US Department of Health and Human Services
Centers for Disease Control
National Institute for Occupational Safety and Health
Advisory Board on Radiation and Worker Health
Oak Ridge National Laboratory (X-10) Work Group
Wednesday, June 30, 2021

The Work Group convened via Video Teleconference, at 10:30 a.m. EDT, Gen Roessler, Chair, presiding.

Present:

Gen Roessler, Chair
Josie Beach, Member
Bill Field, Member
Loretta Valerio, Member

Also Present:

Rashaun Roberts, Designated Federal Official
Nancy Adams, NIOSH Contractor
Bob Barton, SC&A
Ron Buchanan, SC&A
Grady Calhoun, DCAS
Nancy Chalmers, ORAU Team
Joe Fitzgerald, SC&A
Rose Gogliotti, SC&A
Joe Guido, ORAU Team
Lara Hughes, DCAS
Tom Labone, ORAU Team
Pat Mccloskey, ORAU Team
Jenny Naylor, HHS OGC
Chuck Nelson, ORAU Team
Lavon Rutherford, DCAS
Tim Taulbee, DCAS
Keith Varnado, ORAU Team
Diane Whitten

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Proceedings

(10:30 a.m.)

Welcome and Roll Call/Introductions

Dr. Roberts: So, good morning and welcome, everyone. I'm Rashaun Roberts and I'm the Designated Federal Official for the Advisory Board on Radiation and Worker Health, and this is a meeting of the Oak Ridge National Laboratory (X-10) Working Group.

We do have a meeting agenda for today as per usual. If you have not seen the agenda, you can find it on the NIOSH website under scheduled meetings for today's date, along with all of the meeting materials and background materials, which were disseminated to the Work Group and posted on the NIOSH website in advance.

I do want to note, if you are looking at the agenda, that there are four items listed on it, but it really should only contain three items. Item 3 is a place for petitioner comments, but please note that there is not an active petition for this site at this time.

So I want to officially welcome all of you to the video conference, and first off, let's go ahead and address that issue of conflict of interest, and I will speak to that with regard to the members of the Board who sit on this Work Group, and in order for them to serve on the Working Group they can't have any conflicts of interest.

So with that, let me move into roll call for members of the Board on the Working Group, starting with the Chair, Gen Roessler.

(Roll call.)

Dr. Roberts: Before we officially move into the meeting, I just wanted to cover a couple of brief items.

So, if you are participating by phone, you need to, of course, make sure that you're on mute on your phone unless you need to speak. If you don't have the mute button, press *6 to mute. If you need to take yourself off, press *6 again. If you're participating by Zoom, you want to make sure that your mute button is engaged. You can find it on the lower left-hand side of your screen if you're not speaking. And I just want to ask people to periodically check your phone or computer to make sure that you're remaining on mute if you're not speaking.

As I mentioned earlier, the agenda for the meeting can be found on the NIOSH website. Access to the materials were provided to the Board Members and staff prior to the meeting. The agenda presentations, background documents that are relevant to today can be found on the NIOSH DCAS website.

So, with that, let's go ahead and get started. And I'll turn the meeting over at this point to the Working Group Chair, Gen.

Chair Roessler: Thank you, Rashaun. And welcome, everybody. This is actually the first meeting of the ORNL X-10 Work Group. The members of the group are myself as Chair, Josie Beach, Bill Field, and Loretta Valerio.

And even though this is the first meeting, this material will sound familiar to most of us. In the first place, some of us, maybe two or three of the Work Group Members, were Members of the Board in 2012 when SEC-189 was approved by the Board. And at that meeting we had a very nice introduction by Tim Taulbee of the ORNL X-10 Site.

And then, secondly, three of us on this Work Group are also on the Y-12 Work Group, and there's, as we know, an often lot of overlap there. So a lot of this will seem familiar.

This is a complex site, and it's a complex situation; at least I find it that with the overlap with Y-12, and I'm sure people are going to guide us through that. But, to begin, we thought we'd start with an overview of the site. And we asked Dr. Lara Hughes, who's the NIOSH lead for this site, to present that.

So, if there aren't any questions or anything, Lara, it's all yours.

(Pause.)

Overview of ORNL (X-10) Efforts

Dr. Hughes: I'm sorry, I was on mute. Thank you, Dr. Roessler. I started sharing my screen. Can everybody see this?

Member Beach: Yes, I sure can.

Dr. Hughes: Okay. This is actually a PDF file, but this is -- since the DCAS ORAU RPRT-90 was published, in 2018, there hasn't really been any product of NIOSH, other than responses to the SC&A review that was presented to the Board. So, today I'm just going to reuse my presentation that I gave to the Board in April of 2018 in Oak Ridge. And I'm just going to present the part of this that addresses RPRT-90 just to give everybody an overview, because it's been three years and everybody might need a bit of a refresher.

So, this is the Oak Ridge facilities update. And I want to start at slide 8. This is an overview of what was done for SEC-189. That was an SEC petition, and a Class was added up until 1955.

During this petition evaluation, there was a section that was reserved in the Evaluation Report, and that was to look at what we call the exotic radionuclides. This was anything that wasn't plutonium, uranium, thorium, and fission products. This was the stuff that ORNL produced for, like, commercial and government applications. They produced various radionuclides in the reactors, or in the cyclotrons or calutrons, at Y-12, and separated those out for use in various applications.

So, this reserved period since the Class was added until 1955, this effort was not really addressed under SEC anymore. It was the period after the SEC period for ORNL, up until 1988 when the majority of the radioisotope production had ceased. So this was the period that was evaluated for this report.

Again, I'm not going to go into detail here. The ORNL history, a lot of it -- or some of it involved around the graphite reactor at the site. And that was also a reactor used to produce some of these isotopes that we're talking about here.

This is a photograph of the site. I'm not sure from when; it's relatively modern.

This is a slide I'd like to go over a little bit more. So, this gray, black and white schematic here is a cutout of, like, a schematic map of the ORNL facility map. And this area here is what's called the isotope circle. This is a collection of around ten buildings that were used to separate radioisotopes that were produced either in the graphite reactor or the Y-12 cyclotron/calutron.

So, again, here is a schematic that shows the production of the radioisotopes that took place at X-10, ORNL, or Y-12, followed by a separation either at the ORNL labs or the Y-12 labs, and then the use was

either on-site or off-site. So, right now, what we're looking at is kind of like this area where the chemical separations took place after the isotopes were produced.

Again, some of these materials were produced in the calutrons at Y-12. After the calutrons initially were used for uranium enrichment, they were later repurposed for other isotope production. This is a picture of the beta calutron at Y-12.

So, the goal of RPRT-90 was to assess if there's any glaring infeasibilities with regards to dose reconstruction from those exotic radionuclides.

So, to do this, we looked at the available ORNL bioassay data for the period from 1955 to 1988. And so we have access to the ORNL database, which has about 100,000 bioassay results in it, starting in 1949. But 95,000 results apply to this period 1955 through 1988.

In this collection, there is an analyte code, so the different analytes that were assessed are coded with a number. And so the triple zero is what we're looking at. This indicates a non-standard method; so that's like the exotic radionuclide material.

We know from this database that it's incomplete, but it's not inaccurate. So, we have not collected all of the data that's available. We know there's some gross beta data missing from 1955 to 1959, but that doesn't really cause too much of a problem to do this review.

So, we also compared the available bioassay to the NOCTS, the bioassay that was available on NOCTS, about 20,000 results, and we did a comparison. So, we found that NOCTS data is a little more complete than the ORNL bioassay database, which was kind of what we expected because we knew it wasn't

complete. There wasn't any issue with accuracy, though.

The in vivo data that's available for ORNL, that started in June 1960 with the start of personnel counting at the in vivo counter. This program ramped up to -- ended up with routine operations starting around 1963 and increasing from there.

So, they had a system capacity of about 100 persons per month to count. And the selection who was to be counted using the in vivo counter was based on the decision of the area health physicist. There was kind of a five-part criterion, but, you know, depending on what the person was working with and what they potentially exposed to. They did baseline termination quarterly and semi-annual counts.

So, after we looked at the bioassay data, we also looked at the radioisotopes. What was there? What was the radioisotope inventory? And that data was parsed out of the available documentation that we have using shipping/sales reports, operational technical reports, logbooks, everything that was in the SRDB. So, this was quite a significant effort to do this.

And at this point I'd like to, you know, point out that this was all done by our contractor staff, ORAU. They did a very thorough job finding all these radionuclides.

So, this list does not include service irradiations. Service irradiations are if an entity from a different site brought their material to ORNL to have it irradiated for some application. And that was not included because that would have not have been processed on-site. It would only have been considered if, like, we ended up with something like target rupture and there would have been a dispersal

of material.

So, as a result of this analysis, we ended up with a table, Table 6-3 in RPRT-90, that lists 213 isotopes. So this table starts with hydrogen, ends with fermium. It shows the radionuclide and the years during which this radionuclide was present at the site.

And then, to assess whether or not there might be an infeasibility, we compared the annual production history of those 213 nuclides to the available bioassay methods for each year. We considered characteristic radionuclides emissions, such as type and energy, and the sensitivity of the analytical method. We did not reconcile quantity of radionuclide with the frequency of the monitoring method.

And we determined that once an adequate method was indicated that this method would also be available in the following years, if that kind of makes sense. And a gap is defined as if there's no monitoring results present for the years of interest. And so we --

(Audio interference.)

Dr. Roberts: So, excuse me, Lara.

Dr. Hughes: Yeah, sorry.

Dr. Roberts: I can see that there's a phone ending in 744 that is not muted and I think it's causing some interference. Thank you.

Dr. Hughes: Okay.

Dr. Roberts: I also see a phone now highlighted, 576, ending in 576, that may not be muted, either. So if you could please mute? Thank you.

(Pause.)

Dr. Hughes: Thank you, Rashaun. Are we good to go?

Dr. Roberts: I think so.

Dr. Hughes: Okay. Thank you. So, the main product of this analysis is this table that's presented in RPRT-90, of which you see an excerpt here on this slide.

NIOSH/ORAU RPRT-90 (March 2018): Monitoring Feasibility Evaluation for Exotic Radionuclides Produced by the Oak Ridge National Laboratory Isotopes Division

And so, for each radionuclide, we list the bioassay method code that was found. And then at the top, this is the years, this is the section that starts in 1955. And then for each field, we did a color or an indicator. So, green, G, means that for this year, 1955, for technetium-99, the radionuclide was present at the site, there was a bioassay method available that could be used to identify this radionuclide, and that we do have sample results in the bioassay database.

N means the radionuclide wasn't present and there's also no samples -- that no radionuclides were present in that year, so we did not identify any samples, either.

Yellow means the radionuclide was present. A bioassay method was present, but we did not have any samples during that year.

And red means the radionuclide was present, but there was no method identified to detect radionuclides, and that we needed to do some further analysis to see what's going on with this radionuclide.

So, we identified 34 radionuclides that needed

additional research. Six of those are iodine radionuclides, for which we suggested a dose reconstruction method in Appendix C. Twenty eight of these have very short half-lives, less than one year, and decay was either electron capture or isomeric transition.

So, for these, we came up with a kind of source-term-based approach to assess what doses could be incurred from these radionuclides where we don't have a method. So we did the listed inventory for each of these radionuclides and estimated an intake of 1 times 10 to the minus-5 of that listed inventory, and calculated 50-year committed organ doses to the highest organ from these. And we determined that a dosimetrically significant intake was not likely.

This is the table. It's also in the report. So, these are doses in millirem. These are fairly small. I think the highest we're looking at is this one, it looks like 1.4 rems to the lung, which is probably in the first year of exposure. So this is not an extremely high dose.

And, moving on, we did a little special assessment of the iodine exposure potential. Iodine was produced for commercial application since 1946 at ORNL. Since 1958, this was done through separation from reactor fuel. And the production from 1946 through 1964 ranged from 1.3 to 3,600 curies per year.

We have limited personnel monitoring data during that time. There's thyroid monitoring that took place from 1944 to 1954. There was also workplace controls available. And the separation buildings were 3026D and 3028, but the exposure really was possible wherever reactor fuel was produced. And they also started whole body counting for iodine in 1961.

So, what happened is we looked at the data from

1944 through 1957 -- actually, 1943 through 1957. We looked at all available data that was available for iodine, and we separated out into chronic and acute intakes. And we took the chronic data that was available and fitted it to a log-normal distribution, derived a 95th percentile intake from this data, and came up with the 5.4 times 10 to the 5 picocuries per day intake. This is represented here by this black line.

So these blue squares here, diamonds, are acute intakes that were present during that time. As you can see, the issue is that we're having a little gap here where we don't have a lot of iodine monitoring data. But there's some acute data here, there's some acute data here.

So, what we suggested is that the chronic 95th percentile of this intake derived from 1943 to 1957 bounds the acute intakes in the post-1955 period, right here. And so we determined that this was appropriate to assign to unmonitored workers from 1955 to the onset of the whole body counting for iodine in the early '60s.

So, this really wraps up the RPRT-90 recap. So, this RPRT-90, this was published in March of 2018 and it was presented to the Board in April, and then we received an SC&A review of RPRT-90 in October of 2018. And there were seven findings and six observations.

In June of 2020, NIOSH issued a response to this SC&A review, and then SC&A provided a review of the NIOSH response to the SC&A review of RPRT-90. So we have quite a number of reports now. And this latest SC&A report came out in January 2021.

So this is where we're at with ORNL and RPRT-90. So I think this concludes my introduction and summary and recap, so if you have any questions, I'd be happy

to take them.

Chair Roessler: Does anyone have any questions of Lara?

Member Beach: Yeah, Gen, this is Josie. I have a couple of questions, but I guess I'm looking for something overall, Lara, and maybe Tim. This kind of reads like an Evaluation Report, and I know the one that is out, what, 182 or 189, went through '55.

Do you not have claimants for this time period, the '55 to '88? And I guess I'm wondering why we're doing a 90 report instead of an ER?

Dr. Taulbee: Well, we don't have a petition. The petitioner originally filed only up through, I believe it was 1954, and we extended the petition up through '56 where we found the original infeasibility. And so we don't have a petitioner for that latter time period.

