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

+ + + + +

ADVISORY BOARD ON RADIATION AND
WORKER HEALTH

+ + + + +

WORK GROUP ON MOUND

+ + + + +

WEDNESDAY
JANUARY 6, 2010

+ + + + +

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

MEMBERS 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*
BOB BISTLINE, SC&A
LIZ BRACKETT, ORAU Team*
RON BUCHANAN, SC&A
MEL CHEW, ORAU Team
JOE FITZGERALD, SC&A
STU HINNEFELD, OCAS
EMILY HOWELL, HHS
KARIN JESSEN, ORAU Team
JENNY LIN, HHS
JOYCE LIPSZTEIN, SC&A*
ARJUN MAKHIJANI, SC&A
JOHN MAURO, SC&A
JIM NETON, OCAS
EUGENE POTTER, ORAU Team*
KATHY ROBERTSON-DEMERS, SC&A
WARREN SHEEHAN, Mound worker*
DON STEWART, ORAU Team
BRANT ULSH, OCAS

*Present via telephone

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


Call to Order 5

Roll Call 5

Stable Tritium Compounds 9
Bob Bistline

Brant Ulsh 13

Discussion 15

Data Adequacy and Completeness of Internal Dose Records 60
Brant Ulsh 61, 66, 75, 126, 146, 192

Bob Bistline 62, 124, 138, 188

Warren Sheehan 72

Discussion 69, 77, 128, 150, 193, 200

Action Item - SC&A 104

Action Item - Brant Ulsh 134

Don Stewart 143

Action Item - Brant Ulsh 181

Action Item - SC&A 182

Shallow Dose 240
Ron Buchanan

Discussion 243

High-Fired Pu-238 246
Brant Ulsh

Joe Fitzgerald 248

Discussion 251

C-O-N-T-E-N-T-S (CONTINUED)


| | |
|---|---|
| Road Map" Integrated: Issues 1,3,4,7 & 8<br>Kathryn Robertson-DeMers | 268 |
| Discussion | 269 |
| PAAA Violations: Issue 21<br>Kathryn Robertson-DeMers | 284 |
| Brant Ulsh | 286 |
| D&D: issue 10<br>Joe Fitzgerald | 300 |
| Brant Ulsh | 303 |
| Action Item - Brant Ulsh | 330 |
| Unfinished Issues | 332 |
| Adjourn | |

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, the Mound Working Group. We are just getting started on our second day of this meeting.

We are going to begin again with roll call. Please, for everyone affiliated with the agencies and the contractors, speak to whether you have a conflict of interest as well.

So, beginning with Board members in the room.

CHAIR BEACH: Josie Beach, Mound Chair. No conflicts.

MEMBER CLAWSON: Brad Clawson, Work Group. No conflicts.

MEMBER ZIEMER: Paul Ziemer, Work Group. No conflict.

MEMBER SCHOFIELD: Phil Schofield,

Board member. No conflicts.

MEMBER PRESLEY: Robert Presley, Work Group. No conflict.

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

(No response.)

Okay. Then the NIOSH ORAU team in the room?

MR. HINNEFELD: Stu Hinnefeld, Interim Director of OCAS.

MR. KATZ: No conflict?

MR. HINNEFELD: No conflict. Sorry. I always forget that part.

(Laughter.)

DR. NETON: Jim Neton, OCAS. No conflict.

DR. ULSH: Brant Ulsh, OCAS. No conflict.

MS. JESSEN: Karin Jessen, ORAU team. No conflicts.

MR. CHEW: Mel Chew, ORAU team. No conflict.

MR. STEWART: Don Stewart, ORAU team. No conflict with Mound.

MR. KATZ: And on the line, NIOSH ORAU team?

(No response.)

Are you expecting any folks on the line?

Bob Morris, are you on the line?

DR. ULSH: He would only have been on for the neutron discussion.

MR. KATZ: Okay, right.

Then SC&A in the room?

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

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

MS. ROBERTSON-DEMERS: Kathy Robertson-DeMers. Conflicted.

MR. FITZGERALD: Joe Fitzgerald. No conflict.

DR. MAKHIJANI: Arjun Makhijani. No conflict.

DR. BUCHANAN: Ron Buchanan, SC&A. No conflict.

MR. KATZ: SC&A on the line?

DR. LIPSZTEIN: Joyce Lipsztein, SC&A. No conflict.

MR. KATZ: Okay. Then HHS or other government employees or contractors in the room?

MS. HOWELL: Emily Howell, HHS.

MS. LIN: Jenny Lin, HHS.

MR. KATZ: And on the line?

MS. ADAMS: Nancy Adams, NIOSH contractor. No conflict.

MS. AL-NABULSI: Isaf Al-Nabulsi, DOE. No conflict.

MR. KATZ: Okay. Then, at this point, we don't have any members of the public in the room. But on the line, any members of the public or staff of congressional offices who want to identify themselves?

(No response.)

All right, then, Josie?

For everyone on the line, please mute your phone. Use *6 if you don't have a mute button. You can use *6 again to come off mute, and please do not use hold, but disconnect and call back in, if you need to leave at some point.

Thank you.

CHAIR BEACH: Okay. Thanks, Ted.

The agenda is posted on the web. We are going to make a slight change to the agenda. I know we said we were going to start with data adequacy this morning from yesterday's schedule, but we are going to go back. There's some discussion on the stable tritium compounds that we didn't finish with yesterday. So we will start that discussion this morning, and then go into the data adequacy and completeness.

SC&A I believe is going to tee off the tritium discussion this morning. Bob, if you're ready?

DR. BISTLINE: Yes, okay. I just

want to go on record for SC&A in terms of some of the discussion yesterday with regard to stable metal tritides.

I got the impression from the discussions yesterday that we are looking at hafnium as being the most insoluble tritide, which there is still some discussion, if you look at Zhou and Cheng's 2004 paper.

But, be that as it may, the big issue is I got the impression that NIOSH is treating hafnium as the most insoluble. Then, on the other end of the spectrum is HTO and gaseous tritium, and that everything else, basically, is being handled as an intermediate.

I want to make sure that it is understood that there are other stable metal tritides which really are in the literature that have been studied that are listed as S type tritides. If a person is doing dose reconstruction, that they should be treated as insoluble tritides and not as intermediate or

M type tritides.

There are at least a half a dozen or more that, if you look at the information, are of a stable form, insoluble forms, and need to be handled as stable insoluble S type tritides. For instance, zirconium in Zhou's paper is treated, it says, estimates of the effective dose coefficient based on data for rats receiving zirconium, halnium, and tritium by intratracheal instillation decreased in the order of zirconium greatest, hafnium next, and titanium after that. And overall, the results show that these should be treated as S and not M.

And the same thing goes for carbon tritide, titanium tritide. So there are other tritides out there that have been used throughout the complex. As I say, there are at least a half dozen or more that are well-known that are being used.

I want to make sure that you realize, and we realize, that uranium is not

in that classification. Uranium tritide is fairly soluble and probably is more of the soluble form. But some of these others that are used throughout the DOE complex are of an insoluble nature, and I just want to go on record that SC&A, when we start talking -- Mound is not the end of the trail as far as stable metal tritides is concerned. We are going to be facing this in dose reconstruction when you get into Savannah River, Pantex, Sandia, and other DOE sites.

So I just want to make sure that people understand that this is not the end of the trail, that Mound is not the only one, but there are other facilities where stable metal tritides, and I should also hasten to add that the OBTs, there are a number of those which are also classified as stable and very insoluble forms, and that we can't just treat hafnium as being the only one in the S category, but there are a number of others that also fit into that category.

I think that explains my position on this issue. I just wanted to go on record and make sure that this is understood.

CHAIR BEACH: Okay. Thank you.

Okay, go ahead, Brant.

DR. ULSH: I have Zhou and Cheng Health Physics 2004 right here, and I'm reading from the conclusion on page 5 of 6. It says, "Among these three" okay, first of all, the three tritides that are looked at in this study are hafnium tritide, titanium tritide, zirconium tritide.

"Among these three tritides, hafnium tritide was classified as a type S, slow, material, whereas, titanium tritide and zirconium tritide ranked between type M, moderate, and type F, fast, materials, according to ICRP 66."

That's a direct quote, Zhou and Cheng, 2004.

DR. BISTLINE: Yes, but that is saying from ICRP 66. But if you read in that

same paper, it talks about the effective dose coefficient.

DR. ULSH: Okay. From the abstract, "The doses were on the same order of" -- okay, hold on now. Let me make sure here.

"The doses calculated by ICRP 66 model for all materials were approximately two orders smaller than the doses obtained by the animal studies. This bias was caused by the different intake methods of the ICRP 66 model, inhalation, and in the animal study, instillation. The doses were on the same order while correcting for deposition fractions. The effective doses for hafnium, titanium, and zirconium tritides were" -- I can give you the numbers, but on the order of 5 times 10 to the negative 10, 9 times 10 to the negative 11, and 6.5 times 10 to the negative 10 sieverts per Becquerel, respectively, according to the animal studies.

The bottom line is, even if you

want to call it that, we have information here that gives you exactly how to estimate, how to reconstruct doses from hafnium tritide. At worst, this is a TBD issue. It is not an SEC issue.

DR. LIPSZTEIN: May I? This is Joyce.

I got a Road Map with 75 different forms of stable tritides that people could be exposed at Mound. I don't know which ones are more relevant or which ones were workers exposed to, but only about a dozen of them we have papers that talk about their solubilities. What do we do with the other 60 that we don't know anything about?

DR. ULSH: I think what we do, Joyce, is we go to the literature and we talk to the people who were involved with this. If you look at the publications by Zhou and Cheng and by Yang --

DR. LIPSZTEIN: So Zhou and Cheng have a limited number of papers.

DR. ULSH: Exactly correct.

DR. LIPSZTEIN: And they have studied a limited number of nuclides.

DR. ULSH: Right. They studied the ones were there was actual exposure potential.

DR. LIPSZTEIN: But if you only go by Zhou and Cheng, I think they did a very, very good study. I don't doubt that. But they only have studied a limited number of nuclides, and we have 60 more that we don't know anything about. There is nothing in the literature.

And also, there are some papers that don't agree too much with the place of titanium tritides. I have a paper by Balanov that says it should be type F, while if you read the Cheng, et al., paper, it would be type S, but I'm not discussing this one.

I am more worried about the ones that we don't have papers to assign. So we don't have any solubility studies to assign

anything. So what are you going to do with these radionuclides that you don't have any solubility studies?

DR. ULSH: Well, I would again say you have to consider the exposure potential. The Road Map was built --

DR. LIPSZTEIN: Yes.

DR. ULSH: -- from the [identifying information redacted] document. The [identifying information redacted] document was meant to be, I don't want to use the word "biased", but inclusive, over-inclusive of everything that could have possibly been in any location.

The piece that SC&A continues to not consider from the [identifying information redacted] document is it lists major radionuclides of concern, and the mere fact that a material might or might not have been present, just the possibility that it might have been present in a particular room does not, in and of itself, demonstrate an exposure

potential.

For example, if I take a sample of hafnium tritide from a doubly-contained glove box and I put it in a sealed glass ampule, and I walk it into the next room, that doesn't constitute an exposure potential.

The whole purpose of this program was to identify and manufacture an effective storage mechanism for tritium. In other words, you are looking for a stable compound that will grab the tritium and hold it. And hafnium tritide has repeatedly been told to us by the workers and in the literature that it was the most stable compound. I can't prove a negative. If there are --

DR. LIPSZTEIN: I'll mention one thing --

DR. ULSH: No, let me finish, Joyce. I let you finish.

DR. LIPSZTEIN: Yes, okay.

DR. ULSH: Let me go through --

DR. LIPSZTEIN: Okay.

DR. ULSH: If you could go through the entire periodic table and look at every metal and speculate about the solubility of a particular tritide formed with that metal, I can't prove a negative. All I can tell you is these are the tritides that were used at Mound. There were several that were investigated on a bench scale that does not equate to an exposure potential.

The reason that Zhou and Cheng and others -- and Cheng at least is from Lovelace, so it is part of the DOE complex -- the reasons they focused on the particular tritides that they did is because these are the ones that were in wide-scale use and presented a significant exposure potential.

We investigated a couple that were listed in [identifying information redacted], as suggested by the Working Group and SC&A from our meeting in Germantown, and we confirmed that they were, indeed, simply science fair experiment-type scale.

So, if you guys have evidence that there was widespread exposure potential for some other tritide and that it is less soluble than hafnium tritide, I would gladly evaluate it, but I haven't seen it.

DR. LIPSZTEIN: We have to divide this thing in two fractions. Do we have evidence that there is something more insoluble than the hafnium tritide? Hafnium tritide was treated as type S. We know that its halftime in the lung is longer than the one predicted by type S. But it is okay to treat it as type S because you are not taking into consideration the self-absorption of the particle within itself. So it is okay. You are being very claimant-favorable not to take into account the self-absorption.

So, this way, I don't have any question that hafnium would be assigned a type S. It is okay. It is claimant-favorable to assign to hafnium type S. I'm okay with that.

What I am saying is not that there

are others that could be more insoluble. I think the type S is the maximum you can assign because of the self-absorption. So, if you assign type S, you are being claimant-favorable.

What I am saying is that I got a list of 76, not 75, 76 stable nuclides, and I don't know, and I would like NIOSH to tell me from this list, from the Road Map of 76 stable tritides, which ones were often used at Mound and which ones should we consider also an exposure to type S.

For example, europium tritide, is a possibility of people being exposed at Mound? The DOE, Zhou and Cheng did the work on this one, but the DOE classifies it as type S. Carbon tritide, Cheng and Zhou, they worked with it, and they classified it as type S. Is it a real possibility that the Mound people could have been exposed to carbon tritide? I heard yesterday no.

But what I mean is that Zhou and

Cheng, they worked with some stable tritides and not with everybody, and not with all of them.

I have a list of 60 that there is no study. Some, like scandium tritide, I have a paper by Potter saying that there's type F.

So, the scandium tritide, for example, the in vivo study from Zhou and Cheng should be classified as type S. The in vitro study from Zhou and Cheng, it should be classified as type M. The list from DOE 2004, it should be classified as type S.

So I don't know what was the exposure to this kind of tritide. Is this important? This is something that you have to give us and tell us which from the Road Map are the important stable metal tritides. And what are we going to do if there is no paper on how to assign a solubility to them? Are you going to be claimant-favorable and treat all of them as type S? Are you going to treat them as type M because you don't know? I

don't have this position from you.

DR. ULSH: Okay. To the best of my knowledge, I have no indication that there was any exposure potential at Mound to europium tritide or carbon tritide.

Okay. Let's agree on a definition here before we go further, just so no one gets confused.

DR. LIPSZTEIN: Okay.

DR. ULSH: Stable metal tritide, I am not necessarily equating with only type S. So, for instance, uranium tritide, I think we would all agree is not type S, but I would say that that is a somewhat stable metal tritide.

DR. LIPSZTEIN: Yes.

DR. ULSH: So that is the way I'm going to use the term here for the next couple of minutes. Okay?

DR. BISTLINE: Yes, I agree.

DR. LIPSZTEIN: Okay.

DR. ULSH: So, in that category of stable metal tritides at Mound, off the top of

my head, the ones that I know about, the most widespread are uranium tritide and lithium tritide. Hafnium tritide, as we have discussed ad nauseam here, was very discrete, very small, but not zero.

Mel, am I missing any other major ones? There might be some very minor players.

MR. CHEW: No.

DR. ULSH: But those are the big ones.

MS. ROBERTSON-DEMERS: Yes, you are missing one. You're missing palladium.

DR. ULSH: Okay, correct.

DR. LIPSZTEIN: Do you have anything on lithium tritides in fact? Do you have any papers on lithium tritide or about the solubility of it?

DR. ULSH: Two points. First of all, I don't know if you could hear Kathy. Her voice is not at normal. But she mentioned that I am missing one, and that's palladium tritide, and that's possible. Yes, I do

recall hearing that that was at Mound. I can't recall exactly to what extent, but we could look at that.

DR. LIPSZTEIN: Palladium tritide.

DR. ULSH: Yes.

DR. LIPSZTEIN: Okay, that would be a type S.

DR. ULSH: What was the other one you asked about? Lithium, lithium tritide, yes.

DR. LIPSZTEIN: Yes.

DR. ULSH: It is definitely less soluble than hafnium. I don't know if it's type M or F off the top of my head.

MR. CHEW: It is much more soluble.

DR. ULSH: More soluble, yes.

And then, with regard to your larger question, what are we going to do in a situation where, if there was a tritide with an exposure potential that we didn't know how to handle it?

Let's assume, just for the sake of discussion -- and again, I'm not proposing this, but just to make this clear in terms of an SEC context -- let's assume that there are other tritides out there that are type S. Well, so what? That's not an SEC issue.

So we treat all tritium as type S. I'm not saying we are going to do that, but what I'm saying is that demonstrates to you that it is not an SEC issue. We can argue about it under a TBD context or even under a dose reconstruction context.

For instance, if we've got a claimant where we do a dose reconstruction and he's got tritium exposure, I mean the mechanisms are in place for SC&A to review these dose reconstructions, and you could come up with a possible finding, "Hey, NIOSH, you didn't give this guy potential exposure to uranium tritide or even hafnium tritide." And we could discuss that under the context of a dose reconstruction review. We have an entire

subcommittee dedicated to that.

But even under the worst circumstances, if we accept everything that you say, it's not an SEC issue. We're going round and round in circles on this, and it's just not an SEC issue under any scenario.

DR. LIPSZTEIN: No, it's an SEC issue if you don't know what solubility the tritium compound is.

DR. ULSH: How about type S?

DR. LIPSZTEIN: If you are telling me that, if you don't know, you are going to treat it as type S, then we have to go into the problem of the dose being too high and the source term being too high. That's what you discuss on OTIB-0066.

DR. NETON: I'm confused, Joyce.

DR. MAURO: Maybe I can help out.

DR. NETON: Yes.

DR. MAURO: It goes, again, toward plausibility. Interestingly enough, during the course of this meeting we ran into the

duality of the problem. We noticed when we were talking about radon that there was some discussion that it would be implausible to assume that the doses could have been as high as 10,000 rem associated with some of the measurements that were made.

Now it's my understanding that some of these assumptions regarding tritides, we could talk about those and going from the bioassay data to the respiratory tract dose. If you assume it's type S, it may be on that order of doses of that magnitude.

So we have a very interesting -- and, listen, I'm sympathetic. This business of plausibility, the bounding, sufficient accuracy, and finding that place where you strike that right balance is not an easy thing to do.

Now what we are hearing right now is a strategy that, for this particular application, certainly it is claimant-favorable. It is claimant-favorable off the

charts. But, unfortunately, it is so claimant-favorable that it starts to bring us into the world of plausibility.

I think it is important that we talk about this. It's only fair that we share it with the Work Group that we know that this is a difficult question. When and where and under what circumstances does the issue of plausibility rise and needs to be dealt with? I guess I could put that on the table.

MEMBER ZIEMER: But, John, is it implausible, if you're unsure of the solubility class, to select the higher one?

DR. MAURO: No.

MEMBER ZIEMER: Why is that implausible?

DR. MAURO: It's that the person was always -- here's where I think implausibility comes in, in my view. We have a person that worked there for many years. We know that probably most of the time he's working with tritiated water. But there's

also the very real possibility that, by the nature of his job responsibilities, that from time to time he may have found himself in the situation where he's dealing with one of the intermediary or possibly even the type S, even for a short period of time.

Now we know with type S we're talking about a difference in lung dose of a factor of 10,000. In other words, going from the urine sample to the dose, a 10,000-fold difference.

So it doesn't take very much assumption of how long was the person exposed to hafnium type to deliver a substantially higher dose, even if it was only for a few days. I believe, even if it were only a few days.

So what I'm getting at is that, where I'm heading is, in the end, you may have a person that worked there for five years, where you really don't know when and under what circumstances he might have been exposed

to hafnium, but you agree that, yes, that was possible that some period of time he may have encountered and had to work with hafnium, and some of his intake may have been hafnium, which leads us to a place where, from what I'm hearing, you will assign the entire duration of his exposure, which could be several years, every one of those bioassay samples collected every two weeks are going to be assumed to be due to the intake of hafnium. As a result, you are going to assign to him a dose that is going to be tens of thousands of rads to the respiratory tract, as opposed to 1 rad.

MEMBER ZIEMER: I assume you wouldn't start that assignment until the hafnium work started.

DR. MAURO: Yes, and that would be fairly reasonable.

DR. NETON: But let's back up a little bit, though. I mean you are kind of making a couple of arguments here.

One is that these doses are going

to be extremely large. I think I would like to clarify, yesterday, I mean, it was stated in this meeting that these radon doses from ET1 and ET2 were going to be large. I don't necessarily believe that it was the doses that are implausibly large. I think the exposures have ended up being these huge exposures. That's a different issue.

I think any time you have a valid dose model and it comes out large, it is what it is. I mean on face value we can argue all we want about the technical adequacy of the models. But if it's accepted, then it is a valid model, end of story.

When you get into these tritium tritide exposures, though, what you are arguing is that it's not plausible to give the guy 18,000 rem. Well, in fact, we may or may not do that. If it only takes, like you're suggesting, a couple of days, then it truly is plausible. If two days' exposure puts the guy over 50 percent, we'd probably stop the dose

reconstruction. That is how we do it. These dose calculations are done to the point where you don't waste time reconstructing exposures over 50 years if a very small exposure will put the guy over 50 percent. I mean that's the way the program is set up.

So, by your very argument saying it is implausible he worked five years, but on the other side of the coin you're saying only two days' possible exposure could put that guy into the 50 percent.

DR. MAURO: Oh, no, it's good.

DR. NETON: I'm missing your point here.

DR. MAURO: No, I hear what you're saying.

DR. ULSH: And here's another point that we haven't discussed in turn that is specific to Mound. People who were working with tritium at Mound, as similar to other tritium-type facilities, were giving urinalysis samples, I want to say, at least

twice a week, maybe more frequently, but I would have to look to be sure, so very, very frequently.

If you're talking about a highly insoluble tritium compound, there is a distinctive pattern that you would see in the excretion curve from these people. That is explained in McConville and Woods, 1995.

McConville, we interviewed him. He explained to us that, when the concern about hafnium tritide surfaced in the nineties because of the DOE order that I can never remember the number of and the technology shortfall that existed, he went back through the bioassay records and looked for any possible evidence of exposure to insoluble tritium compounds. He found three.

So I think that needs to be brought to bear here, too, to give you context of how big an issue we're talking about here.

DR. MAURO: I have a problem with the concept you just described. The reality

of the situation is, if a person is simultaneously being exposed to tritiated water and hafnium tritide, and let's say 90 percent of the intake is tritiated water and 10 percent of the intake is tritide, the tritide contribution to what you are going to see in the urine is going to be invisible. It is going to be completely dwarfed.

So, therefore, you would never know. I mean there's going to be an excretion pattern associated with the hafnium contribution that is going to be completely hidden by the excretion from the tritium. So the fact that you don't see patterns in the urine of individuals who are sampled on a weekly basis that would be indicative of hafnium is not surprising because we all know that it is likely that the person, if he was exposed to some hafnium, he probably was also exposed to some tritiated water, and it is going to be completely hidden.

So I don't think you can make your

case that you are not seeing very much because only when he is only exposed to hafnium by itself would you observe the kinds of patterns that you would see. Once you get a little bit of tritium, it's gone; you can't see it.

DR. LIPSZTEIN: Yes, that's very right. You can't see it because it is going to be covered by the other exposures. So you really can't distinguish.

Jim, there is a paragraph in OTIB-0066 that describes our problem. It says, if the metal substrate of the SMT is not known, type S solubility should be assumed. However, fairly modest tritium urine concentration can imply extremely large type S SMT exposures that might be quite implausible, and it gives an example of a urine excretion of 1 microcuries per liter of tritium that would result -- begin 30 days after the exposure, would mean 300 millicuries, assuming a fraction of 10 to the minus 6 escaping from the source term. So that is from OTIB-0066.

CHAIR BEACH: Kathy has been trying to come in for -- sorry.

MS. ROBERTSON-DEMERS: Can I say something, too, to add to that? The Mound special tritium compound Technical Basis Document also re-emphasizes this. From this perspective, what they are doing is they are using the standard bioassay procedure, which is designed to detect soluble tritium compounds in applying the model.

It says, because the identity of all tritiated materials encountered in the workplace is not well known, and the dissolution rates applicable to the encountered tritium materials are not well known, uncertainty in dissolution rates to be applied to the deconvolution of the urine bioassay excretion curves ranges over three to four orders of magnitude, conservative assumptions for identities in dissolution can lead to greatly overestimated doses of three to four orders of magnitude.

Because, in addition, urine data for particulate intake is readily obscured for extended periods of time by small intakes of readily assimilated HTO, urine bioassay is considered to have substantial shortfalls for assigning intake in dose from stable tritiated particulates.

DR. ULSH: And again, I would remind you to consider the context of that document. Yes, there is a technology shortfall because you can't, with bioassay, detect doses as low as 100 millirem per year, as required by the DOE order. That is totally different from what we do in this program.

MS. ROBERTSON-DEMERS: Could you explain to me why you think that that is based upon the 100-millirem limit?

DR. ULSH: Because that's the order that was in place at the time that document was written, and that was the nature of --

MS. ROBERTSON-DEMERS: But this is

not the order itself. This is a different document.

DR. ULSH: I know that, but that was in response to the requirement placed on Mound, and every other site in the DOE complex, to be able to detect exposures as low as 100 millirem per year. They were concerned because they couldn't do that in this case.

DR. NETON: What date is that document that you're reading from?

MS. ROBERTSON-DEMERS: 2001.

DR. NETON: Yes, I believe the order became effective in 1994, I think, or thereabouts. So anything after 1994 would have to be in compliance with 10 CFR 835, which had a 100 millirem AEDE requirement, CEDE requirement.

MS. ROBERTSON-DEMERS: Well, I guess the document is not calling that out. This document is not calling that out.

DR. NETON: What's the genesis of the document? Who wrote the document?

MR. HINNEFELD: This is Stu Hinnefeld.

If I could offer a comment, it would sound to me that this was the Technical Basis Document from the Mound internal dosimetry program. That's what you're reading from? Is that right?

MS. ROBERTSON-DEMERS: This is the Mound Technical Basis Document --

MR. HINNEFELD: I think this was the DOE one.

MS. ROBERTSON-DEMERS: -- for stable tritiated particulate and in organic compounds--

MR. HINNEFELD: All right, so it is a portion of the internal dosimetry. So this document was instructing essentially the dosimetrists at Mound and writing for the sake of reviewers, because that's what those were written for, so reviewers could come and make sure you had technically evaluated your program.

It was describing to them, it was telling the dosimetrist do not assign a dose based on type S stable metal tritides from all tritium doses because we don't want to record doses that high because we don't really think they're that high.

I mean that's what they did that for. That's certainly the way it sounds to me. That's why I would write something like that that way.

So they would say don't just assume that you have this type S material, to record the dose, you know, the dose of record, which in generating the dose of record, we all know sites generating dose of record, some of them did a good job and some of them maybe were a little shady. And for that reason, we don't rely on dose of records in the program, and we have developed approaches that we will not underestimate people's dose.

So the fact that it was not appropriate for Mound to do dosimetry from

bioassay because of the mixed exposure, which is certainly a decision I would have made, had I been in their position, I don't see how that really pertains to the decision of the suitability of this, using this for dose reconstruction in this program, which is, of course, the subject of debate.

So I am just listening to the debate today.

MS. ROBERTSON-DEMERS: Can I ask you, then --

MR. HINNEFELD: Sure.

MS. ROBERTSON-DEMERS: -- when Tom Lebone wrote OTIB-0066, and he made the statement that Joyce just read, what was his intention there? Was it for 100 millirem?

MR. HINNEFELD: I don't know. I don't know what that was about, and I didn't know the statement was there.

DR. NETON: Right, Tom's statement stands. I mean it is true that if you have a large tritium output in the urine and you

apply blindly type S to it, you will come up with some very extremely high intakes that are implausible. That's true.

But underlying in there is the assumption that there may have been some type S material. It doesn't take much in there to put you over the 50 percent PC calculation.

DR. MAKHIJANI: Jim, wouldn't the same argument apply to radon? Because you've got this person sitting there. You could say, well, you could assume that he sat there for two days, and it put him over the 50 percent. Then why go to -- it seems to be exactly the same thing.

I mean you've measured high --

DR. NETON: You are talking about like the Mound radon situation?

DR. MAKHIJANI: Yes. Yes, exactly the --

DR. NETON: No, we have one measurement taken over, one series of measurements taken over a couple of days over

a 20-year period, trying to reconstruct the doses back in time 20 years, that's the big issue there, in my opinion.

With different building ventilation rates, patterns, unknown cracks, ventings, that is really the problem there.

DR. MAKHIJANI: In doing the calculation that you did, you know, going back and extrapolating, and so on, there are certain reasonable assumptions that you can make, possibly reconstruct ventilation, you can't do the cracks, and so on.

