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

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ADVISORY BOARD ON RADIATION AND WORKER HEALTH

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SUBCOMMITTEE FOR DOSE RECONSTRUCTION REVIEWS

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TUESDAY

MARCH 13, 2018

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The Work Group convened by Teleconference, at 10:30 a.m. Eastern Daylight Time, David Kotelchuck, Chair, presiding.

PRESENT:

DAVID KOTELCHUCK, Chair
JOSIE BEACH, Member
BRADLEY P. CLAWSON, Member
WANDA I. MUNN, Member
DAVID B. RICHARDSON, Member

ALSO PRESENT:

TED KATZ, Designated Federal Official
NANCY ADAMS, NIOSH Contractor
DAVE ALLEN, DCAS
BOB ANIGSTEIN, SC&A
BOB BARTON, SC&A
KATHY BEHLING, SC&A
NICOLE BRIGGS, SC&A
RON BUCHANAN, SC&A
GRADY CALHOUN, DCAS
DOUG FARVER, SC&A
ROSE GOGLIOTTI, SC&A
JENNY LIN, HHS
JOHN MAURO, SC&A
BETH ROLFES, DCAS
MUTTY SHARFI, ORAU Team
SCOTT SIEBERT, ORAU Team
MATT SMITH, ORAU Team
JOHN STIVER, SC&A

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(10:30 a.m.)

Welcome and Roll-Call

MR. KATZ: Let's just get going with the preliminaries. John Poston not being here makes it easier, but I'll just address the conflicts of interest instead of you guys, because he's the one that has more than one.

So, this is the Advisory Board on Radiation and Worker Health, the Dose Reconstruction Review Subcommittee. David Kotelchuck, Dr. Kotelchuck, is our Chair. He has no conflicts. But I should note for the other Members, Ms. Josie Beach and Ms. Wanda Munn, who are both on the line as well, and Members, both of them are conflicted at Hanford. So they will not be in the discussion on any Hanford cases that might come up.

And Mr. Brad Clawson, the other Member on the line with us, is conflicted at INL and won't discuss any cases that might come up. I'm not sure we have either of those cases coming up,

but we'll see.

And there's an agenda for today's meeting, but it's not that informative. But it does tell you mostly what sites are being covered today. And that's at the NIOSH website, under the program, the EEOICPA program, the Board schedule of meetings, today's date. So you can go there and see that agenda, if you wish.

And then, moving on from there, let's do roll call for everyone but the Board. We have a quorum for the Board, and we expect David Richardson, who also has no conflicts. He'll be joining us a little bit later.

(Roll call.)

MR. KATZ: Okay, then. There is some buzzing and so on, probably from some of the lines. If everybody would keep their phones on mute except when addressing the group, that would probably be helpful. And press *6 if you don't have a mute button and *6 to come off of mute.

CHAIR KOTELCHUCK: The buzzing has stopped, at least on my line.

MR. KATZ: Yeah. And so it's your meeting, Dave.

Review Set 24 Blind Dose Reconstruction Cases

CHAIR KOTELCHUCK: Okay, very good. Well, folks, let's start off with the Set 24 blind dose reconstruction cases. We're going to look at three cases today and then the next three at the next meeting. I gather the next three have already been done, but we'll deal with them next time.

So, do we want to start out with the first one that you put on the list, Rocky Flats?

MS. GOGLIOTTI: That's what I was thinking. Kathy?

MS. BEHLING: Yes, I'm ready. Okay. This is, obviously, as you're seeing on the screen, this is a Rocky Flats case for an Energy Employee with a little more than one decade of employment, as shown in Table 2-1 on page 10. I'll let Rose get there.

If we move on to -- and we'll come back to this Table -- but if we move on then to

Table 2-2, that shows that this Energy Employee was diagnosed with multiple cancers. And you can see that list on your screen.

Now, if we back up to Table 1-1, on pages 7 through 9, this shows a comparison of the doses that were assigned by NIOSH and calculated by SC&A. As shown in Table 1-1, for all of the cancers in all of the exposure pathways, NIOSH and SC&A estimated nearly identical or extremely similar doses.

If we now move on to Table 2-3, we can see that there's a close agreement in the doses. And I'm going to spend a lot of time on this table because I felt -- let me also back up a second -- the doses and the PoCs were very close and similar, and, in both cases, NIOSH and SC&A calculated a total PoC value that was less than 50 percent. So the case would not have been compensated.

I plan on spending time talking about the similarities and differences in this particular case rather than going through a lot

of detail on the derivation of the dose calculations. However, if there are any questions at the end or along the way, please stop me and we can discuss them.

To start with, this EE was monitored externally for photons, electrons, and neutrons, and both NIOSH and SC&A calculated doses for recorded missed dose for all three exposures, as well as unmonitored dose that was based on coworker models. The reason their values are so similar is that both used the same guidance documents. NIOSH did utilize their workbook, which incorporates the Technical Basis Document guidance which SC&A used.

In all cases, NIOSH and SC&A used identical EF values, energy fraction values, and also applicable correction factors. Both used same DCF values but they applied them in a different way. NIOSH used the triangular distribution of the Implementation Guide 001 DCF, and then it used a Monte Carlo method to determine the uncertainty, where SC&A just used the mode

DCF value and applied that consistently to all doses.

And this resulted in some differences in the dose distributions that were entered into IREP. Occasionally, when NIOSH uses the Monte Carlo, that will result in the Weibull distribution being the best fit. And for SC&A, we entered the data based on guidance in the TBD, which is typically normal or log-normal distribution, so that created a few minor differences.

Other minor differences is that NIOSH assigned the recorded shallow dose for five years of employment, while SC&A assigned the recorded shallow dose they interpreted the records with and only shallow dose for three years. Also, for missed photon dose, NIOSH calculated 28 zeroes, gaps, or less-than-one-half-of-LOD values, and SC&A counted 25. And that, again, happens because sometimes the complexity of looking at the records and interpretation of the records.

Other minor difference is that, for

coworker dose, there was a slight difference in the fractions of months assigned, and that's shown in Table 2-4 on page 16.

Am I going too fast here?

CHAIR KOTELCHUCK: We are jumping around a lot.

MS. BEHLING: Okay. It's just that I didn't think it was necessary to go through and calculate each one of the doses. I just thought I'd give you an overview and if we want to go back we can do that.

CHAIR KOTELCHUCK: No, no, that's fine. The way you're handling it, from my mind, as one person, is just fine. It's just that the screen is jumping a lot as you go from place to place.

MS. BEHLING: I'm giving Rose a hard time.

CHAIR KOTELCHUCK: Please slow down your narrative so that the screen can catch up, if you can.

MS. BEHLING: Okay. I will, I will.

Okay. We're now on Table 2-4 where, as I said, again, just a minor difference in the fraction of months that coworker dose was assigned.

If we then leave the photon, neutron, and electron, the shallow dose discussion, we'll move on to the occupational medical dose. And neither NIOSH nor SC&A calculated any occupational medical dose. And that was based on guidance in OTIB-79, which states that if the exams, if the X-ray exams, were performed offsite they do not get included in the dose reconstruction.

Also, if we go on to the onsite ambient dose, neither SC&A or NIOSH calculated any onsite ambient dose, and that is in accordance with PROC-60 guidance because there was already missed dose assigned.

Alright. We are ready to move on to internal, unless there are any questions. I hear none. So, this Energy Employee, again, was monitored for plutonium via urinalysis, fecal, and chest counts. And both NIOSH and SC&A

concluded that the urinalysis results were the best method to model the plutonium dose. So both used the same exposure periods and the same plutonium mixtures. And this resulted in nearly identical intake values that are being shown on Table 2-5 on the screen, on page 18.

Both also concluded that Type S was the most claimant-favorable solubility type. The only difference is both also considered Type Super S because we're looking at plutonium. However, NIOSH didn't make any adjustments to their doses after considering the Type Super S, while SC&A did multiply one year's dose by a factor of four to account for the Type Super S.

And this resulted in SC&A's internal dose to come to 22 millirem for the first diagnosed cancer and 25 to 35 millirem for the remaining cancers, while NIOSH assigned a dose of 7 millirem to the first diagnosed cancer and a range of 8 to 10 millirem for the remaining cancers. There was no environmental internal dose calculated and it wasn't necessary to

calculate that.

So, in summary, doses were very similar, PoCs were similar. And I think I described the minor differences, but if there are any questions, I can take them now and get assistance from other SC&A participants, if necessary.

CHAIR KOTELCHUCK: I don't have any questions. I did learn from your work what a canthus was, the skin lateral canthus, and I had not heard of that part of the body. And of course, I went to my dictionary and learned about it. So, good. Thank you.

MR. SIEBERT: This is Scott Siebert. I just want to point out, since there was a question about the Super S and where SC&A applied it and we did not, I'll address why that is case if you would like me to.

CHAIR KOTELCHUCK: I think that would be good.

MR. SIEBERT: Okay. The reasoning is, as you know, the OTIB-49 factors are set up such

that you can't actually model Super S and do projections between the two types of chest counts and urine counts. Directly you can't do those comparisons, but there are ways to do projections out to the chest if you're starting from urine and vice versa.

And what we did in this case is, as was mentioned, we started with the urine and made the assumption it was a Type S plutonium intake projected out to a chest count. And then when we made our adjustment for how much would be in the chest of Type Super S, since it doesn't clear from the chest and the lungs nearly as quickly, there's a lot more of the material in the chest that you would expect to see in a chest count.

We projected out to determine that we should have seen it in the chest counts, and since there was no americium detected in the chest counts that were given for this EE, we determined that Type Super S was not appropriate and we did not adjust the doses accordingly. That's the difference.

CHAIR KOTELCHUCK: Okay. That's helpful.

MEMBER CLAWSON: This is Brad. There's a certain amount of difference in between that. So which way is the right way to be able to do it, then?

MR. SIEBERT: I'm not sure I understand your question, Brad.

MEMBER CLAWSON: Okay, you felt that there was not a reason to put Type S, correct? Because you --

MR. SIEBERT: Type Super S, that's correct.

MEMBER CLAWSON: Super S. Well, what I'm trying to figure out is, well, then which is the best way to be able to do it?

MS. BEHLING: In SC&A's thinking -- and someone from SC&A can correct me if I'm misspeaking here -- but I think our philosophy was that we were using urinalysis data, and so, based on OTIB-49, we strictly looked at that urinalysis data, which would have indicated that

we should have made an adjustment to at least one year's worth, which is what we did. It was just one year's worth of dose, because we went strictly by the urinalysis data; we did not factor in the additional information that Scott's talking about from the chest counts.

MR. SIEBERT: So yours is slightly more claimant-favorable/overestimating because you didn't limit it by the chest counts?

MS. BEHLING: Correct.

MR. SIEBERT: And as less than 50 percent, that's --

MEMBER RICHARDSON: This is David Richardson. I got two questions. Can you hear me?

CHAIR KOTELCHUCK: Yes, we can hear you.

MEMBER RICHARDSON: So the first question is, this indirect procedure for making an adjustment about what type of plutonium it is, is that documented or is that something which was developed here in this case?

MR. SIEBERT: Well, we use it in many cases. It's actually part of the OTIB-49 corrections pool that we --

MEMBER RICHARDSON: So that procedure is clearly described and has been evaluated?

MR. SIEBERT: I'd say the process is available, yes.

MEMBER MUNN: Yes.

MS. BEHLING: That has been evaluated, yes.

MEMBER RICHARDSON: I mean, part of my understanding was, in the in vivo counting looking for americium, was that the detection limit was relatively high for that. So you feel confident saying that, given the absence of detection of americium in the lung counts here, regardless of the magnitude of the intake, you can make a judgment about whether there's Super S present or not?

MR. SIEBERT: Well, yes, if we determine that, by projection, that you should have detected it, regardless of the actual

detection limit, as long as -- you know, we know that americium can be detected in the chest, that's not disputed. Yes, the detection limits may be relatively high. However, in this case and the other cases that we projected out to, it would have been detected in the chest count.

Let me point one other thing out that we do in cases like this, is we also work backward from the chest counts and compare it to the urine samples to determine which is a more claimant-favorable assumption. Which we did in this case, and Type S is the claimant-favorable assumption, you know, when you start from the chest count.

MEMBER RICHARDSON: I'm not following quite what you -- I mean, the "irregardless of the detection limit" seems like that doesn't make any sense. I mean, if you don't detect something and there's a detection limit, then it does depend on the detection -- so I thought the committed dose, remind me what it was to the lung in this case, from this intake. I thought we were talking in the 10 millisieverts, millirems.

MS. BEHLING: No, the actual dose to the first diagnosed cancer, based on SC&A's results, was 22 millirem.

MEMBER RICHARDSON: And based on NIOSH's, it was 7 or something like that?

MS. BEHLING: Yes, 7.

MEMBER RICHARDSON: And you're saying for an intake, for inhalation of plutonium of Type Super S, you could detect that with reasonable certainty looking for evidence of the americium signal from that?

MR. SIEBERT: Yes.

MEMBER MUNN: This is Wanda. I would just have to comment that when you're talking about millirem in the quantity of double digits it's hardly likely that it's going to be a balancing factor one way or the other with respect to injury to the patient -- rather to the --

MEMBER RICHARDSON: I'm not concerned about that, Wanda. I'm just concerned about the logic of the argument. I mean, again, doses in

that magnitude, I'll defer to one of you, buy my discussions previously had led me to believe that it would be an area of uncertainty about using in vivo counting for finding that type of intake. But if you all are convinced of that, okay.

MEMBER CLAWSON: Don't say that we're all convinced.

CHAIR KOTELCHUCK: Right. I would say this: it's a small effect. I'm actually glad that we're discussing the process to the extent that it may be that one of them is better than the other, even though we understand that neither will have a significant effect on the PoC.

So I'm happy with this discussion, and maybe we should continue it if there's any question about which should have been used. In general, in comparing the blinds, you know, we assume that both procedures are perfectly good and sensible experienced professionals use them, but it can be that we'll come across something that one might feel one group really did it, if you will, the better way.

So I'm more than happy to continue this, if people would like to.

MEMBER CLAWSON: I'm just looking at the claimant-favorability, right? You know, we put a lot of emphasis on getting over the top and small dose versus what isn't. I'm a firm believer, if it would have been your dose, those small doses add up, but I'm just trying to get a feel for which is really the best way to have been able to do this.

I understand what Scott's saying, but some of our monitoring hasn't been that good, and I just -- I don't know which way is the best way, but it just doesn't make me feel very good looking at this that way. But I just wanted to understand why we were doing what we were doing and why we came out. And, yes, it's only in the millirems and stuff, but I'm just wondering why we ended up so far off. To my eyes, it is a little bit off.

DR. MAURO: Dr. Kotelchuck, this is John Mauro. I have a simpler question before we leave this subject. It sounds like we're close

to leaving it. Apparently there's a convention in place that I don't recall, regarding the equilibrium factor between the americium and the plutonium that's taken in. I assume that's been standardized and I may have forgotten about it. But, obviously, in order to use the whole body count looking at the americium, you have to make certain assumptions about what the equilibrium is between the americium and the plutonium.

What's the convention? It's probably in an OTIB somewhere.

CHAIR KOTELCHUCK: Can someone answer that?

MR. SIEBERT: Yeah, the ratio of the americium to plutonium is given in the TBDs of interest for the different types of plutonium mixtures that are prevalent at the site. So, in this case, this is Rocky Flats, so we would have dealt with the ratio that's given in the Rocky Flats TBD.

DR. MAURO: And those matters have all been hashed out as part of the Site Profile review

process?

MR. SIEBERT: Correct.

DR. MAURO: Okay. Thank you.

CHAIR KOTELCHUCK: Okay. Shall we go on, folks? Do we approve the results here? Approve in terms of, I guess, record, observe, and accept.

MEMBER MUNN: This is Wanda. I would suggest that we do that, again, with applause. It is an amazement to me that different people are looking at the same material, and, even with the individualized approach to some of the finer points, have an end result that is so remarkably similar. I don't think we've encountered anything so far that is more than, what ,perhaps one, at the most two percent differential between the final dosages. And that's, in my mind, a remarkable thing. So, good review, Kathy. Thank you.

CHAIR KOTELCHUCK: Yes. Well, we actually have found some that are a little bit more than one or two percent. But upon average,

they're generally within one or two percent. But what's so impressive about this is that we are choosing as cases PoCs that are awfully close to 50 percent, and then we're dealing with one in which we have nine, what is it, nine different sites, nine different primary cancers.

MS. GOGLIOTTI: Dave, we've been trying not to say the number of cancers in order to prevent any personalized information --

CHAIR KOTELCHUCK: Well, very good. Thank you for saying that. And that's a perfectly -- that's a sound idea. I had not noted that before. Anyway, there are many cancer sites and they're very close to 50 percent, and the two groups got results that were within a percent or so from each other. And that is impressive.

MEMBER CLAWSON: Dave, this is Brad. I know this will come down as a mark in history that I agree with Wanda, but all the applause and all that stuff, we are hitting really, really close. If these weren't blinds, I would have a bigger issue.

CHAIR KOTELCHUCK: Absolutely. And that's the reason we're doing blinds.

MEMBER CLAWSON: But I was just trying to bring up the point, trying to understand which way is the best to be able to do this. And you're right, these are blinds, and that's what I needed to remember going from there. But I do agree and I think they've done a great job on it. I was just trying to understand if there was a breakdown in the process either way on this. And both of them sounded sound to me, so I just wanted to make sure that -- I wasn't finding fault, I was just trying to understand the process a little bit better.

CHAIR KOTELCHUCK: And that is welcome. That is welcome, and I'm very glad you did.

DR. MAURO: This is John Mauro one more time. It goes more towards the ground rules, and I'm sorry if this is redundant and you've already covered this, but let's presume for a moment that Rocky is still undergoing review and

we have some outstanding issues, technical issues on the Site Profile, that sort of thing are still being discussed. That may or may not be the cause.

Now we move into a world of comparison, like these blinds. If there is, in fact, an issue -- let's say, for example, there was an issue regarding equilibrium that we're discussing: how would that be dealt with when we go to a blind process? Do we explore that at all, or do we just presume that there are no issues outstanding with regard to any of the OTIBs or TBDs that are in place and we just sort of follow the rules and see if we come up with the same results, notwithstanding the possibility that there may be some outstanding issues that have not yet been resolved.