And what we do in these scenarios, when we identify an issue of concern, such as exotic radionuclides, what we do is we'll evaluate it, and if we find an infeasibility, then we'll go through the 83.14 process, and we'll identify a petitioner, or a potential petitioner, and contact them that we're having difficulty with the dose reconstruction, and then they can file under the 83.14 process, and we move forward that way.

Member Beach: Okay. And then I have a question for Lara. On page 18 of your slide presentation, the coding, you'll have some radionuclides in a particular year, and then none in the next year, and then you'll see them again in the next year.

I guess I'm a little confused. Does that radionuclide just go away between, say, '66, then you don't see it in '67, but you see it again in '68? So --

Dr. Hughes: Yeah, I mean, that's possible. I mean, we go by inventory of information found in, like, logbooks and shipping records and sales records. So it's possible that they produced a certain radionuclide for -- you know, maybe they had a request, and then they produced it and sold it, and then they did not produce it the next year. At least that's my understanding of the process.

Member Beach: Okay, so the presumption would be that it wouldn't be available at all? There would be -- okay, anyway, that's fine.

Dr. Hughes: As far as we know. I mean, it's --

Dr. Taulbee: Right, and that's possible for many of these radionuclides because they have such short half-lives. And so there'd be campaigns to make them, they would sell the product, and then, you know, basically any residuals decayed away completely. And then the following year, or two years later, they'd make it again. That type of scenario.

Member Beach: Got you. Okay, thank you.

Chair Roessler: Okay, are there any other questions?

Member Field: Yes, Lara, this is Bill. I was just curious. If you can remember, where'd that data came from for iodine from '57?

Dr. Hughes: '57?

Member Field: I think there is some -- '57, you said there weren't any programs to monitor for iodine at that time.

Dr. Hughes: Well, there's a few. I think --

Member Field: Yeah.

Dr. Hughes: Yeah, there's a few acute.

Member Field: Oh, okay.

Dr. Hughes: So, I would have to look at my spreadsheet. There were a few acute measurements. So, this would come from thyroid measurements.

Member Field: Was there some sort of accident or something in post-'65 that's --

Dr. Hughes: Oh, '65?

Member Field: The potassium iodide was administered in, what's that --

Dr. Hughes: Yeah, this was based on an incident report. A lot of the acute data that comes out of incident reports.

Member Field: Okay.

Chair Roessler: Any other questions or comments?

(No audible response.)

Chair Roessler: Well, then I just have one comment. For the rest of the meeting today, there are obviously a lot of situations at ORNL and the related site, Y-12, but we're going to concentrate just on RPRT-90, on the exotic radionuclides. And it's my understanding from the agenda that we're going to -- and maybe I'm wrong -- but have four presentations.

First, NIOSH is going to present on RPRT-90. Then SC&A is going to talk about their review of RPRT-90. Then we'll hear NIOSH's response to the SC&A review. And then, finally, the fourth report will be SC&A's response to NIOSH's response to SC&A's review.

So it seems to me that the Work Group really doesn't have to make any determinations or conclusions until we get down to the final report, which will be by

SC&A. And maybe NIOSH will respond to that.

Am I right on that, Rashaun and Tim?

Dr. Hughes: Gen, this is Lara. We don't have another presentation. We will respond to the issues in SC&A's presentation. I mean, that would just be based on the discussion in the Work Group, but there's not another presentation.

Chair Roessler: Okay. So, on the agenda, then, for Item Number 2, the presentation, we're not going to have the first presentations, we're going to go right into the iii. Is that correct?

Dr. Taulbee: Gen, this is Tim. The way SC&A has really laid out their presentation is that Lara just gave the review of RPRT-90, an overview of it. And then SC&A, the way they've laid out their presentation, is going through each of the findings, and they discuss their finding and then NIOSH's response, and then their response. So, really, your Items 2 and 3 are combined into that next presentation.

Does that make sense to you?

Chair Roessler: That makes sense. That's the way I thought it should be. I just wanted to make sure. Then we'll be ready to jump right into SC&A's presentation, which is the January 2021 report.

Member Beach: Gen, I wanted to point out, under Section 2 on the agenda, the first under i, that report shows it's in October 2019, and that should be '18, I believe. I looked for an October –-

Chair Roessler: Right. Yeah. That was corrected in one of the emails we just got the other day.

Member Beach: It was? Okay. So I have an older one, then. Okay, thanks, sorry.

Chair Roessler: That's right, though. Okay, then. I think we're ready for the SC&A's report.

Mr. Fitzgerald: Yeah, thank you, Gen. This is Joe Fitzgerald. And Bob Barton, I think, is going to put the presentation up.-

Mr. Barton: Yeah, Joe, that right. Lara, I'm going to need you to stop sharing so that I can take over the screen, because it won't let me while your slides up, okay?

Dr. Hughes: Okay.

Mr. Fitzgerald: And just while you're doing that, Tim is correct. Given the back and forth, we thought it would be much more coherent for the Work Group if we presented our original findings and cited NIOSH's response to that finding. And, of course, NIOSH can jump in as we go through this if there's anything to add. And then, you know, pretty much our reaction to that. You know, it's been a couple years, obviously, since the 90 was tasked. We were tasked in April 2018. Actually, it's three years. So, there's been some fruitful, I think, exchanges on this in the meantime; clarifications, if you may. So I think, you know, again, time was well-spent, but I think this is an opportunity to catch the Work Group up on pretty much, you know, what was in that exchange. So we'll go through this that way.

Bob, if you can go to Finding 1? The intro was pretty much covered by Lara in terms of RPRT-90 and, you know, some of the exchanges. So, really, we ought to just go ahead and start with Finding 1.

And Finding 1, you know, again, RPRT-90 certainly was a bit of a different type of report from a number of ones that we have looked at. And so we had some questions on scoping, because, obviously, the isotope production was the central part of the review, but, as

with many sites, the D&D and waste management, some of the other handling aspects of radiological materials, we were looking for some clarification as to how this review was scoped. So, on this particular one, we asked for the scoping in terms of whether it would include D&D and waste management. And NIOSH responded that the scope of RPRT-90 was purposely limited to the production phase in terms of both the Oak Ridge and the Y-12 footprints. And it was not intended to be an evaluation of whether a co-exposure model type approach could be developed for every single nuclide.

So that was the response. Again, I thought that clarified our question, again, regarding, you know, where this began and left off. And so we accept that, and understand that it is specifically the production phase and does not address other phases, including D&D, construction, maintenance, and all the other issues.

Now, I would footnote that recommendation for closure by saying that, of course, if there's any infeasibilities, then that could have implications for these other phases. I mean, obviously, if the monitoring was not feasible, then certainly that question might carry over to D&D and waste management, as well as other activities, like maintenance.

But in the context of production, we're satisfied with that answer. Finding 2.

Chair Roessler: Joe?

Mr. Fitzgerald: Yeah?

Chair Roessler: Can I interrupt a minute? I'm wondering if this would go more easily and smoothly if we discussed each finding as you went. And I think, in a case where there's a recommendation, then the

Work Group should have a vote.

Mr. Fitzgerald: Okay. Well, fine. I'll turn it over to the Work Group, then.

Chair Roessler: Okay. So on Finding 1, then, SC&A recommends closure. Are there any questions or comments from Work Group Members on that?

Member Beach: I guess for me, just when you talk about excluding the D&D and that maintenance group activities, where does that get picked up at? Because this is such a different scenario than we normally deal with. I just want a little clarification on that.

Dr. Taulbee: This is Tim. I'll jump in, if you don't mind, Lara.

Where that would be picked up is in like a co-exposure model, is where that would be picked up. The scope of this report was to evaluate what radionuclides were produced, and which time periods, and was there bioassay or a monitoring method associated with them? That's the full scope of this particular report.

What you're asking about, you know, which workers are monitored and were they monitored, would all be covered under a co-exposure model development.

Mr. Fitzgerald: And if I can add, Josie, this is Joe Fitzgerald. That was my sort of footnote comment a minute ago, that, you know, assuming that, you know, the feasibility analysis as presented RPRT-90 is accepted by the Work Group as-is, there would be no reason or basis for a co-exposure model consideration.

So, you know, that's kind of the basis for the recommendation, but it does have that asterisk. Of

course, when we get to Finding 3, we do raise some questions about how that all works, you know, this question of capability versus feasibility.

Member Beach: Okay, thanks.

Chair Roessler: Okay, any other questions, comments?

(No audible response.)

Chair Roessler: Then I move that the Work Group accept SC&A's recommendation for closure on Finding 1. And I guess the Work Group Members can just respond.

Member Field: This is Bill. I agree.

Member Beach: Yeah, this is Josie. I'll agree with that also.

Member Valerio: This is Loretta. I agree as well.

Chair Roessler: Okay, thank you. So, go ahead, Joe, then on Finding 2.

Mr. Fitzgerald: Okay. Finding 2, you know, it's a pretty extensive listing, and I'll certainly give credit to ORAU for the work that they did putting this together, but we did go through and just do a bit of a cursory validation of the listing, looked at some of the source terms that were in the SRDB as applying to the buildings and facilities that were radioisotope-handling facilities. And we identified some discrepancies, mostly just to clarify how that relates to how the document was put together. And NIOSH's response was that the discrepancies that we did cite were related to the scope of the document, but the isotopes reduced by the isotopes group certainly is different than a more general analysis of the overall nuclide inventory at Oak Ridge.

So, if one were to, again, confine the scope of 90 to production per se and not look at everything that was in these facilities, which would be representative of a broader Oak Ridge National Lab inventory, then you would have a somewhat more constrained list. And, again, we wanted to understand the scoping for RPRT-90 since, again, I think that's also important for the Work Group in terms of what we're actually considering.

And I think in this last bullet NIOSH made it clear that the inventory listing itself was developed independent of a broader facility listing, which is what we were looking at, and therefore would be different from that list.

So, that clarifies the question of the nuclide scoping, and, again, we accept that and recommend closures to the Work Group on that.

Chair Roessler: Okay. Any questions on Finding 2?

(No audible response.)

Chair Roessler: If there are none, then I move that the Work Group close Finding 2. Work Group Members?

Member Beach: Gen, this is Josie, I'll accept that closure.

Member Field: This is Bill. Agreed.

Chair Roessler: Loretta?

Member Valerio: Sorry, I was trying to unmute. I agree.

Chair Roessler: Okay. Good. Then we can move on to Finding 3.

Mr. Fitzgerald: Okay. Well, obviously, Finding 3 is

probably a bit of the meat of our concern over the approach in RPRT-90,

And, again, RPRT-90 is a bit of a unique animal. I mean, it certainly is a good effort to scope out the nuclides for a particular production operation. But, as we wade into the waters of looking at capability and feasibility, both of which are terms used in RPRT-90, obviously, as the Work Group knows, and as we've done in the Board, feasibility is a loaded term. And we're concerned about the context by which feasibility's being used in RPRT-90.

Anyway, our finding basically is Attachment A, which is the lengthy listing of in vitro bioassay methods as a measure of capability lacks information about how those methods were actually applied, or whether they were even applied, for the nuclides in question.

And in our report we raised questions about how, certainly for other sites in terms of reviews, in terms of bioassay procedures, the actual practice, what was done at the site, didn't necessarily marry up to what the procedures called for, or even what the technological capabilities provided for. And we offered up some examples.

Most pointedly, I think we cited Los Alamos as an example, an illustrative example. Obviously, we don't try to compare SECs, but I think the precedent is there, where a technological capability in terms of in vivo monitoring of mixed activation products was claimed as a basis for the feasibility of monitoring for MAPs. And it turns out that, in that particular case, even though that even though the capability existed for some years, it was not applied routinely to looking at MAPs at Los Alamos in a way which would have provided a reliable basis for dose reconstruction, and was certainly a basis for the SEC for a time period at Los Alamos.

So, just one example, but there's other examples at other sites where the procedures for bioassays and other dosimetry techniques were available at the site, but were, in fact, not applied in practice. And that's our concern, in that regard, that, in terms of RPRT-90, there is an equating of capability which comes down to an existing procedure and technology with actual practice, which is the feasibility of monitoring that particular nuclide, and therefore being able to dose reconstruct against that nuclide.

And even though, I think as Tim was saying, that, you know, this is a survey to look at nuclide-by-nuclide feasibility to ascertain whether an evaluation review, Evaluation Report, would be necessary, this in effect becomes a de facto evaluation because you're looking at the monitoring for these exotics and establishing whether the capability construes feasibility of dose reconstruction. That's what's basically stated as an intent. And it becomes a de facto evaluation, which would preclude a full-fledged evaluation review, an ER, which would go further and establish whether the weight of evidence would support dose reconstruction.

So we're concerned about, you know, what seems to be a de facto evaluation, that has some of the elements of full-fledged evaluation but does not look at the weight of evidence, which would include implementation, would include whether in fact the procedures were applied consistently, would look at whether or not the monitoring for the years in question could be validated or not.

So, that part, that's the essence of Finding 3. Now, the NIOSH response is that NIOSH intended RPRT-90 to be a review of the isotopes handled by the isotopes production group in comparison to the available bioassay capability. Okay? And though not all available data on sporadically produced radionuclides

will be of sufficient quantity to allow for their use in a co-exposure model, this alone is not indicative that a potential exposure could not be bound with sufficient accuracy.

And, again, our response was a review of dosimetry capability, while necessary to validate that measurement techniques were technically acceptable and available -- which I think RPRT-90 accomplishes -- is not sufficient to address the feasibility of dose reconstruction, which is the intent that was listed in RPRT-90.

And our point is identifying the number of samples devoid of exposure potential considerations over the 30 years of isotope division production we do not believe satisfies the co-exposure guidelines, even though this is not a co-exposure review. Certainly, a presentation that there were samples taken -- in some cases, missing samples -- without at least tying that to what the potential for exposure to that particular source term is, we don't think satisfies this question of data adequacy.

So, again, we get back to the conundrum of what RPRT-90 actually means. You know, is it an evaluation of feasibility of dose reconstruction? Because it basically lists that as one of the purposes. Or is it a comparison of capabilities for the isotopes produced, which is the response that NIOSH has provided to this finding.