But you have a measurement that -- I'm just throwing this out for argument, as to whether there's a consistency in the discussion. You have the measurement that is made near a hole in the ground, as I understood the discussion. So you're going to have an inlet of radon that --

DR. NETON: We don't know what that measurement really means, Arjun.

MEMBER ZIEMER: Well, that was the

same question I asked yesterday. Can you really not reconstruct that? And you have not only the measurement issue, I guess, but you have all the other parameters and the time issues and the nature of these, but it is kind of a separate --

DR. NETON: You have no direct measurements of the three different gases that were coming out of that hole. They were measuring, they were trying to measure radon-222 with something like a diffusion barrier device. Then the concentration came out something like 200 picocuries per liter. No one knows what that really was because it was calibrated to measure radon diffusion through a permeable barrier.

DR. MAKHIJANI: But you have a parallel situation here, I would argue, just listening to the discussion, at least possibly parallel situation, that should be considered before the issues get settled, which is that seems highly unlikely that anyone is exposed

to hafnium tritide as the principal exposure over a long period of time.

And you're going to assume that they were exposed to hafnium tritide over possibly a much longer period of time than they were. I mean where it goes over 50 percent is not a relevant consideration because you could talk about other cancers. I mean this is not just about lung cancer. You're going to compensate lung cancers probably just on the basis of plutonium.

So you can say, well, you never even go to the tritide. You've got the plutonium-238. They are going to compensate all the lung cancers. So the tritide argument doesn't even enter the cancer, lung cancer, argument for the most part, I would say. Right?

But if the situation at Mound is that you are confronted with a mixture, and the typical mixture is mostly soluble stuff, then you don't really have a model for the

situation at Mound.

I think it is at least arguably a very similar situation in that you can't model the radon because you don't have the measurements. In this case, you have no measurements for what metals, what kinds of tritides were in the air, just like you don't have measurements for what kind of radon was in the air.

DR. NETON: But let's go back, I mean, again, Super S issue, though, I think is analogous to this. We're doing exactly the same thing for Super S complex-wide. There's a huge difference in dose per unit intake to the lung. And why is that different? Why is that acceptable and this one is not?

I'm not sure if it's because the population is potentially smaller. I'm not sure that --

DR. MAKHIJANI: No, no, that's different because you actually know that people were exposed to Super S plutonium, and

a lot, for protracted periods of time.

DR. NETON: Everybody? Not everybody. Well, that's my point. So how do you know which ones are getting the over-assigned doses that are implausibly large?

DR. MAURO: Yes, I think that is where it comes down to. If you could say that here we have a person -- let's take one person at a time. Is it plausible that he was exposed for protracted periods of time to Super S at Rocky? And the answer might very well be, yes, it is possible because he was working at this location and during this time period. So it is plausible that that could have happened.

This also goes toward uranium type M and S. Is it plausible that this facility person was consistently exposed to this particular form of uranium? And the answer, for that person, the answer is, yes, it could very easily be yes. And therefore, you always, whatever the form is -- so you're in a

situation where plausibility is manageable. And you can say, because of the magnitude of the amount of material and the time period over which the amount of material was handled, for any given individual, it is not out of the question that he could have gotten a worst-case situation.

I guess what we are asking here is, is it plausible that there's anyone who was exposed to hafnium tritides exclusively over long periods of time? And if the answer is no to that, we have a plausibility issue.

DR. ULSH: The answer to your question is, yes, there were a couple of people who were exposed to hafnium tritide. We know who they were. There are very well-documented incidents.

Over long periods of time, no. These are discrete incidents, accidental.

DR. MAURO: There's where I think the different concepts, and how they're applied and their decisions were made, put us

in a different arena. I think the arena with regard to Rocky and high-fired plutonium in general, this is a fairly widespread, large quantity situation.

We're in a different arena. It is very unusual, very, very small quantities with the potential, though, to have doses that are 10,000 times higher, if you use that assumption. This is a very challenging situation.

DR. NETON: I still think we need to look at the individual dose reconstructions that were done, as Brant started off at the beginning of the session. Take an individual dose reconstruction and do an evaluation. Was this reasonable to assume that this person was or was not exposed to hafnium, you know, some very insoluble tritides?

I think that is based on a composite, looking at his file, his exposure history, what buildings he might have worked in, the job category. All kinds of things go

into these dose calculations. These are not aggregate. We just don't take 300 cases and say, okay, all these lung cases are going to get hafnium tritide. That's not the way it works in dose reconstruction.

So we have to make some value judgments about the potential exposures.

MR. FITZGERALD: I think my reaction yesterday, when you were, I think, citing or referencing, we were sort of saying, you know, by extension, one couldn't define these worker cohorts. We're, obviously, still in the process of doing that.

I think your reaction was, well, you know, it doesn't really matter. I mean you might end up not being -- it might be a much more expansive group of workers, but that doesn't bother us because, if we don't have definitive information, we'll default to applying a type S.

MEMBER ZIEMER: It still has to be plausible, though.

DR. NETON: Yes, if it were truly plausible that that cohort of workers were exposed.

MR. FITZGERALD: Well, I'm just saying, if you're in SW and R, and you can't come up with a roster, that I think Brant has brought to us for the 10 workers, but you come up with sort of we don't really know. We don't know if maintenance workers went in and out, say it's scrap metal, whatever it is going to be. So it becomes sort of an undefined class in those buildings. It then becomes a little more analogous to the radon issue, where you are not going to have that information to make that judgment.

I think you even alluded to this yesterday. Well, you know, you might end up assigning everybody to type S because you can't do that. That's where I think we end up moving from the couple of folks that Brant was referring to, which is a plausible situation, to one where the plausibility comes into

question because you know there wasn't a long-term exposure to hafnium.

But, nonetheless, since you can't define by worker category or location those who might have been likely exposed, then everybody is going to get this assignment. That assignment is, by definition, going to be extremely high.

In the tritium areas, particularly in the earlier years, these exposures were high. They exposed them right up to the limit. That's what we got from the people at Mound, that, basically, in the early days, the production era, they had extremely high tritium exposures. So this is not trivial.

If you, in fact, apply that factor for some of these workers, it is just going to be implausibly high.

DR. ULSH: What do you mean by early days, ballpark?

MR. FITZGERALD: Well, I am just saying --

DR. ULSH: Fifties?

MR. FITZGERALD: No, even the seventies, sixties and seventies, the tritium levels were pretty, I mean the exposure levels were fairly high.

This multiplication factor I think would put you in that realm of just --

DR. MAURO: Yes, tens of thousands of rem.

MR. FITZGERALD: Tens of thousands of rem. So I think it is analogous to the radon issue from that standpoint. Once you end up having to default in applying this to a larger population, many of whom already have high tritium HTO exposures, I think that's where it becomes -- and this is what I think one sentence was trying to convey in that piece -- but, in a sense, I think it puts you in that realm.

DR. ULSH: Well, what you're talking about here, as Jim has said, number one, Super S is not just at Rocky. We're

applying it complex-wide. We are applying it to people who there is no evidence to suggest that they were ever exposed to Super S plutonium, but we're doing that.

And you don't even have to go to Super S. Look at uranium. That's type S. Hafnium tritide is type S.

We're applying, if we don't know the solubility class of the uranium, we're applying type S. I don't understand why that's acceptable everywhere else, but at Mound, doing exactly the same thing only with a different element, is not acceptable.

DR. NETON: That is very reasonable. Eighty-plus percent of the lung cancers in this program are compensated.

MR. FITZGERALD: I think the difference goes back to John's comment, which is, if the likely exposure pathway -- and I think Brant alluded to it -- was limited to a small number of workers, but by virtue of lack of measurements and the ability to measure,

you by default have to apply it to everybody, then I think you get into this question, is that plausible to do so?

We've already acknowledged that, whether it's two, three, four, ten that are clearly exposed, if on a maximizing strategy one assigns it to everybody, and that is a tremendous dose, I don't see how -- I don't know if that is directly analogous to the high-fired Pu.

DR. NETON: Well, let's back off from everybody. I mean let's, again, confine it to people who work with tritium --

MR. FITZGERALD: Right.

DR. NETON: -- had lung cancer. Okay? Because it is only going to really -- lung cancer may be --

MR. FITZGERALD: That's the universe.

DR. NETON: That's the universe. So it is much more confined. Those who are not monitored, such as administrative staff,

secretarial staff, professional staff who didn't work with tritium, are not going to be assigned this. So it's not everybody, all claimants. Okay? It is a much smaller subset of the population.

MEMBER ZIEMER: Well, the other comment I would make on this thing is that, if you can't delineate specifically that they were not in that area, you have made the statement it's plausible that they were. I mean you can't say it's not plausible and yet say they could have been in there.

It seems to me, logically, I mean I don't personally think it's likely, but the statement that says, "I can't show that they weren't in there," you are, in essence, saying it's plausible that they were.

I think if you get to that point, you are not assigning a lifetime dose. You are only saying they have to be there a couple of days, or whatever it is, right? And that is the dose you are assigning, and then you

are stopping. So you are never saying that they got 10,000 rem. I don't know, whatever it is.

But I'm having trouble with, I mean I don't like the idea of that sort of a big group, because we have this everywhere, and our gut feeling is it can't be.

But if you can't show that it's not plausible for them to be in there, you are saying it's plausible. Right? Think about that.

MR. FITZGERALD: Yes, I understand what you're saying and I don't disagree.

DR. MAURO: The reason we reopened this is I felt it was important to get this on the record. I understand it is a tough one. But now I believe we have a nice, complete record, and how it's dealt with --

DR. NETON: For the third time.

(Laughter.)

DR. MAURO: Is that right --

MR. FITZGERALD: Well, we had the

radon discussion after the tritide, and that kind of, you know, brought some of these things in focus.

Your questioning yesterday certainly, you know, some of these, I guess, criterion for probing this thing struck a cord as far as --

MEMBER ZIEMER: Well, I'm as uncomfortable as Arjun was on the radon issue, simply because your gut feeling is, well, you have some information; why can't you reconstruct? But I guess it gets to a point where --

DR. NETON: There's too many unknowns, especially going back 20 years.

MEMBER ZIEMER: Yes.

DR. NETON: That, to me, we've never been successful at taking contemporaneous measurements and going back 20 years. I can't recall when we've ever been able to do that, I think, convince folks that it is sufficiently accurate.

MR. FITZGERALD: Well, this is helpful. I think just comparing the two issues and making sure that there is a rationale between the two is helpful.

CHAIR BEACH: So are there any action items that came out of the earlier discussion with Joyce? I know Joyce asked for some information. I just want to make sure. So we're okay there? I don't need to -- okay, I just wanted to make sure I didn't miss anything.

DR. LIPSZTEIN: I want to know from the Road Map which were the important tritides.

DR. ULSH: Does that mean that you are just going to answer that again or --

CHAIR BEACH: No, I just wanted to make sure Joyce was okay --

DR. ULSH: No? Okay.

CHAIR BEACH: -- and didn't need anything in writing or anything from you on that.

So the next item on the agenda is data adequacy and completeness of internal dose records.

So is NIOSH -- Brant, are you prepared to start on that? I have it down as NIOSH, but --

DR. ULSH: Yes. Well, I can just kind of go over the sequence of events, and then just open it up to discussion.

This has been an ongoing topic of discussion. The latest development, I think, is our issuance of our response to SC&A's latest White Paper.

Our response came out in November of 2009. That builds on the iterative cycle that we go through typically on these issues.

It originally started as a matrix issue listed in SC&A's issues matrix in February of 2008. We discussed it at a number of work groups. I list that in the White Paper.

SC&A, the significant event is

probably us issuing our responses to the matrix items. That occurred in July of 2008.

That was followed by SC&A issuing their reports in April of 2009 on this issue. And then we again discussed it at a Working Group meeting in May of 2009. Then we issued our response in November.

There were a number of issues raised by SC&A. I feel like we have addressed them thoroughly in the November release and many times previously.

So I guess I would just open it up to anything else that SC&A wants to discuss or the Working Group.

CHAIR BEACH: Bob?

DR. BISTLINE: Well, I'll go ahead and try to kick off a few points of concern that still exist as far as SC&A.

The first of which gets back to the discussion of gross alpha bioassay methodology that was used and the rapid gross alpha bioassay radiochemistry methods that

were referred to as plutonium analysis in the procedures; that it was sufficient to cover actinides, including plutonium, uranium, protactinium, americium, and possibly curium, but that radium is not brought down by this method. So the process is missing the radium. So it is sort of a misnomer here.

The recovery of the gross alpha technique was usually quoted at 90 percent, but ranged from 60 to 90 percent by [identifying information redacted], and equal chemical recovery yield, there's a real question, again, as to whether the recovery was equal for all of the components. If the recovery for thorium, protactinium, uranium, plutonium, and other radionuclides are recovered at the same or comparable percentages, this wouldn't be an issue. But the question is whether they are coming down in the same recovery.

Mound bioassay personnel did not specifically evaluate whether there was a

differential in recovery for particular actinides recovered with the gross alpha procedure, nor has NIOSH provided the differential recoveries of alpha emitters.

Another point is, with the implementation of the anion exchange, specific rate of nuclides were eluted from the column. This was primarily done for plutonium. Then, unless the field of health physicists communicated to the bioassay group that workers were exposed to other radionuclides, it wasn't done.

So the question is whether there was consistency in routine. Whenever it was an incident involved, the field person in general was communicating this, it appears. But the question is whether this was done on a routine basis, that it would not be specific after the anion, they went to the anion, that it was only pulling -- they were only looking at one specific isotope. That would be your plutonium.

[identifying information redacted], in 1992, indicates this rapid gross alpha determination was used through 1977, and the bioassay supervisor indicates that anion exchange was implemented earlier than this. MESH indicates a bioassay type of gross alpha or total alpha up through 1970, and we know the procedure was in place in the mid-'66 and mid-'67 and started.

And with the anion exchange, there is now a gap of radionuclides other than plutonium and specific radionuclides. At this point, the bioassay is radionuclide-specific, and other alpha emitters were not covered. It kind of gets back to the point earlier.

NIOSH has made two assumptions with respect to the rapid gross alpha. First, for the purpose of monitoring other alpha emitters, the bioassay represents a gross outflow result. Two, for the high-fired plutonium-238 modeling, the same bioassay represents plutonium-238 results. It is not

clear how the use of these two different guiding assumptions on NIOSH's part can be rationalized.

The next point, dealing --

MEMBER ZIEMER: Could I interrupt just a minute?

DR. BISTLINE: Yes.

MEMBER ZIEMER: Just to help us out, can you kind of tell us where you are in either the tables or the NIOSH paper, so I can track?

MS. ROBERTSON-DEMERS: We're working off our own list.

MS. JESSEN: Yes, I think it's comment 1-3.

DR. ULSH: Actually, I was going to cover that in my response, Paul, if you can wait that long.

MEMBER ZIEMER: Oh, okay.

CHAIR BEACH: Well, before you start again, let's break this down into the issues, too, and we will just have you

respond.

DR. BISTLINE: Okay.

CHAIR BEACH: So, the first one with the gross alpha.

DR. BISTLINE: Okay.

CHAIR BEACH: But go ahead.

DR. BISTLINE: Go ahead? Well, that completes the gross alpha, I think, that I have.

CHAIR BEACH: I think, that way, we all won't get lost in that --

DR. BISTLINE: Yes, that's a good idea.

CHAIR BEACH: -- if that's okay.

DR. BISTLINE: I think that is a good idea.

DR. ULSH: Okay. So you want to start with gross alpha?

DR. BISTLINE: Yes.

DR. ULSH: That was responded to by NIOSH in Response 1-3. I want to quote a paper that was written, of his own accord, by

Warren Sheehan recently. It is in the SRDB. And here's what it says:

Mound's primary need was detecting plutonium uptake, although there were other radioactive materials being handled in small quantities. The adopted procedure was, in fact, a gross alpha method. This was considered an asset in meeting Mound's needs.

Mound was aware that other radionuclides, such as thorium and protactinium, also carried through in this method. As such, it was a catch-all for the many minor projects going on in Mound in the late fifties and early sixties.

I would ask you to remember that time frame.

Mound's position was that training laboratory technicians to run one non-specific procedure was preferable to having a host of procedures applying various chemical separations. This practice -- and I emphasize this -- This practice also reduced the chance

of using the wrong procedure for a particular individual's analysis.

Individual employee results were associated with employee work assignments and recorded into the records accordingly. Specific chemistry could always be applied when the situation called for it.

Plutonium was, by far, the most potentially harmful isotope at Mound. Therefore, if an analysis result was erroneously assigned as plutonium, results would have been overstated favoring the employee.

Now that was written -- I was surprised when Warren sent this in. He was the guy in the bioassay section doing this.

MS. ROBERTSON-DEMERS: May I say something?

DR. ULSH: Yes.

MS. ROBERTSON-DEMERS: I believe that what we have said is that the rapid alpha technique does bring down the alpha emitters.

DR. ULSH: Yes.

MS. ROBERTSON-DEMERS: Okay. Except for radium. Where I was able to obtain that information was from the same person who wrote that document.

We did a subsequent interview with him asking very detailed questions about the radiochemistry procedures, to the point of, what radionuclides are you bringing down when you add the cerium --

DR. ULSH: Kathy, sorry. Have you made those notes available? Do we have those notes?

MS. ROBERTSON-DEMERS: This has just been done. Okay? So, yes, we can make them available.

Our problem is not so much the rapid gross alpha, but when you implement the anion exchange, you have a column. By adjusting the pH, you can move various radionuclides off that column. Okay?

What this individual was telling

us is, as a routine practice, they pulled off plutonium only, even though they could, by the procedure, if they wanted to, pull out thorium or uranium or americium. But that was very time-consuming. So they pulled plutonium. Okay?

DR. ULSH: Okay.

MS. ROBERTSON-DEMERS: Only.

DR. ULSH: Okay.

MS. ROBERTSON-DEMERS: Unless the field HP came to them and said, Joe Smith is working on a thorium project. You need to pull off the thorium also. Okay?

DR. ULSH: Okay.

DR. NETON: That seems counter to what Brant just read.

DR. ULSH: No, it doesn't. I can explain that.

DR. NETON: Oh, okay. I'm sorry.

DR. ULSH: But I don't want to interrupt. Go ahead.

DR. NETON: Sorry about that.

MS. ROBERTSON-DEMERS: I will make this information available to you. Okay? This is what I have been told, and it is not counter to what he read because, if their primary concern was plutonium, they would only elute the plutonium.

DR. NETON: That's not the way I understood that to be.

DR. ULSH: Okay. Shall I respond? Are you done? I mean I don't want to cut you off.

MS. ROBERTSON-DEMERS: Right.

DR. ULSH: Okay. Was there someone else on the line that wanted to say something? I thought I heard someone.

MR. SHEEHAN: Yes, this is Warren.

DR. ULSH: Hello, Warren.

MR. SHEEHAN: Brant? Is this Brant? Hello, Brant.

DR. ULSH: Yes, yes. Hello, Warren. Go ahead.

MR. SHEEHAN: Okay, I want to say

something about not pulling the radium down. By the time that we dropped that step of the procedure, the radium was no more concern. This was like '59-60. So I don't see where that's a problem, period.

That was brought up, was it not, earlier?

DR. ULSH: Yes, it was.

MR. SHEEHAN: No, I don't see that as being -- in other words, the last of the radium samples were run probably in the '59-60 time frame. Beyond that point in time, the cave was already old history, and there was no reason to be offering surveillance there.

Now the other issue about the column and the manipulation of the column, I don't frankly remember what year we started using the column, but, again, I think this is beyond the period when we had all these little small ionium, protactiniums, whatever. Programs were already history by this time.

When we went to the column, then

we were primarily -- we really had only one concern, and that was plutonium. I don't know if this clears that up or not.

DR. ULSH: Let me get something clear in my head. When you say, "the column," are you talking the anion exchange procedure or is that a separate issue?

MR. SHEEHAN: No, to add the column was just an adjunct that you could insert into the normal procedure, the gross alpha procedure. Instead of mounding the cerium fluoride, you actually dissolved it and put it through a column.

So the chemistry up to that point would be identical whether you went through the column or whether you didn't. So, when you went through the column, now you could perform specific chemistry, as the lady pointed out, you know, by altering the normality, the acid to which you did the elution off the column.

So she's perfectly right. If you

went on to the column and eluted only with 8 normal nitric acid, you probably are only going to see the plutonium. You're not going to see some of those other isotopes, but we weren't looking for them at that point.

DR. ULSH: And that's why I asked you, when I read that statement from Warren's document, to remember the time frame. Because the programs that you are talking about, off the top of my head, the reactor waste program, the uranium program, the ionium program, those were all concluded in the fifties, at latest the early sixties.

MR. SHEEHAN: Right. Yes. Actually, we were still doing gross alpha primarily clear up into probably '63. I don't know. These dates kind of elude me right now, but maybe we should have maybe kept a better count of that, but we didn't. We didn't go into column chemistry until we had passed that phase. Let's put it that way.

DR. ULSH: I would like to read to

you another comment. Anion exchange was used in 1966 and 1967.

SC&A made this comment in our response. It is designated as Comment 1-8. And our response says, SC&A seems to be implying that, during the '66-67, that the Mound bioassay program lacked the capability to detect alpha emitters other than plutonium or uranium because the anion exchange procedure was selected for these two radionuclides, the fundamental mistake being made is SC&A's assumption that only the anion exchange procedure was used during this time frame. This is inaccurate.

Though the anion exchange procedure was conducted for most of the work in this time frame, when the primary exposure was to plutonium, in fact, the gross alpha procedure was conducted during this time as well.

[identifying information redacted], 1992, page 336, reports that, on

February 11th, 1966, a memo was issued -- and this is a quote -- urine results would be reported as plutonium or uranium since they were then using anion exchange separation.

Here's the important part, It is to be noted, however, that for certain work areas they continued to report a small number of gross alpha as well as a few radium and thorium extractions, they were not just doing anion exchange.

As you would expect, I mean it's logical, if there was potential exposure to other radionuclides, they used the appropriate bioassay method.

MS. ROBERTSON-DEMERS: Can I say something about your quote?

DR. ULSH: Sure.

MS. ROBERTSON-DEMERS: That is a quote from the [identifying information redacted] document.

DR. ULSH: Right.

MS. ROBERTSON-DEMERS: And I

actually went back to the memo, and I would like to read the quote from the memo.

Starting with this report, all 24-hour urinalysis results are being reported as plutonium and uranium, as we are now using anion exchange separation, which is selective.

DR. ULSH: I believe that's what I said.

MS. ROBERTSON-DEMERS: That is not exactly what you said.

MR. STEWART: That was [identifying information redacted] memo, Kathy?

MS. ROBERTSON-DEMERS: That was the radiochemist.

DR. ULSH: Right. We are not denying the anion exchange procedure is selective for plutonium. What we are saying is, in situations where there was a potential exposure to other radionuclides, and they were very far and few between at this point in time, they had the capability to do the non-

specific gross alpha, and, in fact, they did.

MS. ROBERTSON-DEMERS: We are not -- okay. When I looked at the plutonium bioassay data in MESH, okay, and I looked at the type, okay, I can see the radionuclides that they did the analysis for. Okay?

And through 1970, you will see gross alpha or total alpha. After that point, they start listing either thorium or plutonium-238, or whatever they eluted from the column.

I realize these procedures were available, but that does not necessarily mean that there is a bioassay result available, that they actually pulled off all these items.

MR. KATZ: Warren, this may be your phone, actually. I think it is a cell phone, but we are hearing cut-ins from the phone, and I think it is because someone is not on mute. If you could mute your phone? Use *6 if you don't have a mute button. Someone on the phone, again, whoever doesn't

have their phone on mute, can you try muting your phone? Use *6, if you don't have a mute button.

Thank you.

MS. ROBERTSON-DEMERS: The question comes down to, were the radionuclides present when anion exchange was being used, and was bioassay sampling actually collected? And was the field effectively communicating with the bioassay group when they needed to be eluting other radionuclides or performing special analysis?

Then, also, how are you going to differentiate a result that is labeled as plutonium-238? How are you going to differentiate whether that was done by anion exchange or rapid gross alpha?

DR. ULSH: I am probably not because what it says here is that they recorded the results as appropriate. So, for instance, if they did this procedure and did the specific chemistry to pull off uranium, it

was recorded as uranium. If it was recorded as gross alpha, we will assume that it was plutonium-238, unless we have indications otherwise, because, by and large, the work that they were doing at Mound was plutonium-238 at that time.

Again, during the time they had anion exchange, '66 to '67, specific to plutonium, they also had gross alpha. In fact, they did do a small number of gross alpha commensurate with the size of the programs involving these other radionuclides.

Yes, of course, we have to assume that the field communicated with the bioassay laboratory. We have no evidence to suggest that they didn't. In fact, to the contrary, that's not what [identifying information redacted] indicates and it's not what -- well, I don't want to speak for Warren. Warren can speak for himself.

But I just don't see what the SEC issue is here. They had the bioassay

capabilities to detect the elements that they were working with.

MS. ROBERTSON-DEMERS: Bioassay capabilities do not equate to actually collecting samples.

DR. ULSH: So you're saying that there were situations where they should have collected samples and they didn't?

MS. ROBERTSON-DEMERS: Yes.

DR. ULSH: Okay. Let's talk about those. Give me some examples.

MS. ROBERTSON-DEMERS: I've given you an entire table where these things were noted as being handled in the Road Map, and there's no coverage of bioassay.

DR. ULSH: Okay. So we're talking about the Road Map now. Again, as we discussed earlier, the Road Map lists any element, any radionuclide that could have possibly been in a particular room, not that there was a confirmed presence of it, but just that it was possible.

And again, the piece that you are not considering is the exposure potential. If I walk through -- I'll use the same example again -- if I walk through a room, if I even stored in a room sealed sources, that does not equate to an exposure potential and it does not equate to a need to do bioassay.

If the Road Map is your basis, you are misinterpreting the Road Map.

MS. ROBERTSON-DEMERS: Well, then, in that case, the Road Map is not valid because you haven't fine tuned it to such a level that we know when the radionuclides were actually at Mound.

DR. ULSH: What radionuclides do you want to talk about, Kathy? We'll go through them. I can tell you when the uranium program was. I can tell you when the ionium program was, protactinium.

MS. ROBERTSON-DEMERS: Well, let's just throw out actinium.

DR. ULSH: Obviously, actinium was

present at Mound from 1949 to '59. That was the basis of the SEC. They had a small where they opened an ampule of it in the SW new cave in the early 1960s, '64 I think.

After that, as far as I am aware, the only actinium activities that presented an exposure potential, even a theoretical exposure potential, would have been residual contamination. Of course, the most notable of that is during D&D that resulted in the Price-Anderson Act violations, which we have discussed at length in other situations.

What else do you want to talk about?

MS. ROBERTSON-DEMERS: Well, I'm going to give you some data that you probably ought to consult --

DR. ULSH: Okay.

MS. ROBERTSON-DEMERS: -- about varied actinium drawings in counting soil in an outside area in the 1990s.

DR. ULSH: Okay.

MS. ROBERTSON-DEMERS: There was actinium, for example, in the soil, and that would indicate to me that it was not a sealed source.

DR. ULSH: Well, I'm not saying it was a sealed source.

MS. ROBERTSON-DEMERS: What I'm saying is there's a lot of contamination that has been identified that indicates a lot of what you have said are encapsulated sources were, indeed, not encapsulated.

DR. ULSH: I used that as a specific example. I'm not saying -- I did, I believe, say residual contamination.

MS. ROBERTSON-DEMERS: And I guess we need to get to the bottom line on the Road Map because that Road Map answers several matrix items, and if it is, indeed, just kind of a pie in the sky, and not really giving us the information on when and where items were handled, then we don't have an answer to several matrix items.

DR. ULSH: No, I never said that it was pie in the sky. I never said that it was -- I forget what other term you used.

What I said was it was built off of the [identifying information redacted] document. So it is a visual representation of what you find in the [identifying information redacted] document. That's all it is. It is not meant to be a categorical list of every exposure situation.

You can imagine that Mound had an operating history from 1940ish up through ultimate D&D. You can't capture that in one particular document.

If there are particular situations that you are concerned about, we will be happy to discuss those. But in terms of -- I can't talk about generalities here.

MS. ROBERTSON-DEMERS: Well, if I go to the Road Map, and I look up, you know, when actinium was handled, it gives me a very long period of time. Okay? It stems from the

forties through closure. If you were to take it was handled at some building at some location --

DR. ULSH: No, it was present at the site potentially in -- the Road Map is meant to indicate the possible universe of places where, if you were going in to do D&D, you want to be conservative. You want to take samples, workplace characterization for even the potential elements that might have been there. It's not to say that they were. It is just this is kind of, well, it's a Road Map for people who are doing D&D to go in and say, okay, what kind of a bioassay program should I establish here? What should I look for?

It doesn't mean that it was there. It just means that that is what you should probably look for during D&D. That was the purpose of the [identifying information redacted] document, and then, consequently, the Road Map.

