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

convenes the

WORKING GROUP MEETING

ADVISORY BOARD ON

RADIATION AND WORKER HEALTH

ROCKY FLATS

The verbatim transcript of the Working Group Meeting of the Advisory Board on Radiation and Worker Health held in Hebron, Kentucky on April 19, 2007.

C O N T E N T S

April 19, 2007

TRANSCRIPT LEGEND

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-- (sic) denotes an incorrect usage or pronunciation of a word which is transcribed in its original form as reported.

-- (phonetically) indicates a phonetic spelling of the word if no confirmation of the correct spelling is available.

-- "uh-huh" represents an affirmative response, and "uh-uh" represents a negative response.

-- "*" denotes a spelling based on phonetics, without reference available.

-- “^”/(inaudible)/(unintelligible) signifies speaker failure, usually failure to use a microphone.

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(By Group, in Alphabetical Order)

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BARRIE, TERRIE, ANWAG
BOLLER, CAROLYN, CONG. UDALL
BRACKETT, LIZ, ORAU
BROEHM, JASON, CDC WASHINGTON
BUCHANAN, RON, SC&A
CHANG, CHIA-CHIA, NIOSH
ELLIOTT, LARRY, NIOSH
FALK, ROGER, ORAU
FITZGERALD, JOE, SC&A
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HOFF, JENNIFER, ORAU
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HOWELL, EMILY, HHS
JESSEN, KARIN, ORAUT
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LANGSTED, JIM, ORAU
LITTLE, CRAIG, ORAU
LOPEZ, TERESA, ORAU
MAKHIJANI, ARJUN, SC&A
MAURO, JOHN, SC&A
MCFEE, MATT, ORAU
MINKS, ERIN, SEN. SALAZAR
NETON, JIM, NIOSH
PHELEN, CHRIS, CONG. LAMBORN
RAFKY, MICHAEL, HHS
SEWELL, JESSICA, CONG. DEGETTE
SHARFI, MUTTY, ORAU
SMITH, MATTHEW, ORAU
ULSH, BRANT, NIOSH

P R O C E E D I N G S

(9:30 a.m.)

(Court Reporter's Note: The following transcript contains intermittent drops of speech due to telephonic transmission problems. This is indicated by the "^" symbol.)

WELCOME AND OPENING COMMENTS

DR. LEWIS WADE, DFO

DR. WADE: This is Lew Wade and I have the privilege of serving as the designated named federal official for the Advisory Board. And this is a meeting of a work group of the Advisory Board. This work group is looking at issues related to the Rocky Flats site profile and the Rocky Flats SEC petition. The work group is chaired by Mark Griffon, members Gibson, Presley, Munn. Griffon, Presley and Munn are here in the room.

Mike Gibson, are you with us on the telephone?

MR. GIBSON (by Telephone): Yeah, Lew, I'm here.

DR. WADE: Ray, you're ready to begin?

Let me begin by asking if there are

any other Board members who are not members of this work group who are participating in this call? Any other Board members participating in this call?

(no response)

DR. WADE: That's important because we don't have a quorum of the Board which we cannot have and conduct this meeting.

What I'd like to do is go through introductions, and I'll do that by just going around the table here. Then I'll ask for other members on the telephone of the NIOSH and ORAU team. I'll ask for other SC&A participants. I'll ask for other workers, worker reps, members of Congress or their staffs, other federal officials who are on the call by virtue of their employment, and then anyone else who would like to be identified.

For the members of the Board and the NIOSH/ORAU team and SC&A I would ask you also to identify whether or not you have conflicts relative to the Rocky Flats site.

So I'll begin. This is Lew Wade. I serve the Advisory Board. I also work for NIOSH.

MR. ELLIOTT: Larry Elliott, NIOSH, I have no conflicts.

DR. NETON: Jim Neton, NIOSH, no conflicts.

MR. PRESLEY: Robert Presley, Board member, no conflict.