It can't be both, in the sense that if, in fact, it is a feasibility review for dose reconstructability, that is almost one and the same with what an ER examines, and goes beyond the simple comparison of capabilities. Essentially, it equates the capability in terms of the paper analysis, the procedures, the technology, with the feasibility of dose reconstruction, which gets into whether or not you

can establish an exposure potential and whether the monitoring on-site actually took place. Did it actually take place?

At other sites, the reality did not match the paper, and that's the concern that we are expressing.

Chair Roessler: Okay, I assume NIOSH is going to respond to that. Or are there questions from the Work Group? Either can go next.

Dr. Taulbee: If I could interject here, I want to clarify a few things that Joe stated that caused me a lot of concern.

In no way does RPRT-90 preclude an Evaluation Report. It was never intended to be that, from that standpoint. This was an evaluation, as I indicated, of a comparison of what was produced and whether there's a bioassay method that was available.

What we are looking for, at the beginning of this, was there any gross infeasibilities that could exist that we do need to move forward with an SEC evaluation from that standpoint? And we did that, actually, coming out of this report. Plutonium-241 is one of them that was produced at Y-12 that came out of this report that ended up resulting in a recommendation here.

This is kind of a screening method, if you will. So, there's feasibility of different degrees. Okay? This is also never intended to be a co-exposure model, full evaluation, that SC&A seems to be wanting us to do all in one report here. That was not the intent of this particular report, neither was it to be a final evaluation.

When we go through and we do a co-exposure model and we find that maybe one of these radionuclides, we go through, and we find that we don't have

enough bioassay in order to do a co-exposure model for that particular radionuclide, then we can go down the 83.14 path and designate an SEC from that standpoint.

So, to, you know, flat-out say that we haven't proved all of these things, we know that with this report. This report was to be an initial evaluation of what radionuclides were produced at which time and which bioassays available.

In an Evaluation Report, bioassay is only one of the keys for whether we can do dose reconstruction. There's workplace monitoring that goes on, air sampling data, contamination surveys, as well as source term data that can be used to estimate doses.

I just wanted to make those clarifications before we go forward.

Mr. Fitzgerald: And before the Work Group examines this, Tim, on page 6, bullet 4, which gets into the purposes of RPRT-90 -- and this is what gave us real pause, and I'll quote -- this is one of the purposes: evaluation of identified monitoring gaps to determine if dose reconstruction for these exotic radionuclides is feasible, period.

I hear what you're saying, but that's not really what this says. I mean, this is very deliberate and very specific, the fact that this will determine if reconstruction for these exotic nuclides is feasible.

So, yes, I can see where this could be a bridge to a screening process, but this review will conclude whether or not dose reconstruction is feasible. So I think this goes further than what you're alluding to, based on what actually is stated in the report.

Dr. Taulbee: And if I could follow up to that?

Chair Roessler: What page were you on? Were --

Mr. Fitzgerald: Page 6, bullet 4 of RPRT-90.

Chair Roessler: Oh, RPRT-90, okay. Go ahead, Tim.

Dr. Taulbee: And if you look at the date of when RPRT-90 was approved, and if you look at when IG-6 was approved for methods for co-exposure models, things have changed. Okay?

Back in 2018, we had a draft Implementation Guide that had not been approved by the Board yet. We now have an Implementation Guide approved by the Board. We now know the criteria that we have to evaluate. And this report was written before that, okay? So, we now know the criteria that we have to grade against in order to do that, in order to meet these things.

So, while it might have been the initial intent to be that, from a feasibility standpoint, now that we have IG-6 out and approved, we know what the criteria are that we have to meet. This report didn't meet that. We know that. That's why we would do a co-exposure model that would meet all of those criteria in IG-6.

Member Beach: And this is Josie. I'm just going to make a comment. I feel like what you're saying, Tim, really makes this a difficult issue for the Work Group when we're comparing something to an earlier report that you say basically needs updated, and we're supposed to make a determination on a report that is outdated at this point.

And there's a lot of questions that SC&A has posed to NIOSH in their final report --well, in all their reports. The one I'm specifically talking about is January 8, 2021. There's several questions that need to be answered by NIOSH. And so this creates a difficult situation, I believe.

Dr. Taulbee: Right. If I could interject a little bit of kind of our plan here along those lines.

Whether you want to leave this finding open or closed is entirely up to the Work Group, you know. Obviously, it is. But what I'm trying to make the case for is that the types of things that SC&A is asking for in Finding 3 and in Finding 4 are -- well, Finding 3, mostly -- are really things that would be addressed in a co-exposure evaluation or OTIB. This isn't something that we would revise RPRT-90 to incorporate. And that's what I'm trying to get at from this particular comment.

We know these things have to be addressed, especially for a co-exposure model, and we plan to do so. We know that RPRT-90 needs to be revised based upon, you know, some of our response to SC&A findings. We've agreed to incorporate some of the things that they've said. But this particular finding of wanting us to go through and basically do a full co-exposure evaluation within RPRT-90 is not the intent of RPRT-90. That would be a separate OTIB that would be done. And from that standpoint, SC&A is, obviously, free and would be welcomed, and the Board to review that and provide comments, and hopefully we wouldn't see this comment again because we would address it in the OTIB for the ORNL co-exposure model.

Does that make sense, Josie?

Member Beach: It does, Tim, but it's also a huge issue, so, yeah. Thanks.

Chair Roessler: So, to me, it seems like Tim has answered the concern here. However, I think before the Work Group would accept it, we have to have a mechanism for making sure this is in writing, that we have something to go back on and follow.

Member Beach: Yeah, I agree with that. I'm in no way ready to close this item.

Dr. Taulbee: May I suggest, then, that you leave it open pending the development of an ORNL co-exposure model?

Chair Roessler: Leave open until development of an ORNL exposure model. Okay. How does the Work Group feel about that?

Member Field: Hey, Tim, this is Bill. You also said you were going to update RPRT-90. Is that right? I mean, if you clarify the intent, I think that takes care a lot of the concerns.

Dr. Taulbee: That's correct.

Member Beach: And this is Josie. Can I ask when the plan to update 90 is, when it's on your work scope?

(Pause.)

Dr. Hughes: I think, Tim, you're on mute.

Member Field: You're on mute. Yeah.

Dr. Taulbee: Sorry. The update was pending the outcome of this Work Group meeting, in large part to get feedback from the Work Group. So, I mean, if we're in agreement with these, you know, some of the things that SC&A reported to close out, and the Work Group agrees with this type of response, then we will do the update.

So, I can't give you an exact timeframe. I'm sorry, Josie, but it would be on our list to do next with this.

Chair Roessler: So, Joe, if NIOSH says that they will develop an ORNL exposure model and they will update RPRT-90 with regard to these comments, would you be willing to close this finding?

Mr. Fitzgerald: Well, you know, I think what we said earlier was we can keep it open pending the delivery of these products in the future.

Our only concern is that RPRT-0090 has been issued, and clearly it's been overtaken by events, as Tim was suggesting, and what it basically states is not what it's intended to provide, which is a firm finding on feasibility. So, that's a pretty important change, so I would recommend to the Work Group to consider keeping it open, but I'm quite comfortable with what Tim is suggesting, which is, you know, to look at a co-exposure model and to come up with a revision to RPRT-90 which would emphasize it's a comparison of capability which is fine as a screening tool, but not go so far as to establish feasibility which is what the co-exposure work would do. So, I think that would be the resolution.

Chair Roessler: So then I guess I make the motion that the Work Group accepts SC&A's recommendation that this item remain open, pending the development of an ORNL exposure model, and an update of RPRT-0090. Does that include everything?

Member Field: This is Bill, I think you've covered it.

Chair Roessler: Okay. Then, so I guess Bill accepts.

Member Beach: This is Josie. I'll accept that also, Gen. Thanks.

Member Valerio: This is Loretta. I agree.

Chair Roessler: Okay, thank you. Then I think we can move on to Finding 4.

Mr. Fitzgerald: Okay. It's going to be Ron or Bob. I can't remember which one.

Dr. Buchanan: Yeah. This is Ron Buchanan with

SC&A, and Finding 4 I'll discuss now.

Now, Finding 4 was concerned with the feasibility of monitoring the 28 radionuclides considered not adequately addressed.

And this centers around Table 7-6 of the original RPRT-90. So can we have the next slide, Bob?

Okay, the red blocks in Table 7-2 and 7-3 mean that the specific radionuclide was present in inventory in a specified year, but additional analysis is necessary to determine if a nuclide represented an infeasibility from a monitoring perspective.

Now, Table 7-6 on page 41, RPRT-90, uses the derived air concentrations from Table 7-5 to illustrate the maximum organ dose for a hypothetical intake.

Next slide, please.

Now, SC&A expressed concern about the results of this table and what it meant to feasibility of a dose reconstruction, and we understand NIOSH's response was that implementation of the monitoring program is indicated by the availability of bioassay cards showing results for the respective method. And any available bioassay data would be used to assign dose to claimant.

And additional review of available records and monitoring procedures will be ongoing using the data available in the Site Research Database.

And next slide.

Okay, gaps in the table. Table 7-6 in the original RPRT-90, there were some gaps where there was missing data, and in NIOSH's 2020 response, they presented some supplementary information to address some of the gaps in Table 7-6 in the original

RPRT-90, and we evaluated those additional information, and we agree that they was correct, but we feel that the doses in Table 7-6, although they're not alarming, they don't appear insignificant for potential unmonitored exposure.

For example, you know, we report on the IREP tables anything one millirem and greater, and all these were greater than one millirem.

And so, we don't feel that just presenting the derived dose maximum maybe an organ would get from exposure to this, does not correctly address the problem of assigning dose to someone who had intake if they were monitored for some radionuclides, a whole body counter, a urinalysis, or whatever, and they were assigned a dose for certain radionuclides.

If these radionuclides weren't being monitored for, and apparently there wasn't a method at time to monitor for them, then we don't feel that addresses the monitoring feasibility question.

Next. And so in this slide, we say that we don't feel that the nuclide represents an infeasibility from a monitoring perspective. We feel that remains relevant.

And that is not that it should be addressed more completely, and we don't feel it was completely addressed in Section 7.2 or Section 8 of the original report or NIOSH's recent response.

So, where it leaves SC&A as saying, okay, the 28 radionuclides was considered to need further analysis, and the only analysis it really received was to calculate the maximum hypothetical dose to an organ that it might receive from a certain exposure to them, so we don't know if it answered your question that was originally posed, and can we monitor for these or are the bioassay records there?

So that's why we recommend it remains open, and I don't know if NIOSH has plans to further address these in any updates, or have done any further work on these or not. I'll let them respond to that.

Chair Roessler: Okay, yeah, Tim probably wants -- or Lara might want to respond first on this?

Dr. Hughes: Oh, I can. Is Tim here? If he wants to chime in.

Dr. Taulbee: No, go ahead, Lara.

Dr. Hughes: No, I mean, one thing I'd like to remark on, bioassay is not the only, you know, the only means that we can use to do dose reconstruction, so this is based on a source term, basically.

So this is like a bounding source term approach, kind of, application, so this is kind of what we're suggesting for these radionuclides for which there is no bioassay method available, and there has not been any additional work on this since this was published, so this is kind of what we're suggesting.

And Tim, if you'd like to add.

Dr. Taulbee: Sure. The part that I would add is, what exactly is SC&A, the Work Group, looking for for us to demonstrate the feasibility for these radionuclides for which we do not have bioassay?

So these would not appear in a co-exposure model, it would be an exposure model, either from source term or from air sampling data, or contamination surveys, along those lines.

What we presented to you was a source term-based type of model that Lara and her team developed of taking the inventory that was produced and assuming a fraction of it was inhaled, and what is the resulting

doses? And Lara presented that back in her presentation earlier, and you see these 50 year committed doses are very low.

You know, as SC&A said, they're not alarming, but they're not insignificant. Well, we agree they're not insignificant, they're more than one millirem.

What we're proposing is to assign an exposure model, this source term exposure model, that would then be used for dose reconstruction for these radionuclides.

And again, you know, these 50-year doses are rather low when you look at it, and it's mostly due to the type of radiation emission.

Many of these are electron capture decay mechanisms for these radionuclides, which don't deliver very much internal dose at all.

Member Beach: So, Tim, this is Josie, or Lara. Has that exposure model been developed as yet?

Dr. Taulbee: It is proposed. They're in RPRT-90. That Table 7-6 --

Member Beach: Okay, and so --

Dr. Taulbee: -- goes through what it is we proposed.

Member Beach: So that is the proposal?

Dr. Taulbee: Correct.

Member Beach: And there's no other work that you intend to do for that exposure model?

Dr. Taulbee: Well, if the, you know, Work Group doesn't like that exposure model, or, you know, wants us to do further work, we certainly can.

We have not looked at air sampling data, we have not looked at other, you know, mechanisms. We simply looked at the source term method.

Chair Roessler: So what I'm hearing you say, Tim, is that, in the cases where you don't have the bioassay, you're going to depend on using the source term as a bounding way of estimating dose, and that seems really consistent with everything that you do and have done.

I don't really understand SC&A's comments or why that's not suitable.

Dr. Buchanan: Okay, this is Ron again. Well, because in RPRT-90, it just presents the maximum dose. It doesn't say when this will be applied and to whom it'd be applied. How do we know who's going to be exposed?

Would you apply this to everyone all the time, or, you know, how would you know that this was going to be applied and when, to who?

Dr. Taulbee: So, if I'm understanding correctly, SC&A's question then is really more of who this would be applied to, and from an implementation type of standpoint, not that those doses don't represent a bounding dose.

Is that correct, sir?

Dr. Buchanan: Well, assuming that we accept this, because it really wasn't proposed as a co-exposure model or it wasn't stated in RPRT-90 when it would be used.

It just says, okay, here's the facts, here's the list of doses. But there is no further explanation of how or when it'd be used. So, say that we were to accept it as being bounding, when would it be used, to what

workers, because you had many years, and you had a lot of different workers.

Sometimes the stuff was here, sometimes it wasn't. Just like you said earlier, there was periods of use, periods of non-use.