MS. ROBERTSON-DEMERS: So it

doesn't really answer those matrix items because you haven't adequately characterized when radionuclides were present.

DR. ULSH: No, I just told you -- sure we have when radionuclides were potentially present. Now, if you're interested in particular ones, of course, I would refer you, start with the Road Map as to what's possible. If you're interested in actinium, go look at the reports on the radium, actinium, thorium program primarily. Whatever the guy -- I'm not going to say his name for Privacy Act reasons -- interview him, which we did on the actinium issue. The thorium program, the same thing. You know, the re-drumming program, we talked to the people involved in that.

So, no, I wouldn't say you stop with the Road Map. That is not a shortcut to doing any more research.

MS. ROBERTSON-DEMERS: How is the dose reconstruction supposed to use this?

DR. ULSH: To use what?

MS. ROBERTSON-DEMERS: The Road Map.

DR. ULSH: The Road Map was not constructed for dose reconstructors. It was constructed for this Working Group to evaluate the SEC petition. We never presented the Road Map as an addition to the TBD or an addition to instructions for dose reconstructors.

The way that a dose reconstructor would evaluate for a particular claimant what internal doses do I need to reconstruct is the same way you would do it for the 30,000 other cases that we have in the complex. You would look, first of all, at their bioassay record. You would look at their job history to see what radionuclides they might have been exposed to. You would look at their CATI, where we specifically asked, "What radionuclides were you exposed to?" And they can have the opportunity to tell us that.

So those are the kinds of things

that you would do, similar to any other dose reconstruction. Yes, you might even pick up the Road Map and have a look, but I wouldn't say that that is an essential dose reconstruction document. That's not what it was designed for.

MS. ROBERTSON-DEMERS: In that case, I would say that you need to go back and fine-tune and tell us when those radionuclides were really there, so we can determine whether there is a bioassay method applicable to those.

DR. ULSH: Well, that is certainly a topic that the Working Group can discuss, and if you want to task us to do that, at this point in time we could do it. Keep in mind, we have discussed specific radionuclides over the course of this investigation for the past two years. If there are particular ones you are concerned about, ask us. We will go look.

DR. NETON: Yes, let's talk about actinium maybe, because that seems to be what

the current issue here is. I mean it seems to me that Brant has put forth the idea that actinium production in any types of quantities ended in the late 1950s, early sixties, something like that.

DR. ULSH: Well, the old cave operations ended in 1959.

DR. NETON: Okay. So the cave was D&Ded, and then, as far as you know, no subsequent research activities occurred with actinium, except for maybe this couple of little source --

DR. ULSH: Yes, they did some calorimetry, but nothing that would present --

DR. NETON: But, in the interim, somehow all that material got buried on site, and Kathy is talking about this actinium found in drums in the 1990s. So, presumably, this material was on site, but I guess the question I have is, what is the potential for exposure to these drums that were there in contaminated soil discovered in the 1990s? Were the

workers out there romping around in these burial grounds, digging them up? So is there any potential for exposure?

MS. ROBERTSON-DEMERS: In that particular case, when we identified actinium in the soil, it was remediation --

DR. NETON: That is what I am saying.

MS. ROBERTSON-DEMERS: There was a remediation program.

DR. NETON: So there is a big gap here between the 1960s, when everything was dug up and buried, and you're speculating maybe that in those 30 interim years something occurred to expose these workers anew to this actinium source. I'm missing --

MS. ROBERTSON-DEMERS: Let me do this in a little bit different way. Actinium-227 bioassay specific is available through determination by radium-223 for 53 through 59. There's actinium in sample in 64, in 89, in 94 through 2005. Okay?

I'm going to turn the question around on you. Okay? Demonstrate to me that actinium was not present in the years where I don't have any bioassay.

DR. NETON: Well, wait a minute, Kathy.

DR. ULSH: Well, go ahead.

DR. NETON: No, go ahead.

DR. ULSH: The time frame was 40 to 59, 64 --

MS. ROBERTSON-DEMERS: No.

DR. ULSH: I'm sorry.

MS. ROBERTSON-DEMERS: The bioassay data, there is data available for 53 through 59, 64, 89, and 94 through 05.

DR. ULSH: Okay, 53 through --

MS. ROBERTSON-DEMERS: Okay. Now let me add one other thing. Okay? Your own Road Map says it was there from 1948 to present.

DR. ULSH: Okay. Again, first of all, let me start with your first question.

53 to 59, that's the basis of the SEC. Sixty-four, that's I think probably the operation in the SW new cave. If you want the name of the guy, I can give it to you under the right circumstances. Eighty-nine, I'm not sure. I don't know. I know that they were starting D&D then. Ninety-four through 05, that gets into the heavy-duty site D&D. That's why you see actinium bioassay there.

Yes, the Road Map says actinium was present on site from 48 to present. It sounds about right to me, but that does not indicate an exposure potential.

Let me give you an example. R corridor 5, which I assume -- this is an assumption on my part -- there was specific, small, discrete spots of actinium contamination related to the old cave operation. Those were identified. Those were painted over.

Fast forward to 1994, and they go in and start D&Ding. I don't know if they

demolished that. I think there was a demolition project. Certainly, they were doing sandblasting.

Now you re-expose that actinium because you blast off that paint that was put on. There's an exposure potential again.

In the intervening years, no. That's why you do the D&D. That's why you immobilize it.

Was it present? Yes. That does not indicate an exposure potential.

MS. ROBERTSON-DEMERS: All I'm asking you to do is to tell me there was no actinium present from 60 to 63, from 65 to 88 --

DR. NETON: Kathy, I don't think anybody is saying there was no actinium present.

DR. ULSH: I'm not saying it.

DR. NETON: I think what people are saying is there was no exposure potential. There were no ongoing activities to generate

airborne or exposure potential activities.

MS. ROBERTSON-DEMERS: But I cannot determine that from the Road Map.

DR. NETON: Well, you have access to the same documents we have, Kathy. We have not identified any operations or activities that would generate a potential for actinium.

You are free to look at those as well and see if we have missed something. But you have heard Brant say that we know of none. So I don't know what else we could provide you. I really don't.

If you want us to put it in writing --

MS. ROBERTSON-DEMERS: It seems to me that --

DR. NETON: -- we have identified no actinium --

MS. ROBERTSON-DEMERS: It seems to me that you've got a document, [identifying information redacted], that says dates are present. Yet --

DR. NETON: Present versus exposure is a different story, Kathy.

DR. ULSH: It doesn't even say present. It says potentially here in a particular location.

MS. ROBERTSON-DEMERS: Okay. Now prove to me that it isn't.

DR. NETON: I think we're done talking. I think our position --

MR. HINNEFELD: I don't know how we would ever prove a negative. How would we prove something is not somewhere?

MS. ROBERTSON-DEMERS: You revise your Road Map to be more accurate.

MEMBER ZIEMER: Well, the Road Map is the [identifying information redacted] document summarized. You can't revise what the [identifying information redacted] document -- I don't follow the logic at all.

MS. ROBERTSON-DEMERS: What I'm saying is, if actinium wasn't really there from 1948 to present --

MEMBER ZIEMER: Nobody has said that.

MS. ROBERTSON-DEMERS: The [identifying information redacted] document says that.

MEMBER ZIEMER: It says it wasn't there?

MS. ROBERTSON-DEMERS: It was.

MEMBER ZIEMER: Yes.

MS. ROBERTSON-DEMERS: It was.

MEMBER ZIEMER: Nobody is saying that that's wrong.

MS. ROBERTSON-DEMERS: Okay. No, what I'm saying --

MEMBER ZIEMER: I don't understand the argument, even, that you're making. It doesn't --

MS. ROBERTSON-DEMERS: What I'm saying is, what the [identifying information redacted] document says, that it was there from 49 to present, and it was not available in the form where individuals could have an

uptake, then we need to know during which specific years. Because that's what we're hearing, there's only specific years from 48 to present that it was available for uptake.

MEMBER ZIEMER: That's what we just said.

MR. HINNEFELD: What you are asking for is another document like the Road Map, but rather than just show presence, show the exposure potential? During what time there was an exposure potential?

MR. FITZGERALD: Can I jump in just a little bit?

I think let's just go back and I think Brant summarized where this all came from. I think in the very beginning we looked at the Site Profile and brought some issues forward to the ER review, which spoke to whether or not there was bioassay capability for, I think for other sites we called it other nuclides, but, you know, these very specific nuclides, and we identified and there

were probably five or six issues where we asked the specific question, and that did get discussed, I think, on the table.

Because of this sort of collection of various and sundry nuclides, I think the response was to roll these up into a Road Map based on the [identifying information redacted] report, just to have an easier way to look at all these nuclides.

What we are, I think, establishing not the first time, but maybe establishing in a more firm way is that the Road Map just reflects the [identifying information redacted] report, and the [identifying information redacted] report just reflects the potential presence of these nuclides, but it doesn't really speak to maybe the original question that we had for some of these nuclides, whether or not both exposure potential existed and a bioassay capability was available.

So I think there is a gap there.

I think we are maybe talking past each other a little bit on this one.

But, in terms of the exchanges of White Papers, I think we are sort of down to the point, okay, the Road Map helps, but it didn't necessarily add any new information that we couldn't get from the [identifying information redacted] report. We still have some questions on specific nuclides, you know, issues. You know, we did lay these out.

I think what we could do, just to bring this to a close, is just identify what specific nuclides remain in terms of whether or not there was an exposure potential, then, in fact, whether there was a bioassay capability at the site in that time frame for that exposure.

I think we could nail that down a little better, but get away from deciding whether or not the Road Map does the trick or not, because the Road Map really is a mapping of the [identifying information redacted]

report. So we are arguing over something that won't get us where we need to go anyway.

So what we can do that would be helpful, and if it is agreeable to the Work Group, is just simply -- it sounds like we started doing it for actinium, but just kind of nail down some specific examples. Go back to the nuclides that we identified in the original matrix and pull some of those and say, you know, can we, for those time periods where we do have an identified presence, can we establish whether, 1) exposure potential existed. And we can look at the same documents as Jim has suggested. And if, in fact, we can establish that exposure potential, then can we nail down whether the bioassay capability existed or not? And just kind of nail this thing down, rather than deal with it in a very broad sense.

DR. ULSH: And as you do that, Joe, I would refer you to our White Paper. Look at Attachment A, which starts on page 18,

and then look, also, at page 24, which is another table that talks about specific radionuclides that SC&A has raised a concern about.

MR. FITZGERALD: I'm sorry, what was the second one, Brant?

DR. ULSH: It's page 24. Unfortunately, it's not numbered.

MR. FITZGERALD: Okay. Do you have --

DR. ULSH: But, to give you a summary, I'll start with the second one first. It has three columns, radionuclides and era, summary of SC&A-identified issues -- that's our summary, by the way, I think -- and, also, our response, our NIOSH evaluation of these issues.

Some of the radionuclides listed are actinium-227, bismuth-210, cobalt, cesium, a number of others.

Stop me if you need me to. So 24 there.

Now, to go back to the first one, page 18, this list, this is called Attachment A. The first column is informal source term and title. It gives a description loosely of the program involved. The second column is the constituent radionuclides, and it lists the major radionuclides of concern.

So I agree the approach that you have suggested would be very helpful, so that we can talk specifically and not generally. But I would also say that we are pretty far a ways down the road here, taking in mind what's already been done. And if there are additional questions or remainder issues --

MR. FITZGERALD: Well, I think that's what I want to get to, rather than sort of keep this in a broad discussion, which we have had, but get down to specific examples and let those examples pretty much settle the question of whether to present gaps or questions that remain. But get it very specific, so that we are talking in

generalities now, if that's agreeable.

CHAIR BEACH: I am agreeable to that. It just takes us back to the matrix.

MR. FITZGERALD: Well, to some extent, but there has been a lot of work done since then. I don't think we're going back to the matrix --

CHAIR BEACH: Right, right.

MR. FITZGERALD: -- as a starting point. I agree with Brant, we just build on what we have done already and what NIOSH has presented, but getting a lot more specific and come up with specific examples to present.

So we will take that action and provide you those specifics, and then see where that settles.

DR. ULSH: And, hey, if you've got names of people, that would make it real easy, but I suspect you probably don't.

MEMBER ZIEMER: Well, whose action is this?

MR. FITZGERALD: Names of people,

I mean in terms of --

DR. ULSH: If you are concerned about a particular program or exposure incident --

MR. FITZGERALD: Oh, no, I think we've got to be as explicit as possible. If we can nail it down --

MR. CHEW: Brant, could I ask SC&A a question?

Joe, what would you consider evidence to you that that particular radionuclide in that particular area on the Road Map was an exposure potential or not an exposure potential?

MR. FITZGERALD: Well, I think I go back to Jim's comment that we have access to the same operational documentation that NIOSH does, plus interviews. I mean the same body of information. If we can select two or three areas where I think -- well, the first question is to reach some agreement there was an exposure potential. I mean, if we can't

get there, then discussing whether or not bioassay capability was available doesn't make any sense.

So we would present what we think is an argument that there was an exposure potential for that time period. Then, basically, ask, since we probably don't have that specific information, whether or not we can establish bioassay capability.

MEMBER ZIEMER: Well, I would like to ask why this isn't a NIOSH activity. NIOSH is stating that they believe they know the periods where there was exposure potential. They believe they have the information about when bioassay was done and what the particular projects were and the locations as well. Why isn't this just a table that they put together and then you say we agree or we don't?

I have my usual problem with having SC&A do it. In my mind, it is a task of the agency.

DR. NETON: Well, I think the

question is a little different, Paul. I think the question is, where we have bioassay data, I think SC&A would agree that, well, there was something going on and it was monitored. What they are saying is, how do you know something didn't happen -- something happened that wasn't monitored in those intervening years.

And we see no evidence of that. So it would be hard for us to put together a list and say we looked at the list and nothing happened.

MR. FITZGERALD: What I heard was sort of this, if you can show us or give us some indication of where that gap or that question is, then we could at least have that to go by.

DR. NETON: Well, but Kathy has clearly enumerated that. I mean she's posited these years where there was no bioassay, and she is suggesting show us. She is saying to us, show SC&A why there was no bioassay program. We're saying because we see no

evidence of any activity occurring.

MR. FITZGERALD: Right.

DR. NETON: So that's all we can say.

MR. FITZGERALD: Right. And I think what we are trying to supply is, okay, we owe you --

DR. NETON: Right.

MR. FITZGERALD: -- not only the nuclides, but we also need to give you some indication of why we think there's --

DR. NETON: That's the question.

DR. MAURO: I think the burden, if you folks have laid out a network of scenarios over time and bioassay programs, and there are windows of time where the judgment was made at that time that there was no need to look specifically at those radionuclides -- and obviously, in the words you read, that was the judgment.

What we just said is that, well, but there were these windows of time that

maybe there was something going on. I think that if we are going to make that statement, we have to show why we believe that might have happened.

I don't think we default to the assumption that, just because it was unsoluble, that automatically increases your exposure potential.

Now the only reason I say that is that, because there are other time periods where exposure potential was admitted to, engaged and dealt with. So it wasn't as if it was something that the administration of the program was blind to right up to the nineties.

So, I mean, I'm thinking about it as, clearly, you've made a case that the people in charge who were collecting the data were well aware that actinium was a problem when it was being handled, even up to the D&D operation, so I'm hearing. But there were time periods where some judgment, obviously, was made that it wasn't necessary to

explicitly look for that.

Now your position is, well, you trust that judgment, that --

DR. NETON: Well, not only do we trust that judgment, but we see no evidence --

DR. MAURO: Now we are saying that, and this is an interesting question now, if we are going to raise this as an issue, I guess we have to offer up some evidence that we think, wait a minute, we might have had a window where you missed something important.

I think that's my read of this, and this, certainly, -- ground rules --

MEMBER ZIEMER: Well, it seems to me it's got to be more than presence on the site.

DR. MAURO: And I would agree with you that it has to be more than presence on the site.

MEMBER ZIEMER: I thought the question was whether or not bioassay capabilities corresponded to the use periods

in question, that that was not --

MR. FITZGERALD: Well, I don't think you get to that issue until you answer the exposure --

MEMBER ZIEMER: Well, I thought that's what Kathy was asking, whether or not --

MS. ROBERTSON-DEMERS: The use periods, as I guess, not defined by [identifying information redacted], but defined by national use of the radionuclide on site.

MEMBER ZIEMER: Right. As I understand it, the Road Map isn't defining use. It is defining presence, pretty much.

DR. ULSH: Potential presence.

MEMBER ZIEMER: That is the reason I was asking whose job it is. If it is only an issue of whether or not there's a coincidence between bioassay capabilities and actual use periods, I think you have that information; what use periods you are

defining, you already have that. If you're asking, is there evidence of use outside of those values, that's a different question.

DR. MAURO: Yes, I would say, and I will defer to the Work Group in terms of interpretation of, how far does SC&A go when we present the case. Now, in my mind, if there's a window of time where there were no bioassays collected for a particular radionuclide, but there were before because they knew certain things were going on, and they were after because they knew certain things were going on, I would say, obviously, the program had the wherewithal to make those judgments.

Now, if we're going to come in and say there's a window of time where it wasn't collected, I think the onus is on us to show that there was a judgment made that was incorrect at that time.

MEMBER ZIEMER: Or, yes, you find that there is some --

DR. MAURO: Yes.

MEMBER ZIEMER: -- work going on.

DR. MAURO: Yes, and as opposed to imposing that on NIOSH, it seems to me that we have to have affirmative evidence that there was something wrong that they didn't do it here.

I don't know whether or not the Work Group agrees with that or not, but, in my opinion, in this particular circumstance, if we are going to say there's a window of time where the material was present but there was no bioassay, but at the same time we know that there was the wherewithal to deal with the problem, then we have to say that, uh oh, I think there were certain things going on in this time period where the bioassay wasn't collected, and that's a problem. Obviously, we haven't done that.

MEMBER ZIEMER: Well, I just have two other comments. One is that I want to make sure that there is a match-up or that you

guys have provided for Kathy the match-up between what you say the existing programs are, I mean the active -- if SC&A needs that, it seems to me you could provide that pretty easily.

The other concern that I have is that, if you go on the path you are talking about, you may be in the position of trying to prove the negative, also. It may be an unending task to show, to say, well, I haven't found anything yet, but give me another five years and I'll find something.

(Laughter.)

No, I --

DR. MAURO: And I agree with you 100 percent. When do you stop?

MEMBER ZIEMER: If there's some obvious regime or some obvious activity that jumps out, but, otherwise, you're searching for an unknown.

But I want to make sure that I understood whether you, Kathy, have what you

were asking for originally.

MS. ROBERTSON-DEMERS: I would propose that they take Attachment A and add the years to it.

MEMBER ZIEMER: That would be helpful then, and see how that matches up with the bioassay. It seems to me that just integrating some data you already have, is that right?

MR. CHEW: John, I just want to make one other comment. I think it would be important to also see what you are going to define as exposure potential. Okay? I think that's important. Because if you are going and doing D&D, and it's only 100 d per m per hundred square centimeter, is that exposure potential?

DR. MAURO: It is certainly a reasonable question.

MR. CHEW: Good. Okay.

MR. STEWART: I just have one observation. It was mentioned earlier that we

should look in the record and find negative judgments for when bioassay was required. We don't see that in record. What we see is we decided we needed a bioassay for X. We need a bioassay for X. They don't say, we determined today again that we don't need the bioassay and for Y. We don't see those judgments on the record. So that is going to be difficult for us to base any decisions on.

Then, the other thing is, when we do see a problem with the bioassay, typically, in the record, what, in fact, happened is, [identifying information redacted] at one point said we didn't really have a program for actinium, radium, and thorium in the fifties, in the early fifties, until their procedure came out in 54, I believe.

So they owned up to that. So this is an example of how they have handled the negative judgments, but that's all we've got, as far as I know.

MEMBER CLAWSON: Well, I guess I

kind of need to have it cleared up, too, because my understanding of this [identifying information redacted] document was the Holy Grail of all. My understanding was that there was potential for that in these areas for all this time. And now I'm hearing that, no, it's only for this time.

My question, too, is, Brant, you have put that they had certain projects going on from this date to this date, and then they stopped like this. I don't really think that they would just throw everything into a barrel and clean it up and walk away.

I am kind of wondering how long it took them to get rid of the process, how long it took them to clean this up and get it out and where did they store it and what did they do with it? Because in my experience, we may have stopped a project six years ago, but in a lot of our cells it was still sitting there for years. We never cleaned it up until years down the road, but we were off the bioassay

programs.

DR. ULSH: Right.

MEMBER CLAWSON: But it was still there.

DR. ULSH: Okay.

MEMBER CLAWSON: I was really under the impression this [identifying information redacted] document was not -- I was under the impression that there was a potential for these.

DR. ULSH: Okay. I can maybe clarify that, Brad. The purpose of the [identifying information redacted] document, I think it was written in 95-ish, give or take, and revised maybe after that. The purpose of it was, okay, we're now facing a large-scale D&D of the site.

MEMBER CLAWSON: Right.

DR. ULSH: Before I send workers into D&D, I want to know what potential nasties they might encounter while they are there. So you can imagine, if you were in

that position, you would take the position that, if there's any indication at all that actinium, for instance, might have been there, we're putting it on this table, so that they do the appropriate monitoring for it or, similarly, for thorium, uranium, plutonium, whatever.

So it's meant to be all-inclusive in terms of what might have potentially been there, enough that you would say, as a D&D manager, I had better be doing some monitoring for this. That was the purpose of the [identifying information redacted] document.

MEMBER CLAWSON: And I understand that now, but previously I did not understand that that's what this document was for. I thought this was going back in time and showing the potentials that were in these rooms for all these years.

DR. ULSH: Okay.

MEMBER CLAWSON: That's what I took as this document.

But, also, the thing is that you cut off at 64, or whatever, that there was no more exposure. I hope that there's something there that can prove, yes, that operation may have stopped, but, again, I know from my experience that it takes years to take care of a lot of these problems.

DR. ULSH: Let me give you an example of exactly what you're talking about. The radium, actinium, thorium separations in the old cave were the basis for the SEC. That program operated, I think, 1954 or 1955. So you might ask, well, why, then, do we extend the SEC period up to 59.

MEMBER CLAWSON: We know why.

DR. ULSH: Well, it's because there were several iterations of trying to D&D the place. Starting in, I think, 57, they tried, went in and characterized afterwards, and decided this isn't clean enough. That effort extended all the way up to 1959, when they concreted in the whole place. Well, was

it done there? No. That's why we have a radon problem in the next 20 years.

MEMBER CLAWSON: Right.

DR. ULSH: Let me give you another example. The thorium refinery program, they had planned to do a thorium-232 refinery, and in anticipation of that, they received a large quantity of thorium-resilient oxides, among other material.

MEMBER CLAWSON: The K-65 stuff.

DR. ULSH: No, monazite, not K-65.

MEMBER CLAWSON: I see some K-65 --

DR. ULSH: That's a different one. That's different. The example I'm talking about is only thorium refinery.

Shortly after they planned to do that and received the material, they canceled the project. The thorium refinery, I think they did a couple of runs, but shut it down before it ever operated.

Now I'm left sitting here with

these drums of thorium-232. What am I going to do with it?

Well, they stored it onsite. It is the subject of numerous re-drumming because it was corrosive. That happened in the summertime, the summer months, because it was stored outside, and that's when you want to do it, when it is warm.

Again, a perfect example of presence onsite, but they were only doing active operations in the summer months. It is an intermittent project.

Eventually, they dumped it into Building 21, which is located on the south boundary, one of the boundaries of the site. I think it's south. I might be wrong on that. Anyway, it is towards the unoccupied side of the site.

It sat in Building 21 up until 1970-something, when they contracted with a company to come in, haul it away.

That is exactly the kind of

situation I think that you are talking about, and it makes it a good point that I think I'm trying to make. That is, yes, the material is present onsite, but that is not the end of the story. You have to consider what was done with it and when. Was it an intermittent operation? And what was the exposure potential during that time?

CHAIR BEACH: Can I jump in?

DR. ULSH: Sure.

CHAIR BEACH: Is it time for a break?

(Chorus of yeses.)

(Laughter.)

CHAIR BEACH: 11:35, does that work for everybody?

MR. KATZ: Okay. So I am just going to put the phone line on mute until 11:35.

(Whereupon, the above-entitled matter went off the record at 11:18 a.m. and resumed at 11:36 a.m.)

MR. KATZ: This is the Mound Working Group and we are reconvening after a short break.

CHAIR BEACH: We are still discussing the adequacy and completeness of data. I believe, Bob, you are ready for the --

DR. BISTLINE: Yes, let me key up one other issue. That is just to make a point of it, and that is the issue that we kind of slid over in the process here that dealt with the recovery in the gross alpha, that when they are bringing down all of this, eluting this down, the question that Mound bioassay personnel did not specifically evaluate whether there was a differential in recovery for particular actinides recovered with the gross alpha procedure, nor has NIOSH provided the differential recoveries of alpha emitters.

I think this is a serious question, because if you had an instance where, for instance, thorium-232 and

palladium-231 had only 10 percent recovery, it is going to make a big difference. If they are not coming down equally, you don't have an equilibrium situation. So this is a concern on the part of SC&A.

There is nothing that we have been able to find dealing with this issue, with the efficiency, whether there was equal efficiency.

DR. NETON: Are you talking about the anion exchange column, Bob?

DR. BISTLINE: Yes, yes.

DR. NETON: When we did the stripping off --

DR. MAURO: No, no, no, the gross. The gross, right?

DR. BISTLINE: Oh, the gross, yes. This is on the gross.

DR. NETON: It is alpha with cerium precipitations?

DR. BISTLINE: Yes.

DR. ULSH: I would direct you to

SC&A Comment 1-7 in our Response. The comment was, it is important to validate the ER's assumption that the chemical recovery is equivalent for all alpha emitters in the generic gross alpha procedures.

DR. BISTLINE: Yes.

DR. ULSH: So our response, Response 1-7 says that, the ER makes no statement, the Evaluation Report makes no statement that the chemical recovery for all alpha emitters is equivalent. However, for the MLM1-003 procedure, radium recovery averaged 94.3 percent. The actinium/thorium fraction recovered an average of 96.3 percent. The reference for that is [identifying information redacted] and [identifying information redacted], 1954, pages 10 and 8, respectively.

Plutonium also carried through in the thorium fraction, as did protactinium. For this reason, Mound considered this a gross alpha procedure. The reference for that is

Sheehan, 2009.

MS. ROBERTSON-DEMERS: Excuse me, but the [identifying information redacted] documents are a different procedure than the rapid gross alpha procedure for plutonium. They are relevant to the radium procedure, as I understand it, and were modified for plutonium.

DR. ULSH: Well, I believe we called it the MLM1-003 procedure. You're right, we are talking about MLM1-003, which is what they used for actinium, thorium, and radium.

So what we are saying here is I don't think the gross alpha was used necessarily for radium, actinium, and thorium. Just MLM1-003 is used for that.

Then, to finish the response, recoveries for plutonium bounds primary bioassay need in the late 1950s, according to Sheehan, 2009, when monitored and investigated, as documented in Sheehan, Woods,

and [identifying information redacted], 1963.

So we have at least addressed, prepared a response to SC&A on this issue. So, if the Working Group has further concerns, we would be happy to follow up, if you want to task us with a follow-up item, but that's our response that is on the table.

MS. ROBERTSON-DEMERS: I just want to make it clear that the procedure from 1954 that you are talking about is not the same as the rapid gross alpha procedure for plutonium.

DR. ULSH: I understand, Kathy, and I'm not saying that MLM1-003, which is what is clearly referenced here, is the same as the rapid gross alpha procedure. What I'm saying is the recovery for the technique that was used for actinium, thorium was as specified here, and that's MLM1-003.

MS. ROBERTSON-DEMERS: And that's only applicable to the radium, actinium, and thorium era.

DR. ULSH: Well, if you use the

same technique in a different year and do it the same way --

MR. HINNEFELD: Can I ask kind of a process question here? What Brant has read from is something that we prepared and submitted to the Work Group some time ago.

DR. ULSH: November 2009.

MR. HINNEFELD: Okay. So what we are hearing today is that that response did not satisfy the question. That's what we're hearing today.

DR. ULSH: I don't know. I guess that's what I'm asking.

MR. HINNEFELD: Okay. Well, I'm just trying to sort out where we are.

DR. ULSH: Yes.

MR. HINNEFELD: But we have not yet seen a description of the deficiencies in our response. So wouldn't that be the next step in the process?

MR. FITZGERALD: Yes, this is a recent dialog, yes. It is a recent dialog.

There has not been an opportunity for exchange. I mean we're talking a little over a month. So this is real-time in a sense.

MR. HINNEFELD: I mean we can do that today, if you want, but it sounds to me like an additional response. I mean, if this response is not adequate, and as I understand it, there was a procedure called the gross alpha procedure --

MS. ROBERTSON-DEMERS: Actually, it was called the plutonium bioassay.