MR. SHARFI: Mutty Sharfi, ORAU team, no conflicts.

DR. LITTLE: Craig Little, ORAU team, no conflicts.

DR. ULSH: Brant Ulsh with NIOSH, no conflicts.

MS. JESSEN: Karin Jessen with ORAU team, no personal conflicts.

MS. HOFF: Jennifer Hoff with the ORAU team, no personal conflicts.

MR. BUCHANAN: Ron Buchanan, SC&A, no conflicts.

MS. HOWELL: Emily Howell, HHS.

MR. GRIFFON: And Mark Griffon with the Board, no conflicts.

MR. FITZGERALD: Joe Fitzgerald with SC&A, no conflicts.

MS. MUNN: Wanda Munn, Board member, no conflicts.

DR. WADE: Those are the people in the room

at the moment. Let me go out now and ask for other members of the NIOSH/ORAU team to identify themselves and to identify if they have any conflicts on this site.

MR. RAFKY (by Telephone): Michael Rafky, HHS, no conflicts.

MR. FALK (by Telephone): This is Roger Falk. I'm with ORAU, and, yes, I have conflicts.

MS. BRACKETT (by Telephone): Liz Brackett with the ORAU team, no conflicts.

MR. SMITH (by Telephone): Matthew Smith with the ORAU team, no conflicts.

MR. LANGSTED (by Telephone): Jim Langsted ^, I have conflicts.

MR. McFEE (by Telephone): Matt McFee with the ORAU team. I have no conflicts.

MS. LOPEZ (by Telephone): Teresa Lopez with the ORAU team. I have no conflicts.

DR. WADE: Other members of the NIOSH/ORAU team?

MR. LaBONE (by Telephone): This is Tom LaBone. I have no conflicts.

DR. WADE: Other members of the NIOSH/ORAU team?

(no response)

DR. WADE: Members of the SC&A team?

DR. MAURO (by Telephone): John ^ with SC&A, I have no ^.

DR. WADE: John, you cut out on us. Could you please repeat?

DR. MAURO: John Mauro with SC&A, no conflicts.

DR. MAKHIJANI (by Telephone): This is Arjun Makhijani, no conflicts.

DR. WADE: Any other members of the SC&A team?

(no response)

DR. WADE: Other federal employees who are on the call by virtue of their employment?

MR. BROEHM (by Telephone): Jason Broehm, CDC.

DR. WADE: Other federal employees?

MS. CHANG (by Telephone): Chia-Chia Chang, NIOSH.

DR. WADE: Other federal employees?

(no response)

DR. WADE: Workers, worker reps, members of Congress or their staffs?

MS. BOLLER (by Telephone): Carolyn Boller

with Congressman Udall's office.

MR. HILLER (by Telephone): David Hiller from Senator Salazar's office.

MS. MINKS (by Telephone): Erin Minks from Senator Salazar's office.

MR. HOLEN (by Telephone): Bill Holen from Congressman Perlmutter's office.

MS. SEWELL (by Telephone): Jessica Sewell with Congresswoman DeGette's office.

MR. PHELEN (by Telephone): Chris Phelen with Congressman Lamborn's office.

MS. ALBERG (by Telephone): Jeanette Alberg with Senator Allard's office.

MS. BARRIE (by Telephone): Terrie Barrie with ANWAG.

MS. BARKER (by Telephone): Kay Barker with ANWAG.

DR. WADE: Other workers, worker reps, members of Congress or their staffs?

(no response)

DR. WADE: Is there anyone else on the call who would like to be identified for the record?

(no response)

DR. WADE: Anyone else who would like to be

identified for the record?

(no response)

DR. WADE: Okay, let me just do a little bit of a discussion of phone etiquette. Again, we're getting better at this. Please, if you are going to speak, speak into a handset and not a speaker phone. If you're not speaking, and you have the ability to mute the instrument then please do that. Be mindful of background noises, babies crying, dogs barking. If you put the phone on hold, and you know that there's background music played when you do that, don't do that. Again, it's important that we observe those simple rules so these meetings that are terribly important can be as inclusive as can be of people who want to participate and hear the deliberations.

