lowest percentage here would be the electricians at 61 percent. Okay?

But again, when matched to coworkers it actually goes up to 98 percent. So they were -- less of the subcontractors were being monitored than before, sure, but we actually have more that we can match to those subcontractors.

Okay. So here's these totals for plutonium, strontium and americium, all right? And you can see that the total is lower. Americium is quite low, 34 percent, okay. It has bioassay. Okay?

But a large number of them were matched directly to those subcontractors. Okay? Sixty three of them are matched to 52 people with bioassay.

So the results of plutonium and strontium are slightly a little over, about 10 percent in this time period compared to the '90 to '98 time period. Americium is significantly lower in this time period.

So we only had the job plans, Josie, to answer your question there, for A area, but we had incident report data for F and H area. And so we went and looked at that. And by the way, that says '80 to '89 and it actually should be, I believe, '85 to '89. I think I corrected another slide later on in here.

So we did have, and this was limited data, but we do have indications of reasonable monitoring in these other areas during this time period.

The combined evaluations of no significant difference by year, craft or area, less than 10 percent -- less monitoring, about 10 percent than the modern era, but significantly lower for americium.

This is the incident monitoring data from '85 to '89. So if you go back up here to this plot -- no, wait a minute -- this one here we have no data for A area

in '89. But in F and H area we have actually do have some 1989 data. It's small, but following an incident did they do follow up?

So this is if something happened in the workplace did these other areas where we have incident data, did they do follow up?

Are those workers involved in incidents part of this coworker distribution, part of that population that we would do the TWOPOS values on, do the further analysis of going through the fitting and the intake modeling. Are they representative?

And again, this is just the subcontractors. So these are subcontractors that are involved here. All right?

You know, to the left we've got plutonium, to the right it's the strontium, which we have even less data from from those areas. But I mean, a lot of these incidents were on the plutonium line so you're not going to have much strontium monitoring at that time period. So that's why those numbers are lower, okay?

So we have significantly lower percentage of subcontractors being monitored for americium, 34 percent here. Okay? What we're interested in in Work Group feedback on whether direct monitoring of a third of the population with documentation that 76 percent is effectively monitored is sufficient for a coworker model?

Keep in mind that this is subcontractors only. Okay? So we've got a third directly monitored, third working with the directly monitored, roughly, I'm rounding here greatly, and a third that is unmonitored with no coworker, of subcontractors.

Considering that less than 15 percent of the construction trades work was conducted by subcontractors this comes back up to this point, this graph here of subcontractor pages, roughly, versus DuPont construction trades work.

In the coworker model we've combined them both. Okay? So to here we've got less than 15 percent, an estimate, okay? This is an estimate in this time period. Less than 15 percent of the construction trades work was conducted by subcontractors.

The majority of the subcontractors were directly monitored for plutonium and strontium and a third of this subpopulation was monitored for americium. Again, the majority, 76 percent of the subcontractors are effectively monitored.

Does the coworker model that we developed that's a combination of DuPont Construction and subcontractors, is that sufficiently representative when you consider how we apply it, of assigning the full distribution or the option of assigning the 95th percentile?

So we continue to believe that the coworker models developed from workers with plutonium and strontium are sufficient based upon what we found here, to estimate the dose to the few workers without monitoring data, supplement the monitoring data through those with incomplete internal monitoring.

And the americium is going to require, I think, some further discussion here. Okay? All right.

Well, let's get to the '72 to '79 time period. Now, I call it the mid-DuPont. I don't know what else to call it. It's post-SEC time period for our evaluation but it's kind of in the middle of DuPont era.

Again, we only had the job plans available for A area. That was the primary source. Instead of sampling we did a census. We looked at all of them.

There was no data at all found from '75 to '79. Could not find any job plans with subcontractor construction trades on them. There are a few job plans but none indicated subcontracted work.

So our evaluation's really limited to those first three

years '72 to '74. Here's the actual job plan pages and you can see, I mean, 1,100, 1,000, 146. You can see it drops off very quick, and we just do not have any of this data in this time period in order to do an evaluation.

Interestingly, of the data we do have, again, we see this same ratio of about three percent subcontractor work from a construction standpoint. So they're making up about around 15 percent of the total construction work. Okay?

We went through and did the same evaluation as before and instead of 219 subcontractor construction trades on 145 job plans, we got 31 on 59. Data is extremely limited.