So, it's just kind of to me hanging there in the air, okay, this is a map from dose for these radionuclides, but how would you actually use that?

Mr. Barton: And this is Bob.

I think a lot of this goes back to really what has been clarified as a more limited scope of what RPRT-90 was intended to accomplish, whereas we read the words, you know, feasibility of dose reconstruction, which, as Joe mentioned, is a very loaded word in this program.

We took that to mean that this was essentially laying out dose reconstruction methods, and in some cases, it did explicitly do that, such as for radioiodine, which we're about to get next.

But I think the clarifications that were made today, that, listen, this report was issued back in 2018, I believe a year before the actual IG-006 was actually put into action officially.

So I think there's a little bit of confusion, and again, our original review was also in 2018, so we were operating sort of on the draft IG-006 IG-006 guidelines, which don't -- as you pointed out, Tim -- don't necessarily apply here because they hadn't been approved yet officially, and so that's going to be further down the line, is some of these methods, such as the source term method that was sort of outlined but not explicitly laid out, as Ron pointed out, as to who's going to get what doses, from what areas, and how do you identify workers?

All these sorts of things that usually go into the overall feasibility discussion really probably aren't applicable here, even though the word feasibility was used.

So I think that's where the confusion lies in a lot of these findings, which again date back to before IG-006 was finalized as well.

So I think, Tim, partly we do agree with you there that, listen, a lot of these questions are about, can it be implemented in a feasible way such that dose reconstruction overall is feasible for all these different radionuclides?

And as has been made clear today, that was not the intent and scope of RPRT-90, which appears to be, possibly with the exception of radioiodine, limited to a comparison of what we know about the source terms of the site and what we know about the bioasssay capability at the site, and that seems to be a scope of RPRT-90, and a lot of these other questions remain relevant but weren't intended to necessarily be answered by RPRT-90, but will be answered down the line, at least that's my understanding of the discussion so far on this.

Member Beach: Well, and this is Josie. I have a question then for you, Bob and/or Ron.

Based on what Tim has discussed earlier today, has that changed your recommendation for Finding 4, or do you still believe it should remain open?

I believe this is a little bit connected to Finding 3 also. But anyway, have you changed your recommendation?

Mr. Barton: Well I guess, in my opinion, it's very similar to Finding 3 in that it's a relevant question going forward, whether or not we want to close it

under RPRT-90 review and await the forthcoming of a co-exposure model, which I assume would cover all of these different exposure sources.

It would essentially be commuted to any future reviews of the co-exposure, depending on how that comes out from NIOSH's research and evaluation of co-exposure feasibility.

So in a sense, this is possibly not entirely relevant to RPRT-90 specifically, but certainly relevant to the X-10 discussion going forward.

So whether or not we want to keep it open so that we're keeping this in mind when co-exposure modeling is being discussed, when that happens in the future, and certainly the overall question I don't think is going away, but possibly under the guise of RPRT-90, it could go away.

Ron, I think I talked over you there a little bit, if you want to weigh-in.

Dr. Buchanan: Yeah. This is Ron again. Well, the problem I had when I did the original evaluation of RPRT-90 was on page 43. I was left with the impression, in the summary, the last sentence there, it says that these relatively low radiotoxicity of these, some isotopes in comparison with a bounding potential intake, Table 7-6 lends credence to the decision that a significant intake of one of these nuclides would not be credible.

So, to me, significant intake means that -- well, it told me that you weren't going to consider it further.

Significant intake, that it wouldn't be dose-significant.

So, that is why I felt that Table 7-6 just left the reader hanging in the air what are you going to do with it?

Okay, these are the doses, what are you going to do with it?

And even if RPRT-90 isn't a co-exposure model paper, it really left me the impression that it was just going to be disregarded.

And now you're saying that's not true, but I think the wording on page 43 could be improved to not give that impression.

Chair Roessler: So it seems then what we should do here to be consistent with Finding 3 -- and let me know if I'm wrong in this approach -- but it seems that SC&A wants to recommend that this finding remain open until NIOSH updates RPRT-90, and then some wording in there, some short wording that will identify exactly what you're looking for?

Member Beach: Gen, yeah, that's what I was thinking also. This is Josie.

I also, Gen, wanted to -- Tim mentioned other sampling results, air samples, things like that associated with this finding.

Tim, can you expand on that a little bit, of what --

Dr. Taulbee: Well, what I was meaning is that if, you know, if our current approach is not acceptable, I mean, there are other methods that we could do, but I mean, we feel the current method is bounding, and demonstrates that, you know, dose reconstruction is feasible using this type of approach.

And, you know, one of the goals of all of our reports whenever we do an analysis of something like this, is that it eventually ends up in the Technical Basis Document for dose reconstructors, and, you know, it might just be a reference type of scenario.

But what we wanted to try and point out here is that these were radionuclides where we, you know, identified they produced, produced these, and there is no bioassay method.

So, the only method for dose reconstruction that we've come up with is this, you know, source term type-based approach.

And as you noted here now, these are, you know, 50-year committed doses, so, you know, when we go to apply these or implement them, some of them that are very low, there may not be any dose to be assigned, you know, because, you know, if it's less than a millirem, you know, in a given year, we generally don't, you know, assign that dose. That's kind of the de minimis type of value.

So, but then there's others in that list. You know, clearly I think it's tin-119 metastable that, you know, would have, you know, a dose assigned from that standpoint.

So, we can update the implementation that SC&A is asking for in RPRT-90, or we can clarify it as, you know, as I think you were suggesting there, Ron, of, you know, the feasibility statement that we, you know, stated that there would not be a significant intake.

We can change wording there, or we can, you know, add the additional -- well, we need to change that wording anyway -- but we can add the additional language in this one or we can give the instructions in a Technical Basis Document update.

Is there any preference from the Work Group there?

Member Field: This is Bill. I think changing the wording would go a long way to solving the problem.

Dr. Taulbee: Okay.

Member Field: Yeah.

Dr. Taulbee: We can do that.

Member Field: Tim, I did have a question, though.

And it seems reasonable to use a source term model, but I'm wondering, when you have air sampling data, wouldn't you look at that to see if there's any red flags with using source term?

Dr. Taulbee: We don't physically -- well, I don't know that we physically have it in the SRDB. I can't check right now.

Member Field: Okay.

Dr. Taulbee: But I don't believe that we have captured the air sampling data in large scale. We know it exists.

Member Field: I see.

Member Beach: This is Josie again, sorry.

It seems like there's a lot of information that could be still obtainable that NIOSH or SC&A doesn't have access to. Is that correct?

I mean, you haven't gone back to the facility.

I know one of SC&A's recommendations was to do some more interviews, or do interviews, because I don't believe that's been done.

Anyway, it seems like there should be more out there that would help this process.

Chair Roessler: But the thing is that they often use, when they don't have bioassay, the source term

approach, and then particularly in cases where it's very low, and before we accepted this approach as a bounding method, so to me, I'm satisfied.

Member Beach: No, and Gen, I agree with that, but I also know that there could be a lot more information available that is not available at this point --

Chair Roessler: But if --

Member Beach: Because it hasn't been looked for on-site.

Chair Roessler: If you don't need it to come up with a satisfactory estimate, then it seems like it's not necessary to go look for it.

Member Beach: Oh, I'm not sure I agree with that, but -- and I'll leave it at that.

(Simultaneous speaking.)

Member Field: But Tim, you said there's no bioassay data. Is that correct?

Dr. Taulbee: Not for these radionuclides.

Member Field: Yes.

Dr. Taulbee: That's correct. But -- yeah. That's correct.

Chair Roessler: So to address this finding, I guess I'd ask SC&A, I think it's okay to leave this finding open and it's comparable to Finding 3, but we need some wording in there that says until NIOSH does -- what do you want to put in there to make sure that this is covered?

Dr. Taulbee: I propose that until NIOSH revises RPRT-90.

Chair Roessler: And would that be suitable to SC&A?

Mr. Fitzgerald: Well --

Dr. Buchanan: Would it remain open until --

Mr. Fitzgerald: Sorry, Ron, please.

Dr. Buchanan: Yeah, you were saying that finding would remain open until NIOSH puts suitable wording in the revised RPRT-90?

Chair Roessler: Okay, did somebody get that wording? I didn't get it written down. Does that sound satisfactory to everybody?

Member Beach: Yes, that Finding 4 would remain open until RPRT-90 is revised.

Chair Roessler: Okay. I move what Josie just said, and we can look back to the transcript to get the exact wording here.

What about other Board Members?

Member Beach: I agree with that.

Member Field: I think the concern with the wording has already been pointed out, so I think we know what needs revised. I think Tim does.

Chair Roessler: Okay. And how about --

(Simultaneous speaking.)

Member Field: So I'm all for it.

Chair Roessler: How about Loretta?

Member Valerio: I agree with that. I agree that it needs to remain open until it's revised.

Chair Roessler: Okay. Then unless somebody

objects, I think we can move on to Finding 5.

Mr. Barton: Okay. I assume everybody can hear me.

Finding 5 is going to be a little bit different in that it does actually discuss explicitly assigning co-exposure values to a period where there was an identified gap by NIOSH in RPRT-90, and this is the subject of Appendix C for assignment of intakes of radioiodines.

So Finding 5 is related to that, as well as Observation 6.

So because they're sort of tied at the hip, I'd like to do both this finding and Observation 6 sort of as one issue of really the question of, how do you reconstruct the doses to radioiodine?

And again, this is sort of a different question than what we've been tackling so far because, whereas RPRT-90 is very limited in scope about how co-exposure estimates might be developed and the feasibility of that, in this case, it does explicitly come up with a method for iodine.

However, I'd also like before we really dive into this too deeply, I'd like to pose a question because it sounds like a lot of this, especially as it relates to co-exposure modeling and IG-006, and all that, which post-dates RPRT-90 and our review of it, does NIOSH intend to reevaluate its radioiodine exposure model for the identified gap going forward in any future TIB or what have you, or is this the method that's on the table, and regardless of the acceptable of IG-006, the co-exposure modeling guidelines, it would not change?

Or will this change? Because I think that's really going to have a big effect on discussions today.

Dr. Taulbee: It could change. Let's put it that way.

There are some areas that we are continuing to look for some data that we believe exists, or potentially exists, and reaching out to the site, you know, from that standpoint.

So, is this the, you know, absolute final? No, it's not.

And this was proposed before as, Bob, you correctly pointed out, before the whole co-exposure modeling implementation guide was finalized.

And so, kind of in light of that, yeah, I would say there would be a bit of tightening up associated with this, but I think it's important to go through, you know, what your concerns are and outline everything because it really kinds of puts it all on the table and brings everybody up to the same speed and the same level of knowledge here.

So I think it's important that we go through all of this and we hear what you have to say, and our responses that we've had to date about that so that we've got a good path forward.

Does that make sense?

Mr. Barton: It does to me, and if it makes sense to the Work Group, I will continue.

Member Beach: I think that makes (audio interference).

Mr. Barton: Okay, I think that was an affirmative. Gen, I saw you pop on, and I think you might have stayed on mute.

But without further ado, okay, so again, we're talking about radioiodine here, and it's important to remember that there is an SEC up through mid-1955, and RPRT-90 identified that there's a gap in any sort of personnel radioiodine monitoring essentially from

1955 through 1962.

And so, what RPRT-90 did is they constructed a dose reconstruction method for the unmonitored workers during that gap, and again that's '55 through '62.

What RPRT-90 does say is a coworker -- because again, this is before we adopted co-exposure -- a coworker analysis based on thyroid monitoring between 1944 and 1954 could be used to assign internal exposure to potentially exposed individuals during the period from 1955 to 1962.

So again, the idea here is that you're using earlier data to reconstruct a gap that appears between '55 and '62, or essentially the end of the established SEC period through 1962.

That's when routine monitoring data for iodine are not available, and that quote can be found on page 111 of RPRT-90.

So just to briefly go through some of the specifics so you all understand sort of the components of this, the method that's on that table, it does use thyroid measurements, which is basically you take a GM probe and you measure how it's coming off of the thyroid, and there are ways to convert that to a thyroid burden, which then you can revert back to an intake of iodine using certain assumptions.

So there were about 230 thyroid measurements that were identified in RPRT-90. That's 230 total measurements among 147 identified individuals, and those actually span from 1945 through 1957.

Not every year in that range, but the first year in which NIOSH found thyroid measurements was 1945, and the last year was 1957.

Just under half of those are associated with what is

assumed as a chronic exposure to radioiodine in the 1947 to 1949 timeframe, and the rest were evaluated as acute exposures, and I think this is sort of indicated on the slide that Dr. Hughes put up where they compared the 95th percentile derived intake rate for radioiodine, and then there were a bunch of blue diamonds that showed evaluated acute exposures in relation to that chronic intake.

And just as a clarifying question, because it did get a little confusing for me, I know in our original review that chronic exposure -- in other words, that line that was being proposed for unmonitored workers, again, during this gap period, I think it was derived from just the chronic exposure data from 1947 to '49, so not the full '45 to '57, but the co-exposure intake value was developed based on data from '47 to '49.

Is that correct? Can I just verify that that is correct?

Because that's one part of this, though it does not necessarily affect the entire question of how do we deal with radioiodine?

Dr. Hughes: Based on my understanding of how the calculations were done, yes, I do think that's correct, but if anyone from ORAU can correct me if that's wrong, then please do so.

Mr. Guido: Yeah, this is Joe. That's correct. That's the period that we had data that we consider to be routine monitoring. We didn't want to use any data that was related to acute intakes or incidents for that purpose, so that's kind of why the timeframe's laid out like that.

Mr. Barton: Okay.

So, really what the co-exposure approach put forth in RPRT-90 is for the gap, which is from 1955 to 1962 when you really don't have a lot of personal

monitoring data -- is to use a chronic intake derived from thyroid measurements from 1947 to 1949.

So that's an important thing to keep in mind as we sort of go through here.

But the key question is for consideration by the Work Group -- and this Finding 5 and Observation 6, which we'll get to during the course of this discussion -- and the real question is, is it appropriate first of all to use the older thyroid monitoring data to forward-extrapolate to these potential exposures in the '55 to '62 period, first of all?

