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NATIONAL INSTITUTE FOR OCCUPATIONAL
SAFETY AND HEALTH

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ADVISORY BOARD ON RADIATION AND
WORKER HEALTH

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PINELLAS PLANT WORK GROUP

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THURSDAY
FEBRUARY 11, 2016

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The Work Group convened telephonically at 1:00 p.m. Eastern Time, John W. Poston, Sr., Acting Chairman, presiding.

PRESENT:

JOHN W. POSTON, SR., Acting Chairman
BRAD P. CLAWSON, Member
R. WILLIAM FIELD, Member

ALSO PRESENT:

TED KATZ, Designated Federal Official
NANCY ADAMS, NIOSH Contractor
BOB BARTON, SC&A
PETE DARNELL, DCAS
BRIAN GLECKLER, ORAU Team
DONNA HAND
JENNY LIN, HHS
JOYCE LIPSZTEIN, SC&A
JOHN MAURO, SC&A
JIM NETON, DCAS
MUTTY SHARFI, ORAU Team
MATT SMITH, ORAU Team
JOHN STIVER, SC&A
KATHY LUDWIG TALBOT
JOE ZLOTNICKI, SC&A

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(1:00 p.m.)

Welcome, Roll Call, Introductions

MR. KATZ: Alright. Well, it's about time. So, welcome, everyone, to the Advisory Board on Radiation and Worker Health. It is the Pinellas Work Group.

And someone has their speakerphone on because it's echoing.

And so, some preliminaries. Let me, for everyone on the phone who may be joining us from the public, for example, all the materials being discussed today, the agenda and related White Papers and so on, should be found on the NIOSH website, under scheduled meetings, today's date, so you can follow along with the articles as they're discussed.

And so everyone who is actually in the Work Group, the staff, you should have all those documents in the non-PA cleared form.

We should have joining us today Bill

Field, too. Is that correct? Bill, are you on? Oh, actually, he said he was going to be about ten minutes late. But he should be joining us.

And Phil Schofield, who normally chairs this Work Group, is having an operation today. So he's out today. He can't join us. But Dr. Poston, John, has basically volunteered to chair in this place, and we thank you, John, for that.

ACTING CHAIRMAN POSTON: Now that you've told me that, I think, gee whiz, he'd rather have surgery than chair this meeting. What's wrong here?

(Laughter.)

MR. KATZ: I think he would rather chair this meeting, but he's doing what he needs to do.

So, let's just go through, to begin with, roll call.

(Roll call.)

MR. KATZ: Okay, then, let me just remind, in case you came on late, members of the

public and others, staff as well, except for when you are addressing the meeting, please mute your phone. If you don't have a mute button, press *6. That will mute your phone. And also, do not put this call on hold at any point. Hang up and dial back in if you need to leave for a piece because putting it on hold will cause problems for everyone else on the line.

And it is your meeting, Dr. Poston.

ACTING CHAIRMAN POSTON: Okay. Well, we will go to the agenda and start with the recap. And I thought I saw, is this Pete, are you going to do this?

MR. DARNELL: Yes, I am going to do this.

ACTING CHAIRMAN POSTON: Okay, it's all yours.

Summary of Changes Made to Pinellas Site Profile

MR. DARNELL: I'll be speaking mainly from the summary changes made to the Pinellas Point Site Profile in 2011, sent to the Work Group a while

back. It's on the website.

The Technical Basis Documents making up the Pinellas Plant Site Profile were revised in 2011. I'm not going to read the documents word-for-word, but since the TBDs were originally written it was discovered that a number of the key references were actually from the General Electric X-Ray Division, the GEXM site. Pinellas and the GEXM have a lot of items in common and it was described in the revised site description of the TBD.

I want to apologize in advance here, guys. I am short of breath today.

Reference to the GEXM documents have either been removed or replaced with the appropriate Pinellas Plant document references. I just wanted to make sure that was up-front.

In the introduction, Technical Basis Document Issue 1 Resolution was addressed in "Summary of Data Capture Searches for the Pinellas Plant." This is a rather brief part of the TBD.

It was completely rewritten to update and correct some of the information from the GEXM site and select information in the other revised TBDs.

The site description, TBD Issue 6 Resolution --

MR. KATZ: Pete?

MR. DARNELL: Yes?

MR. KATZ: Pete, I'm sorry to interrupt, but could I just ask, not just for you, but really for everyone when they are discussing matters, instead of referencing issue numbers, which aren't going to mean something for people who don't work with the issue matrix on a day-to-day basis or what have you extensively, could you please just, at the outset of mentioning an issue, give it a sort of English title, instead of an issue matrix number? Thanks.

MR. DARNELL: Okay. I've got to get out another set of notes. I didn't write it up that way. So, I -- alright.

So, we are into the Site Description

Technical Basis Document Issue 6, which was the decontamination and decommissioning era of Pinellas operations not specifically addressed.

The resolution that was added was added in 2011 as Section 2.3.4 of the TBD. I do want to note that since the TBD has been revised there were two D&D periods that were added in 1999 and 2008 through 2009.

The secondary issue, which was Issue 2. Give me a second so I can get to that one. Inadequate descriptions of certain plant operations was added to the Site Description Technical Basis Document in section 2.4.1, which was entitled "Radioactive Materials." But please note that some of information was added throughout the TBD.

A number of significant changes were made to the Site Description TBD. A lot of information was incorporated on the process and facility information and some of the redundant information was eliminated from the TBD.

Occupational Medical Dose TBD, Issue 10 -- Issue 9, 10, and 11, actually, were all addressed in the TBD.

Issue 9 of the Technical Basis Document fails to adequately define and assess occupational medical exposures. Issue 10, the technique and protocols increase uncertainty in DCFs in the TBD. In number 11, the frequency in type of X-ray exposure is uncertain.

The Technical Basis document was updated. The equipment and techniques section being updated in Table 3-1 has all the new information that was found. Pre-1972 X-ray doses are still based on OTIB-6. Photofluorographic X-ray dose values were changed in a revision of OTIB-6.

Issues 9 and 11 resolutions, exam frequencies are now included in the TBD as well as indicating lumbar spine and abdomen and kidney/ureter/bladder X-rays are considered part of the occupational screening done at Pinellas.

A stipulation was also added that not to assess X-ray doses for X-rays from employee records that were clearly indicated for diagnostic reasons or for other work-related injuries.

In addition, the section now indicates to assign occupational medical doses based on the X-ray records when provided. In Pinellas, they typically were. And we assign medical doses based on OTIB-6 when no records were provided.

Secondary Issue 1 resolution, which was additional factors contribute to uncertainties related to occupational medical exposures is addressed in an update to the TBD.

An error in the applicable period for photofluorographic X-rays was corrected to reflect the recommendations of OCAS-PER-004, and it's now 1957-1959 versus 1957-1960.

A little bit more that was updated in the occupational medical section you can read. None of it is specifically addressing the primary and secondary issues.

Moving on to the Environmental Dose TBD, Secondary Issue 3, which was perimeter tritium air monitoring stations. They are now provided in Section 4.3.5 of the Environmental Dose TBD and a comparison of the air concentrations based on atmospheric dispersion calculations, and actual air monitoring is now provided.

Section 4.4.2 of the TBD recommends more claimant-favorable unmonitored external dose assignment from the Occupational TBD to be used in lieu of the estimated doses, mainly because it's more claimant-favorable, provides a higher dose.

Bounding of the on-site environmental doses were assessed in Section 4.4.1, which were determined to be negligible, under 1 milligram for all internal organs. As a result, it was determined that the environmental doses will never need to be included in the IREP input sheet.

A number of other changes were done to better organize the information being presented, incorporate all the new information, and correct

some inaccuracies.

The occupational internal dose TBD Issue 2 resolution was covered until Section 5.7.1.2, which would be the dose reconstruction that Mutty Sharfi will be talking about later. This issue has to do with insoluble forms of metal tritides.

Issue 3, which was the MDC and uncertainty information. Let me get to that one and read it directly. The MDCs and uncertainties for zirconium and bioassay measurements are inadequately addressed in the ORAU and in this portion of the TBD.

This one has had some rather interesting work done on it. In the past, we were looking at plutonium internal doses and coming up with methods to calculate, but based on the plutonium paper that you have seen in the past and what came out as a reminder for this meeting, there really were no plutonium intakes.

The new plutonium information was

supposed to go in Section 5.7.3, and it actually was printed into the Technical Basis Document and then later removed, as was required during the October 2011 Working Group meeting. If you need to look that up, it is on page 84 of the transcript.

Issue 7 Resolution, Issue 7 was missed internal dose estimation methods for unmonitored workers, for example, main entry support personnel were not provided.

Section 5.7.2 of the TBD has been updated to address the unmonitored exposures to tritium. And Sections 5.2.4 and 5.2.5 address potential unmonitored exposures to nickel-63 and carbon-14.

Secondary Issue 5, which was -- I apologize for the delay in answering these because I'm flipping back between two documents. The rejection of plutonium bioassay results based on plutonium-238 to -239 ratios, and non-detectible plutonium-239. Again, kind of coming back a little bit earlier. A lot of work went into

looking at potential plutonium doses went into Section 5.73 and then was later removed.

The Secondary Issue 6 resolution, Secondary Issue 6 was plutonium solubility. Again, a lot of work went into it. Section 5.7.3 was updated then removed. Internal dose reconstruction for OTIB-60 requires a dose reconstructors to use the most claimant-favorable lung absorption types, so if we ever have to do a plutonium intake, and so far we haven't after quite a few Pinellas dose reconstructions, we would use whatever is the most claimant-favorable.

A number of other changes were done in the Occupational Internal TBD in February 2012. This was discussed in the October 2011 Working Group meeting. Because external exposures to plutonium were likely at the Pinellas Plant, all of the discussions at Section 5.2.2 would determine, again, necessarily, a potential source of confusion. So, they were removed. If you want to look that up, it is on pages 80 to 82 of the

Working Group meeting.

External Dose Technical Basis Document Issue 4 resolution. Issue 4 was assessing the personnel badging policy during early years needs further review. Information was added to Section 6.1.5 to address Issue 4.

Issue 5 resolution, dosimetry technology and missed dose sections. That is actually part of the agenda for later today. So, we will address that then. Information was added to the TBD and there has been a couple of memos by SC&A and NIOSH discussing this topic. NIOSH has more information that will be presented today. Due to lack of time, it wasn't able to be written up and sent out as a formal document.

Secondary Issue 7, which was assumptions for unmonitored workers, Attachment B was added to the external section of the Technical Basis Document. It provided a comparison of the unmonitored doses to the assignment of the maximum likelihood doses for unmonitored workers.

Secondary Issue 8, which was assumptions relative to the minimum detectible level adjustments to dosimetry for missed dose. And again, the missed dose section was revised.

Neutron doses is replaced with an approach that is consistent with approaches used for other sites for neutron generator areas. The new approach that was added for the years 1957 to 1969 added higher yield neutron doses and lower doses for the years 1969 to 1997.

RTG areas, measured photon doses for the years '79 to '81 are now higher because of a new dosimeter correction factor. Missed photon doses for the years '79-'87 will also be higher because of a correction factor and a higher LOD.

RTG areas now have a more claimant-favorable neutron energy distribution.

Basically, a number of other significant changes have been done to the TBD to better organize information and incorporate new information.

The only issue I didn't talk about was Issue 6, which, again, is part of the agenda for today and that deals with collection of data mainly from Albuquerque on the D&D era.

Any questions on that?

MR. KATZ: Let me note for the record, too, that Dr. Bill Field has joined us or joined us a little while ago.

MEMBER FIELD: Yes, I'm on the call. Thanks.

ACTING CHAIRMAN POSTON: Any questions on what we've heard? Well, thank you, sir.

MR. DARNELL: Alright.

ACTING CHAIRMAN POSTON: And the next is John Stiver. Do you want to do the closed issues update?

Closed Issues in SC&A Issues Matrix

MR. STIVER: Yes, I will. Pete has actually covered a lot of this. So, instead of replicating what he's done, I'm going to try to touch on some of the aspects that were more

important to SC&A and maybe get some of the background on how we got here, considering it has been a very long time and there were quite a few Work Group meetings.

What I am showing right now on Live Meeting is an annotated version of the issues matrix. I just put in some notes to myself here to make it a little bit more straightforward.

To kind of set the stage, we need to remember that we did the Site Profile review almost ten years ago. I think it was delivered in September of 2006. The first Work Group meeting was in June of 2008, when a lot of these issues were initially presented. And the following year, in June of 2009, a lot of the issues were -- I guess a way to put it would be to kind of put it in abeyance, in a way. NIOSH had agreed with us, basically, and agreed to rewrite the TBDs that hadn't been actually accomplished at that point.

They went ahead, as you know, most of the revisions that Pete described were in 2011.

There was an additional revision to the TBD 5, the internal dose TBD, in early 2012 that basically stripped out the discussion of plutonium, as Pete mentioned.

And then we had two other Work Group meetings, one in October of 2011, where NIOSH presented the changes to the TBD. And then SC&A had some follow-on items that we felt were important.

And then the last meeting was in 2012, November of 2012. And so, it has been kind of a long history. A lot happened. We have a lot of changes. The program has matured. A lot of things that were kind of a concern back in the initial review have been resolved in this as well as in other Work Groups. So, to just kind of get the big picture there, I wanted to say that.

And we can just kind of run through the issues. This first, Issue 1, the reconstruction of doses in the absence of early health physics, industrial hygiene, and environmental records.

What we were kind of concerned with here was that there were a lot of data from 1980 and beyond and we thought we might be dealing with a classic back-extrapolation issue, where later time periods and data for those periods might not be indicative of exposures that were placed early on.

And this was discussed in the 2011 revision of the TBDs. I believe it's TBD-6, Table B.1, lists all the external dose data by year from 1957 all the way through D&D. And we were convinced that there is enough information out there and enough good data that this really was put to rest. And you can see it was closed during the November 2012 Work Group meeting.

Issue 2 is the metal tritides which will be discussed today.

Issue 3 was this whole aspect of the sparse plutonium bioassay datasets. And there was a lot of discussion a year early on about the appropriate MDCs, whether there was any indication of exposures, what MDC was really more

claimant-favorable and applicable. And in response to those discussions, I think were in 2009, NIOSH went and added a lot of information in the revisions.