MR. HINNEFELD: Okay, it was called the plutonium bioassay.

MS. ROBERTSON-DEMERS: To confuse everybody.

MR. HINNEFELD: Okay. That doesn't help. But they considered it gross alpha because it brought down things in addition to plutonium.

MS. ROBERTSON-DEMERS: Right.

MR. HINNEFELD: It brought down everything but radium, is what I heard a while

ago. Is that right?

MS. ROBERTSON-DEMERS: Right.

MR. HINNEFELD: Okay. And there is a state of recovery for that procedure, and the comment here is that, well, this recovery was stated to be that, but they never really evaluated bringing down thorium or uranium or americium, or whatever the other alphas were that they were bringing down. So they didn't really evaluate that. So how do we really know recovery is 60 percent? And how do we know that that is a suitable -- in order to interpret this gross alpha result for non-plutonium intake? So, essentially, that is the issue.

I don't know. I don't know if we need to research more or if we can answer that today or not.

MS. ROBERTSON-DEMERS: The recovery that they used was 90 percent.

MR. HINNEFELD: Okay.

MS. ROBERTSON-DEMERS: All we want

to know is that the thorium came down at 90 percent, the uranium came down at 90 percent.

MR. HINNEFELD: Right. So, to the extent that a gross alpha analysis is used for non-plutonium, then there is this open question there of, is this 90 percent recovery really appropriate for these other radionuclides? That's the question. Okay.

So I think we understand the question. I don't know if we can talk about that today or not.

DR. ULSH: What radionuclides are you concerned about? I assume uranium is on that list.

MS. ROBERTSON-DEMERS: Uranium, thorium, americium, protactinium.

MR. FITZGERALD: Weren't these identified in the original White Paper?

DR. BISTLINE: Yes. Thorium, protactinium, uranium, plutonium, and other radionuclides.

DR. ULSH: I have got thorium,

uranium, protactinium. What am I missing? There were more of these --

DR. BISTLINE: Thorium, protactinium, uranium, plutonium --

MS. ROBERTSON-DEMERS: Yes, and there's a couple of others that he said came down, americium --

CHAIR BEACH: But they're in your White Paper.

MR. FITZGERALD: Those were identified in the White Paper that went over in April, it would have been.

DR. ULSH: Well, basically, what I did in our response document is that I went through piece by piece.

MR. FITZGERALD: That is what I'm just wondering. You know --

DR. ULSH: It may be in here, but --

MR. FITZGERALD: It may be in there. I don't have it. We can check.

DR. ULSH: Okay. How about this?

I've got thorium, uranium, protactinium, plutonium, and americium. If we're missing any, let us know. Is that reasonable?

MS. ROBERTSON-DEMERS: Well, and the other one that came up in the interviews, curium and we're not sure if that comes through or he wasn't sure it came through.

DR. ULSH: If it is agreeable, Josie, what I will do is go back and look at the references again and see if I can come up with numbers for recovery for these other radionuclides for the gross alpha technique, or I've also got to check to see whether thorium was actually, whether this was the technique used for it. I'm not sure. I can't say at the moment.

DR. NETON: But let me ask a question. I thought earlier I had heard that this gross alpha technique was used in general, but there was specific concern about some operation that would rely on some other process. Is that not correct?

DR. ULSH: Well, the way it was, Jim, it was a 20-or-so-step procedure, and at different stages in those 20 steps -- maybe it was after; Don, correct me if I'm wrong -- but they did the same procedures up to a point, and then they would have a branch. Okay, if we're concerned about thorium, we're going to do this one elution. If we're concerned about something else, we will do this different elution. I think that's --

DR. NETON: But we're talking about the gross alpha, though. We didn't go further.

MS. ROBERTSON-DEMERS: The first 20 steps.

DR. NETON: But the gross alpha would bring down all the gross alpha emitters. I mean, presumably, that's what they're saying. This is the sort of cerium precipitation is the way I understand it.

DR. ULSH: Right.

DR. NETON: And they would not go

any further with that unless they believe there to be some type of a potential exposure, unique exposure scenario, where they could go and isolate the individual radionuclides.

DR. ULSH: Right.

MS. ROBERTSON-DEMERS: Well, there's some question as to when they started routinely implementing anion exchange, which is now part of the procedure.

DR. ULSH: Well, the best documentation -- I didn't know that there was a question on that. That's '66 and '67. That is when they did the anion exchange.

MS. ROBERTSON-DEMERS: Actually, they did it starting in '82, according to [identifying information redacted]. What we brought up was that the date provided by [identifying information redacted] may or may not be the right dates, and that's what Warren was saying on the phone.

DR. ULSH: He was? Maybe he did. Maybe I missed it. I don't know.

DR. NETON: Okay. Well, I guess I didn't want to complicate the issue here. It sounds to me like we've got an assignment here to go back and look at the quantitative processing of these samples for different radionuclides.

I would suspect, you know, I've done chemistry like this before. I would be surprised if there was a differential. I mean the rare earths go down -- cerium precipitation will bring down most of the stuff out of the solution.

MS. ROBERTSON-DEMERS: Well, let me clarify here. It is the first 20 steps of the program. It's the rapid gross alpha determination, is what we are talking about, not the anion exchange.

DR. NETON: I understand, but I don't think there's 20 steps in a gross alpha determination, are there? That sounds to me like --

MS. ROBERTSON-DEMERS: Well, I was

trying to communicate to Brant, you know, he can go back and look at the procedure.

DR. NETON: Yes, but you're talking about the gross alpha, where there is no attempt made to isolate individually the radionuclides. I understand.

CHAIR BEACH: And, Kathy, you will get other radionuclides if they don't have them on the list.

DR. ULSH: Okay.

DR. BISTLINE: Ready to move on? I think the next issue is, and it sort of goes into the same vein as what we were discussing with gross alpha, but this is a different issue. It's the beta gamma issue, beta gamma emitters.

First of all, the fact that the availability of bioassay technique does not equate to appropriate implementation of the fact that there is an absence of beta gamma in the internal monitoring period for a majority of the years when beta gamma emitters were

present at Mound, particularly the production era.

Urine bioassay data have been located for cesium-137 in '93 through '95; cobalt-60, '93 through '95; manganese-54, '94 through '95, and the strontium-90, '93 through '97.

And NIOSH has indicated that beta gamma emitters played a minor role at Mound, in Mound activities, and for the most part only existed in trace quantities, research and production-scale operations. They have not produced objective data regarding the quantities of material handled or processed or the concentration for these radionuclides.

Going along with this, well, let me say that the Road Map -- and again, this talked about Road Map, but it identifies situations where beta gamma emitters were handled in the absence of alpha emitters. However, a method of reconstructing doses from beta gamma emitters has not been presented,

closely linked with the issue are previous SEC determinations made for other sites.

It gets into the issue that Mound extracted polonium-210 from bismuth targets, irradiated at Hanford for the development of initiators, beginning in 1943 at the Dayton Laboratory, and work was transferred to the Mound lab in Miamisburg, Ohio, in 1949. This process was started in February 1949 at Mound.

NIOSH stated that the Monsanto Chemical Company Evaluation Report, that polonium impurities produced a number of activation products that were beta emitters. Silver-112 was a particular problem with beta particles, and there are others, other beta-emitting radionuclides of concern, antimony and iron, cobalt, cesium, bismuth, tin, zinc, mercury. I could give the isotopes of those, but will not to save time.

I think it is more important to deal with this issue, and that is that NIOSH has determined at this time that there's a

lack of sufficient monitoring and source term data for nuclides other than polonium between 1943 and '49 at MCC, Monsanto Chemical Company. Although polonium bioassay data used in conjunction with coworker data from Mound lab, an ambient environmental polonium intake, internal intakes could be used to support internal dose reconstruction, due to a lack of information, internal exposure data for the use and production of radionuclides other than polonium.

NIOSH has concluded that there are insufficient data available to support internal dose reconstruction with sufficient accuracy at the Monsanto Chemical Company for the time period 1943 through 1949. This inability to complete internal dose reconstruction at MCC for the 1943 through '49 time period is because of a lack of information and internal exposure data for radioisotopes other than polonium, such as antimony, and so on.

NIOSH has provided justification for excluding Mound workers from 1949 through September of 1949, although they have granted an SEC for MCC for the period immediately prior to this, and for Mound, starting in October of 1949.

But polonium work continued at Mound through 1971, with decontamination of the major polonium production area completed in '73. There has been no explanation of why the situation at MCC, which was a basis granted the SEC at MCC, is different from that in the Miamisburg location.

In addition to the polonium work, beta gamma emitters were associated with operations at LLNL and LANL, where SECs were granted for fission and activation products prior to '74, actinium, curium, neptunium, thorium, strontium.

Limited beta gamma measurements at Lawrence Livermore National Labs and Los Alamos National Lab were not suitable for dose

reconstruction. Yet, the absence of data prior to the 1990s for beta gamma emitters at Mound does not warrant an SEC.

Again, at LLNL and LANL, the availability of workplace air monitoring is limited and covers only some buildings and time periods. For many situations, NIOSH has indicated materials were handled in small or trace quantities. However, they have not provided quantitative information, ratios of secondary radionuclides, of primary radionuclides, or the relative dose secondary radionuclides will deliver, and whether it will influence the claims.

So I'll stop at that because that kind of summarizes the issue of beta gamma emitters issues that we have.

MR. STEWART: Firstly, I would just like to make a couple of comments about Monsanto Chemical Company's approach at the Mound site, because there's really no data out there right now.

Before, when we had some information on the Monsanto Chemical Company or the Dayton Laboratory operation in the Mound site Technical Basis Document, but that has been taken out, since it was separated out as an SEC.

I just wanted to bring up a couple of points. We have a fundamentally different exposure at the Dayton Laboratory operations than we do at the Mound site. The Dayton Laboratory operations did some other operations when they were researching the parts that they were fabricating, eventually using polonium. But, in the early days, there was a significant amount of work with radium as well. There were several other radionuclides that they look at.

And you can see in the internal -- in the external dose records that are available -- they're not complete -- there are some very large beta doses to some of the researchers, and those are episodic, or

intermittent, rather, based on what they were doing at the time. Okay?

Another important fact to keep in mind was that, when they eventually settled on the polonium process, they had not yet settled on the source of that material. By the time they got to Mound, they had determined that the best way to fabricate polonium was not from recycling it from lead tailings and things like that. But, by processing irradiated bismuth bricks, which went through the reactor at Hanford or another, I think Oak Ridge, which are fundamentally different than fuel. I'll just point that out.

So, by the time they got to the T Building at Mound, they were just using irradiated bismuth. So you have kind of a different source term than you would have in, say, in a fuel operation. Okay?

And finally, the levels of control at the Dayton Laboratory facilities were a lot less. They were working in fume hoods and in

some cases bench tops. So there were a lot of internal doses at that point, and they had very high levels, levels that would scare us under our current controls.

Part of the reason, they kept getting better controls, but they couldn't really get there until they had designed a purpose-built facility to do that. Okay?

Now they also had other radionuclides there. I mentioned radium earlier, but they only had bioassay for polonium. So all we can predict was the polonium intakes from their records. Okay?

Having said that, I'll turn it over to Brant to respond.

DR. ULSH: Well, yes, I was going to make some of the similar points. If you think about how we have handled other situations in an SEC context, we have repeatedly been questioned on our ability to back-extrapolate in time. I would say that that cuts both ways.

You cannot back-extrapolate blindly in time from Mound to Monsanto and assume necessarily that the similar problems exist. And in fact, there's reason to think that they didn't.

A couple of the reasons Don mentioned. The source term is different. But, also, one of the main reasons of building the T Building at Mound Laboratory was to build on the experience of the Dayton Lab, take in mind the problems that they experienced at Dayton Lab, and to improve the controls that were instituted in the T Building to minimize exactly the exposure problems that they experienced at Dayton Lab.

It is true that there were some beta gamma emitters produced and activation products, in particular, produced in the cans that were used to encase the bismuth before they went through the reactor. That was the source of a lot of the beta and gamma problems associated with the polonium program.

I would also remind you that we have thousands of, I believe it's thousands, of polonium bioassay. It's hard to imagine that, if you're talking about contaminants in polonium, you would, number one, be concerned about the minor constituents and not the polonium. The polonium would be the primary radionuclide. If you got an intake of these other alpha or beta -- sorry -- of these other beta gamma emitters, you would see it in a polonium intake.

So some of the other programs, and there weren't many, that involved beta gamma emitters would have been the reactor waste program. Again, that is entirely within the SEC period. I don't think it's claimant-favorable to say that the people were exposed to beta gamma emitters during the SEC period and we cannot reconstruct it. If you want us to say that, we will talk about it, but I don't think you really do.

In terms of other things that were

handled in isolation, we are not aware of any big project, you know, major-scale programs that occurred with beta gamma emitters in isolation that would have led to a significant exposure potential.

Sure, on occasion, I think there was a small strontium operation, and to call it an "operation" is even an exaggeration. I think it involved two people, Don? Two research chemists, we know who they were. It is not like they had a strontium program or a cesium program that we're aware of.

I don't think I'm misspeaking, am I?

MR. STEWART: There was one operation of cesium; they were pulling it out of a waste stream.

DR. ULSH: Right, right. So I just don't see how there was a large exposure potential to beta and gamma emitters at Mound.

Again, the primary purpose, the primary work at Mound was, first, polonium and

then later plutonium-238. That's the haystack that we're talking about here, not the needles.

MS. ROBERTSON-DEMERS: Do you know the timing of the exact date when they started the differential processes for polonium, meaning extracting it from the lead, extracting it from the bismuth slug, and so forth?

MR. STEWART: That is in the Dayton Laboratory period. So it is not specifically covered.

DR. ULSH: Well, at Mound, Kathy, I think that was one --

MS. ROBERTSON-DEMERS: What I would like to do is compare the processes that were used for polonium extraction from the beginning through '73.

MR. STEWART: Dayton Laboratory is already an SEC.

MS. ROBERTSON-DEMERS: I want to compare the different processes at Dayton Lab

with what happened at Mound.

DR. ULSH: If that is something the Work Group would like us to follow up on, we can look for process descriptions for the polonium program at Mound and Dayton Lab.

MS. ROBERTSON-DEMERS: Right now, I am just asking for the dates for when the changes in the processing of polonium happened.

DR. ULSH: Well, whenever it started at T Building, I don't know; I'm guessing here, Kathy, but I know that Mound Lab started operation in 1949. I believe that it was either '49 or '50 that the T Building went operational. It might be a year or two --

MS. ROBERTSON-DEMERS: Okay, I'm going to ask the question differently. What was the process at the Dayton Labs when it closed, the polonium process? And what was the process in T Building in 1949?

MR. STEWART: The T Building

process was irradiated bismuth bricks. Later irradiated bismuth cans. That's what they were set up to do.

MS. ROBERTSON-DEMERS: What was the process in 1948 at Monsanto?

MR. STEWART: I honestly don't know when they stopped using lead tailings and when they started to do the bismuth bricks prior to the Mound operation. I can get you that information, if you feel it is useful. However, I will point out that that is covered under an SEC at this point.

MS. ROBERTSON-DEMERS: What we are trying to understand is exactly -- I'm trying to get a better understanding of exactly why, and I know that you guys have talked, given your points why the situation at Monsanto, where you're calling out the same radionuclides that were in the activation products, is different than the situation in 1949 at Mound.

DR. ULSH: Well, I think the

primary reason that we consider the situation to be different is that it is a totally different facility, and the T Building at Mound was built specifically to minimize the exposure potential that occurred at the Dayton Lab. The exposure potential was not only from polonium, but, as Bob I guess mentioned, all of their activation products.

That's why they built T Building the way they did, a closed system, remote handling, to minimize the beta and gamma exposure potential.

MS. ROBERTSON-DEMERS: Okay. So what you're saying is the difference is radiological controls?

DR. ULSH: As opposed to what? I mean, yes, radiological controls certainly plays into it, yes.

MS. ROBERTSON-DEMERS: I'm trying to understand the differences between them.

DR. ULSH: Radiological controls is certainly a significant factor in this. I

agree with that. And again, the source term differences that Don described.

MS. ROBERTSON-DEMERS: And I guess I'm not quite understanding why Monsanto calls out, you know, all of these specific radionuclides.

DR. ULSH: Again, because there was an exposure potential at Monsanto Chemical Company to be exposed to these different radionuclides because of their lack of radiological controls as compared to, say, for instance, the T Building.

MS. ROBERTSON-DEMERS: Okay. So it's coming down to radiological controls?

DR. ULSH: By and large, yes.

MS. ROBERTSON-DEMERS: Because the isotopes --

MEMBER ZIEMER: Well, wasn't it also process?

DR. ULSH: And process for at least a portion of the time.

MS. ROBERTSON-DEMERS: Well,

that's what I'm trying to understand.

DR. ULSH: So I think, then, what you're asking -- Don, if I understand what you said, was they started with, let's just call it, a led tailings recovery effort at Monsanto Chemical Company. At some point in time, and it's probably not a bright line -- I'm just guessing here -- they decided, no, this isn't going to work out; we're going to do irradiated bismuth bricks.

MR. STEWART: Right.

DR. ULSH: And that happened sometime during the Monsanto Chemical Company -- and then that process carried on at Mound. Am I correct so far?

MR. STEWART: Yes. I don't have those dates for you because I wasn't prepared for this question.

DR. ULSH: Yes. If the Working Group is interested in this question, we can try to track down the date at Monsanto Chemical Company, when they switched from lead

tailings to bismuth bricks. I don't know why we need to, but if that's something you are interested in, we will do it.

MEMBER ZIEMER: It sounds like the decision to do that was based on radiological issues, that you had all these beta gammas that were a problem in the process, and there may be some efficiency issues, too, but --

DR. ULSH: I think that might be a major factor, too.

MEMBER ZIEMER: Yes, but from an exposure dose point of view, as I understand, at Monsanto they had a lot of beta gamma stuff that was problems.

MR. STEWART: Well, we don't know a lot about that. Certainly I saw some beta dose rates on film badge results. So I inferred from that that there was a beta gamma problem.

DR. ULSH: Well, and in fact, we have been told that.

MEMBER ZIEMER: But your focus

here is on internal --

DR. ULSH: Right.

MEMBER ZIEMER: -- dose from beta gamma, which per unit activity is typically much lower than alpha, but I guess it's a good question: is the beta gamma, as I understand, Bob, your question, is the beta gamma, do we know that it is insignificant compared to the alpha? Is that sort of the underlying question?

DR. BISTLINE: That's it.

MEMBER ZIEMER: And then --

MS. ROBERTSON-DEMERS: Was it insignificant to all organ cases.

CHAIR BEACH: Is there a specific period you're looking for? Is it the pre-SEC for Mound time period of February --

MS. ROBERTSON-DEMERS: Well, can I break it up into two periods?

CHAIR BEACH: Sure.

MS. ROBERTSON-DEMERS: There is February 1st, 1949 through September 30th,

1949. Okay?

MEMBER ZIEMER: Which is what?

MS. ROBERTSON-DEMERS: Which is an uncovered period that the petitioner requested.

DR. ULSH: February 1st through September what?

MS. ROBERTSON-DEMERS: Through September 30th.

CHAIR BEACH: And you said you were going to break it into two?

MS. ROBERTSON-DEMERS: Yes. Okay. Now there was another piece of information on that time period, too. In addition to the issue with polonium, there was also for a period of time a lack of neutron monitoring, which was also specified in the Monsanto report as rationale for granting an SEC.

DR. ULSH: So are you implying, then, that there are no or, rather, insufficient neutron monitoring during the polonium program at Mound? Is that where

you're headed?

MS. ROBERTSON-DEMERS: Neutron monitoring started up in August. So there was a period of time when there was no neutron monitoring.

MEMBER ZIEMER: But that's a different question.

MS. ROBERTSON-DEMERS: Yes, but it plays into that time period.

MEMBER ZIEMER: Yes, yes.

DR. ULSH: August of '49?

CHAIR BEACH: The same time period you mentioned before, February 1st --

MS. ROBERTSON-DEMERS: Yes.

CHAIR BEACH: -- 1949 to September 30th, 1949.

MR. HINNEFELD: Pardon me. Is the origin of this question that the Mound SEC starts in October something, or when did it start?

MS. ROBERTSON-DEMERS: It starts in October.

MR. HINNEFELD: Of 1949. But you would have to say there was radiological work there starting in February? Is that what you're saying?

MS. ROBERTSON-DEMERS: Right.

MR. HINNEFELD: Okay. Well, I certainly wasn't aware of that.

DR. ULSH: I don't know. All I can tell you is that the basis for the current SEC at Mound was when the material for the radium, actinium, thorium separations came on site. It was not related to the polonium program.

Off the top of my head, I don't have memorized when the T Building went hot. I mean that's a time of transition from Monsanto to Mound. There might be a gap. I don't know.

MR. HINNEFELD: We will have to go check. We are not prepared to do that today.

MR. STEWART: Yes, I will observe that your quotation from [identifying

information redacted] that you're referring to actually says we have neutron monitor prior to September.

DR. ULSH: Are you looking at me or -- okay. I didn't think I said that.

(Laughter.)

MR. HINNEFELD: We'll have to investigate that period of time. That seems to be what the question is about, is that period of time from February to October.

MS. ROBERTSON-DEMERS: The question is there's indication that the polonium process started up in February.

MR. HINNEFELD: Okay.

CHAIR BEACH: Okay, so that's for polonium. Now you brought up neutrons.

MS. ROBERTSON-DEMERS: Well, no, I meant --

CHAIR BEACH: Okay.

MS. ROBERTSON-DEMERS: I'm just saying that, for that same period of time, there's also a question that Monsanto was --

also another reason for granting the SEC was for the lack of neutron monitoring. And you have the same situation from February through August of that time period.

MR. HINNEFELD: Have you cited any references for us about the origin of the radiological work? Or what's the basis of stating that the radiological work started in February of '49? Are those in something you provided to us?

MS. ROBERTSON-DEMERS: I don't know. I'll have to go back and get the reference for you.

MR. HINNEFELD: Okay. I mean it seems to me what you are saying is there is this gap period from February to October of 1949 when radiological work was going on at Mound, when there hasn't been a lot of consideration of how we are going to do that.

I think I'm not sure about the guys in the room, but I kind of thought that, well, Mound started as an SEC. That's kind of

what I thought was going on. Maybe I'm wrong on that.

DR. ULSH: Well, again, it's a little misleading to think of MCC, Monsanto Chemical Company, as one facility. I mean there was Unit 1, 2, 3, 4.

MR. HINNEFELD: Yes, okay.

DR. ULSH: And they involved -- I mean polonium was the primary operation, but, yes, there is this time of transition between MCC and Mound. In terms of when the polonium work actually started at Mound, I don't know. It's not a question that we have focused on. I don't have that off the top of my --

MR. HINNEFELD: Okay. We're not prepared to deal with this question today, but you'll find out.

DR. ULSH: Right.

MR. CHEW: In the [identifying information redacted] document.

DR. ULSH: Go ahead and tell us what [identifying information redacted] says.

MR. CHEW: The [identifying information redacted] document said in R Building, 127, and just gave a time frame. In 1948, the polonium pilot program started in room 127 and room 120, 1948, but did not give any more details.

MR. HINNEFELD: In R Building.

MR. CHEW: In R Building, in rooms 127 and 128.

DR. ULSH: I think you're right. I think there's gap between the end of the Monsanto SEC and the beginning of the Mound SEC. I wouldn't argue with that.

In terms of these other issues that you talked about a lot, you said --

MS. ROBERTSON-DEMERS: There's that time period, and then the other concern is the beta gamma emitters from March 1st, '59 forward -- this is the other section of it -- with the lack of bioassay data.

DR. ULSH: Which beta gamma emitters are we talking about? From '59

forward?

MS. ROBERTSON-DEMERS: Involves cesium --

DR. ULSH: Well, again, I'm not aware of any --

MS. ROBERTSON-DEMERS: Anything that was a part of the aluminum --

DR. ULSH: So you're associating these with the polonium program?

MS. ROBERTSON-DEMERS: Not exclusively.

DR. ULSH: All right. As I understand your concern, you've named cobalt, cesium -- I don't know, maybe a few others, I don't know -- associated with the polonium program, but you're also, I think, saying that these presented an exposure hazard at Mound outside of the polonium program. Am I --

MS. ROBERTSON-DEMERS: The gamma emitters were not only associated with the polonium program, but they were associated with other programs --

DR. ULSH: Yes, they were.

MS. ROBERTSON-DEMERS: -- outside of the SEC.

DR. ULSH: Okay. Can you give me a hint as to what you're talking about, which ones?

MS. ROBERTSON-DEMERS: What about the process in the WD Building? I mean this comes back to the Road Map, which you're asking us to provide additional data for.

DR. ULSH: No, actually, I'm just asking what your concerns are. I just want to make sure that we answer your concerns.

MS. ROBERTSON-DEMERS: So that kind of goes into WD 101, 104.

MR. CHEW: What page are you on there, Kathy?

MS. ROBERTSON-DEMERS: Ninety, 94, 95.

MEMBER ZIEMER: Is this in the Road Map, now --

MS. ROBERTSON-DEMERS: Yes.

DR. ULSH: Okay. This --

MS. ROBERTSON-DEMERS: It's page 98, 112. And there's several examples of where this stuff is coming up. The same page, 98, WDA118A.

DR. ULSH: Okay. Hold on, just give me a sec. Do you have a time frame, Kathy? I'm not looking at the Road Map right now.

MS. ROBERTSON-DEMERS: For which one?

DR. ULSH: Your concern about WD Building.

MS. ROBERTSON-DEMERS: Okay. Let me go backwards here.

For WDA112, for example, 1980 to '84.

DR. ULSH: Okay. Here's what I can tell you about cesium at least. It was a one-time shot. Oh, by the way, the reference here is -- do I have a SRDB number on there? Well, the MLM number is MLM-2929, and it's got

an SRDB cover sheet here and unfortunately, I'm not adept at picking out the SRDB number.

First author is W. H. Bond. What we're talking about here is removal of cesium from a salty aqueous waste with sodium tetraphenylboron.

What I have highlighted here is that in this waste it's supernatant. The cesium-137 counts ranged from 570 down to 4 counts per minute.

Okay. "The waste disposal group" -- this is from the intro -- "The waste disposal group at MRC Mound has the responsibility of processing low-level contaminated aqueous waste generated during normal operations. Usually, these wastes are contaminated only with plutonium-238. Occasionally, other isotopes, such as actinides, occur in the waste.

"With minor process alterations, these isotopes are easily removed. However, cesium-137 leaked from a tank used for

development waste and was the major contaminant in one 4600-gallon batch."

"This waste" -- I'm moving around now. This is not a continuous quote. "This waste was not likely to be encountered again. The physical processes, such as reverse osmosis and evaporation, were eliminated because of the one-time-only aspect."

So the point I'm making there is that this is not an ongoing program. This document clearly indicates that it is a one-time-only situation.

"The cesium concentrations are provided."

I think that might be all -- and I think this is dated, this is 1982. So it corresponds with the time frame you're talking about. I believe someone might have said -- I don't know if it was you -- that there were some cesium bioassays around that time period.

MS. ROBERTSON-DEMERS: Ninety, it was in 1990.

DR. ULSH: Okay. Nothing around this time period?

MS. ROBERTSON-DEMERS: No.

DR. ULSH: Okay.

MEMBER ZIEMER: What was the date there?

DR. ULSH: Well, it's a little unclear, Paul. I think I guessed from the references -- there's a card attached; I can show it to you -- 1982.

MR. STEWART: The data captured says 8/13/81.

DR. ULSH: Okay, '81 or '82.

So that's what I know about cesium in WD Building, this one-time operation. I'm not saying there's nothing else. I'm just saying this is all I'm aware of. If there's anything else --

MR. STEWART: Are you talking about bioassay results in the mid-nineties?

DR. ULSH: Well, she said that there are cesium results in the nineties.

MR. STEWART: Are you talking about in vivo results?

MS. ROBERTSON-DEMERS: No.

MR. STEWART: They're not in vivo results?

MS. ROBERTSON-DEMERS: No. They're urinalyses.

DR. ULSH: I'm not sure. I haven't seen the cesium results in the 1990s, and I don't know what the rationale for taking them was.

MEMBER CLAWSON: When you say a one-time use, was it one time for a great period or was it just one day we did this?

DR. ULSH: No, I didn't say one-time use. The authors did.

MEMBER CLAWSON: No. Well, this is what I'm saying. I'm trying to understand if one-time use, is this a run of so much?

DR. ULSH: Yes. Yes. It's one batch of waste that was contaminated with cesium. It says, "This waste was not likely

to be encountered again. The physical processes, such as reverse osmosis and evaporation, were eliminated because of the -- quote -- one-time-only aspect. That's the words of the author, not me.

CHAIR BEACH: But no dates? Or did you say '80 --

DR. ULSH: Well, this document I think is dated around 1982, but it might be '81 because that's what the data, the document date says. So it's either '81 or '82, Josie. That's about as close as I can narrow it down right now.