Because again, you're taking data from one earlier period and deriving a chronic intake and applying that to a later period for which you don't have that personal monitoring data.

And so, a lot of these questions that we have really go back again to IG-006, which post-dates RPRT-90, post-dates the methodology put in here, and so a lot of these questions I think are important to put on the table for discussion, as Tim said, so that we're all sort of on the same page, and a lot of them will likely be or should be addressed in any future TIB addressing how all the exposures from these different radionuclides, including radioiodine, are going to be reconstructed.

And the things I think you really have to consider when you're talking about extrapolating data is what were the operations and conditions between the two periods in relation to the exposure potential?

That is, what was going on in that period where you have some data that you're deriving an intake value, versus what was happening in that later period where you're trying to apply that derived intake value?

And of course, this gets to a weight of evidence

argument.

And the Finding 5 here, again, one thing to remember is radioiodine, there's really two what I would call major sources at Oak Ridge. There's the RaLa program, radioactive lanthanum, which produces radioiodine as a byproduct because they're essentially taking freshly irradiated fuel and trying to pull off the barium and lanthanum for use in weapons experiments, but also they produced a lot of commercial iodine at Oak Ridge, and those two operations overlap some, and the magnitude of each operation, the RaLa and the commercial operations differed between the different time periods.

So what this Finding 5 was pointing out was that RaLa actually went up through 1956, so '55 and '56 are the first two years of that period for which we don't have data.

Fifty-six was actually the last year in which they did the RaLa at Oak Ridge. In October of that year, they moved it over to Idaho and continued the production there.

So again, there's sort of two different source terms between these two eras. There's some RaLa that's happening in the unmonitored period, I'll call it, in '55 and '56.

Definitely some commercial operations during the entire unmonitored period, and there's RaLa operations happening during the monitored period, which he just verified is '47 to '49, as far as developing that co-exposure intake.

And so I said, and again the question is, how appropriate is it to use this older data as a substitute for the period when you don't have data?

And we think that you have to consider some of the

operations and conditions, which again may be outside the scope of what RPRT-90 was trying to accomplish.

But again it's a weight of evidence argument, but whether you can use this older data for later time periods, for the most part, a weight of evidence argument.

And to sort of illustrate that, I just want to show one portion of RPRT-90 where there was really three main, I call them facets, or points put forth to say, these are our weight of evidence of why we feel, or why NIOSH feels that the data in '47 to '49 would bound the intakes for this unmonitored period, which was considered '55 to '62.

So let me just pull that up real quick, and I pulled it out so hopefully it will go quickly. And we'll see if I can -- one moment here. Okay. Yes, right here.

So this is from Attachment C of RPRT-0090, and again, Attachment C was the radioiodine section. Let's move this out of the way.

So you can see these numbered -- sort of numbered points here is that the intake value that was derived from '47 to '49, they put forth these three arguments, and really it's four arguments when you get to that first sentence in the bottom paragraph, but essentially they said, all right, let's take our derived intake and let's compare it to any sort of urinary excretion values that we see.

Now again, there's not many at all during this period of unmonitored exposure, and really not many more afterwards either, but the ones that were found and they compared to, they found that if you used the co-exposure intake, again, derived from thyroid monitoring in the '47 to '49 time period, you have more than an order of magnitude greater projected

urinary excretion rate than what they've seen in the limited data that is available.

A similar comparison was done for whole body counts, which was as noted in Lara's presentation, I think they first started making measurements in the whole body counter some time in I think it said '60, but not necessarily for iodine, and only then in a limited capacity in '61.

It starts to ramp up in '62, and then increases again past that into '63 and '64.

And we'll go through each of these because I think these are the important facets to look at when, again, we're evaluating how appropriate is it to use these '47 to '49 data for this latter period?

So you have a comparison with limited available urine data, limited available whole body count data, and also this third one here, is they compared it to what were the air sampling limits in place at the time?

And so, if you take those intake values that were derived again in the late '40s, you get a projected air concentration that NIOSH calculated as nearly a factor of two greater than the operating level that was used at the facility during that unmonitored period.

So those three are really the three main points as to why NIOSH believes their approach is bounding those potential exposures, and then again this fourth line, and this goes to the amount of iodine processed between the two periods, and this is '55 through '61 -- I believe that should be '62 -- but they note that from '55, the unmonitored period, it was between 1,000 and 3600 curie per year, whereas in the earlier period, you're looking at more like 9,000 to 42,000, so, I mean, you have almost a factor of ten increase in that.

But again, we feel like there are certain issues with that comparison, which we'll get into each of these as I go through this presentation, but I think you want to keep these in mind as we go through, that these are the four I guess bases that, as I said here, support the application of the intake for the unmonitored workers in that period when limited to no data exists.

All right, so I'm going to pop back here to my presentation. And where did it go? Okay, here we go. Okay.

So moving along, so that first finding was really about the releases that we pointed out and that the 1956 releases were reported to be close to 67,000, and the 1947 releases -- and then again, these are the maximum releases based on a specific source term.

So in 1956, this is talking about the Hanford slugs that were being dissolved at the site for RaLa production. The 1947 value is a different source term. I believe that might be the graphite reactor.

So we don't have these values on a year by year basis, we only have what was the maximum year of, again, stacked releases, and this is reflective of the RaLa operation, not the commercial operation.

I'll do the source term last. I want to get into -- because the source term goes right to Observation 6 as well, so if we can leave that one alone for a second and focus on the other three, which were the bioassay comparison, the in vivo comparison, and the air concentration guideline comparison.

Okay. So if we start off with that bioassay comparison, so Table C-7 in RPRT-90, it gives a number of iodine urinalysis samples, and they're limited in this unmonitored period. The only years that have them are 1958, with two samples, 1961

with one sample, and 1962 with five samples.

So you really only have eight urinalysis samples during this unmonitored period for which to compare with the co-exposure assignment.

And even in the years immediately afterwards, 1963 and '64, you only have three samples and four samples.

And I'll note that again there's two different source terms here, which we'll talk about later, but I think really what I just want to point out here is that when you start making these comparisons to these routine or the urinalysis samples that you have for iodine, they're very limited, so I think that needs to be taken into account when you're forming this weight of evidence argument, and should be also considered by the Board how much these weight of evidence support the proposed co-exposure model.

So they were comparing it to the highest -- that bullet that I showed that was in the NIOSH report -- they were comparing it to the highest routine sample, which actually occurred in 1966, November of 1966.

So the urine sample they're comparing it to is outside of this unmonitored period by four years, four or five years.

And as we'll note, this is during the year in which there's no RaLa anymore because RaLa ended in 1956, not 1966, and commercial iodine also dropped significantly during that year, so again, you have to think about whether these comparisons are really reflective of what could've been experienced by workers during this unmonitored period from '55 to '62.

The actual highest observed sample was not considered a routine sample, but was an acute

sample, presumably in response to an incident, and that sample, the highest acute sample, that was in June of 1967 that was identified, and again that's outside of the period with no monitoring data, and that one was a factor of 130 higher than what was projected based on the co-exposure estimate, and again that was probably an acute sample.

So again, that may be apples to oranges in a certain sense, but again, this is all part of weight of evidence, so when you start comparing it to certain urinalysis results, you have to consider how many do we have, and is that a sufficient number to be able to make a meaningful comparison?

So our position on this is that, you know, again, it's comparing one data from one period to another.

You want to establish the similar working conditions. Again, this is a lot of concepts that were codified in IG-006, and so it's not that surprising that these topics weren't necessarily evaluating discussion RPRT-90.

However, again, as we approach this with the discussion about feasibility -- which has been clarified today, and I think that's very helpful -- it appears that a lot of these concerns would not be captured by RPRT-90, should not be captured by RPRT-90 based on the clarified scope of the document, but are valid questions to ask when you're talking about co-exposure, and that's exactly what RPRT-90 did for radioiodine, even though it was before IG-006.

So as Tim pointed out, some of it may change, some of it may be tightened up based on those new guidelines. But again, I point out that the bioassay comparison is you only have eight bioassay samples during that unmonitored period of eight years, from '55 to '62.

There were comparisons made to urinalyses outside of that time period, but again you had no RaLa operations going on anymore and commercial operations were significantly decreased by '64, I believe.

But we'll see that. That's provided in a graph later on when we talk about source terms.

Having said that, one thing that NIOSH provided in their response is, you know, what indication does SC&A have that if there was urinalysis data during that unmonitored period that you would have much higher values than what is established by the co-exposure model?

And we obviously have to agree. If there was evidence of that, I mean I don't think we'd even be having this discussion.

Because if there was evidence the exposures were higher, then, you know, it would completely invalidate any co-exposure approach from a different time period.

But I think the important point there is that there's a significant lack of data to be able to meaningfully say what all those exposures were during the unmonitored period.

So we don't have any evidence that they would not be bounded by the co-exposure approach, but that would really be -- again, it's proving a negative, and I think what we want to point out here is that the data itself is very sparse, so that and a lack of evidence is not necessarily an affirmation of different exposure levels.

So that was the bioassay comparison. There's similar concerns with comparing it to in vivo. Again, the whole body count didn't appear until 1961, so the

second to last year in this unmonitored period, and they really didn't ramp up the program until 1963, so in 1961 I believe there were about 100 in vivo results.

In 1962, the next year, it was more like 400. ‘63, you're up over 1,000, and by '64, you're more up around like 1,500 a year.

So you can see there's a ramping up period which spans both the last two years of what was established as the unmonitored period in RPRT-90, and really the ramping up of the in vivo program, which seemed to be hitting its stride by 1964, which again, the period that this co-exposure estimate was intended to assign doses to was '55 to '62.

So again, the question is how relevant is this whole body count data for the period prior?

And now again, there were a couple in '61 and '62, but we have nothing prior to that in the unmonitored period with which to make any comparisons. RaLa had ended in 1956, so again they're not really reflected of that source term.

You know, and a lot of these questions again, the in vivo data, we only have it in essentially summary form, so what do we really know is how many individuals measured? At least that was the references presented in RPRT-90.

We know how many individuals are measured, and how many had a measurable radioactivity that wasn't associated with some of the two standard elements that you'll find in in vivo, which is potassium-40, K-40, and caesium-137.

And what we don't know is how many folks who worked with the radioiodine were included in that in vivo program, you know, what percent, and not only

how many, but what percentage. And the documentation really only suggests that the in vivo program was directed for its investigations of known or suspected internal contamination exposures.

So there's a lot we don't know about those in vivo results, and as I said, they were very limited. Very limited for the time period that we're discussing because they didn't really start measuring iodine until 1961, and then only a very limited number of people, then it started to ramp up from 100 in 1961 to basically nearly 1,500 in 1964.

So there's that ramp up period that starts in the unmonitored and goes into the subsequent years. And so again, NIOSH's response is basically, you know, that wasn't the point of looking at this in vivo data.

The co-exposure estimates are done using the thyroid monitoring, and again, it's the thyroid monitoring from '47 to '49. And this was just for a comparison, again, to build a weight of evidence that the method proposed would be bounding compared to the limited data that you do have in the '60s.

So we concur in the context of that comparison purpose. Again, some of these comments are not entirely relevant to RPRT-90 anymore now that the scope has been clarified. But again, this is all about when you're extrapolating data from one period to the other.

In this case, we're forward extrapolating the exposure data from '47 to '49, forward into '55 to '62, and now the comparisons are using in vivo data after the period, so that extrapolated through the co-exposure derived thing.

So there's a lot of caution that we believe must be taken that I think is outlined in IG-006, so I think a

lot of these concerns probably would be addressed in any future TIB, again, identifying what the co-exposure method is going to be.

And so the third one was again a comparison to the air concentrations, and just like in vivo, these come from annual HP reports that reported the total number of air samples per year. Not any more granularity than that, and there was no information on the location, or even the magnitude of the individual samples.

And in NIOSH's response, again, it's like, hey listen, this was just for a comparison and a weight of evidence comparison to our proposed co-exposure guidelines.

And the comparison isn't to the actual measurements. We don't have those. It's just to the allowable operating levels.

And what SC&A had pointed out is that based on the Site Profile, the operating levels indicated that we're actually 50 percent higher than what was projected from the co-exposure estimate, which NIOSH said, well no, actually the Site Profile is an error, and what it was recording as that operating level, and so that would have to be corrected to the lower level, which would then conform with the NIOSH's conclusion that the co-exposure model is actually two times higher than the operating level, as opposed to the operating level being 50 percent higher than the co-exposure.

Moving on. So again, our point here is that it's important to evaluate what the actual air concentration measurements were, so we may know how many there were in a year, but where were they?

Are they general air samples, are they breathing samples, are they in the iodine production years? What were the actual measurements in the iodine

production years?

Were there certain job locations where you were regularly, you know, close to the limit, exceeding the limit, and what have you?

And we don't have that granularity right now, and again, that might be something that would have to be explored again going forward when evaluating co-exposure methods against the IG-006 criteria.

But also, this notation that the original Site Profile was an error when SC&A pointed out that, well listen, your Site Profile has an air concentration that's actually higher than what would be projected by your co-exposure model, and NIOSH said, well that citation is actually an error, and they provided their reference for it, and we're going to look at that in a second.

We don't believe that the original Site Profile was actually an error, and you'll see in a moment why. And this comes down to whether the limit, again, for beta gamma, an error was one times ten to the minus eighth, or was it three times ten to the minus eighth? And I think that's going to be enumerated on the next slide. Yes.

So this was the record that was provided in NIOSH's response, and here's your max permissible values, and as you can see, you have the type of contamination. The air concentration, one is without masks, one is with a filter type masks, with a gray canister, and one is with positive air supply masks.

And then you have the permissible levels in that second column, and as you can see, for beta gamma, it just says ten to the minus eight.

And in the Site Profile, we recorded that it's three times ten to the minus eighth, which I believe is

correct because if you look at this, this might've been just a convention of the day, but I believe that three times ten to the minus eleventh -- the three times I believe was intended to be repeated all the way down to all those other entries, for the mask entries and the air supply, positive air supply entries, because if you think about it, with respiratory protection, it offers you a protection factor against particles.