MR. KATZ: Let me just address that. It doesn't matter whether they're the blinds or the regular dose reconstruction reviews. I mean, whenever we come up with an issue that is unresolved, you know, we chase it down to the

end. That's what we're supposed to do. So, you know, if this is an item where there's an issue that's unresolved that may matter for other cases, then we have to chase it down to the end, right?

CHAIR KOTELCHUCK: Right.

MR. KATZ: I'm not sure what else you're asking about.

DR. MAURO: That's it. I didn't quite understand, I just wanted to be sure of what the ground rules were, whether or not we would challenge, for example, some of the underlying premises, either because they have been previously challenged or during the blind review process --

MR. KATZ: It doesn't matter. It doesn't matter whether if we haven't previously challenged them and they come up and it's a real issue, it's an issue, and that has to be chased down.

DR. MAURO: Good. Okay. Thank you.

CHAIR KOTELCHUCK: Absolutely. And we

don't -- I mean, when we're comparing blinds, we assume these are both professional groups and experienced groups, so we make no choice as a Subcommittee about which is better. They're just two different professional groups approaching difficult calculations together and comparing. But, as Ted said, if we were to find an error or that one of the processes used seemed to us to be incorrect, then we, of course, have to go all the way back and feed that back into the process of dose reconstruction.

DR. MAURO: Thank you. Please, I apologize if I'm hashing over old questions. Thank you.

CHAIR KOTELCHUCK: Discussion is always welcome.

MEMBER CLAWSON: Well, you know, what John is saying is absolutely correct. And we've got into this before, John. But at one of the sites that we have not resolved an internal or an external issue coming into it, and these blinds have kind of sat, we've done that before. We

have gone through the process, but there's still outlying issues that have not been resolved with the Site Profile and that creates part of the problem. And we have had some of these that have come up that way.

CHAIR KOTELCHUCK: Right. Okay. Well, I think we're ready now to go on to the next case.

MS. GOGLIOTTI: Dave, if I can really quick, I just want to point out the Table 2-3 here. This is different in the 24th Set than was previously done. At the Board's request, we simplified this table considerably, so now every time you see a dash mark here that means that SC&A and NIOSH did identical things. And that should make it easier for everyone to view the differences between the two in the summary table.

CHAIR KOTELCHUCK: Certainly, it does.

MEMBER BEACH: Thanks. I was questioning that, too. I was wondering what that was, so I appreciate that explanation. This is Josie.

CHAIR KOTELCHUCK: Okay. So let's go on to the next blind case, W.R. Grace.

MS. GOGLIOTTI: Nicole, are you on the line?

MS. BRIGGS: Yes, I am.

MS. GOGLIOTTI: Great.

MS. BRIGGS: Okay. This is blind dose reconstruction B-28. And this individual worked at W.R. Grace for about a 30-year period, which spanned both the operational period at W.R. Grace and the residual period. This Energy Employee was diagnosed with several cancers. And for anyone following along, the list of the cancers are on our Table 1-1, which is on page 6 of our report.

Now, this is a particularly interesting case. In fact, I don't think we've ever had a blind case quite like this one, and it definitely sparked a lot of discussion among our group at SC&A. So I'll start with the end result, and then we'll just work our way back through the case.

NIOSH's Probability of Causation for this case was 51.14 percent, so it was recommended for compensation. And SC&A's PoC is 49.5 percent, so it came just below the compensation line.

Now, adding a little bit more interest here, even though SC&A had a lower PoC, for each of the cancers for this case SC&A actually assigned a slightly higher dose in comparison to NIOSH's assignment. It is a relatively small difference. SC&A's total dose assignment was only about, depending on the cancer, you know, I'll say averaged 350 millirem higher than NIOSH's assigned dose. But like I said, SC&A's PoC actually came in lower; and not only did it come in lower, it came right below the compensation line.

So, for this comparison, I'll go through, you know, as Kathy did, go through each section.

MR. SIEBERT: Hey, Nicole? I'm sorry. This is Scott Siebert. This is a big enough

issue, if people don't mind, I'd love to address that before we got into the specific differences in the case, if that's all right.

MS. BRIGGS: Okay, yeah.

MR. SIEBERT: Because, obviously, the biggest issue here is that SC&A's value was less than 50 percent and ours is greater than 50 percent. I just want to put that to rest before we move on. The reason -- and SC&A does discuss this in their report -- is that once we're in best estimate territory, which is between 45 percent and 52 percent, due to the uncertainty differences that you get when you run Monte Carlo calculations, we no longer run IREP just in the standard manner. The standard manner is 2,000 iterations with a random seed of 99. When we're outside that range, we all run that and that's the PoC of record that we send to DOL.

Once we're in that range, we have a process where we actually run IREP 30 different times with 10,000 iterations, rather than 2,000 iterations, and a random seed for each of the 30

runs. So it gives us a much better cross section of what the PoCs are. We have a range of PoCs, and we take the average of that.

When I took SC&A's IREP files in this case and ran them through that process, their overall PoC actually came out over 50 percent, matching ours relatively closely. So, just the process involved is a huge reason for the difference there, taking that additional step to get the best answer possible in that best estimate range.

MS. BRIGGS: Okay, alright. Since you're jumping ahead for me, then I think I'm going to, if it's all right with everybody -- I don't know. Rose, how do you want to proceed? I can jump ahead to our last table and we can go straight to there, I guess.

MS. GOGLIOTTI: It's entirely up to you.

MS. BRIGGS: Okay. You know what then? Why don't we go ahead and do that? We'll jump ahead to Table 3-2 at the end of our report.

CHAIR KOTELCHUCK: Alright. Thank you.

MS. BRIGGS: Sure. So, this is a comparison. We started doing some work-ups. I think it's very interesting that you came up with right over 50 using that average -- I'm sorry, using the 10,000 iterations with the increase in the 30 runs. When I went into the actual runs themselves, I guess I didn't find any that were -- I guess one of my questions, if I had run into that, none of NIOSH's numbers actually came below 50 percent. So I was just, you know, trying to figure out, that was one of the things I took to look at, just to see if NIOSH's numbers can get below 50 using any of their information, which is why we sort of brought this up was, well, we're trying to take a look at all the different scenarios. I mean, obviously, we're dealing with -- the way I see it is we're actually sort of operating at the edge of this program, the fact that we're coming in right at the line above and below, the difference being about one-and-a-half

PoC percentage point. So we really are at the absolute limits of this program, and it just happened to be that way for here to show --

CHAIR KOTELCHUCK: I'm not worried about you're trying to see why you folks are one is above and one is below. The question is, did each of you do a sound professional, technical evaluation? I'm not at all 0-- I mean, I'm not, if you will -- put it his way: I want to compare two good calculations. Why one is a little above and one is a little below, it seems to me fits perfectly in the range of the other blinds that we've looked at. And the average, as I recall at the Santa Fe meeting, the median of PoC differences is 1.5 percent difference.

So I'm not worried about whether one is near the edge and what could have been done. We're looking at each independently, and the question is, in your report, is each one doing a sound job? And it seems to me you are, but you could go over the individual components. To me, the effort to compare them, one to another or

what would have happened if one of you had used some other different parameter. No, the question is, are the parameters used by each one appropriate in our judgment now, as a Subcommittee. And it seems to me they are. You could go over things a little more in detail in terms of external dose, internal dose. I'd be happy if you wish to --

MS. BRIGGS: That would be great. I actually have that prepared, so I can work my way through. You can see all the different elements of the dose reconstruction.

CHAIR KOTELCHUCK: Let's do that.

MS. BRIGGS: Okay.

MS. BEHLING: This is Kathy. I don't mean to interrupt, but, while we're on that Table, do we want to explore the Table a little bit more or do you want to wait until we get through all of the doses?

CHAIR KOTELCHUCK: I would like to get through the doses. What do other Subcommittee Members think?

MS. BEHLING: Okay. Because there still are some questions that we have regarding the calculation of the PoC, but we'll get to that at the end.

CHAIR KOTELCHUCK: Okay. We'll get to that later.

MS. BEHLING: I apologize if --

CHAIR KOTELCHUCK: No, no, not at all. No need to apologize. That's fine. Thank you.

MS. BRIGGS: I just wanted to give a little introduction to sort of -- because it was sitting right there, the difference in PoC. So I wanted to address it right up front and then sort of go back and go through dose reconstruction.

Alright. So if we go to our Table 1-2, which is on page 7, and that is the comparison of the SC&A doses and the NIOSH doses broken down by type.

Now, for this case, the overwhelming majority of the dose, well over 90 percent of the total dose, was attributed to external doses.

And the total doses overall, which span both SC&A and NIOSH's range from about 6.3 rem to about 7.8 rem depending on the cancer. And the difference between our assigned doses between SC&A range from 298 to about 367 millirem, which is why I said, roughly, we're talking about a difference of about 350 millirem, you know, average per case, depending on the cancer.

CHAIR KOTELCHUCK: Okay.

MS. BRIGGS: And to start with the external doses, this was monitored for both external photon and beta. And for both the recorded and the missed photon doses during the operational period, NIOSH and SC&A assigned identical doses with identical distributions and use the exact same methodology.

And for the residual period, like I said before, W.R. Grace is broken into both an operational and a residual period. Both NIOSH and SC&A assigned what was an unmonitored photon and shallow dose using the guidance in the TBD.

Now, here we also have a very small

difference in how the dose was assigned. NIOSH prorated the doses for about four years of this individual employment period in order to account for partial years of employment. And SC&A assigned for those years actually a full year of unmonitored dose, and this resulted only in a difference of about 2 millirem for the unmonitored photon dose and about 6 millirem for the unmonitored shallow dose. And for occupational medical doses, SC&A and NIOSH again assigned -- the assignments were identical, the distributions were identical, methodology was identical.

As I said before, the total eternal doses that were calculated by SC&A and NIOSH ranged from about 5.7 to about 6.7 rem. And, depending on the cancer, the difference between SC&A's and NIOSH's external dose assignment was only about 7 millirem per cancer. So they were extremely close. So this represents the overwhelming majority of the assigned dose for this case, and both SC&A and NIOSH are coming in

essentially identical.

If there are any other questions regarding the external dose, I can run through the internal.

CHAIR KOTELCHUCK: Questions? You can go on.

MS. BRIGGS: Okay. So, let's see, the internal doses: This individual was monitored for uranium and plutonium exposure with bioassays and whole body counts. For the uranium exposures, NIOSH and SC&A both used the methodology described in the TBD for the site. For the operational period, they both assumed the exposures were from the U-233 reactor fuel mixture. And for this dose assignment, NIOSH used what's called the dose and risk calculation software, the DCAL software program, to fit the bioassay measurements to -- they defined ten acute intakes looking at the data. And NIOSH used that DCAL program instead of the usual IMBA program based on the guidance in OTIB-28, which actually states that the dose coefficients using

the DCAL program are more accurate for internal dose assessments involving U-232 and U-233. And NIOSH also calculated the potential missed dose from a chronic exposure to uranium based on the one-half of the MDA value.

Now, for each year, the doses from the fitted intakes were compared to this missed chronic intake, doses from those missed chronic intakes, and the higher of the two values was assigned and used as input into IREP, which in all cases was the missed dose for all years and depending on the cancer that NIOSH assigned is about 218 to 261 millirem from this uranium exposure.

NIOSH uses a slightly different method. They use the IMBA program to fit the bioassay measurements. And they defined about seven acute intakes based on the data. And they also used IMBA to assign, again, an underlying chronic intake from the bioassay measurements that were below the MDA.

Now, instead of comparing and

assigning the higher of the two, all of these intakes were used as inputs into the Chronic Annual Dose Workbook, also called the CADW. And depending on the cancer, SC&A's uranium dose assignment here was between 510 and 620 millirem for each of the cancers, depending on the cancer.

So SC&A's annual doses for internal are roughly, thereabouts, about 300 millirem higher, which we feel like is most likely due to the fact that SC&A's assigned doses from both the acute and the chronic intakes and NIOSH compared and assigned the higher of the two and assigned the chronic.

Next, I'll go onto the internal dose from the plutonium exposure. This individual only had one plutonium bioassay, which was below the MDA, so both NIOSH and SC&A -- well, they used the same assumptions to assess a missed internal dose from plutonium exposure. NIOSH used IMBA to calculate the chronic intake and used IMBA to calculate doses. And depending on the cancer, NIOSH got a missed dose from

plutonium which ranged from about 77 to about 97 millirem.

SC&A used the IMBA program to calculate the intake but used the CADW workbook program to calculate the doses. So SC&A's missed doses ranged from about 167 to about 213 millirem. And it appears that the difference is due to the fact that, again, NIOSH prorated the intakes for partial years of employment, but, since the CADW program only allows for a full year, SC&A assessed the plutonium intake for that entire year. And that added, I think it was, roughly, about six months of intake. And that seems about right since SC&A's missed plutonium doses were just about double, a little more than double than those calculated by NIOSH.

So for one small part of the internal dose assignment, this individual's employment was broken up into two periods. Both NIOSH and SC&A used that same method and assigned a missed dose from uranium exposure for the latter part of this individual's employment period separately. And

for that, they both used identical methods, identical assumptions, and got identical results, which is only about 5 to 7 millirem, depending on the cancer.

And just to sum up the internal, our Table 2-2 on page 15 lists the comparison of the internal dose totals. And as I said, SC&A doses are roughly 300 millirem higher, which is fairly close.

And let's see. Are there any questions about the internal doses?

CHAIR KOTELCHUCK: Questions?

MEMBER BEACH: None here.

CHAIR KOTELCHUCK: Okay. Go ahead.

MS. BRIGGS: Okay. Let's see. Now I'll head back to -- next I was going to start on our discussion of IREP, but we've already got that started.

CHAIR KOTELCHUCK: Okay.

MS. BRIGGS: Let me see if I can work my way back. Let's see. Well, as I said before, we've got a difference of about 350 millirem, and

a difference in the PoC of about 1.5 percent, as we had mentioned. And, you know, we had this interesting result that we saw which seems to be coming from the fact that SC&A used a 2,000 iteration method and NIOSH used the 10,000 iteration method.

Our Table 3-2 is back up. So, you can see even our doses, they're all falling within the range. What I found interesting was when I went into NIOSH's report, at the bottom of every report that was produced using these 30 runs -- so the iterations, SC&A used 2,000 iterations and generated one PoC per cancer; NIOSH did 10,000 iterations and generated 30 different runs. And the IREP manual guidelines indicate that you average those 30 and that'll be the final PoC for each cancer.

So what I wanted to do was look into the range of values of those PoC values, which I listed here in the last table on Table 3-2. So I was really wanting to see if SC&A's number was falling into that range. And sometimes they're

a little lower, and sometimes they fall right in. So, as I said, we really are operating at what I feel like is the edge of the program, you know. This was like the limit of the IREP program, and we're really dealing with a very fine structure here.

I know Kathy had mentioned that she wanted to bring up a question regarding this table.

MS. BEHLING: Nicole, I thought when we went through this, when we did the average of --

MS. BRIGGS: Oh, I'm sorry, yes. Yes, we were just questioning, when we went in to look at the bottom of each of the IREP reports -- I'm sorry if this is getting kind of confusing. Please stop me if it starts to get kind of confusing and I can backtrack.

In the case records, NIOSH has an IREP report for each of the cancers, which lists the 30 runs and has the average on the bottom. We just noticed that that number was actually not

the number that we've used to generate the total cancer using that multiple cancer calculation.

So we just weren't sure where that number had come from. That's another reason why I put out all of these numbers in this Table. You can see that the average that was posted at the bottom of those reports was actually not the number that was used in the multiple cancer calculator. So we weren't sure if there was another step here that we were missing.

CHAIR KOTELCHUCK: I'm not following you completely.

MS. BRIGGS: Yeah, I'll back it up a little bit. For each cancer, NIOSH will generate -- and this is according to the guidelines in the IREP manual -- when you have a PoC this close to 50, you actually run the PoC values, you do 30 runs, so you get 30 PoCs for each cancer. And then you average that number, and that number -- presuming in the cases where you have multiple cancers, which is true for this case -- you take that average and put it into the multiple cancer

calculator program, which is a subset of IREP, and it will give you the total PoC value. In this case, it was 51.14 percent.

CHAIR KOTELCHUCK: Okay.

MS. BRIGGS: Now, when we looked into those reports and scrolled down to the bottom of the reports and saw the average PoC for each cancer, we noticed that those numbers weren't exactly -- now, we're talking, most of them are very close, but those numbers weren't exactly the values that were put into the IREP multiple cancer calculator.

CHAIR KOTELCHUCK: And what were the values?

MS. BRIGGS: Let's see.

CHAIR KOTELCHUCK: For example.

MS. BRIGGS: Yes. So, if you look at Table 3-2, let's see, if you look at the first cancer, say, the average PoC of the 30 runs came to 1.68, and the PoC that was used in the multiple cancer calculation was 1.61.

Now, obviously, we're talking about

hundredths of a percent, and I'm not sure if that's going to come into play here, but sometimes it's a little bit more. I guess, Rose, for some reason I can't get the whole Table on my screen.

MEMBER RICHARDSON: This is David Richardson. Could I ask a quick question?

MS. BRIGGS: Yes, sure.

MEMBER RICHARDSON: When you're talking about the PoC here, you're talking about the 95th percentile of the distribution from the IREP run?

MS. BRIGGS: Yes. Actually, I think it's the 99th percentile. Yeah.

MEMBER RICHARDSON: The 99th percentile. And so the procedure is to do multiple runs with different seeds and to average the 99th percentiles?

MS. BRIGGS: Yes. Correct. Yes.

MEMBER RICHARDSON: And, I mean, for somebody who worked on the development of procedures, is there any basis for expecting the

99th percentile to be normally distributed?

MS. BRIGGS: Oh, I'm not sure.

MEMBER RICHARDSON: My thought was the reason we were doing the whole IREP/MCMC thing was because we don't have a simple linear equation and the law of large numbers wasn't really going to lead to expecting that either the central -- I would think the 99th percentile is going to be normal, so that you if do multiple draws of it, I'm not sure that you would want to average it.