And again, this is A area only and no data from '75 to '79.

Co-Chair Anderson: What was the total workforce again?

Dr. Taulbee: In that time period I don't know, of construction trades workers, oh, that's going to be -- Liz, could you look that up? That's under the americium section here. Yeah.

I want to say construction trades workers --

Co-Chair Anderson: How did they compare to the --

Dr. Taulbee: -- was around about 500 or so. But I don't know about, I don't know that that total encompasses subcontractor construction trades because I'm not sure how they were counted ever onsite. But we really don't know if they were ever considered in their force plans.

There should be a table in that section of subcontractors. Maybe it's -- okay. Liz is going to try and see if we can find it.

Co-Chair Anderson: Just curious on at Savannah River, I don't know the total workforce over the years. Was there a big increase in it over the years

or was this pretty stable from '72 through --

Dr. Taulbee: No, it was not stable. It ramped up tremendously in the 1980s and into the early '90s and then began to ramp down around '93, '94 time period. So there's a big ramp up in the late '80s into the '90s.

Okay. So if you look at these results from the years you'll see that the plutonium, most of the results we have in 1972 and we have 65 bioassay required. Only 50 of them were monitored and you're 77 percent. You can match 11 subcontractors to them.

In '73, 64 required, only 18 were monitored. Thirteen we can match to it for 46 percent. Only 28 percent, though, were directly monitored.

1974 we only had seven, but again, the data is extremely limited here. Only one was monitored, 14 percent. One was matched, 29 percent. This 51 percent has a huge error bar across it, okay? So that just -- keep that in mind here.

We didn't see any significant difference there amongst craft. Maybe with pipefitters but, you know, that, if you look at the number of bioassay that was much larger than any of the other groups. Most of these job plans were, I guess, in a sense geared toward pipefitters there. They make up a larger portion of the population here.

And so here's the total of plutonium, strontium and americium. There's only one bioassay required that we could find that needed americium and that person was not monitored. Okay. So there's only one data point under the americium.

The evaluation was limited and dominated by the 1972 data. So this time period of '72 to '79 is really 1972 for all intents and purposes, but that's the starting point there.

So less than majority of the subcontractor construction trades were monitored for plutonium in

'73 and '74.

Strontium monitoring was better, though we only have one data point for americium.

Again, we're interested in Work Group discussion on the sufficiency of the monitoring data of the subpopulation for coworker model. We have less than 15 percent of the subcontractor construction trades work was conducted by subcontractor construction trades. Okay?

Now, we do have coworker models that we've developed for construction trades workers. Clearly, based upon what we've seen here from this sampling, is that is being dominated by the DuPont construction trades workers here.

Evaluation indicates some workers were monitored for plutonium. The majority were monitored for strontium. Again, that's primarily on 1972 though.

We normally use the full uncertainty distribution of the coworker model, co-exposure model for unmonitored workers wherein have the option of the 95th percentile.

So here's our summary and conclusions here. And I understand Jerry's talking about the context here so I'll go quickly on this.

The initial question was the 79 percent incompleteness. Okay. And that's where I'm getting at here and they had concluded that the bioassay datasets for construction trades workers specifically and construction trades workers generally is demonstrably incomplete for 1989 to 1998. And it didn't satisfy the criteria set forth in the draft criteria evaluation use of coworker datasets.

Okay. We, again, respectfully disagree. The 1990 to 1998 data indicates that in fact, most, 38 percent of the subcontractor construction trades workers were directly monitored for the radionuclides of concern by either routine or job-specific bioassay.

A census review of all the job plans in A area supplemented by the incident reports from 1980 to '89 also indicate that far more than 21 percent, most were monitored for plutonium and strontium. Americium is 34 percent and may require some additional discussion here.

The job plan and RWP data are insufficient to really evaluate whether subcontractor construction trades workers were sufficiently monitored or represented by coworkers from '72 to '79.

There's limited subcontractor construction trades work, less than 15 percent during this time interval. Some subcontractors were clearly monitored, we have their data, therefore they're part of the coworker distribution.

But the uncertainty as far as how much, what fraction are they represented in the coworker model, 15 percent, there just isn't data to try and verify that. So we're interested in the Work Group discussion on the sufficiency of the monitoring data for the subpopulation for coworker model inclusion.

