U.S. DEPARTMENT OF HEALTH AND HUMAN SERVICES
CENTERS FOR DISEASE CONTROL
NATIONAL INSTITUTE FOR OCCUPATIONAL
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ADVISORY BOARD ON RADIATION AND
WORKER HEALTH

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WORK GROUP ON MOUND

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TUESDAY
JULY 27, 2010

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The Work Group convened in the Frankfurt Room of the Cincinnati Airport Marriott, 2395 Progress Drive, Hebron, Kentucky, at 9:30 a.m., Josie Beach, Chair, presiding.

PRESENT:

JOSIE BEACH, Chair
BRADLEY P. CLAWSON, Member
ROBERT W. PRESLEY, Member
PHILLIP SCHOFIELD, Member
PAUL L. ZIEMER, Member

ALSO PRESENT:

TED KATZ, Designated Federal Official
NANCY ADAMS, NIOSH Contractor*
ISAF AL-NABULSI, DOE*
ROBERT ANIGSTEIN, SC&A*
ROBERT BISTLINE, SC&A*
RON BUCHANAN, SC&A
MEL CHEW, ORAU Team*
JOE FITZGERALD, SC&A
STU HINNEFELD, DCAS
EMILY HOWELL, HHS
KARIN JESSEN, ORAU Team*
JEFFREY KOTSCH, DOL*
JENNY LIN, HHS
ARJUN MAKHIJANI, SC&A
JOHN MAURO, SC&A
ROBERT MORRIS*
JAMES NETON, DCAS
BRANT ULSH, DCAS

*Participating via telephone

C-O-N-T-E-N-T-S

P-R-O-C-E-E-D-I-N-G-S

(9:30 a.m.)

MR. KATZ: Good morning, everyone in the room and on the line. This is the Advisory Board on Radiation and Worker Health.

This is the Mound Working Group, and we're just getting started with roll call. I'm Ted Katz. I'm the Designated Federal Official of the Advisory Board, and we'll begin with Board members in the room.

Chair.

CHAIR BEACH: Josie Beach. No conflicts with Mound.

MR. KATZ: Yes, thank you. Everyone address whether you have a conflict situation.

MEMBER PRESLEY: Robert Presley, no conflict with Mound.

MEMBER CLAWSON: Brad Clawson, Work Group Member, no conflict with Mound.

MEMBER SCHOFIELD: Phillip Schofield, Work Group Member, no conflict with Mound.

MEMBER ZIEMER: Paul Ziemer, Work Group Member, no conflict with Mound.

MR. KATZ: And do we have any Board members on the line?

(No response.)

Okay. Then, NIOSH ORAU Team in the room.

MR. HINNEFELD: Stu Hinnefeld, Interim Director, no conflict with Mound.

DR. ULSH: Brant Ulsh, no conflict with Mound.

DR. NETON: Jim Neton. I have no conflict with Mound.

MR. KATZ: NIOSH ORAU Team on the line?

MS. JESSEN: Karin Jessen, ORAU Team, no conflict with Mound.

MR. KATZ: I'm sorry. Who is that again?

MS. JESSEN: Karin Jessen.

MR. KATZ: Thank you.

MS. JESSEN: You're welcome.

DR. CHEW: Hi, I'm Mel Chew, no conflicts with Mound, ORAU Team.

CHAIR BEACH: Hi, Mel.

DR. CHEW: Good morning.

MR. KATZ: Okay. Welcome, all of you. And SC&A in the room.

DR. MAURO: SC&A, John Mauro, no conflict.

MR. FITZGERALD: Joe Fitzgerald, no conflict with Mound.

DR. BUCHANAN: Ron Buchanan, SC&A, no conflict with Mound.

MR. KATZ: And SC&A on the line.

DR. BISTLINE: Bob Bistline. SC&A. No conflict.

MR. KATZ: Very good. Federal officials or contractors to the feds, HHS, DOL, DOE in the room: right now we do not have attendance yet.

On the line?

MR. KOTSCH: Jeff Kotsch, Department of Labor.

MS. ADAMS: Nancy Adams, NIOSH contractor.

MR. KATZ: Okay. That was Jeff Kotsch and Nancy --

MR. KOTSCH: I'm sorry, yes. Jeff Kotsch, Department of Labor.

MR. KATZ: And Nancy Adams that's a contractor to NIOSH.

Others?

DR. AL-NABULSI: Isaf Al-Nabulsi, DOE.

MR. KATZ: Welcome.

DR. AL-NABULSI: Thanks.

MR. KATZ: Very good. And now any members of the public on the line. There are none in the room.

Great. Okay. We'll acknowledge others as they join us because I'm sure OGC, at least, will join us.

So do you want to get things rolling on the agenda?

CHAIR BEACH: Yes. The agenda is

posted online for anyone that doesn't have it in front of them. We are going to start this morning with neutron dose reconstructions. I did not put times down purposefully because I do not know how long the discussions will take. And the end time today is, I'm assuming, 4:00 to 4:30.

We're going to then go into stable tritium compounds, discuss radon, adequacy/completeness of internal dose, the high-fired Pu-238. We're going to talk about the roadmap and D&D issues.

At the end of this, we will hopefully make recommendations amongst the Work Group to take to the Board for our next meeting in Idaho in August.

Ted, I'll turn it back over to you.

MR. KATZ: Sure, and Emily Howell has joined us in the room for OGC HHS.

So I just want to make a disclosure at the head of this meeting. SC&A is rolling out but doesn't -- hasn't had any place in the past, an attribution policy for its documents such that all authors involved in any given document are identified in its document, as well as the review chain for clearing the document are identified.

So that's coming, but it doesn't exist in a consistent way currently or it hasn't in the past. So we have two documents that I think -- I believe just two documents that we're dealing with today.

MR. FITZGERALD: Three. Two on neutrons and one on completeness and adequacy of internal --

MR. KATZ: Well, let me finish and then you can correct me if I'm wrong.

MR. FITZGERALD: All right.

MR. KATZ: I think there are two documents that are being discussed today where we have -- I should make a disclosure because we have a person who is a primary or a leading author for it who has a conflict. And that is the adequacy/completeness of internal dose. I believe that document or however it's titled, Adequacy of Data. And a very brief piece on tritium, stable tritium. Joe authored that, but that was investigated by Kathy and Joe substantially, too.

MR. FITZGERALD: Right.

MR. KATZ: So, Kathy Roberston-DeMers, just again for disclosure, she worked at Mound and she, thus, is a potential claimant down the road or a potential beneficiary if there's an SEC Class to be added to Mound down the road.

And so going forward, people, since February we've had a policy. We've sort of -- well, continuing this program. This program has worked with tightening its policies for conflict of interest and appearance of bias.

And we've been doing a lot of work over the past year and we rolled out a policy in February that sort of canvasses and sort of equalizes things across the landscape here with Board members and contractors and NIOSH employees to the extent that can be equalized in terms of how conflicts of interest and appearances of biases are addressed.

And SC&A is busily implementing -- developing and implementing a new conflict of interest plan which will end up on the web when it's completed. Steve Ostrow is leading that effort and getting its ducks in a row to implement it at the same time as they're developing the plan that will be published. And there will be new disclosure statements and so on. That will all appear on the web.

But so, I just wanted to say at the outset of this, since we're discussing two documents for which, under the new policy Kathy DeMers would be found to have an appearance of bias issue, that she was the author of those.

And I don't know any of these other documents -- is she a primary on any of these other documents that are being discussed today?

MR. FITZGERALD: No, the only correction I would make is I'm the primary on the tritides piece.

MR. KATZ: Right.

MR. FITZGERALD: Right.

MR. KATZ: Right. Okay. And that's it. Thank you.

MR. FITZGERALD: Okay.

CHAIR BEACH: Okay. So, Joe, if you would like to get us started on neutrons?

MR. FITZGERALD: Okay. This is Joe Fitzgerald, SC&A. We're going through on the topic of neutron dose reconstructibility, and I was reflecting on the history. We've had, I think, a pretty vigorous and productive discussion on this. It's covered a lot of time, but it's been, I think, a pretty good discussion on the aspects of the ER that dealt with neutron dosimetry.

As far as background, I'm going to turn to Ron in a bit to give a little bit because every time we have these meetings that are six months, eight months apart, the thread gets a little weak. So I think it's useful just to make sure we're on the same page as far as what we would see as the history of this thing.

We identified in the past issues related to the coworker approach in terms of applying derived N/P ratios. That was one issue and certainly also mentioned some concern over the use of the categorical dose rather than the actual dose felt at the NTA. And this is the early period: 51 to 60.

So there was a number of issues. Some of which we felt were, as John would say, tractable and the discussion was centered on that. The most recent development, the one that perhaps we were particularly concerned about was the proposal, the proposed application of the MCNP model, the Monte Carlo model for addressing the low-energy neutrons being at issue. And that was introduced in the December 2009 -- I think I got the date right -- White Paper that the Work Group received.

And this was just before the January 5th and 6th Work Group meeting. And I remember because we were quickly and busily looking at this over the holidays, but we didn't really get a chance to spend much time with it except just to ask clarifying questions. And as this group will remember, we had a pretty detailed discussion with the help and facilitation of two former Mound workers at the last Work Group meeting which, you know, we were looking at the configuration that they could recall in some of these plant locations.

We were asking questions about the shielding involved. I think there was some question of shielding. And that was helpful because I think they shed some light on what shielding would have been used back in that time, which has some real significance for what the attenuation might be.

And the way that was left, I think, was to go back and examine the MCNP application, the use of this new approach, this new tool and the implications of applying that new tool relative to things like the attenuation afforded by this shielding, the thickness of the material in the gloveboxes and some of the other issues, and this was a large part of what we examined.

It is a bit of a detour because this was an issue we had not seen coming in terms of the MCNP application and these implications. But I think over the last several months both NIOSH and SC&A had looked at that and are bringing that back to the Work Group. And this is the thrust of the several White Papers that have come out: was to look at this particular issue and to examine it since it is a relatively new proposal that has come before.

And as I recall, this is the first time, and, Jim, you can correct me, but that

MCNP -- used in this application. So it was something we wanted to take a look at, and I think the Work Group wanted NIOSH to come back with something as well.

So we had done that and we do have some questions which we'll get into, but again I think Ron's been sort of our go-to person for neutrons. So I wanted to go ahead and have him walk through a little bit of this history, then where we came out relative to these analyses.

DR. BUCHANAN: Okay. Thank you, Joe.

Mr. Ron Buchanan with SC&A. And what I'd like to do, we've all done a lot of things since January 5th, so I wanted to go back through how this progressed the last couple of years and why it is an issue.

Recently in TBD-6, it was decided to apply some correction factors for the lower-energy threshold cutoff and for fading for angular response.

And later on then when these came in to question, then the MCNP tool was used to further qualify the amount of dose lost between -- below the threshold. And again I'd like to go back over some very basic interactions of the neutrons with the dosimeters so we can understand why this is an issue.

NTA film was used at most sites in the 50s, 60s and 70s. At Mound it was used -- we're talking about the period from 49 through 77, NTA film, which is an emulsion.

The neutron interacts with the hydrogen, creates a recoil proton, creates tracks in the emulsion, and then someone has to look at that through a microscope and count the number of tracks and relate that to the dose. And I go into that level of detail because this is different than photon film. Most gamma and X-ray films, you read the density automatically through a densitometer and it's a fairly simple process.

Neutron detection is always more complicated. NTA film is more complicated; it's prone to errors and also calibration factors.

And so NTA film starts to decrease its response as the energy of the neutron decreases because it doesn't create as many tracks. The reader has to see at least three dots in the track to be able to identify it as a dot and not some background.

And so the problem at Mound is that if the worker is exposed to low-energy neutrons, then some of these neutrons will create minimum tracks, say three dots or less and so some of that information is lost to the reader. And so if you're calibrating with a higher-energy neutron source and using that calibration factor and you're reading film that's exposed to a lower-energy neutron source, you'll not read all the dose. You won't record all the dose and so this is where you need a correction factor to compensate.

Say, for example, the person was exposed to a hundred millirems and the reader only reads 75 millirems because some of the dots were too short to register. And so you would need to correct it by 1.3, one over .75, to get back to the hundred millirem.

Now the problem is you have to know what the energy spectrum is out in the field where the worker is actually working to attain this correction factor. And so originally in the TBD-6, it was set at 1.14 from the Savannah River data. We questioned that and some other factors, and so NIOSH went back and used the MCNP tool, which is simply a scientific program.

It's like your -- a very complicated calculator. Okay. You put parameters in and those calculations follows each neutron, says how many below the threshold, and that's what NIOSH used then to make the adjustment factor. You must realize that MCNP is a tool. It's a computer program and what it depends on is what you put into it. Do you put in the right parameters, you know, garbage in/garbage out or correct information in/correct information out.

And so what we wanted to look at was what parameters NIOSH was putting into the program, and were these realistic for the Mound site? And so the debate came out, like Joe referred to, in the January 5th meeting as what was the parameters that were put in and was it realistic.

Well, some of the former workers said you can have up to 12 inches of moderation. Now shielding is good in any case. However, as you moderate the neutrons, they decrease in energy, and so you lose more and more of them falling below the readable threshold. And so in that case it's detrimental because you lose more and more of the information. And so you have to make an increased adjustment factor for that.

And so what we wanted to see was we

-- SC&A ran their own simulation to look at how many would fall below the threshold as compared to what NIOSH presented and did we agree.

Number one, did we agree that the neutron energies did not fall off to the point where you couldn't detect them? I mean, you could envision a situation where the neutrons would fall below the threshold, all of them or 90 percent of them, and you couldn't detect them. And so you don't have that information to correct.

Well we did these simulations using our own equations and such and we found out our number one thing we wanted to look at was, did they all fall below the detectable threshold. And, no, they didn't. Even if you went out to 12 inches of water, you still -- the neutron spectrum flattened out and you still had an array of neutrons, some higher-, medium-, and low-energy that were detectable.

And so this was one of the basic

questions we wanted to answer. And using our model -- our simulations, we found out that that about eight to ten inches of water was the most claimant-favorable position to use.

Since we didn't know what all the gloveboxes consisted of and stuff and we said the maximum 12 inches, we ran it from zero to 12 inches. We found eight to ten inches of water maximized the correction factor and would be claimant-favorable without -- and be plausible. And so we ran those simulations to check on that.

Also, we ran the simulations to see how they compared with NIOSH's model presented in their December of 09 paper. And what we found was we actually, to put it simply, NIOSH looked at using the Monte Carlo technique to look at the number of neutrons that fell below a half MeV and say that 25 percent fell below a half MeV.

So you do a correction factor of one over .75, 1.3, times the recorded dose. A fairly simplistic point of view. Now what we did, we went back and we got to looking at the Mound data and the -- Meyer's log book and his report, and found out that Mound used a lot of different conversion factors during their history.

Back in the 50s, 60s and into the 70s, neutrons were kind of a new area that people were working in, and they weren't sure what the conversion factor from flux to dose was. In other words, how many particles per centimeter squared per second created one millirem of dose?

Sometimes they used 50, they were going to use 27 and a half, sometimes they used 70. So it varied over a wide factor through the years at Mound, but it was recorded.

And so what we did with our simulations, we used the Lehman document to go back to the very basics, the primary principle of neutron interactions in the emulsion and determine how many tracks would be created that were recognizable in the emulsion, and then calculated what the correction factor would be from that -- folding in.

What we did, we backed out, we -- Mound -- flux-to-dose conversion factor which changed periodically. We knew when it changed. Backed that out and in so that that wouldn't influence the dose on a superficial basis that we would have the raw data, so to speak, without the correction factor that Mound had used, implied.

And so going from first principle, we derived that, like I say, eight to ten inches was the most claimant-favorable thickness moderator to use. The neutron flux flattened out so it was usable. And that the observer position -- now when you talk about tools and modeling, the MCNP is a tool, a complicated calculator. The modeling comes in when you put in parameters. Okay. What parameters you put in.

Okay. The problem came up was, at Mound they had no real specific neutron energy. And so we had to say -- measured in the field that was really documented that we could use as benchmarks. And so what we have to use is what we think a maximum thickness would be for the person that would be exposed to the maximum low-energy neutrons that wouldn't be registered, et cetera.

And NIOSH set up what they considered a maximum exposure potential, which was a concrete silo with a source in the middle with zero to six inches, we extend it up to 12 inches, of water moderator of polyethylene, and then count the scattering the low-energy neutrons created. So that's the modeling we did and the parameters you put into the MCNP.

And so what we found out was that it appears to us that the MCNP can be used as a tool, a complicated calculator, to take these parameters and calculate the amount of correction factor that should be applied from first principles, not necessarily just how many fell below the half MeV, because we came out with factors that differed from NIOSH. Some were lower correction factors; some were much higher.

And we really don't know exactly why, other than we backed out the dose conversion factors and started off with the raw data, so to speak. We used up to 12 inches of water rather than stopping at six, and several other details which we can get into more, if it's necessary.

But we did reach two conclusions. Number one is that it looks like it's a usable tool. Number two, we don't agree with the correction factors provided. So far we think they missed too much of a dose. Another reason is that, really, the decrease in sensitivity in NTA film is not a step function.

You don't really have a threshold

at .5 or .7, .8. It varies across, depending on who you talk to, but .5 is a little low. But even if you assume that, it isn't a step function.

Any time your exposure field is lower, is more moderated than your calibration source -- which Mound used an unmoderated calibration source -- then you're going to lose some neutrons. And it's a rapid decrease from your calibration source down to where you can't read anything at all about .4, .5.

And so we actually used the slope of the curve as opposed to a threshold. And so that could explain some of the difference, too. So we came up with the fact that it is usable. However, I think first principles need to be used rather than just a cutoff point.

Now this did lead to another situation we found, was that fading is important in NTA film both for if you use a correction factor or you use a cycling method.

And let me go a little bit into fading because it's somewhat connected to what I just talked about, is that as the neutron energy decreases, you create smaller tracks. And so if you have a high-energy source, say a 4 MeV or even a 2 MeV neutron source, bare source and you expose the film to it and you create six to eight dots per track and some of them - half of them fade away, you still got three left and so you count that.

With high-energy Pu-Be or Po-Be sources around 4 to 5 MeV, you can expose them and within a week or two read them, and you have a small amount of track fading. However, if you expose an NTA film to lower-energy neutrons, say plutonium fluoride which Mound started using then in 63/64 time frame, then you start getting more fading. And Mound recognized that, and they did three papers.

One was an undated, unsigned two-page letter memo -- it wasn't a report -- that saw nine percent fading. Another one was a published Mound Publication 1490: 33 and 56 percent fading at one week and two weeks respectively. Another one saw 16 and 30 percent fading or something like that.

Now the problem is none of these matched the workers' fields because the workers' fields was moderated. And these were done without moderation other than the last one I spoke of and it was moderated higher energy source. So it brought it back down to about 50 fluoride energy range.

So we're looking at the fading studies done at 1.3 MeV average energy, and we're looking at the worker, what little, scarce information we have at Mound, around .8 MeV was some of the average energies measured out in the field, .75, .9.

So your fading is going to go away faster on your low-energy neutrons and that's just a known fact. And so our concern at this point is that in the TBD, they recommend on Page 30, they recommend 33 and 56 percent fading, 33 percent for one week, 56 percent for two weeks.

And this was taken from an unmoderated PuF source which would be slightly higher in energy than you have out in the field. And then in the ER they recommend a nine percent fading factor taken from the unnamed, unpublished report.

And so we find that fading -- doing this MCNP analysis and looking at the fading documents at Mound, we did not find where -- that it was documented where they did a fading study using the appropriate moderated source either for a correction factor to go back and multiply it by, or when they started cycling.

Now when they recognized this they decided, well, we need to do our calibration in the same sequence that the worker is exposed. In other words, if we're on a two-week cycle and we expose a calibration film, one every day to a little bit of radiation for two weeks assuming the worker is even exposed through those two weeks, then our calibration fading will match that to the worker, and that's a good idea. That's halfway home.

But the other part we didn't incorporate was when they used the calibration source, they didn't use a moderated source. They used a bare source, which would have higher-energy neutrons.

The worker was out there, say his film badge went a week or two weeks, and he was exposed to lower-energy neutrons in many cases. So he would have a greater percent of fading.

And so the fading studies done at Mound, like I say, was halfway there, but they didn't use moderated. So the worker would have a lower reported dose than he should have even after he corrected for the cycle or a calibration factor.

So, as far as the recorded NTA data that the dose reconstructor is going to use when he does -- dose-reconstructs a claim, he will be using the data that's recorded which everybody admits is low. However, the correction factor for the number, the amount of dose lost below the threshold, and that's lost because it wasn't readable because of fading, will be lower than what the ER reports its correction factors will correct for.

And so that is where we're at on that. That's the two issues we have with MCNP is that we feel that it needs to be run more realistically, and that the fading factor needs to be addressed.

We don't feel that it has been sufficiently addressed. It's kind of been on the table but not really addressed, and we felt it is headed in the wrong direction going from the TBD to the ER.

CHAIR BEACH: So, NIOSH, do you want to jump in?

I know, John, you had --

DR. MAURO: The only -- I guess in listening to the issues, it seems to me that the -- when we first began this, the main concern was that, you know, we have more than two inches of shielding, could have as many as 12.

I guess I walk away after talking to Joe and Ron about, well, this really made me concerned, you know. That has to be looked at and whether or not we had a tractable situation. And the reality is we do have a tractable situation.

That is, yes, you could add 12 inches and there are ways to accommodate that. It's not that when you have 12 inches all of a sudden you can't detect anything. You're going to get a reading on your film badge that - and you can derive adjustment factors to account for the fact that you've attenuated the film.

So the way I look at it is that part of the problem appears to be a tractable problem. The problem, of course, is that we feel that the adjustment factors that you folks derived and the method you used needs to be looked at again because we've actually come up with adjustment factors that are somewhat different. As Ron pointed out, in some cases our adjustment factor is lower, but in some cases they are quite a bit higher, but I think it's tractable.

The part of the problem that Ron just described that we don't know how to approach it is the business of fading. What factors do you apply?

Right now I believe you are recommending a nine percent fading factor per week, I believe it is. Based on -- as Ron described, we don't think that number is necessarily the correct number. It could be higher, and it could be substantially higher because that nine percent was based on looking at fading from a naked source with a relatively higher energy distribution than an attenuated source.

Now it's possible that there's some

literature out there that would give insight into, okay, what's the fading factor per week, the percent per week loss or an attenuated neutron spectrum that's closer to the attenuated spectrum that we now know based on our calculations.

So I mean -- so I see that we have what I would call part of the problem is certainly tractable, but right now with the other part we're not sure. And that's the fading part.

With regard to modeling, I know that modeling is of great importance to the Work Group and to the Board. And I know it was extensively discussed regarding Blockson, and they were concerns. Some folks liked the model, some folks didn't like the model.

I just wanted to point out that in this case that we call MCNP a model, but it's important to recognize it's a physics model. Which means that it's sort of like gravity or a point-kernel. In other words if you know the initial conditions and you correctly design the initial conditions, define the initial conditions, the physics of it are straightforward.

The questions you could ask: are the cross-sections proper. Well, these are well established. The MCNP cross-sections, neutron interaction, well researched, well documented, well tested. So if you define your initial conditions correctly, you are very confident that the outcome of your calculation is going to be correct.

One of the issues that was raised originally was, well, we have to use at least some information that's site-specific. And so this is required by Part 83.

And so the way I see it is that, well, some of the site-specific information we have is we know something about the neutron sources. And we also have information regarding the actual readout on the NTA film. So that's site-specific.

One of the questions that came up that we specifically looked at, but we really didn't look at the geometry and the kind of glovebox and could that have a bearing on the outcome, an initial condition, and it turns out it doesn't. It really doesn't matter what kind of glovebox you have.

What does matter is the thickness of the shielding, of course the original naked source, and what the outcome of your film badge reading is, but it doesn't really matter what glovebox you use. Another thing that mattered is we assumed when we looked at the problem, one of our concerns was, is the source in front of the person or is it possible that there's another glovebox behind the guy.

For example, I'm working on a glovebox standing here and working. Okay. Here's my neutron source. And here's some shielding between me and my film badge, right?

And we model that using MCNP, a

physics problem. And the thing that's important is we know what the energy spectrum is at the source, we know how thick, how many feet of water. Okay. And then we've got our reading, and it's a physics problem now.

And the fact that it's in this box, how the box is shaped and what it's made out of really doesn't change anything. But what does change something is if there's another guy over here working in this neutron source right back to back. Okay. Then what happens is all bets are off.

But based on the information we have, and, Joe, you could confirm this when we were speaking, and you folks were interviewing a lot of folks, we really only have AP, anterior posterior, exposure geometry. We don't have a significant source of neutrons coming from behind the person.

So when we looked at the problem, our initial conditions, we basically said here we have the source and there's a concrete room, we have certain dimensions, it was concrete because you get scatter and it's all built into the calculation, but we do not assume that there is another neutron source behind the guy coming in through him from the back. And if that's the case, if there's reason to believe that that in fact exists, well, then we have a problem.

But right now given the initial conditions and our understanding of them, we feel that you have a tractable problem. And the only part of the adjustment factors that we don't know what the answers are, and I guess we look to NIOSH to look into this matter, is the fading question. Because the fading is going to be greater for an attenuated source than an unattenuated source.

But we do feel strongly that MCNP is a very useful, powerful tool as long as you have the initial conditions well defined. Thank you.

CHAIR BEACH: And just so I

understand, and then the moderator, how many inches you use, that's important also?

DR. MAURO: Yes, what we found out -- in fact, we have a table that you haven't seen this. What happens is the -- let's say I -- we have a naked source, and I know what the dose is to me from the naked source. And let's say that's one. The dose is one. All right.

Now as you increase the amount of water attenuating it, what happens is you have to multiply that. Because you're starting to attenuate a source, you have to apply an adjustment factor. You're going to start to lose the tracks. You're going to start losing tracks and you're going to -- if you don't take into consideration the attenuation.

And you hear people say, well, it flattens out. Well, what does it -- the adjustment flattens out. Well, what happens is if you add one inch, you have to multiply whatever dose you're reading by a factor: 1.1.

You have to say, well, it's reading one, let's say. But because you have an inch -- that would be for the naked source. But if you put an inch in of water, you might have to multiply that by 1.1. If you put two inches, you might have to multiply by 1.15.

And what we did is we looked at as you added more and more inches of water, shielding, you have to have an adjustment factor that gets higher and higher. Well, it turns out that it does flatten out. When you reach around -- depending on the distance, there are other variables, but it does flatten out.

That is once you reach eight, nine inches, the multiplier may go up as high as 1.3, maybe 1.4. And then when you add more inches, it doesn't change. You have to go to 11 inches, you go to 12 inches, it's 1.4, 1.4. It doesn't change.

So the multiplier flattens out and that's a very important finding because that makes this a tractable problem. And so that was, I would say, the single most important finding that concern -- initial concern at the meeting. Does it flatten out?

Or the real concern was let's say you had 12 inches and you're getting -- and all the neutrons that are coming off the naked source are all below .4 MeV.

Now here's a guy standing there, he's getting hit with a flux of .4 MeV neutrons, but the film badge is not reading anything. That was the problem. That's an impossible situation, but that didn't happen.

Reality is we still get plenty of neutrons that you can count and you could predict what the adjustment factor is and it does flatten out. Now where it flattens out is -- it depends, but we're finding out even, you know, maybe under all circumstances at around eight inches, it flattens out.

So you can put an upper bound on what the adjustment factor is to account for the amount of shielding there is. So I mean we come away from this thinking that a large portion of the concerns we have, have been alleviated in terms of, I think we have a tractable situation, except one. And that is the adjustment factor for this fading issue.

And I think that -- is there any more to the story that you think -- or is that really what it boils down to?

MR. FITZGERALD: No, I think that captures it. And the question, you know, the one question in January was applying the MCNP as a whole in terms of, you know, a more generalized tool, model and whether that 83 -- the regs, but I think that again we felt better going through the analysis and doing that.

We have other issues on the coworker model, but I think I want to hold those because I think we're focused -- pardon me?

CHAIR BEACH: And I was going to say

if anybody had any questions on the first two issues that Ron brought up and then --

MR. FITZGERALD: Yes, I think we should keep on those issues and then --

CHAIR BEACH: Yes.

MR. FITZGERALD: -- go to NIOSH and, you know, I think that's pretty much a thumbnail sketch of where we came out.

CHAIR BEACH: Any other questions on those first two?

MEMBER ZIEMER: I just wanted to clarify because the issue of the possibility of another bank of gloveboxes behind, we discussed that at pretty much length the last time.

And it seems to me as I recall, that we had a pretty good picture of the layout from the workers that were here and then we determined that either the distance or the -- in fact, there wasn't another bank behind it. I can't remember which it was, but --

CHAIR BEACH: Paul, I thought there was, but the distance was great.

MEMBER CLAWSON: There's two sets of gloveboxes.