And based on our discussions in October of 2011, it was determined that there really isn't any indication that it was ever a true exposure potential. I mean, the only scenario we could derive would be the swiping and checking of triple-encapsulated RTGs when they came to the site, and the rejection criteria was 200 dpm. And none were ever rejected and this was all done in a hood. They were checked in the hood. And so that would be the only credible inhalation scenario in our internal plutonium exposure scenario, because there were never any breaches or any kind of destructive testing of the RTGs with the sources intact.

So, rather than -- NIOSH had presented all this information, all the evidence against an exposure, and then they went ahead and said, well,

but if we get a positive bioassay, we'll go ahead and do it on a case-by-case basis. And we said, well, wait a second. You don't have any exposure potential. The weight of evidence would certainly indicate that. So there was no reason to put this in. I mean, if you come up with some data later on to indicate there was a plutonium exposure, you're going to have to develop a dose reconstruction methodology. So, that's basically what was the backdrop to that issue. And that was closed out, again, at the November 2012 meeting.

Number 4, this whole idea of a badging policy which took place, we were concerned initially that maybe, because there was incomplete badging, that there might have been a cohort badging policy as opposed to selecting those individuals who had the highest exposure potential.

Based on lots of discussions and the revisions to the TBD, and discussions after the revisions, it became pretty clear that the health

physics policy was to badge those with the highest exposure potential. So, we closed out, actually this was the 2009 Work Group meeting.

Issue 5 is this sub-issue about the limit of detection appropriate for the post-1974 film badges. We're going to talk about that later.

Issue 6, again, we will discuss this a little later, it's about D&D and whether there was adequate monitoring during the D&D period.

Issue 7, missing internal dose estimation methods for unmonitored workers. This was the whole idea about how are you going to -- if somebody doesn't have bioassay, how are you going to assess their doses? And NIOSH has come up with a very claimant-favorable coworker model that uses the 95th percentile of the whole body dose, which is really a mixture of external gamma, neutron, and tritium at the 95th percentile, assuming a chronic exposure at the 95th percentile.

And that, in combination with putting to rest the idea of the nickel-63 and carbon-14,

and also the fact that there really is no plutonium exposure to be addressed, really puts this one to rest. I put a note in there about the tritide model being applicable, because that really is not for a coworker model application for those who actually had tritium bioassays. So, I probably shouldn't have put that in there.

Issue 8, missed dose from depleted uranium. Initially, it wasn't clear whether the DU tritium and tritide beds were being cut in -- the cutting took place at Pinellas and we thought that there might be an exposure potential there that needed to be addressed.

Research showed that it was actually done in Milwaukee in a sister plant. And so, this never took place at Pinellas. So, that was closed out in 2010.

Pete mentioned 9, 10, 11, and sub-issue 1 related to TBD-3, medical exposures, occupational medical. He discussed that pretty well. I don't think we need to replicate that

here. Suffice to say that those are all closed, along with Secondary Issue 1.

Secondary Issue 2, okay, this is we felt that the descriptions of the plant operations were inadequate in the first Rev. 0 in the 2005 version. That was taken care of. We concur. This was actually, yes, in 2009, we had agreed that if the new information was added to TBD, this would be resolved. It was, in fact, incorporated in Rev. 1.

This is Secondary Issue 3, perimeter tritium monitoring. This was addressed in the new Rev. 1 TBD-4.

Secondary Issue 4, the uncertainty. Once again, this was addressed in TBD-4, Rev. 1.

Secondary Issue 5 goes away because of the lack of plutonium exposure potential, as does Number 6.

Secondary Issue 7, we concur. Pete described this one as well. There is no need to go through that again.

And then Secondary Issue 8, the MDL for dosimetry for missed dose, again, the language clarified in Section 6.4 of TBD-6, Rev. 1.

So, that was kind of a cameo view of nine and a half years of true resolution. Are there any questions?

ACTING CHAIRMAN POSTON: Any questions for John?

MEMBER CLAWSON: Yeah, we are going to discuss this tritium a little bit more in-depth, correct, here coming up?

MR. STIVER: Yes, that is going to be the next topic. That is really the long pole in the tent now.

MEMBER CLAWSON: Right, because I have been trying to keep up on all those emails back and forth.

The one question I had about the plutonium samples, we have not seen any positive bioassays for plutonium so far, have we?

I know that you changed that and had it

put in. We discussed that a little bit but we probably got some more information that has come in. At this time, we haven't seen anything yet, have we?

MR. STIVER: That would probably be a good question for Pete or Brian, but my understanding is that, no, they haven't.

MR. GLECKLER: This is Brian from the ORAU Team. There are positive bioassay results, but there's significant issues with those results.

MEMBER CLAWSON: Well, what's the issues with those, Brian?

MR. GLECKLER: It's part of what was in that paper that is posted on the website right now. Like, one of the key ones is the source terms that were used were dominated by Pu-238. And a number of the bioassays that are positive are baseline bioassays before they went into those areas, or was positive for Pu-239 and negative for -238, which they should have had a much higher positive Pu-238 number in their bioassay result.

And let me see, some of the other ones, the ratio is way off on one instance, enough to where it couldn't be the Pinellas Plant material. And a lot of it, like, results, re-analysis results, were less than the MDC at that point, where they did reevaluate the result or re-analyze it.

And in several instances, the lower bound, there is an uncertainty associated with each sample result. The lower bound to that was actually below the MDC also, but I don't think we discounted any of them solely based on that criteria.

MEMBER CLAWSON: Yeah, I was just trying to follow this information. I read the report on this and just did we ever figure out if these people were coming from another site that had brought this? It is just kind of stressed me a little bit that we're are not doing any Pu samples but we have had some that had come but they were pre-employment samples for the pre-employment sample or for others were part of this group. And

I'm just wondering if we ever pulled that string just to justify why we are not doing this, that these were previous samples and this is where they came from. I was just trying to follow where that went, if we had done anything.

MR. GLECKLER: Those have to do with more of an error, an issue with the analytical process for how they analyze the samples. And possibly it might have been because they used a Pu-242 tracer, if their yield results or percent recovery is the way they report it for their analysis. And there is impurities for the other Pu isotopes in those. You know, a number of those samples, based on the amount of tracer that they are spiking the sample with, typically, it that could account for the Pu-239 result in some of those instances. So, it's things like that, because they are not subtracting that out, that contribution from the tracer.

And if they were from an exposure received at another site, we haven't been able to

connect that dot yet with any known claims. But if, say, someone transferred from another site where a Pu exposure could have occurred and their Pinellas Plant baseline is indicative of a potential intake, or an indication of a potential intake from that prior employment, we can use that for that prior employment. But we would need --

MEMBER CLAWSON: Well, it just kind of struck me interesting because these bioassays were in the '88 to the '90 timeframe and you would kind of think that they would have had this down pretty good by that timeframe.

That is why I just would be interested in what we have done, and reading through this, I was just trying to follow it and I just wanted to make sure that what we have, you know, John kind of clarified it, but if we do have some come up positive for this, then we're going to take other actions that way. But I was just trying to get a better idea of this other one.

So, I appreciate that.

MR. GLECKLER: For the 1988 timeframe ones, they were transitioning to a completely different set of instruments for doing their analyses and they were modifying their procedures, and that's part of the problem, why you see so many in 1988.

MEMBER CLAWSON: Okay, thanks.

ACTING CHAIRMAN POSTON: Any other questions?

DR. MAURO: This is John Mauro. I have a question for Pete and John Stiver that's more process oriented.

In listening to the very nice overview provided on the 11 issues and sub-issues, it appears that certainly previously many of the issues were officially closed at earlier meetings. But it's not apparent that, in light of the new material that has been provided, we do know that there are certain issues that are open that we will be discussing real soon, tritium, other matters like that.

But are there any issues here that, as a result of the review we just went through, where we have a situation where SC&A concurs with what changes were made to the TBD and that we recommend closing the issue? And are we in a place where perhaps some of these items that we just discussed, the Work Group should officially say closed? I just want to make sure that we are not moving on without closing things up that perhaps need to be closed at this time.

MR. STIVER: John, this is Stiver. I can answer that.

The ones that are listed as closed in the matrix are officially closed. They have been closed in the meetings, in previous meetings.

DR. MAURO: Got it. Now, are there any here that we --

MR. STIVER: It was just kind of recapped because it had been so long since any of this information was talked about and discussed that it was really more to refresh everyone's

memory as to where we are now and how we got there.

DR. MAURO: No, and I think that was clear and I understood that, but it wasn't apparent to me whether there were some other issues that, in light of what has transpired, that we are recommending closing now and there does need to be some type of a vote.

MR. STIVER: Basically, that's going to be Issue 5 and 6 and the tritium.

DR. MAURO: Oh, which we're about to discuss.

MR. STIVER: Which we're going to discuss today.

DR. MAURO: Okay, thank you. I just wanted clarification on that.

MR. STIVER: Okay.

ACTING CHAIRMAN POSTON: Any other questions?

Okay, I guess we will move on to Number 3. Jim, are you going to do that, Jim Neton?

Updated NIOSH Internal Dose Model for Stable Metal

Tritides

DR. NETON: This is Jim. I can get it going, and I think I'm going to pass the baton to Mutty to have him flesh out what we've done in this area.

ACTING CHAIRMAN POSTON: Okay.

DR. NETON: Back in the November 2012 Work Group meeting, NIOSH suggested that we need to take a step back and look at the tritides, insoluble tritide model that we're using at Pinellas. It was based, if you all remember, on what we did at Mound, the resuspension model, but there were some notable differences, particularly in how the tritide samples were processed.

So, we identified five areas to take a second look at. And Mutty put together a nice little White Paper, and I'm going to leave it to him to summarize those issues and where we currently stand.

So, Mutty, are you there?

MR. SHARFI: Sure. Yes, I'm here.

DR. NETON: Okay, great.

MR. SHARFI: So, we've got, as Jim said, five issues that asked us to maybe expand on, and I'll just go through one-by-one on these issues.

Issue 1 is about the appropriateness of a resuspension factor, E to the minus 6, 10E to the minus 6. The reference to the Mound currently uses a resuspension factor of 5E to the minus 5.

So, generally, Pinellas was aware of the impact that of the contaminated areas and tried to maintain a clean work environment. Since the type of work that's done at Pinellas and Mound is similar and the resuspension factor amount is considered obviously more claimant-favorable, we decided that we should just go ahead and agree to change the resuspension factor that's in the TBD from 1E to the minus 6 to 5E to the minus 5.

Mound does their surface contamination, they use the 95th percentile. So they have a much lower contamination level. So,

in the end, the combined effort of the resuspension factor and the surface contamination probably leaves this to be a very bounding and claimant-favorable estimate.

Issue 2 is the question of the highest contamination survey reported and the basis for that. SC&A, in their response, found some additional samples, but at the time that we had gone through for the surveys that we had available to us, we looked at the health physics summary reports and found that at the time the highest reported surface contamination level was about 4 million dpm or 100 centimeter squared. So, it's about 10,000 times their control limits.

I note they were generally short-term, incident-related and they cleaned up immediately after identifying these areas. So, we felt that a value of that high assumed to be constant throughout all time is considered very bounding and unlikely to occur, as compared to the Mound TBD where they looked at the 95th percentile based on

a bunch of contamination surveys that they had. They had about 60,000 contamination air sample results that they went through. And so they went through annually and looked at the 95th percentile annually. And so their contamination level changes per year, where ours does not.

So, in comparison, if you look at what their numbers are, ours, the Pinellas, even though the work is similar, we're looking at maintaining a contamination level at least an order of magnitude higher than any given year that Mound uses. So we feel that is certainly bounding and claimant-favorable.

As I mentioned, in SC&A's reply to this, and I think we'll get to this a little bit later, but they found some additional surveys that had a few that were reported slightly above the 4 million dpm. We still think the 4 million dpm, as a long-term, is still very bounding and unrealistic to occur on a very constant level.

Issue 3 is about the method for the

paper filters to actually detect the particulate, the metal tritide and metal particulate. This one more focuses on there is a report that we found that, when they would take their smears, that they would rinse these cotton balls, run the rinsate through a Whatman #1 filter paper and then they would count the rinsate. And so the question was around whether or not the rinsate would be -- or whether or not the filter paper that they used to filter the rinsate would remove some of the metal tritides from the rinsate, and, therefore, not be counted. Would the underlying contamination level reported actually being an underestimate of the metal tritide concentration?

Generally, the purpose, I would imagine, of the Whatman #1 filter would be able to reduce quenching of the sample and you would try to get rid of all the big dirt. You would cotton ball the wiping area, if you have a dirty sample, that would, I imagine, be the main purpose of filtering the rinsate.

We talked to a number of health physicists that worked there. They don't ever remember actually doing, filtering any of the rinsate. That was something they don't remember doing, but it was actually in a procedure. So, we felt it was worth investigating whether or not there was an impact to the contamination level if they did use filter paper.

A Whatman #1 filter paper has a particle size where the holes for the Whatman filter filter out particles above about 12 microns. This is much larger than the actual metal tritide particle size, especially even respirable. So, even if there was any kind of filtration, you would either be filtering out non-respirable metal tritides, or more realistically, you're just going to get all the metal tritides that are just going to go straight through the filter paper and they'll be captured.

In the case that if there was some, we looked at a report, Pinellas did research on -- they

took some -- they looked at the deposition of the vapor on a ceramic or metal substrate and they wanted to see how stable the metal tritide was on those substrates. So, their concern was that there was a metal flaking from the vacuum tubes in places, so that is basically an accident, which is what, you know, obviously, in exposure to an area for a worker, you are concerned with.

So, based on their review of this, a paper titled, "A Study of Particulate in Gaseous Emissions of Tritium from Neutron Generator Targets." And that is in SRDB-12275.

So, when they looked at the emission rates for that particulate and then there is the HTO emission rate was actually much higher than the particulate tritium rate. So, anytime you have any release of the particulate, you are going to get a large, usually a 1.5 to about 2.0 factor of tritium HTO more than you're going to get of the actual particulate.

So, even if you had some loss of the

particulate in these filters, either in the cotton ball or in the Whatman #1 filter, you are still going to get a much larger portion of the HTO that's going to pass right through and be counted in the swipes.