DR. ANIGSTEIN: This is Bob Anigstein. If I can weigh in on this: From my understanding of statistics, that is a random number that comes out of a random calculation, I see no reason why the 99th percentile from repeated runs would not be normally distributed.

MEMBER RICHARDSON: Well, start with the single distribution. Is it symmetrical?

DR. ANIGSTEIN: I mean, if you do a repeat run side by side I would think that it would be symmetrical and normally distributed.

MEMBER RICHARDSON: But the distribution itself, a single distribution on 2,000 runs, for example, is it normal?

DR. ANIGSTEIN: I haven't actually seen the plots, but basically any time you have a --

(Simultaneous speaking.)

MEMBER RICHARDSON: If it's just the 95th are they symmetrical around the mean? I don't think they are. And so we've got an unstable tail there, because we're generating something from a complex process now. It's not doing 1,000 draws from an underlying normal distribution. It's got all sorts of truncated distributions on these weird tails. I mean, it's just a question. What's the justification for averaging?

I mean, also, when you do MCMC things, you know you've generated all those chains, why not sum them and then take the distribution off combining all the chains? You run it K times, so you've got K times as many runs. That would seem where you would get the better 99 percent bound.

MR. KATZ: This is Ted. David, I would just suggest, this is perfectly good to raise questions about this, but I don't think, unless Grady corrects me, we have the folks on the line that were involved in developing this. It seems like if you want to pursue this you could arrange for having them on the line at the next meeting and then you could have a satisfying discussion of it. But I think it would probably be frustrating to you if you don't have the right folks on the line right now.

MR. CALHOUN: That's correct. That's correct, Ted. We don't have those folks on the line right now and so --

MR. KATZ: Why don't we do that? Why don't we just --

MR. CALHOUN: That's kind of a global thing, too, you know.

CHAIR KOTELCHUCK: Alright. That's fine, because we are not going to be able, as a Subcommittee, to resolve this, so let us get some more information on this.

However, I would like to start back at a much simpler question than David and Bob and others have talked about, and that is: Why is the PoC used in the multiple cancer calculation, let's take the top, 1.61, why is that different than the average of the PoC of the 30 runs? That is, I would assume, if you went to the average of the PoC of the 30 runs as dictated, that you would have used 1.68 for the multiple cancer calculation.

MR. SIEBERT: This is Scott. I can address that.

CHAIR KOTELCHUCK: Could you?

MR. SIEBERT: Yes.

CHAIR KOTELCHUCK: Thank you.

MR. SIEBERT: The reason is because we did not use that second column, the 1.61 and so on. Those are the runs that were run with the 99 random seeds for 2,000 iterations to get our initial PoC to determine if it was in the best estimate range.

Once that determination is made, we go

through the IREP 30 runs process, and we did actually use that second to last column, the average PoCs. SC&A has reported the incorrect final PoC, just probably not understanding the file structure. Everything that's done as the normal IREP structure is filed, is a normal IREP file name. Everything we do under the 30 runs, we actually do that with a version that's called the "Enterprise Edition" (EE) and there's an extension of EE on the end of all those files.

When you look at the EE files, which is what those average PoCs come out for in that last column, you also find there's a combination of all of them for the final PoC with an EE extension, and that final PoC is actually -- let me flip through my pages here -- it's actually 50.99 percent. And that is the final PoC that was reported to DOL, not the 51.14 which is based on that second column. That's why there's a difference.

MS. BRIGGS: Oh, okay. Yes, this actually was our question when we were looking at

this, because we realized there may have been something that was missing, that we weren't seeing with all of these numbers. So, what you're saying, just to clarify, the second column there, those values are the PoC values that were generated from the 2,000 iterations?

MR. SIEBERT: Correct.

MS. BRIGGS: Okay. And then the average runs, that average PoC, I think I'm a little confused about. So the final PoC that was reported to DOL was, you said, 50.99?

MR. SIEBERT: Correct.

MS. BRIGGS: Okay. And that was used generating those, I'll say the second to last, the average PoC for the 30 runs?

MR. SIEBERT: Correct.

MS. BRIGGS: Okay. Now, my next question, and this is something that we actually realized subsequent to publishing this report: Is the version of IREP that SC&A has access to, which I guess it's just called IREP Version 5.8, how is that different from this Enterprise Edition that

we notice is the title on the IREP reports that come from NIOSH?

MR. SIEBERT: The only difference is IREP Enterprise Edition does an automation of the selection of 30 separate random seeds. It pulls those up, it does a random number generation and comes up with 30 random seeds, and it automates getting those and running the 30 and doing the averaging.

If you take the version that you're looking at and run it through with the random seeds that are given at the bottom of the EE IREP runs, you will get the same answers through the normal IREP as you get through IREP Enterprise Edition. You'll just have to run it 30 times with each random seed.

MS. BRIGGS: Okay. And the Enterprise Edition generates the random seed for you?

MR. SIEBERT: Correct.

MS. BRIGGS: Oh, okay. I guess my next question is, can SC&A have access to that version for future blind dose reconstruction? Is

that an appropriate question to ask?

MR. CALHOUN: This is Grady. I don't know if we can do that or not. I just don't know the mechanics of that.

MS. BEHLING: This is Kathy Behling. Also, I see, as Nicole mentioned, you also used DCAL as opposed to IMBA, and can we get access to DCAL? Because we haven't used that in the past.

DR. ANIGSTEIN: This is Bob Anigstein. I can speak to that. I routinely use DCAL. It's available for download from the ORNL website.

MS. BEHLING: Okay. Thank you.

CHAIR KOTELCHUCK: Let me ask folks, is there are any questions about what NIOSH did in terms of -- I'm sorry, let me start again. The question is, did the SC&A report give a proper evaluation of the PoC?

I'm not worried about if you had run their programs you would have gotten this. You ran a set of programs that were supposed to be correct, right? That was one way of doing it. Are you at all backing off, in SC&A, on the number

that you've given of 49.5? Are you thinking that there's an error or are you just trying to see if you could get the same thing as the other person, as the other group?

MS. BRIGGS: Oh, I don't believe -- please, any of the SC&A members please jump in -- I don't believe -- we certainly didn't generate an error. It's just that the nature of the programs, at least, you know, for the Monte Carlo where you can get these kinds of differences, especially if, you know, depending on how you run it and also the edition, this other edition where it will give you a random seed and do 30 runs and that nature.

CHAIR KOTELCHUCK: If each is a correct procedure, then I don't see a need to see what would happen if you had used exactly the same programs.

MR. KATZ: Dave, this is Ted. The one thing that -- unless I missed it, Nicole, and pardon me -- but the one thing that wasn't correct about SC&A's approach is they didn't run it as

the conservative approach with the high number of iterations that are supposed to be used when the PoC is this close. And if they had run that, if they had run that process, then their average number probably would have come over 50 percent. There wouldn't have been this difference.

MEMBER BEACH: This is Josie. Is that normal? Will SC&A run that approach if it's close or is this just something you didn't do on this one?

MS. GOGLIOTTI: We have never done that in the past.

CHAIR KOTELCHUCK: But then the question is, is your procedure not what it should have been, if you will, is it, if you will, incorrect, somewhat incorrect, or incomplete, let's say, in which case you want to run it again?

MR. KATZ: Dave, that's the procedure. I mean, that's why NIOSH has that procedure, to avoid this issue, to have better certainty about the results. Or more robust results, I should say. That's the whole point of that procedure.

CHAIR KOTELCHUCK: So that is, if you will, the specified, and, therefore, proper procedure.

MR. KATZ: Yes.

CHAIR KOTELCHUCK: And SC&A did not use that procedure.

MS. GOGLIOTTI: We have never used that in the past. We have always done single --

CHAIR KOTELCHUCK: Yes, and I'm --

MS. GOGLIOTTI: We can certainly modify our procedures to follow this in the future, but at the time we weren't --

CHAIR KOTELCHUCK: No fault finding. I'm not trying to find fault at all.

MS. BEHLING: Yeah, this is Kathy. It's more of a time issue. Even with all of the other blinds, as Rose indicated, we only do the one PoC, and it's just because it takes a very long time. In fact, I think often NIOSH and ORAU let it run during the night or something, from what I understand. So it just was an efficiency issue, and in the past we haven't run into this,

this is the first time.

CHAIR KOTELCHUCK: Right, okay.

MS. BEHLING: In fact, we should be getting some guidance, I think, while we're on this discussion, does the Board want us to do the 30 iterations going forward?

MS. BRIGGS: Well, Kathy -- I'm sorry to interrupt -- but, Kathy, it looks like the Enterprise Edition actually automates this whole process. Am I correct? It sounds like the Enterprise Edition will automate the whole process so you don't have to manually run and manually assign a new random seed. Is that correct?

MR. KATZ: That's correct.

MS. BRIGGS: Okay. So I guess running the Enterprise Edition would be, I don't want to say as simple, but it would be the equivalent of running one regular IREP run for us.

(Simultaneous speaking.)

MR. KATZ: Can I make a suggestion? I mean, because this is just a resource issue,

really. If they can get the Enterprise Edition, of course, then that puts the matter to bed.

If they can't, I don't think -- I mean, my personal opinion is it's not worth SC&A spending the -- now that we've sort of flagged this matter and understand it, it's probably not worth SC&A spending a ton of extra time just to have the certainty, because they're not delivering the report to a claimant and so that certainly isn't so important. If this arises again, I mean, you'll already have been sort of primed on what's going on here, and it's probably not worth a lot of SC&A resource just to ensure that they get the exact same result.

CHAIR KOTELCHUCK: Well, it's not a matter of that. This is what the -- we reviewed 28 blinds, this is the very first one in which we have a difference in [what would have been the]compensation decision. Now, in and of itself, that doesn't, quote, bother me. That is, we might expect that when things are really close to the 50 percent PoC level.

But, since this is the first, it would not have been so had they used the procedure that now we agree should be used. I am tempted to, as one Subcommittee Member, I would like to see it. I'm not necessarily going to suggest that we do it now, because if we're going to ask people to come in and talk to us about the basic procedure and why we're doing it next time, then, you know, we might want to hold off on requesting this because it's a resource issue.

But what do other Subcommittee Members think? I haven't heard too much.

MEMBER CLAWSON: Dave, to me, in the beginning of this, we had trouble with each side being able to get the same tools. I don't see it is that much of an issue because we have gone through all this and found out what the difference is, as what Scott has just addressed to us. I'm not seeing that so much as a problem as I want all the players that are doing this to be able to have the same tools to play with because it's just important there. I'm now

seeing and understanding better why there was the difference that there is.

But this is the same thing we've always got into is different players playing with different tools, and if one side has got it we've got to be able to see if the other one can use it or whatever else like that so that their questions are answered. But I don't see too much of a problem on this now.

MR. KATZ: Certainly, we'll follow up.

MEMBER CLAWSON: And I understand that, Ted.

MR. KATZ: We'll follow up on that. It's just a question of whether the computer and the people that protect security can make that work for SC&A. That's the only question.

MEMBER CLAWSON: Right. And we've worked through issues on that before and stuff like that. When reviewing this, I was sitting there going, holy cow, you know, to me it's all looking the same, but what Scott just explained

to me now brings better understanding to me of where we did get a difference.

CHAIR KOTELCHUCK: Well, we've been asked essentially do we want to ask SC&A to get the tools and do the procedure, and I think, Brad, you're saying it probably is not important to do so.

MEMBER CLAWSON: That's just my personal opinion.

CHAIR KOTELCHUCK: No, no, and I'm polling folks on the Subcommittee. I'm sorry. I cut you off. I did.

MEMBER CLAWSON: No, go ahead.

MEMBER CLAWSON: No, I mean, what do other folks on the Subcommittee think?

MEMBER MUNN: Well, this is Wanda. I thought one of the purposes in our exercises here was to indicate that even if one uses different but acceptable methods to approach the issue, if the result was similar, then we had essentially proved two sides of the issue, and that's what I see in a case like this. I guess it is -- it

would be nice if everyone did the same thing every time they added two and two, but everybody doesn't do the same thing. And if you use some other method to approach it and you still get four or a very close proximity thereto, then you've, in some ways, indicated the strength of each method of approaching it.

So from my perspective, there's no reason to belabor this to the point that it's necessary for SC&A to spend additional time. In working through the minutiae here, if we have methods which are not the same but achieve the same purpose and the end result is not significantly amiss, then it seems to me that we've proved what we set out to prove.

DR. MAURO: This is John Mauro. I'd like to step back -- I understand the IREP question and where it is. I'd like to move back to the DCAL question. I think one of the important things that come out of these comparisons are: Were there judgments made that are not in the procedures that would apply here?

For example, if a fit was done not using IMBA but was using DCAL, if that's all documented and in a procedure somewhere when you do that and when you don't, then everything is fine because you've got a consistent approach that everyone follows, but SC&A did not follow it. We went ahead with IMBA as opposed to DCAL.

So my question is do we have a potential consistency problem if the DCAL selection is not -- is that clear when you do it and when you don't do it?

CHAIR KOTELCHUCK: Is that professional judgment, in other words?

DR. MAURO: That's my question, yes.

MR. SIEBERT: This is Scott Siebert. That is not professional judgment. First and foremost, we do not use DCAL to fit intakes. That is, all run through IMBA. The reason for DCAL use -- and, Liz, feel free to jump in if I'm going off track here -- the reason we use DCAL is IMBA is not designed with its kinetics for certain specific radionuclides to give a most accurate

answer. We have determined this. We have documented this. And in those cases, we use DCAL for best estimate cases to determine -- to ensure -- that we're using the best tools available.

Liz, do you want to --

DR. ANIGSTEIN: Could I interrupt with a question? This is Bob Anigstein. Scott, do you use DCAL to treat the doses, the intakes as acute? Because that's what DCAL normally does.

MR. SIEBERT: We're not fitting doses with DCAL. Are you talking about for dose calculation purposes?

DR. ANIGSTEIN: No, are you talking about for -- I see, you're talking about how to relate intakes to urine?

MR. SIEBERT: No, we never fit the bioassay using DCAL.

DR. ANIGSTEIN: How do you use DCAL?

MR. SIEBERT: We use IMBA for all fitting of bioassays to determine intake amount.

DR. ANIGSTEIN: Okay.

MR. SIEBERT: And then we use that

intake amount to -- we use DCAL from that intake amount to calculate the organ doses based on the differences. It's a shared versus independent kinetics issue that we run into with IMBA and --

DR. ANIGSTEIN: But normally DCAL is used for -- is designed only for acute intake. IMBA is for chronic.

MR. SIEBERT: Correct. There's a process for actually doing that work. I'm not the guy to answer that. We have somebody very specific who runs that for us for chronic intake, which is the reason -- we actually have those DCAL calculations in CAD as well, so it doesn't have to be independently run each time. The reason it had to be run in DCAL this time is because we were in best estimate territory, as well as they're not full years of exposure. We needed to prorate it.

So, as to the process for assessing a chronic in DCAL, I can't walk you through that. I'm not the guy who does it.

DR. ANIGSTEIN: There is a way --

MR. KATZ: Let me, let me -- this is Ted. Let me please interject at this point. I think it would be fine, it is fine, and I'll actually suggest this related to the Enterprise question and so on, and SC&A's original befuddlement with the different results -- it is fine when you need a technical call to have one.

I think this is level of technical matter is not really matter for the Subcommittee to wrestle with unless we find that there is some issue that the Subcommittee needs to wrestle with. But let's have a technical call on the side to go over these kind of things, they're really not about at the level that they should be for the Subcommittee to be getting its work done.

DR. MAURO: Ted, this is John. I'm not --

MR. KATZ: The problem then, absolutely at that point, you know, bring it up with the Dose Reconstruction Subcommittee.

DR. MAURO: Ted, I'm sorry to interrupt, but I'm not raising a technical

question when I talk about DCAL. I'm raising more one of process whereby is it clear that when, you know, is there a procedure that is --

MR. KATZ: Right.

DR. MAURO: You understand where I'm headed?

MR. KATZ: Yes, it's just the way this conversation has gone --

CHAIR KOTELCHUCK: Well, I mean, this conversation has developed as a result of the Subcommittee looking at the comparisons of the two results. I agree that we are moving to a technical level beyond some of our Subcommittee Members, and I think it would be useful to have an internal discussion between NIOSH and SC&A, and it would be valuable to have someone talk about the procedure of running -- of why and how we run the 30 runs, if you will.

And I think that my own sense is that we are now in an area where I think it would be wise to have those things happen before the Subcommittee, if you will, approves or registers

its final decision on this, on this blind. So I don't think we can, if you will, pass on it today.

MS. BEHLING: Excuse me. This is Kathy Behling. I believe I can very briefly answer John's question, because Nicole put that into this report. There is an OTIB that specifies that under these conditions you should use DCAL for the uranium. She even identified it in this report.

DR. MAURO: That was my question.

MS. BRACKETT: This is Elizabeth Brackett. Beyond that, the IMBA documentation says that it is not correct for particular radionuclides, so it cannot be used for that. And the values in CAD, when you run that, those are actually from DCAL for the specified nuclides also.

So it's been incorporated into all of our assessments, and there's a fair bit of documentation of how that was done, how DCAL was done, and that the dose values came from DCAL rather than IMBA. There's a document that gives

specifically which nuclides are not correct in IMBA also.

MR. KATZ: Okay. Thanks, Liz.

Let me go back to what you're saying, though, Dave, about not being ready. The matter of IREP, that has been in place since the beginning, and it's a whole -- you know, that is a policy that is really not even in the province of the Dose Reconstruction Subcommittee. And I would suggest you do not have to resolve that, which I think has probably been well put to bed before, but certainly Dr. Richardson has a right to hear how that matter was addressed. I don't think it's a matter for the Dose Reconstruction Subcommittee with respect to putting the case to bed, because whatever the matter is with that, it's not a matter of doing the dose reconstruction case correctly.

CHAIR KOTELCHUCK: I thought I had understood that SC&A used the procedure which it will not use again when it comes across this particular kind of problem.

MS. GOGLIOTTI: No, that's not correct.

CHAIR KOTELCHUCK: Okay. And what is correct if it is not?

MS. GOGLIOTTI: It has not been decided whether or not we can get Enterprise Edition and even run what NIOSH has done. For instance, if there was ten cancers, we'd have to make 300 runs of IREP and then do lots of averaging, which is probably not the best use of resources even if we have access to Enterprise Edition.