Mr. Fitzgerald: Tim, just to clarify, this sort of goes back to one of the original questions. I know this bounced back and forth between Work Group and NIOSH, which is how complete is complete? I mean, I think from what you're demonstrating here it's somewhere between 79 and 88 percent were the two percentages you thought were most, the most monitored, directly monitored?

Dr. Taulbee: Most means 50 percent to me.

(Laughter.)

Mr. Fitzgerald: Well, it's just that we have kind of not returned to that question but that sounds like what you're broaching to the Work Group is, is that the right threshold for completeness or not?

And it's not an objective or quantitative question. It's a, I guess, again, a judgement as to how

complete is complete.

Dr. Taulbee: And that's where we're looking for --

Mr. Fitzgerald: And you're saying 50 --

(Simultaneous speaking.)

Mr. Fitzgerald: -- more than 50 percent --

Dr. Taulbee: I feel that that is sufficient --

(Simultaneous speaking.)

Mr. Fitzgerald: That number has shifted over time. You're now saying over 50 percent would be, you know, complete enough, I guess is what you're saying?

Dr. Taulbee: I'm -- yes, with the caveat that it could be less than that.

(Laughter.)

Dr. Taulbee: No, no, seriously.

Mr. Fitzgerald: Yeah, I know.

Dr. Taulbee: I mean, I go back here to the americium here because you're looking at, let me go back up here to this one, this -- and here's the reason that I'm saying that. I'm not trying to be difficult here.

But you're looking at, for the americium 34 percent of the subcontractors, okay? And another third of them we know worked directly with those folks. So those unmonitored people we would be applying the coworker model to, they're -- that direct connection is part of that coworker model.

One-third of these subcontractors are part of the coworker model now, is what I'm getting at. They are represented in there.

Mr. Fitzgerald: Okay, all right.

Dr. Taulbee: And so it's not -- we don't, we're not parsing out subcontractors, subcontractor construction trades and DuPont construction trades. We have a combined model and we know that subcontractors in this time period conducted less than 15 percent of the work.

So a third of them are already in the coworker model. They make up less of the construction work that was being conducted at the site, 15 percent of it. Okay?

So if I was looking at all construction trades workers and it was less than 50 percent then yes.

Mr. Fitzgerald: But the 50 percent --

Dr. Taulbee: Taking out this vulnerable population I'm not sure that that 34 percent isn't sufficient.

Mr. Fitzgerald: Right, but the --

Dr. Taulbee: And that's what I'm asking for the Work Group to --

Mr. Fitzgerald: Right. The caveat though is the context that the 15 percent is one facility. We're talking still 773-A, right?

Dr. Taulbee: Yes.

Mr. Fitzgerald: Okay. I'm just trying to make sure I understand the time.

Dr. Taulbee: But is there any evidence that this is different than that for the other areas?

Mr. Fitzgerald: No. I'd turn it around. You have evidence for the other 34 major facilities. That's kind of what we're going to get into. I don't want to get into it here, but --

Dr. Taulbee: Okay. Let me --

Mr. Fitzgerald: -- just to clarify, that's kind of what we're talking about, 773, isn't it?

Dr. Taulbee: So let me get to the final here. All right. So all right. It's limited, okay. This will be -- okay. Our review demonstrated that unmonitored -- I believe that our review demonstrated that unmonitored workers worked alongside the monitored workers in the same radiological environment from the 1980 to 1998 time period.

The bioassay data is present within the individual monitoring records and can be used for dose reconstruction. These internal monitoring records can also be used to develop the coworker models and subsequently used in dose reconstruction to supplement the gaps in individual monitoring data for these subcontractors. Okay.

Again, coworker model use, we developed these co-exposure models because we recognize some workers were not monitored. For a coworker model to be valid it has to be a representative sample. That's all that we need to have, okay, is a representative sample.

Co-Chair Anderson: Of what though? Of who?

Dr. Taulbee: Of the exposed population.

Co-Chair Anderson: Not just of the construction trades workers?

Dr. Taulbee: Well, if you're going to stratify then it's got to be --

(Simultaneous speaking.)

Co-Chair Anderson: No, but I mean, the --

Dr. Taulbee: It's got to be both.

Co-Chair Anderson: The point is this morning you really showed the whole population.

Dr. Taulbee: I --

Co-Chair Anderson: And now you're showing the --

Dr. Taulbee: A sub, part of that.

Co-Chair Anderson: Yeah.