So, by treating the entire swipe as all particulate and not partitioning out any of the HTO, we feel that the use of the contamination surveys, as is, and assuming 100 percent particulate, will bound any possible losses that would be captured, either from the filter paper or from the cotton ball itself.

Let me know if you have questions as I am going through these issues and let me know if I am going a little too fast. I am trying to summarize various sections.

Issue 4 --

ACTING CHAIRMAN POSTON: They will certainly ask you questions if they have them.

MR. SHARFI: Certainly. Does someone have question?

ACTING CHAIRMAN POSTON: No, I said they will ask you questions if they have them, I know.

MR. SHARFI: Oh, okay. Yes.

Issue 4 is the magnitude and the extent of the potential for metal tritides at Pinellas. We did a review of all the monthly health physics reports. As I said before, the main exposure to tritium would likely be more associated with the HT gas, the HTO, and any organically-bound tritium that would be associated, like the pump oil and stuff like that, working with the glove boxes.

Generally, the metal tritide was contained within the tritium charging system. And so, when they did have a breakage or they did have a spill, the health physicist reports did indicate that the affected areas were cleaned up immediately. They surveyed in the morning every day to see if there was contamination control issues, and they surveyed after any incident to make sure that any cleanup was complete.

And so you generally didn't see long-term contamination issues throughout the facility. They did note in some of these reports a couple of the breakages. So, we do know they did happen, but they were infrequent.

The type of materials, generally, the glass tritium beds were eventually switched out from the glass beds to the stainless steel beds, I think in the late, I want to say early '70s or late '60s, I believe. And that helped reduce the breakage. Early in the '50s and '60s, they would have to treat the glass to -- because obviously glass is brittle when you're handling it, to minimize breakage. So they went in later with these tritium beds to a stainless steel tritium bed to help reduce the number of breakages.

I will note that, in our response to Issue 4, which is identified in SC&A's response, there is an error in our text. I indicate that we assumed that there is a 2,000 hour per year exposure assumed. That actually should be 2,600. In the

example DRs that we did for the metal tritide stuff, we did use a 2,600 and that's actually what was used in the TBD.

So, I just wanted to note, in our White Paper, on the survey, that there is an error that the 2,000 is actually supposed to be 2,600 for Issue 4.

Issue 5 is about the solubility of the various metal tritides. Pinellas used a variety of number of types of metal tritides and these are also some of the ones that are common at Mound. They used erbium, scandium, titanium tritides. Eventually, the tritium beds were converted to uranium tritide beds.

The solubilities for the metal tritides ranged from Type F all the way up to Type S. So, all various solubilities for metal tritides are possible. Most of the tritium was titanium tritide, which is generally more a Type M material, but they did work with some of the scandiums and stuff like that that are Type S.

And so, in general, we have agreed that we will modify the TBD to increase the resuspension factor from 1E to the minus 6 to 5E to the minus 5. That will increase the intake rate. But we do believe that the 4 million dpm -- which is about 400 times what it now would be at HCA, high contamination area, level -- we do feel is very bounding and very claimant-favorable and it's unlikely to have the workers in an environment at that level at 2,600 hours per year.

And that's basically the White Paper. I don't know if there's questions or if you want me to go through the example DRs, too.

MEMBER CLAWSON: Mutty, I had a question. And maybe this is a little out of the realm of it right now, but who is going to be getting this tritide dose? Do we have a certain group that is going to be getting it or is this everyone?

MR. SHARFI: Anyone that was on the tritium urinalysis bioassay program would also get the metal tritide exposure.

MEMBER CLAWSON: Okay, now, is that going to be capturing any of the maintenance people and stuff like that, too, or just kind of --

MR. SHARFI: The tritium bioassays, the urinalysis program was very widespread throughout the plant. So, if you were in any kind of tritium area for any reason, they bioassayed you for urinalysis for HTO, for the solubles.

MEMBER CLAWSON: Okay. Now, this system, the one at Pinellas is a little bit different set-up than the one at Mound, wasn't it, if I remember right? It was opened up more to the area and that's why we have kind of taken this route of a little bit higher?

MR. SHARFI: Well, you mean, why we didn't have higher contamination levels on Mound?

MEMBER CLAWSON: Yeah, I was wondering what --

MR. SHARFI: One of the main reasons is, at Mound they had all the individual survey results for the R108 and SW8 rooms where the tritium

beds were contained. In this case, we more had the monthly summary reports. So, we don't have the individual surveys. So, we are looking at this at a higher level, where the monthly reports just gave us the highest survey results. And so Mound was able to do a more statistical analysis of the conditions, where we're having to look at a more outside view of what the more worst case scenario was.

MEMBER CLAWSON: Okay. You were mentioning in there that we did have some broken glass and so forth like that. What kind of readings were they getting when they had these kinds of upsets?

MR. SHARFI: I will tell you, in the monthly surveys, that the one we used for the 4 million dpm per 100 centimeter squared wasn't even a metal tritide incident. It was actually a maintenance activity.

Generally, the ones that were identified associated with the metal tritide

particulate work were an order of magnitude or more less than 4 million. So, they weren't even seeing this high of a level of the particulate.

MEMBER CLAWSON: Okay. Well, thank you.

ACTING CHAIRMAN POSTON: Any other questions?

DR. LIPSZTEIN: Yeah, may I? It doesn't happen exactly with tritide, what you just said, that the workers were exposed to OBT also. And also in TBD-5 it says that the workers were exposed to OBT. But there is no mention on what do you do and how you calculated those for the OBT. Because it just mentioned the HTO water and not the OBT, and the dose per unit intake of OBT is twice the dose of the tritiated water.

MR. SHARFI: OBT is covered in OTIB-66, I believe, is the tritium OTIB. And so that TIB would cover any assessments of OBT and HTO. The White Paper I'm generally discussing was really just trying to discuss the metal tritide issue.

But you're correct, if a worker had a potential for OTB, OTIB-66 would then be the OTIB that would cover taking the urinalysis bioassay and doing an organically-bound tritium assessment.

MR. GLECKLER: Hey, Mutty, this is Brian Gleckler. We actually used the same approach for the OTB and the TBDs in the internal TBD for Pinellas right now, rather than the OTIB-66 approach. That's why in the TBD we used the terms soluble and insoluble tritium, to where the insoluble tritium, and that addresses Type M and Type S, regardless of whether it is a metal tritide or organically-bound tritide.

DR. LIPSZTEIN: Oh. So, the organically-bound tritide would be treated like a tritide, like an insoluble biological?

MR. GLECKLER: Yeah, be it a liquid or a solid.

DR. LIPSZTEIN: Why?

MR. GLECKLER: What's that now?

DR. LIPSZTEIN: Why? Why didn't you

use the ICRP model?

MR. SHARFI: Well, for an insoluble, like an oil mist, an oil mist intake, the organically bound tritium actually is more like a particulate in the sense that it's an insoluble, organically-bound tritium and it actually acts a lot like a metal tritide. This is also covered in the DOE handbook on tritium for metal tritides.

DR. LIPSZTEIN: Okay. Okay, so are you going to add this to the paper or on the tritium example or on the paper on tritides or on the TBDs? Because there is no mention of OBTs.

MR. SHARFI: When we update the TBD, I'm sure we can add some more text to clarify organically bound tritium.

DR. LIPSZTEIN: Okay, thank you.

DR. MAURO: This is John Mauro. I want to take this a little step further so I understand. I'm visualizing a man that is working in an area where he is on the tritium bioassay program. And his dose needs to be reconstructed. From what I'm

hearing, three different types of possible tritium exposures. One would be metal tritides, one would be organically-bound tritium, and the third would be tritiated water.

And the information you have available to you to reconstruct his doses from those three different types of tritium are either swipe samples, and we understand what you are doing there, and those swipe samples from the metal approach would be you take your swipe, you do your measurement, and you assume that the swipe activity is tritides, and that the way you will calculate his internal dose is use the resuspension factor approach.

That is, assuming that all of the activity that's on the swipe that you pick up -- and I understand the surrogate nature of that -- in the water and so forth, but you basically are saying, okay, this is how we are going to reconstruct his doses from resuspended tritides. And you can use the air concentration and so forth.

Then, you also have tritium in his urine. You don't assume that the tritium in his urine is entirely due to the inhalation of tritiated water, and so you add that. So, okay, we just picked up his dose in tritides from the swipe data. We are going to pick up his dose now from tritiated water based on his bioassay data.

And I think where you've just left me now, and this has struck my questions, is now you have the organically-bound tritium. Now, what I just heard you say is that you are going to assume that any organically-bound tritium -- which of the two approaches, where does the organically-bound tritium come in? Does it come in assuming that it doesn't come in? That you are assuming that all of the swipe that you pick up and the way in which you measure it, it will also accommodate the organically-bound tritium somehow? You lost me there. Or somehow is it the bioassay sample of the urine? You see?

MR. SHARFI: It's a combination of

both, John. I mean, the soluble forms of tritium would be accounted for in the bioassay. On top of that, we would assign the insoluble forms, the swipe data, the intake based on the swipe data.

DR. MAURO: Okay, and where does the -- I understand the tritide. I understand the tritiated water. You're saying somehow the organically-bound tritium is going to be captured in this also and accommodated, because it is two times higher than if you were basing it on the bioassay sample. But you're saying you are not going to base -- somehow the organically-bound tritium dose contribution is somehow going to be captured by the way you are dealing with the metal tritides.

DR. LIPSZTEIN: And the second question, to complement John's: are you going to show that what you are measuring in the urine is not the OTB because it has something different from what I see up here recommends?

MR. SHARFI: I mean, obviously, if you

had a huge -- I mean, if we're trying to do a fitted, huge result, and we can look at the tail to fit it. Otherwise, the approach to assessing tritium urinalysis is covered in OTIB-11 and -66, which, at that point, 11 covers the area of the curve assessment assessing tritium bioassay, proportional area in the curve. And in 66, it provides, I think, adjustments. If you have a potential for OBT, and if there is a difference in the biokinetic models, that you can make adjustments for soluble OTBs. Then any insoluble OTB would be wrapped up -- you would basically be assuming that very little gets to the bioassay in any insoluble form and that that would be captured within the assessment of the -- you want to call it the particulate side or the metal tritide and insoluble, the T side.

DR. LIPSZTEIN: And you would think to update the TBD-5 for that? Because there is no mention of this here.

MR. SHARFI: No, the tritium section of

the TBD will have to be updated for all of this. And then even the intakes are going to be updated anyways because we are going to adjust the resuspension factors. So, the intakes are going to change.

DR. LIPSZTEIN: Okay. And because also, even tritiated water, there is no explanation on how you calculate intakes from composite samples. Because apparently they took one week of samples, 1 millimeters of each day, and combine and then measure it. So, there is no explanation also of how you calculate intakes for such composite samples of tritiated water. Or OBT, I don't know if there is OBT there.

MR. SHARFI: I mean, the assessment of soluble forms of tritium are well-covered in 11 and 66.

DR. LIPSZTEIN: Yes, yes. It is just that this is a particular situation where instead of having one sample, they combine seven days' samples and measure it. So, the measurement is for

the seven days composite sample, until 1970.

MR. GLECKLER: This is Brian Gleckler again. I remember this being brought up a long time ago and it's still kind of foggy. And we explained it, I'm pretty sure, at one point in time, but I'm just trying to -- I don't know if anyone else remembers. It goes back to one of the much earlier conference calls, I think, or Working Group meetings.

DR. NETON: Yeah, this is Jim. I don't recall exactly what you discussed, but I think we had put this issue to bed. I think the best thing to do is to focus on this tritide issue and close this issue now and not start treading over old ground.

And we will acknowledge that the TBD needs to be revised to better discuss the individual assignment of dose from organically-bound tritiated water and then the tritides. I mean, we can do that.

But right now, I think that the main

issue of discussion is, can we do these tritide exposures? And I think it would be better if we focused on that right now.

DR. MAURO: This is John. I understand what you're saying, Jim, and I think that is a reasonable approach. This way, we could segregate and take care of the metal tritides in a clean way and we'll worry about the organically-bound and how that complicates the ---

DR. NETON: I think we all agree we can reconstruct doses for organically-bound tritium and tritiated water. It is whether or not -- how much detail we put in TBDs, is an issue. But right now, the issue is can we do these tritides? So, let's see if we can get that solved first.

MR. STIVER: This is Stiver. I'd prefer to do that, too. We haven't even talked about our review yet of the tritide model.

ACTING CHAIRMAN POSTON: So, is everybody agreed on how we're going to proceed here, before we leave this issue?

MR. STIVER: Well, we still haven't even discussed SC&A's review of the paper yet.

ACTING CHAIRMAN POSTON: Okay. Alright.

MR. STIVER: That would be the next item.

ACTING CHAIRMAN POSTON: That's the next item?

MR. STIVER: Yeah.

ACTING CHAIRMAN POSTON: Is everybody willing to go ahead and do that, go to the next item? Yes, no, maybe?

MEMBER FIELD: Yes, this is Bill. I think John's questions have really helped to clarify some things for me. I think that was very helpful.

MR. DARNELL: This is Pete. I agree, we should go ahead and move on to SC&A's review. I just want to make sure I captured this correctly. SC&A wants a TBD update to address how tritides, organically-bound tritium, and tritiated water are

captured individually. Correct? Is that correct or not? Hello?

DR. LIPSZTEIN: Yeah, I think so.

MR. DARNELL: Okay.

MEMBER CLAWSON: John, this is Brad. I'm fine with continuing on and listening to SC&A's response.

ACTING CHAIRMAN POSTON: Okay. Hearing no objections, let's go ahead with the presentation of SC&A's review. Who's going to do that?

MR. STIVER: That will be Bob Barton.

ACTING CHAIRMAN POSTON: Bob Barton?

MR. BARTON: Okay, I guess I'm on.

ACTING CHAIRMAN POSTON: You're up, Bob.

MR. BARTON: Alright. I don't have a typical formal presentation, but I do think it would be helpful to be able to look at some of the tables and figures from our review as I sort of go through what we found here.

And for those of you, members of the public on the phone, I'm going to be working straight off the document that was posted on the website. So, when I say something like Table 1 or Figure 1, you can open that document and follow right along.