MEMBER ZIEMER: Yes, yes. But it's not like they were right -- the distance was really great. You know, there's another important factor that causes the value to fall off. It's basically an inverse square thing plus the moderation.

And I think one could calculate this, but intuitively the contribution from basically thermal neutrons at that distance compared to the direct has got to be awfully small.

DR. MAURO: That's where we came out also.

MR. FITZGERALD: And that was the most valuable input, as you said, having the workers put --

MEMBER ZIEMER: Right.

MR. FITZGERALD: -- schematics up

because in one case you had a horseshoe with the workers on the outside of one building, and then you had a bank of --

DR. ANIGSTEIN: If this is --

CHAIR BEACH: Just a sec, Bob. We'll get right to you.

MR. KATZ: Bob, there's a discussion going on.

MR. FITZGERALD: You had, as you said, two parallel banks, but they were so far apart.

MEMBER ZIEMER: Right, right.

DR. BUCHANAN: Twenty-five feet or so.

MR. FITZGERALD: Twenty-five feet or so. So it wouldn't have been an issue. So that was --

MEMBER ZIEMER: Right, but I didn't want us to get into that sort of complicating factor because I think the main issues at least have been identified here, we can, you know, debate on what the correction factors would be.

And the theoretical models we like to rely on, but also in a practical situation those field measurements still are important. And they don't always agree because you can't really model the detail of the whole thing, you know. You have the workers' bodies moderating and so on. But I think the issue of how they calibrate is an important one to think about with the bare source.

As I understand it, the film badges in those days didn't have a -- a lot of neutron badges in more recent decades have had a moderator ahead of the film, but they weren't doing that at that time, I don't think.

DR. BUCHANAN: Other than just the wrapper and the --

MEMBER ZIEMER: No, no, I'm talking about the --

DR. BUCHANAN: Cadmium filters?

MEMBER ZIEMER: Huh?

DR. BUCHANAN: Cadmium filters or something?

MEMBER ZIEMER: No, no, not cadmium. No, no, no, no.

DR. ANIGSTEIN: Excuse me. This is Bob Anigstein, SC&A.

MEMBER ZIEMER: Yes, Bob may recall, but -

DR. ANIGSTEIN: The Mound film badge had a one-millimeter cadmium filter in front and behind.

MEMBER ZIEMER: That's not a moderator, though.

DR. ANIGSTEIN: No, no, no, but they did filter out -- so, Mound never made any attempt to count thermal neutrons.

MEMBER ZIEMER: It filtered them out, but --

MR. KATZ: Bob, do you want to just address because you -- I don't think you were with us when we began, right?

CHAIR BEACH: Yes, he was.

MR. KATZ: You want to address whether you have any conflict of interest with Mound?

DR. ANIGSTEIN: No conflict of interest.

MR. KATZ: Thank you. Just for the record.

DR. MAURO: Bob, did you have any -- I saw that you wanted to add something or --

DR. ANIGSTEIN: No, I was just commenting, somebody, I'm not sure who, maybe this was Brant Ulsh, mentioned thermal neutrons. And thermal neutrons don't even enter into this because Mound deliberately or at least consciously did not count thermal neutrons.

And we did not consider thermal neutrons either, because they were filtered out by the -- first they used one millimeter of cadmium. Later they switched to one millimeter of lead.

That was my only comment other than

the fact that I think it was -- I think it was pretty well covered.

CHAIR BEACH: Thanks, Bob.

Any other questions for Ron? John?

MR. MORRIS: Brant, this is Bob Morris. Did you want me to jump in at this point?

DR. ULSH: Well, in just a few seconds, Bob. I'm going to make a big attempt at some artwork.

MR. MORRIS: All right.

DR. ULSH: Which, unfortunately, you guys on the line won't be able to see.

MEMBER ZIEMER: Or fortunately.

DR. ULSH: Yes, considering my lack of artistic ability.

MR. MORRIS: Ted, this is Bob Morris. I have no conflict at Mound.

MR. KATZ: Thank you for doing that, Bob.

MR. MORRIS: I notice you didn't ask our last caller who has now hung up, whether she had a conflict or not.

MR. KATZ: I don't think she works for the Agency.

DR. ULSH: Okay. So, it seems to me that while we can discuss whether or not we picked the right parameters for MCNP, I mean first of all we never had a question about MCNP whether it was applicable or not, because it's an industry standard.

I mean pretty much everybody uses MCNP or some variant thereof. So, we always had confidence in it.

But as Ron said as with any model, the validity of your output depends on the validity of your input, and so I think we could have further discussions.

John uses the words tractable issues, I use the words TBD issues, and there might be some things for us to discuss there and, Bob, you might want to get into that when I turn it over to you, but by and large I guess what we're talking about now is the fading issue. And I think Ron did a good job of queuing up what the issue is here, but there's one piece of the puzzle that I think we haven't discussed yet.

And I would refer you back to our report dated March 18th, 2009, and there are a series of graphs there. And I'm going to attempt to reproduce it here, at least one of the examples, if I have a good marker.

I'm also going to try to remember to speak into the microphone. But if I forget, someone please speak up if you can't hear me, and let me know.

Okay. So, in our report back in March starting with -- Figure 7-10 is an example of one of those figures.

And I'm going to draw here in the room -- this is an approximation of what that figure shows. And the x-axis is the energy of the neutrons. And the y-axis is the dose equivalent weighted spectra. And this is the piece that I think we're missing, dose equivalent.

So, think about in terms of a neutron source. And this is going to be a very imperfect analogy because I just thought of it during the discussion.

If I'm standing in front of a glovebox with source material inside the glovebox and it's emitting neutrons, think of maybe a hose shooting out marbles. Okay? Again, I admit this is an imperfect analogy.

There's a couple of things that you're going to be concerned with. How many marbles are coming out, that's one. Number two, how fast are they being -- what kind of energy do they have? How fast are they going to hit you?

So when we talk about fading, what we're talking about is the number of marbles. Are we counting the right number of marbles?

And the problem is, as Ron mentioned, once these marbles get below a certain energy, they are not registered on the

NTA film.

So, you might undercount the number of marbles that actually would hit a person.

Here's the missing piece, though. As those marbles lose energy, as they go through the shield and hit water, as they lose energy and become not visible or not detectable on an NTA film, you have to consider what is the effect on one of those marbles when they actually impact a person. And that's the piece that we're not considering.

So, what I've drawn here is Figure 7-10 from our report. And this is a dose equivalent. Dose equivalent is a way of calculating what the actual physical damage is to a person when hit by, in this case, a neutron.

And what you see here is that most of what we see in this spectrum occurs up around one MeV, easily detectable by an NTA film.

And for those in the room when you're talking about dose equivalent in terms of dosimetric significance now, you're talking about the area under the curve. That's what we're concerned about.

This is -- by the way, this x-axis is logarithmic scale. So, you've got one here, 0.1 here, 0.01 here.

So, what I've drawn essentially if you think in terms of a normal x-y graph, is starting from the y-axis and going to the right, pretty much a straight, flat line at zero and then a hump out here by 1.0.

So, what we're talking about with fading low-energy neutrons, those that are undetectable to an NTA film, largely we're talking about this part of the spectrum and whether or not we've counted the right number of marbles.

I would submit to you it's not zero. I'm not going to say it's zero. But the impact of undercounting this number of marbles even if we grant for argument's sake that that's the case, is minimal. We're tilting at windmills here. The impact in terms of a dose that a person would experience is almost trivial.

So, you know, we can discuss whether we should apply a different fading factor. I think that's something that we can talk about. But we can't lose sight of the fact that the real action is up here where the NTA film adequately detects what a person is exposed to.

So, Bob, with that I'll turn it over to you to pick up on that issue or any other issues that we discussed.

MR. MORRIS: Okay. Thanks.

Yes, we've come a long way since the original discussions where we were discussing whether MCNP was valid. The second conversation was, does it have site-specific gloveboxes designed into it.

So, now we're to the point where

we're discussing what's the correct threshold for the cutoff for NTA sensitivity.

Note that we have already picked a number at half an MeV, 500 keVs, that has been already endorsed under one of the NIOSH documents, Implementation Guide 1, if I recall correctly.

DR. ANIGSTEIN: This is Bob Anigstein. Can I comment on that?

MR. MORRIS: Can I just talk for a moment, please?

DR. ANIGSTEIN: Sure.

MR. MORRIS: Is that okay?

DR. ANIGSTEIN: Yes, fine.

MR. MORRIS: And so now we're at a point where we've actually accepted a number in terms of the threshold cutoff to use for the conversation at least that is based on guidance that can be reviewed by a procedures committee or something, some other form if we choose to. But at any rate, we've got a basis for choosing what we've chosen.

We have now gotten to the point where we're having a conversation about the location of the person who's getting the dose, the size and shape of the room that they're in.

You notice that in many cases we've taken conservative, claimant-favorable approaches to these questions.

For example, we put the worker in a silo of concrete that's fairly tightly constrained, actually, compared to the real workplace.

Now, when you put concrete on all sides of a worker like that, you're going to increase the amount of scattering, lower the neutron energy and maximize the amount of low-energy neutrons in that room, probably, compared to the reality of the situation. I think we need to acknowledge that.

DR. ULSH: Actually, Bob --

MR. MORRIS: Also when we have chosen which correction factors to apply, we've chosen to use the ones for the observer location, not for the arm's-length worker location.

And when you choose that one that's two-and-a-half meters away from where the worker is standing or from where the source is, you actually are going closer to those concrete walls standing in a softer neutron spectrum and consequently using the correction factor that is 10 or 15 percent higher than the one that the worker who's getting the most dose would actually see.

So, whether or not we've got these numbers exactly right in terms of, you know, have we got a tally that is exactly the one that Drs. Ulsh and Anigstein would have chosen, I don't think that's really the issue.

The issue is have we got a materially different outcome from what they would predict, or has it failed to be claimant-favorable, and I haven't heard in either case that we've got that.

It certainly was not listed that way in the findings of the June 29th report of your review of the topic.

So, my sense is that we can tune this up. We can leave it alone. In any case, we've got an approach and a value already on the table that's going to be good enough to make these dose estimates.

DR. ULSH: So, I would just add, Bob, that, you know, you were describing the scenario that we modeled, a concrete silo and some other things.

And for people who are listening on the phone who might have actually been there, I'm not aware of a situation where someone was actually working in a concrete silo.

So, someone could make the argument that, well, this isn't realistic to what I was exposed to. And I think at least at the beginning, SC&A raised the same kinds of objections, in other words, that we had to model what the exact layout was at Mound.

And I -- the point that I made earlier and I'll repeat here, is that you are absolutely right. These are not realistic scenarios. They are not designed to be realistic scenarios. They are designed to be worst-case type of scenarios.

Worst-case meaning whatever we're looking at. In this case, the amount of the neutrons that fall below the NTA threshold. These scenarios are designed to maximize that.

So, there are some site-specific parameters that we're using. For instance, the source terms that actually existed at Mound, the kind of NTA or the kind of neutron detection systems that were used at Mound, that kind of thing, but we don't purport to show or to assert that these scenarios that we've modeled are 100 percent accurate for Mound. They're designed to give you a worst-case answer.

MR. KATZ: Bob, were you done?

DR. ANIGSTEIN: Yes, I want to talk

about this .5 MeV issue. When he said it was in the guidance, the guidance does not actually say that. It simply mentions some reference. It says that it's not detectable below .5 MeV.

The closest guidance that I was able to find in NIOSH documentation is OTIB-51, which is -- technically it's applicable to Y-12, but the author reviews the literature on the thresholds: Kerr, et al. Kerr is the senior author.

And to quote, he says the threshold energy of 700 keV appears to give a conservative estimate of the missed dose from NTA film measurements at most facilities.

He then goes on to cite that there were some authors suggest higher, 800, 900. He settles on 700 as a conservative compromise.

DR. ULSH: Okay. Well, here's the thing. This has been extensively discussed at Y-12, as you mentioned, with George Kerr. And the threshold, the energy threshold that you have for NTA film depends a lot on how you actually count the film. How many grains you actually count as a track. And that is site-specific.

So, yes, at some sites it might be 800 because you had a higher threshold for -- higher criteria for determining what was a track. At some sites if you count three grains as a track, then the threshold is different.

So, yes, there are some differences and we could discuss until we're blue in the face, because I know we already have, under Y-12, what the exact value of that threshold is.

Everyone agrees that there is a threshold and it's not a step function. We all agree to that, too.

DR. ANIGSTEIN: But that's the main point. I mean when Bob Morris, I believe it was, said there is so little dose below the threshold, using his marble analogy, I don't quite agree because the -- our analysis which used a sliding threshold, and we made use of the curve that was derived by a man named Lehman at Berkeley Laboratory, at the Lawrence Berkeley Laboratory.

Actually it stops at .4. At .4, he says it's zero. And then it starts gradually increasing. And then there is a maximum in the levels also.

We actually multiplied each neutron that hits the badge through the attenuation. We multiplied the neutron energy by it's detectability.

And what we found is that the -- as compared to the bare source which is used for calibration film, you might have to increase the factor by as much as 35 percent to account for the dose.

In other words, if the film reader at Mound, by simply taking the worker's film and comparing it to the calibration source said, okay, we have a one-to-one relationship here if there is maybe eight inches of water, it could be that it -- if he recorded a hundred millirem, it actually could have been as high as 135 millirem.

So, I consider that not trivial.

DR. ULSH: Okay, but we're talking about --

DR. ANIGSTEIN: And, by the way, the measure that we used was the ambient dose equivalent because that's one of the two measures that is in IG -- OCAS-IG-0001 for converting the measured dose to organ dose.

Dose equivalent is not used in IG-0001, and actually it's an obsolete concept going back to, what, 1971 from the NCRP 38 report and there are big differences.

There are differences, depending on the energy, as much as plus or minus 30 percent between the ambient dose equivalent and the old conflict of dose equivalent.

Just wanted to -- that may be a little pedantic, but I'm going to throw that in.

So, we agree that the issue -- that John Mauro said and as Ron Buchanan said, we all agree that the issue is tractable. It's just a matter of debating which is the best correction factor to use.

I'll just reiterate what I said before. The fading, we at SC&A having looked at -- having examined this and having a dosimetrist from -- a former dosimetrist from Los Alamos that worked with NTA film and specializes in neutrons, and we could not come up with -- find any literature or come up with an adjustment factor that would take care of fading of different energies.

The various reports that Ron Buchanan mentioned that were cited in the Mound literature seem to be energy dependent.

There was the one report that was done very carefully and was published, which was for PuF4, which has an average energy - this is interesting. It has a total average energy of 1.3 MeV.

But if you then discard the low-energy neutrons and weight the neutron spectrum by its detectability using this Lehman calculation, it comes out that the average energy is actually 1.49.

Well, that's the average energy of the neutron that you actually detect on the NTA film.

And here we have many measurements within the plant, and I'm not looking at it now, but my memory serves that at some time at least one measurement, I seem to recall, is in that database referred to as NIOSH. NIOSH actually wrote about a 50-page report and then there is about a thousand pages of various documents interspersed between the pages. It's a report that we actually wrote.

And I seem to recall .5 something in one particular location as the measured actual average neutron spectrum.

So, they go down quite low, and the

answer is how the fading -- the two reports on fading where they give details, there is more fading for the PuF_4 source.

The attenuated PuO_2 source where they attempted, deliberately ran an experiment trying to see, can we account for the energy dependence of fading, and there was apparently an error in the report which was never issued which was in draft form where they say, well, with the -- I think they said eight inches of polyethylene, that the average energy is .9 MeV. That's incorrect.

It's actually about 1.8 MeV because we ran that simulation. We did ten inches of water. We did the same amount of hydrogen as eight inches of polyethylene.

He did his calculations, he put down his result, but he made a misstatement in his report.

So, actually he had a higher-energy source. And as one would predict from just first principles, there was less fading because, as Ron pointed out, I believe the threshold that they used at least in some cases, I mean the Lehman report is four grains.

Anything under four grains could be background. Three grains you can just get from radiation background.

So, if you have four grains and you lose one, you no longer have a track. If you have ten grains, I mean let's say you lose half the grains. So, if you lose - if you have four grains, you lose half the grains or even if you have six grains, you lose half the grains, you don't have a countable track.

You have ten grains, you lose half the grains, you're left with five and you have a countable track. So the higher the energy, the less the fading.

And there was another report that I can -- not from Mound; I think it was INL -- where they did a polonium-beryllium source which has over 4 MeV average energy and they said, there's really no fading.

There's some -- we did -- we look at this, we look at that, sometimes it's higher, sometimes it's lower, we can't really tell, we admit there's probably some fading, but the data is such it's so little that they really can't assign a number to it because they had high-energy source. They were losing dots, but not losing numbers of tracks.

So, I'm just amplifying that this -- what my colleagues have said, that this is an issue. And unless someone comes up with literature or someone commissions a laboratory study, which is something that certainly is doable, I mean the study can be doable, the commissioning of it may not be.

I don't know how to do this. None of us know how to do this.

DR. MAURO: This is John. I think what Bob Morris had mentioned is there are differences in models, the degree of conservatism, the assumptions made, certainly there's judgment involved in that, and I agree with all that.

And when we did it and the judgments we made and the techniques we used, for example, using a continuous distribution of the energies that might be attenuated as opposed to a step function, there's all of this and I would agree it's all tractable.

And the bottom line is that, you know, when we look at the problem and talk about what kind of adjustment factors might be needed, we actually say that, well, depending on the circumstances, we might even have an adjustment factor that's lower than yours.

But if our interest is to make sure that we're placing a plausible upper bound, we're saying that, well, our adjustment factor might be higher than yours by a factor of two or so, depending on the circumstances.

So, what I'm getting at is I don't -- I think that we do have some differences of opinion and methods of approaching this problem, levels of granularity to which we did an analysis, and where we come down is, yes, we're going to come down with differences in our adjustment factors where ours might in some circumstances be twice as high as yours.

And I'm making a very simple generalization. I don't think that's important to the SEC.

Okay. What I do believe is important to SEC, what you just did up on the blackboard is something very important, because you're coming at the problem of fading in a different way that I haven't thought of and I think it's important.

What you're saying is, yes, there might be some fading and that would drive the curve down, but it's not going to change the dose.

I'd like to hear more quantitatively if you can demonstrate that, yes, that -- you're right. We don't have any studies at least for right now in front of us that say this is the -- it's 50 percent per week for an attenuated spectrum under humid conditions, you know. We don't have that.

Okay, and if you don't have that, one could argue, then how are you going to deal with it.

You just came up with an idea that is interesting to me. And that is, well, one way you could deal with it is let's see what kind of effect it would have on the dose. And what you're saying is it shifts the distribution in the way that it drives more neutrons down to an energy where those neutrons are not going to contribute to dose.

MEMBER ZIEMER: Well, that's exactly right. And, in fact, the more important the fading becomes, the less important the dose is for that neutron, is another way of looking at that.

The ones that you lose like the three trackers that you lose by fading, weren't very important to start with. The ten trackers are.

And so as you move down that curve, where you will start to lose the count for tracking, for tallying, the less important those are in terms of contributing the dose.

This curve also is an expression of what traditionally was called quality factor. And that's why fast neutrons, you know, the one where a millirem of fast neutrons takes about, what is it, about ten neutrons, for thermals it takes thousands to deliver the same dose.

So, losing large numbers down in this range doesn't mean very much.

DR. BUCHANAN: Well, I'd like to put a qualifier on that, is that this here, say, is at your 1.3 MeV bare source.

Now, as you moderate that bare source, that whole thing shifts downward. Okay. And so you're going to be -- actually, this line should come up to .5. That's where we're talking about.

And I agree with what you --

MEMBER ZIEMER: Yes, and so you worry about this lower quadrant, right?

DR. BUCHANAN: This becomes more important as this shifts down, because you have less up here. You have more of your dumps down here, and we don't know what that is.

Maybe that's a way to solve the problem just to show how much of that dose equivalent is down in this region to a moderated source compared to an unmoderated source.

And we're not saying that's not a solvable problem. We would just like -- we don't think the fading has been sufficiently addressed in the ER and we didn't fight going from a -- before we had 33 and 56 percent one week, two weeks. And we got down to ninety percent in the ER. And we've seen that we flew in the wrong direction especially when you consider this.

Is this a solvable problem? Maybe it is, but we need to -- and maybe you can't do it in detail, but maybe you can set a limit and say, okay, this amount of -- this difference in dose here can't exceed over 25 percent or something and factor that in.

DR. ANIGSTEIN: I'd like to weigh in on this.

CHAIR BEACH: Just a second.

MR. KATZ: One at a time, please.

DR. MAURO: Yes, Ron was just speaking, Bob.

DR. ANIGSTEIN: Well, okay. I thought he finished.

DR. MAURO: Yes, please, go ahead.

DR. ANIGSTEIN: Okay. I mean I just want to comment -- I want to go back to what Bob Morris said when he said the amount, you know, that only the low-energy neutrons fade, I mean at least they fade more. And, therefore, being low energy, they contribute less to the dose.

That would be fine if you had a worker who was exposed to a range of sources and much of his exposure, much of his tracks came from high energy, and some of them came from low energy and you say, well, that doesn't count very much to the dose.

What about a worker who's in a location, and there are such locations at Mound, or were such locations at Mound, where the whole spectrum is a low-energy spectrum?

Does that worker -- do we say that the neutron dose to that worker is simply unimportant?

Because by taking this 1.3 MeV spectrum, I actually went back and took the -- not just they said -- well, they said 34 percent, but he actually showed the actual numbers.

So, I did that. I did a curve fit and I came up with a slightly different number than what we have currently, and I came up with about six percent per day.

And if you integrate that over -- and some of the badge periods, at one point they went to a 28-day, four-week badge cycle.

So, at the end of 28 days from the first day, you only have 17 percent left. And if you -- and it saves half of -- and then if the real fading is twice that because you have a much lower energy spectrum, it's a significant difference.

I don't think it can be waved away by simply saying the fading only affects the area where there is no dose, so we can just ignore it.

DR. ULSH: All right. Let me clarify.

I'm not saying it only occurs at low doses and so we can just ignore it. I'm not saying that.

What I'm saying is it's a bigger issue with lower-energy neutrons. And as Paul said as they go lower in energy, the dosimetric significance diminishes.

DR. ANIGSTEIN: That's true.

DR. ULSH: And, furthermore -- I don't know, I don't want to speak in absolutes here.

But since Mound was working primarily with plutonium fluoride or polonium beryllium sources, I can't think of a situation at Mound where a worker would have been exposed only to low-energy neutrons.

Now, there may be --

DR. ANIGSTEIN: There are surveys that show areas where the average energy is as low as I think .59 -- I'm going by memory now. So, I'm a little shaky, but I remember a very low number -- and definitely a whole building where the average of all the locations is less than 1 MeV average -- average energy.

So, the workers were exposed. Some workers were exposed to low energy in the neutron spectrum.

DR. ULSH: Please understand what I'm saying.

DR. ANIGSTEIN: And all the calibration of the badges including when they started compensating for fading, was based on the unattenuated, the bare source.

So, there is no question that some workers are going to get shortchanged by ignoring the increase fading of the low-energy neutron spectra.

DR. ULSH: Please understand what I'm saying. I'm not saying that workers weren't exposed to low-energy neutrons. They were.

Because for one thing, especially in PP Building when they moved operations, the plutonium operations into PP Building, they increased the moderator -- or increased the shielding, which of course would lead you to low-energy neutrons. But it's not only low-energy neutrons.

And in fact if we go back to SC&A's summary at the beginning, you all concluded based on your own modeling that there weren't situations where it was all below the threshold. You all concluded that.

DR. ANIGSTEIN: I'm not saying that.

DR. ULSH: No, let me finish.

I understand, but there's not going to be a situation at Mound or anywhere else -- okay, let me back up before I make a mistake.

There's not going to be a situation at Mound where workers were only exposed to low-energy neutrons. There will be some high-energy neutrons there.

Now, maybe most of those marbles are low-energy neutrons, but not all of them. But for those marbles where -- and, by the way, that was my analogy, not Bob Morris'. So, all blame goes to me on that.

Once those marbles are knocked below that energy threshold, knocked into the low-energy region where you can't see them on an NTA film, the dosimetric significance is minimal. That's what we're saying.

I'm not saying it's zero. I'm not

saying we should ignore it. We should talk about it. I think it's important to decide whether or not the factor is nine percent or 35 percent or 20 percent or something entirely different.

We can discuss that, but we've got to keep in mind the fact that as -- we've got competing phenomenon going on here.

As the neutrons drop in energy, they become harder to detect on the film. But at the same time, they become less and less dosimetrically important.

MR. MORRIS: Brant, this is Bob.

One thing I would add is that is exactly the reason that those curves asymptote at a -- and then turn and actually go flat at eight inches of water.

DR. ULSH: Yes, exactly. Exactly.

And when we say it's an important thing to check out and to investigate, I would refer you back to our March 18th report where we did exactly that.

CHAIR BEACH: Is it the same diagram you're talking about?

DR. ULSH: Yes, it's that one up there on the Board.

DR. MAURO: But the nine percent per week fading adjustment factor that you folks offer up in your ER, are you saying that you probably need to revisit that in light of the discussion we just had?

DR. ULSH: Well, John, I would say, you know, we can talk about that. I mean we can talk about whether nine percent is the right number.

But I think you hit the nail on the head earlier when, yes, let's talk about it, but it doesn't seem to me that this is an SEC issue.

Maybe the number is not nine percent. Maybe it's whatever you guys use. We can do some modeling. We can have some more interactions about this. I think maybe we should.

CHAIR BEACH: It has the potential to be an SEC issue.

DR. ULSH: Well, it's not a hundred percent. I mean you have to consider first of all how long they wore the badges and how long until they developed it. Because the longer the wear time, the higher the fading that you get.

Now, I'm stepping out on a limb here, but just going from memory, the people who were in the highest neutron exposure fields were the ones that had the most frequent badge exchange cycles, which would tend to minimize -- not -- okay. It would diminish the effect of fading.

I'm not saying it's zero. But the quicker you change out the badges and develop them, the less impact fading has.

The problem that you get into is when someone is issued an NTA badge and they wear it for six months. And if you make a worst-case assumption and say they got all of their exposure on the first day that they wore it and it was by and large low-energy neutrons, that signal could fade before you actually develop the film.

CHAIR BEACH: It will fade, yes.

DR. ULSH: Absolutely it will fade.

DR. ANIGSTEIN: Let me comment on that. The policy at Mound from what I read, was that the so-called visitor badges were issued on a quarterly basis.

And the NTA film was not developed unless -- there were two requirements, one is the photons had to be above a certain threshold. I think it was a hundred millirem. I don't know. It doesn't really say. It just says significant photon dose. And then, two, they have to know on which day they were exposed to neutrons.

So, if they take that badge on June 30th and say this is a three-month badge and they say, okay, are we going to develop the NTA film?

They won't even develop it unless there was some evidence, oh, yeah, on May 31st you went into this high-neutron area and that's where you would have gotten the neutron dose. And, therefore, we can correct the fading. Otherwise, they won't even bother doing the NTA film. And, rightly, because they won't have any idea what correction to apply to it.

DR. BUCHANAN: In that case they would apply the coworker model, I understand.

DR. ULSH: Right. That's not an unimportant issue. It's just a different issue than what we're talking about now.

Josie, I don't know if you want to get into that discussion.

CHAIR BEACH: No, no, no, no. What I'd like to do is before we get into coworker, I want to take a break.

But I would like to ask NIOSH if you would come back with the response to the Work Group on the fading issue, and then with the adjustment factors as a White Paper, because I know this has come up in several meetings and it's never really been answered in writing.

MR. HINNEFELD: So, now just kind of thanks for doing that, because I was thinking we should wrap this up and you did exactly that.

One is that there is -- so, you want an evaluation back from us on both issues. Both an evaluation of SC&A's sort of recalculation of the correction factor.

CHAIR BEACH: The correction factor.

MR. HINNEFELD: Which everybody kind of agrees that's just a question of what will the number be. Not can you generate a number, but what will the number be.

CHAIR BEACH: Right.

MR. HINNEFELD: But it's still something we need solved.

CHAIR BEACH: Yes.

MR. HINNEFELD: And then the second

item is the fading issue and is in fact that a tractable problem, because the discussion here doesn't convince either side, anybody of anything. At least doesn't convince me of anything.

So, I think there needs to be some more discussion of that issue in order to decide whether that's an SEC issue or a Site Profile issue.

Is that where you're at on this?

CHAIR BEACH: Yes, yes.

MR. HINNEFELD: Okay. Perfect.

CHAIR BEACH: Everybody okay with that?

MR. MORRIS: This is Bob Morris.

I would note that we don't have anything in writing on fading in terms of the findings in the June 29th paper.

And so if we could get that data that you're suggesting we should look at, I'd like to see it.

We haven't been able to get access

at least that I've found yet, to the recent paper, the 2010 paper by Anigstein and Olscher titled Sensitivity of NTA Film -- The Sources At Mound Laboratory, which is cited in your review, but wasn't made available to us.