Mark, it's all yours.

WORKING GROUP UPDATE

MR. GRIFFON: I did put out a short agenda. I hope everyone received that. And basically as an introduction I think our main goal here, and we're coming down to the wire, is to be in a place where we can have a vote in the May

meeting, the May Advisory Board meeting. And at this point we have, we have received SC&A's report although there's going to be a supplement to that report.

But I think I'd like SC&A to kind of go over their report, just the executive summary. I think there's five primary points in the executive summary, just to give us a perspective on sort of what's left out there, what the main conclusions of SC&A's report are.

And I think as Joe probably is going to say, a lot of the primary points lead into the subsequent agenda items that I have down here. So maybe we won't get into the full discussion of those, but this is kind of an overview of where we're going.

I think today the most detailed discussion is probably going to be around the neutron questions and obviously that's been the, or maybe not so obviously, but since the last work group meeting I think there's been a lot of activity around the questions on the neutron dose data, the NDRP report. And just as an introduction here, I mean, I think

everybody was working diligently on these action items.

And part of the reason this comes up now as a big issue is because we received some actions, some outstanding actions from NIOSH, SC&A received those. And as they did analysis on some of those, you know, it raised questions and it raised questions about the NDRP report itself. So we had some spreadsheets that were sort of provided as actions, outstanding action items that we had for the neutron questions and that led into some further discussion on the NDRP report itself.

And for those that don't know, in between the last work group meeting I think we had two technical calls, was it? Right?

MR. FITZGERALD: Two calls.

MR. GRIFFON: We had two technical calls between SC&A and NIOSH. I was on both those calls on and off actually, but I was on part of both those calls. And a lot of that had to do with, we had Roger Falk on the phone basically asking about the NDRP report itself, how the NDRP research was conducted, some

background on that just to get a sense of what in this database we were looking at.

Also since the last meeting, and this all came out of that last action, too, I had asked, since there were questions on the spreadsheet I asked Brant can we just have the NDRP database posted itself. The action items before that were actually based on claimant data, NDRP data but claimant data only. And I said can you just provide the entire NDRP database on the O drive so we can have a look at all the data instead of just claimant data. And Brant did post that.

So then when we started looking at that full database that raised some questions. And we thought we don't want to delay this. We want to keep things moving. Let's see if we can get Roger on the phone and ask some questions about what these different data columns mean, how they came to the conclusions they did in that report. And so Brant was nice enough to set up these two technical calls, and we had that happen in between the work groups.

And also just last point of

introduction and then we'll get into the meeting, both these calls we did have full minutes taken. They're pretty in-depth minutes of the calls, and they will be available as attachments to SC&A's supplement report on this neutron question. I don't know if they --

Lew, have they been, they haven't been reviewed yet or circulated at this point. So for those on the phone you haven't seen these, but there are detailed minutes of these calls, not transcripts, but detailed minutes. So we want to make sure that everyone knows exactly what was discussed on these calls. Really, we did these in a non-work group setting just to keep, to expedite, to keep this moving on course for the May meeting. And that was the driver behind that.

Anyway, I think I'll go into the items and let Joe Fitzgerald from SC&A give us an overview on their report, and then we'll go into the in-depth questions, the neutron-thorium, data completeness, et cetera, on the agenda, if that's okay.

Questions?

(no response)

MR. GRIFFON: Joe?

SC&A REPORT

MR. FITZGERALD: Good morning, this is Joe Fitzgerald. I am the SC&A lead for Rocky Flats. You now have the draft report that we've been working on for some time, and Mark mentioned, I guess, five issues which we summarize in the executive summary to that draft report. That certainly isn't necessarily the scope of the issues that we addressed in the report as you will see by reading the executive summary and going through it.