MR. STIVER: Bob, this is John. Do you want me to bring it up on Live Meeting for you?

MR. BARTON: I think I can -- if I run into trouble, I will lean on you, but I think --

MR. STIVER: Okay.

MR. DARNELL: Yes, please bring it up on Live Meeting.

MR. BARTON: Okay. For those of you who see Live Meeting, you should all see Table 1. Can everybody see that?

MR. STIVER: That's coming through fine.

MR. DARNELL: Yes.

MR. BARTON: Okay, great. Based on our review, we had seven observations and a single

finding. Basically what we did, the first thing we did was to go and inspect the underlying reference, which is the collection of health physics reports from 1957 to 1973. That's referenced in the NIOSH White Paper as GE 1957 to 1973.

And as I said, it's collection of monthly health physics reports that in some cases would report the highest swipe sample and give you numerical results for the given month. In other cases, it would simply state that the contamination control measures were effective for that period of time.

So, right now, we're taking a look at Table 1, which sort of provides a general summary of that reference. In the left column there, we see the total number of reports on a monthly basis for each of these years. So, for example, for 1957, we have five monthly reports for that year.

The next column over, it may be a little bit cryptic. It says, "complete reports." And

basically what we were seeing in this reference was you would have a health physics report that would give you, for instance, page 1 of 7, page 2 of 7, but you might be missing certain pages in there based on this reference.

The next column over shows the number of smear samples that were indicated to be taken based on what we have in this reference. As you can see, they go up to almost about 10,000 in 1962 and then they kind of decrease a little bit.

And on the far right, we see the actual number of numerical results we had for that year. So, these would have been the actual swipe sample numbers that sort of form the basis for the original methodology that we had there. And there was something, there was only about 40 individual numerical samples that we were able to pull from this particular reference.

So, that was sort of the first basis and the first step, was just to review that reference.

The next thing we did was to go and start

digging into the SRDB. And for those of you on the phone, members of the public, the SRDB is the Site Research Database. Essentially, it contains all of the documents and data that have been captured for Pinellas to-date. So, this is pretty much the extent of what we have to-date.

And the reason we really did this is, subsequent to the last discussions, which were all the way back in November of 2012, there had been a number of reports. I think the number was somewhere around 350 additional references that had been uploaded. Not all of them had to do with tritium swipe data, to be sure, but a number of them did have some additional data.

So, we examined these documents, which were certainly new to us, and really with four things in mind. The first one was we wanted to try to fill in some of the temporal gaps. And let me just scroll down here to kind of give you a visual.

So, this was the original reference. And we can see that this visually shows the number

of monthly reports we had for each of these years. As you can see, starting in 1957, we have about five of the months covered. Some of them are down around three. Some of them have all 12 months covered within that reference. For '68 and '71, we didn't find any actual monthly reports than that. So, that is sort of the reason we went looking, to see if any of those additional documents that had been uploaded could fill in some of the gaps on Figure 1 that we are looking at right here.

Now, the second thing we were looking for is to see, well, this sort of ends in 1973. What can we find after that? So, either additional HP reports or other additional survey data that we can find.

The third sort of facet of this was to really give value weight whether that 4 million dpm per 100 square centimeters was the bounding or the highest observed contamination value at the site.

And the last thing we wanted to do was

really try to get a grasp on the characterization of the HP program: what kind of decontamination efforts were there, contamination control, et cetera. And this is obviously important because it provides a significant perspective and sort of a weight-of-evidence that workers may or may not have been chronically exposed to the levels that are currently assumed in the model.

So, for that first facet, which were the gaps we observed in the original reference -- and I am going to scroll on to -- oh, one more thing before I head along.

In this original reference, which was GE 1957 to 1973, these show those 40 swipe sample results, where we actually had numerical values, and I have them plotted here by date. As you can see, there is sort of a cluster around 1959 to sort of the end of 1960. There's a pretty good gap there. And then there is a smattering of numerical results that we had for '67 on to about '73. And I point out where the NIOSH proposed value falls

in those numerical results from the original reference.

I'm going to move on to Figure 3. And please, obviously, stop me if you have questions as I'm going along.

So, with the additional reports we found in the SRDB, we can see that the gaps significantly improved with many more years having all 12 of the health physics reports. Some of them stayed the same in the early '50s -- or late '50s, rather. We didn't really find any more health physics reports and we still could not find any for 1968 and 1971.

And so that sort of leads us to what our first observation was, and I'll read this into the record.

Observation 1: SC&A identified several supplemental periodic health physics reports that had recently been uploaded to the SRDB and that account for some of the observed gaps in the primary reference, which was forming the basis

for characterizing the tritium contamination at Pinellas.

So, that's the first thing, was to try to fill in some of the gaps we observed in the primary reference. The second thing was that we were going to try to look for some data or some additional health physics reports beyond 1973. Unfortunately, we did not find any health physics reports, or not at least in the form that we observed up until about 1973. But we did find some sparse examples of survey logbooks and smear surveys that had been taken at Pinellas. Specifically, we found some additional data for 1976, 1980 to 1981, '86 to '88 and 1991 to 1994.

So, the second observation here is that SC&A concurs with NIOSH that individual contamination survey results are pretty limited until you get really into the last 1980s as far as having actual numerical results of these surveys. And we could not find any monthly or quarterly health physics reports for anything past the third

quarter of 1973. So, that was our Observation No. 2.

The third facet that I had discussed was sort of characterizing the bounding nature of the value currently chosen for the method, which is the 4.4 million dpm per 100 square centimeters.

Based on what we would find in the SRDB, we essentially went through and just tried to tabulate the highest contamination value we could find, by year, all the way up through 1994. Now, as I said, we found some sparse contamination data for a number of years past 1973, but there are certainly a few gaps there.

And I'm going to move along to Figure 4 here. This kind of shows what we found. And, again, these are the highest observed values that we could find in pretty much all the available documentation on the SRDB to-date. And as you can see here, we have, in green, the proposed contamination value. And as Mutty had indicated, we found a couple of situations in the late '80s

and '90s where we found contamination survey data that actually exceeded this value of 4.4 million dpm.

Just to give you a little back story on what these three values represent, because it's very relevant on how you do dose reconstructions and the whole issue of exposure potential. The first red value there is in 198,8 and that was associated with the removal of a hood or a glove box, which you can imagine that could certainly result in some elevated levels of contamination but would likely be of somewhat limited timeframe, certainly not a full year exposure or a full employment exposure.

The second one there was from 1992. This one actually had consecutive days where they were measuring more on the order of 10 million dpm per 100 square centimeters. And this was for a flow bench and there was a handwritten note on this record next to it that the smear actually came from the rad exhaust hood.

So, again, it raises the question of whether exposure potential really existed in that situation where you would assume there would be some negative air flow. But, again, that value is a little bit higher.

And then the final one here, 1994, that was associated with what was listed as a pipe but based on what we were able to glean from that survey data, it was actually part of some interior tubing that was used in the accelerator that they were removing. So, again, it's sort of almost a D&D activity or a maintenance activity that you wouldn't expect to be a consistent type of exposure scenario.

So, that leads us to Observation 3: SC&A agrees with NIOSH's assertion that contamination values in the millions of dpm per 100 centimeters would have been unusual and likely of short duration. But nonetheless, if the intention was to use the maximum value that you see at the site, we did identify these three years that had

slightly higher values. And that is really just, I guess, for informational purposes. We still certainly agree that the chosen values very likely are going to bound exposures, especially when considered on an annual basis. But again, we wanted to make that information known in case NIOSH wished to use it.

And the fourth facet of our SRDB search had to do with the characterization of the Health and Safety Department. Basically, let's look at how the Health and Safety Department reacted to spills or just contamination found on a routine survey. And we have it in Figure 5. Okay, here's Figure 5. We'll get that out of the way.

Here's one example that we pulled. And as you can see here, it talks about one localized incident of contamination when solution was spilled on the floor in one of the laboratory areas. And I've underlined it here: decontamination was immediately effected.

So, again, this is one example. And

actually, for those of you who are interested, Appendix C contains a number of other examples, where the same thing was identified and they state that it was decontaminated immediately or efficiently or within a few days. You can see the direct quotes from, again, Appendix C has those.

So, that was the basis for our Observation 4, which is that available monthly health physics reports indicate that when contamination was discovered, through either routine surveys or incidents, the area was immediately decontaminated.

We also have some further evidence on that subject: survey logs from the late 1980s and 1990s had also showed evidence of this. And this is going to be sort of a roaming example of Figures 6 through 9. So, I'm going to head there right now.

Okay, here's Figure 6. This is one example of the daily survey reports that we started to see beginning in the late 1980s. And as you can see, they had a value that was significantly less

than what's being proposed by NIOSH, but, nonetheless, above the control value that was for Pinellas. And you can see it was noted and they wanted to recount the sample. And you can note the date, that it was June 1st, 1992 at about it looks like almost 9:00.

So, this one was earmarked to be recounted. So, here's the recount. Again, it's still June 1st. It's about an hour later. They still had about the same contamination level. Actually, it's a little bit higher. So, again, they said, well, let's recount it again. And, again, this time it came in a little bit lower, but still above their control limit. And again, this is still the same day, just a little bit afterwards.

And in Figure 9 here, a couple of things to note here. One, that same area that had been showing contamination in the 3,000 dpm per 100 square centimeter department is now down to 40, which is ten percent of the control level at Pinellas.

And this also indicates, and I have circled here, that they resurveyed after they mopped up the incident or mopped up the spill, whatever it was that was giving that higher result. They mopped it up, resurveyed it, and it came in below the control level. And again, this is still the same day a few hours later.

So, this is one of those pieces of evidence where they detected contamination, and just to be sure, they counted the sample a couple of times, determined that the contamination was real, went back, cleaned it up, resurveyed until it came back to under the control limit.

So, that kind of leads us to Observation 5, which is: SC&A observed evidence in survey logbooks from the '80s and '90s that indicated situations where contamination above the control limit -- which I believe is 440 dpm per 100 centimeters -- was often recounted and then the area was decontaminated and resurveyed.

One more thing along these lines. The

Health and Safety Department, in the reports, actually indicate that they would sort of predict situations, whether it be maintenance activities or what have you, where they said this type of activity is probably going to bring us to contamination levels above the control limit. And so they would take sort of extra actions or send the Health and Safety personnel out there to sort of monitor it.

One such example we have in our report was from December of 1969. And they actually talk about anticipating contamination in several areas. And the highest observed value they had was disassembly of a vac-ion pump. The report notes that for this activity continuous air monitoring was provided by Health and Safety.

I guess the key is that they anticipated it and were taking measures to contain it and monitor the operation in real-time as it was happening. This, to me, suggests evidence that any sort of long-term contamination is rather

unlikely and that they are aware of the activities that would likely cause the highest contamination and took corrective actions, or at least were right there to make sure it wasn't a long-lasting source of exposure potential to the workers.

Interestingly, the value that was chosen by NIOSH, the 4.4 million dpm per 100 centimeters squared, that was part of a maintenance activity on a glove box, and that Health and Safety monthly reported indicated that they had put extra controls in place and that those controls had effectively limited that contamination to the room where the activity was happening, though they actually admit that the levels were significantly higher than they anticipated.

Finally, there was also a 1982 report or procedure that the activities that were expected to produce contamination above the control limit should use paper, essentially, along the floors and work surfaces. And I'm going to scroll to that so you can see the example.

Here we go. So, as you can see, there were sort of three steps that were outlined in this procedure. You want to tape the work surfaces and floor areas that were in the vicinity, tape them off so they wouldn't come back up. Once the work was done, they would remove the paper and fold it in a specific way, put it in a bag and dispose of it. And then after that was done, they would have Health and Safety come in and do a contamination survey. So, again, this is another piece of evidence that sort of leads us to believe that any sort of high level contamination was likely not there for very long.

So, that leads us to Observation 6: Based on the review of available health physics reports, it appears that Health and Safety staff recognized the situations that posed an elevated threat to tritium contamination above the control limit and they took precautions to minimize the potential exposures.

So, those were sort of the four facets

that we really focused on going into this, is basically what data do we have to characterize the contamination and what indications do we have about the operation of the Health and Safety Department as far as how quickly these things might have been detected and cleaned up.

But we did also look at some of the other facets of the approach that Mutty talked about. These are choice of a resuspension factor, the 5E to the minus 5. The measurement system that was used to be able to detect the tritium contamination from these smears. The solubility type of the actual contaminant being assumed. The breathing rate of the worker. And the actual annual exposure time for the worker.

So, just to quickly go through these, I think we can probably start with the easy ones. The resuspension factor, again, this is a factor that was chosen to be consistent with Mound.

And John Mauro, if you are still on the line, I know you did a lot of research related to

the selection of resuspension factors at Mound, and certainly in other places. I don't know if you want to add a few comments on that particular assumption.

DR. MAURO: A little bit. The very fact that this accommodation was made to increase it is favorable. Under the circumstances you just described, you could visualize that 10 the minus 6 per meter resuspension factor would be used after the area is cleaned up. Before the area is cleaned up, the resuspension factor may very well be close to 10 to the minus 5.

So, the story we just heard is that, yes, for some short period of time, freshly deposited tritides -- that is what we are talking about, of course -- and probably it's mostly not tritides -- it's probably mostly tritiated water. But assuming it is tritides, if it did have a higher E suspension factor because it was freshly deposited, it would move toward a 10 to the minus 5.

Of course, once it's decontaminated, you will have reduced the likelihood of its being able to be resuspended, and the 10 to the minus 6 would be more favorable.

So, my takeaway from this is that since you're using the 4 times 10 to the 6th dpm per 100 centimeters squared coupled with 5 times 10 to the minus 5, it is quite claimant-favorable from many perspectives.

And the one last thing I think I might want to add to this is something I looked into a bit, is all of our resuspension factor information that we have in the literature really deals with airborne-deposited activity, like uranium and plutonium and other metals and metal oxides. And what we are dealing with here is something a little bit unusual, you know, a metal tritide. I don't think it's happening, but you mentioned that it might have been some other types of metal tritides.

And one of the things I was thinking about was, is it reasonable to assume that the

experience in resuspension factors that we have collected, and there is a vast amount of experience on internal resuspension factors, which heavily deals with things other than metal tritides, would apply to a metal tritide.