MR. CHEW: The [identifying information redacted] document is between '80 and '84. So you're right.

DR. ULSH: So this is a bit more specific of a range.

MR. STEWART: I will just make a statement here about dose reconstruction. If we were to encounter cesium-137 results in a claim, even outside a period of concern that

was mentioned in the TBD, we would assign this dose for negative results, and we would assign, you know, an assumed dose, based on bioassay results.

But the results are in the record. It's going to end up in the dose reconstruction, regardless of whether the TBD says to do it or not.

MS. ROBERTSON-DEMERS: The point is there are no results.

MR. STEWART: I thought you just said there were results.

DR. ULSH: No, no, she said in '90.

MS. ROBERTSON-DEMERS: No results in '90, '93 to '95.

MR. STEWART: Okay.

MS. ROBERTSON-DEMERS: Not in '80.

MR. STEWART: We're not presuming the source term during that time. But, even though we're not presuming the source term during that time, it's going to end up in the

dose reconstructed because the result is in the record.

DR. ULSH: No. Hold on. Hold on, Don. I think you're confused here a little bit.

What we have here is a description of an event or a run here of a waste processing event that happened around the 1981 or '82 time frame, and at least what they are saying here is that there are no corresponding bioassay results for cesium.

MR. STEWART: For that event. I see.

DR. ULSH: And we see cesium results in '93 to '95, is what they're saying. And what you're saying, I understand what you're saying. For those results in '93 to '95, we would include them in the dose reconstruction. That doesn't address this situation here, though.

MR. STEWART: That's correct. It does not address that.

DR. ULSH: Right? Have I accurately summed up everybody's words?

I'm not sure. I mean, obviously, well, first of all, at least according to this document, it says that it is a one-time-only thing. It's not an ongoing thing. That is what this document appears to indicate to me.

I would have to look at the details of the process to determine whether or not there was an exposure potential. I mean, if this is an entirely closed system, you wouldn't expect there to be an exposure potential, but I can't say that at this point in time.

If you would like, we can examine this a little further.

MS. ROBERTSON-DEMERS: I thought that was part of the table we were doing.

MR. FITZGERALD: Yes. In the larger context of providing, I think, specific examples with the question of exposure potential, this seems to be part and parcel of

that, that we would identify, for example, this one as one that we would want you to pursue and give you some -- actually, in this case, we actually have the evidence that would be the starting point perhaps for you to look at it.

So we would include this as part of the list that we would provide. Rather than trying to parse this out, make it part of the package.

MEMBER ZIEMER: Do you have the concentrations of the solutions?

DR. ULSH: The abstract, Paul, says the concentration in the supernatant of the waste was from 570 down to 4 counts per minute per mil.

MEMBER ZIEMER: Well, you would have to know efficiencies, but --

DR. ULSH: I think there might be more information, hold on.

MR. CHEW: That is down to the nanocurie level then. It says in nanocuries.

DR. ULSH: There is a -- hold on. Let me read it to you, so I don't misstate it.

"However, cesium-137 leaked from a tank used for development waste and was the major contaminant in one 4,600-gallon batch."

MEMBER ZIEMER: Is that the concentration of that batch?

DR. ULSH: I believe -- that's the way I'm interpreting it.

DR. NETON: What year was this batch?

DR. ULSH: Well, Jim, we've got two possibilities. The data capture sheet says 8/13/81.

DR. NETON: Okay, '81. That's close enough.

DR. ULSH: Yes, so it's '81 or '82.

I don't know. There's some interpretation here. I don't want to give you these other -- I'll show you these numbers, if you would like, but I don't know quite how to

interpret.

DR. BISTLINE: What source term is this waste? Does it say? Or where it came from?

DR. ULSH: Not specifically here.

MR. STEWART: It said it was from development wastes.

DR. BISTLINE: Yes, so that's what I'm wondering. Something was going on that generated this.

MR. STEWART: Well, it leads me to wonder if that wasn't that bismuth phosphate plant process way back when.

DR. BISTLINE: Now we're in the eighties. It makes you wonder.

MR. STEWART: Yes, but if they didn't process it, it's been around in the waste stream for a long time. You know, cesium is going to come into solutions. It's quite easy.

DR. BISTLINE: Yes, it's half-life and everything.

DR. ULSH: I would have to look at this closer, but on my cursory inspection here -- it's been a while since I've looked at this -- I don't see any clues that would answer your question or yours one way or the other.

DR. BISTLINE: It just raises a question in your mind as to where it came from and what was going on.

MEMBER CLAWSON: This also comes back to what I said earlier about, when a process stopped --

DR. BISTLINE: Yes, certainly.

MEMBER CLAWSON: -- where did it all go? You know, if this had been sitting around since the forties in there, or whatever, that's --

DR. BISTLINE: Well, we ran into it at Rocky. We knew of a project that was going on in the 83 Building back in the 1960s, and in the 1990s they came to me and I said there had been some plutonium-239 in there,

and everybody said, no, it couldn't have been; they never did have plutonium there. I knew of one project that went on and, sure enough, they found it under the lathes and up in the ventilation system.

DR. NETON: Well, this is kind of normal operations. It was generated during normal operations at Mound, is what this says. So it wasn't something that had been brought in --

DR. ULSH: Wait where?

DR. NETON: "The waste disposal group has the responsibility for processing low-level contaminated aqueous waste generated during normal operations."

DR. ULSH: Keep reading.

DR. NETON: "Usually, these wastes were -- occasionally, other isotopes occur in the waste."

Yes, but this sort of indicates that something was generated during the normal -- during an operation at Mound. It

wasn't that they brought in this waste from somewhere else.

DR. ULSH: Right. It is not waste that was brought in from somewhere else and processed at Mound. I didn't get that impression from this.

DR. BISTLINE: No, but the question is, where? You know, what room or what building generated this? So who could have gotten exposure? Or was there a potential --

DR. ULSH: If you would like, we can take it as an action item to review this particular situation further.

CHAIR BEACH: And I think it will be part of SC&A's --

(Simultaneous speaking.)

MEMBER ZIEMER: Brant, do we know if Mound had access to whole body counting services at all?

DR. ULSH: Yes, I believe they did, yes.

MEMBER ZIEMER: Because if you had a concern about cesium, I am not sure you would be doing urine analysis. It would be much easier to have -- I mean they didn't have their own whole body counter, right?

DR. ULSH: I believe they did.

MEMBER ZIEMER: Oh, they did?

DR. ULSH: Yes.

(Simultaneous speaking.)

DR. BISTLINE: It was in the seventies because it was after that accident in Rocky Flats, and they came, got plans from us as to how to go about building that lung counter.

MR. HINNEFELD: They had their own at some point.

DR. BISTLINE: Yes.

MEMBER ZIEMER: Cesium distributes in the total body, mostly tissue, but it is pretty easy to detect, either a crystal or a --

MS. ROBERTSON-DEMERS: I think

there was some question as to the type of the whole body counter in the energy range that was affected over.

MEMBER CLAWSON: Well, that comes out --

MEMBER ZIEMER: Well, if you calibrate, it doesn't matter. You can calibrate for cesium. You may have issues of efficiency, but then you just count longer. I mean you can use a crystal that's not designed for -- if I were doing cesium, I would use a big sodium iodide. But if it's a little one that someone was using for --

DR. BISTLINE: 238.

MEMBER ZIEMER: -- 238 X-rays or something, you can still do it, but the efficiencies are just poor.

DR. NETON: Well, I guess the question is, do we have any cesium-137 unusual counts in bioassay records?

MR. CHEW: Yes.

DR. NETON: We do?

MR. CHEW: No, that's the question.

DR. NETON: We need to look at that. I mean, yes.

MEMBER ZIEMER: Well, I wasn't asking that specifically, but it might be a good question. But it just occurs to me, if someone was concerned about internal exposures, I'm not sure I would expect them to be doing a urine bioassay. It's so easy to do these things.

Cobalt would be the same way, a very specific peak. But strontium would be a different problem.

DR. ULSH: Strontium is different.

MR. CHEW: Bob, to answer your question, at least I'm going to reference the [identifying information redacted] document again, recognizing what it is supposed to be for. It says, "In 1948 to 1951" --

MEMBER ZIEMER: Can you talk a little louder, Mel?

MR. CHEW: -- "five shipments of bismuth phosphate was received from Hanford, which included crib materials and samples from two other stages of the process, and the plutonium separation program from irradiated uranium-235, PUREX, and tributyl phosphate materials from Oak Ridge."

DR. ULSH: But that's the reactor waste program.

MR. CHEW: Yes.

DR. ULSH: And what was the end date on that, Mel?

MR. CHEW: And that was '48 to '51.

DR. ULSH: It could have. I don't know.

MR. CHEW: Cesium came along with that. So, when they scooped it off, they probably stored it. We don't know that.

CHAIR BEACH: So let me check in -- excuse me. Sorry. We're getting close to the lunch hour.

How much data adequacy do you

think we still have to go? I was hoping to wrap it up before lunch, but if not --

DR. BISTLINE: Well, there is the completeness issue. That might be a bit of a discussion. I think we ought to just do lunch.

CHAIR BEACH: Okay. So I will suggest that we go ahead and break for lunch then, 12:30 to 1:30.

MEMBER CLAWSON: So am I clear on this last item, that you guys are going to research into it? Part of it was looking at the whole body.

DR. NETON: Well, that's going to be part of, I think, the SC&A list of areas where there were activities that we may not have adequate bioassay data for.

MEMBER CLAWSON: Okay.

DR. NETON: Didn't show up on their list. We certainly would be aware of -- we will pursue that.

MEMBER CLAWSON: Okay.

MR. KATZ: So we are adjourning

for lunch. We will be reconvening, for folks on the phone, at 1:30.

Thank you, everybody.

(Whereupon, the above-entitled matter went off the record at 12:32 p.m. and resumed at 1:31 p.m.)

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

1:31 p.m.

MR. KATZ: Good afternoon. This is the Mound Working Group, the Advisory Board on Radiation and Worker Health. We are reconvening after lunch break, and we're ready to go.

Josie?

CHAIR BEACH: We are going to continue our discussion on data adequacy and completeness.

One agenda item, I am going to note for the record, right after data adequacy and completeness, we are going to go into shallow dose. That should be a very quick topic. One of the members is going to leave early.

So, with that, we will go ahead and I can turn it back over to you, Bob. Would you like to continue?

DR. BISTLINE: Okay. The next issue is the data completeness issue. One of

the issues, one of the big things about this is our concern that dose reconstructors use a comprehensive set of internal and external dosimetry information on individual records.

When one looks at the list of sources of information, internal dosimetry and external dosimetry, it's about a page long of different data files and sources of information. The primary data available for use by the dose reconstructors are internal and external dosimetry information found in the individual's radiation exposure file and electronically through MESH, if printouts are not already available in the file.

NIOSH, in their response to data completeness, failed to address many items raised in Mound's internal dosimetry data completeness, such as, number one, multiple bioassay results for a single day, which are not two independent samples, but one sample that was split.

Two, inconsistencies between the

PORECON and POLON data, and incomplete fecal data in the individual exposure file and electronic data.

And fourthly, incomplete in vivo data in the individual's exposure file and electronic data.

Fifthly, the absence of MJW database results, absent from the individual file and MESH, which contain unique bioassay information for other radionuclides.

To obtain a full monitoring history for any individual, the dose reconstructor must have to consult, may have to consult sources other than the individual file or MESH. Based on conversations with our own individual reviewing DRs, this is not a routine practice.

Another item is tritium data in MESH prior to October of 1981 is only available in milligram and is based on an HTO intake. Log books are in many cases the only source of tritium bioassay data for this time

period. There are two years for which bioassay data has not been located. And based on the approach defined by NIOSH for STCs, this data is critical for assessment of dose.

And lastly, the petition raised the issue of Mound plant employee health records being removed from Mound and buried in Los Alamos, New Mexico. Implicit with the bound records burial in Los Alamos is whether the buried records contain dose reconstruction data that, one, are not available elsewhere and, two, are critical to conducting the dose reconstruction with sufficient accuracy.

The point is that a review of the classified set of records retrieved from MJW Corporation, by MJW Corporation from Los Alamos, which is available at OSTI, does not provide direct evidence that unique dose reconstruction information was available in the buried records. However, the only direct evidence that can be obtained is by digging up the records and reviewing them.

I think that pretty well covers the points dealing with the data completeness issue.

Response?

DR. ULSH: I would first note that a number of these issues have been discussed at length at previous Working Group meetings, and I'm thinking specifically of the buried records issue. It is almost like that conversation never happened because here we go again talking about the same thing.

Also, I tried to catch all the issues that you mentioned, Bob. The first few, anyway, it seems to be it boils down to the dose reconstructor has to look in multiple places to get a complete file.

Without commenting on the merit or not of that, that may be true. It may very well be true. I don't see why that, in and of itself, would be an SEC issue.

If there's an instance where they didn't do that, that would certainly be a

valid criticism of the dose reconstruction. You might even be able to say we should alter our procedures to ensure that that was done for Mound dose reconstructions. I mean that would certainly be a valid criticism there, but that is not, in and of itself, an SEC issue.

Fecal data, yes, it's true, it's the same as everywhere else. You would like to have more fecal data, but, it was for various reasons, it wasn't done a lot, the same as everywhere else. When we have it, we use it. But, primarily, Mound used urinalysis data. So that is true.

MS. ROBERTSON-DEMERS: Can I identify something here? We are not necessarily saying that these issues are SEC issues because somewhere out there the data exists. We just want you guys to be using all the data available.

DR. ULSH: Noted. We will use all the data available. And where we don't, we

should expect to be criticized for that. And if it's not an SEC issue, why are we talking about it in an SEC meeting? That would be my question.

MS. ROBERTSON-DEMERS: Because we haven't closed out Items 12 and 13 yet.

DR. ULSH: If it's an SEC issue, we should be talking about it. If it's not an SEC issue, we should be talking about it either not at all or at TBD review or a dose reconstruction review, depending on where the issue would most appropriately fit.

Now, in terms of the tritium data, I don't know. It seems like you guys didn't get our response on this.

MS. ROBERTSON-DEMERS: Yes, you said that there were a lot of the books also, and the question is, what are you doing? Are you going and pulling those tritium bioassay data out of the log books, which are not necessarily in the individual exposure file or MESH?

DR. ULSH: To answer your question, I don't know if we're doing that yet. Because what we said in our response was that, yes, we originally stated, and you accurately captured what we stated, and that was that pre-1982 data are available only in terms of the annual dose in MESH, but that is no longer accurate.

What is true now is we initially said that, but we discovered, I discovered a few sample pages that were captured by SC&A in September or October of 2008. Then, in 2009, I discovered those sample pages. That led me to re-request those boxes that SC&A had reviewed from DOE.

I went and we had a data capture. We opened up those boxes, and they're full of the tritium log books, the tritium bioassay data. So we captured them.

Now that happened fairly recently, the latter half of last year. So those have certainly been scanned and captured. Whether

or not they have been coded and are being routinely used in dose reconstruction yet, I can't say, but they certainly will be.

MS. ROBERTSON-DEMERS: Well, that is the bottom-line question, is whether they are being used in dose reconstruction.

DR. ULSH: Well, what I said was these data have been captured and will be available for tritium dose reconstruction. So, yes, we are committing to using that data as soon as we can get it in a form where it is routinely available.

And I would anticipate that this would probably result in a PER, where we would go back and look and make sure that either we redo the dose reconstructions as appropriate or determine that it's not necessary, just likes any other situation.

MS. ROBERTSON-DEMERS: The reason that we brought 12 and 13 up is because they have not been closed down.

DR. ULSH: I understand, but we

responded to those issues. It appears that those responses are not being registered.

I mean, if you want to say that our response is inadequate in some way, fine, we'll entertain that. But it's a little frustrating that it has not even been taken into account.

MS. ROBERTSON-DEMERS: I will repeat what I just said, okay, about all the different sources of internal dosimetry data. The problem is not that it doesn't exist, okay, because it does. The problem is making sure it is used in dose reconstruction. That, I'm telling you, is not an SEC issue, I agree with you.

DR. ULSH: Okay.

MS. ROBERTSON-DEMERS: Okay?

DR. ULSH: Do we need to keep discussing then?

MEMBER ZIEMER: Well, the data have been captured now, and the intent is to use it? That's what we're hearing.

DR. ULSH: Well, specifically, Paul, the tritium log books that contain the early tritium bioassay data --

MEMBER ZIEMER: Yes, right.

DR. ULSH: -- have now been captured.

MEMBER ZIEMER: Right.

DR. ULSH: And we are at some point in the process in terms of making that available for dose reconstruction, yes.

MEMBER ZIEMER: Right.

CHAIR BEACH: Is there anything else, then, on --

DR. ULSH: Buried records.

CHAIR BEACH: Buried records, yes.

DR. ULSH: If it is the judgment of the Working Group that the only way to resolve this is to go dig up the records, I would say it is pretty clear what the path forward is.

I didn't hear any new information in regard to this issue beyond what has

already been discussed extensively at, I believe it was, the last Working Group meeting.

I think I heard Bob say that MJW's review did not definitively show that there was unique bioassay data -- I might be getting some of this wrong, Bob -- in the buried records. But the only way to know for sure is to go dig up the records.

I can speak for NIOSH, I think, that we're not going to go dig up the records without compelling evidence that it contains unique bioassay data. Even if we did have it, I don't know that it would be feasible to do it. And even if it was done, I don't know what condition the records would be in to be used anyway.

I don't think the status on that issue has changed since it was discussed last time. I don't know what more we could provide.

MS. ROBERTSON-DEMERS: What we're

saying is we can't give you any direct evidence. We can't prove that what's down underground has that data. Okay? And the only way we could do it is to dig it up, so they will be scanned. They're not going to dig it up.

DR. ULSH: Well, then I would present to the Working Group, you have heard all the relevant information that NIOSH can provide. Correct me if I'm wrong, but I would say that you guys have probably done the same, and now it is in your hands. I don't know what else we could offer on that.

MEMBER CLAWSON: So it is proving a negative or --

DR. MAURO: This goes to the heart of every coworker model. We always have an incomplete database to reconstruct the dose for an individual, always. And usually there is a protocol. Every site has a protocol for dealing with how do you fill in information for missed dose and for workers who weren't

monitored but should have been.

Now this is very similar to the second case. There may very well be worker records that are not in a worker's file that are perhaps buried somewhere, whether it's bioassay or not. The question is, is the coworker model and the data on which it is based -- I presume we have a coworker model to fill in the blanks.

In other words, if you're reconstructing someone's dose in tritium, or external dose, you have a film badge or a bioassay record for that person. It is probably missing some information, and you are going to have to fill in the information. I assume you have a coworker model.

The question always becomes, whenever we deal with any SEC issue, do you have sufficient information to build a scientifically-sound claimant-favorable coworker model?

Now, when we were looking at the

records on which their coworker models are built that we find, usually the test we use for any site -- this goes to every site we look at -- we develop a little matrix. We say, okay, here's time and here's different job functions, and here are the different radionuclides as a function of time and job function that might be important to reconstruct a person's dose.

Can we, for each one of these little boxes, and we think about it like a Rubik's cube, every box, do we -- and I say, "we" -- have the wherewithal to reconstruct the person's dose who may have operated in that box at this time period doing this job? We know that he probably was exposed to certain radionuclides, but we don't have a complete bioassay record for him. Do we have a coworker model that can assign to him a dose with sufficient accuracy?

And this goes to the question of whether those records are lost or whether they

were never collected in the first place. So I think that really goes to the heart of the matter: Is the coworker model adequate? Notwithstanding the fact that there may be some records that either were lost -- it's almost like when we were talking NTS; there was a lot of badges that were left behind. There's no doubt about it. We interviewed enough people to say that.

But our research showed that, notwithstanding the fact that there were badges left behind, a coworker model could be built where you could assign, where we felt that the upper end of the distribution wasn't compromised by that process. As a result, a coworker model could be built.

What we have here is a similar situation. It sounds like it's clear that some records were buried.

DR. NETON: Well, we don't know that.

DR. MAURO: Oh, we don't know

that? Oh, okay. I didn't know that.

DR. ULSH: We do know, John is correct, we do know that some records were buried.

DR. MAURO: Okay. Now I'll take it --

MS. ROBERTSON-DEMERS: We have indirect evidence that there were RadCon records buried, but not direct.

DR. MAURO: Okay. I am just trying to look at it as, if I was doing the dose reconstruction, can I still do this person's dose reconstruction, notwithstanding the fact that there might be some records that are not there that were lost, buried, or never collected in the first place? If I can't do that, we've got an SEC issue because I can't reconstruct this person's dose with sufficient accuracy. I mean I guess that is the question on the table.

The fact that there may very well be buried records, I don't think that

necessarily means we have an SEC issue. Also, the fact that there might be some records that are buried that are bioassay records and, therefore, it is not a complete dataset, does not automatically mean you can't build a coworker model.

So I guess the question is, you know, do we have some question whether you can build a coworker model?

DR. NETON: Well, that is a different question. I mean, first, we start off talking about these records missing, and now you're saying that the coworker model should be robust --

DR. MAURO: Well, I mean, in the end, that's the only reason why it is important.

DR. NETON: Well, maybe we should talk about that. I haven't heard any criticism of the coworker model.

MS. ROBERTSON-DEMERS: Actually, there is a coworker model for polonium. There

is a coworker model for plutonium, period.

CHAIR BEACH: Paul?

MEMBER ZIEMER: The way I kind of look at this is, well, a couple of points. One is, if there were really a dearth of information, then gathering this additional, if there were additional, might be really critical. You have a really decent database here at this facility to start with.

One could even argue that there is some likelihood, if there were rad records there, they might be duplicates even. I mean that would be an argument. I don't have any real basis for that, but you could think about why would you bury some records and not others.

The other thing that I think is sort of practical, and we sort of have done this in other cases, is to ask a kind of cost-effectiveness issue. What do we gain by the extra cost of -- what is the program gain?

It's my, I don't know if it is a

feeling or just more of kind of the picture I get from what I hear from DOE as well as others who have looked at this, is that retrieving those records is not a trivial exercise. If it were, it would have been done.

I'm not even sure DOE would be willing, without really compelling evidence, be willing to go in and dig those up. It was my understanding that that could be an issue with DOE even.

So you would have to say, well, what's the cost/benefit of that? If it is going to cost -- I don't know, pick a number --

MEMBER CLAWSON: Well, the last one was at $5 million.

MEMBER ZIEMER: Yes. If you're going to cost that, is it worth that, particularly with the dataset we have? So there's kind of a practical issue, too.

I think, at the end of the day,

you would have to say, yes, there is this possibility. Is it such that it's a show-stopper in terms of, as you say, John, either a coworker model or just individual dose reconstructions?

So all you can say is, yes, that is a possibility. There may be more data out there that we don't have available.

MS. BRACKETT: This is Liz Brackett. Can I say something?

MR. KATZ: I'm sorry, do you want to repeat, Liz?

MS. BRACKETT: I just said my name, that's all. I wanted to throw something in.

MR. KATZ: Yes.

MS. BRACKETT: We discussed this, I believe, at the last meeting, and I don't have the documentation in front of me because I thought this was a closed issue. But I think it was in the MJW documentation. I mean, granted, we did not look at every single

box. We went looking specifically for bioassay data.

But, in our final report, it says that we did not find anything at Los Alamos that was not already at Mound in their microfiche. We verified that everything that had been sent out there that we looked at was already still present at Mound on site, and there was a discussion about the microfiche and what might have happened to that. I don't know, microfilm and microfiche.

DR. ULSH: Yes, you're right, Liz, that's exactly what I was referring to, the discussion at the last Working Group meeting. I don't believe that anything has changed since then, at least certainly not on our end. I haven't heard about anything that has changed on SC&A's end. I can't speak --

MEMBER CLAWSON: Liz, this is Brad. Let me ask you a question.

I remember this comment that was made out there. How much of it did you review

out there at Los Alamos?

MS. BRACKETT: Well, what I reviewed out there was a very small fraction of what got sent back. Unfortunately, I mean this was -- what -- 15 years ago? I don't remember the details.

MEMBER CLAWSON: I understand.

MS. BRACKETT: But on site, I didn't look at a lot. But, after going on site and finding polonium log books, we asked for something like 45 or 50 boxes to be sent back, I think. Those were looked at. There was a lot reviewed on site because it was all shipped back to the site.

MEMBER CLAWSON: And how many total boxes were buried?

MS. BRACKETT: I don't know that because I wasn't aware that they were buried until this whole thing started.

DR. ULSH: I think it's also important to put this into context. We are treating this like it is an unusual event. In

fact, it's not. DOE, and the government in general, have a records retention schedule. Records are destroyed in one way or another all the time. The requirements for keeping records depend on what kind of records they are.

Theoretically, at least, and everyone knows that no system is perfect, dosimetry records are supposed to be retained for -- I don't even know if there is a limit. It might be 75 years.

MR. HINNEFELD: I believe it is 70 years, but that can be different --

DR. ULSH: Seventy years.

So you have to look at the weight of the evidence here. I think we have assembled the weight of the evidence, and now you just have to decide what you think about it.

We don't have anything that suggests that unique bioassay data was included in this lot of records that was

buried. We do know some of the other types of records that were buried. I mean it is listed in there, financial records, some engineering records, those kinds of things. But that doesn't violate any records retention schedule.

I just don't know what else could be provided. You can always speculate. Actually, I agree with SC&A; the only way to know for sure is to go out and dig them up, and I don't think that's even going to do it because who knows what condition the records would be in?

But you have to ask yourself, given the weight of the evidence that we have available, do you see a dramatic deficiency that would compromise our ability to do dose reconstruction with sufficient accuracy? I don't see it, but it's up to the Working Group.

CHAIR BEACH: What does the Working Group think about the data

completeness issue? It's kind of a separate issue.

What I have heard is the first couple of issues you brought up are TBD issues.

MS. ROBERTSON-DEMERS: That is 12.

CHAIR BEACH: That's 12? Okay, I guess I have these under 13 then.

MS. ROBERTSON-DEMERS: You mean because it's multiple records?

CHAIR BEACH: Yes. Well, under data completeness issue, I have that all listed under 13.

MS. ROBERTSON-DEMERS: Okay, data completeness is broken into, actually, data completeness, and then the Los Alamos records is the 13.

CHAIR BEACH: Okay. I just don't know that we should continue this conversation on the buried records. Unless someone feels differently, I think we should close out this issue. Because unless we get something from

NIOSH that they are going to dig up those records, then we just keep going back and forth on this same issue.

MEMBER CLAWSON: Jim brought up something, and I guess I'm just going to voice my opinion of why it kind of concerns me somewhat.

I know that we can fill in the gaps, and so forth, but to what accuracy? That gets into one of the real big questions and stuff.

They have just found what they have called log books, and they were tritium log books. Also, in all these buried records, it indicates that there were log books. Now they're saying that they're engineering ones, and so forth like that.

We have heard from so many petitioners, and so forth like that, that there was bioassay information that got buried, and so forth. Be it what it is or whatever else like that, it's kind of --

CHAIR BEACH: So are you suggesting we ask them to --

MEMBER CLAWSON: No, I don't think that we can. I just want to voice my concern. When you get into data accuracy, or whatever, well, that's fine, I can take bits and pieces of it and make this model, but to what accuracy really is it? I have a problem with that. But I don't think we are going to be able to dig them up, either.

MS. ROBERTSON-DEMERS: Just for clarification, I'm not suggesting that we should go out and dig them up. I'm just suggesting that the only way we can give you direct evidence is to actually look at the records.

CHAIR BEACH: Right.

MEMBER CLAWSON: That's not going to happen. So I guess we can close it and go with what we've got and go from there.

MEMBER ZIEMER: Well, the other comment I think -- I don't know who made it;

maybe you did, John -- but just to emphasize that having 100 percent of the records at a site is not that common. It always comes down to, do you have enough ultimately to make the right decisions, whether it is an individual dose reconstruction or an SEC? Whatever that decision is, do you have the information you need to make that decision?

DR. MAURO: It's always the coworker, I mean when it is all said and done, it's always the coworker model that is in play.

MEMBER ZIEMER: Yes.

DR. MAURO: Always.

MEMBER ZIEMER: Yes.

DR. MAURO: And if you can't build a coworker model that is scientifically-robust and claimant-favorable, you've got an SEC.

MEMBER ZIEMER: Right.

DR. MAURO: If you have the data --

MEMBER ZIEMER: You either have an

SEC or the coworker model uncertainty gets bigger.

DR. MAURO: Yes. Oh, yes, but that's not a model, yes. To the point where, of course, then you go to the test of plausibility.

MEMBER ZIEMER: Yes, right.

DR. MAURO: I mean, in the end, this is the dilemma.

MEMBER ZIEMER: Right.

DR. MAURO: The horns of the dilemma we are always on, you know. And if you have very limited data, that puts you in a position to make an extremely claimant-favorable coworker model, which places you in a place of, wait a minute, are we walking into the territory called plausibility? It's the same story.