We have done quite a considerable amount of effort to try to resolve and converge on a number of these concerns. And certainly issues like high fired plutonium oxide. We're concerned about measuring that, the concern about the reliability and the integrity of the data itself. These are all issues that we treat in quite a bit of detail and have spent a considerable amount of time with NIOSH in this working group to reach resolution.

So what we're focused on at this particular meeting, and what we've been focused on for the past several weeks and will be focused on until the meeting, is addressing the issues which we feel, frankly, are either not resolved yet, meaning that even through all these efforts, we still have some remaining concerns, or we feel there is legitimate concern about the adequacy of the data or its completeness. So we're certainly beyond issuing this draft also addressing with this work group and NIOSH these remaining issues.

And the five issues, and these are outlined I think in the draft that you have. Certainly, Mark mentions the neutron question. This has been a longstanding question that we've been concerned about since the middle of last year because of the uncertainties incumbent on, and this is not just Rocky Flats, this is really across the Department of Energy complex in the 1950s.

Certainly, the technology was evolving, and the ability to, in fact, measure neutrons was evolving as well. So our concern

with the so-called early years, the 1950s into the early ‘60s, was the ability to estimate those neutron doses where, in fact, you might have gaps in the data or the technology had not caught up yet.

And for Rocky Flats the concern that we were focused on was the so-called back extrapolation which is a fancy way of saying if you don’t really have all of the neutron exposure data for, say, the 1950s in this case, specifically 1952 to ’58, the approach that’s evinced in the NIOSH model -- this is in their coworker model -- was to back extrapolate to apply the values, in this case the neutron/photon ratio values of 1959 backwards to those years and to apply them.

And we certainly had questions about their concerns and wanted to see that data, the individual information for the workers involved and to understand the basis for that approach. And that basis, of course, as Mark mentioned, is rooted in the NDRP, the Neutron Dose Reconstruction Database, which Rocky applied.

And we did get this data finally in

March and have spent, I think, considerable effort to understand how that's used and to understand whether there would be any issues in terms of using that for this specific purpose, understanding that that time was used certainly to come up with a better means of measuring neutrons. We're looking at it in the context of how effectively and adequately it could be used for the purpose of dose reconstruction. And we'll get into that in greater detail since that's certainly a foundation concern for us at this point.

MR. GRIFFON: I'm not sure, just one clarification. We'll get into details later, but I think that back extrapolation from '59 was actually in the NDRP project, not in NIOSH's coworker model.

DR. ULSH: Well, it was in the NDRP. That was the original source, but it was adopted.

MR. FITZGERALD: And certainly in looking at the NDRP, as we indicated in the draft report, we're also looking forward into the 1960s as well just trying to understand how that circumstance may change and whether there would be any similar concerns getting into the

early ‘60s, understanding this wasn’t a stepped function, that certainly the situation evolved over time at Rocky Flats. So we’ll get into that in more detail.

Certainly, the other issues that are identified in the report are probably ones that are familiar with certainly others on this phone call. We looked at the completeness of the database. I think we made a lot of headway, frankly, in terms of establishing what, in fact, was complete but where there may have been gaps in a couple of instances for in particular 1969 and ’70.

The work group asked SC&A to focus on the observed prevalence of zeros that were assigned to a number of workers at the site, and we did so, and I think with NIOSH established that there was, in fact, a badging policy where some of these badges were not read and zeros were assigned. I don’t think there’s any, frankly, disagreement there. NIOSH has, and we can certainly touch on this later after we get through neutrons, but has offered a revision of this coworker model, OTIB-58 which is the external coworker model

which has removed in a sense those zeros as a way of addressing, at least in a preliminary way, addressing this question of assigning zeros in the absence of badges being turned in. So certainly, that's been progress.

Now the other issue that we're looking at that's identified in the report is non-plutonium workers, in this case specific to Building 441, where --

DR. MAKHIJANI (by Telephone): Four-four-four.