Dr. Taulbee: But this morning I just showed the non-construction trades workers. This is a subpopulation of the construction trades workers. That's what I'm showing.

Again, if all the exposed workers were monitored, I mean, if we had 100 percent of the subcontractors monitored for americium we don't need a coworker model. So that's something to keep in mind.

Actually I'm going to skip this and let SC&A go through this because I think this even shows that we -- I mean, their conclusion is very similar to ours.

So this is kind of our, what we're seeing in the evaluation status in a sense, a summary and where we need further discussion. '72 to '79, to be determined. There just isn't data there. We'll have to -- what do we want to do here from that standpoint?

'80 to '89 we believe plutonium and strontium is fine. Americium we need to have some more discussion on.

Now, here I want to go through the evaluation of time and can we find a more efficient way. And maybe this is better for the end of the discussion, but this gets to some of what Josie was bringing up earlier and Joe was bringing up with regards to we only have A area data and so it's not across the site.

But this is the evaluation timeline, okay? So when we started in February of 2018, kind of all of the major steps, when we did the data capture, and it took us 16 months to get this report out, all right.

I thought it was going to take us eight to nine months tops. It took double that to get it out.

SC&A has been reviewing it for another four months

for us to get out their comments, okay. So we're at 21 months right now of getting this out to have this discussion. It's very labor-intensive. This is a difficult analysis even for a site with very good monitoring records.

This is currently being attempted to be repeated, RWP analysis from Los Alamos, and I believe, Josie, you had an email over there this week from Bomber talking about delays already from getting data out of Los Alamos.

Member Beach: Got a solution for that.

Dr. Taulbee: SC&A had raised with Bomber, LaVon Rutherford, the issue of fairness indicating we did this work for Savannah River. We should do the work for Los Alamos. If we attempt to do this type of evaluation for every site these evaluations won't be completed for over a decade, all right.

Can we conduct something simpler and obtain similar results or insights? Will it be this detailed? Are we going to get down to where we can do that analysis, you know, and look at individual workers on RWPs?

Again, this is very hard. What we had ORAU look at was the subcontractor monitoring using only NOCTS data, okay. And we evaluated those subcontractors who were externally monitored, so subcontractor construction trades workers who were externally monitored, and evaluated whether they had any internal monitoring results. Yes or no, simple.

We didn't look at tritium, or actually we did but I'm not reporting that here. We just looked at the non-tritium bioassay, the actinides and whole body counts for fission products.

Here's the results. This is our report RPRT-92 and RPRT-94, okay. Remember that time period of 1970 to 1980 where we were talking about having difficulty getting records? We're not seeing them in the claimant files either, okay.

This is monitoring data within the individual files. The blue bars are RPRT-92. And in these earlier years it's the plutonium bioassay here.

We get to the 1980s and there was an increase in subcontractor monitoring that is quite clear when you get into that time period.

The reason that I kept using 1990 to 1998 and 1980 to 1989 is you look in that time period on this plot here, we don't have any RWP data, we've got a lot of claimant bioassay that's being done in that time period. We have a lot of monitoring that's being conducted.

We might not yet be able to prove or demonstrate the job plans or RWPs, because they don't exist, they might have been destroyed for whatever reason, but we still have a lot of individual bioassay monitoring data in that time period.

So to just discard that -- I don't think it's right to just discard this information from NOCTS that's showing we have a monitored population in here.

Now, can this particular NOCTS data evaluation be done in more detail and instead of any bioassay, you look at just americium, look at just plutonium, look at -- it can be, but I'm trying to figure out a way where we can try to do some analysis without going back to the sites and digging through all of the RWPs trying to find them, develop a reasonable sampling, something that's faster that gets you all the answers so that we can move forward with other -- some of these sites.

Member Beach: Tim, excuse me, but I thought you guys already looked for all the RWPs? Didn't you pull those?

Dr. Taulbee: What do you mean?

Member Beach: The RWPs? You said to go back to the site and try to find more RWPs.

Member Ziemer: The other sites.

(Simultaneous speaking.)

Dr. Taulbee: Other sites.

(Simultaneous speaking.)

Member Beach: Other sites? Okay.

Dr. Taulbee: Yeah, other sites.

Member Beach: I thought we were speaking -- okay.

Dr. Taulbee: Yeah.

Member Beach: Well, I'm --

(Simultaneous speaking.)