MEMBER ZIEMER: Yes.

DR. MAURO: And the only reason I brought it up this way was that I think that the fact that records are buried, it would be

great if they weren't. But my question is, you know, do you have a robust coworker model for the various exposures and everything else we're dealing with?

We talked about a lot of subjects here. It sounds like you have lots of data. But, I mean, I haven't looked at it. I don't know if it is even an issue.

Is the coworker model one of the issues that we're looking at on this SEC review?

CHAIR BEACH: Under 13?

DR. MAURO: Yes, on one of these items.

CHAIR BEACH: Not that I'm aware of.

Kathy, John just asked if coworker was part of 13. I don't believe so.

MS. ROBERTSON-DEMERS: No, there's a coworker model for polonium. There's a coworker model for plutonium. That's what's available, and then I will let Ron speak for

the external.

And the concern for these buried records was not merely internal dose, but all dose.

MEMBER ZIEMER: Yes, could it affect other things besides dosage here?

DR. BUCHANAN: As far as I recall, we closed the external coworker model at Mound, SEC issues.

DR. ULSH: To clarify, I think it was external data completeness. Right?

MR. FITZGERALD: Yes. I don't know the number. Whatever the number was, yes.

DR. ULSH: Yes, I don't know the number, either. Okay.

CHAIR BEACH: So I guess I need to ask the Work Group what your thought is, either to leave it open or to close it at this point? And I am suggesting that we close it.

MEMBER CLAWSON: Buried records?

CHAIR BEACH: Thirteen.

MEMBER CLAWSON: That is all we can do.

MEMBER SCHOFIELD: Say they are digging up the records in that area, and they're either digging everything up or we have done a lot of that, but the reality is you will have to assume all those records are now contaminated.

MS. ROBERTSON-DEMERS: They were from the beginning, yes.

MEMBER SCHOFIELD: Yes, but they are in worse shape now.

MS. ROBERTSON-DEMERS: Yes.

MEMBER SCHOFIELD: This is based on some of the workers who were out there working in that hot area, repacking and things. We will have to assume that they are just beyond reach forever.

CHAIR BEACH: Okay. So 18 and 19 was closed. It was adequacy, completeness of external dose records, and we closed it on May 27th.

MEMBER ZIEMER: Well, I agree we should close this issue. I think it is fine if the record shows that there's potential records there that couldn't be used. I mean you'll use what you have. In my mind, there are enough records to make an adequate, if you can get the coworker, to make it, and it could be somewhat modified, if you had some other data.

But, as you said, John, this is like others where you're going to work with what you have. If it's inadequate to bound doses, then you go in one direction. If you believe it is adequate to bound, you go in another direction.

So I think the possibility that Kathy raises is probably you have to say it is a real possibility, but we are probably not going to get those records, if they exist.

MS. ROBERTSON-DEMERS: I guess the question becomes, you've got a coworker for polonium; you've got one more for plutonium.

Do you need one for anything else?

MEMBER ZIEMER: Oh, well, that may be a separate question, I guess.

MS. ROBERTSON-DEMERS: And that's actually a new question.

MEMBER ZIEMER: Yes.

MEMBER CLAWSON: Well, that comes down to, do we have the right coworker models for Mound? I know at some sites we use a bounding one, but --

MEMBER ZIEMER: Well, I can't answer that. I mean you're addressing it to staff. Are there other --

DR. ULSH: No, Kathy is correct. The coworker models that are in place for Mound are polonium and plutonium because those were the primary radionuclides of interest at Mound.

The other one that you could maybe make a case for would be tritium. What I can tell you is that tritium was confined; the operations occurred in certain areas, access-

controlled areas. If you went into the tritium building, you were on tritium bioassay, with the exception that Kathy specified yesterday, you know, in the DOE era, the 54, whatever it is. You know, if it's less than 100 millirem, you don't have to be monitored.

But, prior to that, if you went to work in those buildings, you were on tritium bioassay. So it is our position that we don't need a coworker model.

The same with external, and I think we discussed this at one of the early Board meetings as well, our basis for concluding that. I think that went into the decision to close out the data adequacy/completeness issue for external.

CHAIR BEACH: Well, if you go back and you look at our matrix, we actually combined 12 and 13. Under 13, without reading it, I know you can go back and look at it yourself, that closes out the 453 boxes.

Basically, we would be leaving the top paragraph open, No. 12, and closing out 13.

MEMBER CLAWSON: What was No. 12?

CHAIR BEACH: It was the internal dosimetry data completeness. I don't know if you have --

MEMBER CLAWSON: I've probably got it, but it's in my file.

CHAIR BEACH: But, from the way we wrote it up, it doesn't affect anything but what we have just discussed on the buried records, basically.

MR. FITZGERALD: The two are combined because they are two different facets of the same issue.

CHAIR BEACH: Phil, what's your thoughts?

MEMBER SCHOFIELD: I think we can close this issue and, like I said, personally knowing Area G, those records are a dead issue. I mean there's just absolutely no way, regardless of what is in them, that we will

ever have access to them.

CHAIR BEACH: Okay. So I would say that we officially close 13, the external data completeness portion of the matrix. Do you need more words?

DR. ULSH: You said "external".

CHAIR BEACH: Yes.

DR. ULSH: Is that what you meant to say?

CHAIR BEACH: Oh, I'm sorry. Internal.

DR. ULSH: Okay.

CHAIR BEACH: Data completeness. Excuse me. Internal. Yes, that's correct.

DR. ULSH: Well, I don't know where you are headed, but are we going to discuss other aspects of this issue? I mean I think what you did was just close the buried records issue.

CHAIR BEACH: Yes.

DR. ULSH: But there's still other things --

CHAIR BEACH: Well, 12 and 13 were combined.

DR. ULSH: Yes.

CHAIR BEACH: So I don't want to mistake that we have closed 12. We have only closed 13.

DR. ULSH: I understand.

MR. FITZGERALD: I think he's asking for some clarification on it.

DR. ULSH: Well, I guess what I'm asking, Josie, is, are there further actions, not the buried records part, but the other part that's not closed yet, are you requesting any further actions from NIOSH?

CHAIR BEACH: I think what we need to do is go back and look at that separately and see if there's anything more, after looking at your White Paper, if some of the answers you gave are not complete or not satisfactory to SC&A.

MEMBER CLAWSON: That would be covering the coworker models or --

DR. ULSH: We haven't spent a lot of time on coworker models.

CHAIR BEACH: No, we haven't. No, that's separate from what --

DR. MAURO: In a way, for example, the conversation we had earlier on this gross alpha protocol, where in the early years they were collecting urine samples and then precipitating all the alpha emitters out, and I would say that you have, apparently, a lot of data in the gross alpha activity in urine without going to isotopic specific.

We talk about, in my little Rubik's cube picture, okay, so there's this time period where people were working with some suite of radionuclides that were transuranics or actinides. That is well-documented in the literature. Okay?

Then the question becomes, well, how are you going to reconstruct the doses to the workers who might have been working with that material at that time period? And the

answer, I guess, is, well, what we have is data on all these workers, and we have all the gross alpha activity.

Now it turns out, however, one of the issues that came out regarding adequacy of data, which is really the subject, is, well, it seems that there's some question whether all of those different forms of the actinides were, in fact, precipitated out at a 90 percent level or an 80, whatever the percent number is that you are going to pick.

In effect, you are going to ask yourself the question, am I in a position where I could assign a dose to these workers, making some assumptions on what the recovery was?

MEMBER ZIEMER: Don't we have a follow-up action?

DR. MAURO: Yes, we do, and we haven't found out -- no, I'm trying --

MEMBER ZIEMER: That's what we're waiting on for that? Is that it?

DR. ULSH: We do have a follow-up action on recoveries, yes.

DR. MAURO: I do have a place where I'm going with this. So what I am saying is we have gone down a very linear process to deal with that question.

But now, superimposed on that is, okay, good, you've got a way to adjust or to make use of this gross output data. However, do you have it for all the workers you need to have it for, and all the different buildings, and all the different time periods that are necessary?

Because, you know, for example, there may be some workers that worked in a given building in a given different time period where you don't have that sample. There may be a lot of workers. There may be categories of workers that did a certain job. This goes back to over and over again we're in the same position, which means that you have to build a coworker model.

And it's, oh, okay, we do have a bunch of workers that worked in this building -- I'm talking in principle -- in this building at this time period, where we don't have that data. Okay? We don't have that data.

If we don't have the data, that means you are going to have to assign, but you do believe there's a real possibility they may have inhaled some of this stuff. Well, that means you have to build a coworker model.

Now the test that we put that to is, okay, do you have enough data for that time period for that category of worker or at that building that you could build a distribution, you know, enough data to build a distribution that says, yes, the exposures look like this for the workers? It may be 700 or 1,000 measurements made for that time period in that building.

Well, in my mind, if each of those measurements have been appropriately developed

by taking into consideration recovery, you're in a position now where you could pick off and assign, you're in a position now to build the coworker model; that is, either to assign the full distribution or the upper 95th percentile to any given worker that, for some reason, was not monitored.

So, in a way, everything we are really talking about goes toward really the coworker model. I mean there are technical issues embedded, like recovery fractions, that certainly you have to deal with. But if you can't deal -- see, the problem that comes, if you can't deal with the recovery fraction properly, you can't build that coworker model.

MS. ROBERTSON-DEMERS: First of all, the reason we didn't do a Nevada-type data comparison is because MJW did a rather extensive QA on the polonium and plutonium data during the pre-1989 dose reconstruction process, and we accepted that.

DR. MAURO: Okay.

MS. ROBERTSON-DEMERS: Okay? For those two radionuclides.

It kind of goes back to what we were discussing earlier. We have identified gaps in the data based upon what's in the [identifying information redacted] document, but now we're being told that the [identifying information redacted] document is not the tell-all of things, and that we need to go back and provide further examples on where material was handled.

The comparison has already been done to the [identifying information redacted] document, but now the [identifying information redacted] document has gone away. So now that comparison has to be made to something else.

CHAIR BEACH: Okay. So, on 13, Kathy brought up originally in the internal completeness records several different issues. So what I would like to do is get back to you on exactly where we are at with the remaining issues, if that works.

DR. ULSH: Yes.

CHAIR BEACH: Okay.

DR. ULSH: And I would just add one small comment to what John said.

Think about other sites, and I'm only going to use this one because I can't think of another one, and I hate doing it.

MEMBER CLAWSON: Oh, no.

DR. ULSH: Thorium at Rocky Flats, I thought I would never speak those words again.

(Laughter.)

But the situation is that, for instance, at Rocky Flats, the primary radionuclides are plutonium and uranium. You don't build a coworker model for all of these little exotics because, No. 1, there wasn't a large exposure potential to a large group of people, and consequently, you don't have a large enough population of urinalysis results or other results to make a valid coworker model, nor do you really need one.

I would present to you, John, that at Mound the ones where you really need a coworker model are polonium and plutonium, and that's why we chose those two. You could go through the laundry list, like in the [identifying information redacted] document.

So I'll just pull out one off the top of my head, iron-59, I think. That is an exotic that is listed in [identifying information redacted]. I'm not saying that there was an exposure potential to that.

You wouldn't build a coworker model for that because we would say that, well, first of all, I think for that particular one, there is no exposure potential. But, okay, that's maybe not a good example.

Curium, a small, discrete situation. You wouldn't necessarily need a coworker model, the assumption being, of course, that if there was an exposure potential, they were monitored for it.

So I would say to you that a lot of these other radionuclides are in that category where they are not of a sufficient scale to warrant a coworker model.

DR. MAURO: I hear what you are saying. I understand that. There's the whole suite of radionuclides, and you say to yourself, well, in the end, we know certain radionuclides were present. Okay?

And what you're telling us is that, well, there's certain radionuclides that might have been present, but you feel, and maybe rightly so, that they were not handled in sufficient quantity and in a manner that could have contributed importantly to anyone's dose.

DR. ULSH: Absolutely correct.

DR. MAURO: Now I think the fact that the material was there, present at the site, and you have some knowledge of how much of it was used and under what conditions it was used, needs to be disclosed because that

becomes the basis for your not including them in your dose reconstruction.

I mean I would think, if I were doing the dose reconstruction, and I was held accountable for that, and I knew that this worker worked in this building at this time, and I knew that there were certain radionuclides there, I would take it upon myself to say, okay, I have to convince myself that by not including the actinium or this isotope or that isotope, I did not underestimate this dose because of the way in which that material, the quantity and the way in which the material was handled.

And I could look in the mirror and say, you know, I feel good, and I could tell this person whose dose reconstruction I just did I think that we did the right thing by them.

And that's the judgment you are making right now. I don't know the extent to which all of that has been documented, of

their rationale, but not including a coworker model or a database for many of these, we'll call, exotic radionuclides, the ones that you, for example, you pointed out there's a time period when actinium was there, but there is no bioassay data for it.

And the argument that is being made is, well, you know, based on everything you could tell, there is no reason why anyone would have been exposed to any significant extent during that time period.

Now, interesting, who has the burden of proof?

DR. NETON: Well, I guess look at the question, John. I think the question is, there was no activities and there was no monitoring is a different question than there were activities, and we're saying monitoring wasn't required.

DR. MAURO: Well, I would say --

DR. NETON: That's different. That's a different question.

DR. MAURO: No, I'm just thinking, as a health physicist thinking about the problem, I think in the case of the actinium, because there was comprehensive attention, detailed attention, to it during this time period here, and then nothing over here, and then, again, in this time period, I guess I would say -- I'm always afraid to go down these roads because I'm not a Board member. I'm just one of the guys sitting around the table.

But it sounds to me that they had the wherewithal to make prudent judgments, and they made some prudent judgments in the back end of the process to monitor for actinium when they were digging that stuff up, because they knew there was the potential.

So I have to say, just me asking myself the question, I think they probably did the right thing by actinium because there's every reason to believe they understood what they were dealing with and they knew what to

do, when to do it.

Now is that convincing enough for you or for you? I don't know.

The same thing goes for the other radionuclides. Now, apparently, there's a long laundry list of radionuclides. Each one, somehow, a person has to come to grips with themselves as a health physicist and say, listen, am I doing the right thing by the workers by not including some contribution from this source?

And if you say no, in the end, unfortunately, in the end, implementing all these guidelines becomes ultimately a degree of subjectivity that we collectively have to think was designed here.

Now I don't know. There's these other radionuclides we're talking about. So I don't know. I think you mentioned a number of them.

You know, what are some of the radionuclides that you do? It might have been

important. The cobalt is good. Those are good. The cobalt, cesium, and strontium, now, apparently, they were there. Okay?

Right now, there are time periods when we know that they were there, but you're not reconstructing the doses to some workers from those in those years. Now there's got to be a reason why you feel it's okay not to include that, and I think that has to be articulated, your rationale for not explicitly addressing those.

DR. NETON: I think this goes to the action item that SC&A picked up in the morning session, I think, which is to identify those activities where we don't feel there are any there. You're going to put together a list that says, hey, but this stuff was here, and they were doing something with it. Prove to us why bioassay was not necessarily required for these activities.

DR. MAURO: Is that something you would like us to do or them to do?

CHAIR BEACH: No, we're okay. I think we already solved that.

Okay. So we're finished with that, with issue 13. It's closed.

I would like to go ahead and move on to the shallow dose issue briefly before break.

I know Ron does have a --

MR. FITZGERALD: Ron has a flight to catch.

CHAIR BEACH: -- flight to catch.

MR. FITZGERALD: We're moving that one up.

CHAIR BEACH: Which I mentioned right after lunch.

So, Ron, if there's no objection, you're on.

DR. BUCHANAN: Okay. The shallow dose issue, of course, stems from the fact that there was some low-energy photons and beta exposure at Mound. In the beginning, there was some, but not a lot. Off and on,

there was beta and low-energy photon exposure potential at Mound.

They did some badging. They didn't always read the badges, and they didn't become DOELAP-accredited until 1991. So SC&A raised concerns a year or two ago that the data wasn't there to assign dose during dose reconstruction.

So NIOSH came out with a review of Mound site shallow dose prior to 1991, issued a White Paper in March of 2009.

SC&A reviewed this White Paper and presented their results, I believe, at the May 28th Working Group meeting here.

What NIOSH proposed in Table 4 of the March 2009 White Paper was to do some adjustments based on some correction factors, mainly a ratio of the gamma ray to calculate the shallow dose at certain times in certain operations, because it was a facility widespread problem. It was certain operations at certain times in certain locations.

So SC&A reviewed that, then, their White Paper, and pretty much agreed with the concept they presented in Table 4 of their March 2009 White Paper, except we felt that the period 1979 through accreditation in 1991 was left out and should be addressed, because in the original White Paper, they said that this would be used as stated in the dose of record.

So NIOSH responded in September of 2009 with another White Paper which had some modification. In Table 1 of that White Paper, they do make adjustments up through June of 1991, when Mound became accredited for beta and low-energy photon dosimetry.

So SC&A reviewed that. We did not go into all the correction factors and how they were derived, and NIOSH did not state numerical values for all of them.

However, from a concept point of view, SC&A finds that this is not an SEC issue, that if there are items or issues, it

would be with a Site Profile, you know, the No. 1.2 or 1.3, or something like that, as opposed to not being able to reconstruct adequate shallow dose.

So, at this time, SC&A recommends that, if these conditions in the revised White Paper of September of '09 are implemented, that we do not have an SEC issue with this.

CHAIR BEACH: Any comments back?

DR. BUCHANAN: That's issue 16 --

CHAIR BEACH: Pretty easy on that one.

So the last thing that the Work Group asked was that NIOSH comment to SC&A's April White Paper, which they have done, and we are hearing the report now from Ron that SC&A is satisfied with the answers from NIOSH.

So I guess I would ask the Working Group if you're ready to close this item, based on SC&A's response and NIOSH's?

MEMBER CLAWSON: Yes.

MEMBER ZIEMER: Yes.

CHAIR BEACH: Everybody's in agreement? Okay. So, then, I would suggest that this item be considered closed. So one more.

Are you ready for the break?

MEMBER ZIEMER: I'm glad you stayed for that, Ron.

CHAIR BEACH: Yes, thank you. That was easy.

DR. ULSH: Josie, after our break, what's next? Is it plutonium-238?

CHAIR BEACH: Yes.

DR. ULSH: Liz, what's your schedule?

CHAIR BEACH: We can do it now, also.

MS. BRACKETT: I have another conference call at 3:00, but I can skip that, if needed. I don't think Tom is on because I think he's probably traveling by now.

DR. ULSH: If you can skip it, that would be great.

MS. BRACKETT: Okay.

DR. ULSH: Thanks.

CHAIR BEACH: So would you prefer to not take a break and just go right into it?

DR. ULSH: No. No, no.

CHAIR BEACH: No, no. You want a break. Okay.

DR. ULSH: Yes.

CHAIR BEACH: Let's do that then. So 10 minutes?

MR. KATZ: Okay, until 35 after.

CHAIR BEACH: So 2:35, yes.

(Whereupon, the above-entitled matter went off the record at 2:24 p.m. and resumed at 2:39 p.m.)

MR. KATZ: Okay, this is the Mound Work Group, and we are just getting started again after a brief comfort break.

We are on to perhaps our last --

CHAIR BEACH: Oh, no.

MR. KATZ: No?

Nice try, though. Nearly our last

agenda item.

CHAIR BEACH: Yes. Okay, so we are going to get started with high-fired Pu-238.

Brant, are you ready?

DR. ULSH: Oh, no. I'm trying to pick up the thread about where we left this issue.

This is another issue, just like all the rest, where we have had a number of iterations here.

CHAIR BEACH: Yes.

DR. ULSH: I think the last significant event was our issue of our response, and that came out in September of 2009. That document was two parts. Now it's coming back to me.

Our document was meant to respond to two of SC&A's documents on this issue. One I think was the White Paper, and we went through in the normal point-by-point format for that.

I think the second one was the additional material that was sent over related to, I think prepared by Rich Leggett --

MR. FITZGERALD: And Joyce.

DR. ULSH: -- and Joyce.

So our responses I think are on the table there. I guess, rather than walk through the 20, or whatever, issues, 22 issues, I guess I would just like to say, you know, what is it you guys, what's still hanging out there that you want to discuss on this?

MR. FITZGERALD: I am going to summarize this.

Joyce, are you still on? Joyce Lipsztein?

(No response.)

DR. ULSH: Well, I guess I should ask, is Liz Brackett there still?

MS. BRACKETT: Yes, I'm here.

DR. ULSH: Okay.

MR. FITZGERALD: Okay. Well, let

me try to summarize it.

I thought the last exchange was pretty productive. I think we did start out with a number of issues. I sense and I observed we converged to the point where it's really the assignment of what we're calling a type J dissolution model versus what I think NIOSH has coined as type L. I'm losing track of these letters, but I think that is where we left it.

You know, certainly one comment has been -- well, if we're talking about which version of the dissolution model should be applied, is that an SEC issue? I think what we are looking at at this point is which one would be bounding of the phenomena that may have existed or that would have existed at Mound during the handling of Pu-238 at the site.

I want to just tick off the -- I wouldn't call them arguments, but the comments that we have that would support the type J

model that was based on an event at Los Alamos that we think has pertinence for Mound.

First off, even though it was based on a Los Alamos event, we think there is a high likelihood that the pellets involved did come from Mound. Mound produced most of those cermet pellets back then.

So let me just go through that and just try to cut to the quick, because I think we have made a lot of progress. We have converged this thing down to what would be the most appropriate bounding model.

Unless Joyce joins us -- Joyce, are you there?

DR. LIPSZTEIN: Yes, I'm here.

MR. FITZGERALD: Okay. We can talk about the model after I just kind of outline why we think the type J is a better fit.

DR. LIPSZTEIN: Okay. We don't know what is the best model, and we don't know exactly what kinds of, now let's call it, non-

monotonic behavior of plutonium to circulate, which one is the best fit for Mound.

The only thing we know is that the type J that was at Los Alamos, observed at Los Alamos, is the most claimant-favorable model. We know that some of the plutonium-238 heat sources, the molybdenum cermet disc, most of them that were handled at Los Alamos came from Mound. So this makes us think that probably Mound workers could be exposed to type J also.

The other problem is that at Mound people, workers, could be exposed to various -- there were various techniques that were used at Mound for the production of heat sources, not just one technique.

So we think that the most restrictive model should be used for Mound. For example, there was the development of a model by NIOSH which was type L, and then we found some data that were more restrictive than type L, and things like that.

So I think that the best thing

would be to go to the most restrictive one that we can find and that is possible to be found at the Mound, which we think is the model that was found in the accident at Los Alamos National Laboratory.

DR. NETON: If I can jump in real quickly, I'm recalling this conversation now that Tom Lebone was involved at that time. I believe that Tom's -- the type L model was based on Mound-specific data.

DR. LIPSZTEIN: Yes.

DR. NETON: Some folks that Tom saw that clearly showed evidence of an incident, and he could model.

The J values from Los Alamos, if I remember correctly, Tom made some comments to the effect, and I think it's actually an appendix to one of the White Papers, that the generation of that type J material was the result of some very unique set of circumstances that caused that to happen.

We were at that time unconvinced

that that scenario actually happened at Mound.

DR. ULSH: I can give more detail, if you would like.

DR. NETON: Okay, yes, go ahead, Brant.

DR. ULSH: Joyce, I think that we would probably agree that there's at least a reasonable likelihood -- in fact, it is probably probable -- that that material involved in the incident at Los Alamos originated, was prepared, originally manufactured at Mound. I don't think that we would say otherwise, barring evidence to the contrary.

But Tom's point was that it's not just the identity of the material, but the particular details of what happened at Los Alamos that contributed to the generation of this material.

First of all, let me see if I can recall the details here. And, Liz, you jump in and correct me where I go off the rails.

I think it has got to be relatively fresh material because the problem with, well, the issue with plutonium-238 is that it has got a high specific activity. So it breaks up the matrix in relatively short order. That is why it is different from, say, for instance, Super S 239, plutonium-239.

What happened at Los Alamos was they were cutting apart a heat source, I believe, immediately after or very shortly after they did extensive vibration testing. So it generated a respirable aerosol.

I think what happened was they overpressured a glove. They overpressured the chamber in which they were cutting on this heat source and it blew this material. Once they cut into it, the freshly-generated aerosol, it blew out into the room and exposed some people.

Now, at Mound, what you had was, I think this was what was called the microsphere program, maybe not officially. But they would

drop plutonium-238 through a plasma torch and make microspheres.

Now the microspheres themselves are not respirable. I mean, from a respiratory standpoint, they look like boulders.

So that's not an issue at Mound. It is only the unique exposure conditions, I think, that Tom documented, and he actually interviewed some of the people that were investigated at the Los Alamos incident. I'm fuzzy on the details.

So it was our contention that, yes, the material probably did come from Mound, but it was the unique conditions that occurred during this incident that led to the formation of this type J material.

Liz, have I captured it accurately?

MS. BRACKETT: Yes, that sounds right, that they had done some vibration testing for something like 40 days. It was

some very long test.

Unfortunately, I had hoped that Tom could be here because he's the one who has worked on all this. He was available all day yesterday until about noon today, but we just missed him.

DR. LIPSZTEIN: Let me put it in another way, then. What we all think together, I think NIOSH and SC&A, they all believe that there might be some kind of model that would be a bounded model for this non-monotonic material, right? Okay.

So there was the development of a model by NIOSH that would describe very well the accident that happened at Mound, one accident that happened at Mound. Then we found another accident at Mound that was not bounded by that specific model. On the contrary, the other model was more, let's say, more restrictive than the one that was developed by NIOSH.

We had several discussions about

all the urine data and all the graphs that you have provided us. I remember specifically Jim saying you don't expect us to go through all those thousands of graphs and try to develop a model for anyone that looks like -- that had a pattern that would come up and down. And it's true, you cannot do that.

So I think we have to have a model that is bounding. So, if you have a model that is bounding, and we don't want to analyze all the data that exists for Mound and make all the scenarios that could have happened. So that those graphs were developed, we have to have the most bounding model, the one that is more restrictive that delivers the higher dose to lung and to systemic tissues, right?

So, because type J really happened in an installation, and because people could have been exposed to type J at Mound, because there was this material there, and because there were other processes in which workers of plutonium-238 were involved and handled, then

we think that the most bounding as possible model has to be applied to be claimant-favorable and not to err. If we err, we err on the side of the claimant.

So that's why we think type J would be a better model for Mound and for all plutonium-238 non-monotonic exposures.

DR. ULSH: Okay. Jim and I are having a little sidebar conversation here, and it is triggered by what you are saying, Joyce.

I'll just put an idea on the table for discussion. We have reasons for preferring type L because that was developed on Mound-specific data. But what if we committed to, as we do dose reconstructions at Mound, if we come across one or two, or however many, that don't appear to fit type L, then we would certainly entertain the possibility of using type J or whatever model is appropriate for that particular dose reconstruction.

DR. LIPSZTEIN: Yes, we gave you

an example of two people that we knew that were exposed in an accident at Mound and whose type L didn't fit because you had another fix. So that's why we don't know.

So, if we don't know, we have to apply the one that gives the highest dose, which would be type J. And it's not implausible because, as you say, the molybdenum cermet discs came from Mound.

DR. NETON: Let's back up a little bit, though.

I think where we have a sufficient number of bioassay points, we would probably fit the model ourselves. I mean there would be no reason to -- you know, we wouldn't blindly default to a type L model if the bioassay data itself, themselves, would not appropriately fit that model.

So I think really what we are talking about here is what our default would be in the absence of sufficient bioassay data. Isn't that more correct, Liz? I think that

is what we are talking about here.

DR. LIPSZTEIN: Yes, it's not treated well to develop a model for each exposure for each worker. You don't do this for the others, for example, type S. Type S plutonium, it's not everybody behaves like type S plutonium. Actually, you have particular parameters for the lung that are not exactly type S, but, yes, you apply type S. Type M also, it's not particular for that worker. For each worker, the lung parameter will behave differently.

For me, it doesn't make sense to develop a model for each worker that could be exposed to plutonium-238 with special non-monotonic behavior.

DR. NETON: I'm not saying for every worker, because the reality of it is that we probably wouldn't have many workers. You know, barring these incidents where you found type L materials, we wouldn't have that. So we would have to come up with some sort of

default value.

What I'm hearing you say, though, is that there is a possibility of a model out there, whether it's J, which you believe is the most bounding, or whether it is L, that could be used to reconstruct dose for this type of plutonium-238.

DR. LIPSZTEIN: Yes, all the time, we have agreed on that.

DR. NETON: Well, I'm not sure I understood that as well.

So I'm not sure where to go with it, other than, as a practical matter, I don't know that it makes that much difference whether it's L or -- J is the other model? Yes.

CHAIR BEACH: It might have for those two.

DR. LIPSZTEIN: Yes. Yes.

DR. NETON: Well, I mean --

DR. LIPSZTEIN: It makes sense for the lung.

DR. NETON: The lung doses are going to be sufficiently large --

DR. LIPSZTEIN: Yes.