DR. ANIGSTEIN: Yes, can I comment? This is Bob Anigstein again.

We reissued that paper because it was just one error, one slip-up in one of the links.

So, that paper was reissued on July -- I think it came out on July 23rd and it was transmitted to NIOSH and to the Work Group. So, everyone -- at least everyone in the room from the Work Group and from NIOSH should have a copy of this.

DR. ULSH: Right.

DR. ANIGSTEIN: And this, the one that you've cited, the sensitivity of NTA Film, and that has an analysis -- what I just cited I was reading from the report -- that has a section on track fading.

DR. ULSH: All right. Let me clear up perhaps some misunderstandings here.

The June 29th paper that Bob referred to I think we do have. That's -- Bob Morris has seen that.

MR. MORRIS: Yes.

DR. ULSH: That's another problem is we've got two Bobs on the phone.

And that's not a problem, but in that June 29th paper that SC&A issued there is a reference to a document, Anigstein and Olscher 2010, Sensitivity of NTA --

DR. ANIGSTEIN: NTA Film. That's the one I was just referring to. That is the one that discusses the fading issue.

DR. ULSH: Exactly.

DR. ANIGSTEIN: It was originally issued in May 24th, but then there was a revision that came out on July 23rd, I believe.

DR. ULSH: Okay. That's the one at least Bob Morris hasn't seen.

DR. ANIGSTEIN: Yes, but it was transmitted by our production manager, Nancy Johnson, to the Mound Work Group. And I think it went to Brant Ulsh and --

MR. FITZGERALD: Yes. Apparently Bob hasn't seen it, but that can be taken care of.

DR. ULSH: Okay. So if I got it, I will forward it to Bob Morris.

DR. ANIGSTEIN: Right.

DR. ULSH: And that's not an issue then.

DR. ANIGSTEIN: I mean it's not PA cleared, but that shouldn't be -- but it has been DOE reviewed.

MR. FITZGERALD: And as I recall, it's essentially one table that was really --

DR. ANIGSTEIN: It was one table with change.

MR. FITZGERALD: The change was numbers were --

DR. ANIGSTEIN: There was basically

a reference to the other table where it said -- I had them aligned and in the final production it came out to be shaded areas, which was a little confusing.

MR. FITZGERALD: Well, I did hear Bob mention that, you know, that should satisfy your need, Bob, for the fading discussion that you don't have right now. So, that should take care of that issue as well.

MR. MORRIS: Okay.

MR. FITZGERALD: Okay.

CHAIR BEACH: All right. So, let's take a ten-minute break.

Is that enough time?

MR. KATZ: Sure.

DR. ULSH: So, back again at five past the hour?

CHAIR BEACH: Yes.

(Whereupon, the above-entitled matter went off the record at 10:54 a.m. and resumed at 11:06 a.m.)

CHAIR BEACH: Okay. Is everybody

ready? Let's go ahead and start.

MR. KATZ: Okay. We're just reconvening after a short break.

CHAIR BEACH: Okay. And before we leave Mound, we are -- or not Mound, neutron discussion. We have one more issue under the coworker issue that I know Ron's ready to discuss.

DR. BUCHANAN: Okay. Coworker issue, we're talking about people with dose of record, is what we've been talking about so far. They had NTA film dose of records, how we'd be able to adjust that.

Now, what about the workers that did not have NTA film dose of record? Might have photon dose of record, but no neutron dose of record either because they weren't badged for neutrons, weren't anticipating exposure at that time, or they actually wore a badge, but it wasn't read because the photon dose was below a certain level. And so they didn't go to the trouble of reading the NTA film.

As I say, the NTA film was much more consuming and costly to read than photon film. Well, this is one way of sorting them out.

So, in either way the worker does not have a neutron dose of record and for some reason they're in dose reconstruction. It is by today's standard, should have been monitored, and so how do we assign a neutron dose?

As standard practices at other sites, one method is to use a coworker dose. In other words, look at the neutron exposure to the people that were badged and read and have records, and see what their doses were each year on a yearly basis and assign either a 50th or a 95th percentile of that dose to the unmonitored worker.

And so in NIOSH's paper of December of 2009, they presented a method to limit that dose. In other words for an SEC, you want to write a method that would limit the dose.

And one way was to use N/P method, which is that you look at all the workers that had records above a certain point, say 50 millirem. And there's something like 10,000 records for the whole time period.

I went back and looked at some of those and they are actually there on the MESH database of recorded NTA film and gamma-matched pairs. And look at this on a yearly basis and say what was the N/P values, and then assign that worker for that year.

For example, let's say the average N/P value for 1960 was four. And so you -- if the person got a hundred millirem of gamma dose, that would -- you would assign them 400 millirem of neutron dose in addition to that, and this is an acceptable method.

However, this is a -- NIOSH limited this as a limiting method -- or labeled this as a limiting method to bound the dose.

Another method that they proposed

was that -- the way I understand, it would be more realistic assigning the individual doses was to use categorical data from 1951 to 1960, those ten years in which some HP reports listed categorical information for neutron dose, which was not specific dose recorded, but how many badges read in a zero to a hundred millirem range, how many read in a hundred to 300, and how many read over 300 millirem, which I'll call categorical data.

And then this information, both the neutron NTA-recorded data and this categorical data, was multiplied by the MCNP correction factor, fading factors and angular distribution factor which we previously talked about. So, those factors bear upon the coworker dose also. And then they provided tables of the 50th and 95th percentile in their paper.

Now, SC&A would like to address two issues. Number one is the validity of the N/P values which we talked about in January. We don't have any further information on that, which I reiterate that.

And secondly, the fact that the categorical data, SC&A does not feel this is necessary or valid data method to use.

And so in our opinion, the categorical data doesn't really add to the ability to assign dose.

If we're going to use something, we have quite a bit of NTA film data. We'll use it if it's verified. If it's not verified, then the categorical data isn't any good either. So, we would like not to use the categorical data.

The neutron-to-photon ratio data, the two issues we have there is we think there's quite a bit of data there. However, when we look at the spread in the data from year to year or within a year, there does not seem to be a good correlation between the neutron and photon ratios.

And we did not go through and do a

lot of analysis on it. But in the ER paper, we have Table 4-4 which lists the medium and -- 50th and 95th percentile distribution for each year. And we see that there's large variations in this, which we brought up before, from one year to another. It might change by a factor of two or three years.

And then the box and whisker plot on Page 20 in Figure 4-2, shows a large variation within the year.

So, we question the applicability of this N/P data. And we also question why just -- we haven't looked at it. We just wonder wouldn't the NTA film data for each year, just use it as coworker dose as we do gamma dose.

In other words if you have a hundred readings, you look at the 50th and 95th percentile of a hundred readings for 1960, and the same thing for `61, and just do a coworker dose assignment based on the NTA film rather than trying to use the N/P values which seem to fluctuate a lot.

So, maybe there's a valid reason we don't want to use the NTA data by itself. And we also would like some assurance that the N/P values are paired -- I mean are correlated, the neutron is correlated with the photon since the information we have doesn't appear to be very correlated.

So, that's where we stand on the coworker neutron issue at Mound.

CHAIR BEACH: Anybody have any questions for Ron before NIOSH?

Any other comments?

Okay.

DR. ULSH: Okay. So, this is Brant Ulsh.

Basically, to go back to the approach that NIOSH has put on the table and just kind of summarize where we are, we've talked about earlier in this discussion, a situation where people who wore visitor badges -- now, this is a little bit different than what you might be thinking. This does not refer to visitors to Mound.

So say, for instance, someone came to Mound from Los Alamos. That's not the situation we're talking about here.

The visitor badges that we are describing are, for instance, SM Building. If a Mound worker was not routinely assigned to SM Building, say, for instance, I don't know, a pipefitter, but he got called up to SM Building to do some work up there, he would, at least, in the early years, be assigned a visitor badge.

So, this is a Mound worker who's not normally assigned to that building. And the visitor badge would consist of a gamma film and an NTA film.

So he goes in, he does his work, he drops his badge when he's done. And as someone described earlier, I don't recall who, there was a time period where, if the gamma badge didn't read above a certain level that I don't know off the top of my head, then they wouldn't bother to read the NTA film.

So, in a situation like this, even though the worker wore an NTA film, we would consider that an unmonitored dose because the film wasn't read. So, he might as well not have been wearing it. So, that's the situation we're talking about in the early years.

And for that time -- well, one more point to make. Ron described two categories of people to whom the coworker model might be applied, the neutron coworker.

The first was people who were not badged at all, and the second was the category I just described where people were badged, but not read.

Now, regarding the first category, people who were not badged at all, we've discussed that there were a couple of workers here at the meeting in January, and then -- I'm trying to think -- at least one of them --

I think both of them participated in a subsequent interview. And, Ron, I know you weren't involved in that interview.

But during that interview, we discussed with them what the badging policy was in terms of who wore badges and who didn't. And we went into a bunch of different examples, scenarios. People who took out the trash. People who moved boxes from here to there. Would they have been badged?

And I recall very clearly that the input that we got is, yes, people would have been badged.

So, I would contend to you that that first category of people, people who just simply weren't wearing a badge, I'll never say it's zero. But by all indications that we have, people were badged if they had an exposure potential.

DR. BUCHANAN: For gamma and neutron, or just gamma?

DR. ULSH: For gamma and neutron.

DR. BUCHANAN: Okay.

DR. ULSH: That was the specific topic of that interview was neutrons and neutron issues.

DR. BUCHANAN: Okay.

DR. ULSH: So, that category I think is going to be fairly -- it's going to be really small. I won't say zero. But the other category is a bit problematic, people who wore badges and the badges weren't read. So, essentially you're talking about they essentially weren't monitored.

Now, we've proposed a number of different approaches based on the data that we have readily available.

For the early years when we have the health physics progress reports, and those run from I think day one, 1949 up through about 1960. It's been a while since I've looked at them. And those reports typically contain the categorical data that Ron described.

So, the number of badges read that fell into the zero to a hundred millirem, for instance. And then the 100 to, what was it? 300, Ron?

DR. BUCHANAN: Yes, and above 300.

DR. ULSH: Yes, and then above 300. So, different categories of neutron exposures there.

The problem is, is we don't have those reports past 1960, as we've described before.

I'd sure like to have them, but I'm ready to conclude that they simply weren't written after 1960, because we looked really hard for them and just don't have them.

I don't know. I've never understood the objection to categorical data. I understand that it lacks the resolution that you might have from looking at just the entire population of NTA films -- and, by the way, I think that the reason we didn't propose just looking at the NTA films themselves was because prior to some year, I think it's in the 1970s, what we have readily available in terms of electronic database, data that is readily useable for this kind of an application prior to 1977, what we have is annual totals.

I don't think that we've got in an electronic format, the individual cycle-by-cycle reads.

Now, that's not to say that we couldn't go grab the neutron dosimetry logbooks, code all that data and use it, you know. I'm not saying that. It's just that it wasn't readily available.

And keep in mind the purpose of all of these reports that we have written that we're talking about here and that's simply to determine whether or not we have an SEC issue, a completely unboundable neutron exposure, at worst, we contend that we don't have an SEC issue here. Because that categorical data while it lacks resolution, it's perfectly adequate for this.

I mean we can establish an upper bound dose that can be applied to people who perhaps wore a visitor badge and their neutron film wasn't read. There's no reason why we can't do that.

I know that SC&A doesn't like that data, using that data, but I've never really understood the basis for that.

Now, in terms of the N/P ratios, Ron referred to a couple, you know, a table and a graph from the report that we wrote, and I think we're in agreement that those values are variable. We don't deny that.

However, I would contend that that works in the worker's favor. As in other situations here in this program, the more variable the data and you take, you know, an upper 95th percentile, well, then the higher the N/P ratio you pick.

Is it going to overestimate its dose? Sure it is, but why is that a problem?

I don't understand why that would be an issue.

Bob Morris, do you have anything that you want to add on this?

DR. NETON: Before Bob talks, we talked about this N/P ratio issue several times where there's a lack of correlation, and I've never understood the objection there either.

Because effectively, what we have is the distribution of the N/P ratio to the worker. It is what it is and there's no prior reason to believe that they're correlated.

But as Brant said, we're not using a point value here. We're using either a distribution that's applied or the 95th percentile at worst case.

So, I'm not sure why there's sort of an up-front impression that the N/P ratios have to be correlated for them to be useful if you apply distribution.

Because the distribution is what it

is, and you can't argue that the 95th percentile in distribution is the highest N/P ratio or one of the high-ended N/P ratios that was observed. That's always been an issue.

I think we talked about it at several other meetings. It seems to keep coming up.

DR. ULSH: Bob Morris, do you want to add anything?

MR. MORRIS: No, I have nothing else to add, Brant.

DR. ULSH: Okay.

DR. MAURO: We were talking about this, and we've talked about it before and I was thinking about this.

So, we have two numbers that are measured, they're a couple. There is no apparent correlation for some reason. Often there is, but in this case there's not. And whatever the reason is, it is.

Okay. Now, bear with me because I'm not trying to be a wise guy.

Let's say I had numbers where I measured a person's height and measured his -- had paired numbers. Height and neutron dose. Okay. No reason to be correlated -- maybe there is. I don't know.

But I made a table, and here's the ratios and I say, well, I'm going to pick the highest one. There's something about that, that disturbs me.

DR. NETON: Yes, except for the fact that those were not measured in the field at the same time.

DR. MAURO: No, I'm saying if you did that.

DR. NETON: No, but the height -- the height is not a variable that was observed.

You observed two variables in the field that were measured simultaneously, and all we're saying is that the neutron, the photon ratio, the highest possible one that you found, which is valid, is a valid worker assignment.

The height has nothing to do with the exposure of the worker at all.

DR. MAURO: Well, we're saying if they're not correlated. If there's some reason in this case --

DR. NETON: I'm not saying --

DR. MAURO: The correlation coefficient is one, .1 or something some very, very low.

So in other words, unlike -- see, intuitively we feel that there should be some relationship between whatever the neutron exposure is and what the photon exposure is. And so you measure -- you pair them up.

DR. NETON: All I'm trying to say, John, is the upper end bound of that ratio. These are measurements based on a worker, right?

I mean, so what is the highest experienced neutron-to-photon ratio? Let's say we're going to use the highest value.

We're not proposing that, but let's say what is the highest possible scenario that existed in the planet that this worker had ten neutrons for every photon measurement?

That's a valid bounding estimate.

DR. MAURO: You measure two parameters.

DR. NETON: But they're measured simultaneously.

DR. MAURO: Measured together. Okay.

DR. NETON: Right.

DR. MAURO: Again, let's say instead of doing that, when I measure the neutron dose for that change-out period, I also measure, as I said at that time period for that person, his height or his weight.

DR. NETON: But his height has nothing to do with the exposure of parameters that we're --

DR. MAURO: Well, they're not related, right. But the two parameters if they're not correlated, there's no reason to believe there's a relationship between neutron and photon dose.

DR. NETON: Yes, there is.

DR. MAURO: I'm saying is that any more meaningful than if I was to couple up some other paired parameter?

DR. NETON: It is because I can confidently say that no one was exposed to more neutrons than ten times the photon dose. So, whatever it was. Because that's the highest value I observed in the workplace setting.

I've done an empirical measurement and I said any time there's photons, the worst case I've ever seen for neutrons is this. So, I bounded the worst-case scenario.

Now, we can argue whether it should be the 95th percentile or you do it by distribution, but these are empirically measured numbers -- I mean values.

MEMBER ZIEMER: John, I would

maintain that they are correlated, but we don't know the correlation.

DR. NETON: Maybe that's the --

MEMBER ZIEMER: I always tell my students in -- I don't know of any case where you have neutrons where there's not a gamma field. They are present at the same time. You can have a gamma field without neutrons, but you never have a neutron field without gammas.

There is a correlation, but it's not consistent because there are so many factors that affect it.

There's geometrical factors, there's --

DR. MAURO: Shielding.

MEMBER ZIEMER: Shielding factors, there's all of these things that go on. The neutron spectrum changes in a different amount than the gamma and so on, but there is a correlation in every instance and it's different.

You go in a different room, different sources, it's a different number.

DR. MAURO: Under the circumstances that this is --

MEMBER ZIEMER: Unlike height and neutron dose where there truly is no relationship, if everybody's geometry and source was identical in that plant at every instant, you would probably get the same ratio, but it isn't.

DR. MAURO: It isn't.

MEMBER ZIEMER: It isn't. At least this is how I think about it.

DR. MAURO: I see.

MEMBER ZIEMER: So, you go through the plant and you measure a whole bunch of different situations. You get one ratio, here's another, here's another, and you get a distribution of ratios.

But that informs you, you know, what's the lowest, what's the highest. That's how I think about it.

Could there be another one that's even higher that you didn't measure? I think there could.

But if you do a distribution, you actually allow for a tail to go on up beyond what you actually measured.

DR. MAURO: I see what you're saying.

So, yes, in other words, the fact that every circumstance --

MEMBER ZIEMER: We sampled the workplace of --

DR. MAURO: There's an unlimited number of situations.

MEMBER ZIEMER: Right. An unlimited number of ratios.

DR. MAURO: Ratios.

MEMBER ZIEMER: We have sampled them throughout the workplace. And from that we build the distribution, which is not unlike what we do in other cases where we've sampled the workplace.

That's one way to think about it.

DR. MAURO: What you just explained to me, I was struggling with this and I had that silly relation here, but I understand the difference now the way you just described it.

There is a relationship, but it's not -- we don't -- we don't -- in any given circumstance, we don't know what that relationship is.

But we do know that when we measured it, we got thousands -- I don't know how many. Thousands of them. And you know that it was never really higher than this, which might represent the worst circumstance where you've --

MEMBER ZIEMER: Or at least you have a picture of the distribution no more than eight point or two or -- you've got lots of points and you get a distribution.

DR. MAURO: Right.

DR. ULSH: And there's one more important point to build on the picture that

Paul just painted.

What we've got is a sample. So, we've measured the N/P ratios at certain points, and that's a sample of what actually exists in the workplace.

But the point that I would make is that's not a random sample in any sense of the word.

In fact, we would have picked the points that would have been the worst where the neutron field is the highest. Those are the points that we would have non-randomly selected to measure.

So, when we're talking about this distribution that we've built, we've got a biased representation high. It's claimant favorable to do that.

DR. BUCHANAN: This is Ron Buchanan, SC&A.

Okay. I think the problem comes in as when we use this data at the assigned dose, by definition, we are saying to the worker that there is a correlation between your photon and your neutron dose we're going to assign by using that method.

Yet, on the other hand, the data we're using does not correlate it, and so that's where the rub comes in. We're kind of talking out of both sides of our mouths.

It's not correlated, but we're going to use that data. And we're telling the worker this is correlated, we're going to assign you this dose.

DR. ULSH: I understand exactly what you're saying, Ron, and I've been thinking about it while we've been talking here.

And I think if what we were trying to do is to provide a best estimate, a most accurate estimate of the dose, we might have a problem because there's no - we don't know what the correlation might be if there is one. However, that's not what we're trying to do here.

In terms of an SEC discussion, what

we're trying to do is put an upper bound on it.

So, when you take that already biased population of samples that we have and we pick some high percentile value, whatever we choose to pick, what we're saying is we don't really know what your neutron dose was. It's somewhere between zero and this upper limit that we're establishing. That's what we're saying.

But we're not trying to say we're going to use the neutrons or the gamma dose and that is a reliable predictor of the exact number that your neutron dose was.

I think there we would have a problem, because there's an unknown correlation, if any.

DR. NETON: Actually, I think what we're trying to say is we don't know what your neutron/photon ratio is. We don't know where you actually work. So, we're going to assign you the highest neutron/photon ratio for a person who did a job where it was the greatest or at the higher end of the field. That's all we're really saying.

DR. ULSH: And keep in mind here, I mean, the people that we're talking about assigning this to with maybe some exceptions, I don't know, but by and large, these are the people with low exposures, the people whose badges weren't read because they didn't go in there five days a week and work and then their gamma badge exceeded that threshold. These are the people who went in, did a quick job, came out.

So, when we're using the most exposed workers to bound our dose, there's another claimant favorable factor built in.

CHAIR BEACH: When you say they did a quick job, you're talking about the workers that didn't have badges that were assigned to that building for a job.

DR. ULSH: Yes.

CHAIR BEACH: And it could be a

quick job or it could be an evolution of a job, a day, two days.

DR. ULSH: Yes.

CHAIR BEACH: So, you have to kind of put it in terms of a quick job is not just always in and out.

DR. ULSH: I agree.

MEMBER ZIEMER: But they were not permanently assigned in that area.

DR. ULSH: Correct.

CHAIR BEACH: Right.

MEMBER ZIEMER: It was a temporary job.

DR. ULSH: Discrete, generally short term, which I would define as, you know, I don't know, a week or less. You might be able to find one longer. I don't know. But not guys that worked up there for quarters at a time.

DR. BUCHANAN: Now, tell me again why -- I mean, just intuitively I would like -- I guess if I was doing this, I would want to look at the NTA data since you have that data, and compare it to the top dose you assign using N/P ratios.

Why isn't the NTA data readily accessible to do a coworker dose model?

DR. ULSH: Okay. This is really going back into the memory banks here, Ron.

I think it's because prior to -- okay. The data that we have readily available is, for instance, what's in the MESH database.

The problem with the MESH database in this particular instance, is that prior to a certain date we don't have cycle-by-cycle NTA badge reads paired with cycle-by-cycle gamma badge reads. I think what we've got is annual totals.

DR. BUCHANAN: Okay. But that number from each individual worker paired data, because you had used paired data from individual workers on an annual basis, so you had a neutron number and you had a gamma number.

I can't -- and I know that it's probably a simplistic look at it, but it looked like it would be fairly simple to go back and take that neutron data and just for each year do a distribution on it and see how that compares.

I guess it would be more comfortable to say, okay, we agree with what you're saying there if we knew the neutron data didn't say, hey, this isn't right, you know.

DR. ULSH: Bob Morris, do you have a more clear recollection of the data that's available?

MR. MORRIS: Sure. I've got something to add here.

If you go back to look at Table 6-1 which lists the categorical data by month or by year or quarter that's available, you'll see, for example, that in March of 1954, second quarter of 1954, there were 225 badges or films read that were in the range from zero to 100 millirem intervals. Nine that were 100 to 300. And zero that were more than 300.

So the point of that is, is that when you actually take that and you re-image that in the annualized roll-up of the MESH data and you would drop, say, you know, how many by every month, you're dominated in almost every case by an annual roll-up by the zeros. And so all you're reporting is the missed dose for neutrons versus the gamma dose that was measured.

The missed dose dominates the roll-up data, and that's why we didn't successfully find a way to use it. It's not very informative.

DR. BUCHANAN: Okay. This is Ron Buchanan.

On Table 4-4 we list the N gamma matched pairs. I assume Column 2 in there, say 1954, is the -- we have 32 matched pair that --

MR. MORRIS: Let me catch up with

you. I'm not on that page yet.

DR. BUCHANAN: Okay. Table 4-4.

DR. ULSH: Page 21.

DR. BUCHANAN: Page 21. 1954 N, we have 32 matched pairs which we used to do the N/P value.

Why can't we look at the NTA film values for neutrons for `54?

You have the same absolute data there that you used to derive the N/P values above a certain threshold. Say 50 millirem, I think.

So, that data ought to be as valid to create a coworker model as to determine the N/P value.

DR. ULSH: So, Ron, are you saying -- let me see if I can accurately summarize what you're getting at.

For the example that you used, 1954 where there are 32, I guess -- I don't know if those are people or film badges.

DR. BUCHANAN: Matched pairs, the

way I understood it.

DR. ULSH: Okay. Instead of using those 32 numbers to generate an N/P ratio, why don't we use those 32 numbers to generate a neutron coworker data?

Is that what you're saying?

DR. BUCHANAN: Yes, that's what I'm saying.

DR. ULSH: Well, Bob, do you see an issue with that?

MR. MORRIS: Well, I don't -- I mean we certainly can do our arithmetic, but I -- what I said before I think still applies, is that our data is going to be dominated by missed dose.

DR. ULSH: So let's say, Ron, for example, let's say we agree to do this. And we came back to you and we said that for 30 of those 32 badges, they were less than the LOD.

DR. BUCHANAN: Okay. Well, let's clarify something.

I was thinking, and maybe I'm

thinking about another site, but I was thinking that these had to qualify to appear on Table 4-4. They had to be greater than 50 millirem.

Is that not correct?

MR. MORRIS: I think you're right in that case, Ron.

DR. BUCHANAN: So, therefore, we do have data that is at the LOD value or greater.

DR. ULSH: Yes, I see what you're saying. Yes.

DR. BUCHANAN: And so I guess before we say, okay, the N over P value is what it is and it sets the upper limit, I would like to see that verified by looking at the NTA data for each year by itself as a coworker model, and see if they're out of line, you know.

Perhaps we'll find that the NTA data would provide a lower dose.

DR. NETON: It seems a way to get past the hurdle that we talked about earlier though, which is this fading issue, right?

DR. BUCHANAN: Yes, you have to correct it for all that.

DR. NETON: Right. And we have to -- it would seem that we need to solve the first -- or convince people that the first problem is solvable. Otherwise --

(Simultaneous speaking.)

DR. MAURO: But I mean after that first hurdle, the fading hurdles, what we're really saying is we have a lot of options in front of us dealing with the problem.

We would have certain preferences on how to come at it that -- some which we think are not as strong as other strategies, but they're all tractable once you solve the fading problem.

DR. ULSH: So, I think if we can perhaps just set aside our disagreement on the suitability of the categorical data, let's just for the moment say we agree to disagree on that.

And then what we could do then is

go back to the data that's reflected in this Table 4-4, however many film badges there are, and generate distribution by year, neutron coworker type data, and then we can bring that back to you.

CHAIR BEACH: Okay. So, you would actually look at it.

Did you want to have access to it as well or would you rather have --

DR. ULSH: Well, I mean we can -- we'll generate it. And then of course it will go to somebody to review.

CHAIR BEACH: Gotcha.

DR. NETON It's got to be reviewed.

DR. MAURO: The rock we're going to stand on though is the neutron -- the first problem -- in other words, to go through this exercise before we solve the fading problems, it's sort of a waste of time.

DR. ULSH: Well, yes.

DR. MAURO: That's solved, and then after that, then it becomes an entire, as far as I'm concerned, an SEC issue -- Site Profile issues on how do you best come at the problem that's most claimant favorable and scientifically sound.

But until that first piece is put in place --

DR. ULSH: Right. So if there were, for instance, the Working Group was not convinced of the reliability of film badges and on that basis recommended an SEC, the full Board agrees and it becomes an SEC, then there's no point even -- well, actually --

DR. MAURO: Well, eventually there is because for the non-covered cancers.

DR. ULSH: But no, if this doesn't come back, we can't do it.

DR. MAURO: We could reconstruct any neutron dose.

DR. BUCHANAN: You can't use NTA film.

DR. ULSH: All right. Well, Josie, I don't want to step on your toes here. It sounds to me like we need a graded approach and do fading first.

If we can get past that, then maybe we do this.

CHAIR BEACH: Okay. Okay. Because I was going to ask you to rewrite, but we'll start with the fading. I agree with that.

DR. NETON: I think that makes sense.

DR. BUCHANAN: That's fine.

DR. NETON: In fact, doesn't our original model also rely on the fading issue to be resolved?

Because we've corrected for- it anyway, so --

(Simultaneous speaking.)

CHAIR BEACH: But also to come back to that in the essence of time would not -- wouldn't it be wise to just go ahead and look at that data so that we're not --

MR. KATZ: Well, I mean if they first address the fading, once you address that, if you feel confident in that, then you just go ahead with addressing the second part, right?

DR. ULSH: Right.

MR KATZ: I mean you don't want to wait another Work Group meeting before you address the second part.

CHAIR BEACH: Right. That's what I was worried about.

DR. ULSH: Well, let me present another scenario to you.

We come back to you with a piece on fading and the stars align, and you all agree with us fading is no longer an issue, our issues have been satisfied.

At that point, even though we haven't done this second analysis that you're talking about, as John suggested, it's just a matter of crunching the numbers.

Maybe we'll have some discussions on our numbers a little higher than yours, but could we agree that that's most likely a TBD-type issue and though it needs to be done for the purpose of an SEC decision, you guys would be able to make an informed decision on that part of it?

DR. MAURO: I would agree with that.

Now, if Mark was here, he would say I approve in principle. And in my mind, I agree.

You solve that fading problem, and then it becomes a matter of what I call a classic Site Profile issue that needs to be resolved.

DR. NETON: Right.

DR. MAURO: The degree to which the Work Group wants that issue resolved before they make a recommendation to the full Board, that's the Work Group's call.

DR. NETON: I also think we should consider in fact, though, this N/P ratio thing.

I think we're in agreement that there's not an absolute requirement that we correlate. That kind of goes away.

I think a lot of the --

DR. MAURO: I have to say, I listen to it from the way you both describe it to me, and I have to say I'm inclined to agree.