DR. MAURO: This is John. I have a suggestion. When we are in this very unusual circumstance where we run into this, which this is the first, why doesn't SC&A just flag it and say, listen, I think we do have a difference here and we believe it has to do with this seed and number of runs related to this enterprise version, so at least every one is aware that, yes, we have another one of these circumstances where that difference makes a difference, and we

stop there and just leave it in the hands of the Board.

DR. ANIGSTEIN: If I can cut in, this is Bob Anigstein. We have in-house capabilities of programming, which are not extensively used for this project, where we most likely could create a program, and if we cannot get the Enterprise Edition, we could simulate it at home by creating a program which will drive the IREP program. So, hands-off, the operator would simply specify, I want to run IREP 30 times, and go out and have a cup of coffee and the program will drive IREP to do these runs and collect the results. We've done this numerous times --

MR. KATZ: Let me just interject again. We can deal with this. We have this on the table. If they can get Enterprise, they can. If there's other ways to go at it, we can go at it once we find out that they can't have Enterprise if they can't for some technical reason. But we don't need to spend time on this right now. It's not important right now, and

this can be all addressed in technical calls. It doesn't need to be a Subcommittee discussion on how to get the right equipment to SC&A if they need it.

The only matter that I think the Subcommittee's call is whether the Subcommittee wants to -- and they might as well wait and find out first -- whether they want to spend a large amount of resources if it turns out to be required for SC&A to duplicate the procedure that's in place with these close calls for ensuring that the close call is correct. We don't need to go over it now.

MS. GOGLIOTTI: So we'll set up two separate technical calls, one to discuss the IREP runs and a second to discuss the DCAL. And it sounds like, David Richardson, you'd like to be part of the technical call for the IREP run, is that correct?

CHAIR KOTELCHUCK: David?

MEMBER RICHARDSON: I'm fine just to hear what the resolution is.

MS. GOGLIOTTI: Okay.

CHAIR KOTELCHUCK: And so you would report back to us next time?

MS. GOGLIOTTI: Yes. And any Board Member that wanted to participate in those calls, let me know and we can set that up also.

MR. KATZ: Absolutely.

CHAIR KOTELCHUCK: Okay. That's fair enough. Then we will return to this next time, right, after your calls?

MS. GOGLIOTTI: Correct.

CHAIR KOTELCHUCK: Okay. And if you wrote something up briefly, if you could, before our next meeting, so that the Subcommittee Members could look at it, that would be fine. But if you can't or if that's a problem, then you'll give us a report verbally next time.

MS. GOGLIOTTI: Okay. We can certainly do that.

CHAIR KOTELCHUCK: Okay. I'm satisfied with that. Subcommittee Members? David, you indicated you were okay with that.

Others, any reason, does that seem okay to you?

MEMBER CLAWSON: This is Brad. I'm fine.

MEMBER BEACH: Yeah, this is Josie. I think that's a good path forward. Thank you.

CHAIR KOTELCHUCK: Okay. Good. Wanda?

MEMBER MUNN: Sure.

CHAIR KOTELCHUCK: Okay, fine. It is now, folks, 12:02. It is appropriate to stop for breakfast or lunch, depending on which coast you're sitting on. But I think it might be reasonable to take our break now and then come back to the third blind at one o'clock.

MS. BEHLING: And this is Kathy Behling. Can I just interject one last thing while we're talking about various software programs and things like that? One of the issues that we did get resolved this past day or two because of David Allen's help, I just wanted to make the Subcommittee aware, we had been -- SC&A had not had a version of IMBA that was able to

run technetium-99. And David Allen has provided us with the files that we have loaded into our IMBA program and so we are now able to run that technetium-99.

CHAIR KOTELCHUCK: Very nice. Okay.

MS. BEHLING: Yes, thank you, David.

CHAIR KOTELCHUCK: Yes. That's fine. That is excellent, and that's been hanging over for a while.

Okay. So, it's a few minutes after 12. Let's take a break and resume at 1:00 Eastern daylight savings time. Okay, folks? Okay. See you back at one.

(Whereupon, the above-entitled matter went off the record at 12:03 p.m. and resumed at 1:03 p.m.)

CHAIR KOTELCHUCK: The third case here. Who will be? --

MS. GOGLIOTTI: I will turn the reins over to Ron.

CHAIR KOTELCHUCK: Okay, Ron?

DR. BUCHANAN: Okay, so --

(Simultaneous speaking.)

DR. BUCHANAN: Twenty-four, this is case B-29. And if we'll go to page 8 of the report, Rose. Okay, that looks like it. So we see that this is a EE [employee] who worked at the Mallinckrodt Chemical Company in St. Louis, Missouri in the early years. The EE was monitored by only four film badge exchanges one year. There were no other records of external or internal or medical x-ray examinations in the worker's DOE files. You'll see that both NIOSH and SC&A used the guidance in the TBD for Mallinckrodt. And the OTIB-17 for shallow dose and OTIB-79 for x-rays. And using this guidance, SC&A and NIOSH both calculated the best estimate of the annual doses for each of the cancers. And so if we go up to the previous, page six I believe it is, we see Table 1-1 which lists the cancers and their location and date of diagnosis.

And so we will then go down to the next page, seven, to Table 1-2, which is a comparison of NIOSH's and SC&A's doses assigned

to the cancers. And we see that they're -- the external dose assigned was recorded 250 keV photons and greater than 50 keV electrons, according to the TBD for this site. And we see that unmonitored dose was assigned for the period that the worker had no dosimetry results. And we see we do not assign any medical dose according to OTIB-79 for this site. If you look at the doses to those various cancers there on that page, you'll see that SC&A and NIOSH assigned exactly the same doses for the recorded doses and very, very similar doses to the unmonitored doses -- within 100 rem or so of each other. And we'll go into a little bit of description of that.

See the internal dose. Of course Mallinckrodt processed uranium so they had to have an electron dosage from that and both assigned less than one millirem to each cancer site. We see that the total PoCs and the individual PoCs were very close. The total PoCs ranged from 45 to 50 percent. So with that, we will move on to -- back to page eight and look at

Table 2-1 -- just on page eight and nine. And that's a comparison of the data and assumptions used by NIOSH and SC&A. And I will just cover the ones that showed any differences. The dashes, like we said previously, are the ones that were in agreement -- used the same technique. So either used the same technique for external dose assignment. The only difference was that NIOSH assigned the external dose as an acute exposure, and SC&A assigned it as a chronic exposure in all the external dose assignments. There was no ambient or medical dose assigned.

We see as we go down the table there, we see for internal dose that -- now this is the main difference in these two DRs is that NIOSH used Table A-40 from TBD to assign internal intake and dose whereas SC&A used two dust exposure records that were in the worker's DOE files to calculate the intake in dose. Both used type M-UT34, assigned it for slightly different periods -- and we will discuss that when we get down to that. And because the sources for

different ways of determining intake -- the IREP doses were assigned slightly differently. NIOSH used a constant with no uncertainty and SC&A used a log normal with a standard deviation of three.

So if the -- I won't go into detail on the recorded photon and shallow dose, which is showing at the bottom of that page and the next page, because we assigned exactly the same dosage using the dosimeter readings. And there was no missed dose -- there was nothing below LOD over two, so there was no missed dose assigned. And so that brings us to the unmonitored dose. So if we go down to Section 2.1.4, we get one monitored dose. And we have photon and shallow dose. And we see that the years that worker -- the worker was only monitored for one year, so the rest of the years were assigned unmonitored dose using Table A-41 for photon and A-42 for electrons. And we see that we used the same tables, SC&A and NIOSH, the only difference was that we both assigned it as triangular distribution, whereas NIOSH assigned it as an acute exposure and SC&A

assigned it as a chronic exposure.

The unmonitored photon dose, we'll look at that now. We both assigned very similar doses. The only difference between these, the unmonitored photon and unmonitored shallow doses, was that the worker terminated employment in middle of the year, and so that year -- determining how you calculate the number of days the person worked, the fraction -- it looks like you could assign a fraction of an annual dose. Whether you used days or you used months or part of the year, it comes off slightly differently. So it came up a few millirems different.

And we see if we go down to the unmonitored shallow dose to the uncovered part of the body, which had a cancer, then we see that, again, we assign very similar doses other than the last year of employment, calculating the exact partial year of exposure. And same way with the covered locations. We both used the coveralls -- two pair of coveralls at 0.85 percent transmission. Of course, [we] didn't

apply that to the uncovered area. And assigned very similar doses, again, that varied slightly and depended on how you calculate that last year of exposure fraction.

So we looked at the onsite ambient. Again, there was none assigned through that site because they had been assigned each year -- recorded or coworker dose. And there was no end dose assigned. Same way with the medical dose. There was none assigned according to OTIB-75 because all x-rays were taken offsite. We had agreement on the external doses. Is there any questions of it before we get into internal?

CHAIR KOTELCHUCK: I don't have any.

DR. BUCHANAN: Okay. We will move on into internal dose. Now the Mallinckrodt did have an SEC which stated that it was granted because of lack of internal monitoring records through 1958. So you can use records if they're available. And if they're not, well then you can assign dose. And so however -- in this case, NIOSH and SC&A attempted to assign some internal

intake. And they used two different methods. NIOSH used the Table A-40 intakes in the TBD, which assigns intake for the period 1959 through '61. This was beyond the employment period of this worker. We back extrapolated that to prior years and assigned those intakes to the prior years of employment and found Type N Uranium resulted in the highest dose. And so they used that dose and assigned it to each cancer site and came out to less than one millirem and put that as a chronic exposure using a constant distribution and zero uncertainty.

Now SC&A did not use the Table A-40, but it listed the intakes after the end of the employment period. And it was after 1959 they listed intakes. So SC&A did not use those values and it found two dust data sheets in the workers' files that were measured during the period the person worked there. And so what they did was use that dust information and air sample information there. And you can see, they go through the math there and then Table 2-2, it

lists how to calculate the inhalation and ingestion intake -- used this in the Chronic Annual Dose Workbook and to calculate the dose. And they totaled those for each of the sites -- for each of the cancer sites. So it's less than one millirem, which is similar to what NIOSH got. However, [it was] assigned in the IREP table as a log normal distribution with a GSD of three. So similar results but with using two methods there for internal.

That brings us to the summary. On page 13 we'll see that Table 3-1 lists the dose and the PoCs there. And you'll see that --

CHAIR KOTELCHUCK: Ron, Ron?

DR. BUCHANAN: Yes?

CHAIR KOTELCHUCK: Before we go off of internal --

DR. BUCHANAN: Yes?

CHAIR KOTELCHUCK: I didn't quite catch -- it started in two different time periods. '67 and '69 -- you said you were coming back to it. But I must not have followed what

you just said about --

DR. BUCHANAN: Okay, the -- the table that NIOSH used was A-40, which covered the period 1959 through 1961.

CHAIR KOTELCHUCK: Okay.

DR. BUCHANAN: And the worker worked prior to that period. And so they used that [and] added to the assigned dose back to the worker through '49. Okay? SC&A used the dust load air samples to assign the intake during the period that the person worked back to the original employment date.

CHAIR KOTELCHUCK: Got it.

DR. BUCHANAN: Which was back further.

CHAIR KOTELCHUCK: Okay, fine. Thank you.

DR. BUCHANAN: Yes. And so we see on the summary, then, [that] what we have for the total doses are very much the same -- the external, the internal are very much the same. And so the total comes out similar. NIOSH had 5.6-something dose and SC&A had 5.6-something,

just slightly higher. The total PoC for NIOSH was slightly higher than that calculated by SC&A. And so we want to go through and look at these differences and see why they occurred. And we see that they're very similar -- less than 50 percent. But the slight differences show that the dose was slightly different for SC&A and NIOSH because of how you calculate the last year of employment – the partial year. [This] makes a few millirem difference.

The assignment of the internal dose was that NIOSH took it from Table A-40 for '59 through '61 and projected that back to the worker's employment period back through '49, but not through the beginning of it. And SC&A used some air sample data to calculate and assign an intake all the way back to the original start date.

Now the doses weren't large here. But in the next part, their assignment of combined PoCs, we see that we had the same external dose almost exactly. And NIOSH had a PoC slightly

greater than SC&A's final PoC. And this occurred because NIOSH had a shorter latent period because they had projected it back to '49, which was the beginning [of an SEC class]. And SC&A took their air sample back to the beginning of employment, which was earlier. And so NIOSH had a shorter latent period than SC&A. So the PoC was slightly different than for SC&A -- slightly higher. And I reran some of these and did some exploratory work to look at this to find out if SC&A had a shorter latent period, then it increases the PoC. And so several of the cancers were sensitive to the latent period. So that concludes my presentation. Are there any questions?

MEMBER BEACH: None here.

CHAIR KOTELCHUCK: None here.

MEMBER MUNN: I just have one, Ron, when you said you played around with those, did your numbers get close to what NIOSH had? Even though I know you're not very far off, I was just curious.

DR. BUCHANAN: There were so many

possible different combinations.

MEMBER MUNN: Yes.

DR. BUCHANAN: Because of so many cancers. But, yes, I did try it for a couple of the sensitive cancers and it did come up to very similar to what they had.

DR. MAURO: Ron, this is John Mauro. Since you were above the 45% PoC, did this trigger the differences in the IREP runs that we talked about earlier? Where, you know, you're running of IREP versus NIOSH's running -- did that have any play here?

DR. BUCHANAN: No, It did not -

MS. GOGLIOTTI: John, we never run -- we never change that. We always did the same.

CHAIR KOTELCHUCK: Okay. Now, so I understand. NIOSH did run their 10,000 simulations 30 times, and 50 average? Is that what was done here for NIOSH?

DR. BUCHANAN: I would have to look, but I don't think 45% triggers that.

(Simultaneous speaking.)

CHAIR KOTELCHUCK: Oh, okay.

MR. SIEBERT: Actually -- this is Scott. Yes, it actually does. 45 to 52%. And we did run -- and that is correct. We did run it the 30 runs.

DR. BUCHANAN: Oh, okay. I'd have to go back and look.

MS. GOGLIOTTI: Well, in theory, that applies to every blind dose reconstruction because that's the range we took from.

CHAIR KOTELCHUCK: Good, good. So, I think as a Subcommittee, we should approve -- that is to say, we accept both procedures are appropriate and therefore the comparison is appropriate. And do we agree on that?

(Simultaneous speaking.)

MEMBER CLAWSON: I agree.

CHAIR KOTELCHUCK: Okay.

MEMBER MUNN: I'm agreed.

CHAIR KOTELCHUCK: Okay. I hear no other -- so, I will assume we are all in agreement and we will finish that up, and thank you. Thank

you, Ron, and thank you folks. We will complete the other three – and we will return to the second one, W.R. Grace, next time.

Review Cases from Sets 14-18

CHAIR KOTELCHUCK: Okay, so we are ready to go on to see if we can resolve any of the 11 cases remaining from Sets 14 through 18. I am not sure how folks would like to go. Would you like to start with the AWEs? I just reviewed them in the order that Rose put them in the file. But however you would like.

MS. GOGLIOTTI: If you don't mind, I will just go down my list, just because it's easier.

CHAIR KOTELCHUCK: Okay. Well, what does your list start with?

MS. GOGLIOTTI: Well, I notice that you left the SRS and the INL case off of the agenda, which I assume is because those cases are still with other working groups. Is that correct?

MR. KATZ: Oh, yes, that's me. It's me

that did that, Rose. Yes.

MS. GOGLIOTTI: Okay, yes. So we have not -- there is no change for --

(Simultaneous speaking.)

CHAIR KOTELCHUCK: Right.

MS. GOGLIOTTI: Okay. So I will pull up the DCAS Matrix Search. And the W.R. Grace case, as far as I know, is still being worked on by NIOSH. So there are no changes with that. So we will start with the Westinghouse case, which is Tab 434, Finding number one.

CHAIR KOTELCHUCK: Good. And you reported earlier that 435, Observation One, with the technetium you can now work on.

MS. GOGLIOTTI: Yes, yes. And we will officially close that out when we get to it.

CHAIR KOTELCHUCK: Sure, good.

MS. GOGLIOTTI: Okay, so with the Westinghouse case, it's been going on for some time. The finding states that there was an unsupported method used for determining proton bursts during the residual period. And NIOSH had

agreed with us, just to summarize what's happened so far, that they had not included all their calculations in the DR files. Those files were provided to us. We did review them and SC&A put out a White Paper response to those -- in it we had four recommendations. And NIOSH responded to each of those recommendations. And then we went back and looked at them and with the first one, intel was provided to us to validate that the surface concentration values were correct. And we were able to verify that.

The second was NIOSH agreed to revise the natural thorium activity fractions to represent secular equilibrium. And that was done. Third, NIOSH agreed to revise the resuspension factor, and that was done. And fourth, we were able to validate the air concentrations that were used in this case. And so there was one remaining question after that that we had -- which was why was this case not included when the PER was wrong? And NIOSH responded that the claims had already been

flagged for rework because the new cancer diagnosis. And so it was left off the PER cases because it was already being reworked. And we verified the dates associated with that match up. So we recommend closure.

CHAIR KOTELCHUCK: Okay. Any question, folks?

MEMBER CLAWSON: No, seems clear.

CHAIR KOTELCHUCK: Hearing none, we will approve and go on.

MR. KATZ: And just for the record, David [Richardson] is back online with us.

CHAIR KOTELCHUCK: Okay. Good, David. Thank you.

MS. GOGLIOTTI: Okay. So the next finding from the same case, finding number two, the finding states that the method for determining occupational external dose is inconsistent with the information provided by the EE [employee] in the category report. The EE had a very firm recall that he was consistently monitored. And NIOSH has agreed with us and they

felt that the records were fairly complete for this site. But at the last meeting, the Subcommittee asked NIOSH to go back and look at the records. The EE had said that their coworkers were monitored. And so NIOSH did go back and reviewed the records and did find some of the coworkers that were mentioned by the EE in the category report, and they were found to have smaller doses than were assigned by the ambient dose. So based on that, we recommend closure because the ambient dose was, in effect, more conservative than it would be had this person had records.

CHAIR KOTELCHUCK: Okay. Sounds good. Questions or concerns?

(No audible response.)

CHAIR KOTELCHUCK: Good, okay. We will close then on that.