And what I did is I made a phone call, and the fellow I spoke to agreed that I could mention his name: [identifying information redacted], [identifying information redacted]son, he's a recognized expert in aerosol physics. And he was at Los Alamos and he said it's okay if you need to mention his name during this meeting.

And I talked to him a little bit about this question, is there anything about a tritide, something he's familiar with because he did work on Mound during the decontamination operations. And his sense is that he believes that the metal tritides, as a particulate, would behave very much like any other particulate, as long as the particle size distributions are more or less the same, from the experience in other metals.

So, I guess my takeaway, and this might help a little bit, is a great excess of assurance that the overall strategy adopted by NIOSH -- and, of course, we will be talking about this some more, though, with respect at least the resuspension factor -- rings true and is claimant-favorable. I hope that helps a little bit.

MR. BARTON: Thanks, John. Okay, so that was the resuspension factor.

Another one to consider is the solubility type. Now, the NIOSH White Paper had concluded that, to be claimant-favorable they would always consider the tritide exposures to be Type S or a very insoluble type compound. And we feel that is likely to be claimant-favorable in most situations, especially when you are talking about doses to the lung and such.

But I did have a question. When we looked at the example DR because in that, it appears that both Type S and Type M were evaluated and it looked like Type M was bounding for some of the

hypothetical organs. So, it seems like the solubility type changed based on what organ was evaluated. And I guess my question is, would that be standard practice that both Type M and S would be evaluated and each organ would be selected separately or would it be the cumulative effect of one solubility type or the other, or, is it as the White Paper says that it should just be assessed as Type S solubility in all cases? So, that was one question because it seemed to be a little bit confusing between the White Paper and what we were seeing in the actual dose reconstruction example.

So, I don't know if that is a question for Mutty or for Jim.

MR. SHARFI: Yes, I mean you should assess which would be more claimant-favorable for the specific radionuclide. I mean I think, and this is probably just a natural -- I think we generally, when we talk about the metal tritide, we considered the Type S because it is the most hardest. It is in the urine. Dosimetrically, it

makes the biggest difference from the lung -- I mean the drop difference in the lung is huge between the M and the S where inside the systemic organs, there is not as big of a dosimetric difference.

So, I think that is probably just my fault in terms of the response paper just naturally gravitating toward the Type S. But no, you would look at what is most claimant-favorable from a solubility perspective between M and S.

Now, F you would not consider because F would quickly move into the urine and be treated no different than HTO or HT.

MR. BARTON: Okay. And just a follow-on to that. Would you, say, for an individual, would you be restricted to selecting the one I guess solubility type that gives your highest cumulative Probability of Causation or does NIOSH actually, for each organ evaluated, could select, feasibly select a different solubility type and add those together?

MR. SHARFI: Well, your exposures to

one material are no different than uranium and you had multiple cancers. If you had a lung cancer and a bone cancer, you wouldn't give the lung cancer Type S and the bone cancer Type F just because the material is either one or the other when you inhale it. So, you can't inhale both mixtures and double compound. That would be double dipping on the solubility.

So, you have to determine overall, is Type S overall more claimant-favorable to the claim or is overall all Type M more claimant-favorable to the claim.

MR. BARTON: Okay, thanks for that clarification.

DR. LIPSZTEIN: Can I come in just one second? The only thing that matters is lung. The other organs, it doesn't matter. The dose is so small.

So, I think the important thing is to consider Type S for the lung. Otherwise, the dose is very, very small. You can see from the example,

the dose is very small, and if you did with Type M or Type F, the dose to the other organs would be very, very small. The only amount that counts is lung.

MR. SHARFI: I agree dosimetrically the respiratory tract is where the more dosimetric concern is.

DR. LIPSZTEIN: Yes, otherwise, it doesn't make a difference. The dose is too small.

MR. SHARFI: Yes, I believe there was a difference in M and S if you look at like the liver, I think it was like 9 versus 7 millirems. And dosimetrically, it is very -- there is a much smaller difference in the systemic organs than, obviously, the respiratory tract.

DR. LIPSZTEIN: Yes. Yes, the only thing that would really give a dose is lung. So, Type S is the most claimant-favorable.

MR. BARTON: Yes, I agree with that, Joyce. I think what we were mainly talking about was, for example, if you had a worker that didn't

have necessarily a lung cancer but did have a liver cancer, you would still have the option to use Type M. Even though it is a small dose and a slight difference, you would still be able to have the option to use Type M, if that, indeed, was most favorable to the claimant. And that was confusing between the DR report but I certainly see where Mutty was coming from. And sort of the knee-jerk reaction is to think lung, just like you said, because that is really where the dosimetric significance is.

So, that did clear it up for me. Thank you.

MEMBER CLAWSON: Bob, this is Clawson. I have just got a question for Mutty.

And I understand why you have done, and I agree with how you have done this, but looking at it from the dose reconstruction, is there something in the tools that is going to allow the dose reconstructor to be able to understand that he can do this?

MR. SHARFI: I mean this is, like I said, no different than if you had a thorium or a plutonium intake. In all cases, if you have the possibility of two solubility types, you always consider both and you assign the more claimant-favorable to the claim.

MEMBER CLAWSON: Okay, I was just wondering because we just, from my other one yesterday, the dose reconstruction, we were just getting into some of the tools.

MR. SHARFI: Yes, this isn't tools as much as it is just -- I mean you would run M and then you would run Type S, I mean if you had a lung cancer.

I mean generally, I would say this is probably a little more intuitively obvious that any respiratory tract is likely to go Type S and any systemic organs likely are going to be Type M. But I believe the question is more of what if you had both. What if you had a liver and a lung cancer, then can you mix them? And I would say per the

OTIB-60 in the sense of how we do it in terms of dosimetry, you have an intake of one or the other. You can't have an intake of both. More likely because you have the lung cancer and it is more dosimetrically significant to the lung cancer, you would likely assess them both as Type S because the difference in the liver is very small. So, the difference in the lung is very huge. So, it would be more claimant-favorable to assume a Type S intake and assess all organs Type S than it would be to assess them all with Type M.

MEMBER CLAWSON: Okay, thank you very much. I appreciate it.

MR. SHARFI: Sure.

MR. BARTON: Okay, if there are no questions on the solubility type, the next sort of factor, this was the breathing rate and exposure duration.

The breathing rate was chosen as 1.2 cubic meters per hour, which is pretty standard. I think the ICRP classifies that as sort of a light

labor category, which would certainly be indicative of sort of laboratory type work.

The exposure duration, which Mutty already pretty much cleared up for us and was the source of our sole finding on this was whether you assess it over a 2,000-hour work year or the 2600-hour work year and the TBD and the dose reconstruction example that you provided had the 2600. But we had saw the 2,000 in the White Paper so it kind of got us confused but it sounds like that was probably just a typo and it will be assessed as 2,600 hours. Is that correct?

MR. SHARFI: Yes, that was done in the example DR and 2600 hours will be used.

MR. BARTON: Okay, great. As I said, that was our sole finding because there seemed to be a little bit of a disconnect between the White Paper and the TBD. But NIOSH is electing to go with the longer work year, which is 50 hours per week.

MR. SHARFI: Yes, and I will correct it. The TBD does use 2600 hours. I just, for some

reason, put 2,000 in the response paper.

MR. BARTON: Understood. Alright, well, that definitely clears it up.

The last thing here is the ability of the system, the measurement system to be able to actually detect tritides. And this was touched on certainly during Mutty's presentation.

The White Paper, itself, mainly concentrates on the filtering step, which is the Whatman #1 filter, and NIOSH demonstrated that the actual pore size for that would really only restrict particles that were 10 to 12 microns, which is really out of your respirable particle range. So, we didn't really see that as a problem.

The second part was this issue of whether, if you are swabbing up stable metal tritide particles with a cotton swab and then rinsing the swab, we were concerned about whether the tritides would get sort of trapped within the cotton swab and never really make it to the counting liquid to be registered in the contamination

survey.

John Stiver, I know that you had brought this issue up. I don't know if you want to add a little bit to that characterization.

We also queried NIOSH when this came up to see what was there and NIOSH provided a response. So, John, I don't know if you want to add to it or if we should --

MR. STIVER: Yes, sure, I can add a little bit to it.

This came up I know when we looked at Mound back in the 2012 time frame, the final version, there was an extensive back and forth on that. And there are a couple of material differences, one, obviously being the amount of data that was available for swipe samples at Mound versus the summary reports that are available for Pinellas. We understand obviously why NIOSH would go with the higher value for Pinellas in order to make sure those uncertainties are captured and bounding.

The other thing was, at Mound, they used the PC5 proportional counter and they would take a swipe and they count the sample directly. So while you are probably obviously going to have predominately HTO, tritiated water that is being counted. If there was some component of tritide in there, that would also be counted.

So, we can say, alright, you know we are going to have to assume it is all 100 percent tritide because you are actually directly counting some of that if it is there. We felt that was probably a claimant-favorable decision and a reasonable decision to use, especially considering that we were taking the 95th percentile of a chronic exposure.

And you know at Pinellas we are throwing in one more layer of uncertainty here. You are taking this cotton ball, you are swiping, you are rinsing it out into a paper cup and then counting the rinsate through a liquid scintillation apparatus. And we were starting to think well, you

know what happens if you have all this tritide captured in the swab and it never makes it out? And you know, looking at the health physics paper and Mutty's description, we found, you know if you are dealing with fresh tritides, freshly produced, which is what we have at Pinellas, obviously, you are basically depositing the vapor, the scandium or hafnium or what have you on the ceramic or a metal substrate and you are loading tritium onto it, that is about as fresh as you can get.

The question would be well how long were these tubes kept around on-site. Ones that did implode and caused a spill or caused contamination, how old were they? Were the contaminations cleaned up quickly? And our sense is that it is probably reasonable to assume that they are fresh.

Bob had described in detail how the health physics program is very responsive and aware of, A) what procedures were likely to result in the contamination events and the responses when those did occur in cleaning them up quickly.

So, I don't think you have a situation where you are going to have tritides sitting around on a workbench for days, months, years, and never cleaned up.

So you know you have a situation where with a fresh tritide more of the tritium is going to off-gas. I think the paper showed that it was about a factor of two to three more than what was present in particulates, based on the way they did their counting in the sample.

And so even though you were using a surrogate, in a way, by counting what we are almost, it would probably be tritium or gaseous which would then convert to a tritiated water. It does provide a bounding surrogate count and this is based on what came off of the source term itself. So, it is kind of a secondary step. It adds more uncertainty. But given the weight of evidence by the quality of the health physics program, the incident reports that show things were cleaned up quickly, the fact that things are being produced for distribution

throughout the complex, we felt that NIOSH was probably on pretty solid ground with this as well.

So, that is all I have to say about that.

DR. NETON: John, this is Jim. I have got a little bit of a clarification on that new versus old tritiated substrates.

The paper actually made a differentiation, not between new and old, but new and used as in the tube had been run.

MR. STIVER: So, it actually had been discharged a few times.

DR. NETON: Yes, I mean the column is milliamp-minutes and they would run for like a couple hours or an hour or so. And I forget the ratios over time but it certainly seemed to be correlated with how long the tube was actually run, not how old or how --

MR. STIVER: Okay. I was under the impression it was more a reflection of age.

DR. NETON: No, no, it was whether they were used or not. So, I think you know they didn't

obviously analyze new versus sitting on the shelf, old. But at least their paper differentiation was based on tube usage. Just as a clarification.

MR. STIVER: Okay, thank you.

DR. MAURO: I'm sorry to interrupt. Jim, this is John. My understanding also has to do with age, when I looked at that Health Physics paper. So, we are not dealing with tritides that -- where the tubes were run. This is something I am not familiar with. We are dealing with tritides where the tubes -- where if there was a spill, it wouldn't be a spill from whatever a tube is, was not run.

DR. NETON: Right. This is where they are being manufactured. I mean they are actually making, they are installing the tritium onto these metals.

DR. MAURO: Got you. Okay, so that makes for an even stronger case.

DR. NETON: Yes, and in many of the years, I forget the break point but the tubes were

actually, the metal material, the substrate was actually charged with it in the tube and, in some years, it was not. But yes, it really is, these are being made at the plant, not somewhere else.

DR. MAURO: I understand and that is even more assurance that the surrogate approach will work. That is my takeaway from what you just said.

DR. NETON: I prefer not to call it a surrogate approach. That has sort of a specific meaning in this program.

DR. MAURO: And I agree with you completely.

DR. NETON: I would call it an indicator applied or something to that effect.

DR. MAURO: I understand completely. An indicator of tritide metal. Yes, thank you.

MR. BARTON: Okay, well that topic was really the subject of our last observation, Observation 7. And as I said, when it came up, we really weren't sure if it had been considered. And

so we queried NIOSH and they responded and we at SC&A had a pretty lengthy discussion of that issue leading up to this meeting and we were certainly satisfied with NIOSH's response. I don't know if any of the Work Group Members would like to ask questions on that particular topic or on our review as a whole.

Just to summarize, we had seven observations. So, basically it boils down to the contamination value chosen may not be the absolute highest. There are at least a few, a handful of samples that we have done that were slightly higher than that but they were also in situations where it is really quite infeasible there would be a long-term exposure to that level of contamination. We feel that any of the other parameters chosen are certainly claimant-favorable. And our assessment of what we can tell about the health physics program is that any sort of spill or contamination, whether it be regular or incident-based, would have been picked up pretty quickly and decontaminated.

So, pretty much on that basis, we only had that one finding concerning how many hours per year was going to be assessed and Mutty cleared that up. The higher exposure time was 2600 hours per year.

So, that really concludes our review. I would be happy to answer any questions that the Work Group might have.

ACTING CHAIRMAN POSTON: Any questions? Any questions for Bob?

MEMBER CLAWSON: This is Brad. I have just got one question for John Mauro.

I received an article about the study of particulate gas. Was this covering what you talked about with the gentleman? Was this part of that information?

DR. MAURO: Yes, well, I'm not sure the article you are referring to. But my concern was this broad, sweeping generalization regarding resuspension factors, you know this 10 to the minus 5 versus 10 to the minus 6, which we have all

resolved. We know when to use 10 to the minus 6 and when to use 10 to the minus 5. And within that context that is exactly, it was handled appropriately here.