DR. BUCHANAN: Unless -- one reservation there is that if we come back with NTA data and it shows a completely different picture, and then we still have an issue to resolve, I don't think it will, but it could.

DR. MAURO: I mean what happens then?

So, what you're saying is that you have two different ways to come at the topic. One is dealing with the validated, verified, corrected NTA films and building a coworker model on that basis.

And then from there, theoretically, you could address all issues just from the N from that.

In other words, you don't have to go to your categorical data. You don't have to go to your neutron/photon issue. In theory, you can go straight to there.

Ron points out, all right, let's say the question then becomes, well, let's take a look and compare the difference that you would come up with.

And if I were to use the neutron/photon approach as opposed to let's say some other strategy, one might be more -- now we're, you know, which is the one that is really more claimant favorable.

In light of everything, all considered, all factors considered, which approach do you think is in the best interest of the Work Group to try to reconstruct his dose, his neutron dose?

But I would say that question is a Site Profile issue.

DR. NETON: Right. I mean you could evaluate both. And both are options on the table. We could evaluate both and pick one which makes the most technical sense or the most claimant favorable.

DR. MAURO: The best that will work for the claimant, yes.

CHAIR BEACH: What does the rest of the Work Group think?

Just get to the fading first and --

MEMBER ZIEMER: I think you have to.

CHAIR BEACH: Okay.

MEMBER CLAWSON: Yes, the fading issue's got to be taken care of before --

CHAIR BEACH: Okay. Makes sense.

Are we ready to move on or are there any other lingering issues for neutrons?

MR. FITZGERALD: This is a two-part action, just to clarify, that the fading analysis provided for the Work Group to examine or SC&A examine. And then, if that's a meeting, but certainly maybe a call or something so we have a juncture where we can move forward.

I mean this is not going to be staged for each Work Group meeting.

DR. MAURO: When I mentioned that before I said, listen, before we move on to the second phase --

MR. FITZGERALD: Right.

DR. MAURO: Certainly the first phase -- now, whether or not you want to, you know, you want to schedule Work Groups, but I mean to me that's the sequence --

MR. FITZGERALD: Yes.

DR. MAURO: Moving through that process, you know, but let's get that first.

Then the sooner we can see your fading issue White Paper and that you feel comfortable that you've got your handle on it, you know, I think then we're standing on very solid ground and you may want to move immediately forward for evaluating.

MR. FITZGERALD: Yes, I was going to say from a process standpoint the Work Group may want to consider a technical call or something just to --

DR. MAURO: Yes.

MR. FITZGERALD: Keep the momentum going.

DR. MAURO: Yes, you don't want to --

MR. FITZGERALD: The only concern would be a two-part thing and --

MR. KATZ: Yes, except that, if the Work Group is -- it's more in the technical call, if the Work Group is going to actually make a judgment about the fading piece. Then that's actually what --

MR. FITZGERALD: That's a Work Group meeting.

MR. KATZ: That's a Work Group meeting, but -- so -- that's why I said if DCAS is confident in their fading White Paper, I mean they could go ahead and knock the other thing off too before you have a Work Group meeting.

MR. HINNEFELD: I think that should be our planned position here because there could be scheduling difficulties in getting a

Work Group together in a timely fashion.

And if we are comfortable with the fading, then we can proceed on with the analysis and talk about here this coworker part without -- and if the Working group then later on decides that, you know, this fading thing isn't convincing and that falls apart, well, so we spent some effort, but whatever.

I mean it's timely for the claimant. It's more timely for the claimant to keep the work going.

CHAIR BEACH: Okay. So, everybody clear there?

The next issue on the table is tritium compounds.

MR. KATZ: Josie, it's ten to 12:00. What's your ballpark? What do you want to --

MEMBER CLAWSON: Well, now that we got the easy one out of the way.

MR. FITZGERALD: That wasn't supposed to go all morning. Yes, that's a consideration. This could take an hour, hour and a half.

CHAIR BEACH: That's a good point.

MR. KATZ: Want to have an early lunch and then take it on all at once?

CHAIR BEACH: Yes, let's do that. Let's do that.

MR. KATZ: Some blood sugar.

CHAIR BEACH: Okay. Let's take lunch then.

MR. KATZ: Okay. So, it's ten to 12:00. So, certainly by 1:00, right, we --

CHAIR BEACH: 10 to 1:00.

MR. KATZ: 10 to 1:00?

CHAIR BEACH: Yes.

MR. KATZ: We'll reconvene, for folks on the phone. Thank you.

(Whereupon, the above-entitled matter went off the record at 11:47 a.m. and resumed at 12:55 p.m.)

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

12:55 p.m.

MR. KATZ: So, good afternoon. We're reconvening after a lunch break. This is Advisory Board on Radiation and Worker Health, the Mound Work Group.

Do we need to check about anybody in particular on the phone?

CHAIR BEACH: No.

MR. KATZ: No. Okay.

CHAIR BEACH: I don't believe so.

Okay. So, right now we have two papers on the table. One that was produced by SC&A, April 15th. It was just after interviews that we did in April. And then NIOSH's paper that's dated in July 2010.

And, Joe, do you want to kick off the topic of stable tritium compounds?

MR. FITZGERALD: Yes. Just a little

history.

At the last meeting -- actually, a couple of different meetings, SC&A expressed some concerns over the NIOSH approach that was arrived at over the last -- I guess it's been eight, nine, ten months where it was proposed, claimed, whatever, that the operations at Mound that handle the -- and I'm going to talk hafnium tritide because I think there has been some confusion in the past.

We want to make sure that we're focused on hafnium as the insoluble -- the more insoluble compound that has figured in a lot of our discussions.

And for hafnium tritide I think the position that we had some concern over was that this compound was handled in a discrete, controlled operation wherein, you know, there was a potential for exposure to ten workers that, in fact, could be identified by name. And the exposure potential of -- meaningful exposure potential is limited to those ten.

And we expressed concern I think going in when we first heard that, that in terms of the basis for that very, you know, again, it was very defined and the assessment was that those were the workers that would, in fact, have the hafnium tritide figure in their dose reconstruction.

I'm not going to go through the entire history of some of the questions regarding how you dose assess with the insoluble tritide. We could do that, but I think we've spent a lot of time doing this. I want to focus in on that issue.

Because we thought, at that time and discussed it with the Work Group that, you know, this is an issue we should be able to get to ground truth, get to the facts because really the operational information surrounding the handling of hafnium tritide should be available.

Now, I would add, that should be available in the classified information that would be available through interviews and through documentation from the site because of the nature of these operations.

So, I think what we proposed at one or two meetings was that there would be a concerted effort to try to validate some very specific questions; the exposure potential that might have existed from operations, the operations themselves that took place at Mound, historically, and in fact the workers who may have been, you know, potentially exposed to hafnium tritide in operations, and to conduct the interviews and look at the documentation and, just again, let the chips fall where they may rather than sort of have this question of can you or can't you apply it to these ten named individuals and this very discrete operation.

And from there we scheduled -- and this was done actually in collaboration with the Work Group and NIOSH so that sort of everybody who had a clearance could be involved. And we scheduled a series of on-site records reviews at OSTI where a lot of documentation in fact resided.

Made two trips to OSTI. I think, Brant, I think you did a separate trip. So, there might have been a series of trips.

Scheduled interviews over a couple days with individuals that were associated with the tritium program. And had a couple of secure meetings amongst ourselves in Livermore and Germantown.

And we spent, again, considerable time pouring over the available records at OSTI. We looked at -- interviewed these former Mound workers and tried to glean from them descriptions of the operations and what they could tell us in terms of these exposure potentials, and discussed all that in these meetings.

As I recall, at least three of the Work Group members were present for both the interviews and these discussions. That was

Josie, Brad and Phil. And I think again Brant was with us and myself and Kathy Robertson-DeMers.

And essentially, the objective was to get us all on the same page. I mean I think the objective was to clarify the operational experience and to really get a handle on what these exposure potentials were and if, in fact, the individuals exposed were these ten individuals that were postulated by the NIOSH position.

And we finished this in April. And I drafted the summary that I submitted to the Work Group essentially defining pretty much what I thought this review had left us, had that cleared by DOE. And of course we, this past Friday, received the critique of that position from NIOSH.

Now, we hadn't had a lot of time with the response, but I'm just saying that we do now have the response.

I'm going to just basically say,

based on what we reviewed, collectively reviewed in terms of the classified database, that we frankly feel that our concerns were validated, that we do have concerns that there were in fact more individuals exposed to hafnium tritide than the ten that were cited in the NIOSH position.

And that the individuals aren't necessarily nameable. And that we take exception to the premise that in fact this was a discrete operation that one could confine the issue to.

And that's pretty much what I can say about it. I think the rest of it I would defer, but certainly in this case the Work Group members were present for all this data capture and all the discussions that ensued afterwards.

So in a way, they were witness and party to what was found. So, I don't see this as so much trying to inform or provide an analysis as to just walk this thing down as far as what we did.

But again I think the Work Group is in a perfectly good position to judge what was found in that classified review. And I'm going to leave it at that.

MR. KATZ: But, Joe, you have two Board Members who weren't participants in any of that discovery.

So, it would probably be helpful for them to hear rather than relying on the other three Board Members.

MR. FITZGERALD: Yes. Well, I think that's the reason I wrote up the position paper the week after we finished, was to capture what I felt could be said and have that cleared by DOE and distributed to the entire Work Group.

Obviously it wasn't so much for the people that were there with me, but for the rest of the Work Group, as well as the Board Members to see.

CHAIR BEACH: Well, and correct me

if I'm wrong. The unclassified notes are available.

MR. FITZGERALD: Yes.

CHAIR BEACH: So, those were available to --

MR. KATZ: Yes, Joe's two-page write-up is --

CHAIR BEACH: And that's available.

MR. FITZGERALD: Yes.

CHAIR BEACH: But I mean just the raw notes, the unclassified version with the whole --

MR. FITZGERALD: Yes, the redacted version of what we got from the interviews themselves of course are available.

So, you know, there's information available to be reviewed on a -- available to uncleared personnel and to the rest of the Work Group.

So, I think that was all we could do, but, you know, knowing the nature of this beast, knowing that some of this information, the details, which are very important to solving this question, are in fact classified.

I think we took the special approach of saying we really need to have as many cleared members of the Work Group firsthand present to hear the feedback from the interviewees, to look at the documents firsthand, and to be party to the discussions that Brant and I had because I think a lot of this becomes more difficult in an open forum.

So, I think there was a reason to do it the way we did. Didn't have everybody, but I think we took some effort to translate what we could into some form that could be reviewed as well.

That's what I think what you were saying is.

CHAIR BEACH: Yes.

MR. FITZGERALD: Any questions on that?

CHAIR BEACH: Brant, what do you --

DR. ULSH: Well, Joe gave you a

pretty good summary of the process involved.

For us, the process, I mean one of the first -- one of the early events was our interviews with former Mound workers about this topic. About the topic of special tritium compounds and specifically hafnium tritide, because hafnium tritide does present some challenges that you don't see with other tritium compounds.

And if you're used to working with tritium and know the issues that are attended with that, you may want to set that aside because hafnium tritide or particulate tritide is a different beast.

Tritium gas tends to be very mobile. It tends to get everywhere. Particulate tritium is different than that. It is not as -- I mean when we called these stable tritium compounds, we were kind of talking about this before how that's kind of an oxymoron.

By "stable," what we mean here is

that the compounds don't break down as readily and they're not as mobile readily as you might be used to thinking of in terms of the tritium gas operation.

So, that presents some challenges to normal tritium programs where it's very easy to detect.

When you're relying on urinalysis to detect tritium intakes, normally tritium gas is very readily detectable in urinalysis.

The problem with hafnium tritide is that it tends to be more stable relative to other tritium compounds. And so it stays in the lungs and doesn't come out as readily in the urine.

Now, we've always contended that it's not zero, but the amount that you see coming out in the urine is much less. So, the dose that you could miss is much higher relative than what we might see with other tritium compounds.

So, we started by interviewing some

former Mound workers. Three of them. One of whom was involved, specifically, in reviewing the urinalysis data that was available for the workers involved in this program and trying to identify which workers might have been exposed. And for those workers, estimating the dose that they might have received from those intakes.

And they identified three workers that were actually exposed based on that urinalysis data. And the highest dose that they estimated for any of them was three rem.

Now, you know, that's a big dose for tritium, but it's not in the realm of implausibly large doses.

We asked those three workers about a number of topics. And to be clear, the position that this was a small, discrete, well-contained operation did not come from NIOSH. It came from the workers that we interviewed who had direct knowledge of this program.

And the list of ten or so workers who were involved in the program came from those worker interviewees, the people that we talked to.

Now, Joe mentioned that we recently had another round of interviews. And for the record, just to be clear, what we're talking about here, we had a round of interviews here in Cincinnati with three of the Work Group members, the three previously mentioned present, and this was a different set of workers.

And these turned out to be the workers who were directly involved hands-on in producing the material and doing what they did with it.

These workers added to our list. They gave us a few more names that weren't on our original list of ten. So, there are more than that and they gave us a few additional names.

We also talked to them about the

exposure potential to other people. And, I don't know, maybe we differ here, but what I heard them say was here are the people who were directly involved, the principals and their support staff. Their technicians that worked directly alongside them were in a different category in terms of exposure potential than anyone else.

They had a realistic exposure potential, but to imply that the exposure potential to other workers who were not directly involved here is completely inaccurate.

This is not everybody on site. It's not even everybody in the buildings where this operation took place. It was limited very specifically.

And we've actually seen documentary evidence down at OSTI that supports what the workers told us that this was limited to -- primary operations were limited to a couple of rooms.

And then of course there were some other activities that happened like, for instance, NMR operations where you go and analyze some samples. But those were always doubly contained and they didn't have an exposure potential.

So, yes, you'll see the presence of this material perhaps in other places, but you've got to really examine whether or not there's an exposure potential.

So, we came away from the interviews and from the documentary evidence largely supporting what the workers had originally told us, although, granted, with a few more names of people to be included on this list.

We have also prepared a document, OTIB-0066, which tells the dose reconstructor how to reconstruct doses from this compound.

SC&A reviewed that document, and by and large came out with the conclusion that it was an appropriate and claimant favorable way to do it, you do need some site-specific information in order to make this work, and we agree with that.

But then SC&A's review of this topic specifically related to Mound, that is hafnium tritide specifically related to Mound, came to by and large, what I at least interpret as the opposite conclusion.

So, I was a little confused by that, but I come away from this whole thing looking at the weight of the evidence, the interviews that were conducted, the documentary evidence, largely in the same place that I came into it.

This was a very small, very well-controlled operation dealing with a material that was considered very precious.

In other words, you aren't going to spread it all around, because each microgram is very valuable.

And this was done in limited access areas. People were not just wandering through getting a snootful. It was very well controlled. And if you didn't have a reason to go into these rooms, it was security padlocked. You could not go in.

And I think the thing that we've also kind of lost sight of is that we're not talking about a typical situation where we would have some concerns where there's not monitoring. There was extensive monitoring.

Urinalysis, as with the other tritium workers, they gave urinalysis once or even twice a week for the workers involved in these operations.

In addition, there was air monitoring, there was swipe data. They worked in bubble suits whenever containment was going to be breached.

We're not talking about the typical little exotic operation where, you know, you may not have bioassay. That's not the case here.

So, I come away from it unconvinced

that what the workers told us was wrong. I come away from it convinced that what they told us was right. I have not seen anything that would contradict it.

So, I guess we just have to agree to disagree on that point.

MEMBER SCHOFIELD: Well, I strongly disagree with you because you may have two workers there, but you got all these port people, painters, welders, pipefitters, tinners, housecleaning come in, in any facility.

Just because you have a CAM alarm over here and maybe it goes off at 5,000 DPM, you have particulate matter that has escaped over here. It can be a million DPM.

Big freakin' deal. That doesn't tell me how much particulate matter has gotten out and gotten where.

The other thing is when those crafts come in, particulate matter gets scattered around. You know that stuff got into oil. You know it got into the hoses and the vac pump. You know it got there. It's going to because it's the nature of the beast.

You go in and start cleaning that up, every bolt, every nut, every pipefitting, every penetration, every place that thing bolted to the wall, to the floor, to each other has that potential and you will find in almost any facility, you are going to find some contamination under there.

So, when you go in and clean an area, I can go through and clean up the floor, have the, you know, find a few big spots. Big deal.

But now when I go in there and D&D, I'm taking every nut, every fitting, everything apart. Now, you've got all this stuff that's been hidden in there for years, weeks, days, months, whatever it is, is now being brought forth and it's going to be in there.

That stuff's not going to be all

nice and confined to that box. There's going to be excursions. It's going to combine with other things. So, it's not the only compound that you need to worry about there.

And those supporting crafts, I would be extremely shocked if they had a small crew that was just dedicated to that. Usually it's not. All the fitters, all the tinners who were cleared, they would go in and out of there as they were needed.

MR. HINNEFELD: Phil, you said at one point talking about a CAM going off over here or something if particulate material got out.

What's the indication of if particulate material was released?

MEMBER SCHOFIELD: Well, a lot of times when you have those, you'll have a CAM alarm go off in one part -- now, this comes from experience -- many times.

MEMBER ZIEMER: But you're not talking about this facility.

MEMBER SCHOFIELD: I am talking about this facility. I'm talking about basically any facility.

MEMBER ZIEMER: But your experience is not at this facility.

MEMBER SCHOFIELD: No, but what I'm saying --

MEMBER ZIEMER: Your experience is at the facility where you worked, and you're extrapolating that experience to this facility.

MEMBER SCHOFIELD: What I'm saying though is that you can have particulate matter that doesn't necessarily become as much airborne, doesn't spread as much. You can have some of it becomes airborne, and some of it may not become airborne.

And that's why a lot of times you can wind up -- these people can wind up with it on their gloves, down on their feet, somewhere where, yes, a CAM alarm does go off. So, an amount of it's going to get airborne.

But in that particular area where they're working whether they've had a torn glove, they've had a hose failure, whatever it is, would allow more particulate matter in that particular -- that's why -- you've been over there. You have hot spots.

MR. HINNEFELD: So, your point then is that the CAM monitoring location is not representative of the work location where somebody might be.

MEMBER SCHOFIELD: Exactly.

MR. HINNEFELD: Okay. That's your point.

MEMBER SCHOFIELD: That's my point.

MR. HINNEFELD: Okay. I still haven't heard the evidence for this material getting out.

I suppose you mean getting out of a glovebox. The particulate material getting out.

I mean there was testimony, if I'm not mistaken -- I wasn't at these meetings because my clearance wasn't there yet. I didn't have my clearance yet and Brant has subsequently briefed me since I got my clearance.

There was discussion about the amount of material; was there not?

MR. FITZGERALD: Well, let me respond to that because that was, you know, there's two elements to this that are very important.

One is what you're raising. Is there an exposure potential for this to get out of the glovebox?

And, you know, the other issue is are workers beyond the ten operators that would have received, you know, the potential for exposure, meaningful exposure.

The first issue, we spent some time on the interviews, and it's all in the unredacted and redacted notes, but we honed in on that and specifically asked, okay, what's the history of tritium releases from the glovebox within the so-called controlled environment?

If we think back to the early days in the tritium operations, well, you know, gloves, skin puncture, you know, you tend to have tritium releases, tritium alarms. That's not an infrequent thing.

And so we asked the same question. What's the history of tritium releases from these gloveboxes in this particular facility?

And the answer is, yes, we've had those. I mean, you know, whether it was once every so often, you know, it's just something that happened.

And my question very specifically is you have hafnium tritide in that box, you know, the alarm is seeing the gases triggering for sure, but is it reasonable to expect that you would have any hafnium tritide leaking out as well?

Now, that wouldn't be picked up, obviously, by the monitor, but it would, you know, there certainly is the potential for that to get out, and the answer was yes.

Now, it wouldn't be considerable, but, yes, there's an undefinable amount. I think the answer was going to be pretty small that would in fact be potentially out there from that leak because it's being handled in the box.

MR. HINNEFELD: Did he say that a small amount probably got out or did he say there was a small probability that some got out?

MR. FITZGERALD: I can't recall the exact words, but it's in the notes.

But in terms of exposure pathways, I think that is the essential question whether, you know, if in fact you're having leakage from a glovebox, could one postulate that you're also having hafnium tritide get out as well?

And I think that was the --

MEMBER ZIEMER: Well, let me ask

this question then, Joe.

MR. FITZGERALD: Yes.

MEMBER ZIEMER: I don't know how much of this is classified. But if you have a glovebox with both tritium gas and the stable stuff in there, you can be sure the tritium gas is going to get out without any leaks in the gloves. It will penetrate.

I mean tritium always does. That's why you double glove on the tritium gas glovebox and it's always coming out.

MR. FITZGERALD: Right.

MEMBER ZIEMER: So, my question really is was -- and are you allowed to say it? Were there actual breaches, accidental breaches in the gloves?

Because the tritide is not going to get through a rubber glove like tritium gas.

MR. FITZGERALD: Right. No, this isn't a permanent build issue. These are just normal events where you have breaches whether it's in the gloves or the attachment of the glove to the glovebox.

MEMBER ZIEMER: Or moving things in and out.

MR. FITZGERALD: Just the kind of normal thing you would have in a tritium facility. This was a very secure room and a very secure glovebox.

Nonetheless, you do have breaches. On occasion the alarm would go off.

MEMBER ZIEMER: Well, but that alarm was seeing --

MR. FITZGERALD: The tritium gas.

MEMBER ZIEMER: Tritium gas.

MR. FITZGERALD: Right. It wasn't able to see --

MEMBER ZIEMER: And I'm wondering whether you would have that without a breach. That's what I'm saying.

MR. FITZGERALD: You know, we couched in the way could you have these releases? And the answer is yes, we did. And the alarms went off.

Now, you know, the question that we were trying to hone in on and the question I think we're talking about here, is what is the potential that hafnium --

MEMBER ZIEMER: There might have been breaches.

MR. FITZGERALD: Was able to get out as well as the tritium.

Now, they weren't monitoring -- or the capability wasn't there technologically to monitor for tritide. So, this was one of these could you in fact have hafnium tritide being released through these breaches?

And they, you know, were --

MR. HINNEFELD: So, he either said, yes, a small amount probably got out or he said there's a small probability that any --

MR. FITZGERALD: Well, I'll have to go back. We got the notes on that.

DR. ULSH: My recollection is he said that there was a very small probability.

When we asked about whether or not

when CAM alarms go off, was it a gas or was it the particulate, the guy that we were talking to kind of -- I asked that question and he kind of looked at me like I was crazy.

He said so you're asking how much dust could have gotten out of a tritium-tight glovebox?

It was very clear that he was saying that anything that would have gotten out would have been the tritium gas. It's far more mobile.

So, I mean of course you can't say that the probability is zero. I mean a scientist is never going to say the probability is zero. But they were clearly trying to indicate that when you're working with this material, it's always accompanied by tritium gas. And that's what you're going to see.

MEMBER ZIEMER: Well, Phil is certainly quite right that particulates get out. And I've seen this firsthand. It doesn't take very much mass --

MEMBER SCHOFIELD: No.

MEMBER ZIEMER: To cover every square inch of a room, floor, ceilings, every surface in every nook and cranny.

I don't know how much mass we're talking about here. Even with a specific activity some of this stuff is -- could come into play.

I mean I suppose if you -- and you probably did some of this in some classified stuff if you're talking about the masses.

But I guess my comfort level is related to the issue of were there actual known incidents of breaches versus the alarm going off which would not in my mind be so surprising if there's tritium gas there.

CHAIR BEACH: There was one improper pass out of a glovebox.

MEMBER ZIEMER: Okay. So that --

CHAIR BEACH: The container wasn't decontaminated. It was on the floor, tracked through the building. So, yes, that was --

MEMBER ZIEMER: Okay. So, there were incidents.

DR. ULSH: There were two known incidents.

MEMBER ZIEMER: Okay.

DR. ULSH: One was the one Josie just mentioned where a storage tree got knocked into and it led to the situation that Josie just described.

Another involved a person who was manufacturing this material and got an uptake. And I really don't want to go into too much more detail, but those are the two known incidents that happened.

The people who were involved in those incidents are on this list. So, when these incidents happened, we have the people and we are going to treat them as if they could have been exposed to hafnium tritide.

MEMBER SCHOFIELD: Do you have the list of people who cleaned up in there?

DR. ULSH: If you recall back in the interviews, we asked specifically the round that we did in Cincinnati, we talked about exactly those people, Phil.

We talked about the people that came in, did the trash. We talked about the support people, the technicians. Not the principals, not the guy who was actually making the material, but the people that were there with them.

And they clearly said that the exposure potential for the principals and their technicians, their support staff, was up here. The exposure potential for anyone else including the trash pickers or whatever, was much lower.

They didn't say zero. They'll never say zero, but clearly in a separate class.

Now, anyone that goes in here is going to be monitored for tritium bioassay.

DR. BISTLINE: This is Bistline

speaking, and I would like to push this further.

And that is that the issue -- the discussion just focused strictly on hafnium tritide, and there are other tritides that were handled.

And I am very concerned about the diffusion and reactivity that we learned in the meeting at Savannah River from scientists that have handled these materials, that diffusion of hydrogen through tritium through the various media does occur as Dr. Ziemer has pointed out. And in the process, there is also some reactivity occurring.

And so anywhere you had tritium, it's not just one glovebox which this hafnium tritide was handled, but there are other locations where tritium was handled throughout the site.

And in these locations, there is the potential for tritides being formed, either organic tritides or metallic tritides, which will persist for years to come.

And D&D may very well be involved, and so workers -- and you know from your discussions and so forth that bioassays are not good for tritide forms, most of the tritide forms.

DR. ULSH: All right. A couple of issues there that I'd like to address.

First of all, when Joe teed this up at the beginning, he specifically differentiated between hafniun tritide and other tritides, and I think for very good reason.

The reason is that hafnium tritide is the least soluble tritide that we know about.

Now, we're not saying in any way that there may be other tritides present at Mound through the processes that you just mentioned and also through the fact that they made these compounds to use.

So, for instance, there was uranium

tritide, there was lithium tritide, there were other tritides. We know that and we're not saying that they weren't present there.

What we're saying is that hafnium tritide is the worst case from the perspective of detecting it in a urinalysis because it's the least soluble tritide that we know about.

So, yes, Bob, I'm not saying that all these things that you just talked about don't lead to the formation of tritides, but those compounds are much more soluble than hafnium tritide.

They're less soluble than tritiated water for sure, and our position has been that for hafnium tritide we know the workers involved.

For these other intermediate solubility compounds from either what they produced or the processes that you described, those are more soluble than hafnium tritide and don't present the same challenges that hafnium tritide do.

DR. BISTLINE: Well, there are a number of tritides that are equally as insoluble as hafnium or very close to it, we've learned from other sites.

And the concern I have is it just doesn't stop with just Mound. We're talking about other sites, DOE sites, a number of them where tritides were handled in fairly significant amounts.

And talking with these people from these sites, we find out that there are other insoluble tritides that are equally or nearly equally as insoluble as hafnium.

DR. ULSH: Well, this is -- I don't want to go into other sites. I've got my hands full with just this one. I'll let other people fight those battles.

For the record, Brad agrees, I think. And there are certainly some other compounds that are to some degree or other insoluble, but I would represent that hafnium tritide is the worst one that we know about that specifically comes into play at Mound.

MR. FITZGERALD: Yes, can I turn this back around?

I do have the notes. This is one of our interviewees responding to this question, exposed potential.

It is very difficult to prove --

MEMBER ZIEMER: You can show these?

MR. FITZGERALD: Yes, this is cleared.

(Laughter.)

MR. FITZGERALD: I think I would get shot first.

It is very difficult to prove a negative. The likelihood of exposure is low. And one in ten to the minus x, for example.

So, he didn't attach a number, but relatively low.

Contamination in your face does not lead to cancer. This would likely not happen undetected.

What I went on to say is, but you

have a potential pathway of exposure to the workers with tritium alarms, you have a potential situation of exposures in particular rooms. However, I also add that it is -- is it remote? I said probably.

Now, the issue I think -- and this is a difficult issue. If you're not monitoring for something, you know, and you're monitoring for tritium, the issue is what's the exposure pathway? What's the probable exposure pathway?

And there were incidents, the two that I think we certainly agree were recorded for tritides getting out and being tracked around and workers being exposed, that did occur.

And what I was trying to get at is on a more routine basis, not the sort of major incidences, but more routine basis you did have these tritium alarms in the tritide handling areas with the gloveboxes.

And as he was pointing out, well,

they weren't, you know, it wasn't being -- the particulates weren't being detectable -- detected, but, you know, what's the probability of those tritides getting out along with the tritium?

His answer was a low probability. I guess that was what you're looking for. But certainly not zero and certainly the exposure potential would have existed.

Now, the question of how much would have been out, how much would have been available for exposure, that's not answerable. That's also what he was saying. That's sort of his proving a negative standpoint.