Dr. Taulbee: -- potential benefits of doing this, it's a simpler analysis. Like I say, when you look at individual radionuclides it's more resource efficient.

There's less data capture and coding, more timely analysis. There'd be less classification when you have the data in-house.

There is a potential detriment. We can't directly compare coworkers, therefore the data completeness really must be inferred, okay. We can look at subcontractors, but we can't look at this subcontractor, were they paired with another subcontractor on the RWP. We can't do that.

But the other components of this we can with the NOCTS data. It's something to think about. And with that, I'll turn it over to SC&A for you all's presentation.

Mr. Fitzgerald: Just on that last point, I think it's an interesting point, but would that not be consistent? We're talking guidelines most of the day and just, you know, looking at the question of establishing completeness, data completeness, this would not -- this would not -- it doesn't look like it would satisfy

this.

I mean, it would definitely be expedient. I don't disagree with that, but as far as, you know, weighing the completeness of the data as a prerequisite it doesn't seem to touch that base as far as the guideline. I just want to get your reaction. Do you think that's the case?

I mean, this does look more expedient but it doesn't seem like it addresses this.

Dr. Taulbee: I honestly think it kind of would address that. I'm not, I'm trying to think of how this could be, and me thinking live time is dangerous, but it would be more in looking at the percentages.

If you saw that there was no monitoring data for a particular group, I think that speaks to the completeness of what we need to be doing. Keep in mind, a lot of these coworker models we are developing based upon the NOCTS data that we currently have.

And so -- I shouldn't be thinking and talking at the same time. But what I'm trying to get at here is that we don't see any major temporal gaps. I mean, here we do. I mean, clearly there's something going on in the late '70s here with subcontractors that they were not monitored.

I mean, we can see that this is a low percentage here in, what, 1977? Less than 10 percent of our NOCTS claimants have bioassay on the --

Co-Chair Anderson: Well, what's the total number I guess is the question here?

Dr. Taulbee: That we would have to look at.

Co-Chair Anderson: So if it's 10 percent of 100 --

Dr. Taulbee: Yeah, so maybe that's a way of speaking to the completeness issue of, you know, I don't know the answer.

Mr. Fitzgerald: Well, yeah, like I said --

Dr. Taulbee: But going through it, I mean, you're looking at over a decade. I mean it's this is --

Mr. Fitzgerald: I appreciate the dilemma, you know, and the question of establishing the completeness of the data so you have a representative database, you know, if NOCTS is representative then the first precursor is to make sure the data is sufficiently complete.

And if you can't do that then you don't necessarily know -- I mean, intuitively, and actually we did look at the NOCTS database, Ron Buchanan did, from a standpoint of establishing the scope of facilities covered and years.

And I think we were, you know, favorably disposed that it seems like it touches the facilities or whatnot. But as far as the completeness question, if we, you know, go back to the guidelines it would almost seem like your --

Dr. Taulbee: Some of the guidelines were written from the standpoint of when we get, like, an electronic dataset from the site is it complete? We've run into issues at Oak Ridge, for example, where we got the dataset and we learned that 25 percent of the results weren't in there.

And so we went back to the site and started working with that. So, but from this standpoint using just NOCTS I don't believe there's ever been any -- I mean, completeness hasn't been evaluated from that standpoint, but I'm not -- I can't see how it wouldn't show up if you were to look at the individual radionuclides.

Co-Chair Anderson: It's only it's all of the people that have cancer, right? I mean, so --

Dr. Taulbee: Yeah, right, it's --

Co-Chair Anderson: -- if there's an association

between exposure and cancer this is going to -- I mean, I'm not surprised you have the monitoring because that's what the whole program's about is the cancers --

Dr. Taulbee: Right.

Co-Chair Anderson: -- or some of the cancers.

Dr. Taulbee: Right. I guess the leap of faith -- or not leap of faith but the analogy would be these are the claimants. These are the people with cancer and so they are the ones who might have been harmed from that standpoint, therefore did they have any exposure and do they have monitoring data? Is that from a completeness standpoint? I don't know.

Co-Chair Anderson: But again, it's everybody, you know, who had a cancer and filed, so I mean, I'm just getting at the representative of the population.

Dr. Taulbee: But it's really a random sample, NOCTS is really, to a large degree.