MS. GOGLIOTTI: Great, okay. And the next one I have is 435, Observation One.

CHAIR KOTELCHUCK: Four thirty-five, I -- did I miss that?

MS. GOGLIOTTI: This one was actually the Brookhaven Case. These are the cases that Kathy mentioned earlier, that we literally just got the files -- it was either this morning or last night -- for running IMBA for this tech-99 issue. This is all very new. I believe we were just waiting on the software and had already verified. We were able to verify the results, but we didn't -- we couldn't use IMBA that they had. So we've now been provided that. And Kathy, correct me if I am wrong, but we are -- we have been able to get the tech-99 to run?

MS. BEHLING: Yes, yes we have. I have. But I haven't done this case.

(Simultaneous speaking.)

MS. GOGLIOTTI: Okay. But -

MS. BEHLING: Yes, we just got this file -- just yesterday afternoon. But were able to run that a little -- I haven't done that for this case yet.

MS. GOGLIOTTI: So if it's okay with the Subcommittee, I would like to just check into

this issue to make sure that everything lines up with his observation and then we can close it out at the next meeting?

MEMBER MUNN: Yes.

MEMBER RICHARDSON: Sounds good.

MEMBER MUNN: It sounds reasonable.

MS. GOGLIOTTI: Great. Bob Barton, are you on the line?

MR. BARTON: Yes.

MS. GOGLIOTTI: Okay, and the next one is your case, 436.2.

MR. BARTON: Okay, let me just -- we've had a lot of back and forth on this one. So I will quickly kind of give the history on it.

CHAIR KOTELCHUCK: I -- just to say, I am back on the line. I was just cut off somehow. When I left we -- hello?

MS. GOGLIOTTI: Yes.

CHAIR KOTELCHUCK: When I left we were on 435, Observation One. And I started to say that the technetium case that you're working on now, right?

MS. GOGLIOTTI: Yes, sorry. We weren't sure if we'd lost you or not.

CHAIR KOTELCHUCK: Yes, yes. You lost me. I don't know, maybe it was in on my phone. Good, I am glad others were okay -- so that is fine. So we will hear a report on that next time. Okay.

MS. GOGLIOTTI: Okay, and so then we are going to move on to 436.2.

CHAIR KOTELCHUCK: Good.

MR. BARTON: Okay, and this is Bob. I can report out on that. Essentially, this is another BNL case. And what we encountered was that for this particular energy employee, they had a rather unusual external dosimetry reporting format in their files provided by DOE. What we basically saw was the EE had a total gamma dose and a total neutron dose reported by quarter. However, we know that the site monitored workers on a monthly basis. So essentially, we only had summaries. And you have to sort of come up with a framework to break down -- break up those

summaries into hypothetical badge exchanges. Particularly when you're going to calculate mixed doses, which was really the crux of this matter.

And the other question was, we have a total by quarter for gamma and neutrons, but no indication of beta. And that exposure source is important for this energy employee. So our question was originally, you know, how is shallow dose really going to be dealt with? So we went into the IREP file provided with the dose reconstruction, and we did find that, for at least one of the quarters, the shallow dose was assigned. And it appeared to be assigned as a missed dose for one half of one badging cycle. As you all probably know, when you assign a missed dose, you essentially take the MDA, divide it by two, and assign it to whatever badging exchange period it needs in this dose. In this case it was MDA over four, which is -- you know, it looked highly irregular for us.

So our original finding was basically, we really don't understand how the shallow dose

is being assigned here. One, we don't see any record of beta dosimetry in the claimant's file. And in the dose reconstruction, what's there is essentially half a missed dose. So what we found out from NIOSH -- because they have, obviously, a lot more experience with the BNL dosimetry records -- was that during this time frame, beta doses would only have been reported if they were positive. Or put another way in the explanation, if the open window of the dosimeter was greater than the shielded component, then you would have a positive beta dose, and it would appear in the record. So the implication is that if you don't see any beta dose, there is no measured beta dose and only missed dose is needed to be considered.

And so we went through and we did our own missed dose calculation essentially, and this is in our response back in September. And we assumed that, you know, if there's no beta doses -- measured beta doses reported -- we are going to assume every single dosimeter was zero. And that every corresponding gamma dose for each

dosimeter was either positive or zero, depending on whatever the total was for that quarter. And this is sort of how we interpreted the method that was in OTIB-17. Early on, it's on page six, where it kind of gives you the steps on a generic method on how to do it.

Now since then, NIOSH has clarified in further responses that the way to interpret these dose records of BNL is if you don't see those beta doses, it's assumed your open window is equal to your shielded for each badging cycle. Which is a very important assumption, because once you see that, you can start going into some of the procedures for other sites, which are actually contained in OTIB-17, Appendix B, which provides specific instructions for SRS, Hanford and the Gaseous Diffusion Plants. And in that procedure you can see that, well, if you have an open window measurement that's equal to the shielded measurement, you do not assign any missed beta dose in that case. You actually only assume there's a missed beta dose if your open

window is zero and your shielded dose is positive.

So once we had that assumption from NIOSH, both that you only see beta doses reported if they're essentially positive. And the way that the dosimetry system worked at BNL -- if you don't see any reported beta doses, it's assumed the open window equals the shielded. Then you can start applying OTIB-17, like I said, the procedures for SRS, Hanford and the Gaseous Diffusions. And also there's a framework for how you kind of split those out, as a best estimate, which is in Proc 6. And so once we had those assumptions in place, we were able to follow the method and get the same number that NIOSH did.

So essentially, I kicked this around with Ron Buchanan and Doug Farver, because, like I said, we had done our own calculations that looked directly at assumed beta and gamma doses -- and obviously all the betas were zero and the gammas are either positive or zero. And we came up with a different value for the missed dose,

but we really find no technical flaw with the way NIOSH did it either. So we recommend that that the finding be closed. However, we sort of have the caveat that, given the complexity and sort of unusual nature of these external dosimetry records, it certainly would be beneficial to update the Site Profile and DR guidelines to show exactly how these external dose records should be interpreted, especially in best case, best estimate, situations. And given [also] the unusual format that we encountered in this specific case and this specific time period. So I guess that's the long and the short of it. Certainly love to entertain any questions.

MR. SIEBERT: And this is Scott, to add that last piece. As you can see, the last entry is that we state, we have actually updated the DR guidance document to clarify that. So the documentation is there as well.

CHAIR KOTELCHUCK: Okay, so it sounds good. So we will close on that?

MR. CALHOUN: Hey, this is Grady. Is

there a chance that that one should be switched to an observation?

CHAIR KOTELCHUCK: That's a good question.

(Simultaneous speaking.)

MR. CALHOUN: Seems like after you worked through it, we were all right.

CHAIR KOTELCHUCK: Yes. Let me think.

MEMBER MUNN: Sounds like a prime candidate for observation to me.

CHAIR KOTELCHUCK: Okay. Any others?

MEMBER CLAWSON: This is Brad. I agree.

MEMBER BEACH: And Josie, I agree.

CHAIR KOTELCHUCK: Okay, so we -- switch 436.2 to an observation. Okay, so close -- go to observation. Okay, good. Alright, I think that does finish all the DCAS ones that we had left?

MS. GOGLIOTTI: That does.

CHAIR KOTELCHUCK: I think it does. Now, 369.3. That's the last one.

MS. GOGLIOTTI: The 369.3, that was the W.R. Grace one --

CHAIR KOTELCHUCK: Right.

MS. GOGLIOTTI: And NIOSH is still working on a coworker Pu dose during the operation period. So that one's not ready. --

CHAIR KOTELCHUCK: Okay, fine. Three sixty-nine point three. Okay, good.

MS. GOGLIOTTI: John Mauro, do I have you on the line?

DR. MAURO: Yes, I am here.

MS. GOGLIOTTI: Okay, the next one up is the Ventron Case that we were talking about yesterday.

DR. MAURO: Oh, yes, yes.

CHAIR KOTELCHUCK: Okay, at the AWE site.

MS. GOGLIOTTI: Getting it pulled up here on the screen. Being a little slow.

CHAIR KOTELCHUCK: That’s okay.

MS. GOGLIOTTI: I'm hoping we, obviously, keep power throughout this whole

meeting. This storm is pretty bad.

CHAIR KOTELCHUCK: You guys are getting battered?

MS. GOGLIOTTI: Oh, Boston is getting battered. We are supposed to get 18 inches.

CHAIR KOTELCHUCK: Okay, you'd better have --

(Simultaneous speaking.)

CHAIR KOTELCHUCK: You'd better have it shoveled off by this weekend, because I am coming to my granddaughter's birthday party on Saturday. And I don't have snow tires.

MS. GOGLIOTTI: It will be gone by then, don't worry. We can handle our snow.

CHAIR KOTELCHUCK: Okay.

MS. GOGLIOTTI: Alright, now 433 and we're going to start with finding number three on that.

CHAIR KOTELCHUCK: Okay.

MS. GOGLIOTTI: Alright. And the finding originally stated that it needs to have a discussion on the appropriateness of using TBD-

6000 as the surrogate for calculating external dose from uranium reduction operations that took place in the early 1940s. And John, I'll turn the other… -

DR. MAURO: I will get it started. I think, Scott. You had responded to this yesterday and I had a chance to look it over. And maybe the best way to go is to tell -- to explain what I understand the circumstances are and what my perspectives are. But if I misrepresent anything, please help me out.

MR. SIEBERT: This is Scott, John. Just to let you know, I believe Dave Allen from DCAS will be handling this because it is not our site.

DR. MAURO: Okay.

MS. GOGLIOTTI: Yes, this is correct. It's actually Dave Allen's response.

DR. MAURO: Oh, okay. My mistake, I just assumed it was Scott.

MR. SIEBERT: That is okay.

DR. MAURO: Well, I will start.

Ventron, AWE, it's a uranium conversion facility. And it has an SEC for the early years. I think '42 to '48. Okay? So that's sort of like the setting. And what we have is a case of a worker where you needed to reconstruct his doses during the residual period, which is not covered by the SEC. And the question that I raise is typically when you reconstruct doses during the residual period, we need to start -- well what's the amount of radioactivity deposited on surfaces? And from there you could estimate the external dose and you could -- and this is uranium -- and you could, using resuspension models, estimate the inhalation dose.

The original concern I raised was, well, if you don't actually have measurements, what you often do is you resort to one of these generic AWE guidelines, TBD-6000 and there's TBD-6001. Now, we know that TBD-6001, which would apply and used to apply to conversion facilities -- that has been withdrawn. So we are really left with TBD-6000 as being a generic approach

when you don't have site-specific data. Stay with me now.

And I believe NIOSH employed some generic information that was in TBD-6000 that in theory really should only be used for metal -- uranium metal handling facilities, and not conversion facilities. And that sort of triggered my first concerns: Gee, I see they're using TBD-6000 protocols, which really don't apply to conversion facilities. But then David, I believe -- and correct me if I am wrong -- in your response that came in yesterday [you] said, well, really TBD-6000 strategy can be applied here. In other words, the method that is adopted in TBD-6000 for uranium handling facilities could also apply to uranium conversion facilities for the residual period. And I believe the approach that was taken was to assume that the airborne concentration that was responsible -- the uranium airborne concentration -- that was responsible for the surface contamination that settled -- I believe you indicated, you assumed 10 MAC. And

justified it on the basis that well, there is some data, actually, in Table 6-1 in the SEC Petition Evaluation Report that will support that.

Okay, so that's sort of like my understanding, David, of your position that, well, it's okay to use the 10 MAC as your starting point to reconstruct. 10 MAC, by the way, I believe is the order of 700. Anyway, I forget the exact concentration. But, it's a fairly elevated level of airborne uranium. And that's the starting point to determine what might have settled out. From there you could go on to reconstruct external, internal. And I looked at that and I said, well, that seems reasonable, especially since I wasn't quite certain about the applicability of TBD-6000 to a conversion facility. But then I said, well, that's really not the major point here. That may or may not be -- and I didn't go into enough research into the degree to which you could use TBD-6000 and this circumstance for a conversion facility.

But David did point out Table 6-1 of the SEC Petition Evaluation Report that there was some data in the early 1940s on the airborne dust loading of uranium at the conversion facility. Now, here's where I really bring you something I'd like to hear a little bit more about: You have to keep in mind that the SEC was granted to this facility because there wasn't sufficient data to reconstruct external or internal. And now clearly, on the airborne concentrations, they represent that in Table 6-1 with the airborne uranium dust loading a limited amount of data. And it's quite scattered. In other words, the concentrations that were observed -- I think the highest number was 7,200 micrograms per cubic meter, which I believe converts to about 100 MAC. So what I am getting at is, that we have a bit of a dilemma. We have some data on the airborne concentrations, which admittedly was not sufficient to reconstruct internal doses for workers and that's why the SEC was granted. That's a premise I am working on. I believe

that's a correct statement. If I am incorrect, you can stop me. But at the same time, it was felt that, but we can use that data to estimate what might have deposited on surfaces, and thereby represent the source of contamination that workers much later, during the residual period, might have been exposed to.

So the thing I'd like to talk about a little bit is that a reasonable approach to reconstruct exposures during the residual period? Namely, using data collected earlier that was judged insufficient to reconstruct doses, and thereby, you know, resulted in an SEC being granted. And in addition, when I look at the table, there is a broad range of concentrations, this is Table 6-1, a broad range of concentrations. And the highest concentration looks like it was considerably higher than 10 MAC, which is the number that was defaulted to as the basis for deriving what might have been deposited on surfaces.

So I still have some concerns. I think

it's something that the level at which I evaluated it yesterday in preparation for this meeting just to launch a discussion on the matter. So with that, I'd like to turn it over to David to see if I fairly communicated the nature of the issue.

MR. ALLEN: I think you have, as far as how the issue has morphed a little bit since the beginning. But this particular dilemma is something that we have -- we've gone down that road in a number of sites in the past and a number of SEC evaluations, actually. And we have reached agreement in the past a number of times on the residual contamination, you know, any intakes or external dose from residual contamination. That contamination is going to be something more related to the temporal and spatial averaging of the operational airborne. It's -- you're not going to find a peak air sample at one point in time during an operation and assume the entire area is covered, you know, with that -- a deposition from that airborne when

you've got so many others that are low.

And in the past we have, as I said, basically reached agreement that the contamination levels associated with the residual period will be based on more than average operational airborne and in that way you can have a sparse amount of air sample data during the operational period -- you might not be able to really judge how high it could be during the operational -- but it may still be sufficient for determining the contamination levels as your starting point for the residual period. That is essentially the basis why there's a number of AWEs out there that have been granted an SEC during operations and not an SEC during the residual period, simply because the uncertainty can have a big effect during the operational period, but a much lesser effect during the residual.

DR. MAURO: I heard you and understood what you were saying. Now, the only little twist in here, and I agree that what's on surfaces

represents what I would say -- if you have a number of air samples that were collected and they vary widely at a given time. What's deposited and then available for resuspension really represents more of an averaging. But I guess, one of the things that just struck me, was, if you have a time period where you're measuring relatively high concentrations for some period of time, and that that's --

(Simultaneous speaking.)

CHAIR KOTELCHUCK: Could I interrupt you one second?

DR. MAURO: Sure.

CHAIR KOTELCHUCK: My screen is blank. Are other people's? And if they are, would somebody look into that while John finishes speaking?

MS. GOGLIOTTI: Mine is showing, but if it's not, I can reboot it.

CHAIR KOTELCHUCK: Mine is loading, and --

MR. KATZ: Yes, mine is showing. It's

showing for everyone else, I think.

MEMBER RICHARDSON: Yes, it's showing for me.

CHAIR KOTELCHUCK: Okay. If you could, please take care of it. Anyway, John, I am sorry to interrupt you. I thought we could -

DR. MAURO: No, that's okay. The way I see it, is sort of like a layered question. Let's say you have an operation going on in the 1940s, and at some period of time you're measuring relatively high concentrations -- airborne. And then at other times, you're measuring relatively low concentrations. So I could see how the concentrations could vary over time. And then you say, okay, but now what I want to do is estimate what might have accumulated on surfaces during that time period? Okay. And I know that you assume that deposition occurs over some 30 days and that's your accumulation. Now, wouldn't you say, okay, if I had some stretch of time in this facility now where I was observing for that period of time an

elevated concentration. And during that time period, of course, you would have had deposition and it would have accumulated on surfaces, reflecting what was airborne over that time period. Let's say whatever that time period is: a few months or whatever.

Then, of course, you have other measurements -- later or possibly earlier -- where the concentrations were lower. So, from that perspective, so do you really want to average over time? I could see why you would average -- at the same time, if you've got a lot of different measurements of airborne concentrations over the same time period, you would say, okay, what's on the surfaces is going to reflect the average concentration that was in air over that time period. But if you have a time period -- and this is what's not really clear -- where for some period of time you have a high concentration, wouldn't the activity that is accumulated on surfaces reflect what deposited over that time period when the concentration was

elevated? That's sort of like my first layer of question. And I'd like to hear, you know, your perspective on that.

MR. ALLEN: I am not quite sure what you're saying there, John.

DR. MAURO: Okay, let's say we're in the room you're in right now, okay? And maybe we will -- we are working with uranium, okay? And over a two-month period we are doing a lot of work, and we generate relatively high concentrations of airborne uranium in the room you're standing in right now. And over that time period, that airborne uranium is settling, okay? And it accumulates on a surface. And it -- you've got it on your surface now from that activity. But let's say three months from now, the airborne concentration is much lower. And the question is, at the end of operations, which may be three or four years from now you say, well, what's on the surface? And now you've entered the residual period, what's on the surface? What do I assume is on the surface? Well, one could argue what's

on the surface represents the time averaged and area averaged concentration? Or, do you say well, listen, we know we had a stretch of time when it was relatively high, and that's what's going to be on the surfaces once you enter the residual period? Do you see the distinction I'm making in terms of using average versus using high end?

MR. ALLEN: I think I do. I mean, if you had a short burst of high airborne, it will add to your average airborne and it will add to your surface contamination calculations.

DR. MAURO: And I agree with that. I didn't necessarily say short burst, though. Let's say that's what was a little uncertain. But I think that we understand the question. If it's a short burst, sure. [And] for example, if you're assuming that what's deposited on surfaces represents an accumulation that occurred over several months and then it sort of stabilizes, you reach sort of what you would call an equilibrium where the rate of removal is equal to

the rate of deposition, or in other words, you don't just keep accumulating year after year after year.