But our issue was, okay, if you have an exposure potential as acknowledged in the -- I think in Brant's piece of, you know, you got ten workers, the operators themselves, who were acknowledged as having exposure potential, my concern from the very beginning is that we all know that in a typical DOE operation the operators themselves are just sort of the tip. There's sort of a hierarchy.

You have a diverse support staff. You have the people that go in and change the filters. You have the people that maintain the gloveboxes that go in and, you know, the rad techs. You have the people that do the maintenance, I mean the electricians, I mean all the people that keep things running.

And my concern all along was what about those people?

I mean are we saying that the exposure potential of those individuals going into this operating area was essentially negligible, that there was no exposure potential for those workers that were routinely having access to this area or not?

And we spent a lot of time talking about that both in the interviews, as well as amongst ourselves saying that we just had difficulty buying into the proposition that it was just these ten workers.

And the reason that number came up

was a -- and I think Brant was acknowledging this, was the interview with the sort of manager or the key principal people involved.

And if you ask operators who, you know, who are the people that are potentially exposed, they're more -- and this is again just based on my experience, they're likely to name their colleagues. These are the people that are operating and would be potentially exposed.

I think the notion of identifying all these support folks probably wouldn't come to their mind. They wouldn't think of the maintenance guy that comes in and fixes the glovebox or maintains the glovebox. That's not something that would come directly to the mind.

So, I think the number ten represents a sincere estimation on the part of the operating manager or staff as to, you know, who counted in terms of exposure potential.

But I think out reservation is that that is not the complete worker cohort that would have been implicated in any exposure potential in the facility.

The other thing I might want to add is -- and this is something that went back, I think, a little further back. I'm concerned, and have been concerned, that the discrete operation that Brant has referred to, it wasn't the extent of hafnium tritide handling.

I identified in an earlier piece that you have recycle operations, QA operations, you know, Mound was involved. And Brant and I both spent a lot of time looking at documentation on those operations.

So, I think the cohort of workers involved are not just the workers that were associated with this one discrete unit that has been referred to, but there was other units of activity that involve workers that we just don't know who those workers were and nor do we have a good fix on exposure potential.

And again I think that's an uncertainty that sort of begs the question as to we're trying to draw a line around a very defined set of operations and a very defined set of workers.

MEMBER ZIEMER: Joe, are you referring to other Work Groups outside the support people?

MR. FITZGERALD: No, I'm referring to other activities besides the one discrete operation that has figured in the --

MEMBER ZIEMER: That would be using hafnium?

MR. FITZGERALD: Yes, that would be handling hafnium. That's as far as I can go.

MEMBER ZIEMER: All right. But if that were the case, why wouldn't we know who those were?

MR. FITZGERALD: That's my question. It's difficult, you know, again it's --

MEMBER ZIEMER: An operation somewhere else in the facility?

MR. FITZGERALD: Well, to specifically give you an example is we looked at QA activities, we looked at recycling operations and certainly they have figured at Mound historically. And the question is, who are those workers and what were the potentials there?

And we did spend time looking at those, but again it just becomes difficult to identify those.

MEMBER ZIEMER: Well, do we know something about the movement?

Somebody orders this stuff, it comes into the facility and there's some -- it goes somewhere.

Do we know anything about --

DR. ULSH: Yes.

MR. FITZGERALD: We may know too much and that's why we're hesitating, Paul.

MEMBER ZIEMER: I'll ask the question. If it's not answerable here --

DR. ULSH: I can enter the picture.

MEMBER ZIEMER: But it does make it a little tough. And this generically is a problem we'll face probably in places like Pantex where not all Board Members are privy to all the information and they have to make a decision on something.

DR. ULSH: I can help. I think I can help.

This material in terms of the program that we've been talking about was manufactured at Mound. So, it didn't come from somewhere else. It was manufactured at Mound by the people that we've been talking about.

MEMBER ZIEMER: By these people.

DR. ULSH: Yes.

Now, there was -- we did spend some time talking about QA work. And specifically in our Livermore meeting, we talked about what was involved with that.

And my position was we walked through exactly what happened and examined the exposure potential. And for reasons that I presented at that time, I don't feel that there was a real exposure potential from those activities.

Now, quite separate from those first two things, they also operated -- Mound also operated a tritium recovery facility where they ran compounds -- tritium-bearing compounds through this facility to reclaim tritium.

And Joe and I -- the whole -- well, not the whole, but one of the main purposes for one of our trips down to OSTI was to get some more details on this system. And we did find information on an instance when this material was run through the tritium recovery facility.

MEMBER ZIEMER: Where somebody handled it then.

DR. ULSH: Yes.

Now, the thing is the guy who was in charge of that tritium recovery facility is the same guy who was involved earlier in the hafnium tritide production operations.

He then moved on over to the tritium recovery facility. So, he's on the list.

And for that one instance that we know about when this material ran through the tritium recovery facility, there are no incident reports that we're aware of, he was not aware of any incident related to that.

Keep in mind what they do in a tritium recovery facility. You take, let's say, a can of hafnium tritide. The first thing you do is heat it up to drive off the tritium.

And at that point you've got tritium gas, far more mobile, it might set off the CAM if it got out, but it's not hafnium tritide anymore. It's not particulate tritium.

So, I think we know what was done with hafnium tritide at Mound.

DR. NETON: I'd like to just say something. Maybe muddy the waters here.

(Laughter.)

MEMBER ZIEMER: We need somebody to do that.

DR. NETON: I just got an observation and it may or may not be of relevance, but it's something that strikes me of importance.

And the fact is that Mound now has an SEC Class, had it through 1980, based on radon exposure in the very same building, I believe, where the operation occurred.

And in fact the same people will be called, because the Class Definition is defined as anyone who left a single tritium sample up through 1980.

So, all the workers that we've just been talking about through 1980 are essentially members of that Class.

So, you know, does that have any bearing on this discussion only to the extent that if one comes to the conclusion now that tritide exposures cannot be reconstructed. They no longer have any recourse for reconstruction, partial dose reconstruction.

Again, it may or may not be of relevance, but it may help bracket the discussion somewhat because, again, all the workers through 1980 at least are covered.

MR. FITZGERALD: We're talking post 1980.

DR. NETON: Wait a minute. I thought these activities that we were talking about occurred prior to 1980.

DR. ULSH: An important thing to keep in mind here is that the period of active work with this compound is entirely encompassed by the Class that Jim just mentioned.

Now, I want to mention what I'm not saying here. I'm not saying that there was no hafnium tritide on site at Mound after 1980. I'm not saying that at all.

They did have archive samples, for example. But the period of active work, the program that involved this material was concluded by 1980.

DR. NETON: This includes like the glovebox operations where the CAMs went off and the incidents occurred?

DR. ULSH: Well, that system certainly operated beyond 1980, but not with hafnium tritide.

DR. NETON: Right. That's what I'm saying.

So, a lot of the issues that we've been discussing about the worker testimony and what happened and such really is prior to the existence of this Class. It's included in the Class that's already been defined.

And I'm not saying that there aren't issues after 1980, but it seems like one might want to focus the discussion more on workers that aren't covered than the ones that already are.

MEMBER CLAWSON: Let me clarify something, Brant. This is Brad talking.

That facility where the hafnium tritides were worked with continued on past 1980.

DR. ULSH: Now, wait a minute.

Are you talking about the tritium recovery facility or are you talking about the production operations?

MEMBER CLAWSON: No, I'm talking about the production operations.

DR. ULSH: Yes.

MEMBER CLAWSON: And was it all cleaned out and everything was all good, everything was wonderful?

Because we never found that out and that tritium was in everything that they had in that.

As we found at Mound, they would start into a process, they would work it, they would walk away from it, people would come in with another project and it would resurrect, to say, the dead from the past.

Because we have seen it in the D&D era and everything else where they've given it a clean bill of health and start tearing it apart, and all these old processes would come back to life because there is still residual there.

MEMBER SCHOFIELD: I would venture to say that there is -- in all probability, there is build-up anyplace you had a penetration for a glovebox, the window sills, the gloves, whatever seals they were using in there. There were penetrations for electrical penetrations, any mechanical penetrations.

And then what I do know, you're going to have some back pumps there in the system. Those I can guarantee are going to be -- somebody had to take care of those. Somebody had to maintain those.

And you have build-up where those seals are, you have build-up in those pumps, you have build-ups in those hoses, and most materials have an account balance.

You got XY, you know, so much coming in one end, and then you have so much final product go out the other end.

I would venture to guess there was some material that didn't make it from A to B. Now, whether that's extreme minute quality, I don't know. I don't know how well their operation was done.

DR. ULSH: I can address first of all Brad's points, and then yours, Phil.

Brad, I think your question dealt with once the activities in this program were concluded and they moved on, were these same facilities, did they continue to be used?

And the answer is yes because it is -- they moved on to other compounds. So, yes, they did. However, let me just say that purity was important.

You couldn't tolerate a lot of contamination here. And certainly they cleaned up, decontaminated and moved on.

And then their next product, you can't tolerate contamination with hafnium tritide. That's just not the nature of what they were doing.

Now, we also asked specifically, I asked during the first round of interviews that we conducted with the workers, and then I can't recall if the interviews that we conducted in Cincinnati also dealt with D&D, because I think you mentioned that as well.

CHAIR BEACH: Yes, we did.

DR. ULSH: We specifically asked about the potential for D&D workers to be exposed when they years later went in and demolished this building.

And the response that we got was keep in mind this is particulate tritium and these systems were exhausted with a hundred cubic feet per second, I think is the number that he used. If it was respirable, it was sucked out the pipe and gone.

Now, if it's non-respirable, gets

caught in a bend in a pipe, we don't have a problem. It's non-respirable.

Furthermore, they elaborated that D&D from this operation was a little bit different than what might be typical, go in with the bulldozer and knock down the building.

Because of security concerns, they had to D&D the equipment that was used in this operation, and that was performed by laboratory personnel before it was ever turned over to D&D workers, to make sure that this compound wasn't present not so much from a dosimetric hazard standpoint, but from a security standpoint.

MEMBER CLAWSON: When you say "laboratory personnel," who are you saying?

DR. ULSH: I'm saying --

MEMBER CLAWSON: These ten people?

DR. ULSH: Yes.

MEMBER CLAWSON: Okay. If you remember right, on the interview we asked them so you're telling us that you're the only ones that really got involved? Yes.

Then we asked who took care of your instrumentation? Well, that was the instrument tech.

Who changed out your glass? Oh, well, these people did.

Well, who changed out all of this? Well, there's other people, but they couldn't, you know, it wasn't a part of it.

He was focused on those ten, but he forgets that's just the tip of the iceberg and the rest of it that is sitting underneath the water is the one we're worried about.

The support personnel that came in and did this, the union people that were in there had it very cut and dry and he made it very clear why he was upset, because they did come in and they had certain things that they had to be able to do. He couldn't have total control.

There were people there that did

these jobs and I don't think that they were -- this went well past `80, some of the pumps and everything else that were still in there.

Mound had a tendency to when they got done, they walked away. And, granted, the gas part of it and everything else like that was gone, but residual in all the pumps, in the oil, in the drip tubes and everything was there. He did not say when all that was taken care of.

DR. ULSH: Okay. Going back to the first round of interviews that we did, this was early on in the process, not around when you guys were there, the first three workers that NIOSH ORAU interviewed, we specifically asked the worker who was in charge of D&D here about this.

He's the one that told me about, you know, I asked specifically about what about --

MEMBER CLAWSON: These are the ones at the Mound facility?

DR. ULSH: Yes.

MEMBER CLAWSON: I was there.

DR. ULSH: No, no, no, no, no. We interviewed them downtown at the FBI Building the same place that we had the later round of interviews, but you guys weren't there at that point.

MEMBER CLAWSON: Okay.

DR. ULSH: Okay. He also said that they crawled around up there and took swipes. They took swipes looking for this material and they just didn't find it.

Now, you have to understand here that this material was only one small part of the tritides program at Mound. I mean the amount of material was very -- so, I think there will be enormous amounts of dilution involved even if there is anything left.

I'm not saying that you didn't have these categories of workers go in that Phil described earlier and you just mentioned. I'm not saying that.

What they said was any time they had an activity that would involve a breach in containment, they put the material away, everyone was dressed out in bubble suits, they had monitoring going.

These were not people just wandering through that you wouldn't think of that might have been exposed. They were very well aware that they had an issue here and that they needed to take appropriate monitoring procedures.

So, I guess what we're left with, I mean keep in mind that the topic of support workers, the topic of D&D workers was specifically brought up in the interviews when we were talking to the former workers.

I think we're pretty close to agreement with what the workers actually told us.

They didn't say the exposure potential is zero. They said it was very, very low.

Now, we might each have our own interpretations of what that means. I suspect that we do. So, I guess it comes down to do you believe what those workers told us or don't you believe it.

MR. FITZGERALD: Well, you know, it's this question of how low is low, you know. We're not operating with any numbers, any measurements.

What we're operating with is certainly the ten operators are -- figure in those that would be afforded dose reconstruction with hafnium tritide as a component.

And I think what we're saying is that the support workers that would have been potentially exposed, it's not clear that the low exposures that we would attribute to the operators from tritides is that much different than the low exposures we would attribute to the support workers that would have been in and around changing the filters, supporting the glovebox operations.

Now, I think the interviewee was quite correct in the sense that it's kind of hard to prove a negative. And I mean the thing that overshadows everything is of course there were no measurements on the tritides.

So, you were doing it sort of secondhand from the standpoint of what we would surmise as the potential.

DR. ULSH: Well, that's not really true. They took swipes.

MR. FITZGERALD: I'm just saying for the exposure potential for the support workers, we don't have swipes to what they might have been exposed to.

What we're trying to do is surmise would they have been exposed potentially to levels that would be commensurate with the operators.

And all we can say is that, you know, it was small, but it wasn't zero, and it's not clear to what extent they were different from the operators.

I don't think the operators were exposed to considerable amount of tritides either. However, I don't think we can discount the support workers as being that much radically different than the operators.

MEMBER ZIEMER: Well, let me raise an additional question, and I think Phil is quite right.

I would imagine that you would find traces of the tritides in all the penetrations, in the oils, in the greases and all of that.

My question is what's the potential during cleanup of that becoming airborne, because otherwise it's of no consequence.

Some of it, the tritium will be released as gas. That's almost a no never mind. I'm pretty sure if it's in the -- they have floor pumps and diffusion pumps and so on.

MR. FITZGERALD: Yes.

MEMBER ZIEMER: And that pretty well -- the particulate stuff would be pretty well trapped there and it's not an external issue.

So, how do they get that? How do they inhale that, would be my question. Maybe change the --

MR. KATZ: Excuse me. There's someone on the line that should mute their phone if they even intend to be on this line. This is a conference call, Advisory Board on Radiation and Worker Health.

So, if you intend to be on this line, please mute your phone. You can use *6 to mute your phone if you don't have an actual mute button. Thank you.

MEMBER ZIEMER: So, I'm trying to get a feel for whether any of those cleanup operations -- and I think you'd have to grant that there must be -- the tritide must be present at some level in most of this stuff, but does it have the potential of really becoming airborne during those cleanup operations?

CHAIR BEACH: It has the potential during D&D.

MEMBER ZIEMER: That's what I'm asking.

CHAIR BEACH: Cutting up the gloveboxes or the ventilation --

MR. FITZGERALD: Yes, and most instructive were the -- they didn't routinely monitor. They did some swipes occasionally, but didn't routinely monitor for it.

But the two instances where, you know, not only was it released, but it was tracked around --

MEMBER ZIEMER: No, but I'm talking about the later during the cleanup.

MR. FITZGERALD: You mean D&D?

MEMBER ZIEMER: Yes, because you're talking about after `80, and that pushes it into the D&D here now.

CHAIR BEACH: `80 through D&D.

MR. FITZGERALD: Yes.

CHAIR BEACH: From 1980 on.

MEMBER ZIEMER: Okay. Well, in any event -

MR. FITZGERALD: But, yes, when we got into the D&D phase, I think we had similar questions.

We were saying okay, and we were talking about the operators being asked to essentially D&D their own facility whether for security reasons or otherwise.

And our question was, you know, we were trying to imagine these operators doing that and were there techs and were people actually supporting these folks as they, you know, cleaned out this operation?

It would seem to be the case.

MEMBER ZIEMER: Well, I guess it would depend also on how they did the D&D.

MR. FITZGERALD: Right.

MEMBER ZIEMER: When we pulled tritium gloveboxes, we usually got rid of the whole unit and cut it up.

MR. FITZGERALD: Yes.

MEMBER ZIEMER: I mean you sort of said that's not what I'm going to do.

So, what is the potential for airborne?

MR. FITZGERALD: Yes, what is the potential? And that's what we're kind of focused on.

And the other thing is, you know, we touched lightly on the recovery recycle facility, but you have a D&D involved in that too.

And we asked that question and the response was, you know, that would be a fairly substantial D&D for that operation as well.

MEMBER ZIEMER: Well, I think on the recovery, they ought to be able to get a hundred percent of the tritium back on a recovery operation.

I mean are you saying there's residual --

MR. FITZGERALD: Oh, no, in terms of

any residual tritide in the, you know, the recovery itself I think I would agree with Brant. We spent some time on this looking at the machinery and the off-gas system.

MEMBER ZIEMER: Yes.

MR. FITZGERALD: The only opportunity is at the very front end when you're doing transfer box, but that's in a sealed can.

MEMBER ZIEMER: Yes.

MR. FITZGERALD: The sealed can is opened.

MEMBER ZIEMER: Right.

MR. FITZGERALD: So, there isn't a whole lot of potential there, but certainly you have the D&D of that particular facility as well. And that, you know, that wasn't covered other than --

MEMBER ZIEMER: But there the tritides ought to be all gone in that, right? Or are you saying that they might not be?

MR. FITZGERALD: I think we were

trying to eliminate that one and the response if I can find it again -- I just saw it.

MEMBER ZIEMER: Or maybe outside of the machine where they do the heating. Is that the only --

DR. ULSH: Well, again, I mean we were only able to find indications that hafnium tritide went through that system on one occasion.

CHAIR BEACH: Except there was a report that they got back from -- and that was from `77 to `84 and it went through that same recovery. That was reported at one of the interviews. It's noted in here.

MR. FITZGERALD: Yes, the comment was -- this is relating to the recycle facilities. There may have been a significant cleanup effort involved with that. This was from the worker.

DR. NETON: What time frame was that?

MR. FITZGERALD: It doesn't say a

specific time frame. Just that the cleanup for that particular facility -- and it's right here. Actually, it's R-108. The number is right here.

It could have been a significant cleanup. And he was very much one of these folks that was associated with that operation going way back.

So, I'm just saying that it gets a little more complex and it's tied to the activity that took place where it was handled.

So, D&D is one component. And certainly for the operation that Brant's referring to, the operators were the ones that did the initial cleanup.

But again we ask the question, you know, were these the specific people, were there other people that supported those people? I think that was the question.

DR. NETON: It seemed that there would be surveys during the cleanup operation. It sounds to me like if there's activities now after 1980, we're talking about dose received from residual contamination of hafnium tritide. That's what we're talking about now. It's not operations where they're working with the material at this point.

And if they clean this up, I would suspect that there must have been surveys during the cleanup of the operation. I can't imagine --

MEMBER ZIEMER: Letting them know you cleaned up and --

DR. NETON: Well, yes, yes. Exactly. The cleaning it up, you must have some kind of surveys to get some sort of levels.

MEMBER SCHOFIELD: You would have to.

DR. ULSH: We haven't proposed using swipe data for estimating doses to tritides. But certainly during D&D and during operations, Mound had an active program to monitor for contamination by using swipes. It certainly did.

We haven't focused on trying to capture that data because we're not proposing to use it for dose estimation. But, yes, you're right, Jim, I mean they -- an active program.

MEMBER CLAWSON: They did. But also in later years, too, not all people were badged.

The other thing with the swipe program is, is in DOE facilities and a lot like with Mound, paint and other things are wonderful things.

When you start to break that apart, you resurrect the past. And this is what they also found in Mound. And they had several issues where it had been dedicated that it's cleaned, and then they opened it up and resurrected the past.

That's part of the issue that is there.

DR. NETON: I suppose one can

concoct any kind of scenario one wants.

MEMBER CLAWSON: Well, and I know --

DR. NETON: There's contamination survey data and the facility is -- well, we have to look at what they did.

But I mean if they surveyed it and the removable contamination is within a certain level, I mean it's a matter of getting it airborne like Dr. Ziemer was talking about. And once it's there, it sticks.

MR. FITZGERALD: The only cautionary note on that of course is in the late `90s -- this is actually for contemporary defense boards sort of intervened and there was a -- you may recall some of this. There was a real concern over the dosimetry and the monitoring and the -- basically a whole new standard was developed for the air monitoring, sampling, whatever.

And so the historic data has to be seen in that light that reliability --

DR. NETON: One would have to wonder

what techniques were used.

DR. ULSH: Yes, and I need to speak to that too because it's been brought up before, you know, selected quotes from some of these defense board documents that say that urinalysis is inadequate or --

MR. FITZGERALD: No, no. I'm not even going there. I'm just saying that in terms of these techniques like swipes and air samplings, the cautionary note is just be aware that, you know, again historically they were seen as limited and open to question.

DR. ULSH: Yes, but the context in which these techniques are limited is based on the reporting limit that came into force in the 1990s, I believe, where they had to be able to detect a dose of a hundred millirem.

And certainly using urinalysis for a situation where you might be exposed to hafnium tritide, the missed dose for that is higher than a hundred millirem per year. So, they couldn't meet the reporting limit.

That posed a problem to them and they had to figure out a strategy to deal with it.

That's why it came up in the late `90s when they were talking about getting really hot and heavy into the D&D at Mound, because they didn't have a way to detect doses that small from this material if it was there.

DR. NETON: I think, Joe, and also Brant, I think there was some concern about the measurement techniques that were used to see tritides.

MR. FITZGERALD: Yes, separate from the first one.

DR. NETON: You're getting into the issue of self-absorption of tritium particle within the matrix of essentially a metal compound, but there's been some recent research done on that in the last five to seven years.

I think Strong put out an excellent paper on that where they did a Monte Carlo simulation model. And for all intents and purposes, I think it demonstrates the ventilation counters are quite capable of seeing the tritides or the tritium compounds very readily.

MR. FITZGERALD: Well, I think that my only point is if you go back to the survey data, I think you have to be aware of the history of some of these questions that were raised by --

DR. NETON: Well, that certainly goes without saying.

MR. FITZGERALD: Sure. Sure. And then particularly in this case where it was really being scrutinized.

I want to go back because, you know, really this whole thing started with again hafnium tritide and the proposition that we were talking about a discrete facility with ten nameable workers that would have been potentially exposed.

And we spent considerable time

trying to interrogate sort of that proposition because we were concerned about the ability to draw such a firm line around these ten individuals for this very, quote, discrete facility.

And I, you know, this is a little bit hamstrung by the information that we have looked at and we're trying to be careful about it, but I am just not convinced that these ten individuals were the only individuals that were potentially exposed to hafnium tritide during the historic Mound operation involving inhaling hafnium tritide.

And I, you know, there is some equivocal information involved only because there wasn't any direct monitoring.

But in terms of talking with the workers, in terms of looking at the documentation, I think the basis for making that very, very firm claim is weak. And that's basically where I'm coming from.

CHAIR BEACH: And let me ask back in

April, I think April 12th, there were some emails going back and forth on some tritium swipe data that NIOSH was going to look at with Cheryl Kirkwood.

Whatever happened with that?

DR. ULSH: We captured it.

CHAIR BEACH: Anything interesting or --

DR. ULSH: I'm trying to think of the chain of events that led me to request it or I would have captured that data.

CHAIR BEACH: It was after our worker interviews, I know.

DR. ULSH: Yes. And I know and I got an email from Joe, because Joe had the same concern like, hey, why are we getting this data?

I think the reason that I requested it, if I can recall correctly, was that some skepticism about the utility of bioassay data to detect hafnium tritide intakes continued to be expressed by the Working Group and SC&A.

And, therefore, I considered it prudent to go back and capture that swipe data just in case we should have any --

CHAIR BEACH: Tritium survey on swipe data.

DR. ULSH: Well, yes. I mean --

MR. FITZGERALD: Well, there was also boxes being transferred to Morgantown. I think there was some urgency of capturing stuff before it got shipped or something.

Timing wise I think that was kind of of imperative as well.

DR. ULSH: I'm going to be completely transparent about this. I only grabbed it because I thought there was a remote possibility that the Working Group is going to opine that urinalysis data is no good, throw it out, and I didn't want to be standing there empty handed.

So, we've got that data, we captured it, but again we haven't proposed to use it for dose reconstruction. I just wanted to have it.

So to be honest with you, I haven't done a detailed analysis of it.

CHAIR BEACH: Okay. I just remembered that that had happened.

DR. MAURO: What I heard is that it seemed to me that the bulk of the matter is it sounds like that the people that are known to have handled this material may not be the people that had the highest exposures, that I know of.

In other words, you named these ten people and maybe there's a handful of other people that were associated with the operations and maybe the maintenance, but then --

DR. ULSH: No, I don't think --

(Simultaneous speaking.)

DR. MAURO: I'm listening to --

MR. HINNEFELD: Not that they weren't the highest, but there were other non-trivially exposed people.

DR. MAURO: Right, but --

MR. HINNEFELD: That's the argument. I haven't heard anything about these guys not being the highest.

DR. MAURO: Okay. Good. Well, I want to make sure I got that right.

MR. HINNEFELD: Is that right?

(Simultaneous speaking.)

MR. KATZ: One at a time.

MR. HINNEFELD: But the argument pulls either way. If there are other non-trivially exposed people, the argument is the same. It's not that, you know, and I don't know that you would ever talk us out of the fact that the people named -- especially the ones if they were involved in the incidents, I don't know if you'd ever talk us out of the fact that we believe those were the most highly exposed.

But the question here that we have is have you correctly identified all the people who are exposed to the extent that you need to worry about it?

That's what the discussion has been about.

DR. MAURO: My question goes to what Phil was saying before. There is a model that we're building. We have facts that come back from the interviews. Okay. And what's happening is it's almost as if we all agree that there were some undefined number of people that experienced some level of exposure.

And the -- so, now I think that defining who those people are, I don't think we can. Stay with me for a minute.

MEMBER ZIEMER: John, let me correct something. I don't think we've agreed to that.

DR. MAURO: We haven't?

CHAIR BEACH: No.

MEMBER ZIEMER: I've said that I would like to know if there's a potential for inhalation.

I agree that there could have been contamination in other areas and that people had the potential for exposure.

But, in fact, do we know that the tritium was in a form where they could have actually inhaled it?

Was there something about the cleanup operations like were they sawing up gloveboxes and generating aerosol --

DR. MAURO: Well, I think that's the question I was raising.

MEMBER ZIEMER: No.

DR. MAURO: Because I --

MEMBER ZIEMER: I haven't agreed that --

DR. MAURO: Maybe I'm not posing my -- I didn't word my wording right.

Phil explains that there's a lot of activities that go on during D&D, during maintenance --

MEMBER ZIEMER: Right.

DR. MAURO: Of these facilities

where perhaps those people because of the nature of the things that they are doing, actually have a greater potential for inhaling tritium than, let's say, people who are working under very controlled conditions with the glovebox.

I don't know. I guess that's my question because, you know, if you know -- if you could say with a degree of certainty that the people that we know about that were exposed either during an incident or during operations, it's clear and unambiguous that of all the people that might have come in contact with potential airborne sources of tritide, hafnium tritide, these are the people that clearly had the greatest potential for exposure.

Now, what I heard --

MEMBER ZIEMER: If I can interrupt, we sort of agree on one thing.

I think I would agree that potential for exposure may be higher because of what they're doing versus someone working in a glovebox.

What is very different is the source-term. The glovebox person has the mass of the material. The other person has some amount, granted certainly not the -- it may be a millionth of it and still be, you know, worth considering.

So, the potential for inhalation is one thing, but the source-term involved has to be considered too.

DR. MAURO: I agree with that, yes.

See, I just wanted to get a sense whether or not the people that were in the controlled circumstance and the people that were involved perhaps in the cleanup of the spill, which may very well be wearing bubble suits, I don't know, I don't know the details of it, you know, and a lot of whom that you could actually name, which may extend beyond the ten or 11 people that we know about, then there's this other cadre of people that down the road somewhere involved in decontamination/decommissioning may have opened up, re-mediated, whatever they had to do, decommissioned the facility, they're at play also to a certain degree.

Now, if one could argue they're at play, but their potential for exposure to airborne hafnium tritide is really much, much, much less than any of these other people that we know about.

So we have the people we know about, and then we have the people we don't know about.

And I guess in the end, the most important question is, is the people that we don't know about, is it reasonable to assume that they may have gotten exposures that were even greater than the people we know about?

And I think this is a judgment call almost because that's where the rubber meets the road, you know.