Co-Chair Anderson: Well, that's not going to a tumor registry and looking at the age distribution and say, oh, this is, you know, representative of the population. It's --

Dr. Taulbee: Well, no.

Co-Chair Anderson: Well, but it did, I mean, people with cancer are not the same as people without cancer.

Member Lockey: I think you'd have to look at a specific cancer to come to your conclusion.

Co-Chair Anderson: Well, but by and large this is going to be radiation where there's a number of cancers you could file for. I mean, their -- they've all had cancer.

Member Lockey: Yes, but not all of them are -- not all of the cancers that are compensated for are radiosensitive cancers. So you have to look at --

Co-Chair Anderson: Right, but it's --

Member Lockey: You have to look at cancers that, as well as documentation there's a dose response relationship with radiation. Whether it be thyroid --

Co-Chair Anderson: I've never seen any literature say that --

Member Lockey: Something like that, yes.

Co-Chair Anderson: -- you know, that people with cancer are representative, you would use that as a sample representative of the population.

Member Lockey: Well, that's correct, yeah, that's correct.

Co-Chair Anderson: And that's what I'm saying, that you have in the criteria that's supposed to be representative of the workforce, not a sub-segment of the workforce.

Member Lockey: Yeah, that's right. That's right. I see what you're saying.

Co-Chair Anderson: That's what I'm saying.

Member Lockey: I see. I understand.

Dr. Taulbee: Okay. This was the food for thought here, you know, from that standpoint. And of course, yes, I just looked at the time, wow. Right.

(Laughter.)

Member Lockey: Well, we had a good time.

(Laughter.)

Dr. Taulbee: So I don't know if you --

Co-Chair Anderson: We've got 10 minutes.

Dr. Taulbee: I don't know if you want to get SC&A started and we pick up tomorrow or what do you, what do you want us to do then?

Mr. Katz: Well, that's the question is taking everybody's temperature in terms of endurance and otherwise having to be, for people who are local, whether you have to scat for other reasons.

Co-Chair Anderson: It's already too late for rush hour.

Mr. Katz: So both of those questions, endurance and whether there are people that need to be gone?

First of all, for you folks that are local are there any of you that -- is there a certain time when you need to be gone?

Dr. Taulbee: I would like to be gone by 5:00 to pick up my wife.

Mr. Katz: Okay, so that's what I'm asking.

Dr. Taulbee: Okay.

Mr. Katz: So do we have --

Mr. Fitzgerald: Are we starting at 8:30 tomorrow?

Mr. Katz: Yes.

Mr. Fitzgerald: Okay. I think that would be enough time.

Mr. Katz: Do we, yeah, do you have a better sense of that? Do you have what's left on --

Mr. Fitzgerald: Well, I would imagine just for our presentation --

Dr. Taulbee: Yeah, exactly.

Member Ziemer: -- which I think we can handle in the morning.

Mr. Katz: Okay, then it seems like this is probably a good breaking point, right?

Dr. Taulbee: They could do their presentation and then talk about what it is that we're going to

present next week.

Mr. Katz: Yeah, and folks are going to need to think about your big question with respect to putting to bed or not putting to bed the draft guidelines then, right, because you're sort of raising the question, does that have a bearing on --

Dr. Taulbee: No.

Mr. Katz: Oh, it doesn't?

Dr. Taulbee: No, nothing to do with the draft guidelines.

Mr. Katz: Okay.

Dr. Taulbee: I think that's settled.

Mr. Katz: Okay. Well, we need to actually do that in the meeting at some point, but --

Dr. Taulbee: Well, we could do it now if you want?

Member Lockey: Let's do that now, Henry.

Co-Chair Anderson: I thought we did.

(Laughter.)

Mr. Katz: We didn't because you had that other question.

Member Lockey: Are you willing to put to bed the complete guidelines and accept them as written?

Co-Chair Anderson: Yes.

Mr. Katz: Okay. We haven't had a motion on that.

Co-Chair Anderson: I can't move that because I'm --

Mr. Katz: I know you can't. You can't.

Co-Chair Clawson: I move that we accept --

Mr. Katz: You can't move that either.

Co-Chair Clawson: Oh, shoot.

(Simultaneous speaking.)

Co-Chair Clawson: I've got to have something to be able to say here and I've been sitting here pouting away at it.

(Simultaneous speaking.)

Mr. Katz: -- but this is just the question of the coworker guidelines.