DR. NETON: -- under I think either scenario --

DR. LIPSZTEIN: Yes.

DR. NETON: -- that they are going to be well over.

DR. LIPSZTEIN: Yes.

DR. NETON: That really shouldn't be a consideration, though. The reality of what's there should be the consideration.

I'm reluctant at this point to make a decision for our program that type J is the appropriate model. But I think, now that I understand that you do believe that type J would be appropriate, and it wouldn't be implausibly high, I think we need to take that back and consider our options as to where to go.

I think, under previous discussions, it was my belief that SC&A felt

that, even if we had a model, it was sort of in the same camp as these tritides, insoluble tritides, that applying it would be implausibly high because we couldn't identify who to apply the exposures to.

But if SC&A is of the opinion that we could use it, and it wouldn't be implausibly high, we can take that back and think about it.

MR. FITZGERALD: You know, I think the consideration of to what extent the physical handling of the circumstances of the event at Los Alamos, I think is somewhat speculative as to know, yes, it might have or it might not. I think that is kind of where we are coming from, that the claimant-favorable assumed that this could have been reflective of --

DR. NETON: I'm encouraged. I think we're closer than ever on this. I just don't want to make sort of an ad hoc decision on my own here, and we need to take it back to

our group to talk about how we might want to land on this.

MR. FITZGERALD: Yes, I think the last exchange was, again, I said at the beginning, was pretty fruitful. I think this is where we are at.

DR. ULSH: Can we agree -- I don't remember when they started the advent of the microsphere program. I don't know, whatever the date is.

MR. FITZGERALD: Whatever the date is.

DR. ULSH: I mean this is a high-fired process. So the types of operations that you might have encountered in the very early days of the SM Building, for instance, when they were doing plutonium nitrate or plutonium oxide, before they started doing the microsphere project, it doesn't seem to me that there would be a basis for concluding that there would be a potential for highly-insoluble plutonium-238 because it is a high-

fired process. If you aren't doing high-firing processes -- can we agree on that?

MR. FITZGERALD: Yes. I mean, certainly, you're going to have to have temperatures that would be high enough.

Bob, you would know what this is approximately. I mean you would have to have the temperatures.

MEMBER SCHOFIELD: Are we really sure of that? Because the reason why I say that, I've seen how it's all done. I've been involved.

It goes through two stages, typically. And, of course, I'm having to base this on what I know about the Los Alamos process, where it would definitely be high-fired.

MR. FITZGERALD: Now we acknowledge the RTG. I think we're talking about operations that were non-RTG operations that may have been low-temperature operations.

MEMBER SCHOFIELD: They were low

temperature? Because, actually, I know --

DR. BISTLINE: What do you consider low temperature?

MR. FITZGERALD: Well, that is kind of what Brant's asking.

DR. BISTLINE: Because anything above about 800 degrees, it gets to the high-fired, and you end up with some high-fired, not all high-fired. But, once you get up around a little over 1,000 degrees, then almost all the plutonium is going to be high-fired plutonium.

DR. ULSH: So somewhere in the hundreds of degrees centigrade is what we're talking about, right?

DR. BISTLINE: Okay. Because around 750 to 800 degrees, you're going to have quite a bit of high-fired out of that temperature.

DR. ULSH: Well, I would have to go back and look at what actually happened prior to the microsphere program because, of

course, the inherent assumption in what I'm saying is that there weren't processes prior to that that would have led to those kinds of temperatures, and I need to verify that. I can't say that's the --

MR. FITZGERALD: Yes, there's been some fabrication processes that --

DR. BISTLINE: Because even at Rocky, we had, you know, just the regular production process; we found out that some of those did have temperatures that got up there approaching the 800 degrees centigrade temperature, just in the plutonium process, in the normal production.

DR. ULSH: And you had a couple of little fires there, too.

DR. BISTLINE: Pardon?

DR. ULSH: You had a couple of little fires there, too.

DR. BISTLINE: Yes, 1600 degrees.

MR. FITZGERALD: There was a lot of high-fired --

DR. BISTLINE: But we found, just in natural production, that you did have temperatures, say, in the normal production, routine production, we had temperatures that were --

DR. ULSH: And if that were the case at Mound, then my suggestion would not have -- I would just have to look at it.

DR. NETON: We will look at it. We are going to take this back and look at the issue.

CHAIR BEACH: Any other questions from the Work Group before we move to the next topic?

MR. FITZGERALD: Joyce, do you have anything else?

DR. LIPSZTEIN: No, no.

MR. FITZGERALD: Okay.

DR. LIPSZTEIN: Then, once we accept the model, then we have to know what to do with the coworker model. Because you have a non-monotonic. So I don't know how to

build, but that's some other problem, not just an SEC issue. That is something that can be solved once we are beyond the model.

CHAIR BEACH: Thank you.

Okay, so the next topic we are going to get into, and this is going to be headed off by SC&A, is the Road Map, and it may be a little quicker than what we thought, based on some of the new information on the Road Map.

So, Kathy, are you heading that? Or, Bob?

MS. ROBERTSON-DEMERS: Well, I can just give you some of the items that were raised when we looked at the completeness of the Road Map.

We contended that you could add these if you did a subsequent revision. So some of these issues in themselves are not SEC issues.

MR. FITZGERALD: I don't know. We could provide these as comments, specific

comments, rather than trying to correct, you know, correcting the 140-page Road Map. I mean we could certainly provide those comments.

I think what we were getting to, and I think it was referred to as a tool anyway, or as a way to demonstrate what the [identifying information redacted] report has in it to the Work Group, we didn't see really an SC&A question, other than the questions of adequacy, which we have been talking about.

MS. ROBERTSON-DEMERS: Well, it depends upon what the purpose of the Road Map is. I mean, there are some gaps.

MR. FITZGERALD: Do you want to talk about the corrections or gaps in the Road Map, even though it may not have SEC implications?

CHAIR BEACH: No. I think we need to step back on that, based on the new information --

MR. FITZGERALD: Yes.

CHAIR BEACH: -- and come back with that.

MR. FITZGERALD: I mean that might be part of what we're coming in --

CHAIR BEACH: Yes.

MR. FITZGERALD: But that gets to the heart of the issue, which is bioassay, rather than -- you know, to me, it is not quite a Site Profile issue, but how complete is the Road Map? Well, we could work at that, but I'm not sure that gets us where we want to go.

CHAIR BEACH: No. I think we need to wait.

I think, Brad, you had something?

MEMBER CLAWSON: Well, personally, I was under the misinterpretation of what the Road Map was for. Now we have come back, and that it was more of a D&D guidance in the later years.

I have been trying to find it on there because I remember reading in there, in

the [identifying information redacted] document, that these isotopes were there or considered there in the ceilings and floors, and so forth like that. And now I am being told it was just part of production.

So I think we need to step back and look at how we are really going to use that and what the adequacy was there.

CHAIR BEACH: Well, and the implications it has for previous issues on internal data adequacy.

MEMBER CLAWSON: Well, then the other thing that came up, and you know we did this on a side conversation I believe, but when we talk about a process ran from -- and I'm just throwing out numbers -- 1949 to 1959, then to come to find out that it was only actually for two years in that period, I think that we need to refer to that differently because the actual process was only two years, but then we had it stored elsewhere, and so forth like this.

And we have found other areas where the process, while we're sitting there with strontium or cesium, something like that, had been in these tanks for so long, and all of a sudden, we had a leak. That process ended. It had been there for five to ten years.

DR. ULSH: Well, it wasn't clear that that was the genesis of the material. It's possible, but we haven't --

MEMBER ZIEMER: Well, uncertainty about what the end-point means in the document --

MEMBER CLAWSON: Right.

MEMBER ZIEMER: -- is it the end of the official project or is it the end of when the material is actually sort of available in that facility? I think you showed somewhere that, basically, the end was when everything was concreted up. So I think there's some question. It may be clear, but I guess there were questions at least in

people's minds as to what that end-point means in the document. Or is it the same in every case?

DR. ULSH: I don't know. I don't know if the end-point means the end of active operations or if it means final D&D.

Do you recall, Mel?

MR. CHEW: No, I don't.

MEMBER ZIEMER: If it is consistent throughout, and we know what it means, that would be important.

MR. CHEW: But most of the time, when the specific operation was mentioned, it did give, like in that particular room the material was present, so between 1951 and 1954.

So I think, in essence, it really is both. Okay?

MEMBER ZIEMER: Yes. Well, I think we have all experienced that, where a project ends and you can identify that either by funding or by some other document, but, in

practicality, somebody later has to do something with some stuff, either get it out of there or drum it up or decon something. So it would help to clarify that in some way.

MR. CHEW: But the thing when they were compiling the document, they were trying to gather the best information as they possibly can. If, for instance, there was some specificity to tell you that this particular process occurred in that particular room, they put it in.

MEMBER ZIEMER: Kept it in.

MR. CHEW: If it wasn't sure, they put it to whatever the time was.

MEMBER ZIEMER: Right.

MR. CHEW: So recognizing what the genesis of the document itself is and what its intended purpose is.

To answer your question, it does kind of help you, that if you came back and D&Ded, this material potentially may be up in the ceiling and the wall, and that's --

MEMBER ZIEMER: Regardless of the time frame.

MR. CHEW: Right.

MEMBER CLAWSON: Right. And that's a very good point, but what I was considering that is that this document showed that we had this here, from here to here, and that we should have been monitored for that portion of it.

MR. CHEW: But you've got to remember why we put the thing together in the first place, because Mound, being a significant long period of time, had many, many different campaigns of different materials. It was good to try to put a Road Map together, so we can at least say, yes, we know these particular operations happened at this period of time with these kinds of materials, to the best of how the document was put together.

MEMBER CLAWSON: Well, and I think we also need to look at why this document was

put together. Because, according to some of the interviews that we have had, they started going into the D&D process of this, and it's like the building, and there was no residue, and then they would go rip out a piece of equipment, and they would reveal product and different sources that have been there for 40 years and, all of a sudden, people had uptakes.

And they were trying to figure out what was going on, and come to find out it's because they didn't have a record of what had been where. That was my understanding kind of a little bit of why the [identifying information redacted] document was there.

But I was under the impression that, from here to here, you had a potential for exposure to it. I think we need to kind of look at that.

DR. BISTLINE: But, again, I think there is a question as to how it is going to be an important question, how useful that Road

Map is going to be.

MEMBER CLAWSON: Right.

DR. BISTLINE: Because if a dose reconstruction person goes to that, thinking, you know, I'm going to use this as a trail for this, there are things missing. There's a lot of information that is --

MEMBER CLAWSON: Right.

DR. BISTLINE: For instance, the big explosion is not listed as one of the incidents that occurred. And you've got references in there back to the [identifying information redacted] document. Well, how many of the dose reconstruction people have Q-clearances to be able to go back and look at this document?

MR. CHEW: But they wouldn't do that.

MEMBER ZIEMER: They don't use the Road Map, do they?

DR. BISTLINE: Well, is it going to be used for that?

DR. ULSH: Then, also, it is not the entire [identifying information redacted] document that is classified. It is only one particular appendix.

DR. BISTLINE: Right. Right.

MS. ROBERTSON-DEMERS: Can I read something that might clarify something from the [identifying information redacted] document?

CHAIR BEACH: Yes.

MS. ROBERTSON-DEMERS: Okay. I found a quote in there, all dates represent the duration of actual usage of radioisotopes in their respective projects. It is clearly understood that residual amounts of all radioisotopes referred to in each room may still be found in floors, walls, and ceilings, and should be considered up to the present in every case for decontamination work.

DR. ULSH: So what that tells you, and I think that's consistent with what we're saying the purpose of the [Identifying

information redacted] document was, if I'm a D&D manager and I'm going in to decontaminate a particular, you know, D&D a particular room, what should I be looking for? What is the universe of potential things? Not that it is verified that it is there, but that at some point it might have been. So we had better be monitoring for it. I think that's consistent.

MEMBER CLAWSON: Yes, I just --

MR. CHEW: And monitoring may not be necessarily qualified as, not necessarily looking for that specific isotope, but doing what we consider the Rad Control, smears, swipes, sampling, to see if there is any indication of any activity of any type. When you produce the positive samples, that is when further analysis would be warranted. I think that's where it is.

So it is not in trying to address every isotope that was mentioned in that particular room. That was not the intent.

But I think you know that, Kathy.

You've been an operating person. The screening methods --

MS. ROBERTSON-DEMERS: I think that is insight into [identifying information redacted]'s mind.

MEMBER CLAWSON: And that was what I was reading, and you get to the very last statement of it, you know, where it says that it should be considered. I was taking it that that progeny can be there, no matter what. Even though it's in the ceilings, or whatever, it could still be there.

And a lot of times, in RadCon's eyes, they monitor for certain things and go through that process. That was the way I was looking at the document, and that's not how it really can be used, I take it.

DR. ULSH: No, but, Brad, I think that the attitude that you just described would be exactly the appropriate attitude for a D&D manager in that situation.

MEMBER CLAWSON: Yes, and there is

no question, D&D, but what I was looking at is D&Ds here in the 1990s, or whatever else like that. But what I was reading into this, and this could have been my personal thing, is that they should be considered to be there until this time. You know, that there was stuff there, because there are so many times before this document was ever even done, especially at Mound, we hear of it all the time, them going through the rafters and everything else like this. Well, that's not part of the D&D era, but those radionuclides could still be there.

This is inherent to Mound because they built one facility on top of the other. They were a cobbled-up mess, and I've heard this many times in the workers discussing this.

One of the biggest ones is electricians dragging stuff through all this, because they would wire it for something like this, and then they would have to change this

and drag different pipes through, drilling through concrete and everything else like that. This was well before the [identifying information redacted] document was brought up.

That is kind of what I'm looking at, why I feel the way I do.

CHAIR BEACH: Paul, did you have anything?

MEMBER ZIEMER: No. I think what Kathy read helped define in my mind a little better that those dates, beyond those dates, the residual which implies that the bulk of the stuff has been removed, but that doesn't mean there isn't some activity behind.

MEMBER CLAWSON: Right.

MEMBER ZIEMER: If that's true in every case, I think you've answered my question. At least the intent of that is that the end dates when we have taken the known stuff out of the room, there still might be some unknown residual behind.

MR. CHEW: If you look at the

document, there are specific places and specific processes are mentioned. Then those places you will find specific dates when you would think that that's the campaign time. I think that answers your question.

MEMBER ZIEMER: Right. Yes.

MEMBER CLAWSON: All right, and then there is also ones out to the side of it, that basically 10 years later they re-drummed this, and so forth. And those pop up several places in there.

CHAIR BEACH: Okay.

MEMBER ZIEMER: So what's going to happen on this? You guys are going to indicate the gaps?

MR. FITZGERALD: Yes, we can provide those as information to NIOSH, but I don't think -- again, it's in the SEC context, as much as this exercise that we were talking about this morning.

CHAIR BEACH: I was just realizing that we have had the Road Map on our agenda at

every Work Group meeting, but we have never gotten to it. So it was always at the end. Well, at least the very last meeting, it was actually the first time we were going to discuss it, and it was at the end. So I wonder if that's why we didn't put more emphasis on it and never actually got to discuss it.

So, with that, we've got two remaining topics. They should be fairly quick.

The PAAA violations, Issue 21, where we were, we were close to closure at the last meeting. We asked NIOSH to answer three questions from SC&A's April 2nd document of 2009. I believe that's been done.

I know that part of the issue, and I'm going to let Kathy speak to this, we have deferred some of PAAA to 11, the data adequacy.

So, with that, I believe --

MS. ROBERTSON-DEMERS: Well, I'm

not sure that those questions have been answered.

CHAIR BEACH: Well, no, I'm not saying they have been answered. They have just been referred to that.

MS. ROBERTSON-DEMERS: As far as the bulk of the PAAA issues, we have already come to resolution on that, but there were a couple of PAAA issues that were relevant to data adequacy. And it really kind of overlapped with some of the other data adequacy issues. So we moved them over under data adequacy.

Our concerns were that there were examples of recurrence where inadequate frequencies or failure to effectively implement the collection of bioassay samples occurred.

Just to get a little bit more specific, there were situations where individuals entered under an RWP, required that they submit a bioassay sample. In these

cases, the individual did not submit a bioassay sample after their entry, and that was the end of it. There was no bioassay sample after that entry. And the question was, how are you going to deal with that particular situation?

And the other question was, we had some situations where we had short-lived radionuclides. For example, tritium, which has an effective half-life of 10 days, and the bioassay didn't occur until 30 days or more after the entry.

How would you address situations where you weren't sampling as frequently as you should have? So I really have more questions.

DR. ULSH: Would you like me to respond?

CHAIR BEACH: Yes.

DR. ULSH: Okay.

CHAIR BEACH: Please.

DR. ULSH: I believe that the

questions that Kathy is referring to were the subject of our response dated September 2009. These were the follow-up questions that SC&A had.

I'll read the questions because the response is quite lengthy. I will just refer you to the response.

Question 1 that we addressed was, how will dose reconstruction be completed for individuals who entered under RWPs without appropriate tritium bioassay and did not submit a post-job tritium bioassay sample in a timely manner?

We give a fairly lengthy response here, almost a full page. So I would refer you to that.

SC&A Question --

MEMBER ZIEMER: I have it out here, but can you give us a couple-of-sentence summary?

DR. ULSH: Oh, I've got to read it to find out what we said.

MEMBER ZIEMER: You've got to remember what the response was?

DR. ULSH: Paul, I think in a nutshell what it was was Gene Potter performed a detailed analysis of the RWPs in question that were the subject of the Price-Anderson Act violation, and went through each one of them and looked at when the work, under each one, how many workers, RWPs required a count less than 30 days, and how many workers actually submitted that.

Then, oh, boy, let's see.

MR. CHEW: Gene is on the line, by the way.

DR. ULSH: Hello, Gene.

(No response.)

Are you sure?

(Laughter.)

MR. CHEW: I thought he was.

MEMBER ZIEMER: Well, can I just ask, while you're looking at that, we know who those workers were under the Price-Anderson

thing, right, or do we?

MS. ROBERTSON-DEMERS: There is one other element to this that I have forgotten to mention. For this particular case, yes, you know, because these RWPs that were evaluated were, I believe, from '96 to '97. There is a question, however, well, these are the RWPs that got looked at.

MEMBER ZIEMER: That's what I'm asking.

MS. ROBERTSON-DEMERS: Is there a --

MEMBER ZIEMER: Right, was there a pattern?

MS. ROBERTSON-DEMERS: -- a frequency problem outside of these two?

MEMBER ZIEMER: Well, there's two parts to it. One is it seems to me, if you know the identity of the persons, you can go back and do something with that, based on either the other bioassays or you assume something. So you can reconstruct dose in

some way or another.

But the larger question is, this was a headquarters review under Price-Anderson, was it? So we don't have an indication, or do we, whether it's a sampling? Because, typically, what happens --

MS. ROBERTSON-DEMERS: It is a sampling.

MEMBER ZIEMER: -- if they start to find one or two of these, they keep pulling the string. So sometimes these get to be pretty complete.

So I am just asking if we know the completeness of this. If this is it for that group, I think it's seven.

DR. ULSH: Well, I think the scenario that you describe, Paul, is exactly what happened at Mound. They found a problem on a particular sample. So they went back and looked, and there were actually multiple Price-Anderson Act violations, all related around the same subject.

In fact, we are looking at several RWPs here. One, two, three, four, five, six, seven RWPs here.

MEMBER ZIEMER: Right, right.

DR. ULSH: Of course, we are in the same situation. You can always speculate about, you know, we can't prove a negative. All we can say is that these were the problems that were identified, and we've done an analysis of these identified problems.

Were there others? We don't have evidence to suggest that, but we can't prove a negative. I mean there might have been other problems. Who knows?

MEMBER ZIEMER: On these, you would do something specific for these individuals if there were a claim? Does this show up in their file for the dose reconstructor?

MR. POTTER: Brant, this is Gene. I'm on. I think I missed your calling on me while I was trying to unmute myself.

DR. ULSH: Okay.

(Laughter.)

DR. ULSH: Did you hear Paul's question? And thank you.

MR. POTTER: Yes. One thing that I need to add that hasn't been brought up in the discussion, I mean you've been perfectly right. But the way the program worked at Mound during the D&D era was that if you signed in on an RWP, the clock started running for a bioassay. So as you can see from our list, if you have Table 1 in front of you there, that SW Building, there are four different RWPs being worked in '97 --

DR. ULSH: Yes.

MR. POTTER: -- that required tritium bioassay. Some of these workers were working on various RWPs.

So as a matter of efficiency, the question is really being asked in the wrong way, that people received a periodic tritium bioassay rather than one necessarily after

their very last entry. Therefore, it becomes a missed dose issue sort of thing that is dealt with in dose reconstructions all the time.

But, yes, these are all the cases that were brought up involving tritium, every RWP that was questioned.

DR. ULSH: So our response on that issue is on the table. If there are additional concerns, we would --

MS. ROBERTSON-DEMERS: Were the RWPs -- I don't remember off the top of my head -- from '96 through '97?

DR. ULSH: The ones listed in Table 1 at least are in '97.

Gene, do you have any further insight?

Is that what you're asking about, Kathy, the particular tritium one?

The ones that we were asked to investigate, I think are the subject of the Price-Anderson Act violations; the RWPs look

like they're dated in '97.

MS. ROBERTSON-DEMERS: I guess one of the concerns is if you are sampling for tritium on a monthly basis, are you really adequately capturing an uptake which may have occurred right after the last sample? And how often does this exist?

These are smaller groups of people.

DR. ULSH: Well, again, I guess what I would ask you to do is consider our response here that's on the table. I could read through it, if you would like me to, but --

MEMBER ZIEMER: These are still on routine bioassays as well as the specials?

DR. ULSH: Correct.

MEMBER ZIEMER: So you're talking about a miss, which means that you would overestimate dose because you would take the next one based on an earlier sample rather than the missed one.

DR. ULSH: I believe that's correct. Right, Gene?

MR. POTTER: Right. In this case, we do have the details. As Dr. Ziemer suggested, one could look at the other workers who signed in, but dose reconstructions are not done at that level of detail. You know, it is an underestimation or overestimation approach.

MEMBER ZIEMER: But if this individual had a whole regular series of bioassays, but one of these were missed in this particular work permit, wouldn't you still catch it in the next -- how frequently were they?

MS. ROBERTSON-DEMERS: In this case, the gaps were from --

MR. POTTER: About two-thirds of the people had bioassays within 30 days of their last entry.

MEMBER ZIEMER: Okay, 30 days. What do we have for tritium? Is it eight

days?

MS. ROBERTSON-DEMERS: There is a broader question here.

CHAIR BEACH: And that's the question I want to ask, just for clarification. So there's broader issues that are going to be handled in data adequacy. The questions that were asked in the April Work Group meeting or we asked NIOSH to answer the three questions, those are the ones that have been answered. I believe SC&A was satisfied with those answers. Is that correct?

But I know there is a broader issue related to data adequacy that will be brought up during the data adequacy issue that maybe we should have discussed earlier today. Is that --

MS. ROBERTSON-DEMERS: Which is, were bioassay samples taken frequently enough outside of these identified situations? What are you going to do for people who just kind of drop off the map after they were exposed?

CHAIR BEACH: Okay, because these are kind of new questions. We haven't given these.

DR. NETON: That was the last question that was asked. If you are talking about the actinium people, "How will dose reconstructions be completed for the 11 individuals who submitted actinium samples and did not have a follow-up sample to those discovered in 1995?"

MS. ROBERTSON-DEMERS: Right. We're not talking about those people that are identified. We are talking about others that may exist. These were existing, these people were identified because of a Price-Anderson violation.

CHAIR BEACH: Okay. So I think there's two things going on here. I think Kathy's talking about data adequacy. We should have maybe captured some of that during the data adequacy discussion.

The Price-Anderson, the original

three questions that were on the table, I believe those have been answered. Is that correct?

MS. ROBERTSON-DEMERS: Let me put it to you this way. Twenty-one, matrix item 21, will be closed.

CHAIR BEACH: And that's what I want to get to, but the issues that Kathy is bringing up will need to be put in context and submitted as questions, new questions I would say, probably during -- I think we've already captured that for Issue 11, data adequacy. Is that fair enough?

Because it was confusing to me until I got kind of a handle on where we were. So for the Work Group, the questions that we originally asked at the last Work Group meeting were for NIOSH to submit answers to those first three questions. They have done that, and Kathy has indicated she is satisfied with the answers.

I agree with that, and I would

like to close issue 21 and then take that up --

MEMBER ZIEMER: On your data adequacy?

CHAIR BEACH: Yes. Due to the lateness of the day, how does the rest of the Work Group feel about that?

MEMBER CLAWSON: That is fine, just so we don't lose it.

CHAIR BEACH: We won't lose it. Okay. So that is where we are at with that.

Kathy, do you want to continue discussing that as data adequacy, or do you want to just frame up those questions when Joe submits the questions to NIOSH?

MS. ROBERTSON-DEMERS: I think at this point we haven't asked the question clearly enough to know the answer. So we will put it in --

CHAIR BEACH: Okay. Does everybody agree with that?

Okay. So we are going to consider

PAAA closed, but with some questions that will be in the data adequacy. Okay?

So the last item, D&D issue 10, and I'm not quite sure -- SC&A, do you want to --

MR. FITZGERALD: Yes, let me handle that.

We did a number of interviews, dose and Site Profile and after the ER came out and workers had expressed some concerns over lapel sampling in the D&D era. And we had similar issues, but not the same issues, arise during the Rocky Flats SEC review.

We deferred, I should say the Work Group deferred action on D&D for some time because of some activities going on with the Price-Anderson, Issue 21, whatever. So we didn't really pick it up until, I think, this past summer. So it wasn't that long ago.

And the Work Group requested after the last Work Group meeting to go ahead and put a very brief memo that just highlighted

some of the concerns that were coming out of the interviews and to amplify on what was in the issue matrix, which we did.

I think NIOSH's response was pretty comprehensive, to say the least. I guess I am pleased to announce that there's one issue which will sound familiar to Brant since we, I think, went through this at Rocky Flats, which is the termination bioassays. Of all the issues that were addressed and clarified in the response, I think we are still unsettled about the status of termination bioassays in the D&D era at Mound.

We raised that issue at Rocky because, again, there it wasn't clear, given the transient nature of the workforce, whether there was sufficient termination bioassays upon which to do a coworker model. I think at Rocky the response was to go and take a look at that, compare distributions between the D&D work force data that you had versus the operating work force. It turned out in that

case the distributions were very similar, which certainly facilitated going ahead and using the operating coworker model for the D&D workers.

Here I think the same question is, where do we stand on the D&D work force termination bioassays? Is there enough adequate data there that it is an important coworker model or not?

I think that is the only real lingering question out of all that. I think the rest of it, the lapel sampling and everything else that had been kicking around, I think that has been satisfied.

So that would be the one issue that I would propose the Work Group, I suppose, ask you to maybe come back with, is some information regarding the status of termination bioassays.

I know it was a very small, relatively small percentage at Rocky, but, again, because the dose distributions were

similar between the two workforces, that didn't become an issue in that SEC. But, certainly, you would want to have that at least looked at before we let this one go.

DR. ULSH: I am waiting on the high sign from you.

CHAIR BEACH: Oh, I'm sorry.

(Laughter.)

Please, go ahead.

DR. ULSH: In fact, this issue was included in SC&A's D&D memo, and it was also included in our response to that memo. In our response, we designate it as SC&A Comment No. 7 and our Response No. 7.

I believe that in the original comment that SC&A made they raised this, saying we had a similar concern at Rocky Flats. So our response also focuses on the fact that this issue was already considered and determined not to be an SEC issue at Rocky Flats, and I don't see a whole lot of differences. As you, yourself, stated, the

contractor was the same. So the policies were fairly, were pretty much the same, too.

Just to quote from our response, "It may be true that some D&D workers failed to submit a terminal bioassay. However, Mound also used workplace indicators, such as air monitoring" -- and in parentheses -- "(including lapel air samplers as well as general area samplers), pre-job and process characterization, and routine bioassay. The chances that a worker could receive a significant intake would require the simultaneous failure of multiple levels of monitoring and radiation control, all having occurred after the worker's last bioassay, but prior to termination."

So, certainly, you can speculate a scenario like that, but what we said is this scenario is speculative, and the chances of it occurring are remote, and no evidence to support it has been provided or discovered by NIOSH.

Even in the highly unlikely event that it did happen on occasion, which has not been demonstrated, there would have to be some evidence of selective non-compliance with more highly exposed workers being most likely to skip terminal bioassays. Again, we don't have evidence for that, either.

MR. FITZGERALD: Yes, I think the reaction would be that, again, it's not that dissimilar from Rocky, and I think they also had, as you pointed out, some similar monitoring systems.

But if the percentage of termination bioassays was relatively low, I think -- and again, there is no way of knowing it now; I think that was checked at Rocky, and I think you established it was relatively low. Then that's where we went into this.