If you could say with a degree of

certainty that the people we know about clearly and unambiguously had the potential and actually experienced the exposures that were clearly higher than those that we don't know about because of the nature of -- I don't know. You guys know from your interviews.

Then you could actually say, well, all the people we don't know about, it couldn't have been higher than these guys. We have urine samples. We're going to assume that the urine samples that we have from those people that we know about, we measure these many becquerels per liter, and we know using OTIB-0066 we can convert that to an intake and reconstruct the dose.

And we can -- as a result of that. And we could also say that whatever that dose is to the lung, we know the lung is a limiting organ, that no one is going to have a higher dose than that, including the people that we don't know about.

Now, I think that that's where the

judgment is going to have to be made by the Work Group and then of course eventually by the full Board.

MEMBER ZIEMER: Do we have urine samples on these later cleanup people?

DR. ULSH: If they were involved in D&D in tritium facilities, they were on tritium urinalysis program.

MEMBER CLAWSON: After `80?

DR. ULSH: Yes, even more after `80.

CHAIR BEACH: Well, there is a D&D paper on that, but we haven't actually had time to discuss it.

DR. ULSH: Well, yes. It addresses D&D in general, but --

CHAIR BEACH: It says greater than 90 percent urinalysis report. I don't know if I agree with it, but --

DR. ULSH: Now, with regard to -- I'm losing track of who's raising the points.

We specifically asked in our first round of interviews for the three workers that we talked about, who got the highest exposure to hafnium tritide?

And it was -- right away they said it was that guy who was involved in that first incident early on in the program who was the guy that was making the material, one of the first production runs, I guess, and he got a snootful of hafnium tritide. That guy is the guy that got the highest exposure. So --

DR. MAURO: Was he a three rem guy? You mentioned three rem before.

DR. ULSH: I think so, yes. I think that's the highest guy.

So, John, you're adding an element here that I don't think we've discussed up to this point. And that is what is the exposure potential for, like, D&D workers or other workers relative to the operators.

I can only speak for me, but I haven't heard anyone making the argument that they might have an even higher exposure than the operators.

I think what the argument has been is that while it may not have been higher than the operators, it may still be high enough that we should consider it in dose reconstruction.

Now, I don't endorse that point of view, but that's what I have heard anyway.

MR. FITZGERALD: Well, you know, the first question I think we were grappling with on D&D was, you know, could you even identify it.

I think the -- on the discrete facility you were referring to, you know, the fact that the operators did the first pass was somewhat comforting because you know who they were.

But, you know, I think our question was, was it exclusively them? And there was a little ambiguity about that.

The other question was, you know, in D&Ding the -- and I just mentioned this -- the recycling facility, who did that? I don't think that would probably be operators.

So, you know, there's just those kinds of questions and I don't know if we know what the potential was for D&D workers. That's one reason we didn't really grapple with that so much because if the operators cleaned up the facility to that extent, then the D&Ding of that facility probably would have been -- it would have been negligible. There wouldn't be much left to be exposed to.

DR. ULSH: Okay. Well, for the production-type facilities, we were told that the people who were directly involved in the production operations were responsible --

CHAIR BEACH: He said he hadn't cleared --

DR. ULSH: We're okay. Believe me, I'm not going to say anything I'm not supposed to.

The people who were involved in the production were in charge of cleaning up their own mess, is the way it was put. And they were in charge of cleaning up the facilities and equipment down to clean standards, is the way it was described.

So, they took swipes, looked for contamination. If they found it, they continued to clean it up until that situation no longer existed, and then it was released for general D&D.

Now, with regard to the tritium recovery system, I would almost venture to say that that's not even relevant because this material didn't go through the tritium recovery facility with the exception of one instance that we know about.

And again I bring up the fact that the whole purpose of this facility was thermal decomposition of tritium-bearing compounds. In other words, you heat it up until the tritium comes off.

So, yes, there might have been when the tritium was dissociated, it's driven off, might it have resulted in some fixed contamination? Sure, but that's not a hafnium tritide problem.

MR. FITZGERALD: No, I think the -- excuse me for jumping in here.

I think the issue there is more the D&D side. I think we spent a great deal of time looking at the operation and I think coming to a conclusion that the way it was handled was pretty tight that there would not have been any clear opportunity unless you had a big breach in the off-gassing.

But in the D&D phase of that thing we did raise that specifically. And the one interviewee who had a lot of knowledge of it said, yes, you know, you would definitely be looking at a cleanup of that operation.

And I think it's probably from the standpoint of not only the residual from the one campaign that we were talking about, but also the fact that Mound received, and we heard this as well, returns from other sites.

And I won't go any further than

that, but that does present a question about how much, how often and what was left in the recycling operation after it was all done.

And clearly there's no account that they had operators do an initial cleanup. It might have happened, but --

DR. ULSH: No, I'm not saying that.

MR. FITZGERALD: The D&D of that particular operation would have been, in my view, probably as significant as the D&D in the production operation. I mean I think --

DR. ULSH: As a hafnium tritide issue?

MR. FITZGERALD: Huh?

DR. ULSH: As a hafnium tritide issue?

MR. FITZGERALD: Hafnium and related, you know. The question we asked, were there equally insoluble type of compounds coming from other sites that would have been recycled?

The answer was, yes, there were

others. And I don't want to go any further than that, but I'm just saying that complicates the situation of saying that wasn't one campaign. That was a central recycling operation for the complex.

DR. ULSH: Right. I agree.

MR. FITZGERALD: So, you know, what went through over time was more than, you know, was not only the hafnium, but other compounds that clearly could have had characteristics similar to or approaching hafnium.

So, I think we've got to be careful in just focusing on one campaign. That's one reason we did ask those questions.

DR. BISTLINE: This is Bistline speaking, and I just want to throw one little tidbit in.

And that is one has to be very careful in going too far with the issue of heating up this material and driving off the tritium.

The worst tritium release that we had at the Rocky Flats was exactly that very thing there was heated up and the tritium supposedly was driven off by the people at Livermore and then shipped to Rocky Flats as being a clean piece of material, and it wasn't.

That doesn't drive -- just heating it up doesn't drive off all the tritium usually.

CHAIR BEACH: Thanks, Bob.

MEMBER SCHOFIELD: Let me ask you something quick, Brant, or maybe Joe or one of you could answer. I've got to be careful how I word this.

You have X amount coming in and you have X amount minus one at the other end. If we have an idea of that hold-up in that process, it seems like we should be able to get a rough number.

MR. FITZGERALD: You mean materials balance?

MEMBER SCHOFIELD: Yes, the material balance.

Did you see any numbers like that? No?

MR. FITZGERALD: Even if we did, we couldn't --

MEMBER SCHOFIELD: That I know, but it would just give you a rough idea to think in your mind, you know.

CHAIR BEACH: So, I'd like to wrap this up unless there's just some burning questions or issues that --

MEMBER SCHOFIELD: I was walking on eggshells.

MR. FITZGERALD: Not with a ten-foot pole.

(Laughter.)

DR. MAURO: Is it plausible that there are other people that you don't know about that might have been exposed to hafnium tritide?

I'm not saying how much. Is it

plausible that there might be other people of the nature that Phil asked about that might be exposed to hafnium tritide?

I think the answer has to be yes, from what I'm listening to.

DR. ULSH: Well, if you don't put any conditions on it.

DR. MAURO: I'm not putting -- I'm just saying that -- and now my second question is, is it plausible that those people could have experienced hafnium tritide intakes that were greater than the ones that you do know about?

I mean that's the essence of where we're headed with this thing. And that's going to be a judgment call.

And I guess your judgment is -- I'm almost going to sort of say that I could see where you're going.

Where you're going is that perhaps it is plausible that there are other people of the nature that might have been exposed who you don't -- no one knows.

And the second -- but the other one I'm pretty sure you argue, however, their potential for inhaling hafnium tritide was much lower than the potential for the people you do know about.

Would that be a true statement of your position?

DR. ULSH: Pretty close. I would go a little bit further in some respects.

You asked first of all is it plausible that someone could have been exposed to hafnium tritide other than the ones that -- I'll editorial it -- other than the ones that we've named.

DR. MAURO: Yes.

DR. ULSH: Is it plausible? Yes. I would guarantee it.

If I go to the Mound site today, there is a non-zero probability that I will encounter an atom of hafnium tritide. So, sure.

The problem is you have to consider whether or not if plausible, that they were exposed to hafnium tritide of dosimetric significance.

And my answer is emphatically it's not plausible.

MR. FITZGERALD: And that's where we disagree, because I think the position that we would take and what we have reviewed is that these were not negligible exposure potentials.

DR. ULSH: You're right. We disagree.

MR. FITZGERALD: Yes, we disagree. And that's just central. And that doesn't have anything to do with how much, which is what Stu's point was.

We just don't agree that there was no non-negligible -- is that two negatives -- non-negligible exposures beyond the ten. Based on what we have gleaned from the interviews and the document reviews, that there are more than ten.

In fact, we started collecting names during the interviews, of individuals who clearly were in the facility and rad techs and what have you that clearly would have been doing a lot of operational-type activities in addition to the maintenance people.

So, you know, the question is, is it ten? No, we believe it's not just ten.

DR. ULSH: Well, I agree with you there. We were provided a couple of additional names.

MR. FITZGERALD: But, you know, we could have kept going. The question that we were grappling with was, okay, the ten are clearly the ones involved and everybody agrees they were the operators.

What about Joe Schmo the rad tech? And then we went through an exercise with the operator saying, yes, okay. Yes, that guy supported me, that person supported me. They were in the room, they were rad techs. Okay. So, we started collecting those names.

Then we started talking about, okay, what's the other folks that were, you know, and the list got longer and longer.

So, you know, the point is where do you draw the line as to where it became trivial?

And I'm not sure you can draw a line very easily as to what worker who was in that room would have had a trivial exposure potential.

DR. NETON: What about contamination after 1980 though. It seems to me that the source-term had been put away by then. That's what I've heard.

So, now we're speculating that there were massive amounts, potentially large amounts of contamination left that exposed a large amount of --

MR. FITZGERALD: Yes, but, Jim, let me just stop you there. I agree. There's sort of -- recent events have bifurcated this issue to --

DR. NETON: But that's what we --

MR. FITZGERALD: I know. I know, but we --

DR. NETON: Well, let's not go back to the operations --

MR. FITZGERALD: No, but the premise that was put on the table at the last Work Group meeting was this discrete operation involved ten workers of --

DR. NETON: What I'm suggesting though is that's no longer really a central issue.

MR. FITZGERALD: Well, but I'm just saying that for now --

DR. NETON: Unless you want to make a Class for an SEC prior to 1980 for tritides, and you're certainly welcome to do that.

MR. FITZGERALD: Now, you know, it's a two-part issue. We can agree really on that potential, but now we have the second part which is, okay, you know, with the assumption, and we didn't hear anything different that there weren't any active handling operations, that doesn't deal with recycling, that's just on the production side, but no production-type activities after 1980.

We still have recycling, which we looked at and felt was pretty tight. And then we get to the cleanup on both recycling and on the front end and saying who are those workers and was the potential there not trivial.

DR. NETON: Well, we have surveys for that.

CHAIR BEACH: Possibly.

MR. FITZGERALD: Possibly.

DR. NETON: Brant said there were surveys taken for the D&D operation. That's my point.

MR. FITZGERALD: For tritides?

DR. NETON: Yes.

MR. FITZGERALD: Okay.

DR. NETON: It's going to be a combination, but --

MR. FITZGERALD: Well, I think

that's certainly in question whether or not the surveys were done, were they positive or not, negative, you know.

DR. NETON: Well, that's my position here though is that one needs to determine -- was there significant residual contamination left over from operations that could have contaminated the operators and all these ancillary support personnel that was significant to worry about dose impact.

That's where we are. And I don't know if anybody knows the answer to that right now. Everything is speculation that I've heard.

There could have been massive amounts of contamination in this containment during operation. When they went in to clean it up, exposed a lot of people presumably in bubble suits at that point. I don't know.

CHAIR BEACH: The only thing I heard on bubble suits was when they changed the oil. That was reported, and I went to look for it and didn't see it.

DR. NETON: And this is late enough in the game that in that time frame one would suspect that there's probably RWPs that cover this operation.

I mean I think the answer is that the focus has changed to this D&D operation now in my opinion.

MR. FITZGERALD: Well, I think that NIOSH is prepared to put that position on the table that, you know, we'll agree to disagree.

But in its essence it's made moot by the actions of the Board on the previous SEC.

DR. NETON: Well, I'm not sure of that. I mean one has to evaluate all the merits of an SEC or now --

MR. FITZGERALD: No, I --

DR. NETON: That's why I put it up front that it's sort of --

MR. FITZGERALD: Right.

DR. NETON: Up to the Working Group

to make a decision whether they want to pursue that.

MR. FITZGERALD: What I'm hearing is that we agree on the first part. We agree to disagree on the negligibility of the exposures outside of the ten.

So, rather than beating this to death, I think we agree to disagree based on what we've reviewed as to what that estimate is.

Now, on that note --

DR. NETON: Is it fruitful to keep --

MR. FITZGERALD: Right. And the Work Group Members were party to all this discussion. So, I'm not sure it does warrant much more discussion. They were there and they can make their own judgments based on what they heard firsthand, you know. We've kind of said everything.

The second part, we don't have the survey data in our hands to validate on the

D&D side. As Brant said, we didn't really look at D&D in that context.

DR. ULSH: It's in the SRDB. The swipe data from R and SW Building is in the SRDB. I have not picked it up and looked at it in any systematic way.

MR. FITZGERALD: In the context of this --

DR. ULSH: Right.

DR. NETON: And here's the -- well, I don't know that SC&A made an issue out of D&D other than to mention it and say it's a possibility, but I've seen no convincing evidence on my part that the D&D operators were significantly at risk for --

MR. FITZGERALD: Well, I think we did. We broached the issue of more operations that were implicated with hafnium tritide beyond the discrete one that was identified in the White Paper, and we included D&D as one of those.

And we've had a dialogue, we

brought that up in interviews, we were focused on D&D. And the feedback we got was, I think as Brant -- we had mentioned that, yes, the operators were told -- I'm not sure I have the date on that, Brant, whether the operators cleaned up right after the end of that campaign or whether they did it right before D&D started, you know. It's unclear.

But, you know, I think, yes, we did spend a lot of time trying to at least unpack the implications on D&D. And at one point, one individual down at the recycling facility acknowledged that, yes, that would have been a cleanup issue.

And given the history, it's understandable it would have been a cleanup issue.

So, that's about where we are on the D&D.

CHAIR BEACH: So, let's take a poll amongst the Working Group.

First of all we were looking at

whether meaningful exposure pathways existed for hafnium tritide exposure, whether the small cohort of workers are involved and can be named.

Okay. So, we talked about that and whether exposures in the 1980s could have occurred.

I believe that these have all been proven based on our interviews. Then you add the other end of it, the diffusion issue, reactivity, the recycle operations.

I think that during our worker interviews held last April, it became obvious to me at least that NIOSH is unable to know who may have been exposed -- excuse me -- who may have had exposure potential over time to the hafnium tritides.

And of course this has been mentioned several times today that it's already gone beyond the original ten that was mentioned.

And again three out of the five

Work Group Members were present during those discussions for the classified discussions on the 6th and 7th, and should be able to draw their own conclusions.

Cleared Members have all had the opportunity to examine firsthand the classified site information based on existing evidence.

There has existed a probable exposure potential for workers to highly insoluble metal tritides at Mound, and it remains infeasible for NIOSH to estimate doses with sufficient accuracy due to the lack of monitoring data.

Now, this is the first I've heard -- I knew that Brant was going to go in and look for swipe data, but never did hear any more about that.

So, originally when I wrote this yesterday, I was including reliable air sampling data, and it hasn't been proven that there is reliable sampling air data today.

And to identify -- they are not able to identify these workers who may have had potential exposures.

So, I guess I'm going to ask the Work Group if we were to bring this up for recommendation, I would recommend an SEC from 1980 through D&D.

What do you guys think?

Where are we at?

So, Brad?

MEMBER CLAWSON: I feel the same thing. That's what I've been trying to say. We've got too many loose ends.

CHAIR BEACH: Okay. So, yes, we should bring that up as a recommendation to the full Board in August.

Bob?

MEMBER PRESLEY: No.

CHAIR BEACH: No. Okay.

Phil?

MEMBER SCHOFIELD: Yes.

CHAIR BEACH: Paul, what do you say?

MEMBER ZIEMER: Well, I'm an alternate on this so I don't know if I get a vote on that.

CHAIR BEACH: You do.

MEMBER ZIEMER: I'm not prepared to recommend an SEC based on what we've heard.

I agree with partially the idea that there might have been some exposures, but there's -- we haven't -- I mean part of this has just arisen today and --

CHAIR BEACH: Yes, I agree.

MEMBER ZIEMER: I think it's immature for us to make a recommendation based on what we've heard.

We don't really know what those smears and air samples look like. We do know as I understand it, that we have urinalysis for all of these people so that if there were exposures, doses could be reconstructed.

Am I right that we have the urine samples for all these people?

MR. FITZGERALD: Yes.

MEMBER ZIEMER: Even if we don't go there.

MR. FITZGERALD: But again we have to know -- we have to peg the workers, D&D workers, I'm just saying, to the operation they were working. You wouldn't at all discriminate the tritium, right?

CHAIR BEACH: All the workers.

MR. FITZGERALD: Yes.

MEMBER ZIEMER: Well, I don't know. I mean do we know who did D&D?

DR. ULSH: The people who worked -- okay.

The people who worked --

MEMBER ZIEMER: After `80.

DR. ULSH: The people who worked D&D in the R and SW Buildings were -- tritium was included in the bioassay program that they were supposed to be on.

Does that answer your question?

MEMBER ZIEMER: Yes, I think that tells me we can reconstruct dose if they had tritium uptakes.

MR. FITZGERALD: Well, how would you know who was exposed to hafnium potentially though?

CHAIR BEACH: That's the problem.

MEMBER ZIEMER: Well, it's just an issue of bounding it. I guess you would --

DR. NETON: Well, this was a previous issue that you end up with very large tritium excretions. And if you use a Type S model for that, you end up with some fairly large lung dose and you have to swipe all workers.

And the question is, is that reasonable to do?

DR. MAURO: When I asked this question about these other people that we don't know who they are, and I said is it plausible that they could have experienced exposures higher than the people that we do know had some exposure, that in my mind got to the heart of the issue because what this means is, if somehow we could convince ourselves that these other people though they might have the potential for exposure, it's inconceivable that it could have been greater than the exposures experienced by the people that we do know had a real potential for exposure.

Now, how does that help us?

Let's say we get to that point somehow where everyone agrees, yes, there are other people, we don't know who they are, that have the potential for exposure.

And we could identify a whole bunch of scenarios under which theoretically that could have occurred at some time and some place. We still know who they are.

What we can say based on, let's say, the swipe samples or whatever the weight of the data are, that the potentials are unlikely to be greater, you know, than the people that we know were exposed.

Now, what I just heard is the highest exposure that has occurred in any given year was about three rem. All right. So, I'm looking at -- I'm playing this out in my mind right now, so stay with me.

So, what you're saying now is if you would buy that second part that is it's really not plausible that all these other people -- well, then you assign all those other people the highest dose because it is plausible and you've bound for it.

If you can't say that -- you see what I'm getting at is if you can't say that, that is wait, no, no, no, the nature of the operations and the cleanup that Phil was talking about are such that we really can't say with a degree of certainty that those exposures were less than or had a potential to be less than the people that we do know.

If we can't say that, then where you are is where Jim is. That means we have no choice but to assign everybody in the plant assuming that every tritium analysis in the urine collected was due to the inhalation of hafnium tritide, which of course is completely implausible.

But if you can say it, and I'm just trying to be helpful here, but if you can say the weight of the evidence is clear, it's inconceivable that these other people who might have been exposed that we don't know who they are, could never have inhaled amounts that were comparable to these other people, you've bounded it.

It can't be higher than that. You've bounded it and then what are you going to do?

You're going to give everybody else in the plant that dose. I mean there is no --

MR. FITZGERALD: This is getting back to a thought earlier this morning where we were talking about the empirical basis for N/P ratio.

You're saying the empirical highest potential was this individual --

DR. MAURO: If that's true. I'm not

saying it is.

MR. FITZGERALD: I mean I'm saying if that's the postulation, then empirically that would be overbound.

DR. MAURO: Yes, that's what I'm putting on the table, yes, as a possible way of wrestling this --

MR. FITZGERALD: But then you still have the problem -- I'll go back to, you know, who are those --

DR. MAURO: Everybody.

MR. FITZGERALD: Who would be --

DR. MAURO: Everybody.

MR. FITZGERALD: Potentially exposed?

DR. MAURO: Everybody. Everybody in the plant is going to get that dose from hafnium tritide.

MR. FITZGERALD: Is that plausible?

DR. MAURO: Well, I don't know.

(Laughter.)

(Simultaneous speaking.)

DR. MAURO: Well, no, no. I'm sorry. I'm trying to --

MR. FITZGERALD: I'm just trying to figure out --

DR. MAURO: I will say everybody in the plant --

MR. FITZGERALD: Right.

DR. MAURO: That possibly --

MR. FITZGERALD: Right.

DR. MAURO: Could have been involved in an operation, and that may turn out to be everybody in the plant. I don't know if that's true.

Certainly that would be -- I mean right now we don't know who these other people are.

MR. FITZGERALD: Well, I'll disagree. You either can define them tightly or you end up sort of taking everybody.

I mean it's difficult to go in between. So --

DR. MAURO: I mean there may be a

boundary to place and say of course these people, they weren't exposed to any tritium at all or there's no way inconceivable that they could have been exposed to hafnium tritide.

Well, okay, then they're ruled out. But if anybody you could say that conceivably might have been exposed, but one thing for sure if they were, it wasn't greater than the guys we know about, well, here's your boundary.

MR. FITZGERALD: Well, yes. You only have the two choices.

Either you draw the lines around the workers that were potentially exposed, assign them hafnium tritide, or you have to go the other route.

DR. MAURO: Yes. And you see why what happens when you -

CHAIR BEACH: And so --

DR. MAURO: I'm sorry.

CHAIR BEACH: Excuse me. Is there more work that can be done on the swipe samples?

Is there more work that can be done, Brant, I'll ask NIOSH's -

DR. NETON: Before Brant speaks, which swipe samples are you referring to?

CHAIR BEACH: The tritium.

DR. NETON: Right, but I'd still like to have this delineation because you're talking about after 1980.

CHAIR BEACH: 1980 to --

DR. NETON: So, really we're talking about the swipes from the D&D operation.

MR. HINNEFELD: Well, no, there was the time period before D&D.

DR. NETON: Before 1980?

MR. HINNEFELD: From 1980 until D&D started. I mean D&D didn't start in 1980, did it?

CHAIR BEACH: Well, it started different times.

DR. NETON: I guess it's not clear to me after -- what happened after 1980 with -

- I thought that the hafnium tritide source had essentially been put to bed and then you've got this room that was used for hafnium tritide.

And at some point there must be smears inside that room after active operation stopped. That's I guess what I'm referring to. Maybe I wasn't clear. So, somewhere there must be smears.

I don't know how widespread the extent of the contamination inside that room really was.

We're speculating, well, they worked with large amounts of hafnium tritide. So, clearly there must have been widespread amounts of contamination in there.

MEMBER ZIEMER: Are we allowed to know the size of the source-term activity wise or is that classified?

CHAIR BEACH: Classified.

MEMBER ZIEMER: See, this is a real problem.

CHAIR BEACH: Yes, it is.

MR. ZIEMER: Here's the deal. There's all kinds of experience that shows sort of the upper limit of what a person can inhale based on the size of the source-term. I've had firsthand experience with it.

And it's where the million-to-one or the ten to the --

DR. ULSH: Ten to the minus six.

MEMBER ZIEMER: Every kind of incident which shows that a person --

DR. ULSH: The magic numbers.

MEMBER ZIEMER: Cannot take in more than about ten to minus six of a source-term that's dispersed right in their face.

Now, if the source-term has been removed and you have some -- and it's your magic number. Maybe it's some amount that's left and it's a little bit and you postulate, you can bound. You can say there's no way if somebody is -- and that's already dispersed in the system.

So, you could bound it. But if we're not allowed to know the source-term, then I think half of our Board Members are at a disadvantage.

DR. NETON: But I think, Paul, if you know if you have surveys and smears, you know what the resuspendable source-term is if the source has been removed.

I have a 10,000 DPM --

MEMBER ZIEMER: Right. And that will help if we have the urine samples.

DR. NETON: A millionth of that or ten to the minus four of that becomes airborne --

MEMBER ZIEMER: Right.

DR. NETON: You can come up with a plausible upper bound scenario for exposure to anyone who entered that room.

You could assume they inhaled that 24/7. I mean --

MEMBER ZIEMER: Well, I mean you may have to take into consideration Phil's point that that air sample may not represent the whole room, but --

DR. NETON: No, I'm speaking once the active hafnium --

MEMBER ZIEMER: Right. Yes.

DR. NETON: Now, any smears that you have even if it's a combination of other materials, you smear it, you can then have a contamination source-term that can be used to generate an airborne --

MEMBER ZIEMER: Right.

DR. NETON: Given even very invasive activities like grinding, cutting, welding --

MEMBER ZIEMER: Right.

DR. NETON: And come up with an inhalation source-term that I believe would be credible and probably --

MEMBER SCHOFIELD: Do we have these -- excuse me. Do we have these rad surveys? I mean is there a daily, weekly report?

MEMBER ZIEMER: I don't know. I just heard about them.

DR. ULSH: All I can tell you, Phil, is that we captured several boxes of survey data from the buildings in question.

I have not gone in and examined them in any systematic way, so I can't tell you if it was daily, weekly or whatever. I don't know until I look at it.

DR. NETON: But I think we're talking specifically though about the operation, the glovebox operation that was --

MEMBER SCHOFIELD: Once that suspended -- and here's my ignorance. I'm sorry, but it seems like we could take a sample of those smears after they suspended using it, and that would give us an idea of quantities -- or at least potential quantities that are still left behind.

DR. NETON: That was my point, you know. Until we know that, we don't, you know.

DR. ULSH: So, if I could get some clarity on exactly what the Work Group is requesting that we do?

CHAIR BEACH: Well, there's two paths. One, we make a recommendation to the full Board in August or two, we determine if there's more work that needs to be done and we agree to whatever that work is.

And that's kind of where we're at, I believe.

DR. ULSH: Well, I agree. And I would ask you to consider before you decide which option to take, if I come back to you with the information that you're requesting, the smear data and say here's what the level is, here's the contamination levels, what are we going to do with that?

I mean is that going to convince you that --

DR. MAURO: What has to be done is to show that it's inconceivable that with that level of contamination his exposures could be higher than the people that were involved in exposures.

See, to me that is your boundary.

MEMBER ZIEMER: If you use it as Jim described it, it's not bound or --

DR. NETON: Yes, it's not different than a contamination model that we do for many sites.

We have a service contamination level and we generate an inhalation source-term based on that and certain --

DR. ULSH: But is the Working Group going to accept that approach, is what I'm asking?

MR. FITZGERALD: Well, it seems like you're going to come up with tritium, you know, smear measurements in a particular, say, R-108 for the recycle and for this particular two-room lab. And those values will be looked at. You will do a calibration of how much of that tritium would have been in the air and then what -- but I still don't quite see to what extent you're going to know that the tritide, you know, the tritide --

DR. NETON: Well, take an example

20,000 DPM of a hundred square centimeters.

MR. FITZGERALD: Right.

DR. NETON: And probably almost all tritium as HTO.

MR. FITZGERALD: Right.

DR. NETON: But if you take a resuspension factor, ten to the minus fifth or something, you still are only generating into the air 10, 20, 30 DPM per cubic feet.

You generate a fairly low air concentration that can give you a bounding estimate of what the tritium -- tritide exposures could have been even assuming that all that sort of contamination was related to pure tritides.

And we do this all the time for --

MR. FITZGERALD: Jim, I'm just trying to -- I don't disagree with that, but I'm trying to figure out if it's been done to come up with an apportionment for the tritide.

DR. NETON: Well, not apportionment. We're assuming it's all a hundred percent --

MR. FITZGERALD: A hundred percent tritide.

DR. NETON Because you can't possibly get all of that in the air instantaneously. So, you can assume very conservatively that only -- pick your number, ten to the minus six, ten to the minus fifth, of that becomes airborne, and you're left with very low potential levels of inhalation. Very low.

I mean it exists because of what Dr. Ziemer said. Not much gets airborne even if they are doing mechanical things with it not even entailing the entire contaminated source-term.

And that source-term is much, much lower than what they're working with when the source was in active operation.

DR. MAURO: So, let's say you have an abundant amount of swipe data. Okay. That's collected before, during, after any kind of D&D operation, maintenance operation at all different locations throughout the facility as part of the health physics coverage.

DR. NETON: Well, I'm not saying throughout the facility. I'm specifically thinking about the hafnium area where --

DR. MAURO: Okay. Okay.