But if there's a stretch of time where you get deposition, you know, do you use -- and it's not just a burst. Let's say, one day. -- but it's really an extended period of time -- and I will be the first to admit that I can't say for certain whether that 7,200 micrograms per cubic meter represents, you know, one air sample taken at one location at one short time period, or whether it represents something that might be more protracted. So therein lies a reasonable enquiry that I did not look into. And you may be correct that that high number may be just a relatively short-term number. And it does make sense when it's a relatively short-term number to start the average over, you know, all of the different numbers.

So that's just a question I pose and like to leave on the table. And whether there's an answer now for that or not -- but let me pose

the other simpler question, which I would like also to put on the table. And that is, given that the data that's in Table 6-1 representing it is quite limited and quite variable -- and certainly not adequate to reconstruct inhalation doses to workers that were there at that time, would you want to use that data for the residual period? And I think you've answered that. You basically said well, it's not good enough to reconstruct doses during AWE operations. This is what I think your answer is. But it probably is good enough to reconstruct doses during the residual period.

And I'm not entirely sure you know -- I am not going to really debate that. But I do want to put that on the table as a thought problem for all of us to think about these two layers. You see the two -- so I have a two-layered question that I think we'd do well to air out a little bit because I think it does establish a precedent and a strategy for dealing with these circumstances that's not uncommon. You see --

and I will stop in a second -- usually what we have is we have airborne concentration measurements that were taken during the AWE period and represent pretty good numbers for the end of the operations period. And we'd all agree, yes, we believe that this concentration is representative, what was going on during operations at the end of the AWE period. And that's our starting point for reconstructing the doses during the residual period. And that's something that I agree we've all done. And it's perfectly in accord with, you know, what would be considered reasonable and in accord with TBD-6000. But now we have a little bit different circumstance. And I am not sure whether this different circumstance we have right now can be addressed in the conventional approach that you have adopted.

MR. ALLEN: You know, John, I really don't understand why this is different than some of the things we've done in the past. I mean, for one thing I want to correct you that we didn't

say this data -- we didn't say either way that this data was good enough to estimate dose in a residual period or not. You've got to remember, we used to model a lot of TBD-6000. And then as part of this response, we compared it to those air samples that we do have and showed that 15 out of the 17 air samples are below what we used in the model. Just essentially saying that it seems to be representative because their samples were taken during the operational period even though that's early 1940s, that was another issue -- I think you had articulated on this.

DR. MAURO: Right.

MR. ALLEN: And I think you've already said that you have agreed in the past that the averaging is probably more indicative of what's happening in the residual period. And I am at a loss for exactly what your issue is and what the difference is from what we've agreed to and the Board's agreed to a number of times in the past.

DR. MAURO: Okay, I will try to explain where I see the difference. I think it's

one thing to say we've got lots of airborne data -- concentration data. And it might have a distribution [of measurements] that were collected, let's say, during the last year or the last two years of an AWE operation. So you've got lots of data -- air sampling data -- and they may have quite a spread. And I would say the distribution, representing the variability in space and time of the concentration that was in the air at the end of operations period. And that's what we're going to use to predict what's on surfaces.

The only difference here we have now is we have a sparse amount of data that's quite variable. And we then somehow use that data that was collected in the early 1940s to predict what was on surfaces that we're going to use for the residual period. And you correctly point out, some of those measurements -- I don't see those as being measurements that are indicative of the average concentrations that were air. I see it more as, well, we've got some measurements that

were quite high and they're sparse and some low. And it's hard for me to get comfortable with the idea, geez, why not use the higher end value? Because in theory, that higher end value might have been present for an extended period of time. And that might be what resulted in what was deposited. And of course, you know, at another time, you might have had some lower concentrations, which you could have deposited. But you do know that it may have been some time when you had a relatively high, and that's going to be there on the surface. Do you see the distinction? It's already settled out. It's sitting there and it's not going away very quickly. So I am saying, geez, if you really want to be claiming favorable, why not go with the upper-end value of it? Because that might be, in fact, what was residual once you reach the residual period.

CHAIR KOTELCHUCK: Isn't the 7,200 micrograms per cubic meter the high value?

DR. MAURO: That is the high value,

but we don't know how long that went on. But let's say that was there for quite some time. There was an operational period -

CHAIR KOTELCHUCK: Right, okay.

DR. MAURO: And here's where I haven't checked out -- well, when you use data as the basis for what is on surfaces, and not the lower value -- saying, okay, what's the plausible upper-bound concentration on surfaces? I would argue, well, it would be the concentration on surfaces with the airborne levels are relatively high for a protracted period of time. That would be what accumulated.

(Simultaneous speaking.)

CHAIR KOTELCHUCK: Okay, John -- go ahead.

MEMBER BEACH: John, this is Josie. Would that take him out of the 6000 and into facility data at that point?

DR. MAURO: Yes. Now we've left TBD-6000 now, right? I mean, that's why I say it was two levels. The first level was, gee, why would

you use TBD-6000, which does not apply to conversion facilities? But all the sudden, that issue goes away, when in fact you have -- the argument is -- well, we do have some real measurements that were taken. And granted that they were limited, but here they are. And certainly they were not good enough to do any dose reconstruction during AWE operations, but in theory, we could use them to place a plausible upper bound on what might have been on surfaces later on, during the residual period.

And all I am saying is, well, given that we can use that limited data to predict what's on the surface -- well, can you really do that given that it was limited? It was a limited amount of data and we really don't know what the distribution was because of the limitations. But given those limitations, if you were going to say, let's put a plausible upper bound, let's go with the higher end value, which is I think 100 MAC. And that's where I am coming out. I am coming out, geez, if I was doing this right now,

based on what I know, I think I would go with the higher number just to place an upper bound on what might have been on the surfaces during the residual period. I mean, really conceptually you can understand where I'm coming from. Now, that doesn't mean you agree with me. But that would be my inclination right now -- how I would combat the problem.

MEMBER BEACH: Well, it seems to me that you have raised a couple of different issues and NIOSH would need to answer whether OTIB -- or, excuse me, TBD-6000 is appropriate for this application. And then on to the facilities. So the ---

DR. MAURO: Yes.

MEMBER MUNN: John, this is Wanda, and I always hate to disagree with your position on these things because I respect it so highly, but this is just another type of question that we've gone over probably 50 times in the last 15 years.

And that is the question of what you can derive from the information that you have.

We can't prove what did or did not happen, and we've based a large portion of this entire program on you can't prove that didn't happen.

And when you can't prove it didn't happen, then you're putting yourself in the position of not being able to adequately utilize the data that you do have.

So, I thought we had, at some juncture, almost reached the point where we agreed we have to use the data we do have.

DR. MAURO: Yes.

MEMBER MUNN: And I follow your line of reasoning but I'm a little hampered here because, one, I haven't seen the raw data and if I haven't seen the raw data, then I'm really poorly equipped to try to respond to your question, when I assume you've seen the raw data.

But if one goes the route that you're thinking, in my mind, what you have to do is make such a long list of assumptions that it overrides the number of assumptions you have to make that had been made in the current circumstances.

For example, if you decide that you're going to go with that highest number you have, and you have a range that goes from, say, 50 to 700.

DR. MAURO: Okay.

MEMBER MUNN: And most of the material that you have gives you a number somewhere around 100 but you say, okay, I'm going with the 700 because that's the most that could have been deposited.

And then my next question is, deposited where? We have all agreed that these air samples depend upon, we started out by saying they depend upon size, air flow, all kinds of things.

So am I going to take the highest number that I've seen, which is significantly out of the normal range of the others, and say that is what has been deposited all over this entire area and what people ten years from now are going to have to cope with as leftovers?

Now, when we talk about residual

periods, we seldom -- at least I haven't seen come before us a case that would have the absolute ideal information we'd all like to have had: Someone go around at the last day of the operational period and taken swabs off every available surface and had that neatly recorded somewhere.

But even if somebody had done that, the way our world works, in some fury of file-reducing, probably those records would have gone out with RIDS 20 years later. And we're now 50 years past that.

So the point I'm trying to make here is I don't see how we can give any more credence or any more weight to the single high sample than we can to the single low sample, simply because we don't know all of those other pieces of information.

Even if we knew it, it would be an exercise in probability still.

So if we haven't decided that we're going to use the information that we have in the

best way that we can do it, then I don't think we can do what we're trying to do, if that makes -- I know, this is not --

DR. MAURO: I agree with you completely.

MEMBER MUNN: This is not good science but it's common sense.

DR. MAURO: And I agree with you completely.

And I think that the essence of the difference between what we've done in the past and the circumstance we're confronted with here, which is different than anything we've done before, is in the past, we've always had either airborne samples, a number of airborne samples or a number of swipe samples taken toward the very end of AWE operations.

And that was our launching point.

The difference we have here now, this makes this site a little different than everything else we've done before, is that the data that we do have is represented as quite

limited and collected earlier, and it's highly variable over time, by orders of magnitude.

And now we're basically trying to make a judgment, given that now the kind of data we have really represents some earlier years where the airborne dust loadings were quite variable.

And I'm not quite sure right now to say that we have a stretch of time where there's a high level and then a stretch of time where there's a lower level.

And then we're going to say, well, somehow we want to use that, even though we know it's limited and represented as being limited.

But somehow, we would go on to say, well, look, we do have air data and let's somehow use that data to predict what might be on surfaces.

And we want to place a plausible upper bound on what might be on surfaces later on -- several years later when we're into the residual period.

And I would argue that under these

circumstances, which I think I don't recall we've ever had this before, what is the prudent strategy? Do we go with the averaging of the limited data that we do have?

And under those circumstances, if we all agree that, yes, that's the prudent approach, and MAC is certainly a good number, which is what they used as the launching point, then one could argue, well, under these circumstances, is it perhaps more prudent to go with the higher-end value, which is I believe ten times higher, if I did my numbers right, because of the circumstances we're in?

So I think that's really the essence of the precedent that we're about to establish, because I don't think we've had these circumstance before.

MEMBER MUNN: Let me ask one more question.

Well, no, actually, there are two questions there, one, the first question being, in the raw data, which is the approximate period

of time between the data points that we do have?

DR. MAURO: I don't know that we've done enough homework to answer that question. We may have access to that information, we may not, I'm not sure.

MEMBER MUNN: And the next question, which to me would be a critical one, is the highest value that we have the last value that we have, or was it in the middle somewhere?

(Simultaneous speaking.)

We don't know anything about the housekeeping, we don't know anything about the placement of the air monitors. We don't know.

DR. MAURO: Right, and I'm with you 100 percent. There's nothing you're saying that I disagree with.

Basically, what I tried to deliver to you was how I, after getting the response yesterday, gave a little thought to it and tried to think of what I think the issues are that we have to come to grips with.

And I think everything you pointed out

is absolutely correct but I have to say I have not gone that deeply into the issue.

I know David maybe has more information available that he could help us with so that he could help make a judgment of where do we pick it? Do we go in at the 10 MAC or do we go in at the 100?

CHAIR KOTELCHUCK: Let me ask you -- Dave again -- John, so what do you suggest?

You're telling us that there's a problem and that there may be a better approach, that this is an inadequate approach.

And you're saying, you know, what would you do and I don't understand, but is it not incumbent on you to try to make an estimate based on your best understanding of the data, and then present it to the group?

DR. MAURO: We could do it that way. I think the path forward that was just laid out by Wanda is exactly what we should be doing.

And whether we do it or NIOSH does it -- usually NIOSH does it when we come up with

some thoughts, and to a certain degree, they may have some answers already -- it's mainly, as Wanda said, you do have a limited amount of data taken between ‘42 and ‘48, right?

Here are the numbers, they're in Table 6-1.

The question then becomes, as Wanda properly asked, well, do those numbers represent short-term samples taken at different locations at different times?

And if that in fact is the case, that these are short-term measurements taken at different locations at different times between ‘42 and ‘48, then I would completely agree that the 10 MAC approach adopted by NIOSH is certainly the reasonable strategy to take.

However, I would argue if the measurements that we're making represent, for example, the higher-end value represented that this was across the board throughout the facility over an extended period of time, let's say several months, that's a good number.

So, all of the sudden I say, well, if that 7200 micrograms per cubic meter was taken, which actually represents a longer-term, area-wide and time-wide, then I would say, no, you've got to start pushing up that closer to a higher value, not the average value.

And so I think that's what I believe to be the proper path forward. And whether that information is available to us or not is certainly a question, but that's what my suggestion is.

CHAIR KOTELCHUCK: I'm not clear who's supposed to do that, whether it's NIOSH's responsibility or SC&A's to -- you raise an issue, and it's an issue that we need to come to grips with. Who should do it? Ted, maybe you --

(Simultaneous speaking.)

-- understand procedure better?

MR. KATZ: So, normally, yes, NIOSH would follow up on these questions, questions of when were the measurements made and over what period and location.

Normally, NIOSH would follow up on that, on issues raised like this.

MR. CALHOUN: Yes, Ted, this is Grady, and normally, we do look at some things like this, but this is really getting into the, you know, I think there kind of, sort of may be something that's not quite right.

I think that John needs to go do a little bit more homework and tell us where we're wrong.

I mean, he's really laying out a very generic issue here and it's going to cause us a lot more work because he already said he didn't take a look at a lot of this stuff.

DR. MAURO: Oh, no, I'm pointing out my impressions on the answer you provided on where I think there might be some weaknesses, and what type of follow-up investigations will help close the circle.

And certainly, SC&A would be glad to follow up on it, but usually, the way these things have played out in the past in my experience is

that, when we raise an issue like this, usually, it's something that NIOSH does as opposed to the contractor.

But I'd be more than happy to do it, of course I would.

MR. KATZ: Grady, I think this is absolutely ordinary for NIOSH to follow up on these questions. That's what they're supposed to do in unusual situations.

MEMBER CLAWSON: This is Brad, but I think John needs to give them a little bit more direction of whether the issue is at.

I still think John needs to sum this up and send it to them so they can address it.

CHAIR KOTELCHUCK: Brad, I really agree with you.

I feel like, John, if it's not clear to the folks at NIOSH what you're asking them to do, then you need to talk with them maybe on a technical call and figure out what needs to be done.

DR. MAURO: We could do it that way.

I could write it up and send it in.

MEMBER CLAWSON: This is Brad. I would really like to see you write it up because I'm following what you're saying on this and I'm understanding the path forward that you're looking at. I just need a little bit more clarification too so that I can put my hands around what you are looking at.

Because I do think that you've got something here, I just need a little bit more to be able to understand fully, and like you said, you've got a couple of loose ends.

My suggestion would be to write it up and let NIOSH be able to respond to it.

CHAIR KOTELCHUCK: How does that sound, Grady?

MR. CALHOUN: Yes, that sounds better than what we got.

DR. MAURO: Okay, alright, you got it.

CHAIR KOTELCHUCK: Okay, then you'll write something up and we'll talk about this case next time. It's actually 433.3 and I believe 0.2

is similar, right?

MS. GOGLIOTTI: Correct.

CHAIR KOTELCHUCK: You already said 0.2?

MEMBER CLAWSON: John and Dave, if I could ask one thing, though. I want to fully understand better why this case is different than in the past.

That's kind of where I'm unclear on this. So when you write that up, could you spend a little bit more time for a simpleton as myself to help me understand why this is different?

DR. MAURO: I will, and the way it's going to come out is I'll give some examples of this kind of problem. And really, what it boils down to is something quite straightforward. Usually, we have lots of nice airborne sampling data right at the end of our AWE operations, and it represents the average distribution.

And we know that that was the circumstance people were working in. And under those circumstances, you take the average over

that last year and say, well, this is the stuff that was settling out.

But now we don't have that; we have something else. We're operating in a different domain now that I haven't come across before, where you have the --

CHAIR KOTELCHUCK: If I may suggest, John, you are repeating yourself.

DR. MAURO: Yes, I'm sorry.

CHAIR KOTELCHUCK: That's something you said before at least once or twice. But it's not clear and somebody has asked you to write it up.

DR. MAURO: I will write it up.

CHAIR KOTELCHUCK: So please write it up and then we'll go on.

MEMBER BEACH: This is Josie Beach. Can I ask something? John, can you please add the part about using TBD-6000 as well?

DR. MAURO: Yes, I think that's important because that was a new twist for me, that in this particular circumstance, TBD-6000,

though written for metal machining facilities could also be used for conversion facilities.

And that was a new twist, to tell you the truth, that I was not aware of, and I will do that also.

MEMBER BEACH: Thank you.

CHAIR KOTELCHUCK: Okay, good, all right, let's go on.

MS. GOGLIOTTI: There's only one more left in this, and actually, I believe that --

CHAIR KOTELCHUCK: It's the uranium mill?

MS. GOGLIOTTI: Sorry, my notes are all messed up here. The only remaining one is Finding 432.4.

CHAIR KOTELCHUCK: Yes, good.

MS. GOGLIOTTI: And that one, I believe TIB-11 is being revised that we're waiting on?

CHAIR KOTELCHUCK: That is right, we are waiting on TIB-11 which is due. They had indicated summer of '18 so we can't act on it

now?

MS. GOGLIOTTI: Correct.

CHAIR KOTELCHUCK: Okay, so we'll wait.

MS. GOGLIOTTI: That takes us into the 19th and 21st sets.

CHAIR KOTELCHUCK: Okay, aren't there some Sets 14 through 18 left, the INL and NTS?

MS. GOGLIOTTI: Those ones that we discussed, we're still waiting on different Subcommittee actions.

CHAIR KOTELCHUCK: Let's see, I'm just looking over my notes and you're correct. You are of course correct. I'm checking and I see that what you say is so, from my own notes.

Review Cases from Sets 19-21

CHAIR KOTELCHUCK: Okay, good, so we are ready to go to Sets 19 through 21. I agree with you. Sorry, I'm just catching up and trying to make sure.

Where would you like start with these?

MS. GOGLIOTTI: We can start on 482,

Observation 1.

CHAIR KOTELCHUCK: Which is in which file?

MS. GOGLIOTTI: This is in the SRS/Hanford.

CHAIR KOTELCHUCK: SRS/Hanford, yes. Okay, good, alright.