They, again, still had the same fallback. I mean they, you know, certainly had lapel sampling. They certainly had bioassays for certain classes of workers. If

you remember, there was a couple different classes of workers. Some, in fact, were on bioassays; some were not.

But, again, I think establishing whether or not you had a low compliance on that issue, and what the implications are in terms of the use of that, of the coworker model for that class of workers, would be something that we would see as the one remaining issue.

CHAIR BEACH: I have a quick question. This may have already been discussed. Did they put a lapel sample on every single worker or was it a group?

DR. ULSH: No, not on every single worker.

CHAIR BEACH: Okay.

MR. FITZGERALD: And I would think you would have a body of workers. You know, you're talking about all kinds of different workers here, people that were doing the actual D&D, construction workers,

electricians. The ones that were clearly in the rad zones doing the teardowns had the lapel sampling and probably more routine bioassays, but you had a whole variety of workers that would have different levels of monitoring. In some cases, if they didn't go right into the D&D rad zone, probably didn't have bioassay, and the baseline and termination bioassay may have been it.

So I guess the question is if they were to be a claimant and were to have indicated at a certain time frame at the site at Mound, how would one do dose reconstruction? It would be you would have to use a coworker model, since you, apparently, wouldn't have data. The question is how would you do it if there wasn't a distribution you could rely on?

I think that is a similar issue to the ones at Rocky.

CHAIR BEACH: Well, you may have a problem identifying all the workers under the

one lapel. Because I know right now at Hanford that dose doesn't always follow the group of workers.

DR. ULSH: Okay, that's a separate issue because --

CHAIR BEACH: But it is an issue that I have.

DR. ULSH: Sure. It would certainly be an issue if we were relying on lapel air samplers to do dose reconstruction. And that is a separate issue that was also covered extensively in our response.

We are not proposing to use lapel air samplers for dose reconstruction. I'm not going to commit to saying that they are never any good. All I'm saying is at Mound we use urinalysis.

CHAIR BEACH: But you used lapel to decide who got the urinalysis? Is that not correct?

DR. ULSH: No, the use of -- not entirely correct. It's incomplete. Let's put

it that way. The lapel air sampling was layered on top of routine bioassay and on top of whatever was required by an RWP.

CHAIR BEACH: Okay.

DR. ULSH: Now, if you had a lapel air sampler that gave you a positive reading, that gave you an indication that there might have been an exposure, then that would be a triggering event for getting a special urinalysis sample.

CHAIR BEACH: But it had to hit a certain --

DR. ULSH: Well, sure, it had to give you a positive indication --

DR. NETON: Above and beyond the routine --

CHAIR BEACH: Okay.

DR. ULSH: But that's layered on top of routine and probably RWP-specific urinalysis as well.

MR. FITZGERALD: But, you know, again, the population of, quote, "D&D workers"

is a pretty heterogeneous group, and they were monitored that way, which made sense. But, on the other hand, if you're going to apply a coworker model for that group, can the distribution be relied upon if, in fact, most of them, if they weren't monitored regularly or had lapel sample, didn't have this termination bioassay?

I'm just raising that question. I think, from our perspective, it would be difficult, and it wouldn't be necessarily representative, if you missed 50 percent, say 50 percent was non-compliance. I don't know what that number is actually.

DR. NETON: It is not clear to me that, you know, people don't -- you're not going to have 50 percent of the people leaving en masse, I don't think.

MR. FITZGERALD: Without doing a termination? I think Rocky came out to be almost a third --

MEMBER CLAWSON: A third?

MR. FITZGERALD: -- as I recall. That's why that concern was the genesis of looking at the distribution because that would, in fact, influence the coworker, if you were missing that much of the --

DR. ULSH: Well, again, I get back to the point in our response where we said that in order for that to be a problem -- let's say it's a third, like it may have been at Rocky. I don't remember.

MR. FITZGERALD: Right, right.

DR. ULSH: You would have to have some evidence that there was selective non-compliance where you would have the highest exposed people more likely to skip their termination bioassay. That doesn't sound plausible to me. I can't envision a scenario where you would have the highest exposed people being more likely than anyone else to skip out on the termination --

MR. FITZGERALD: I don't know. I'm just raising the question how

representative is the distribution. I don't know.

DR. ULSH: I would say, from the Rocky experience, and Gene's on the phone and can attest to this, you're correct that we did do some analyses there that turned out to show not a problem. That wasn't a trivial undertaking.

MR. FITZGERALD: Right.

DR. ULSH: I'm not saying that we're refusing to do it. That's not within my power to do that. But I just want to caution you, consider the level of effort and the time that is going to be involved in pursuing this, if we take a similar approach at Rocky.

MEMBER ZIEMER: At Mound.

DR. ULSH: Yes, sorry. It is late.

(Laughter.)

MR. FITZGERALD: Not to meld the two sites any further, but I just think that is the question that we have on the table. It

is the Work Group's judgment, but whether the coworker model would be representative, if you were missing a large degree of the data, the bioassay, termination bioassay data --

MEMBER CLAWSON: So everybody at Mound during the D&D era were on a routine bioassay?

DR. ULSH: No.

MEMBER ZIEMER: No.

DR. ULSH: The DOE order in place at -- I can never remember that darn thing --

DR. NETON: 54.11.

DR. ULSH: -- 54.11 indicated that if you had less than 100 millirem exposure potential per year, you weren't required to be on a bioassay.

DR. NETON: 10 CFR 835.

DR. ULSH: But if you had an exposure potential higher than that, you were required to do routine bioassay.

MEMBER ZIEMER: So, in principle, it is the most likely exposed people were on

the program.

MEMBER CLAWSON: And I understand what the theory behind that is, but I'm watching it right now, that it's interesting because people are popping from one area to another. Unfortunately, in talking to the workers of Mound, there were numerous ones that said, "I have no bioassays." And they were there for only the last five or six years.

DR. ULSH: It wouldn't surprise me.

DR. NETON: I mean I'm sure they had the rad worker, 1 rad worker 2 categorization going. So the people in the rad worker 2 were the ones that were identified having the potential for exposure, exposures typically exceeding 100 millirems. Your rad worker 1 probably weren't going to be sampled. That was some conscious effort went into deciding, making those decisions.

You also have to remember they had

this lapel air sampling program. By and large, lapel air samples are much more sensitive than any bioassay program for plutonium, for example, much more sensitive. You can get down to DAC-hour tracking on lapel samples, where a urinalysis sample is not going to show something at a much, much higher level of missed dose.

So you've got those things that would trigger the special samples, and there's sort of multiple layers built into this program by this era. I agree with Brant that it would be hard to imagine that a worker would have gotten out the door without having -- with having had a very significant exposure.

MR. FITZGERALD: Yes, one last word. Again, the termination bioassay is your safety net in a system where you are taking people off of routine bioassay, relying on these different hierarchies of specifics, monitoring in certain locations.

I think, whereas in a routine program the termination bioassay is probably less important, in this case I think it is actually relatively more important. But, again, I think --

DR. NETON: Let me be clear what we are talking about here then. I mean these were short-term D&D workers that maybe worked three months and had an entrance baseline and a termination? Is that what we are talking about?

MR. FITZGERALD: I think there is a different -- it is a very heterogeneous group. I think you had people that were probably there for a fair amount. There were people that were in and out fairly quickly. Some were probably rad worker 1 that did real hot work. Some people were probably ones that did support work. Some people were probably construction crews that dug things in the ground who may or may not have been bioassayed, depending on the RWP.

In that kind of a process, you would have to be, I think, careful that, because the group is heterogeneous and you do have some reliance on RWPs and how samples that -- if you didn't have some way to verify before they actually ended employment that they didn't pick something up, I'm not sure how you would actually assign something.

If somebody came back as a claimant saying, "I worked these 18 months at this site, did construction work. I can't remember if I did it under RWP", and voluntarily did not do a bioassay at the end --

DR. ULSH: But let me ask you this, Joe.

MR. FITZGERALD: Yes.

DR. ULSH: I mean everything that you just said here in the past minute or so, you could take out Mound and you could put in Rocky Flats. The situation is exactly the same. You had a heterogeneous population.

You had people --

MR. FITZGERALD: Well, and the same D&D era, too, yes.

DR. ULSH: You had the same regulations in place.

MR. FITZGERALD: Yes.

DR. ULSH: You had the same contractor in place.

MR. FITZGERALD: Right.

DR. ULSH: Is there any reason to believe that the experience at Mound would have been significantly different from the experience at Rocky Flats, given all those similarities?

MR. FITZGERALD: I don't know. I don't know, and you don't, either. I just think we are saying that, based on the system in place, we would believe, but don't know whether or not the lack of termination bioassay would matter.

In other words, we are saying it wouldn't matter because the system was

implemented effectively enough that it is unlikely that somebody who would have had an intake would not -- get off the site without having that intake estimated. And I'm saying, well, that may be, but my experience at DOE, I would be a little nervous to make that assumption that the system was so tight, that you are unlikely to have anybody with an intake not being accounted for, particularly if there is a low compliance rate on the termination bioassay.

These were voluntary, and people just, when they left --

DR. NETON: Were they voluntary? I'm not sure if they --

MR. CHEW: At Rocky Flats, they were. I do not know about Mound.

MR. FITZGERALD: Well, I mean the employment contract had them coming back, but a lot of them just didn't, and when they left the site, they were gone.

DR. NETON: I've had that similar

experience at Fernald.

MR. FITZGERALD: Yes.

DR. NETON: I mean you can't make contractors, who you don't even know are leaving that day, submit a sample. Even if you mail them specimen bottles, you're not going to get them back.

MR. FITZGERALD: I guess it's a two-part question. I mean, if the answer is you had 80 percent compliance, and that number I think is accessible, just by virtue of --

DR. NETON: That was going to be my question. I mean, how difficult would it be to just obtain that raw statistic, which is, what percentage of people who terminated --

DR. ULSH: I don't know.

Gene, are you still on the line?

MR. POTTER: Yes, I am.

DR. ULSH: We actually have the benefit of having Gene on the line. Gene did this effort for us at Rocky Flats, and Gene is

also at least moderately familiar with MESH.

So I'll put that question to you, Gene. How significant of an effort are we talking about here?

MEMBER ZIEMER: Just define the numbers now --

MR. POTTER: I haven't worked with the MESH data recently enough to really answer that question. You know, we struggled with this at Rocky Flats, like all DOE contractors did, and I even did a study among the Westinghouse contractors at one point. This is a common issue, as I think we all realize.

But I would just say that, using the example of Rocky, the people doing the heavy D&D work were the union workers who were generally long-term employees. Most of the ones that were, you know, coming and going, they may have been removing asbestos in non-rad areas, for example. A short-term contract, you may not know they are onsite. You certainly didn't know when they left, that

sort of thing. Not heavy D&D people.

I can't think of a single example where we have a guy, you know, as we have discussed, workplace indicators, such as lapel sampling for contaminations, nasal swabs, and so forth, the real way to detect intakes of alpha emitters of regulatory significance, and I can't think of any of our people who had significant intakes that would not be aware of and participate in the termination bioassay program.

MR. FITZGERALD: Is this Rocky, Westinghouse, and which workers? This is Rocky?

DR. ULSH: Rocky, right, Gene? Who are we talking about?

MR. POTTER: Yes, I'm giving examples of my familiarity with termination bioassay at Rocky.

MR. FITZGERALD: Oh, Rocky. Okay.

MR. POTTER: And also, I tried to answer the question on Mound, but I really

didn't. I'm not sure how hard it would be to look at the termination statistics.

DR. NETON: Yes. You would almost have to know that they were required to leave a sample. So you have to get to an RWP level, I would think.

DR. ULSH: I think that is kind of what we --

DR. NETON: And look at a termination date, and it would be, in retrospect, thinking about it, it might be a fairly --

DR. ULSH: Non-trivial. And again, I bring you back to our response, where we lay out the extensive unlikely chain of events for this to happen. I can't say that it's 100 percent impossible, it's not. But all of these levels of monitoring would have to fail and --

MEMBER ZIEMER: And it would have to be widespread.

DR. ULSH: Well, right, and he

would have to get a significant uptake after his last sample, and then leave before he got another one, and we would have to have a reason to believe that the most highly-exposed people were more likely to have this happen. I just don't see it.

MR. FITZGERALD: Now I know, as I recall, the bioassay program at Rocky did keep that statistic because I think that was one of the drivers for us raising the issue, was I think it was a third or something where they were indicating that was the experience in terms of voluntary compliance with doing a termination bioassay.

I think to look at this issue, rather than you bite the whole apple, the first question is, how widespread? I mean, if it turns out that it is 75 percent compliance, that makes it a much different question than if it's, say, 30 percent compliance. If that answer, if that statistic is available in the bioassay program records or from the D&D era,

which is not that far back, that would be something that would help.

DR. NETON: Unless Mound had calculated it and provided it in some sort of a memo format, I don't know exactly if they are available.

MR. FITZGERALD: I don't know. Like I said, I haven't seen it, but I haven't looked for that specific piece of information. I am wondering if somebody, since that wasn't that far back, I don't know if Liz Brackett or somebody could actually maybe put their fingers on that kind of information.

Because I would propose that you don't launch into something without at least having that piece of information maybe available.

DR. ULSH: You know, our response is on the table. I'm reluctant to commit to something when I don't know what level of resources are going to be required. At least in my estimation, it is very implausible that

there would be a problem. Nonetheless, my opinion is not what drives it. If the Working Group has a sufficient concern, and you would like us to check into it --

CHAIR BEACH: I would like to go on record saying, yes, I would like you to check into it because --

DR. NETON: And keep in mind, this could be a huge man-hour effort.

MEMBER ZIEMER: I wonder if we can do it, I wonder if we could consider a two-step process, where you determine -- because Gene didn't seem to know -- whether or not that information is sort of readily available versus -- and then maybe come back and say, Here's what it's going to take in time and effort to do this.

I would be reluctant to task them to a $50,000 effort or something.

CHAIR BEACH: Right. Well, and I can agree with that, but I also --

MEMBER ZIEMER: I mean, if the

information is readily available, that's one thing. If it is a major -- both in time and effort, then we need to know that, it seems to me, in advance.

MR. CHEW: Just let me make a statement here. We've got to be a little cautious. Just because your name is on an RWP and you signed off on it, it doesn't necessarily mean that you went into the work area to do the work. You were there at the time the RWP was discussed. So not everyone whose name is on the RWP, then, necessarily had to submit a sample because they may not have gone into the hot area to do the job. So I just want to use caution.

MEMBER CLAWSON: And, Mel, I understand that, but vice versa, too. Just because you're not on an RWP does not mean that you didn't go into the area. We see this continuously.

MR. CHEW: That is probably more unlikely, though.

MEMBER ZIEMER: That depends on how enforcement --

MEMBER CLAWSON: Yes.

MEMBER ZIEMER: Just a clarification, what document are you in? I guess I'm looking at the wrong one. What response document are you in at the moment? Or what is the date of it?

DR. ULSH: Well, it is September 2009, and the title is: NIOSH Evaluation of Decontamination and Decommissioning Issues at the Mound Laboratory.

MR. FITZGERALD: And that was No. -- what number was it?

DR. ULSH: I don't know. Matrix issue, you mean?

CHAIR BEACH: Oh, 10.

DR. ULSH: Ten?

CHAIR BEACH: Ten?

DR. ULSH: Matrix Issue 10.

CHAIR BEACH: Yes.

DR. ULSH: Paul, I sure hope this

is not one where I forgot to put you on distribution.

MEMBER ZIEMER: Well, I'm not finding it here, but sometimes it's --

MR. FITZGERALD: It is SC&A Comment No. 7 in that document.

CHAIR BEACH: Well, I've got a hard copy here, too, that I would hand to you, if you want to look at it, Paul.

MEMBER ZIEMER: Yes.

CHAIR BEACH: I am just not willing to let that issue go. I understand Paul's suggestion of maybe doing it in a two-step. I would agree with that.

DR. ULSH: So is the action item, then, that I hear is: get back to the Working Group with an estimate of what level of effort is required to come up with the frequency of termination bioassay? Right?

CHAIR BEACH: Yes.

MEMBER CLAWSON: At least the feasibility of it.

DR. NETON: Or, if it is readily available, we would report the number, but if it looks like it is going to be more than just a trivial exercise, we would report back.

MR. FITZGERALD: I guess it is the difference between, if there was some documentation, a memo, or something that would put that information forward or not --

DR. ULSH: If it has already been calculated?

MR. FITZGERALD: Right, yes. In a sense, if somebody was tracking this, if it wasn't done, then, yes, I think doing it fresh would be pretty onerous.

DR. ULSH: Okay, I've got that noted as an action item.

MEMBER ZIEMER: Is this NIOSH response to Mound matrix Issue 10, dated 9/4?

DR. ULSH: Oh, no.

(Laughter.)

CHAIR BEACH: Did I give you the wrong one?

DR. ULSH: 9/4? September 4th?

MEMBER ZIEMER: Yes. Go ahead.

CHAIR BEACH: 2009?

DR. ULSH: Oh, you're talking about an email title? That may very well be it.

MEMBER ZIEMER: No, I'm talking about a document. Yes, it is this one that you -- I have this.

DR. ULSH: What you just read, NIOSH response, was the subject --

MEMBER ZIEMER: That was the email title.

DR. ULSH: -- line of the email, yes.

MEMBER ZIEMER: Yes, yes. I've got it.

CHAIR BEACH: Okay, anything else on D&D?

(No response.)

Any unfinished issues? I think we would like to get back to you in email form on

action items.

DR. ULSH: That would be great.

CHAIR BEACH: Instead of trying to rehash them here with the scribbles that I have and Joe has, and probably everybody else.

MEMBER CLAWSON: Josie, I do have some questions --

CHAIR BEACH: Okay.

MEMBER CLAWSON: -- with Brant on tritium.

You said earlier that the reason that you don't have a coworker model for tritium was because you had specific people that were, 10 people, if I remember right, it was?

DR. ULSH: No. You're mashing together --

MEMBER CLAWSON: Tritides. Tritides. Okay.

DR. ULSH: For hafnium tritide, what I am saying is the workers that we interviewed named the 10 people who could have

an exposure potential for hafnium tritide.

For other tritides, we are proposing that anyone on tritium bioassay at Mound could have potentially been exposed to those others.

MEMBER CLAWSON: Okay. Well, with those 10 people, here's my question. It's twofold. How were they controlled? How did they control that they were on a tritium bioassay? I mean, was it the facilities?

My understanding was these facilities, you have to be on a bioassay.

DR. ULSH: Yes. Yes. In the tritium buildings at Mound, to go in there in that time frame, you were required to be on tritium bioassay. So, for those 10 people, yes, they would most certainly be on tritium bioassay.

MEMBER CLAWSON: Well, what happened to the product after you got done? This hafnium tritide, once they built this, were there any other people that could have --

or what happened to the product after they made this? Who could have been involved with it?

DR. ULSH: It went to its intended purpose.

MEMBER CLAWSON: Okay. So nobody else ever touched it except these 10 people? This is part of my thing. I'm seeing and I'm watching that maybe on this scale in the laboratory they built this project. They built this item, and those 10 people were the only ones that were involved with it. But then it proceeded on down the pathway, and other people were involved with it then, because they weren't involved in the production of it, but they were involved in the handling and processing of it.

DR. ULSH: Let me be careful.

MEMBER CLAWSON: Right.

DR. ULSH: It would not have been in the form, Brad, that would have presented an exposure potential.

Now I think that that is my answer. There might be some situations that we're still discussing with SC&A.

MEMBER CLAWSON: Okay.

DR. ULSH: So I don't want to present --

MEMBER CLAWSON: And I think those we're going to have to do in a different atmosphere because I just want to make sure, because I have seen this happen before. As this product is produced, that part of it was reclassified, and as it went down the line, it --

DR. ULSH: Became less and less --

MEMBER CLAWSON: You didn't have a need to know of it. You know, you were just handling it.

But the only controls that bother me is I know right now in facilities that, if you work in this facility, then you are on this bioassay program. Now, for somebody to come into these facilities and do work, say

electricians versus whatever, that are not assigned to that facility, were the only controls rad worker permits?

Because I see it quite often that people come in and work in areas that require bioassays, but they are not a continuous worker there, so they don't put it. This is where I'm getting with the tritium. There's not a magic door there that you have to slide to be able to get through.

I know in our areas now we have people that, if you don't have keycard access, you can't get into our facilities because of the requirements. But there was nothing like that in Mound.

Some of the employees, and one of them was an electrician who made a comment that he was not on the tritium bioassay because he was actually out of kind of the central shop. I mean he didn't have to submit them because --

DR. ULSH: What time period? Do

you recall?

MEMBER CLAWSON: This was in the, well, he was there for 20 years, and the last era was in the late nineties. So, you know, that's back into the --

DR. ULSH: Without knowing the particulars, it's hard to say, Brad.

MEMBER CLAWSON: Right.

DR. ULSH: But my understanding is that, before DOE -- after that, there was the 100 millirem per year criteria. So it is entirely plausible to me that the situation you are describing, where someone came out of the central shop, or whatever, if there was a judgment that his exposure potential was less than 100 millirem per year, he would not be required to be on bioassay after that time.

Before that time, it is my understanding that, if you went into the tritium buildings at Mound, even if you were a visitor, you were required to leave a tritium urine sample.

MEMBER CLAWSON: Well, this is where we got into this visitor-type step. I'm just trying to get that clarified in my mind because it is amazing to me, and I see it continuously, that things fall through the cracks.

DR. ULSH: Well, and I can't --

MEMBER CLAWSON: You know, you can't -- I realize that. You can't --

DR. ULSH: And, Brad, I can also tell you, though, that if you look at layers of access, I can't tell you that there were keycard controls or that kind of thing.

MEMBER CLAWSON: There was, and I've already looked.

DR. ULSH: But you've got the tritium building --

MEMBER CLAWSON: Now in certain rooms there were.

DR. ULSH: That's where I am headed.

MEMBER CLAWSON: Right.

DR. ULSH: The tritium building does not equate to tritide necessarily, and that certainly doesn't equate to hafnium tritide.

MEMBER CLAWSON: Right.

DR. ULSH: The places where the hafnium tritide were being worked on were security-controlled, security padlock-controlled, and you didn't just wander in. If you were an electrician from somewhere else, if you didn't -- I mean you didn't just wander in there.

MR. CHEW: I think, and I remember that interview, too, when somebody had to come in to do any kind of work that you are talking about, I think the persons we interviewed said they secured the material.

DR. ULSH: Right.

MR. CHEW: That was very clear.

DR. ULSH: Yes.

MR. CHEW: Not only from a security standpoint, but from exposure to

that --

DR. ULSH: Well, it was in the follow-up.

MR. CHEW: Right.

DR. ULSH: Remember, we had the meeting in Germantown, and that was one of the things we were asked to follow up on with that person.

MR. CHEW: Right.

DR. ULSH: And we did that. That was the outcome of that.

MEMBER CLAWSON: Okay. Earlier today, we got into a discussion of when there was a question, and please forgive me for my ignorance, but I'm trying to understand the rules and the laws, too. You made the comment that you didn't understand why -- if this was an SEC issue. I want to ask you, what is considered an SEC issue?

(Laughter.)

Because I will be right honest with you. My interpretation of this is, if

there's a lack of data there, then it is an SEC issue. But I'm seeing that, if we can put some numbers up on it, then it is not an SEC issue. I'm just trying to get a feeling for what truly is the SEC issue.

DR. ULSH: Do you recall the context? Was it in the tritide discussion that this came up?

MEMBER CLAWSON: Yes.

DR. ULSH: Okay. What I meant when I said this is not an SEC issue is that, if we agree that the dose from hafnium tritide can be modeled, then, in my mind, it's my position that the SEC issue was closed. There might very well be a legitimate TBD issue.

In other words, the question would be, okay, well, you've got this model for hafnium tritide, but who are you going to apply it to? Is it just these 10 people? Is it a larger group of people?

A very valid question, but not an SEC issue. It is a TBD issue.

MEMBER CLAWSON: Okay.

DR. ULSH: Because, at worst -- at worst -- we could say we're just going to apply it to everyone on tritium bioassay. So, to me, that's an application question. That is a TBD issue. That is totally separate from SEC.

MEMBER CLAWSON: And this is what I'm trying to understand because, in that context, it basically could be said that there are no SEC issues because we can always put a number on it.

MEMBER ZIEMER: No, no.

DR. ULSH: Not necessarily. Not necessarily, Brad.

For instance, let's say we couldn't agree. Let's say it was -- I don't know; I'm just saying -- let's say it was SC&A's position that there's no way you can estimate doses from hafnium tritide. That would be an SEC issue because then we can't estimate the dose. We can't bound the dose.

But, once we agree that you can do that, then we enter into the arena.

MEMBER CLAWSON: And like I say, please forgive me for my ignorance here, but then we go clear to the other side of if it's plausible or not. And I'm trying to get it figured out because I've seen some lung counts now that, you know, you even say to yourself, Paul, why couldn't we figure this one? Because of the radon, then it went clear to implausible, but it's an SEC issue.

I am really trying to get a handle around an SEC issue because, to me, take the 420 boxes that were buried. You know, there's insufficient data there. Or it's come up so many times, and I really have a hard time understanding about the SEC issues, of what constitutes a lack of information.

MEMBER ZIEMER: Well, the ones where we have had clear SECs are ones where they don't have an idea, for example, on how much activity was there. That would be a

simple case. You don't know how much activity was there. You know that there was, let's say, thorium, but not how much or how it was used. You don't have any basis to come up with any number, high or low.

DR. NETON: If you look at the SECs that have been granted to date, most of them have been internal exposure issues where there is no monitoring data, thorium particularly and some other nuclides. So you have no bioassay monitoring data and no means to determine what the upper limit could have possibly been, based on other values, like air sampling. There's no good air sampling measurements. There's no source term mix.

MEMBER ZIEMER: Which is different than saying it is a big dose, but I know it is no greater than this. You can't bound it.

DR. NETON: Right. Otherwise, we would be guessing, if we had to put an upper limit. I could say it's certainly less than some million rem, but that's not a plausible

value. There's no logical connection there why you believe it --

MEMBER CLAWSON: Well, I'm trying to figure out where that area is in there because I'll give you a situation, and we just have it, and that's NTS. Look at everything that we had there, and then, all of a sudden, it's not.

I'm really having a hard time getting around what really is an SEC issue because it seems like to me that we could put a number on anything, but then we get into the plausible and not plausible.

DR. NETON: Yes, it is a very difficult issue. I think you are not alone in that sense. I mean it is a struggle to determine when it is truly implausible. That is why we have these debates. I mean it takes a long time to -- or discussions, I'll say -- to come to that conclusion.

And NTS is a good example. It took us a while to pull the thread far enough

to say, you know, at the end of the day, it's true, we don't really know with any confidence what the upper limit on these exposures were. We've got a lot of data, but we had to pull the thread all the way to the end, and then finally say there's no more thread to pull, and there's no connection we could make to their exposures, based on the bioassay monitoring program that was in place at the time.

MEMBER CLAWSON: Well, so I hope you, I hope NIOSH understands, as the Work Group, why we pull on some of these threads so far. It is because this has been an ongoing thing. I hope that the frustration with us, me and sort of whatever else like that, but this is a difficult thing for us to get around. It sounds like that it is difficult for all of us. And I know for the claimants because I have heard numerous times, how come this; how come that?

And if there's no data, you have

only got these two points, how come don't we get it? I just wanted to know where we were at on that because I'm really having a hard time getting around that. I'll be honest.

It's yours.

CHAIR BEACH: The only other thing is the security thing we talked about. Do we want to try to come up with some type of a date or should we wait? Because I know the biggest holdup will be getting the documents to one place, which we haven't agreed on where that may be.

DR. ULSH: Josie, can you talk about that after we close?

CHAIR BEACH: Yes, sure.

DR. ULSH: I've got some thoughts, and maybe I can get some clarification from you.

CHAIR BEACH: Okay.

MEMBER CLAWSON: I know DOE-Idaho has got some nice areas.

CHAIR BEACH: So does Hanford.

(Laughter.)

Okay. So, then, I would like to officially close this portion of the meeting.

MR. KATZ: Do you even want to try to schedule the next or is that too many uncertainties to do that?

CHAIR BEACH: Can we do that offline?

MR. KATZ: Yes, we can. We don't have to schedule online.

CHAIR BEACH: Okay. Let's close then.

MEMBER CLAWSON: I think, most of all, we have got to get our kind of note to --

CHAIR BEACH: Well, we need to get the action items out, so that everybody kind of knows. Because I know some things SC&A is going to wait for NIOSH. So it might be difficult to try to plan it.

And we also need to have the secure meeting before --

MR. KATZ: Sure, and that can take

some doing.

CHAIR BEACH: Yes. It's tough.

MR. KATZ: So we are adjourned?

CHAIR BEACH: Yes.

MR. KATZ: We are adjourned. Thank you, everyone who has hung in with us on the telephone.

(Whereupon, the above-entitled matter went off the record at 4:11 p.m.)

1

2

3

4

5

6