DR. NETON: Where hafnium work was performed.

DR. MAURO: Okay. And let's say we have that data and everyone agrees, yes, you do have a lot of data, swipe samples in the areas that conceivably could have been contaminated with residual levels of hafnium.

DR. NETON: Correct.

DR. MAURO: And the very fact that it's swiped, means a certain -- it's not tritium gas. I mean it's --

DR. NETON: Well, could be HTO.

DR. MAURO: It could be HTO or it could be one of the lesser solubles or it could be --

DR. NETON: Could be anything.

DR. MAURO: And now you've got a number and -- okay. Now, I'm just trying -- all right.

Now, the simple question is not try to quantify, because trying to quantify what the inhalation dose is under those circumstances is a tough one, you know.

But what you might be able to do is to say that under any of those circumstances could a setting like that give rise to doses greater than this value. It just is not conceivable.

And that value is less than the highest value that we know of.

DR. NETON: Well, one could easily -- I have to be careful.

It wouldn't be very difficult to demonstrate that the three rem that you talked about earlier -- how much of that material would have to become airborne in order to generate a three rem?

DR. MAURO: See, I'm looking for a plausible upper bound. But if it turns out that the process we go through shows that, geez, it's possible that these people could have been exposed, could have experienced hafnium tritide exposures that are well above the exposures experienced by others, I think is a problem.

MEMBER ZIEMER: Well, it would be useful to know that.

DR. NETON: I'm not sure why you feel it's difficult to convert surface contamination levels into some airborne value. We do it all the time.

There are certain resuspension factors that are used per square meter and you get per cubic meter values out of that.

And I think what you end up showing is, you know, resuspension factors that are very level, as we know they are, especially for particulate like that it's very difficult.

I mean if it's a million DPM for a

hundred square centimeters, I'll withdraw everything I just said.

But if I suspect that it's spotty, 20,000, 50,000 DPM per a hundred square centimeter value, it would be hard to get much internal dose beyond this three rem for sure.

And I think that's where the focus has shifted since the operation stage before 1980 to the -- sort of what I consider to be a residual contamination phase.

DR. MAURO: Okay. So you --

DR. NETON: See, I think that's a fairly boundable problem. That's my opinion.

DR. MAURO: So, the key to whether or not we've got a potential SEC problem here is if you've got lots of good data on swipe samples that were collected under a broad range of circumstances that you feel confident catches, I don't know if that data exists or doesn't. And I mean that's the arguments being made here.

I mean what I'm hearing is if I've

got all that data, that swipe data, and you who know the site and the nature of the D&D operations, the recycling, you folks know behind closed doors what those operations are and you look at all the data and say, wow, we have data -- here's the data, swipe samples collected, at that point a case has to be made that we could place an upper bound on what the exposures might have been to -- the highest exposures could have been for people involved.

We don't know who those people are, but the people involved in working in those capacities. We don't know who they are, and there's your upper bound.

That's the argument that you're making, and that's what I'm hearing is being proposed.

MEMBER ZIEMER: Well, I know, Josie, you're wanting to close this and I think we should.

I would hope, I would propose you are in favor of recommending SEC would at least allow a look at this before final recommendation.

CHAIR BEACH: Well, Paul, I think it's important that we do look at it. We've had much discussion today on it and I don't think anybody would be comfortable --

MEMBER ZIEMER: But it's kind of a new --

DR. MAURO: This is new.

(Simultaneous speaking.)

CHAIR BEACH: I don't think anybody here would be completely comfortable if we didn't explore this. So, I agree. I personally do.

MR. KATZ: May I just add something, too, because it's been pointed to a couple of times in the conversation. It makes me uncomfortable every time it gets pointed to.

Several Board Members have been behind the screen, if you want to just call it that, and have other knowledge, and obviously staff members have been behind the screen and have other knowledge, but the Board as a whole has to rely on what's on the record.

And really what you know yourself personally doesn't help the rest of the Board. It has to rely on reviewing what's been said in the Work Group and what gets said in front of the Board.

So, I'm just a little uncomfortable when people reflect back, well, you can make your judgments based on what you know behind the screen. But, yes, you individually can, Josie, but the Board can't.

CHAIR BEACH: But you have to rely on that if your -- if you have a disagreement with NIOSH, I don't agree with what NIOSH heard in the interview, then how else do you disseminate that except by what you heard and what you believe.

MR. KATZ: What I'm just saying is that that's fine for you personally, Josie, to rely on what you know behind the screen, but it doesn't help the rest of the Board, because the rest of the Board is limited to the knowledge, the information that's been disseminated in the Work Group physically and in front of the Board.

And so I mean, for example, when you sort of read your sort of like a motion as to recommend forward, you went through that very quickly and I'm not sure who well understood all of what you said quickly before you went before recommendation, but that's the sort of information that the Board is going to be limited to when they make judgments, not your specialized knowledge or Joe's or Brant's or --

MR. FITZGERALD: Yes, the ability for us to translate this into a form that the Board --

MR. KATZ: Right.

MR. FITZGERALD: I mean so far we haven't identified a showstopper where it's crucial and it's behind the screen.

MEMBER ZIEMER: And I think, for

example, if we didn't have the monitoring data and we could only do bounding with source-term information, then I -- then we're at the place where we were on Ames.

And, remember, Larry had guaranteed we'd never in the future have to make a decision based on lack of classified information or something to that effect.

MR. HINNEFELD: He did what?

(Simultaneous speaking.)

MEMBER ZIEMER: We would make our decision based on what the -- we would only make our decisions based on what could openly be discussed.

MEMBER SCHOFIELD: But I think like you and everybody else here, you know, whichever part of the table they're sitting on brings forth their experience, their knowledge so that -- well, you know, I mean given your background you guys know things that are so far above me I can't even see that point, but on the other hand I bring stuff, you know, from working in the trenches and this is how we learn from each other.

MEMBER ZIEMER: That's right.

MEMBER CLAWSON: And this is also why the Board was set up the way that it was. There's four people from here, four people from that. Now, it's a little bit more.

And I would also -- and this really comes down to Ted and everybody else. This is just warm up for the one that I plan.

MR. KATZ: That's absolutely true.

MEMBER CLAWSON: And I'll tell you what from day 1, and I've said it, because it makes me real nervous, because there is very, very little that I --

MEMBER ZIEMER: That's going to be a problem.

MEMBER SCHOFIELD: Yes, it's going to be a huge problem.

MEMBER CLAWSON: But I guess, Josie, I guess I'm looking at what our path forward here is. Because if we're going to the point where Brant says, you know, with the swipes and everything else like that, there's a little bit more to it than that, and that's to also see what came into that plant.

Because you know as well as I do that we've had other players that have come into the game between the 1980s and `90s that is going to be -- that's going to play into it.

CHAIR BEACH: I think that's a really good thought.

Should we go ahead and take a break?

MR. KATZ: Sure.

CHAIR BEACH: I think everybody really needs one. We'll definitely come back onto this topic.

MR. KATZ: A ten-minute break or --

CHAIR BEACH: Yes, ten minutes.

MR. KATZ: Okay. So, about five after 3:00.

(Whereupon, the above-entitled

matter went off the record at 2:54 p.m. and resumed at 3:08 p.m.)

MR. KATZ: Ready to go back on.

CHAIR BEACH: Sure.

MR. KATZ: Okay. We're reconvening. This is the Mound Work Group after a short break.

CHAIR BEACH: Okay. And so at this point we have decided that we are going to ask NIOSH to do a little bit more work on swipe data for the tritide issue. And I think we can probably leave it at that and reconvene at the next Work Group meeting once we have determined what swipe data is available and how robust it is.

MEMBER CLAWSON: Well, Josie, this is Brad again. I want to kind of make sure where NIOSH is going with this.

We're going into something else, too, because we have seen we have had other items come into NIOSH and the same issue.

When you've got a recycling

facility, other people want to be able to use it too and we've seen this one from Pinellas.

I want to make sure that we have looked at what has come into that. And I guess also I would like to be able to -- Brant has said that the bioassay data is -- everybody was sampled for tritium and everything else like that. And from our interviews during that era, they weren't.

So, I just want to make sure that bioassays also there, too. And I'm speaking in later, later years.

I know after the project shut down, I just wanted to make sure that we all know which way they're going with it and what we're going to look at.

CHAIR BEACH: So, you're talking bioassay from 1980s on through D&D?

MEMBER CLAWSON: Yes.

CHAIR BEACH: Okay.

DR. ULSH: So, what exactly is it you want?

What information do you want us to bring you?

MEMBER CLAWSON: Well, this is what I want to be able to find. You've made the comment that the bioassay, that everybody was sampled for tritium.

Is this correct or --

DR. ULSH: Well, no. Not everybody on site.

What I've said is that for people who are working in areas where -- depending on the time period you're talking about.

At least for part of the time period if you had an exposure potential of greater than 100 millirem per year, you were required to be monitored.

So, for people who were working in areas where there was tritium present that could have presented a hundred millirem per year, you were required to be on a bioassay program. That's what I'm saying.

MEMBER CLAWSON: Okay. And what I'm

saying is because we're looking clear into the D&D period, that we need to understand if these people that were going into these areas if that was still standing, if that was still a requirement.

Because after the process, there's an interesting belief that once the process stopped, everything has gone away.

And I beg to differ on that. I believe that you still have the residual parts and you still have items there.

Because what year was it that we did the recycling?

Because the drums went out there and they sat for a long time. And then they built the recycling process.

DR. ULSH: What recycling?

Are we talking about the tritium recycling facility?

MEMBER CLAWSON: Tritium. Right.

DR. ULSH: It operated for decades.

MEMBER CLAWSON: Okay. Well, I just

want to make sure because we -- also having a tritium recycling facility, we already know that there has been other product coming from other areas.

I just want to make sure that the bioassay is sound enough that it's going to cover these eras and be able to tear these facilities down.

DR. ULSH: So, if we were to investigate and ask the appropriate people, the people who were involved with this tritium recycling facility, were you required to be on tritium bioassay, and they'll either say yes we were, or they'll say no we weren't, and I brought that information back to you, is that what you're looking for?

MEMBER CLAWSON: Are they the only ones that went in there?

Because, yes, that individual says that he was -- they were the only ones that dealt with tritium until we started pulling the string of, well, who did this, who did this. Well, that would have been these people, that would have been these people.

And this is where we came out with that there's a lot more people than just these few.

And I want to make sure that we're covered on this because we're saying that the bioassay is going to cover these people in these areas and so forth.

Especially the tritium recovery and the other facilities where we had it. I just want to make sure that the bioassay supports what you're saying.

DR. ULSH: And I just want to make sure I bring you the information that it reflects.

MEMBER CLAWSON: Right.

DR. ULSH: And to do that I need to understand pretty explicitly what it is you're asking for.

So, for instance, we know the tritium recycling facility was in the R and SW

Building.

So, if we brought you information about whether people who worked in R and SW Building were on tritium bioassay, is that --

MEMBER CLAWSON: Or we ask clear up through the D&D of those facilities.

DR. ULSH: Okay.

CHAIR BEACH: So, I think there's two parts to this. The first part is pre-1980. The Work Group probably would recommend an SEC for tritides. But because of the radon issue, it became a moot point.

So, the second part of this is looking at post-1980 through to the end of D&D. And some of it in my mind is being a little bit clouded because we do have a D&D report that we haven't really even spent any time on.

And I guess, Brant, I think what we're going to be looking for is anything that's available; bioassay or swipes that were mentioned earlier.

And I think we probably should limit it to a small room as we talked about earlier, the most likely areas, and then move out from there depending on questions that come up within the Working Group.

DR. ULSH: Okay. I will analyze the collection of swipe data that we have currently available to see -- and I know what rooms we're talking about. See what data we have for those particular rooms and --

CHAIR BEACH: Locations and swipes for --

DR. ULSH: Yes, I'll characterize it, what we've got, and then report that back to the Working group.

In terms of -- well, you haven't gotten to the bioassay yet. I'll wait for your request there.

So, yes, that's what I`ll do. I'll characterize what we've got in terms of swipe data and then we'll see where we go from there.

MR. FITZGERALD: I would think there would be some scoping outside of this meeting before that would be finalized, obviously, by going back and forth, just to make sure that it's explicit enough.

DR. ULSH: Oh, and do you want me to address the bioassay part?

CHAIR BEACH: Sure.

DR. ULSH: I mean basically what I plan to do is look and see what documentation is available, look and see what communications we have with workers who worked in R and SW Building with regard to bioassay that was required and was actually performed for people who work in that building all the way up through D&D period.

I think that's what you're asking for, right?

MEMBER CLAWSON: Right.

DR. ULSH: Now, I mean I could probably give you information about there are this many thousand tritium bioassay samples in

MESH, but I don't know if that's what you're looking for and I don't know if I could specifically limit it down to those particular buildings. So, I wasn't thinking of going there unless you want it.

MEMBER CLAWSON: Well, we've just got to be able to make sure because at the very end there everybody was pretty well everywhere. And I just want to make sure that we have sufficient information to be able to cover where they're at.

I know that it may not be possible, but it would be interesting to find out, when the tritium was processed, where it went to.

DR. ULSH: You mean the recycling?

MEMBER CLAWSON: No, the actual glovebox and so forth.

DR. ULSH: Skeptical, but we could try.

MEMBER CLAWSON: Well, just because -- anyway, that would be interesting to see where it went because they had some incidents that came out of that.

MR. KATZ: Emily, can I ask you a question?

CHAIR BEACH: For those on the phone, we are just taking a few-minute break while our Federal Official and lawyer stepped out of the room.

So, we're still online.

(Whereupon, the above-entitled matter went off the record at 3:18 p.m. and resumed at 3:19 p.m.)

CHAIR BEACH: Okay. Any other tritide-related issues before we move on?

MEMBER CLAWSON: I guess something that I would ask is Paul not being involved in a lot of these, is there something more that we could do to be able to assist to be able to help you or understand the problems that are facing us?

MEMBER ZIEMER: Well, I've raised my questions as they've come and I understand that not everything can be divulged, but I think we have to think of the bigger picture as to how these kind of things are going to impact -- it's not going to be just me because approximately half the Board Members are currently uncleared. They're somewhere in various stages of getting cleared.

But even if that occurs, we understand that the claimants have also a right to the information on which a decision was based whether it's an SEC or an individual dose reconstruction.

So, we have to be able to work around the classified information and gather what's needed in a forum that allows us to make a decision.

And I think that's what the bottom line is going to be.

MEMBER CLAWSON: Right. I understand.

MEMBER ZIEMER: And you've all been very helpful as far as this is concerned as far as you're able to go.

And if this works out better than what's been proposed, I think we're fine because we don't need to get source-term information.

I think to the extent that we're able to -- we need to be able to get in and to see some of these things, but the bottom line is we need to get the basic unclassified information that is usable to make informed decisions on SECs or dose reconstructions.

And I think in most cases, we'll be able to do that and work our way around these things. At least I'm hopeful that's the case because --

CHAIR BEACH: Well, and I think it's beneficial having you in the position of not seeing all the documents because it does give us other ideas and other avenues to move forward.

MEMBER CLAWSON: It helps us to be able to look at it because many times when we discuss this in detail, you know, how do we bring this forth to them, what questions do you think that they're going to have on this.

This is why I was asking if there's anything more that we can do because this is kind of a test to see how we can do it.

CHAIR BEACH: Okay. So, let's move on to radon. It's the next topic.

MR. FITZGERALD: Okay. Let me jump in on that one.

One thing that is figured with the radon issue is just a lack of a lot of data. I think from the very get-go there's been a couple of data points and that was the eventual premise behind our concern that there just wasn't enough data.

And I think we did have a meeting of minds and that resulted in the SEC recommendation being voted in and everything.

And what we're talking about now is really what's the posture beyond the current SEC which ends in `80. And I'm -- I was going to say conflicted, but that sound -- that's kind of a loaded word.

I have mixed feelings. Thank you. Mixed feelings on this issue because on one hand we have a couple of clear data points which is the -- a little background. There was some radon measurements taken because of an acknowledged increased radon escalation in this room, SW 19.

And that led to monitoring that was done and a validation that, yes, we've had a source that was coming in primarily with negative pressure or whatever it was coming in.

And a mitigating action being taken which is to vent an underlying tunnel to vent the radon isotopes, and there were several isotopes, to the atmosphere.

And the individual involved did some measurements after that was done and saw levels approaching background, if not background. So, you know, the determination was that was a successful mitigation.

The next documented measurement was in 1990, I believe. And again this was a memo by the same individual who was asked back because the operators or the people that were in charge of the area noted that levels were, quote, approaching D-A-C, DAC levels in SW 19 and asked him to come back and take additional measurements.

Now, as documented in the memo of that time period, 1990, his measurements showed levels -- very low levels, you know, sort of a commensurate background, and that's what we have essentially.

I haven't seen anything much beyond that, but what gives me the mixed feelings is that way back when we did the Site Profile review, we interviewed rad techs that operated in the SW/R complex and they told us that -- and this is in our Site Profile review report, that they would monitor with their monitors, the cracks and fissures in R Building and would see, you know, I think in their words, their cameras would peg out and then attribute that to the inhalation of radon coming in from the foundations. And that time frame was the mid-`80s. `85, `86, whatever.

And that coupled with the fact that the genesis of having this individual come back in 1990 to do SW 19 was an observation of levels approaching a DAC level, it gives me mixed feelings. Because in a way, yes, the mitigation based on those measurements that were done by this one individual, as it turned out, seemed to verify that, you know, the mitigation was working.

On the other hand, you have this -- now I'll call it anecdotal, because in a sense we got this from people that knew what they were doing, rad techs or whatever, but obviously levels were -- or inhalation in level -- increased levels were being seen in those buildings.

Now, I'm not aware of any additional information. And I went through the pains of locating the individual who did these measurements. And I invited Brant to join Josie and I and just frankly talking with him, just saying what do you remember?

Well, he did not recall much of anything up to 1990. So, unfortunately didn't learn much more about the genesis of why he was brought back, what was the background.

All we have is a piece of paper that says the levels that were monitored were low.

So, that's kind of where we are. Those are the facts. I mean, you know, just trying to resolve the question of having sort of these contradictory pieces of information, I don't think that was successful.

So, I guess part of my report for the Work Group and you were part of this discussion, is that what is documented, what is actually in writing in terms of measured levels is what this individual monitored in 1980 and in 1990, and I respect that.

And we did talk to him and he felt that in general the mitigation was successful. And I think we've got to take that at face value since he was the one that was involved.

We do have this additional information that was gleaned independent of that from the rad techs in R Building and, you know, and also the memo itself in 1990 acknowledged that the reason that he came back was this increasing level of radon that was being seen.

So, it certainly leaves me with the mixed feeling that, yes, I guess, you know, what we say, the weight of evidence, the weight of evidence just should go with what's been recorded in lieu of having any better information.

And I think, Jim, you have stated in the past, well, this is 83.14. That if better information or additional information comes to the floor, it doesn't preclude you revisiting.

I guess that's kind of where I'm at that really we haven't been able to find anything better. It is what it is in terms of the data available.

There is some contradiction, but again what's written down and what's measured is what this individual did and that's what we have. That would be my perspective.

DR. NETON: Can I ask a couple questions?

I'm not that familiar with the radon -- the measurements that were made that led them to believe that there was excess radon, but were they actual radon measurements, or were they just like beta-gamma survey meters that picked up excess --

MR. FITZGERALD: The --

DR. NETON: See, I would be surprised if they were radon measurements. If they were doing that, then why would they call Jenkins in?

And it wouldn't surprise me that

you would have extra beta-gamma activity in cracks where the radon had been sealed to enter the building.

MR. FITZGERALD: Yes, and I guess in my observation, it wasn't very clear.

DR. NETON: Right.

MR. FITZGERALD: We had this two-page memo and it just acknowledged that was the reason he was called in. Didn't go into a lot of details and background.

And we were actually talking to him because that was exactly what I wanted to know, you know. What did they use, how did they use it and is there any explanation for why your measurements differed from theirs? And he just couldn't remember.

So, it sort of leaves you with okay --

DR. NETON: It wouldn't surprise me if beta-gamma survey measurement would show a lot of activity with no radon. It's almost an indication that it's being held up and the sealing is actually working.

MR. FITZGERALD: Yes.

DR. NETON: So, I'm not sure those two pieces of --

MR. FITZGERALD: Short of knowing more about what led to their calling him back, all I can say is that this is all we know and it's not enough in my mind to go any further.

But if anything else surfaces -- and we beat this one. We haven't found any -- there was surprisingly little amount of documentation on these kinds of measurements and we have essentially just these two time frames.

But it bothers me that we did talk to rad techs and got this kind of feedback from the `80s.

And knowing how sometimes you're operating a plant in negative pressure, you know, the question is, is the negative pressure defeating this vent that's way over here?

I don't know. And there's no way of knowing that clearly, so I just wanted to sort of this is what I think where we were left. And it's not the best place, but it's the best we could do at this stage.

CHAIR BEACH: Well, being that it is an 83.14 and can be reopened if any other documentation comes to life, I would almost think that as a Work Group we don't really have much choice except to close the radon issue at this point, the post-1980.

I was really hoping that the interview we had would -- he clearly did not remember anything and really he wasn't very clear that the venting worked, but he didn't remember it not working. So, I'm --

MR. FITZGERALD: And to be fair, that is 20, 30 years. I mean --

CHAIR BEACH: Yes.

MR. FITZGERALD: It was a challenge, but that's the best we could do.

CHAIR BEACH: So, what do you say,

Work Group?

Close it, leave it open, is there more work that needs to be done?

MEMBER CLAWSON: I don't know much more that we can, because we have very little data on the radon issue anyway, you know. This was, I believe, two samples or whatever else like that.

The only part that worries me about it, I feel that we can close or whatever, but you said it was an 84 --

CHAIR BEACH: 83.14.

MEMBER CLAWSON: 83.14. The only thing is, is when we usually pull away from these unless something comes up, you know, that pops up, I realize that we can bring that back up, but, you know, we kind of stop looking too. That's my issue.

MR. FITZGERALD: And it appears that, you know, these sort of very specific measurements that were done before the early -- the early `90s they started doing baselines of radon across the complex, and Mound was included.

But before then it was, you know, it was driven by concern for vents and there just doesn't seem to be a whole lot of data points.

DR. ULSH: Well, all right. I've been biting my tongue because I don't want to disagree, specific disagreement, because I think our conclusions are going to be the same, but we do have a few more things than we've been discussing.

In 1980, we have the measurements that were taken immediately after the installation of the turbine, the stack. And those showed reduction in radon levels. We have that.

MR. FITZGERALD: We said that.

DR. ULSH: The person that we interviewed is a well-known expert on radon certainly at Mound, and really was involved in the efforts to characterize radon across the complex.

And what he said in his interview when we asked specifically about this situation was, Joe is right, he didn't recall the 1990 measurements, but he said I truly believe that SW 19 was down to background after 1980 and continued to be so.

And he periodically sampled from the stack for purposes of mishaps, and he is comfortable that the system was functioning and the situation at SW 19 was solid. That's what he said.

We also have where he described periodic situations particularly in the month of August where they would see increases in radon due to whatever the weather conditions were at the time.

And he said it was kind of weird because we saw it pretty much every year. They knew when August came, they were going to be seeing that.

DR. MAURO: Where?

In the same room?

DR. ULSH: Oh, no, no, no. Just in general.

DR. MAURO: The whole facility.

DR. ULSH: Yes.

DR. MAURO: And what levels are we talking about where he sees these changes?

DR. ULSH: He wasn't specific. Although, he did talk about in the same context, he talked about workers that were counted.

And when they came into work, they were counted in the morning and they showed a high level. And when they were counted after lunch, no more high levels.

And so they characterized that as, okay, they were getting it at home and bringing it with them to work.

So, there was some natural fluctuation there, but clearly this is a guy who had an interest in this topic and he just was not aware of a continuing radon problem.

And, furthermore, I'm not aware of another radon source in that building other than what we're talking about here, the tunnel, that was clearly re-mediated.

So, I guess I would agree with Joe that it's not an ironclad case, but there is a reasonable weight of the evidence here, I think.

MR. FITZGERALD: I think we're on the same page rather than --

CHAIR BEACH: Yes. And I did forget your data points from the presentation at the last Work Group meeting where you did show a few samples. So, I neglected that.

So, do you want to leave this open, close it?

I think that NIOSH, you correct me if I'm wrong, you have an obligation that if new information comes in, you go back in and look at what it effects.

DR. ULSH: Absolutely. Yes, absolutely. If anything comes to light that we're aware of -- I mean that we see, we will certainly --

CHAIR BEACH: Right. Right.

DR. ULSH: And that's the whole purpose of an 83.14.

MR. KATZ: Yes, with a new 83.14 is what you're talking about?

DR. ULSH: Right. Yes.

CHAIR BEACH: Yes.

MR. KATZ: Do you understand that, Josie?

CHAIR BEACH: Yes.

MR. KATZ: That would be a new 83.14, right?

CHAIR BEACH: Right. Okay.

So, all in favor of closing the radon post-1980, Brad?

MEMBER CLAWSON: Yes.

CHAIR BEACH: Paul, Phil?

MEMBER SCHOFIELD: Yes.

CHAIR BEACH: Paul?

MEMBER ZIEMER: Yes.

CHAIR BEACH: Okay. Then we'll consider that closed based on previous information.

MR. KATZ: We got through that issue in blazing speed.

CHAIR BEACH: Yes. Now, we're going to go ahead and just -- we're going to juggle. I know Brant needs to leave at 4:30 today, so there's a couple of things we should be able to close quickly also.

Let's move down to high-fired Pu-238. And that should be a relatively, should be a relatively simple discussion.

I know that, Jim, you were going to look at some information from our last meeting on the modeling. I believe that was the issue, was the modeling.

MR. FITZGERALD: I believe it was competing models for Mound's bioassay.

CHAIR BEACH: Yes.

DR. NETON: Yes, I don't -- I mean we did have an internal discussion about the models. And where we ended up with, we believe that the models that we have are adequate in our reconstruction. However, you know, there may be tweaks that could be involved in looking at additional cases if need be.

But it's a Site Profile issue in our opinion, not an SEC. We have sufficient data to -- we developed a model for doing sufficiently accurate. I believe SC&A's position is that we have not examined the universe of all possible models.

We're saying we could do that, we don't think we need to, but at any rate that would be a Site Profile issue.

MR. FITZGERALD: I don't think we were proposing the universe. I think we had a specific -- we called it J or K. I can't remember which is which, but one was -- we felt was more conservative.

And I don't disagree that we're into TBD space, but I think the loose end was that I think NIOSH was going to examine the two and come back with some feedback to the Work Group.

I mean that was, you know, nothing any more than that.

DR. ULSH: Well, I think that perhaps a could provide a little bit more information.

What we're talking about here for plutonium-239 at Rocky Flats when this came up, we were talking about high-fired plutonium. And basically this is plutonium that has been exposed to high temperatures and that would make it refractory. In other words, insoluble.

So, it's got some parallels to the tritide issue. Although, I don't want to go there.

So, that's plutonium-239. And what we're talking about here is the analogous position or the analogous issue with plutonium-238.

There were processes at Mound that would certainly subject plutonium-238 to high temperatures and make it high-fired.

So, you know, the question naturally came up would this lead to similar problems?

Now, even though they're both plutonium, there's a big difference between plutonium-238 and plutonium-239.

Mainly, the specific activity is much, much higher for plutonium-238. So, it tends to break down by itself just due to the faster radioactive decay. And that leads to some differences between high-fired plutonium-238 and high-fired plutonium-239.

Now at Mound, the processes that would have lead to high-fired plutonium-238 by and large were the plutonium microsphere project that they used for the space program where they were generating power sources for the space program.

And what you have there is

plutonium-238 microspheres dropped through a plasma torch which of course high fires it, and could have led to the formation of high-fired plutonium-238.

Now, what I would bring to your attention is that certainly the people who were involved in the plutonium microsphere project/program producing these microspheres, would have been on a plutonium bioassay program. I hope that we can all agree on that.

So, the people that were actually making these things, I would say by and large there's going to be a wealth of plutonium bioassay data.

We have looked at a number of cases and we don't see anything that indicates to us the kind of behavior that was observed down at Los Alamos, which is kind of the genesis of this issue.

However, I think we can say that if we came across a claimant where their bioassay data indicated that kind of behavior, sure, I think we could consider that for that.

I hope that that's sufficient for us to come to an agreement on this, but --

MR. FITZGERALD: Well, I think that's a reasonable approach if that can be affirmed as something, you know, a commitment to look at. It's Type L?

CHAIR BEACH: Well, Type L versus Type J.

MR. FITZGERALD: That would be in the arsenal of a dose reconstructor if they saw something that did not track with the usual model.

DR. NETON: I think there's been some confusion of how we approach dose reconstructions.

I mean if the bioassay data were there, we would not ignore it and just blindly apply this more soluble form.

We would be obliged to use bioassay data for dose reconstruction. So, we would do that.

What we really were talking about earlier was what the default would be if we didn't know.