MS. GOGLIOTTI: Okay, and this one had to do with glove-box adjustment factor. We've been going back and forth on this one for a while.

We didn't understand where this number, 2.19, came from.

And after considerable back and forth, it turns out that number was actually in error. Through whatever process, the number 2.19 got carried through, rather than the number 2.0.

And I thought that maybe it was a typographical error. They agreed to clarify the guidance and I believe the template language has already been revised.

So if that's the case, then we recommend closure.

MR. CALHOUN: And that is correct, it has been updated.

CHAIR KOTELCHUCK: Very good. So, the problem has been identified, corrected and it is fundamentally a typographical issue.

Okay, so can we close the set?

MS. GOGLIOTTI: Can I just ask a quick question?

CHAIR KOTELCHUCK: Sure.

MS. GOGLIOTTI: Was the number 2.19 being used, or was the number 2.0 actually being used?

MR. CALHOUN: The 2.0 wasn't used. It was the difference of the 2.19 was actually the factor itself versus what's being compared. So we were using 2.0.

MS. GOGLIOTTI: So you were using the correct value, it was just incorrect in the documentation?

MR. CALHOUN: Right.

MS. GOGLIOTTI: Okay, great.

CHAIR KOTELCHUCK: Okay, good, so we

can close this, folks, unless I hear some concerns. Okay, good.

MS. GOGLIOTTI: Okay, the next one is the same case but finding number one. And here, the finding had to do with missed shallow dose being omitted, and I believe this is a workbook error.

The dose reconstructor selected something in the workbook that they shouldn't have selected. It's since been updated in the workbook and the DR Guidance.

If that's the case, we recommend closure.

CHAIR KOTELCHUCK: Right, and NIOSH agrees, does it?

MR. CALHOUN: Correct.

CHAIR KOTELCHUCK: Okay, fine, then that error has been corrected and the workbook has been updated so we'll close on that one unless I hear other concerns from other Subcommittee Members.

MEMBER CLAWSON: That's good, that's

fine.

CHAIR KOTELCHUCK: Okay, alright, let's go on.

MS. GOGLIOTTI: The only other one in this one [SRS/Hanford file] is 465.1, and that has to do with SRS coworker dose, which is still undergoing discussion in the Working Group.

And then there's 479.1 and .2, where NIOSH is currently awaiting response from the site. So we can move on to the next matrix if that's okay.

CHAIR KOTELCHUCK: Right, let me just, if I may, 479.1 through .3.

MS. GOGLIOTTI: Point two.

CHAIR KOTELCHUCK: Yes, .2, okay. So it awaits action. Yes, okay, Hanford. Alright, fine. Now where should we go?

MS. GOGLIOTTI: So we'll move on to the Oak Ridge [cases] and that should be from the 19th and 21st sets.

CHAIR KOTELCHUCK: Okay.

MS. GOGLIOTTI: And the first one is

458, Observation 2. This had to do with the column heading.

It was incorrect in the workbook. NIOSH agreed that it was incorrect, and they've agreed to correct the issue.

CHAIR KOTELCHUCK: Right, and that's in an observation. Fine, there should be no problem.

Again, I think that should be closed unless I hear otherwise? Alright, and was there one more [case] in that?

MS. GOGLIOTTI: Yes, this is Tab 500, Observation 1 and this is kind of a funky one. And this we made as an observation. We were just questioning why this case wasn't granted under the SEC.

The cancer appeared to be compensible and -- or both cancers appeared to be compensible -- and it appeared to meet all the SEC criteria.

So we were kind of curious as to why dose reconstruction was needed and we understand that DOL does make these decisions but it's not

NIOSH. But we did make an observation because it was so funky to us.

I mean, I saw this was a truck driver right? And I assume the issue was how much time did that person spend on site exposed?

But I don't see that we have any purview over that; that was a DOL decision.

MS. GOGLIOTTI: It's a DOL decision but we brought it up as an observation.

I'm not saying that NIOSH did anything wrong, I'm simply pointing out that this was funky and we did dig into it further, and that was the EE's occupation and so they were unable to determine how long the EE was on site.

CHAIR KOTELCHUCK: To me, that isn't even an observation.

I could easily understand that I or some of us, if we understood the DOL decision, might disagree.

Funky is not to my mind a category. In terms of the DOL they made a decision, we have to abide by it, like it or not.

MS. GOGLIOTTI: I think that it was more to point it out to the Board and I believe this was even highlighted in our one-on-one and we were asked to keep it as an observation.

CHAIR KOTELCHUCK: Okay, I don't understand why it's an observation even, honestly. There's nothing we can say about it.

So, I don't know, do others feel that way? Maybe an observation is a low enough category and just leave it in and be done with it.

MEMBER MUNN: I think it is indeed an observation because, as has already been pointed out, there's really nothing we can do about it anyway regardless of what the circumstances are.

But I personally would love to have us have a funky category. That would be the most fun category that we had in the entire process.

CHAIR KOTELCHUCK: Alright, others?

MEMBER CLAWSON: Are we discussing the funky category or the observation?

(Simultaneous speaking.)

CHAIR KOTELCHUCK: If it's funky, it's an observation.

MEMBER MUNN: By definition.

CHAIR KOTELCHUCK: Okay, right.

MEMBER CLAWSON: One thing that I do want to bring up, though, is that I do appreciate them bringing this up and just seeing it and bringing it to the Board's attention.

Granted we cannot do anything but it helps us understand what some of the problems and the process that both sites ended up going through.

So I just want to tell them I appreciate them showing us this, and what I'm trying to say is I don't want them to not do this.

I appreciate knowing that this is there and that it is funky or whatever, but it helps us understand the process.

CHAIR KOTELCHUCK: In my opinion, you make a good argument for keeping it as an observation, and also saying why it is of value that the Board's attention should be called to

it.

So I'm going to switch my vote to vote with you and Wanda, and we'll keep this as an observation and close it, right?

MEMBER MUNN: Sounds good.

CHAIR KOTELCHUCK: Alright. Very good, thank you. Let's go on now.

MS. GOGLIOTTI: Okay, I believe that wraps up this matrix.

CHAIR KOTELCHUCK: Right, it does.

MS. GOGLIOTTI: Okay, the next one, the DOE sites also from the 19th and 21st set.

CHAIR KOTELCHUCK: Yes.

MS. GOGLIOTTI: And this one is 453.6. It's an IOP, SNL, PPG and NTS case. And the finding had to be with an improper method used for calculating shallow dose to EE at PPG.

And what it came down to initially had to do with the guidance in TBD 8-6, which is the PPG external dose [in the] TBD.

NIOSH agreed to make some modifications but I'm kind of confused here

because in 2017, I believe, we said that you would make the modification --

MR. CALHOUN: I can go ahead -- that's why there seems to be an issue on dates.

CHAIR KOTELCHUCK: Please do.

MR. CALHOUN: I figured this was going to come up. What actually happened is our initial response was written prior to the PPG TBD being updated.

But it wasn't put into the BRS until after the PPG TBD was updated.

So our initial answer says we're going to deal with it in the next version of the PPG, which was true when we first wrote it, however, the PPG TBD came out before we actually put this into the BRS.

MS. GOGLIOTTI: That's why I was so confused.

MR. CALHOUN: So what it comes down to is our initial response, and it's actually my fault.

If I had gone back and re-reviewed it

before I put it in the BRS, I would have notably made the change in the TBD and that would have been the initial answer as well.

So I apologize for that.

MS. GOGLIOTTI: Not a problem, I just wanted to make sure I understood what was happening.

CHAIR KOTELCHUCK: Okay, well, so we can close then.

MS. GOGLIOTTI: Okay, and the next one and last one in this matrix is 462.2. This is a Pantex [case], and this has to do with the NP ratio for unmonitored worker dose for neutron dose.

And there's been a little bit of back and forth on this one. We thought a value of 1.7 should have been used.

I believe NIOSH used a value of 0.8 and NIOSH came back and said that the 1.7 was the 95th percentile, which is correct.

And there's been substantial Pantex TBD modification since this happened. But I

believe the TBD was kind of -- I don't think the guidance was very clear.

It was somewhat conflicting, there's at least 8 points in the old TBD that said use 1.7, and one place it says use .8 and I think that was where the confusion lay.

It definitely got corrected in the new revision; actually, I think there's been two or three revisions since we reviewed this case. So based on that we recommend closure.

CHAIR KOTELCHUCK: Okay.

MR. KATZ: So is this is an observation? I'm unclear.

MS. GOGLIOTTI: It can be reduced to an observation. It's correct now with the current TBD guidance.

CHAIR KOTELCHUCK: Okay, so 462 will become an observation. Okay, and we will close that. Let's see now, we have --

MS. GOGLIOTTI: That closes out this one. Actually, I can move to my other notes here.

CHAIR KOTELCHUCK: Okay. Oh, the AWE

cases?

MS. GOGLIOTTI: Yes.

CHAIR KOTELCHUCK: Oh, yes, it's fine, yes, many open cases there.

MS. GOGLIOTTI: We have not actually looked at this matrix yet, so everything is fresh. So we're actually going to start with the Type 1 findings.

CHAIR KOTELCHUCK: Okay, good, well, Type 1 is good. These are always nice to be able to start with.

MS. GOGLIOTTI: These are the ones we can easily close out first.

CHAIR KOTELCHUCK: Yes.

MS. GOGLIOTTI: Okay, the first one is 471, Observation 1, and this had to do with electron dose being cited in the Appendix and IREP entries, but it was completely ignored in the text of the DR Report.

There's no mention of it. NIOSH does agree that it was left out of the DR Report inadvertently. There's no mention of electron

dose anywhere in there.

It was defined correctly; it simply wasn't mentioned in the report. So, based on that, we recommend closure, obviously, because the electron dose should be mentioned in the report.

CHAIR KOTELCHUCK: Alright, as we'll do these Category 1, we'll just move straight ahead and then please, Subcommittee Members or others, if there are problems, please say so. Otherwise I'll assume if I hear nothing that we're fine, we agree. Okay?

MS. GOGLIOTTI: Alright, the next one is 477.1, which is a Bethlehem Steel case. This one was unusual, it was a CLL case.

We said that the finding has to do with the inconsistent selection of solubility type for this type of cancer.

NIOSH assigned it as Type S, which is not an option at Bethlehem Steel, and use full years rather than prorated years for dose.

This is our finding. NIOSH came back

and said, essentially, they were doing an overestimate. While we understood that they were doing an overestimate, we didn't realize how overestimated they were going, I guess.

For CLL cancers, claims are ridiculously complicated, far more so than any of the other cancers.

So I completely understand why they took this approach but it wasn't entirely clear to us if they were doing an overestimating approach or if they had selected something that was incorrect.

So there's nothing wrong with it and we can recommend closure, and actually the same thing applies to the next one as well.

CHAIR KOTELCHUCK: Okay.

MR. KATZ: That would be an observation?

MS. GOGLIOTTI: They could be reduced to observations, yes.

CHAIR KOTELCHUCK: Okay.

MS. GOGLIOTTI: And that's for both

477.1 and .2.

CHAIR KOTELCHUCK: Okay, good.

MS. GOGLIOTTI: Okay, the next one BWXT, 443.1. And here the workbook lists an incorrect value for the year 1970 for X-ray doses. NIOSH applied the 1971 to the 1970. It results in a slightly underestimated dose.

NIOSH agreed that actually since the time we reviewed this TIB-79 has been revised.

And actually, no X-ray dose is to be assigned at BWXT since that revision so the finding essentially becomes a moot point.

So there was an error but it's no longer applicable.

CHAIR KOTELCHUCK: Okay, so we'll say it was resolved. Okay, 443.1 but it still is a finding.

MS. GOGLIOTTI: And a similar logic applies to the next one, it's the same case, Finding 2.

NIOSH assigned a three-year scan without justification. I believe there were not

records but the EE recalled being monitored every year but NIOSH assigned a three-year scan.

Again, BWXT X-rays are no longer applied so it's kind of irrelevant at this point.

CHAIR KOTELCHUCK: Right, okay.

MS. GOGLIOTTI: Okay, another BWXT case, Tab 444.1, and actually, the next three all are related. NIOSH applied a clothing attenuation factor without justification for electron dose.

The location of this particular cancer could be covered by clothing, but it's not really reasonable to assume that it's always covered by clothing.

So it was kind of a little gray area here and NIOSH agrees that they should not have been assigned. They redid the PoC calculation without the use of attenuations. It didn't change the PoC.

And we actually went a step further and investigated cases that have previously been done by the CR Reviewer on the 25 cases just to

make sure that the same error hadn't occurred again, and it wasn't an issue.

CHAIR KOTELCHUCK: Okay.

MS. GOGLIOTTI: Based on that, we recommend closing all three of these issues because they're the same issue just recorded as residual electron dose.

CHAIR KOTELCHUCK: That's 444, 1 through 3?

MS. GOGLIOTTI: Correct.

CHAIR KOTELCHUCK: Okay.

MS. GOGLIOTTI: And the next one 444.4 is actually a similar issue to 443.1, which had to do with the X-ray value for the year.

In 1970, they were actually applying the '71 values, which resulted in a slight underestimate in dose. And then the next one is actually the same as in the previous case also.

They assume the three-year scan as justification but in both cases, OTIB-79's most current revision for June 2017, makes it no longer relevant because you're no longer

assigning every dose.

So we recommend closing both of those issues.

CHAIR KOTELCHUCK: Okay.

MS. GOGLIOTTI: Okay, the Carborundum case, we said that we --

CHAIR KOTELCHUCK: We're going to hold that until next time.

MS. GOGLIOTTI: And so the next one here is 473, Observation 1. 473, Observation 1.

CHAIR KOTELCHUCK: Okay.

MS. GOGLIOTTI: This is the GE Vallecitos case and the finding said that indium exposure was assigned to this worker during non-AWE time periods, presumably based on the premise that ambient exposures were due to the residual radioactivity associated with AWE activities.

And NIOSH responded that though the hot cells are typically heavily shielded, the support structures and systems share resources from other hot cells.

Under these circumstances, it's not

possible for them to distinguish dose from one hot cell versus another hot cell during operational and residual periods.

Thus in the circumstance, the site ambient dose is determined to be the most appropriate. Based on that, we accept their response and recommend closure.

CHAIR KOTELCHUCK: Okay.

MS. GOGLIOTTI: The next one is Observation 2 from the same case. This is actually an unusual observation as well. This came out of the one-on-one discussion.

This was the only case of GE Vallecitos that was ever reviewed, and there were a limited number of exposure pathways assigned in this case.

Since there's no Site Profile and the only DR methodology guidance for this case is contained in the DR Report in the form of a template, the two Board Members that took part in our one-on-one recommended that SC&A be tasked with the complete audit of the remaining exposure

pathways that were not considered in this DR.

That would be an observation. Essentially, there's no response needed by NIOSH.

CHAIR KOTELCHUCK: SC&A is tasked with this?

MS. GOGLIOTTI: We have not been tasked with this.

This is the recommendation of the one-on-one conference call that we had and it did carry through, but obviously we haven't discussed it yet.

MEMBER MUNN: What other pathways are envisioned?

MS. GOGLIOTTI: I'd have to look into the exact case details here but I believe only certain pathways were available or applicable to this particular dose reconstruction.

So the other pathways were not looked at and likely won't be looked at again because we're not tasked with an additional review.

CHAIR KOTELCHUCK: I'm a bit puzzled.

MEMBER MUNN: I am too. I don't really

understand what we're concerned with in terms of other pathways.

MS. GOGLIOTTI: Well, I think it's not towards this particular case but other GE Vallecitos cases, there was the concern that --

MR. SIEBERT: This is Scott.

One thing that might help is this claim was over 50 percent, so not everything in the methodology, was applied because it was not needed to be applied.

And I think that's where this is coming from.

MS. GOGLIOTTI: Thank you.

MEMBER MUNN: But then why would we want them to go look at others if it's not applicable to --

MR. KATZ: So I think what's being said here is that, why don't we look at another case where Edison is involved so that we get a more complete evaluation of the methods.

MS. GOGLIOTTI: That or simply do a mini-TBD review of just what document --

CHAIR KOTELCHUCK: So essentially, this would have merit in a future case?

MS. GOGLIOTTI: Correct. It wouldn't affect this particular case but it --

MR. KATZ: It's not a finding, it's just the fact of the matter that SC&A didn't have an opportunity in this case [to go] into the other pathways because they weren't necessary here.

CHAIR KOTELCHUCK: Okay, I'm satisfied with that. Wanda, and I hope I didn't cut you off; I fear I did.

MEMBER MUNN: No, and it wouldn't be the first time anybody had ever done that anyhow, Dave.

CHAIR KOTELCHUCK: Well, there are even gender-related issues, so please [go on].

MEMBER MUNN: Well, no, I'm just wondering how many pathways is it possible for us to be concurrent with?

Is it an appropriate use of time and effort to try to identify every pathway, like who else might have walked in with a suitcase full of

plutonium?

CHAIR KOTELCHUCK: And I guess it will come up again if there's another case.

MEMBER MUNN: Yes, I would think so. I don't see the immediate relevance, I guess, that's what I'm saying.

CHAIR KOTELCHUCK: Right, well, we'll leave it as an observation, again, because it's not relevant here but it may be elsewhere.

MEMBER MUNN: At which time, we can address it specifically.

CHAIR KOTELCHUCK: Yes, we'll have to.

MEMBER CLAWSON: What this came up from was in this case, like Scott said, they had several. They had just these pathways, and when it went over 50 percent, they stopped.

And the thing was, okay, if it wouldn't have hit 50 percent, is the process set up to be able to look at the other pathways? Which Scott has told us, yes, it was, but we wanted to make sure that they were being looked at.

This is what the relevance [of this]

coming up is. It kind of bothers me in the process that all of a sudden we hit 50 percent and we just stop; there's no use going on any further.

Well, I want to make sure that when we don't hit 50 percent, the process continues going on.

So, this is one of the ones, if you remember, we got back into this question, and I was one of them that raised this.

And our thing was I was just want to make sure that the template and everything else are set up.

Because if the templates are set up to be able to stop after 50 percent, what's to say that -- what's triggering that?

And this is what it comes down to and we just what make sure that we're looking at what the tools are doing, what the process is supposed to be.