And if this table were to be taken at face value -- in other words, that three times ten to the minus eleventh is only applicable to alpha without a mask, that would be indicating that you put on a mask and it had a different level of protection for particles containing alpha radiation than particles containing beta radiation.

And what we're looking at here is a protection factor of about 1,000. So –-

Mr. Guido: Bob.

Mr. Barton: Yes?

Mr. Guido: I want to interject here because there's another version of this same table that's in 109-500, and it actually has a line between the three times 100 -- it basically splits the three times ten to the minus eleven, and the one minus eight, and it splits each of those into separate cells, so that's a little bit clearer.

And also, the 1957 applied HP annual report specifically identifies the gross data actual level is one times ten to the minus eighth.

So you know, those are additional references, but when I put this response together, I just figured it would be -- you know, it would make sense that you wouldn't carry that three over, so I understand maybe we should've provided a little bit more clarification back then.

But I think the weight of the evidence does show that it is one times ten to the minus eight, and I can provide the -- but the best reference is actually the 1957 applied HP annual report where they discuss a release that occurred, and they specifically say that that release is greater than the administrative, you know, control limit of one times ten to the minus eight for beta.

And then the alternate version of this same table comes from a training document around that same period, and it's a little bit different.

I think it's a little clearer, although, you know, looking at what you're saying, it's not perfectly clear if the three would carry down, but it is split, the cells are split, and that's in SRDB 109-500.

Mr. Barton: I'm looking at this, and maybe this table is an error in the Sadowski reference from 1953, but I mean if you put on a mask, your permissible level goes from three times ten to the minus eleven just to one times ten to the minus eighth?

I mean it doesn't make sense that if you start at three times ten to the minus eleventh, your protection factor from a mask is not going to be decidedly different for alpha than the protection factor from the mask for a particle that has beta radiation.

I mean --

Mr. Guido: Yeah, I don't dispute what you're saying, I'm just saying what the documentation, it's pretty clear from the documentation that the limit was one times ten to the minus eight for beta.

I can't comment on the other, although you could look at that -- if you look at that training document, it does provide some clarification of those intermediate values that might help.

I mean I think all of this is not super important for this discussion, but just a point of clarification.

Mr. Barton: And that's fair. If there's a better reference out there, that certainly clears this up.

I mean I believe this was provided in NIOSH's response, which is why we put it up here. And maybe this is an error, or perhaps it maybe even changed between 1953, and you said the other one was a 1957 document?

Mr. Guido: Well the training document is of the same era because it's the same table, so that one is also a '53.

You know, that's the training they provided on this same thing, so I think that's the same era as this, and you know, I mean if we look closer, we might be able to find other citations that identified the gross beta limit. It's just the easiest one here, you know, the one I saw that was glaring was in that 1957 report.

But I don't believe this represents a change, although, you know, anything's possible. But like I said, if you take a look at that 109-500 reference in the SRDB, that training document, and see if that helps. I definitely would be –- I think it's fair for us to update RPRT-90 with some clarification here.

You know, on my side, I just thought it was pretty clear that you wouldn't pull the number three down from what I've seen from ORNL's documentation. But you know, anyhow, I think we can clarify RPRT-90 as far as the, you know, where this number comes from.

And then of course, there's an issue with the TBD revision eventually, so that number isn't used anywhere in the TBD, so it's kind of an innocuous value, but you know, it does need to get updated at

some point.

Mr. Barton: Okay. And certainly as the discussion goes forward, and if this method is going to be either modified, tightened up, or what have you, that'll certainly be important.

It makes sense to me that based on this reference, you would pull the three down because you would have the same protection factor between alpha and beta. And that's the reason for that. But as you said, it may not be necessarily that important at this stage of discussions.

Again, this just all goes back to one of the points of weight of evidence, one of the three points on whether the proposed approach from '47 to this later period is bounding, which is a separate question entirely really of whether it's appropriate to do that before you evaluate that sort of approach under IG-006, which of course, we keep repeating it.

It wasn't relevant to RPRT-90 during its formulation, but certainly will be relevant moving forward. So we've gone through the bioassay, the in vivo, and the air concentration comparisons, and sort of the fourth one was the comparison of source term, or throughput of iodine as another basis for why this is bounding, and this moves to observation 6, which I'm going to skip ahead.

Unfortunately I did not have the foresight to put that right after this in the presentation, but I'm going to skip ahead to slide 56, which is observation 6, for those of you who don't have Zoom up and are following based on the PDF.

And this again goes back to that comparison of, well, how much iodine do you really have between those two eras, the '47 to '49 and '55 to '62, as a basis for, again, using that data for co-exposure, and again, it

doesn't apply to the IG-006 criteria yet, which would have to be evaluated, but in a way, it's almost akin to not only the co-exposure guidelines, but also since you're extrapolating data you have to consider whether it's an appropriate surrogate -- and I know that's another loaded word -- that we usually restrict to comparison of data between different sites, but in a more restricted sense, was what was going on during that earlier period reflective of what was going on during the period for which we really don't have any data to use.

So and again, in what I showed earlier directly from RPRT-90, it was stated that, well, in the earlier period, you had between essentially 9,000 and 43,000 curie per year in process, whereas in the unmonitored period, it's down to around 1,000 to 3,000.

And as we point out in our review, we believe that's really an apples to oranges comparison because you're looking at what's presumably the RaLa stack emissions based throughput, which was primarily done in the '47 to '49, to this later period, which the first two years have RaLa, '55 and '56, and then the later years -- or the entire period has commercial in varying extents, which we're going to look at under observation 6 here.

So, to that end, I do want to switch here real quick to one other excerpt from one of the reports, so give me one moment here.

So when we talk about radioactive lanthanum, which was one of the source terms -- the other one was commercial -- there we go.

What SC&A had pulled from, and this goes back to the Finding 5 discussion, the source of Finding 5 is that these are the releases -- iodine releases from a

site, this was put together as part of really an analysis of the effects that might have happened to off-site populations from the release of iodine, and as you can see here, and we pointed it out in Finding 5, the maximum curies released from the RaLa process was in 1956, this time period when you had very little to no monitoring data.

And the next highest one, that is in that period where you have the monitoring data that's being used or proposed for use during the unmonitored period, and it's slightly lower, as NIOSH pointed out.

But I wanted to show really where this data came from that was the source of Finding 5, and really the gist of this goes back to source term because the source term and the throughput and the methods and the production really calls back to exposure potential. Are those sufficiently similar and/or bounding?

So I'm going to head right back to and continue the presentation. Sorry for jumping around here a lot, but I think it's important to see a lot of these things.

Okay, and NIOSH's response, and again, it's a summary. If NIOSH wants to jump in, please do.

But NIOSH agreed that the RaLa production processes and commercial production processes are different, but argues that, you know, these activities were done in the same areas with the same radiological conditions, and ultimately concluded that it was unlikely that in -- during this unmonitored period, were exposed to levels that would have triggered the monitoring program.

And I guess my initial comment on that is that, I mean, RaLa was done in building 3026D, as is pretty well documented in the Site Profile, the previous Evaluation Report, and various other sources during that period of interest. And commercial was done in

a completely different building, 3028. I think you can actually see that on one of the maps that Dr. Hughes put up earlier that showed essentially a schematic of the site. They're different buildings and, you know, they may be 100 yards apart, you know, like a football field, but they are different buildings, so I'm not sure it's the same area necessarily.

And the same radiological controls, and I think that inference is made because it was the same site, the same general HP program. But again, that's an assumption that I'm not sure was, you know, fully fleshed out in RPRT-90, to make that statement.

And then, you know, you really have this other statement that it would be unlikely that the workers were exposed to levels that would have triggered the monitoring program.

And again, we have IG-006 up here, and I know this is probably frustrating to a lot of people on the phone because we really reviewed this as if it was a complete feasibility study for dose reconstruction, when in reality for the most part it was a feasibility study of bioassay methodology and whether it can measure the different source terms on-site.

However, in this case, it actually for radioiodine, it is an actual co-exposure analysis, so even though IG-006 came afterward, a lot of the same concepts do apply and obviously moving forward, IG-006 would have to be used to evaluate any sort of co-exposure estimate.

And furthermore, I mentioned they're in different buildings and that, you know, as we'll look at in a second, the commercial side, not the RaLa, but the commercial is decidedly different in '56 to '62 when you compare it to '47 to '49, so just purely commercial operations, they're very different.

There's a much higher source in this unmonitored period from commercial operations than in the '47 to '49 period, which there's a chart in here that I will show shortly that exemplifies that, but furthermore, the statement that, you know, it was unlikely that workers during this unmonitored period would've triggered the monitoring program, we really can't evaluate that because of the limited monitoring data that we have, personal monitoring data, and we can't justify that necessarily as a reason to apply the co-exposure estimates for 1947 to 1949 because I think that data is simply lacking.

So to say that they definitely would've triggered the monitoring program, that may be true, and those records may be out there that we don't have, they may have been destroyed, or maybe the situation didn't exist, but there's a wealth of possibilities beyond just the exposure levels were low enough that they wouldn't have triggered the monitoring program.

I guess that's my point, and again, it's not that we have proof that the exposures were decidedly higher.

If that evidence was out there, we're not having this discussion, but I think it's important to point out the limitations on the information you do have when you make inferences in the weight of evidence of applying, again, data from this earlier period to the later period.

And so, as I said, here's the production of commercial radioiodine, and so what I pointed out here is you have that '47 to '49 period, and you're down, it looks like below maybe 250 curie in a given year, and that was 1949.

And then you move forward to, again, this is the period, 1955 to '62, which is just after the currently

established SEC Class, and you see commercial operations are significantly higher.

So, one, we felt the comparison which for the earlier period was looking at RaLa estimates, comparing it to the commercial estimates in the later period, we feel that's apples and oranges, and that was the gist of observation 6, in that to really make that jump to see if it's representative of this later period, you have to do some sort of comparison, not just the total amount of curie, but are they different?

Are there different processes taking place between RaLa production and commercial iodine production? Because one thing to consider is, you know, RaLa -- the iodine is essentially a waste product.

In fact, there's a quote in the TBD directly about this, the RaLa situation, that says, much of the iodine volatilized during the slug dissolving process was effectively removed from the dissolver off-gas stream by the reflux condenser and chemical scrubber in line before the gaseous waste went to the stack.

So it was really pulled out via vacuum and scrubbed through, and then released through the stack as more of a waste product, and the TBD goes on to say that liquid waste from RaLa operations, they went to the local tank farms.

So as opposed to actually trying to take radioiodine for commercial purposes, where that's a product, in RaLa, it was really the waste stream.

And so, we felt that a discussion of the different exposure potential between the two different operations was warranted to be able to make that extrapolation.

And of course, that is a concept that is really sort of outlined in IG-006, so again, that may be something

moving forward that wasn't really considered during the formulation of RPRT-90, which is understandable.

I put this slide up here because it was just unclear to me, because I know we've run into it at some other sites, and maybe it's restricted to residual periods, but you know, in general, my impression was that commercial operations aren't usually covered by the program, whereas obviously the RaLa operations are a covered activity, so those are certainly relevant.

But I was curious and I was looking for some guidance, either from Dr. Roberts, or perhaps Tim at NIOSH knows -- is this really a source term, the commercial side, that needs to be considered?

Now, a lot of the times, if you can't differentiate between what's commercial and what's non-commercial, then you have to consider the whole thing as non-commercial, and I understand that, but given the magnitude of the operations, especially the commercial operations, as the main driver of radioiodine separation, I guess under the statutes, how does this apply?

Because a lot of these concerns are about, again, as I just enumerated, comparing commercial operations to this as a RaLa production, and maybe that's not even a relevant discussion that needs to happen.

So I'm looking for clarification on how this is interpreted.

Even though they were commercial operations, are they considered -- you know, the DOE operates, but it's just for commercial purposes, are they simply considered with all the other iodine sources because they can't be separated, or is that really considered a separate source that's not actually covered or should be covered by this program?

I put this slide in here because it wasn't clear to me, and, you know, just the term commercial operations sort of raises the question in my mind, so I guess I throw out that question. Do these commercial operations really matter? Because again, on the previous slide, if you're comparing the actual production, this unmonitored period is significantly higher than the period where you have the co-exposure data being composed.

Dr. Taulbee: This is Tim. I'll take a stab at this, and please others can speak up if I say something in error.

But from my understanding, if the exposure is occurring on a DOE site that is a covered facility, it is included, with the exception of if it was from the Naval Nuclear Propulsion Program.

And so, these exposures are actually covered from that standpoint because it was done within the confines of the ORNL site.

Mr. Barton: Okay. Thank you.

Dr. Taulbee: That's my understanding.

Mr. Barton: Okay.

And it's sort of implied by the fact that a lot of discussion before 90 is about the magnitude of commercial operations, but I just wanted to make sure that that's clear, and we're not going down a discussion that may be irrelevant in the end. But I think that does clarify it, so thank you for that.

And let me just back up again to our sort of conclusion slide about radioiodine. And then I'll certainly open up the floor to any questions or discussion of what the path forward is.

And again, this is kind of jumping back to Finding 5 because we're talking about the different source terms and what's the magnitude, and we do agree that when you compare the highest years with stack emissions, it's pretty small.

NIOSH points out that it's a four percent difference between the highest that was seen for the Hanford slugs, which was in 1956, and the highest that was seen for I believe it is the graphite reactor slugs in 1947.

The one thing I'll point out is that 1956 is the year in which RaLa process ended, and it ended sometime in October based on the documentation, I'm not sure when. And the two comparisons were between the total emissions.

So 1956, you had emissions only going through October, but it's reporting the total. So the difference of four percent, if we start to think of it in terms of a rate rather than a total, it would be a little bit more than four percent, and so you'd have those extra two months if you were going to try to scale up to a rate per month during '56. It's not as small as four percent.

It's going to be more like, you know, 24 to 30 percent, somewhere there, depending on when you assume that the RaLa production actually ceased at ORNL, which again was sometime apparently in October of 1956, so the rest of 1956, no more RaLa, and no more RaLa after that. But again, the uncertainty still exists, and if you accept that you can use data from one earlier period to the other, you still have to use a lot of care to extrapolate, to ensure that those exposures are going to be bounding.