For those cases where they have an occasional routine bioassay sample, you know, our dose reconstructors need to have some default to hang their hat on and that's where we would use it.

Certainly not if there was evidence to the contrary. We wouldn't use that default.

DR. ULSH: And that was kind of my purpose on bringing up this point about the workers who were involved in the microsphere program are going to be workers for whom, in general, there is a wealth of bioassay data.

And if they exhibited this kind of behavior, Type -- what did we call it? Type L?

CHAIR BEACH: Type L versus Type J, yes.

DR. ULSH: Okay. Whichever one it is that's insoluble.

CHAIR BEACH: J.

DR. ULSH: If they exhibited that kind of behavior, well, sure, we would use the bioassay data that's there and model it that way.

CHAIR BEACH: Well, and I guess what the Work Group asked for and what you agreed to was to bring to the worker what approach, to look at both of them, and then to bring to us the approach that you would actually take for dose reconstruction.

So, that was the discussion that had gone -- I mean we had just gotten to that small point.

And to close it out after several Work Groups, that was the end point to be able to bring that to closure.

DR. ULSH: How about this?

We can, you know, at the conclusion of this process there's going to be a pretty large edit to the Mound TBD, Mound Site Profile, and it's going to incorporate all of the results from this process.

We could modify the internal TBD to talk about this issue that the microsphere program generated high-temperature plutonium particles.

If a worker was involved in that and had bioassay data that suggested this more refractory form, that should be considered.

That could be the approach that we would take for this issue.

DR. MAURO: I have just one question.

The dose reconstructions that you have done and the data that you haven't done, is the retention function behaving in a way that you weren't expecting to see.

In other words, does it look like your L or does it look like your J?

DR. ULSH: Well, there was some discussion about that.

DR. NETON: Nothing looked like J, - I don't think.

MR. FITZGERALD: There were a couple of instances that Joyce raised and --

DR. NETON: I think there were slightly --

MR. FITZGERALD: Right.

DR. NETON: There were some cases with slightly longer half-lives in the L model that we developed, but nothing in my opinion that resembled a very long build-up time that you see with Type J at Los Alamos.

And I'm recalling now that I committed to look at the difference between those two and we had done some calculations and the Type J model relies on such a large extra dose that I don't think it's really appropriate to be used.

Additional dose it's at is not appropriate to be used at Mound based on the data that we see.

MR. FITZGERALD: Well, unless the

bioassay --

DR. NETON: Unless the bioassay, but we have not seen anything remotely resembling a Type J in my opinion at Mound.

In fact, that Type J material was generated under some very specific experimental conditions at Los Alamos. Maybe it was radioactive testing or something of that nature.

MR. FITZGERALD: So, I think that the proposal may be sort of a footnote that it's available in a TBD.

DR. NETON: Yes, we certainly make the dose reconstructors aware of the fact that there may be other instances out there, be careful when you're reviewing the bioassay data not to blindly apply the default.

And again most of the time this would be where people had no positive bioassay. I mean you have to have some default.

For those who have positive

bioassays, usually there is a fairly rigorous follow-up with multiple samples where one could establish the clearest pattern.

That's what we would use, the person's own individual clearance.

CHAIR BEACH: Okay. Anything else?

Then I would propose we close this based on the discussion and the revision of the TBD to make both available to fit the circumstance.

Brad, do you agree with closing?

MEMBER CLAWSON: Yes.

CHAIR BEACH: Bob, Phil?

MEMBER SCHOFIELD: Yes.

CHAIR BEACH: Paul?

MEMBER ZIEMER: Yes.

CHAIR BEACH: Okay. So, we have closed Issue 9. The next one is adequacy/completeness of internal dose. I'm actually going to tie that with the roadmap.

I had first thought I was going to close the roadmap issue, but realized that there were four papers written for data adequacy and completion.

So, there's actually four still on the table. Some of the questions have been answered and some of them have not.

The first three papers were -- the answer was the roadmap for one, three, seven and eight, I believe.

So, I've decided I'm not going to close that until we have a written response from NIOSH and making sure that all four papers have all been -- all the issues have been answered completely.

And with that, I'm going to turn it over to SC&A.

MR. FITZGERALD: Yes, with a clarifying comment that, you know, going back to January 5th and 6th when we sort of waded into all those papers and we made it clear that we needed a way to expedite or facilitate some agreement, and that's where the charge from the Work Group came to SC&A to actually identify.

And we were talking about the presence of these nuclides based on the King report and other sources at Mound.

And I think Brant's response was that doesn't connote necessarily exposure potential. We went back and forth on that.

I think finally we just said, well, what we can do to move this thing forward is why don't we identify what we would say would be the nuclides from which exposure potential based on the operational information, whatever, was significant enough that we would identify that to NIOSH.

And I think the Work Group wanted NIOSH to then respond as to why this would not in fact be exposures to which bioassay would be warranted or which we don't see any evidence of actual bioassays being conducted.

And I think that doesn't supplant some of the other issues that were raised in this paper, but I think it was trying to get to the heart of the question which is quite apart from the presence necessarily -- the roadmap was focused on the presence of the -- potential presence of these nuclides, but I think it was made clear it did not necessarily mean there was an exposure pathway.

And what the Work Group wanted us to do was, okay, let's get beyond the King report and the roadmap and let's talk about which nuclides would exemplify the potential that we're talking about and to provide that to NIOSH so they could respond as to why these were not in fact valid examples of exposure potential historically at Mound.

And that's where it was left. I think that White Paper was generated -- and I'm trying to recall. Maybe early May finally it got out at DOE and got to the Work Group and NIOSH sometime in May.

CHAIR BEACH: June.

MR. FITZGERALD: Was it June?

CHAIR BEACH: It was June.

MR. FITZGERALD: Oh, okay. So, early June.

So, I'm not telling you that you necessarily have had it long, but certainly that's what the genesis of our identifying those sources to you are. So, that's where it stands right now.

Bob Bistline is on the phone to -- if there's any clarifying questions or whatever. But again we have not seen a response, so we're pretty much where we were in terms of putting this paper out.

CHAIR BEACH: Well, and I did ask Bob Bistline to kind of go through the first three papers that were out -- I believe 2009, April of 2009 they came out -- to kind of give us an idea of what still remained unanswered.

And, Bob, I don't know if you're ready to do that yet. Bob, are you on the phone? Bistline?

DR. BISTLINE: Yes, I am here.

CHAIR BEACH: Oh, great. Glad to

hear it.

DR. BISTLINE: Yes, I could try to go through it some, but I guess the first thing would be to look at the -- some of the issues that were brought up in those papers.

The one paper was the internal dosimetry -- Mound internal dosimetry data adequacy, and the other one was completeness, Mound dosimetry completeness, and the other was the Q&A that was produced back in April of 2009.

And some of the major issues that I think need to be brought up that never have really been addressed to our satisfaction have to do with things such as the polonium low recovery that the issue is dealt with in the adequacy paper rather extensively. I think it's Pages 8 through 10 or 11.

And it has to do with the fact that the polonium recovery in bioassay was ten percent or less. And the issue was -- it gets into that ten percent -- having been a DOE program manager over internal dosimetry if I had a bioassay -- saw bioassays coming in at ten percent, even 25 percent, I would have said that the program was pretty broken.

And I think that's pretty well reiterated by the MARLAP statement, Multi-Agency Radiological Laboratory Analytical Protocol Manual of 2004 where it says low yield, a very low yield usually indicates a procedural failure caused by incomplete or unsuccessful chemical separation, matrix interference, missing reagents or the inclusion of a key element in the sample processing. And a low recovery of the direct plating method indicates a failure in this process.

It was not appropriate for metabolized polonium, and this goes back to some animal studies that were done where it was recommended that because of the uncertainties they found with the primates, that recovery was ten percent or less.

So, our question has to do with the efficiency here of polonium recovery. And we don't feel that this has been fully answered.

Another issue is on other nuclides, dealing with other nuclides. And this was discussed fairly lengthy in the QA paper of 2009 along about Pages 15 and 16 where the MWJ report indicated possible problems with completeness of data and with quality or usefulness of the data entered in the other radionuclides.

This deals with things such as cesium-137 bioassays until -- there were no cesium-137 bioassays until 1993, but there was work being done in 1968-1969 time frame.

And cobalt-60, NIOSH keeps referring to them as trace quantities, but you have to -- with cobalt-60, for instance, there was research and production. And it shows up in soils later on and is brought out in the adequacy and completeness paper that was published in June, the fact that cobalt-60 was found in soil. So, this raises a very serious issue with regard to other radionuclides.

There's the issue of radium, actinium -- radium, thorium and actinium also as a third issue that the data adequacy paper addresses.

And this gets into the fact that there's a real question about equilibrium with the -- using the radium extraction and differential counting process that was used for -- to measure the radium daughters of thorium.

And the fact that there is question as to whether the equilibrium was established and whether all of the alpha emitters were captured with the same efficiency. And we do not feel that this has been adequately addressed as yet.

And this brings up another issue, and that is that there were 238 samples of Pu-238 during the SEC time frame. And 48 samples during 1960 to 1967, which is after that.

So, you're still -- there were more samples actually for Pu-232 during the time of the SEC than there were during the time of 1960 to `67, which was after that and wasn't included in the SEC.

And the same is true for thorium-230. During the SEC there were 180 samples, and there are no samples during 1960 to 1970 when thorium-230 shows up in production processes.

So, these are some highlights of some of the issues. We get into the issue of inconsistency. We have a real problem with the inconsistency that's shown here.

The Dayton labs, MCC, were granted an SEC for their polonium process. And yet during the -- this was transferred over to -- this process was transferred over to Mound and used basically identical processing. And yet there's no -- there's push not to treat it in the same manner as it was treated at the Dayton labs.

Then I also have noted here that the protactinium-231 and thorium-232, that in 1956 through -- 1956 through 1959 there were bioassays. And in 1970 -- but there was also processing and the use of this Pa-231 during 1970 to 1979, and there is no bioassay data from 1959 through 1993 although there was indication that it was being used at the site. And thorium-232, over 117 leaky drums outside the Building 21 as late as 1973.

So, these are all issues that I see as concerns on our part and I -- just a second here. Let me get my paperwork in order.

From our perspective, SC&A's perspective technical review involves a critical investigation of the programs effective based on available documentation.

And we feel that the treatment of the King report is something that these materials were not just episodic.

SC&A sees that there is no reason to waste further time and resources searching for documentation to substantiate this, that episodic use could certainly explain fluctuations in the number of bioassay samples for particular radionuclide from month to month or year to year or even decades without specific bioassay data, but there's available evidence indicating active use of these isotopes was taking place.

So, I think that kind of covers the majority of the issues that are brought up. And sort of in summary, we feel that the dosimetric significance in terms of the compensation program is not defined by the Energy Employees Occupational Illness Compensation Act or the associated rules.

There is no de minimis dose specified. And the dosimetric significance was therefore determined based on the requirements of bioassay sampling at 100 millirem CED that the radionuclides defined as the nuclides of dosimetric significance during the pre-1989 dose assessment project at Mound and the sensitivity of Probability of Causation codes.

Clearly, alpha emitters emitting radionuclides such as radium-226, actinium-227, thorium-228, thorium-230, Pa-231, thorium-232, uranium-233, uranium-234, 235, 238, americium-241, curium-244 are of dosimetric significance in compensation.

And so we feel that this -- the lack of monitoring information and a way in which the proposed method of trying to treat these by issues such as gross alpha, which has -- we feel has real limitations and as described in the paper, has real concerns.

And there are examples in the June paper. And I'm not going to get into all those examples that are cited in our June paper, but there are examples of potential exposures that occurred. And these examples are engineering controls, work practice controls, safety filter and explosions and fires broken into four sections.

And these I might just point out that one of the questions -- statements by NIOSH has been that these -- many of these were episodic, they were small samples, they were encapsulated samples or they were sealed sources.

A couple of these examples actually point out that there were encapsulation sources and sealed sources that actually leaked and there were exposures to individuals even with those.

And so we feel that there really needs to be a closer look at consideration of these possible exposure potentials that existed and were examples of accidents occurring -- took place that haven't -- that were being questioned on the part of SC&A's considerations.

So anyway, I think that kind of covers it at this point.

CHAIR BEACH: Thanks, Bob.

Hurry up.

DR. ULSH: My turn?

CHAIR BEACH: You have --

DR. ULSH: All right. Yes, it might take me 20 minutes.

To go back and kind of give you a history of this whole issue, data adequacy, data completeness, data integrity, Bob mentioned that there were three papers issued.

I believe that those are the original three SC&A papers that were issued on those topics.

We've responded to each of those papers. We have written response to each of those three.

In fact, by my count we are now in the eighth iteration on this issue depending on how you count an iteration. That's how I count it.

And then after we responded to those three, SC&A issued a report this past -- I guess it was released in June. That's the date that Joe gave. That sounds right to me.

And so I came today prepared to talk about that June paper since that's the latest one. We've already responded to the earlier ones, although it sounds like we may need to do that again if there are outstanding issues.

So if you look at this June report, Joe mentioned in his discussion and it's also mentioned explicitly in SC&A's June report, that the Working Group tasked SC&A to come up with examples of situations -- and I think that this was specifically tied towards the issue of these exotic radionuclides where there were scenarios where SC&A felt that there was an exposure potential, but then that there was no bioassay to correspond to that situation.

That's the way I read it. That's the way it even says it in the report. So, I think there's a couple of problems here with this report. And I'm only going to, in the interest of time, have time to talk about a few of them today.

The fundamental problem is the continuing interpretation of the King report. And Bob mentioned it.

We have said before and we'll say it again, it doesn't reflect the episodic nature of the programs that occurred at Mound. It was made for D&D. It was made to give people during D&D, an idea of what to include in their RWPs. When they say you must sample for these radionuclides, here's the universe that you must sample for.

Yes, it does show what radionuclides were present in these rooms, but it doesn't in and of itself establish an actual exposure potential. You have to consider what was actually done in these programs.

And let me give you an example that Bob already talked about, and that's the Cotter concentrate program where they were trying to isolate protactinium and ionium from what was called Cotter concentrate.

And based on the King report and the roadmap, SC&A lists a gap in bioassay from 1970 through `79.

So, I guess what you're saying is there should be some bioassay for each year or each period there in 1970 to `79.

Now, we interviewed the principal that was involved in this Cotter concentrate program. And he states that they only did work with this material in the mid-1970s. The mid-1970s, not 1970. The material came on site and sat in drums until the mid-1970s.

So, from 1970 up through when they started working with this material, I wouldn't expect bioassay, but it's listed as a gap in SC&A's report. And I present this only as one example.

Now, given the way things have moved today where we have some follow-up items, I came in thinking that we've got to wrap up everything by the August Board meeting, but it sounds like we need to prepare a written response to this report.

So, we will do that. We will talk about this example and many others in here, but that's just one example to show you what I'm talking about.

Now, in terms of specific examples that were cited in SC&A's June report, I'd like to walk through a couple of them and point out a few things just as examples again.

On Page 13 of their report they talk about a document authored by someone -- I can't really correctly pronounce his name, but that's in SC&A's report.

And they talk about on October 17th, 1977, safety was notified by engineering of their discovery that an exhaust duct from two fume hoods located in E-107 was tied into the building's general re-circulating room air system. This could be a risk of potential exposure to building occupants.

There's a little bit more here in

the quote. I would refer you to SC&A's report to get the complete part, but now let me point out something that SC&A did not mention in their report.

There were no radioactive materials in E-107. So, I fail to see how this is an example of an unmonitored exposure potential.

Similarly, if you go down to another example on the same page, they --

CHAIR BEACH: Are you on Page 14?

DR. ULSH: I am on Page 13.

CHAIR BEACH: 13. Okay.

DR. ULSH: They also talk about a reference from a report authored by someone named Butz in 1963.

And I pulled up this incident report and here's what I found that's not mentioned in SC&A's report: No property damage, lost time or personnel exposure resulted from the incident.

So, again I would ask how is this an example of an unmonitored exposure potential?

Let's see. So, I guess I'm a little confused by that. Now, on Page 14 they mention a Bigler report from 1960. And the quote that SC&A gives, it was obvious from this investigation that the facilities for performing the work done in R-149 are inadequate. Contamination levels have been high in this lab at various times since this program began.

Then they give a little bit more. And I pulled up the incident report, and here's the part that was not quoted: The incident did not result in any injuries, radiation exposures to personnel or loss of equipment.

This is in the very documents that SC&A is citing in support to show examples of unmonitored exposure potential.

Madding and Carfagno on the same page, Page 14, they talk about a dry box incident and they give a quote: There's a striking similarity between this incident and the one that occurred in R-127, 149, and they give a date. I refer you to them for the quote.

Here's the part that was not quoted: No significant personnel exposure and no injury occurred.

So, again, how is this an example of an unmonitored exposure? It's not.

Now, when we go back and write our response to this, we're going to pull up every citation and we are going to pull out and determine whether or not this was an example of an unmonitored exposure potential.

I've already started this. I pulled out all the incident reports and I looked at a list of personnel involved.

And so I asked someone to go into the MESH database and determine whether or not bioassay is present.

The first thing to note is that almost all of them -- again, this is a very quick first pass.

Almost all of them involved plutonium-238. They do not involve these exotic radionuclides. That's one thing.

The next thing to note is that in almost all of them there were bioassay data within days of the incident in question.

So, again I would ask how is this an example of an unmonitored exposure potential? It's not.

So, this is just a preview of how we're going to respond to this report. There are a number of programs that are listed later in SC&A's report. Bob mentioned a few of them.

This is just going off the fly from what Bob was talking about, because again I wasn't coming here with the idea of responding to those earlier reports because we already have.

But just off the top of my head, you know, Bob mentioned some problems with radium, actinium and thorium. That was the basis for the SEC being designated 1950 to `59.

Now, early on the Working Group had a question about was there an exposure potential to these radionuclides after that?

And we have already covered this, too, but, yes, in the early years of the 1960s, I don't remember the exact year, maybe `62, `63, I don't remember exactly, they opened up a capsule of this material. And I can't remember exactly what they did with it, but we interviewed the guy who did it.

It was done inside a hot cell. There was no exposure potential. It was a completely isolated environment. So, this radium, actinium, thorium question has already been covered.

Bob also mentioned inconsistency between Monsanto Chemical Company where he designated a polonium SEC and Mound Lab where we didn't.

Well, again I would say we've already got an SEC from 1950 to `59. You're arguing that we should take polonium doses away from the non-presumptive cancers?

That doesn't make any sense. That's not claimant favorable to do that. Furthermore, the processes, the polonium processes were not identical between Monsanto Chemical and Mound Lab.

The very reason that they designed the T Building -- I think it was the T Building -- the way that they did, was because at Monsanto they had a problem with beta and gamma activity among the activation products in the cans around the business slugs that they used to generate this polonium.

Therefore, they made this a remote operation and made it contained. So, right there is a significant difference.

Let's see. I guess that's really all I have right now to get ahead of the Working Group because I know we're going to be tasked to write a response here.

You've seen the way we do this. We're going to go through and we're going to pull this report in and we're going to address it point by point by point, but here's a preview of what we're finding. These are not examples of unmonitored exposure potentials.

MR. HINNEFELD: I want to make sure that we're clear on everything here because Bob started by saying several items from the first three reports have not been addressed satisfactorily even though we've responded.

So, is there a comprehensive list of those things other than what Bob gave on the phone today?

CHAIR BEACH: So, let's go with the latest report, the June report, 2010, answer those questions, and we'll see where we are with the rest.

MR. HINNEFELD: So, we owe a response on the June report.

CHAIR BEACH: Yes.

MR. HINNEFELD: But my question still stands, is that in addition to the June report Bob said there are these other issues from these earlier reports that we don't feel have been answered satisfactorily.

CHAIR BEACH: Right.

MR. HINNEFELD: Do we have that in writing?

CHAIR BEACH: No. And what I was going to say is I think SC&A owes that to NIOSH, what's still outstanding.

MR. HINNEFELD: Okay.

CHAIR BEACH: But I think the June report may take care of most of it, but it may not.

MR. HINNEFELD: If they're lacking on the June report, then we'll respond to the June report.

CHAIR BEACH: Yes.

MR. HINNEFELD: And if there's anything that's outstanding that's not referenced in the June report, then we would need that --

MR. FITZGERALD: We need to highlight those. So, I think there's a bit of a parsing.

MR. HINNEFELD: Okay. That would help. That would help if you'd write that.

MR. KATZ: Bob, do you have any -- I don't want to cut you off.

Do you have any reaction to Brant's comments?

Bob, you might be on mute.

MR. FITZGERALD: He just melted into his chair.

MR. KATZ: Bob, are you still with us? Bob Bistline?

DR. BISTLINE: Is it on now?

MR. KATZ: Oh, yes. There you are. Thank you.

DR. BISTLINE: Oh, okay. I just turned it off, I guess. Okay.

Yes, I think that most of the issues that I brought up in the earlier reports are also reiterated at one point or another in the June report.

And so I think that if they cover the June report, why that will probably take care of most of the issues. Although, there's more explanation in some of the earlier reports of those issues.

One of the things that I would point out is the quote from the King report as to what the reason for -- and, Brant, you're right, you know, it was done for purposes of D&D, but it does state, and it stated in this report, all dates represent the duration of actual usage of radioisotopes in their respective projects.

And it's clearly understood that residual amounts of these probably still exist in floors, walls and ceilings and should be considered up to present in every case for decontamination work.

So, you know, that's true. It was mainly done for that, but it does state categorically that all dates represent the duration of actual usage.

DR. ULSH: So, for example, I assume the King report was Building 21 as the storage location for the Cotter concentrate that came on site I guess in about 1970. And they had it there through about 1979.

DR. BISTLINE: Right.

DR. ULSH: So, was it there? Yes.

But again if the drums are sitting there from 1970 up through the mid-1970s, and in the mid-1970s they took, I think they said, like maybe three drums out of the 1,000 to see if they could work with it, there is no need for bioassay from 1970 up until the date that they took that drum and cracked it open and started working with it.

And if you're expecting to find based on the King report the period of active usage, 1970 to `79, and you're expecting to find bioassay in the first part of the `70s, you're misinterpreting the King report.

You have to establish that there is an exposure potential. And that's just one example, by the way.

DR. BISTLINE: Okay. Well, I can buy that point on that one particular issue.

MR. FITZGERALD: The only thing I would add is that when you go through and go through item by item, which is what was intended by the Work Group, I would be careful about, you know, the exposure was significant. I think I heard you say that.

I think the question we posed on this thing, was posed by the Work Group is, is there nuclides for which there's an exposure potential. And whether or not the resultant exposure was significant or not is less important as to whether it was an exposure pathway which is manifest either in the event that occurred or other instances that suggested that, you know, even though it shows up in the King report, here's an instance.

This is what we challenged, I

think, Bob with doing, is there an exposure potential by virtue of the exposure that occurred by an event, you know.

I'm just trying to shed some light on the fact that there was an avenue by which exposure took place.

I heard you say something, you know, you came up with a quote from the report and was a -- sort of a qualifying statement that however the exposure wasn't significant.

I think what we're after is that the fact there was in fact exposure quite apart from how significant it was.

And I think when you go back and start itemizing this thing --

MR. HINNEFELD: I think just before we carry this much further, I think that it's a fact, Brant, that we need to be cautious about a site report, an incident report that includes what essentially is a boilerplate statement. No personal injuries, no property damage, no significant exposure.

Because quite likely that was a criterion that the site said, okay, we want to know if there was property damage, we want to know if there were personal injuries, we want to know if there was significant exposure, and they may define that in some fashion that's absent to us.

And so I think we need to be cautious about relying on that statement in an incident report.

DR. ULSH: I think we need to --

MR. HINNEFELD: Okay. So, that's the only statement I want to make, and then we'll address the rest of it in our response.

DR. BISTLINE: This is Bistline.

And, again, what you guys are saying was something that I was also going to bring forward. And that is that, Brant, you were saying that there was no exposure, but the point of those examples was whether there was exposure potential and with these examples that were given.

So, it's not necessarily that they didn't have exposure, but that the potential was brought out by these examples.

DR. ULSH: Okay. First of all I will posit that there were incidents. We have never said otherwise. There were hundreds if not thousands of incidents, but these are being presented as examples of situations where there was an unmonitored exposure potential.

Not only was it -- they don't fit the bill on at least two counts. Number one, they don't involve exotic radionuclides. And in some cases, don't involve nuclides at all. Number two, they're not unmonitored.

They've said right in the report in many cases, we sent them for urinalysis, we verified that in MESH or they took nasal swabs.

So, just the fact that incidents happen is not sufficient to demonstrate that there was an unmonitored exposure potential.

And as Stu said, we will address this in more detail in our written response.

DR. MAURO: You'll be in a position to be quantitative. If someone makes a claim that there was not a significant exposure, you have the information available to you to say what does that mean and why did they come to that conclusion and that you agree that, yes, based on these data where there is a bioassay sample and there is a swab, swipe samples or whatever, air samples, that would be, you know, that would put the nail in.

MR. FITZGERALD: Let's just wait for the written response.

CHAIR BEACH: Yes, and that was going to be my suggestion. Also, I want to just touch briefly on D&D.

So, D&D has been one of those that we haven't spent a lot of time, Work Group time on.

The last meeting on January 6, we asked NIOSH to give us a report. And that report was delivered on April 2010.

But to be fair, I don't think anyone has really had a chance to review this. And what I would like is to ask SC&A to look at this report and give the Work Group a recommendation on what's the path forward for D&D.

We've already touched briefly on tritium samples bioassay during the D&D time frame. And I'm not expecting it at this meeting, but I think that we do owe this paper and a future report on D&D and what the Work Group should do.

I'll just point out on Page 5 NIOSH recommends that we close this issue. I don't feel comfortable with that until I have something from SC&A giving us an idea of if we have anything on the D&D issue and the time frame.

So, that's my recommendation unless there are other comments. That's our last item.

MR. KATZ: Do you want to talk about what your plans are as to whether you want to present anything at the upcoming Board meeting?

CHAIR BEACH: Well, right now the only thing that we can report on is -- I mean I can give a report now --

MR. KATZ: A status report.

CHAIR BEACH: A status report. And then radon we close, but we don't really --

MR. KATZ: So then, for example, we like to keep the petitioners informed. We should let them know this is not queuing up for a vote at this Board meeting on Mound. And we should let them know that so that they're not expecting something different.

CHAIR BEACH: Right. Correct.

MR. KATZ: Okay. And then we probably don't need quite as much time.

CHAIR BEACH: We have an hour.

MR. KATZ: We have set aside at least an hour, and you may not even need all of that to report out.

CHAIR BEACH: Probably not.

MR. HINNEFELD: Do you feel like Brant's presence is needed to do the status report?

Brant is making some rather heroic travel arrangements to get there for this on Thursday.

MR. KATZ: That's a good point.

I was going to say, Josie, if you're comfortable reporting out and --

MR. HINNEFELD: Jim and I were here.

MR. KATZ: Jim and you, I think that will cover it and you're off the hook.

CHAIR BEACH: Yes.

MR. KATZ: And in fact we might even be able to move -- well --

CHAIR BEACH: However you want to do this, Ted.

MR. KATZ: I don't know whether to move Mound or not at this late date.

CHAIR BEACH: Well, you had

mentioned that based on --

MR. KATZ: It depends on what happens with --

CHAIR BEACH: So, I think we should go ahead and close then unless there's any other --

MR. KATZ: So, are we adjourned?

MS. HOWELL: I just was wondering if we have any idea about timeline for future meetings.

CHAIR BEACH: Future meetings.

MR. KATZ: Thank you.

CHAIR BEACH: We've got -- I can just go quickly through we have action items for NIOSH on --

MR. KATZ: And SC&A.

CHAIR BEACH: Well, to start with on neutrons -- actually, without going back all through these, that just depends on where NIOSH is and how long --

MR. HINNEFELD: I think we're hard pressed to make some type of estimate. I think we're hard pressed to make one today.

MR. KATZ: Yes. What I was going to suggest is at the Board meeting we're going to be talking about scheduling things. And maybe if NIOSH can give some thought to these Mound issues and SC&A for their next deliverables to be ready at the August Board meeting to discuss where they might be ready, then when we have a Mound discussion and we could also talk about scheduling the next -- because we'll have time to schedule Work Group meetings at the August 3rd meeting.

CHAIR BEACH: Yes. And I'll be out from September 6 to October 9. I'll be gone. So, it won't be during that time.

MR. KATZ: So probably after, right?

CHAIR BEACH: After I --

MEMBER ZIEMER: My calendar, too, is pretty much shot.

CHAIR BEACH: So is Paul's.

MEMBER CLAWSON: When do we have to have our travel and stuff in by then?

MR. KATZ: Well, that's the other thing. First of all, we're adjourned, I think.

CHAIR BEACH: Yes.

MR. KATZ: Okay. So, thank you everyone that's hung in with us on the phone.

(Whereupon, the above-entitled matter went off the record at 4:30 p.m.)