But I was asking, I guess, a little deeper question. And that is the whole idea of using a resuspension factor from the literature, two metal tritides, something that we haven't encountered before and certainly I have not seen nor heard of any literature that specifically looked at that question and is there anything about a metal tritide, which is simply a metal particle with I guess a hydrogen attached to it.

And I mean this is only my thinking about it. I said, gee, I wonder if there is any reason to believe that those types of particles would behave differently than the particles that are the basis for all of the resuspension factor issues data that we currently use.

So, as I mentioned, you folks may not know [identifying information redacted}. He is

the son of [identifying information redacted], whom everybody knows. But he turns out to be, he was recommended --

MR. STIVER: He, John, can I jump in for a second? We probably shouldn't be using those individual names in --

DR. MAURO: Oh, he said it was okay for me to use it during the meeting.

MR. STIVER: Yes, but I think it is more of a procedural aspect of the program.

DR. MAURO: Okay. He is just a knowledgeable person. I just wanted to let you know I called someone very knowledgeable on this subject and that happened by my making some inquiries about who was the world's expert on the subject.

MEMBER CLAWSON: Okay, well, I was just wondering, John, because actually John Stiver sent this article out and it was interesting reading. We just kind of covered what we were there on the fact.

But I would like to take the opportunity to tell Bob Barton and also Mutty that they did a very good job on this process here. It was very clear and to the point and I appreciated it.

ACTING CHAIRMAN POSTON: Any other questions?

MR. DARNELL: This is Pete. I would just like to summarize. I guess what I am hearing is that SC&A agrees with the example DRs as they are written, with the exception of the TBD update that we talked about earlier. Is that correct?

MR. STIVER: Yes, it would just be a treatment of the OVTs, some kind of an explanation of it.

MR. DARNELL: Okay.

MR. BARTON: Well, this is Bob. I did have one question that is sort of -- well, it is related and it really comes down, so I guess it sort of an implementation question because these tritide doses are going to be applied to workers who were in the bioassay program.

And I guess I am curious because I don't think, at least I haven't looked in-depth at the Pinellas bioassay program but what threshold or example threshold would it take to determine that the worker, I guess, qualifies for tritide? And let me just give an example, say you had a worker who was monitored, I don't know, maybe on a weekly basis and then maybe there is a couple of months without any bioassay samples. I mean, is the tritide exposure directly tied to when the bioassays were taken or how would that work, I guess, in practice?

MR. SHARFI: If he is being assessed or if he or she is being assessed for any soluble intake via bioassay, then they are given the corresponding intake of metal tritides.

MR. BARTON: Okay, I guess my main question was when you, I guess, observed a gap, say, in the bioassay results you have, whether that is a couple of weeks, a couple of months, how that situation would be dealt with. That might be too

specific for this forum but I am curious about implementation.

MR. SHARFI: I think that I would have to see the particular claim to know all the extenuating circumstances. Like for instance, they could have in their record they are on leave for two months and that is why there is no bioassay. So, without knowing the specifics, it is hard to answer a question like that.

MR. GLECKLER: It might be an easier one for me to address. This is Brian Gleckler again.

In the claims typically what we will do is metal tritide exposures will get assessed for the periods that they have bioassay monitoring and we will go back to the -- I can't find it but we will go back a month prior to that bioassay sample.

And so, like, if there is a gap, we can have gaps in their tritide assessment.

MR. STIVER: So, it wouldn't be, obviously, on an annual basis. If you had a gap

for a year, then you wouldn't include it. It would be any gap in that routine program, then?

MR. GLECKLER: Correct. One thing that is indicative of the Pinellas Plant claims, because a lot of the workers spent like 20 to 40 years at the plant and they moved around from job to job, and as they changed their job, it is like their monitoring was modified and sometimes they were monitored for just external, sometimes external and internal or just internal or not at all. Because a lot of -- a good chunk of the plant was non-rad type work also.

And so they were constantly changing their monitoring as the years go on. So yes, we will have like a year where there is no bioassay. They won't get an insoluble tritium or tritide dose assigned for that period. It is only when they have bioassay data for those periods. Does that make sense?

MR. BARTON: And you said the grace period is essentially like a month prior.

MR. GLECKLER: Yes, I'm trying to find that. I'm pretty sure that's --

MR. BARTON: Okay, thank you. I was just curious how this particular model was going to be implemented based on that criteria. Thank you.

ACTING CHAIRMAN POSTON: Any other questions? We have been going almost exactly two hours. I'm suggesting we take a 10- or a 15-minute comfort break.

MR. STIVER: Sounds good to me.

ACTING CHAIRMAN POSTON: No objections?

MEMBER CLAWSON: No objections, John.

ACTING CHAIRMAN POSTON: Okay. Well, let's just call it -- I am in Central Time. I have 2:00. Let's be back at 2:15.

(Whereupon, the above-entitled matter went off the record at 2:59 p.m. and resumed at 3:16 p.m.)

ACTING CHAIRMAN POSTON: Now, we are

down to number five on the agenda.

MR. DARNELL: John, if you would, I have a question before we move ahead.

ACTING CHAIRMAN POSTON: Yes, I like Jack Daniels.

(Laughter.)

MR. DARNELL: I'll send you a bottle.

ACTING CHAIRMAN POSTON: Just kidding. Go ahead.

MR. DARNELL: Do we have enough Board Members to vote on whether or not we are going to close the issue for metal tritides or are we going to wait until the TBD is updated?

The reason I ask is the Board meeting is in Tampa and it would be nice to get there and say yes, we are done. I don't know how we want to proceed.

ACTING CHAIRMAN POSTON: Well, you will have to ask the Designated Federal Official.

MR. KATZ: No, it's fine. That is why Bill Field is sitting in for -- first of all, it

is a Work Group so it is not the same as a subcommittee. But that is why Bill Field is sitting in in Phil's place, in effect, although not chairing.

MEMBER FIELD: Yes, trying to fill in for Phil is tough.

MR. KATZ: Yes.

MR. STIVER: Ted, this is Stiver. I have got a question for you. Say if the Board votes -- or the Work Group recommends to accept this, doesn't it still have to go to the full Board at the Tampa meeting?

MR. KATZ: Yes, just to get ahead of ourselves, I mean once we have wrapped up all the issues, the Work Group will then make a presentation to the Board at the meeting in Tampa in March.

MR. STIVER: Right, that is what I thought.

MR. KATZ: Right, and just as a preview, John, we will probably ask you to do a lot

of presenting for John, who is sort of interim Chair here.

MR. STIVER: Okay, that's fine with me.

MR. KATZ: Yes.

ACTING CHAIRMAN POSTON: Yes.

MR. KATZ: So, anyway, to answer whose question --

MR. DARNELL: Pete.

MR. KATZ: -- Pete's question, yes, I mean the Work Group should be getting concurrence on the few issues that are still open. The hope was to be able to close the issues and present to the Board in March.

ACTING CHAIRMAN POSTON: You know what they call Tampa? Tampa is the home of the newly wed and nearly dead. And the main population, they have a huge population of old people and they tend to get married the third or fourth time.

(Laughter.)

ACTING CHAIRMAN POSTON: That and 25 cents will buy you nothing.

(Laughter.)

DR. NETON: This is Jim. I guess one other point of clarification, if these issues are voted to be closed, I still think that they will probably end up still being in abeyance, is that not correct, until --

MR. KATZ: Yes, that is correct. Right but that is nothing getting in the way of being able to present and report out.

DR. NETON: Yes, I just wanted to make sure people understood that we are not saying that this is -- you know that we are completely done with all this. It is just that we agree -- they are going to vote to see if we agree on these. A path forward, I guess, is what we are saying.

MR. KATZ: Right. I mean in cases where you actually put it in abeyance because you agree on exactly what is being put forth, I mean it is as good as closed in terms of reporting out to the Board.

ACTING CHAIRMAN POSTON: Are we ready?

Are we ready to go ahead?

MR. KATZ: Yes.

ACTING CHAIRMAN POSTON: Okay? Let me look at who is next.

MR. KATZ: So, John, I think --

ACTING CHAIRMAN POSTON: Brian.

MR. KATZ: John?

ACTING CHAIRMAN POSTON: Yes?

MR. KATZ: I think Pete was asking -- I mean you guys have just completed the discussion with the one finding related to tritides. And I think Pete was asking for the Work Group's consensus decision on it.

ACTING CHAIRMAN POSTON: Well, I understand but normally, it is not up to the Chairman to make a motion.

MEMBER CLAWSON: Then how about if I do, John?

MEMBER FIELD: And how about if I second it?

ACTING CHAIRMAN POSTON: Okay.

MEMBER CLAWSON: I move that we accept NIOSH's process in what is covering the tritides.

ACTING CHAIRMAN POSTON: Okay, Bill, is that what you are seconding?

MEMBER FIELD: Yes.

ACTING CHAIRMAN POSTON: Is there any discussion? It seems like we have already had that, haven't we?

All in favor, say aye.

(Chorus of aye.)

ACTING CHAIRMAN POSTON: Anybody opposed?

(No response.)

ACTING CHAIRMAN POSTON: Okay, so the motion carries and I assume it will be in the record.

Okay, anything else before we move on?

(No response.)

ACTING CHAIRMAN POSTON: Nothing? No other issues?

MEMBER CLAWSON: No, I think we just

move on on the agenda here, John.

ACTING CHAIRMAN POSTON: Okay. So, next we are going to talk about problems with personnel monitoring. And how is going to do that, Zlotnicki?

MR. STIVER: This is John Stiver. Joe, are you on the line?

MR. ZLOTNICKI: Yes, I am here.

MR. STIVER: Yes, this is Joe Zlotnicki. He is an associate at SC&A and a former vice president of the Landauer Corporation and he is going to talk a little bit about the appropriate limit of detection for film badge dosimeters used in the post-'74 period. This is that sub-issue of Issue 5.

ACTING CHAIRMAN POSTON: Yes, okay, the floor is yours.

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MR. ZLOTNICKI: So, let me just give a little bit of background. This issue of what the minimum detectable for film dosimetry is and was

has come up at a number of sites. And the nature of film dosimetry, as I am sure all of you are aware, is that it involves a chemical process and it is almost like a natural product in the sense that it aged over time and fogged as the emulsion or the badge was stored, often for a period of up to around six months. And so one of the challenges is, when we talk about the limit of detection, are we talking about when the processing and the material and everything is pristine or when something has aged a little bit and everything may not be perfect?

And one of the factors is that the back of the report that Landauer put out always claimed 10 millirem minimum detectable regardless of photon energy. This is for photon only but for photon, 10 millirem regardless of photon energy.

But it turns out if you are at americium-241 60 keV, you are going to have no problem seeing that, even with fairly old film. But with high-energy gammas up around cobalt-60, for example, at 1 MeV or a little more, that is a

rather hard thing to see because the responsiveness of the film emulsion is down by a factor of 20 or 30 from the optimum in the sort of 50 keV region.

So, Landauer claimed a 10-millirem minimum detectible or limit of detection. They don't mean quite the same thing but for this discussion, they can. The issue is, is that true for high-energy photon fields? And I think the answer is no, although it could have been on a good day. And one of the issues is that the way that the film densitometers worked and the response of the film to photons, a single step on the densitometer was something like 6 or 8 millirem of high-energy gamma. And given all the noise and uncertainty with the background fog on the film that was subtracted as part of the normal sort of laboratory process, it is rather difficult to claim that for high-energy gamma, one could see 10 millirem. I think 20 is more realistic and has been used or suggested at least at some of the other sites.

I think that, certainly for the lower energy the 10 is fine, which is most of the Pinellas site but for those that were operating with what were colloquially known as the SNAP generators, the plutonium-238 thermal generators, that was a very hard gamma field, primarily, think of it as a 1 MeV photon field and I think that the dosimetry in general would have had a hard time being 10 millirem. It wouldn't have been trivial to see 20, by the way, but I think 20 would be reasonable.

So, I wrote that up in a report everyone should have seen because we were fortunate enough to get an old NASA report that looked into the spectrum emitted from these generators with a view to the impact to the -- the instruments on the satellite payload because they had a number of ion or radiation sensors so they were very interested in that aspect. Not a radiation damage to purely -- when you are trying to detect low levels of radiation, they had a similar problem to personnel dosimetry.

So, we got a lot of good data but the spectrum was hard and for those workers who were primarily exposed to these radioisotopic generators, their limit of detection should be 20 millirem, notwithstanding that the back of the Landauer report does say 10 millirem.

I don't have anything more specific to say but I am happy to answer questions.

ACTING CHAIRMAN POSTON: Well, I think, based on my experience at Oak Ridge, in fact we used 25 generally without really worrying about the different spectral, the different energies. It was just typical to use 25 as the lower limit of detection with the film. You guys did a better job than we did, I guess.

MR. ZLOTNICKI: Well, let me just jump in and say one of the issues is how fresh your film is and how willing you are to throw away the film and start with a new batch, rather than run with the emulsion until it is all used up. So, part of it is just a cost question of not trying to hang

on to it for nine months, for example.

Also, storage conditions. Oak Ridge, they presumably were using refrigerators but certainly once the badges are out in the field, they are not refrigerated.

ACTING CHAIRMAN POSTON: No, they are not and a lot of times they are stuck over the visor in the car and all kinds of different environmental situations. But we are talking 7,000 badges, though. And so I don't know what the schedule was on buying film but that was what we used. So, I would say 20 seems to me to be reasonable.

Anybody else have comments?

MEMBER FIELD: This is Bill. I think that makes a lot of sense, given what we just heard.

ACTING CHAIRMAN POSTON: Okay. Brad, anything?

MEMBER CLAWSON: Sorry, I was trying to get off mute. No, that sounds fairly reasonable to me.

ACTING CHAIRMAN POSTON: Yes, we need

to invent a phone that tells you when it is muted or something, have a little light on it. I have the same problem.

MR. DARNELL: John, this is Pete Darnell.

ACTING CHAIRMAN POSTON: Yes, sure.

MR. DARNELL: We actually have a response and Matt Smith is going to lead that. Matt, are you on the phone?

MR. SMITH: Yes, I was just kind of waiting for the cue.