So first you have to decide whether it's appropriate based on the conditions, and then I think that it's

prudent that you sort of go the extra mile to ensure that any estimates you put out there are going to be bounding.

And I throw out this example because it's actually in RPRT-90 that -- and this goes back to the source term discussion that occurred just prior to the radioiodine discussion, in which a fraction of the source term was assumed to be inhaled, and during that methodology NIOSH had added, as far as I can tell, a somewhat arbitrary factor of ten in that evaluation just to ensure it was conservative.

So that's the sort of policy base part of this that, you know, if we can determine that it is appropriate to extrapolate data from this earlier year to the later years, I think you have to be very certain that you are going to be bounding those exposures.

And again, sort of the four main points that were put forth to support that it is bounding, there are, when you flesh it out a little bit, you know, the data are still scarce, especially for bioassay, the data are non-existent for in vivo until those last two years in the '60s, and I think there are still some questions about the comparison to air sampling, although as Joe Guido pointed out, perhaps those can be solved with examination of a few other references to really pin down that number, especially in what timeframe.

What timeframe were these different air sampling levels in place?

And of course, that also begs the question of, you know, where were those samplers, and all those other questions that would fall under the IG-006 evaluation process of co-exposure model.

So that ends what I have as far as radioiodine at this point. There are certainly some concerns.

I think really what the Work Group should weigh -- and I'm not sure that this is going to be solved today because as was pointed out, this method was put together before we had IG-006, so it really needs to perhaps be, you know, colored in a little bit, about these different evaluation criteria, such as representation, the completeness of the records you're using to try to apply.

And then another kind of strange point here that -- it's not on this slide, but it certainly occurred to us at SC&A, was that, you know, '47 to '49 is part of an SEC period that it was determined that internal dose reconstruction was infeasible, and that included fission products, which obviously iodine is a major fission product.

I mean, that's the reason it's part of the RaLa production process, is because you're using freshly irradiated fuel.

So you're taking data that was ostensibly deemed inadequate for internal dose reconstruction in '47 to '49, and applying it to a later period, and saying that for that later period it's feasible, but for the earlier period, it's again, ostensibly infeasible.

So that's another kind of facet to all this on evaluating the degree to which this method that's put forth represents the feasibility of dose reconstruction for radioiodine, and again, specific to this period established where there's very little data, '55 to '62.

So I think that that pretty much sums up our concerns, and I certainly think it may be worth discussing today what maybe the path forward is, or it's entirely obviously up to the Work Group whether they want to close these out and wait towards seeing a modified co-exposure method, or keep them open until -- you know, I guess I just open up the floor

either to questions or comments, or what have you.

Chair Roessler: Bob, I think I'll start with a comment.

I think normally we'd have NIOSH respond in detail to all of your comments, but to me it seems like the amount of material you've presented and the complexity of it, that it really I don't think is possible to deal with in this Work Group meeting.

I'll see what NIOSH has to say, but it seems to me that this should be left open and we should give NIOSH an opportunity to go over it in detail and respond in writing, and then we should take it up at our next Work Group meeting, and I'll just throw that out.

Member Field: I'd like to, if possible, just hear what Tim thinks. I'd really be interested in the question too about '47 to '49.

Was it feasible back then to do dose reconstruction for iodine? I think that's an interesting question too. Maybe that’s (audio interference) you can respond briefly, Tim.

Dr. Taulbee: Okay.

Member Field: It's a lot --

(Simultaneous speaking.)

Dr. Taulbee: I'll give this a quick shot.

Well for one, let me start by saying that, you know, the co-exposure type of modeling will certainly be updated in the format of handling IG-006 for one thing.

So, that will certainly be updated, and I mentioned that, you know, kind of before Bob went through this, we'd be tightening things up, but the general

approach I'm not sure will be changing a great deal.

Keep in mind in that early time period when the RaLa was going on, we have a lot of thyroid monitoring data of workers in that time that we know were working with the radioiodine, or being exposed to radioiodine at that time.

That was why we chose that particular group.

So, whether it applies, and you know, we believe it applies to the latter time period because it is bounding from that standpoint.

That's our main basis here, and Bob has pointed out some weaknesses associated with that that we intend to tighten up and will address in the future from that standpoint.

With regards to your specific question, Dr. Field, about the infeasibility of previously, you know, at that same time, in addition to RaLa, there was a lot of other activities that were going on there from the internal dose standpoint, and fission products was one of them that we had difficulty estimating the dose from.

Did we break out iodine specifically to say, oh hey, we've got data between '49 and '59, and say yes, we can do iodine? No, we didn't go into that level of detail.

We knew we had an infeasibility with the fission products in general at that time period, and so we didn't go into detail to exclude certain radionuclides.

So I kind of take exception to what Bob just stated of how can you use it from this standpoint.

That wasn't the intent under SEC 89. It was a general global evaluation from that standpoint. Does that

answer your question, sir? Thank you.

Chair Roessler: So Tim, what do you think about responding to this? Am I just overwhelmed by all that Bob mentioned, or is there some --

Dr. Taulbee: No --

(Simultaneous speaking.)

Dr. Taulbee: You're not overwhelmed at all because we are too.

But he raises a lot of really good points here that, you know, in light of IG-006, that we can go through and begin to address and fill in in an OTIB for a co-exposure model for iodine.

Iodine will be one of the radionuclides that we obviously address there in a full co-exposure model that is compliant with DCAS IG-006.

And so we will be developing that. So I guess in a sense, SC&A will have another crack at this after we've now heard what their concerns are with it, and hopefully we will address it all in that new -- or in that OTIB.

Chair Roessler: So I'd like to hear what other Work Group Members think, but it seems to me we should leave this one open until NIOSH has a chance to respond to everything that came up.

Member Beach: Yeah, Gen, this is Josie. I agree with that also, leaving it open.

Member Valerio: This is Loretta. I agree with leaving it open.

Chair Roessler: Okay, so I guess we should vote then.

I think that was a vote, the Work Group agrees that they should stay open and allow NIOSH a chance to respond, and take it up in our next Work Group meeting.

Dr. Roberts: Hi, Gen --

Chair Roessler: Are there any comments, other comments or questions for Bob while we're on this?

Dr. Roberts: Gen, this is Rashaun. I have received a request for a comfort break, but I wanted to check in with you and see if that's okay with the Work Group.

Chair Roessler: I think that would be fine. What do you recommend, about ten minutes?

Dr. Roberts: Sure. Is that okay for everybody, or do you need a little more time?

Member Beach: No, ten minutes works for me, thanks.

Member Field: Sounds good, thank you.

Dr. Roberts: Okay, so we'll be back at 1:05 p.m. Eastern.

Chair Roessler: Okay, thank you.

Dr. Roberts: Sure.

(Whereupon, the above-entitled matter went off the record at 12:54 p.m. and resumed at 1:07 p.m.)

Chair Roessler: Okay, so I think we have finished with Finding 5, and we're going on to Finding 6.

Now, SC&A, in your report, you say that this finding is subsumed under Finding 3, and you recommend closure. Is that what you plan on this one?

Mr. Fitzgerald: Yes, and just a quick background.

Basically in comparison with the treatment on the in vitro, for example, in Appendix A, we felt that there was very little to go by on the in vivo capability and NIOSH's Technical Basis Document.

I'm not going to go through this, but this is on the bottom of page 11 of our report in a footnote just citing some of the programmatic concerns that connect with the in vivo whole body counting program at Oak Ridge.

So, yeah, our conclusion is that certainly that needs to be treated, but that's something that ought to be addressed as part of the, you know, programmatic considerations that would go into the resolution of Finding 3.

So, we would recommend that, yes, we do believe in vivo ought to be given attention, and that can be done in the context of Finding 3.

Chair Roessler: Okay, then let's go --

Mr. Fitzgerald: That remains our recommendation.

Chair Roessler: Then let's go on to Finding 7.

Mr. Fitzgerald: Okay, Finding 7 is somewhat of a duplication of a scoping question that was raised I think in Finding 1. So here and again in terms of the scope of the RPRT-90, we're clear that it's not going to address D&D and deactivation, and the waste management, and that should be closed for -- you know, again, it's part of Finding 1, so we recommend closure of that.

Member Beach: Gen, are you on mute?

Mr. Fitzgerald: I believe Gen's on mute.

Chair Roessler: I was on mute, thank you, Josie. Yes, I made a motion for the Work Group that we recommend closure of Finding 7.

Member Beach: Yeah, this is Josie. I agree with that and 6, as well. I know we breezed right past that one.

Chair Roessler: Yeah, well, we kind of combined that with Finding 3, but maybe just to make it completely, we should recommend closing Finding 6, too, then.

No, I'm sorry, is that –-

Mr. Fitzgerald: Yeah, certainly on Finding 6 we wanted to emphasize that we do believe that in vivo needs to be addressed, if nothing more than the experience we had at Los Alamos.

It really, in terms of how it applies and what the implementation is, that should be part of the assessment that we talked about in response to Finding 3.

Chair Roessler: You are not recommending closure of Finding 6, then?

Mr. Fitzgerald: Only that it's subsumed by Finding 3.

Finding 3 actually does cite in the finding statement in vivo, so if anything, this provides emphasis, but it could be considered part of Finding 3 as well.

As long as the Work Group and NIOSH both understand our concern on that one, I think it can be handled by what Tim has offered as, you know, the revisions.

Chair Roessler: So perhaps we should restate the motion then on Finding 6 that we recommend closure of Finding 6, since it is subsumed under Finding 3?

Mr. Fitzgerald: I'd be fine with that.

Chair Roessler: Okay. Work Group?

Member Field: That sounds good, Gen.

Member Beach: Yeah, Gen, I agree with that. Thank you.

Member Valerio: I agree with that too, Gen. Thank you.

Chair Roessler: Okay, so then I think we took care of that, so then we go to Finding 7, and I think we, on that one, the Work Group agrees with SC&A's recommendation for closure of Finding 7.

Member Beach: Okay, just one quick question on that, and maybe to Joe.

Your paperwork says that Finding 7 is addressed in Finding 1, and we closed Finding 1, so I just want to make sure that we're still okay with the Finding 7?

Mr. Fitzgerald: Yeah, there's a little bit of duplication because I think Finding 1 is speaking to what in fact the scope of RPRT-90 actually is.

And this one speaks specifically to D&D and waste management, so to some extent the answer for Finding 1 does satisfy Finding 7, as well.

Member Beach: Okay, I just wanted to make sure I was clear, thanks. And I agree with closing it, Gen.

Chair Roessler: Okay. And I think we have the vote from the others.

So I think we're ready then to move on to the observations. We have, SC&A brought up six observations.

I think we've taken care of Observation 6, so then let's go back and start with Observation 1.

Dr. Buchanan: Okay, this is Ron Buchanan with SC&A and I'll be discussing the observations that remain.

And we see that Observation 1 was -- you want to go back one, Bob?

Okay, observation 1 was concerned with inventory discrepancy, and in that, I might expand a little bit in that we found in 72 and 73 and also 76, we were given an Excel spreadsheet that had the inventory in it, and we were comparing it with what was in the original RPRT-90.

However, we found that there was some additional information in RPRT-90 that we couldn't find in the spreadsheet, and so that's where we came up with this observation.

Okay, next slide, Bob. Okay, NIOSH responded that the inventory of the radionuclides processed by the isotope group was developed through a review of the published sales records, and that's what we were referring to, and that's the spreadsheet we were using.

However, NIOSH updated that inventory sheet by reviewing logbooks, and this review resulted in additional radionuclides and years, which were used actually in the report finding.

Next slide.

So, we went through the spreadsheet and the inventory and we found the discrepancies that we had previously identified appearing in Table 7-2 and also 3 and 6.

We concluded that the additional radionuclides or years from the logbooks added to the X-10 inventory original spreadsheet would explain these discrepancies.

And we found that -- we did some spot checking and found that they agree, so next slide, Bob.

So, we find this observation's been clarified and recommend closure. Do you want to discuss each one of these afterwards?

Chair Roessler: Yes, I think we should, and I have a question of Rashaun.

On the observations, which according to my definition are things that have a minor effect or no effect at all really on things, does the Work Group still take a vote?

Dr. Roberts: No, I think you can just discuss, you know, the observation and see if anything remains to be done by NIOSH or SC&A.

Chair Roessler: Okay, so anyway, on Observation 1, SC&A recommends closure, so I think we can then move on to Observation 2.

Dr. Buchanan: Okay, Observation 2 was concerning the specific alpha-emitting radionuclides needs to identify for dose reconstruction. Next slide.

Okay, so what I need to clarify here is that the original X-10 bioassay cards, and that's what the bioassay cards are provided at the time the bioassay was taken, was provided by Oak Ridge for individual claimants and are the basis for the DR.

Now, in addition to that, there's an X-10 database, and that database contains some of the essentials for the bioassay cards, but not all.

It doesn't always identify the radionuclides.

And so, and NIOSH responded, it will not be used for dose reconstruction purposes, but only the original or

copies of the bioassay cards will be used.

So next slide.

So, our response is that considering NIOSH's clarification that the database will not be used for DR, and also the X-10 database only used for coworker intake modeling without further consideration of specific alpha-emitting radionuclide, and SC&A finds this observation's been clarified and recommends closure for Observation 2.

Chair Roessler: Okay, so then I think we can move on unless somebody has questions or comments on Observation 2.

(No audible response.)

Chair Roessler: I don't hear any, so go ahead.

Dr. Buchanan: Okay, Observation 3, okay. This is the trans-plutonium radionuclides, I need further analysis.

Next slide, Bob. Okay so, another way of explanation here is that the TBD for Oak Ridge identifies americium-241 as the default assumption for trans-plutonium, the TPO bioassay results.

Now, NIOSH's response was that 20 of these radionuclides detect a TPO method of 20.

Only two have a higher organ dose reconversion factor than DCF, and that is curium-248 and californium-249.

And so, considering the americium-241 inventory is much greater by orders of magnitude than inventory of either of these two radionuclides.

Next slide.