Co-Chair Clawson: Get coworker done because I've got something I want to --

Mr. Katz: Yeah, of course, of course. No, it's just like, it's like, so we don't want to switch horses in the middle of the madness here.

Co-Chair Clawson: We've been doing it all day.

(Laughter.)

Mr. Katz: Okay, actually, so we have Paul and Andy and --

Co-Chair Anderson: And Josie.

Mr. Katz: -- Josie.

Co-Chair Anderson: Josie.

Member Beach: And Gen.

Mr. Katz: And Gen is on your group, right?

Member Beach: Yes.

Member Ziemer: Do you recall whether the Board actually took action on what we're calling the draft?

Mr. Katz: Yeah, I do. The Board tentatively accepted the draft with the proviso that this would be done, this work would be done and considered, the draft would be considered in that light before the Board would close on the question of whether that's

complete, the draft guidelines.

And they would --

Member Ziemer: No, they already knew that. They already knew Melius' comments at that point, I think, right?

Mr. Katz: No, no, that was all in that context.

Member Ziemer: Yeah.

Mr. Katz: Melius said this is tentatively approved. The whole Board said it was already -- it was tentatively approved but it has to be run through this exercise --

Co-Chair Anderson: To be sure nothing is missed.

Mr. Katz: And then we'll bring it back to the Board and conclude.

Member Ziemer: Okay.

Mr. Katz: So that's what's left to be done is to bring this up to the Board as we think these guidelines are good enough as they are at this point --

Member Ziemer: Okay.

Mr. Katz: -- and the Board puts its stamp on it.

Co-Chair Anderson: We're not going to send it back to be revised.

Mr. Katz: Or there's nothing to add or subtract.

(Simultaneous speaking.)

Member Ziemer: All right. I move we accept the guidelines as they stand.

Mr. Katz: Okay. And then, Jeff --

(Simultaneous speaking.)

Member Ziemer: -- discussion point --

Mr. Katz: Oh, sure.

Member Ziemer: -- on the motion.

(Simultaneous speaking.)

Member Ziemer: No I just wanted to point out that --

Member Beach: I'll second it. I'll second it.

Member Ziemer: The only difference is they're not going to show up as draft.

Mr. Katz: Right.

Member Ziemer: They're going to show up as the guidelines.

Mr. Katz: Correct.

Member Ziemer: And if we find that they're inadequate at some point they can be changed in any event.

Mr. Katz: Or course.

Member Ziemer: So whether we would have Rev 1 or Rev 2 or whatever it is, so it's not --

(Simultaneous speaking.)

Member Ziemer: I think that one of the concerns is they continue to be called drafts so --

Co-Chair Anderson: Yeah.

Member Ziemer: -- no -- let's just call them guidelines. Yeah, that's the intent of the motion.

Mr. Katz: Okay. It's been seconded and that's discussion and so -- and Gen, do you have anything you want to discuss about this?

Member Roessler: Well, I'd like to have you repeat what the motion was, it sounded like bedlam to me.

(Laughter.)

Mr. Katz: So sorry, it probably did. So the motion is to approve the coworker dataset procedures as guidelines.

Member Roessler: The guidelines? Okay.

Mr. Katz: Correct.

Member Roessler: Okay.

Mr. Katz: Yeah.

Member Roessler: I got it.

Mr. Katz: Okay. All right, so all in favor?

(Chorus of aye.)

Mr. Katz: Okay, that's everyone plus one.

(Laughter.)

Co-Chair Anderson: One additional comment, I want to thank all the Members for their participation but I think we can now close down the committee.

Mr. Katz: No you can't.

(Laughter.)

Member Beach: It is, it's called an SEC committee. And that's just one small part.

Co-Chair Anderson: Oh, okay. Good point.

(Simultaneous speaking.)

Mr. Calhoun: I second that.

(Laughter.)

Member Beach: Denied.

Mr. Katz: Okay, so then that will get presented at the Board meeting then, during this session. And now Brad, you --

Co-Chair Clawson: No, it's too late in the day.

Mr. Katz: Okay, it's too late in the day. Maybe in the morning?

Co-Chair Clawson: We'll have to chance on it.

Adjourn

Mr. Katz: Okay. All right, so then I think we're adjourned for the day. Thank you everyone for all this amazing work and great discussion. See you bright and early at 8:30.

(Whereupon, the above-entitled matter went off the record at 4:45 p.m.)