MR. DARNELL: I think now is the time.

MR. SMITH: We took a look at that NASA report as well and we certainly noted that the age of the RTG really does affect the spectrum that you would expect to see. And I know that report is not up on the website. But for folks who might happen to have it in front of them in electronic form, it is page 54 of 191.

That section that is speaking about the cases of films and then refers to a Table 4-9 for

spectral information, and Table 4-9 is on PDF page 57 of that report. And certainly for fresh or one-year-old fuel, the gamma energies are well below 250 keV. They are down in that lower energy range or mid-energy range, as we call it on this program.

Brian Gleckler was able to dig through the SRDB. That, again, is our collection of documents relating to the site. And the SRDB number that proves very helpful in this situation is number 12185. And on page 5 of that document, there is a nice inventory statement and basically at that stage, there was a 1988 inventory. They had 210 RTGs in the production inventory and 200 of them were one year or less in age. The other ten were two years old.

So, with 95 percent of the production inventory at age one or less, we see the spectrum in this case as not being hard. It was a production facility. Things are not encapsulated ready to go on Pioneer 10 and, in addition to that, the fuel

itself is in a very young condition.

So, from that standpoint, the 10 millirem would still seem valid to us.

ACTING CHAIRMAN POSTON: I was looking at this letter from SC&A and on the back, there is a graph that shows the photon energy.

MR. SMITH: And I believe that is for conditions where things have been encapsulated. In other words, that is the RTG unit in whole ready to go.

ACTING CHAIRMAN POSTON: Okay. Alright.

MR. SMITH: And then of course, from the standpoint of the NASA, especially the researchers who didn't want the interference from all those different types of radiation being emitted, they were looking at what things you know five, ten years down the road, in terms of timing as well.

We did focus on Table 4-9, which is giving the gamma emission for different RTG fuel

age.

ACTING CHAIRMAN POSTON: Yes, the letter actually -- maybe I didn't get it all. The letter indicates there was a table but it is not in the --

MR. SMITH: I don't believe that table is in the memo from SC&A.

ACTING CHAIRMAN POSTON: Yes, it mentions a table but I don't see it.

MR. ZLOTNICKI: This is Joe again. I put Table 4-16 in the letter, not Table 4-9. One of the issues is how much -- obviously, if you look at the raw emissions, there is a lot of low-energy emission. The question is what gets out once it is in the form of a lump of fuel, if you will, that is shielded for heat shielding and for radiation shielding and then it is a very hard spectrum. Clearly, when you have the raw material spread out on a surface and the low-energy photons are able to escape, then of course, there is a lot more low-energy photon production than high-energy.

So, the question becomes, in the handling and manufacture, were those low-energy photons shielded by the construction of the glove box or the remote handling facility, however it was done. Were those low-energy photons removed from the spectrum anyway by a spill box or what have you? I don't know the specifics on that.

But once the fuel is sort of assembled, if you will, it is a very hard spectrum.

MR. DARNELL: This is Pete. We've actually had discussions on this, on how the fuel was handled and what was going on with it that Gil explained to us years back. I forget which meeting it was actually at. But the workers who wore thermal gloves handled the material. They were in direct contact with it. There was nothing between them and the sources.

MR. ZLOTNICKI: So, I think one of the problems with Table 4-9 is that that is the theoretical emission from the fuel but it is the emission in the fuel. It is not the emission from

the surface of the fuel.

ACTING CHAIRMAN POSTON: I just don't have the table at all.

MR. ZLOTNICKI: Oh, sorry. Well, I'm looking online at Table 4-9 that was just mentioned. It is not in the document that SC&A sent out.

ACTING CHAIRMAN POSTON: Okay.

MR. ZLOTNICKI: The point is, if you take the plutonium and all the daughters and any spontaneous fission products and so on, you can calculate exactly how many photons are emitted for various ages. And I think that is what Table 4-9 does. But that doesn't say once you have lumped it together as something that is hot, hot enough that you need thermal gloves, that is a large lump, if you will, a large object. And way more than 99.9 percent of those low-energy photons never make it out of the object.

ACTING CHAIRMAN POSTON: I understand. Well, I'm not sure where to go on this, since we

have beta --

MR. STIVER: Look at Figure 4-8, I believe on page 2 of Joe's report. It gives you an idea of the number gammas emitted from the RTG ratio, the number emitted to number produced. You can see that below about 500 keV, there is only about ten percent.

This is a situation where, yes, sure, there is a lot of low-energy gammas being emitted in the unshielded fuel but once you have it triple encapsulated and also into the RTG body, you are hardening the spectrum. I mean a lot of those low-energy emissions are never going to make it out. That is really the point that I think Joe is trying to make here.

Sure, there is going to be a higher number of or a higher proportion of the spectrum will be higher energy over time. But even in early times, when there was a preponderance of low-energy emissions, most of those low-energy emissions will never make it out of the RTG but you still have a

hardened spectrum in terms of the dosimeter response.

ACTING CHAIRMAN POSTON: Yes, and you can argue that all you want. That is not the question. The question is what is the lower limit of detection. Is it 10 or is it 20?

MR. STIVER: Yes, I mean that sensitivity of the films will depend on the energy of the emission.

DR. NETON: This is Jim. That is a question I had. Aside from the fact of what is the spectrum, what did Landauer calibrate those badges with? I thought they used something like cobalt-60 to calibrate the badges.

MR. ZLOTNICKI: If you go back far enough, it could have been radium or cobalt-60.

DR. NETON: That's right. Nonetheless, it was a higher energy spectrum and then MDC was calculated based on that high-energy of radiation. So, I'm having trouble figuring out why their calculated value of 10 millirem is wrong.

It's not like they calibrated with americium.

I'm missing the point, I guess.

MR. ZLOTNICKI: Well, I think the answer is the calibration was not done at 10 millirem. The calibration will have been done at hundreds of millirem or several rem.

DR. NETON: Yes, of course. That's how you do a calibration. But my point is they did some fundamental calculation based on the background of the film and the sensitivity of the film to high-energy photons, not low-energy photons. So, I don't know why that number is not valid, unless they did a calculational error.

MR. ZLOTNICKI: Well, no. What you said is not true. You have to do the calculation of the limit of detection for each different energy that you are interested in.

DR. NETON: Exactly. But the calculation is done at a high-energy photon.

MR. ZLOTNICKI: No.

DR. NETON: Yes, you just told me it was

cobalt-60 or radium.

MR. ZLOTNICKI: That's not how the limit of detection was calculated.

DR. NETON: What did they do, rate it with americium?

MR. ZLOTNICKI: Well, certainly, multiple sources of different energies, yes. Yes. And so I think the challenge is that yes, the calibration, in terms of how much density per millirem was done with let's say with cesium 660 keV, but that doesn't mean the limit of detection was done with cesium for the 10 millirem that was claimed.

And also, as I mentioned earlier, if it was done with very fresh film with zero base fog and all the calibrates and blanks were also fresh, you have a situation where, under those laboratory pristine conditions, if you will, 10 was viable.

So, I'm not disagreeing with you that a calculation could have been done that would demonstrate it is viable, but once you add in three

or six months of age to the film, statistically, you can no longer see the 10 millirem for the high-energy photons.

DR. NETON: Sorry, I dropped off by accident there, Joe, so I missed a lot of what you just said.

But you were saying that the 10 millirem quoted detection limit is based on a calibration with americium. I'm not sure that is true.

MR. ZLOTNICKI: No, it is not based on a calibration with americium. But the response of the film to different energies, obviously, you have to pick your energy to know what your limit of detection is. If the limit of detection with cobalt-60 is 10 millirem, then it would be about .03 millirem or .04 millirem with the americium --

DR. NETON: Well, I would agree with that but it wasn't.

MR. ZLOTNICKI: -- which it wasn't.

MR. SMITH: This is Matt Smith again. In this case, we are talking, and Brian can correct

me if I am wrong, we are talking monthly exchange on this film. And again, when the TBD revision was prepared, we were working from paperwork that provided a dose of record. So, in other words, when it is printed on the back that we are getting monthly exchange results and the stated limit of detection is 10 millirem from the vendor, that is the weight in our mind. To go away from that value, why and where -- all the technical reasons being brought up here, we certainly are all aware of them. Given the frequency of exchange, the 10 millirem seems to be a valid number.

MR. ZLOTNICKI: Well, I don't think the frequency of exchange is the primary issue. The primary issue is you are dealing with the fact that the amount of density produced by a high-energy photon is right on the limit of resolution densitometer. And you have got the age of the batch of film, not just on a monthly exchange.

When Landauer bought film, they didn't buy it once a month and have it freshly made every

month by Kodak. So, they would buy a batch of film that would last, say, four to six months and when they first sort of QC'd that batch, they would pull out of the batch enough film to run all of the laboratory processes they need, such as making blanks and calibrates and quality controls, spikes if you will, that pull all those aside. And so, as far as possible, the badges that we used to calibrate a given batch of film when it was processed were matched in age and every aspect to the film that went out to the site. Nonetheless, over time, the fog level is naturally rising on both the film that left the facility and the film that stayed behind to be the controls.

So, I think that is one of the issues. The monthly wear period is not really relevant one way or the other.

DR. NETON: I'm still having trouble, Joe, reconciling what you are saying. They calibrated badges with cobalt-60, some high-energy gamma, and then they calculated a detection limit

using that high-energy gamma and that is what they quoted on their reports.

MR. ZLOTNICKI: No.

DR. NETON: And you are saying that number is incorrect.

MR. ZLOTNICKI: That number is incorrect for high energy. They quoted a number that was more typical for most of the customers who were working with X-ray or lower energies, iridium-192, americium, X-ray, hospital X-ray. Of all the different things that were handled, only a small proportion of handling were in a high-energy environment and that 10 millirem was quoted as sort of an -- for want of a better word, an average of what could be seen.

DR. NETON: Well, I think --

MR. DARNELL: If I can jump in here for a second. The paper that you sent is not really saying the things that you are talking about right now. Do you have references for us that we can go back and look at that back up what you are telling

us?

MR. ZLOTNICKI: I have my conversations. I don't know how many of you know Dr. Yoder, Craig Yoder, who was responsible for the technical program at Landauer for probably 30-odd years and who set up the DOELAP standard at Battelle prior to his work at Landauer. And this is based on many conversations I have had with Craig regarding this. I have put some of them in writing. And I think that the problem is Landauer were claiming something that was a bit of a stretch. That is not to put too fine a point on it.

MR. DARNELL: Well, I understand that and I understand what you are saying. And I appreciate your opinion but that is what we have is an opinion.

MR. ZLOTNICKI: And I think that is exactly --

MR. DARNELL: NIOSH cannot provide just an opinion and say that is the way it is. You have to provide documentation, too. And what I am

asking you for is where is this documented so we can go back and look at it.

And personally, I have no problem at all changing the LOD if we need to change the LOD. But right now, what I see in references is that the LOD should remain at 10. I'm just not seeing where you are coming from in a document, in a record, in something.

MR. ZLOTNICKI: Well, you are asking for someone to document something that won't have been documented because they were claiming a limit of detection that was too good. So, they are not going to document it the other way. So, you are asking for something that has to be opinion, not necessarily mine, but it has to be opinion. Because you are asking for someone to have documented something and then found something different. They are not going to have done that.

But I can tell you I have discussed this with Craig Yoder at length and I think some of you know Craig Yoder well. And I am not saying

anything out of school that he wouldn't agree with. But trying to craft something in writing about this, I mean we will have to talk about it. It's pretty tricky.

DR. NETON: Well, yes, I understand what you are saying. Joe, there is a lot of technology involved here with some filtration and such and the customers' needs. I understand what you are saying and how it over-responds. Unfiltered film will definitely respond due to low-energy photons because of the predominance of the photoelectric effect. I understand that.

But I believe we discussed this a while ago with Craig Yoder and I think we have an SRDB document where he suggested that the sensitivity of these TLDs was about 3 millirem to X-rays.

MR. ZLOTNICKI: TLDs?

DR. NETON: Kind of the opposite from what you are saying.

ACTING CHAIRMAN POSTON: Are we talking about the film or TLDs?

DR. NETON: Film. I'm sorry.

MR. ZLOTNICKI: For X-rays, that is exactly right.

DR. NETON: Well, that's not what you are saying. You said it would be 10 millirem for X-rays and --

MR. ZLOTNICKI: Oh, it's an average.

DR. NETON: Well, is it 3 or 10 for X-rays?

MR. ZLOTNICKI: I tried to pick one number. It was more sensitive for the X-rays, less sensitive for the high-energy. Often, people are exposed to multiple spectra, not just one. And it is a real world and a commercial business and a number was picked.

DR. NETON: But if it is 3 millirem for X-rays, then it is probably okay to say it is 10 millirems for photons or both, but higher in photons.

MR. ZLOTNICKI: Well, what is your evidence for that?

DR. NETON: We have an email here from Craig Yoder in 2005 that says that.

MR. ZLOTNICKI: That said what?

DR. NETON: I'm quoting his email. For X-rays, the LOD was typically about 3 milliroentgen or millirem as film is very sensitive to X-rays.

MR. ZLOTNICKI: Yes, that is true but it is more than three times more sensitive to X-rays than high-energy gamma.

DR. NETON: But you just said about five minutes ago that it was 10 millirem for X-rays is what they quoted on their report.

MR. ZLOTNICKI: They quoted it for everything. It just said 10 millirem.

DR. NETON: And you said that was for X-rays.

MR. ZLOTNICKI: And gammas. The whole point is it wasn't segregated. It was for beta, it was for neutron. It was for other things.

DR. NETON: I'm having trouble

following the logic of your argument, Joe, honestly. It is like I understand that it is less sensitive at higher energy but if they calibrate it to a high-energy --

MR. ZLOTNICKI: No, don't confuse the calibration with the limit of detection. They are two different things.

If I calibrate at one rem, the limit of detection never comes into the picture.

DR. NETON: What?

MR. ZLOTNICKI: If I only calibrate it at high doses, the limit of detection is a separate issue.

DR. NETON: No, no, no, no. When you calculate a detection limit, it is your efficiency of the device that is measuring it and you include your background in there.