And so, SC&A analyzed NIOSH's response and considering these DCFs, which we looked through those and the inventory amounts from the TPOs, we find that americium-241 as a default radionuclide provided, you know, other information that's not given for the individual dose reconstruction to be a reasonable assumption, so we found this observation had been clarified and recommend closure.

Chair Roessler: Okay. So again, unless there are any questions or comments, let's move on then to Observation 4.

Dr. Buchanan: Okay.

Observation 4 has two parts, and this is concerned with use of gross alpha or gross beta counting data, which in Section 4 of RPRT-90, they list some codes and give you gross alpha or gross beta.

And so, unless you know the radionuclide, then you could have some underestimated assigned dose, and then what you use as a substitute.

Now, the two parts to this question, ruthenium-106 in particular -- so next slide -- we see that in Table 7-3, that ruthenium-106 was indicated as green, which it indicated that there was inventory bioassay method and bioassay data. When we tried to go back to the original source, we could not find any bioassay data for that period, for that isotope, and NIOSH's response was that this was an error in editing, and the revised Table 7-3 will show yellow, indicating for these years for ruthenium-106, indicating it was in inventory, there was a method available.

There was no bioassay data. So next slide.

So, we concur with NIOSH's response and agree that the issue can be resolved by changes in the next revision of RPRT-90.

That was the first half of Observation 4.

Observation, you know, the second part was that the X-10 bioassay cards are provided for the claimant on our basis for the DR -- this is NIOSH's response, and claimant records for specific radionuclides, they were monitored and available for use in claimant-specific DRs, and specific adjustments based on individual radionuclides outside the scope of RPRT-90.

Next slide.

Okay, so our concern about this is that it doesn't appear to address the conversion of counts per minute or dpm or microcuries to a intake.

And perhaps this is more applicable to a revision in a coworker model, except if you have a bioassay card that just lists gross beta or gross gamma, or just dpm to microcuries without any specific radionuclide, then how would you handle it for a particular DR besides a coworker model?

And so, that was our main concern about this. Next slide, please.

So, we realized -- go back one now -- although RPRT-90 is not intended to be a guide for step by step for DR, we feel that if some of the data that we're going to use for each individual dose reconstruction does not contain the full information and then how else at the address.

And so, we recommend that the observation be discussed or remain open, whatever's appropriate.

Chair Roessler: Okay, it seems like NIOSH could respond to this.

Dr. Hughes: Oh.

Dr. Taulbee: Go ahead, Lara.

Dr. Hughes: No, I don't have any, you can go ahead.

I was just going to say about this, a lot of this, would, you know, depend on individual DR, and that's beyond scope of this report, but we can certainly add a little detail in the revision of RPRT-90 if needed.

Dr. Taulbee: That's correct.

I think we can add a little more information that this will be considered in RPRT-90, but really where this will be most addressed is in the co-exposure model from that standpoint, which does provide specific instructions to dose reconstructors on how to use these results and what those assumptions are. So –-

Chair Roessler: Well, this one --

Dr. Buchanan: That would be –-

Chair Roessler: Go ahead.

Dr. Buchanan: Oh, excuse me. Yeah, but that would be, you know, for somebody that wasn't monitored.

What about a person that was monitored?

Where would that be addressed on how to convert cpm or dpm or microcuries to a particular intake if you don't know their isotope or the counting efficiency?

Dr. Taulbee: The Technical Basis Documents go through for dose reconstruction how to interpret each of the bioassay, and so, if you go to the internal Technical Basis Document for X-10, there is some guidance in there, but I'm not sure that it goes into the details of the exotic radionuclides from that standpoint.

Mr. Barton: This is Bob. So would that then be considered not only perhaps a revision to RPRT-90 to say, you know, this is beyond the scope, but guidance will be provided to the dosimetrist how to interpret it, and that information belongs in the TBD, and so it also would require an update?

It seems to also, the TBD -- and maybe that gets addressed on the adjustments you'd have to make in the co-exposure development, and that information just gets ported over?

Is that essentially what you're saying, Tim?

Dr. Taulbee: I'd have to go actually back to the TBD and re-read the entire internal section because there could be information in there saying if you run into one of these other radionuclides, what to do.

That's not on the top of my head as to what it states here, so you know, we'd have to go back to that, and I guess get back to you, so if you want to, you know, leave it open, then we can answer it, you know, later.

I just don't have that information on the top of my head.

I don't know if anybody from the ORAU Team does, but, you know, if you do, speak up, if you don't, then we'll get back to you all.

Chair Roessler: So why don't we leave it –-

Dr. Buchanan: Okay, I'm okay with it.

Chair Roessler: So we'll leave this one open and go on to Observation 5.

Dr. Buchanan: Okay. Observation 5.

Now this kind of relates back to Observation 1 with a detail on Table 7-6, and the results, we talked

about 7-6 before, 7-6, where we had estimated dose from the problem radionuclides.

And what this has to do was, we was trying to recalculate the doses and verify them, and we found out that the inventory didn't match what we had on our spreadsheet that was in the RPRT-90.

And so, but that has been explained. Next slide, please.

Okay. And so, what we found out was that the RPRT-90 was using the additional information in the logbooks and such, and so that was some updated years, updated inventory.

And so, this was also supplemented, Table 7-6 was supplemented by some information in the NIOSH's 2020 response in their table 3 of page 12 of that response.

And we went back and filled in the blanks, and used that inventory then. Next slide, please. Bob. Okay.

So we found that the observation had been addressed and we agreed with that, and so we recommend that this observation be closed.

Chair Roessler: Okay, so that takes us through all the findings and -- because we took care of Observation 6, I believe, so we looked at all the findings and all the observations.

Does SC&A have anything else to add?

Mr. Fitzgerald: Yes, I have one. This is sort of a comment or a question for NIOSH.

You know, SEC 186 -- which is one of the SECs for Y-12, this is all employees through 1957 -- is based in part on inability to dose reconstruct exotics

generated at the calutron. And I know RPRT-90 was reserved for X-10 and Y-12, so I guess my question is, given that there's an SEC Class defined based on exotics generated at the calutron, it overlaps the period that RPRT-90 is examining, what are the implications for, you know, the feasibility assessment that RPRT-90 is doing for isotopes, you know, given that the decision that was made for Y-12?

Because it appears there's definitely an overlap on the source terms that were being generated by calutron, and presumably being transferred over to Oak Ridge National Lab for processing.

Dr. Taulbee: Well, that's a very difficult question to answer off the cuff here.

(Simultaneous speaking.)

Mr. Fitzgerald: Oh, no, and I think that's why I said it was more of a question/comment because I found that rather interesting, and I'm not sure, and certainly, you know, it may take a little digging what the implications are of that definition --

Dr. Taulbee: I mean, keep in mind --

Mr. Fitzgerald: For that time period of Y-12.

Dr. Taulbee: Keep in mind when we are doing some of the SEC evaluations, when we find an infeasibility for a particular radionuclide or for a set of radionuclides, many cases, we stop the evaluation, okay?

We don't go into more detail, we've got an infeasibility, we'll present it to the Board and we go with, you know, in this case, Y-12 to a designated SEC.

And so, you know, we'll say something like exotic

radionuclides, that means there's one of them, we can't separate who was exposed to what, and so, we'll put it all into the Class in a sense.

That doesn't mean there aren't some of them in there that we could estimate, you know, from the current co-exposure modeling methodology.

We didn't go into, you know, that level or great detail from that standpoint.

Should we have been more specific? Possibly.

You know, in the past, you know, it's just this radionuclide and these others, you know, we can do it precisely.

So I would have to go back to the Y-12 and see what that basis was, and then compare it with what came over from Y-12 to X-10.

X-10 really only applies which radionuclides came from Y-12 into X-10. Okay? The ones that were produced at Y-12 and stayed at Y-12 and were shipped out from Y-12 don't apply to X-10, even if they were ORNL workers. Okay?

Mr. Fitzgerald: Right.

Dr. Taulbee: So, there's that distinction there that we have to try and make sure we keep separate between the two facilities, and we have to be really careful here, so I can't answer your question off the cuff.

Mr. Fitzgerald: Yeah, and I guess I would just say, and leave it at that, just in the follow-on assessments that are planned, maybe some reconciliation of that SEC Class definition based on exotics on the calutron and whether it has any implications for isotope production, the survey that's being done under RPRT-90?

That would be the only comment I would make.

Dr. Buchanan: Thank you.

Chair Roessler: Okay, thank you, Joe. So I think then we're at the end of our evaluation of the report.

What I think we have before us -- and I'm going to look at my notes here and see if I do it right -- I think Finding 3 and Finding 4, which are still open, we're really not sure when we're going to be able to follow through on that, so I think that'll just stay on the table.

However, Finding 5, we have quite a bit to talk about, and I suggested maybe we wait until NIOSH puts together a response, and then we hold another Work Group meeting.

Does that sound feasible?

Member Beach: Yeah, Gen. This is Josie. It does to me.

Member Field: Yep.

Dr. Taulbee: Can I ask a clarifying question? With regards to Finding 5, would you prefer to see that as a full co-exposure model, or as an interim product? Just for guidance on our part.

You know, if we put the iodine together, separate from the other exotics, would you want to look at that all together with the other exotics, or kind of interim?

Chair Roessler: See if Bob can respond to that one.

Mr. Barton: Well, I think a lot of the concerns may only be addressed when we see a full co-exposure model for it, so I'm not sure how much would be taken care of by an interim update to the analysis.

You know, again, I think a lot of these questions are overarching, how do you reconstruct radioiodine for this period when you certainly have no data?

And some of the questions may even extend into the period after this, about when it is clearly feasible to reconstruct iodine, and obviously that's been made very clear.

That was not the intent or the scope of RPRT-90.

Maybe Tim's question is whether iodine should be separated out completely from the co-exposure effort.

Just because it has somewhat of a different general approach using thyroid monitoring and extrapolating from another period.

I guess I'm not sure.

It's tough to say until we see what the interim report does and does not evaluate, but I think a lot of our questions might only be answered completely when there's a complete co-exposure model on the table that addresses all of the criteria in IG-006 and extrapolating using sort of a surrogate dataset for a different time period.

I think those would only really be addressed through a full co-exposure model to address feasibility.

I mean, we can certainly maybe knock some of the concerns out with an interim product, but again I'm not sure the entire issue of iodine would go away until the co-exposure model is completed.

Dr. Taulbee: Okay. Then what I would recommend to you, Gen, is that Finding 3 and 5 will be kind of addressed in a full co-exposure model for the ORNL site.

And we'll put that on the schedule for -- well, I think it's already on the schedule, but I'm not sure of that, but I don't know the date of when we'll be starting that effort.

Chair Roessler: Okay. And so, and then you're clear on Finding 5, too?

Dr. Taulbee: Right, Finding 5 will be incorporated in that, that full co-exposure model.

We'll go through and address each of what SC&A has raised here under Finding 5 with regards to the iodine, and then they will have the ability to review that full model, in light of IG-006.

Chair Roessler: Okay, so I'm satisfied that we've covered everything.

And I want to make sure everybody else does, but I do want to comment that I'll have to make a report on this at the August Board meeting, and I would like to have -- I will need some help on putting that together, and I wonder if either Bob or Tim or Lara could help me draft something?

Dr. Hughes: Gen, this is Lara, I'd be happy to.

I'm usually sending out a summary of the NIOSH path forward or to-do list after the meeting, so hopefully that should help, and then if you need additional detail, I'd be happy to write something up.

Chair Roessler: Okay, so you have volunteered to draft something for me. Very good, thank you.

So are there any other things we need to deal with, either Rashaun or Work Group members, or anybody else?

Dr. Roberts: Yeah, just to follow on to your question,

your planning for August, Gen, I just wanted to check in and see -- I think we tentatively allocated an hour for this Work Group's update.

Is that too much, too little?

Chair Roessler: It sounds like too much. What do you think, Lara?

Dr. Hughes: Yeah, that was actually one of the questions I had, if, you know, there's a desire to have a full presentation for NIOSH to summarize this Work Group meeting, or if we're just going to have a verbal discussion?

It is up to the Work Group, obviously. I'd be happy to provide like a PowerPoint presentation update, if needed.

Dr. Taulbee: I understood what Gen was saying.

Gen, you were going to give the presentation and we're just going to help you make the slides for that presentation, correct?

Chair Roessler: If that's okay with everybody, I'd do that.

Dr. Taulbee: Okay.

Chair Roessler: Okay.

(Simultaneous speaking.)

Member Beach: That seems reasonable, Gen.

Chair Roessler: Okay, so you think an hour is good, Josie?

Member Beach: Well, it just depends on questions. I doubt that you'll speak for an hour, but perhaps 30 minutes. What do you think, Gen, I mean, as far as -

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Chair Roessler: Oh, I'm thinking 30 minutes would cover it, if Lara sort of agrees with that. For speaking.

Dr. Hughes: Yes.

Chair Roessler: And I kind of doubt that we'd have many questions, but I don't know.

Dr. Roberts: Okay, I'll make a note for 30 minutes.

Chair Roessler: Okay, and I'll go fast.

Dr. Roberts: Okay. Thirty-ish.

Chair Roessler: Is there anything else –-

Dr. Roberts: Okay.

Chair Roessler: Is there anything else we need to take care of at this meeting?

(No audible response.)

Chair Roessler: Okay, then I thank Rashaun and unless we hear something very quickly, let's adjourn.

Dr. Roberts: Sounds great, nice job.

Mr. Barton: Oh, Gen, I was just going to add that insofar as the SC&A can help collaborate with Dr. Hughes and NIOSH about putting together basically a summary of what was discussed today, you know, we serve at your pleasure, so just let us know how we can help.

Chair Roessler: Okay, well I'll have you take a look at it for sure.

Mr. Barton: Thank you.

Chair Roessler: Okay.

Dr. Roberts: Great.

Chair Roessler: Thanks to all of you.

Dr. Roberts: Thank you.

Chair Roessler: All right, bye.

Dr. Roberts: Bye-bye.

Dr. Taulbee: Thank you all.

Chair Roessler: Bye.

Member Beach: Thank you. Bye.

Adjourn

(Whereupon, the above-entitled matter went off the record at 1:38 p.m.)