CHAIR KOTELCHUCK: On the other hand, Brad, we are looking at a one percent sample of cases.

MEMBER CLAWSON: Correct.

CHAIR KOTELCHUCK: And Grady and others are working through the other 99 percent. And so I don't disagree with them stopping when they hit 50 percent.

MEMBER CLAWSON: And I understand that, but couldn't you also tell me that maybe we're not stopping too early on another one.

So the thing was to check that this process is working the way that it's supposed to.

MR. KATZ: But Brad --

MEMBER CLAWSON: But we've seen problems with tools before.

MR. KATZ: But Brad, if they don't hit 50 percent, they can't stop, they have to do a complete dose reconstruction. Otherwise they do an efficiency method, an efficiency case.

MEMBER CLAWSON: Okay.

MR. KATZ: But it's not the template. If they hit 50 percent, there's no reason to do any more work is the issue with the case.

MEMBER CLAWSON: Right, but if you

remember right when we got into this whole side conversation of this was to make sure the process was working as it was supposed to.

And Dave, you summed it all up, we're checking one percent of this and we want to make sure that the other 99 percent are being done right.

CHAIR KOTELCHUCK: That's true.

MEMBER CLAWSON: This is what we've been tasked to do. That's what it came down to. It's not that it was wrong in any aspect, the question was are the tools working the way that they're supposed to.

Because this was the other question too, and Wanda summed it up, what other pathways do we have and so forth?

So that's what it came down to. I guess you can't guess without someone having a problem with it.

(Laughter.)

MR. KATZ: And you can, in a future set of dose reconstruction cases, you can just

ask for us to say include a case from this guide which didn't make it above 50 percent.

And include that in the set and then you get to review the full methodologies.

MEMBER CLAWSON: That's true, that's true.

CHAIR KOTELCHUCK: Good suggestion. Are we at the end of this? M&C, Metals and Controls, we would like to do next time.

MS. GOGLIOTTI: Yes, so we just have a single one here of Texas City Chemicals.

MR. SIEBERT: I'm sorry, this is Scott. Does that mean we actually closed that observation?

MR. KATZ: Yes.

MR. SIEBERT: Okay, I just wanted to verify that, thank you.

CHAIR KOTELCHUCK: Absolutely. Okay, we have Texas City?

MS. GOGLIOTTI: Okay, Texas City Chemicals, 442 Observation 1.

Here, it was not apparent to us why a

distinction was made between external exposures associated with phosphate plant operations from March 31, 1955 through April 1, 1955 and then again through September of 1955.

There were different doses prepared and it wasn't clear to us why. NIOSH responded saying that that distinction was actually an error. The table didn't mean to have that and it's since been corrected.

And DCAS has actually created a TBD for Texas City Chemicals that corrects this issue, and that was just issued and we're actually in the process of reviewing that document now.

And I did confirm that change was made.

CHAIR KOTELCHUCK: Very good, so we close it.

MS. GOGLIOTTI: I'm not sure if that would be a finding now if they were assigning the incorrect dose based on something that was correct in the template.

MR. KATZ: Yes, if it's incorrect dose, it's a finding.

CHAIR KOTELCHUCK: The table is in error?

MR. KATZ: It's an error. If it's being used by NIOSH, then it's an error in the dose reconstruction.

CHAIR KOTELCHUCK: Right, not the labeling of the table, the table itself, the numbers in the table?

MR. KATZ: Yes.

CHAIR KOTELCHUCK: I think that's true so that we'll have to close it. But 442 or whatever, there may be others.

I don't know if there are other items in 442. And where are we at this point?

MS. GOGLIOTTI: We would get to the Type 2 findings but there's only a few.

CHAIR KOTELCHUCK: Okay, are we prepared for them?

MS. GOGLIOTTI: We could certainly go over them. I'll have to switch to the matrix

again.

CHAIR KOTELCHUCK: Okay, well, if we're moving to the last few, unless --

MS. GOGLIOTTI: We can certainly hold them off until the next meeting.

CHAIR KOTELCHUCK: Right, it's nearly break time. Maybe we can just go through these, not take a break but go through these and finish up?

MS. GOGLIOTTI: Okay.

CHAIR KOTELCHUCK: Okay, folks?

MEMBER CLAWSON: Yes, fine, let's do it.

CHAIR KOTELCHUCK: Okay, let's go.

MS. GOGLIOTTI: Let me just get this pulled up here.

CHAIR KOTELCHUCK: Type 2. It seems like you've gone through this process before.

MS. GOGLIOTTI: Just give me one second here, I've got to get my notes pulled up on my other screen.

CHAIR KOTELCHUCK: Okay.

MS. GOGLIOTTI: Yes, if you can't tell, I spend a lot of time in the BRS. Alright, and the next one here is an ALCOA case and this is 471, Observation 2.

Okay, and this one has to do with the text in the DR Report in the case of the pre-employment X-ray.

And I believe an annual X-ray was assigned in each year, but there was no corresponding annual dose.

And here the disagreement comes down to the interpretation of TIB-6. NIOSH interpreted it to mean that only a pre-employment X-ray should be assigned to the first year of employment.

But SC&A interpreted it to mean that a pre-employment and an annual X-ray should be assigned in the first year of employment.

It would not have a significant impact on the case obviously, but we should get it established: which is the appropriate interpretation?

MR. SIEBERT: This is Scott, I can tell you we're right at the moment in the midst of updating Procedure 6 and we have added verbiage to clarify that specific assignment.

So that's coming.

MS. GOGLIOTTI: Okay, great.

CHAIR KOTELCHUCK: Okay, good, and what is the clarification?

MS. GOGLIOTTI: If a pre-employment examination should be included with an annual scan during the first year of employment or if just a pre-employment scan is sufficient without any additional annual scan during that year.

CHAIR KOTELCHUCK: Aha, that is to say whether it is actually, the question is, is there a first-year X-ray?

MS. GOGLIOTTI: Yes.

CHAIR KOTELCHUCK: Right, okay.

MS. GOGLIOTTI: So based on that, do you want to keep it open so we can verify this, which would be putting it in abeyance until OTIB-6 is issued? Or do you want to close it based on

that?

CHAIR KOTELCHUCK: OTIB-6 is not completed yet, although it's in process, right?

MS. GOGLIOTTI: It's undergoing revision according to them.

CHAIR KOTELCHUCK: I would say let's keep it open until it's done and confirmed. I'm sure it will be done.

MS. GOGLIOTTI: So in abeyance then?

CHAIR KOTELCHUCK: What do you think?

MR. KATZ: Well, we don't normally do that with observations, particularly if the answers -- I didn't hear from Scott, well, what is the correct answer to this?

What is the procedure? Is it to assign both or just to assign the pre-employment?

MR. SIEBERT: Sorry, I'm digging through the update. I reviewed it like a month ago and so it's not off the top of my head.

MR. KATZ: Okay.

MR. SIEBERT: So I'm looking through

the one we're working on right now.

MEMBER MUNN: I had forgotten there was an observation.

MR. SIEBERT: I did too.

MR. KATZ: If you don't have the answer ready, there's no problem with keeping this over. But I thought this was already understood.

(Simultaneous speaking.)

MEMBER MUNN: -- No problem in closing it either.

MR. SIEBERT: Okay, I found it, the update is it's assumed that the annual is taken a year after the pre-employment. So the first year of employment would only have one.

CHAIR KOTELCHUCK: Okay, good, that makes sense.

MR. KATZ: I think we can close this.

CHAIR KOTELCHUCK: I think we can close it.

MEMBER BEACH: Yes, we closed all the other ones.

CHAIR KOTELCHUCK: Yes. Okay, good.

MS. GOGLIOTTI: The next one goes back to the GE Vallecitos case, 473.2.

And here, the finding had to do with we questioned the on-site ambient dose, whether or not it was calculated appropriately.

John Mauro, are you still on the line? We might have lost him.

When we did this calculation, or when we did this review, John went through and calculated background exposures and ambient dose.

And I compared them and it appeared that background exposure was being included in that.

NIOSH took a quote from my report and interpreted it one way. John disagreed with the way they interpreted it and so we performed our own analysis that is documented in our report. And we request NIOSH to provide additional documentation to support their position.

CHAIR KOTELCHUCK: Okay.

MR. SIEBERT: This is Scott. We're

working on that right now.

I just want one piece of clarification, so what SC&A is saying in their finding is the fact that background was included but should not have been.

Am I interpreting that correctly?

MS. GOGLIOTTI: I believe so but can I get back to you with John's input on this?

MR. SIEBERT: Yes, it would really be helpful to us if John wrote an additional response clarifying exactly what he's asking so we could respond to it in a timely manner. That would be helpful.

MS. GOGLIOTTI: He can absolutely do that.

MR. SIEBERT: Awesome, thank you.

CHAIR KOTELCHUCK: Okay, very good, so that's in progress.

MS. GOGLIOTTI: Okay, and just one more, it's not the Metals and Controls or Carborundum, it's actually the Texas City Chemicals, which is Tab 442 and it's Observation

2.

And this states that SC&A is requesting that NIOSH explain how their inhalation rate for uranium-238 was derived and why it differs from SC&A's value.

We got 46 and they got 39 or vice versa. And NIOSH came back and said that our dose had been calculated per work day and theirs was calculated per calendar day, which got us to talking internally because I focus mainly on dose reconstruction and I don't really see all the procedures-review aspects. And we're concerned that perhaps [that] might dilute dose.

So if you were assuming for a day and you're dividing by 7 instead of by 5 in that week, then we would ask some additional questions about that.

According to NIOSH, IMBA and the CADW -- this is all new as of last week so that's why I'm a little off my guard here -- require chronic intake to be specified on a cumulative basis rather than an annual basis, rather than the

calendar-day basis.

So, they don't think inhalation dose is necessary. I think we'd like a little bit more time to look into this one now.

CHAIR KOTELCHUCK: Okay, let's just keep this in progress.

MS. GOGLIOTTI: Okay, and that wraps up everything.

CHAIR KOTELCHUCK: Very good.

MS. GOGLIOTTI: So, for the next meeting we'll have just a handful and then the Carborundum and the Metals and Controls.

CHAIR KOTELCHUCK: Very good. So now I think we just think about time for the next meeting.

We missed a meeting. On the other hand, we're pretty well up to date. You folks at SC&A have Set 25, right, that you're working on now?

MS. GOGLIOTTI: Yes, we're in the process of doing that. We have about three and a half, four months to deliver that and we're on

track.

CHAIR KOTELCHUCK: That's good. So our next Board Meeting is in April and we need at least two months, right, to get notice in the Federal Register?

So, if this is March, we're going to have to go through into May, late May or early June. Right, Ted?

MR. KATZ: Yes, that's correct.

I think the question is when Rose and Grady think they'll be ready to button up the issues on the table, which is the lines in these other cases?

Not counting the ones, of course, that have been parceled out.

MR. CALHOUN: This is Grady and I'm not terribly worried about us getting done but my schedule is a bear beginning May 19.

So I'm off the whole week from the 19th to the 26th, then it's Memorial Day weekend on the 28th. I'm back for three days, 29th, 30th, 31st, then I'm gone again from the 1st to the

9th.

Then I'm back for four days, and then tentatively, we have a workshop in Albuquerque the 18th, 19th, 20th and 21st of June.

So, the 11th, 12th, 13th, 14th of June works for me, the 14th, 15th, 16th of May works for me, 29th and 30th of May works.

And, yes, we're going to have to get later into May just to get the notification out, right?

(Simultaneous speaking.)

Twenty-ninth, 30th of May, the week of the 11th of June, and then after the 25th of June.

CHAIR KOTELCHUCK: How about that week in June, the week of June 11th?

MR. CALHOUN: That one works for me.

CHAIR KOTELCHUCK: I mean something like the 12th, 13th, 14th? Ted, how does that sound?

MR. KATZ: Let me just check my calendar quickly, but otherwise, while I'm doing that, why don't Board Members also check that?

CHAIR KOTELCHUCK: Okay, so we'll do it that time of year on Tuesday or Wednesday or Thursday.

MR. KATZ: So, Wanda, Josie, and Brad, the 11th to the 14th, that week, how does that look?

MEMBER BEACH: That's not great for me.

CHAIR KOTELCHUCK: That is not?

MEMBER BEACH: No.

MEMBER MUNN: We're getting around graduation time.

CHAIR KOTELCHUCK: Okay, sure.

(Simultaneous speaking.)

MEMBER BEACH: What's that?

MR. KATZ: Okay, so the 11th to 14th of June does not work I just heard?

CHAIR KOTELCHUCK: Right.

MR. KATZ: Okay so when in July?

MR. CALHOUN: I was going to say the 25th of June, that week works.

MR. KATZ: Okay, how about that?

MEMBER BEACH: That works for me.

MR. KATZ: That would be on the 26th, we have a telecon for the Board, but otherwise, the rest of that week is wide open.

MEMBER MUNN: Which week are we talking about?

CHAIR KOTELCHUCK: The week of the 25th of June.

MEMBER MUNN: The 25th of June I expect to be in Scotland for a medical school graduation.

CHAIR KOTELCHUCK: And you'll be away that week?

MEMBER MUNN: I expect to be away for two weeks. I'm not going to Scotland and coming back in less than a week.

CHAIR KOTELCHUCK: Okay, we're into July.

MEMBER MUNN: The May dates are okay for me.

MR. KATZ: The May dates don't work so we're into July.

MR. CALHOUN: July, luckily, is the health physics meeting, but that first week is July 4th so I don't think we're going to do anything there.

I'm only going to take off a day.

MR. KATZ: That probably won't work.

CHAIR KOTELCHUCK: And next week, 9th, 10th, 11th and 12th, but a lot of us leave for the health physics meeting on the 13th.

How about early in the week, like Tuesday the 10th or Wednesday the 11th?

MEMBER MUNN: That's okay for me.

MR. KATZ: Tuesday is July 10th?

CHAIR KOTELCHUCK: Right.

MR. KATZ: Is that good for everyone on the call?

MR. KATZ: Can I ask, I am leaving town on July 11th.

MR. KATZ: But this is July 10th we're talking about.

CHAIR KOTELCHUCK: Oh, I thought you were leaving on July 13th.

MR. CALHOUN: I apologize, I had both dates, I just wanted to point that out.

MR. KATZ: July 10th, is that a problem?

MR. CALHOUN: That's workable for me, sure.

MR. KATZ: Okay, and do I have Brad and Josie there?

MEMBER BEACH: Sure, I can change some things around for that one.

MEMBER CLAWSON: I've just got some stuff I've got to change around but I'll work on that.

MR. KATZ: How about David Richardson, are you on the line? Okay, well, I'll check with David.

CHAIR KOTELCHUCK: What about John Poston?

MR. KATZ: Yes, and if not July 10th, what about the following week? The week of July 16th is health physics, you're saying?

CHAIR KOTELCHUCK: Yes.

MR. KATZ: Okay.

(Simultaneous speaking.)

MR. CALHOUN: We have to go to the 23rd.

MR. KATZ: So, not the 10th but it's the 23rd, the week of the 23rd?

CHAIR KOTELCHUCK: Well, let me ask you, what about July 9th, Monday July 9th?

MR. KATZ: Oh, yes.

CHAIR KOTELCHUCK: We don't normally meet Monday or Friday in the summer, but if we need a fallback.

MR. CALHOUN: That's okay with me.

MEMBER CLAWSON: July 9th won't work for me.

CHAIR KOTELCHUCK: Alright, then we'll go to the 23rd, week of the 23rd?

MR. KATZ: Well, how about the 24th, how's that?

MEMBER BEACH: That's good.

CHAIR KOTELCHUCK: Again a Tuesday. Okay, 7/10 with 7/24 backup.

MR. KATZ: Yes, the 10th or the 24th.

CHAIR KOTELCHUCK: Sounds good.

MR. KATZ: And then the other thing, Dave, to keep in mind as we get down towards the end of these sets or case reviews, and at the next meeting we'll have put to bed the blinds as well.

We should probably start thinking, and you might want to talk to the Board about this, about another report to HHS.

CHAIR KOTELCHUCK: Another HHS report?

MR. KATZ: Yes.

(Simultaneous speaking.)

CHAIR KOTELCHUCK: What do you think the occasion is?

MR. KATZ: Well, the occasion is that you've done a whole bunch of sets since then, and if you recall the discussion at the Board Meeting, led particularly by Paul Ziemer, is that really we could do reporting a little more frequently than we are, which is once every ten years or something like that, or eight years, or

whatever it is.

CHAIR KOTELCHUCK: Okay, how about once every four years?

(Laughter.)

CHAIR KOTELCHUCK: It's a lot of work, plus, right now, as far as I know, we don't have a chair.

MR. KATZ: Well, I know, but that'll get resolved. But anyway --

CHAIR KOTELCHUCK: I was just saying Jim was very helpful. I couldn't have done the report without referring back to Jim a whole lot.

MR. KATZ: Right, but we also learned a lot from doing that report.

CHAIR KOTELCHUCK: We did.

MR. KATZ: And it'll make the next one, I think, easier because it's a pretty good template we have now.

So I don't think the next report will be nearly as difficult as the last report was to do. But anyway, that's up to you, I'm just raising the issue.

CHAIR KOTELCHUCK: Okay, and let's see, now we actually turned in the report last year, that is to say we turned in our last report on January 16th, did we not?

MR. KATZ: That sounds right. I don't know what the date was but that sounds right.

CHAIR KOTELCHUCK: Okay, we will raise this, I mean this is a Board issue and we'll talk about it.

MR. KATZ: I just don't recall if Paul said -- Paul was thinking more like a yearly report would be good.

I don't think it really needs to be yearly per se when we've done significantly more work and you guys have done that.

CHAIR KOTELCHUCK: Right. Okay, well, we will do that. You will help remind us as we develop agendas. If we could do it during the conference call?

MR. KATZ: In the face to face meeting we can talk about this.

CHAIR KOTELCHUCK: Okay. Alright,

ladies and gentlemen, it's a quarter after three. We have our dates set and we have good things ahead. We will all see each other in April.

MEMBER MUNN: We will.

MEMBER BEACH: Sounds good.

Adjourn

CHAIR KOTELCHUCK: Very good. Thank you all for today's good work. Bye-bye, everybody.

(Whereupon, the above-entitled matter went off the record at 3:14 p.m.)