The variability of the background, folded in with the efficiency of detection, and that can be done at a very high level, that is very standard in calibrating an instrument. You don't

calculate your limit of detection by reducing your exposure until you can't see it anymore. That is just not the way it is done.

You don't expose something to 10 milliroentgen and say, oh, there is my signal, and you go 5. It is a calculation. It is an empirically derived calculation. That is how it is normally -- that is how it is done.

MR. ZLOTNICKI: Well yes, you are right that it is a calculation and that you don't go and go to a lower and lower dose until you can't see it. However, as you know, it is a system. There is a densitometer involved. There is a film involved. There is subtraction of one number from another number. It is a lot more complex than you are portraying it.

And the bottom line was that there was one number for 30 or 40 years, 10 millirem as the limit of detection. As you know, it will have changed with every batch of film and every time it was processed. But they didn't report a different

limit of detection every time. That was a commercial number that was put out there and that is, I guess, what I am trying to tell you.

DR. NETON: I guess I kind of follow Pete here. I'm not following the logic of your discussion in your memo, then, that because it was higher energy it can't be 10 millirem.

We established that the film is much more sensitive to X-rays and it could be around three and it is going to be maybe three times higher, ten for high-energy photons.

MR. ZLOTNICKI: Where did you get a factor of three from? You just threw out a factor of three.

DR. NETON: I'm just saying that is what Landauer said that their detection limit is for their device. It doesn't qualify it and say this is only valid for low-energy X-rays.

MR. ZLOTNICKI: Well, you should quibble with Landauer about that.

DR. NETON: What are you saying, the

detection limit Landauer reported for decades is wrong?

MR. ZLOTNICKI: Yes, I am saying that.

DR. NETON: Okay.

MR. ZLOTNICKI: It is not wrong. It is a summary. It doesn't go into the details.

MEMBER FIELD: Jim, this is Bill. It seems like this isn't going to be resolved easily. Is there any way to have a conference call with Craig and try to get some more information or some sort of technical basis?

DR. NETON: Well, like I said, we have already discussed this with Craig Yoder in 2005. And he went through the whole process in the limits of detection. I haven't read it in a long time but I do recall this low-energy X-ray response of 3 millirem.

MEMBER FIELD: Well, it seems like it doesn't just affect this site but obviously --

DR. NETON: Yes, that is one of my problems is that everything that is reported on

Landauer badges is now being questioned and I'm not sure --

MR. ZLOTNICKI: No, no one is saying everything.

DR. NETON: Well, every badge that --

MR. ZLOTNICKI: There was something on the back of the report that was generic for all situations, okay, and that was 10 millirem. And again, remember who was mostly using these badges. Okay? It is someone in a hospital, or a dental office, or a facility that knows nothing about radiation. They don't have a Radiation Safety Officer, in many cases. They don't have a health physicist and they need a simple number. It was commercial service.

So, I wouldn't say everything was incorrect. Far from it. I think everything was excellent. This was a touchy subject where for a pure high-energy spectrum, it was pushing it to say you can see 10 millirem.

ACTING CHAIRMAN POSTON: Well, I agree

with Bill. We need to perhaps step back from this and solve it some way so we can move on with this particular meeting. It doesn't sound like we are going to solve it.

DR. MAURO: John, this is John Mauro. In listening to the conversation, there are really two aspects to this. And it sounded like there was a little uncertainty on what the spectrum was.

I heard some of the discussion that the fuel, naked fuel itself, may have an energy spectrum that includes fairly low, abundant low-energy photons. And of course, as the device that is being manufactured is assembled and there is shielding involved, which would harden the spectrum. And I guess I did not hear a clear picture of the workers that worked at Pinellas involved in the manufacture, I guess, of these generators what they were exposed to. Were they exposed, in some stages, to the naked fuel and other stages to the assembled --

MR. STIVER: John, if you look at

Figure 4-8 from the NASA report, it shows you what the emission rate was at the surface or some distance from the RTG compared to what was emitted in the fuel. And about 90 percent or more is above 500 keV. So, that gives you an idea of what the filtration was or the amount of transmission.

DR. MAURO: From the assembled device.

MR. STIVER: From the assembled device.

DR. MAURO: But while you are making it, the workers might be exposed --

MR. STIVER: They aren't making it.

DR. MAURO: Oh, they are not making it. Okay, that was what I did not understand.

MR. STIVER: The fuel elements arrived triple-encapsulated.

DR. MAURO: Oh.

MR. STIVER: And they were already heavily shielded when they were installed in the RTGs.

DR. MAURO: Thank you. I will set

aside this point. I didn't know that.

MR. STIVER: Yes, thank you. I am kind of of the same opinion as Bill. I think for this to be resolved maybe we need to get Craig Yoder and Joe and Jim and the players on a teleconference, a technical call and kind of hash this out and try to come to some resolution because we are not going to do it today, obviously.

ACTING CHAIRMAN POSTON: So we table this. Can you guys work out a situation where you can have this meeting of the minds?

MR. ZLOTNICKI: Sure.

ACTING CHAIRMAN POSTON: Okay.

MR. ZLOTNICKI: Well, I can't speak for Craig's availability but I will certainly try to get a hold of him.

MR. STIVER: Okay, see what you can do, Joe, and then we can notify NIOSH and Ted to go ahead and set up the call and we can, hopefully, resolve this.

MR. ZLOTNICKI: Okay.

ACTING CHAIRMAN POSTON: Okay, so we are down to the last item, then. And this is the decontamination/decommissioning at Pinellas.

MR. STIVER: Yes, this one, I think, is going to go a lot less contentious than the previous.

ACTING CHAIRMAN POSTON: Are you going to do this one, John?

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MR. STIVER: Yes, I can just give you where we are on this and then maybe Pete can -- he provided a response and wanted to talk to it.

But at the November 2012 Work Group meeting, we talked about some of the interviewees that we had spoken with previous in the year, I think we did those back in January 2012, and a very knowledgeable subject matter expert who was employed there during the D&D phase.

Yes, all the contract workers as well as the Pinellas workers were monitored before, during and after D&D. And then the question

arises, well do you have that data. Has it already been captured? And Pete indicated, at the time, that we had had a request to the Albuquerque office to retrieve those data. And he wasn't, correct me if I am wrong, Pete, at the time he wasn't sure whether it had been updated in the site DB. And his response indicates that they had done a very thorough review and had captured just about everything they possibly can. Maybe there were a couple of finding requests to DOE Legacy.

Pete, maybe you want to jump in.

MR. DARNELL: Yes, the only thing I would have to add to that is NIOSH feels we have done the records search. So, I don't think we are going to go out again unless we hear back from Sandia National Laboratories, who has the records, that we didn't capture something. Right now they are working on a finding aid for us so that we make sure we did capture.

I received an update yesterday regarding the release of that finding aid and it

is still under review by Sandia's export control and it is not going to be for another two weeks or so, the way they put it. We are still waiting on that finding aid but, in the meantime, in our response I provided what we did have, what we have looked at, all the SRDB references that we have regarding it. And that is just kind of where it sits.

MR. STIVER: Okay. I am kind of the mind that there is really not much more to be done on this. It looks like everything has been captured. I would maybe recommend keeping it in abeyance until you get those finding aids and have a chance to look at them.

MR. DARNELL: Certainly, I would agree with that. I think the issue itself is probably closed. We are just, as with anything, if we find anything else new, we would put it into the documentation that need it.

MR. STIVER: Yes, I am basically in agreement with that.

So, moving forward then, do we need a motion to put this into abeyance pending the retrieval of those finding aids?

MEMBER CLAWSON: So, John, this is Brad. You are telling us, I guess, from what I have gathered from you that you have done everything you can until you can go check out those leads?

MR. STIVER: Yes, that is the only outstanding -- I don't know if it's really outstanding, basically the only end that hasn't been tied up, yes, is just to check those finding aids. If there is anything, then they can retrieve that. If not, I think that is as far as it can go with it.

MEMBER CLAWSON: So, I don't see there is anything more than we can do. Just put it in abeyance until we get that taken care of and then we can close it.

MR. STIVER: That's fine with me.

MEMBER CLAWSON: Anything else? John Poston, I guess, does that sound right? Mute.

ACTING CHAIRMAN POSTON: Yes, I'm with that. I was just trying to -- sorry, Brad, I got your disease.

(Laughter.)

MEMBER CLAWSON: That's why I threw out mute.

ACTING CHAIRMAN POSTON: Yes, I'm happy with that. Bill, how do you feel about it?

MEMBER FIELD: I would totally agree.

ACTING CHAIRMAN POSTON: Alright. I did have one question. We are supposed to think about the path forward. Before we do that, I would like to go back to number 5 and satisfy my curiosity. Exactly what kind of film was this? I know it is X-ray film but who made it? Is it Eastman Kodak, DuPont, who?

MR. ZLOTNICKI: This was Kodak film.

ACTING CHAIRMAN POSTON: Kodak?

MR. ZLOTNICKI: Yes, this was Kodak film. It is manufactured specifically for personnel dosimetry. So, it is similar to the

normal film used for medical X-rays. They actually had a separate production run. I think it had even more silver in it of this material.

ACTING CHAIRMAN POSTON: Okay.

MR. ZLOTNICKI: And you know for a long time, there was a close relationship between Landauer and Kodak, going back a long time. And I think prior to that, there was a period when it was, I'm not certain of this, but I think it may have been DuPont in the very early days. But it was Kodak film.

Other people used the same film but Landauer bought it unpackaged and then would cut it up and package it in their own packaging. Kodak would also sort of package it in an identical packet, for example, that you are probably familiar with that many people wore in a personnel dosimeter.

ACTING CHAIRMAN POSTON: So, it is a standard packet?

MR. ZLOTNICKI: It was -- no, what

Landauer used was not in the standard packet. Well, if you go back early enough in Landauer's days, it was, but for a very long period of time, Landauer did their own packaging, which allowed some benefits in doing it like that.

It turned out when the film was left on the roll, unpackaged and in cold storage, it tended to fog more slowly. So, there was some advantage to working with it in bulk, in terms of the lifetime of the film.

ACTING CHAIRMAN POSTON: Sure. What kind of filters did you use, any?

MR. ZLOTNICKI: Oh, yes, for sure there were filters. There was an aluminum and a lead and the plastic and the open window of course.

ACTING CHAIRMAN POSTON: Okay.

MR. ZLOTNICKI: I would say all of that was fairly normal, perhaps with the exception that there was one or two of those filters were U-shaped, so it is radiation coming from one of the edges, which is always a problem in a low-energy

environment, that the X-rays bypass the filter.

Path Forward

ACTING CHAIRMAN POSTON: Yes. Okay, thank you. I just wanted to understand a little bit better what is going on.

So, basically, we have completed the agenda. We are going to have more discussion on Item 5, probably involving Craig Yoder and others.

And then we are going to put 6 in abeyance. Do we have -- is there a timeline or a date that we can reconsider or we will have more to discuss? What would you say there, Pete or John, or whomever?

MR. DARNELL: I think with today's meeting, we are finished with the tritium, finished with Issue 6, just waiting that finding aid. We need to do the tech call on dosimetry.

But just in preparation for that, I found the Site Research Database reference for the email that Jim was talking about with the Craig Yoder dosimetry LOD and his discussion of it. It

is SRDB reference 19707. And it talks about 3 millirem and 10 millirem. I would just like you guys to take a look at it before we decide to do a tech call.

ACTING CHAIRMAN POSTON: Okay. Is there anything else we need to talk about in this call?

MR. KATZ: Yes, this is Ted. So, I just think it would probably be helpful provisionally to try to calendar, since you are all on the call right now, rather than having to send out and get agreement about scheduling down the road, which takes a lot more trouble.

Let's just go ahead and book. It will be a very brief teleconference. So, it should be easy to book. But let's just book it for relatively close to the Board meeting, which will allow time for technical calls, who needs to be on, or what have you for parties to be ready.

I was going to suggest we push it out pretty close to the Board meeting, that way we are

most likely to be able to accomplish whatever we are going to accomplish on this issue.

So for example, the week of March 7th might be -- if there is a date during that week that works for all three of you Board Members and of course the staff, too. The 7th is Monday. So, 7th, 8th, 9th, 10th, 11th.

MEMBER FIELD: Monday and Tuesday are booked for me but, otherwise, pretty open.

MR. KATZ: Okay, so how about March 9th?

MEMBER FIELD: That works for me.

MR. ZLOTNICKI: This is Joe. I'm tied up in the morning. I can make the afternoon or in fact that March 9th morning is the only time I can't make it that week.

MR. KATZ: That's fine. So, how about a 1:00 p.m. on March 9th?

DR. NETON: Ted, I'm out of the office on March 9th.

MR. KATZ: Okay, how about the 10th?

DR. NETON: The 10th, I'm here.

MR. KATZ: So how about March 10th in the morning or afternoon? Does anybody care?

DR. NETON: Either one for me.

MEMBER FIELD: Either.

ACTING CHAIRMAN POSTON: Yes, March 10th works for me. I have got an 8:00 class but I am through at 9:15. So, the rest of the day is free.

MR. KATZ: Okay, so 9:15 yours is 10:15 our time.

ACTING CHAIRMAN POSTON: Right.

MR. KATZ: What about we say -- it is not going to be a lot of time. Whatever you guys learn, I'm sure. So how about 11:00 a.m. Eastern Time? That gives you time, John, to get back to your office or whatever.

ACTING CHAIRMAN POSTON: Yes, that will be fine. I'll put that in my calendar right now.

MR. KATZ: Okay, 11:00 a.m. on March

10th teleconference.

MR. DARNELL: Are you going to send out a meeting notice and call-in number?

MR. KATZ: I will absolutely send out a notice.

MR. DARNELL: Okay.

MR. KATZ: As long as that works for everybody, that's good.

Okay, then. And I think regardless of how this works out, I think the Work Group can still report to the Board. Whether they have an item hanging out there or not, they can report about it either way.

ACTING CHAIRMAN POSTON: Okay, that works for me.

MEMBER FIELD: Sounds good.

MR. KATZ: Well, thank you, everybody. We're adjourned, right?

ACTING CHAIRMAN POSTON: Anything else?

(No response.)

Alright, guys, thank you so much and have a good rest of the day and rest of the week.

(Whereupon, the above-entitled matter was concluded at 4:11 p.m.)