MR. FITZGERALD: I'm sorry, 444, Arjun, where we believe that there are still some concerns as to whether there is a valid coworker model which would address those specific workers at that facility. And we can touch on that in a bit as well.

The remaining two issues are ones where I think we did spend a considerable amount of time trying to establish whether the analyses and the model provided in the NIOSH report were, in fact, bounding of the doses that might be presented. One was the case of thorium use at Rocky Flats, and there I don't think there's any disagreement that there may

be sufficient data to, in fact, use it to bound historic thorium doses.

I think the issue that remains is whether the model approach that NIOSH has adapted which is provided in NUREG-1400, whether that has been demonstrated to be necessarily bounding. And I think we feel there's some concerns left there that bear further resolution with NIOSH. And we do think that there is, in fact, some data, available data, that could be applied, but we have not seen evidence that the current model is necessarily bounding.

The final issue is one that also we've addressed from the standpoint of being able to demonstrate whether, in fact, the approach is conservative which is in OTIB-38 which is the internal coworker model. We still have concerns about whether anything but a 95th percentile or higher, which is a statistical approach where conservative fit for distribution is applied, would be sufficient to address the uncertainty that is incumbent in that database. And so we're concerned there whether, in fact, in practice the more

conservative approach would be applied in those cases.

And we're also aware that, of course, for Rocky Flats the number of unmonitored workers for which a coworker approach for internal dose will be applied is relatively small. But nonetheless we feel it's important to go ahead and raise that issue.

Given the spectrum of information and issues that we did include in the report I think just the fact that we're at this point where we have these specific questions left I think is still a considerable amount of progress, but we feel that these aren't quite resolved, and we need to address them before we can actually have closure on the evaluation.

So with that do you want to --

MR. GRIFFON: No, I think, I mean, that's a good overview. I don't think we want to go into any of those points in depth right here. If we could hold questions until we get to the specific items, I think that would be a better process.

MS. MUNN: Mark, I have one question. This

is Wanda. Joe, do I understand that there is going to be a supplement to this report before May, but that that supplement will not, will it or will not, address any of these issues to the extent that they can be considered resolved?

MR. FITZGERALD: Well, I think the supplement is just that, that if we can reach closure on any of these other issues, we certainly would want to acknowledge that in the supplement. I think the supplement will be a snapshot by next week of where we stand on the remaining issues, these remaining five issues. Now clearly, I think the thrust, major thrust, of the supplement is going to be the neutron issue just because that probably had the most to be addressed in the way of actual data analysis.

But certainly, for example, the issuance of OTIB-58 with the recognition of the ’69-’70 issue is certainly one possibility, and we would want to certainly look at that and acknowledge whether that satisfies that particular issue. That would be an example that we would put that in the

supplement as well. So the supplement I think will be sort of the, if you may, the final written word in addition to the draft report as to what we were able to achieve in the last four or five weeks.

MS. MUNN: And hopefully here today.

MR. FITZGERALD: And hopefully here today. In fact, that's the timing of the supplement which is to reflect everything up to and including today and have it available by next week to enhance the meeting.

DR. WADE: Can you be more specific, Joe, about when actually, I'm just trying to make sure that the Privacy Act --

MR. GRIFFON: I know. The timing is critical.

DR. WADE: Especially right now.

MR. FITZGERALD: I guess in terms of pinpointing a day, Arjun, are we able to plus or minus a day or two?

DR. MAKHIJANI (by Telephone): Well, what we've done almost all of the digging on the neutron question in terms of the data. As you know, Joe, some of the writing is done, but it hasn't been checked or QA'd or anything, and

there's a lot there. I think, oh, goodness, Thursday of next week? Wednesday --

MR. FITZGERALD: The concern is the Privacy Act review on the --

DR. MAKHIJANI (by Telephone): Yes, what I'm thinking, what I'm thinking is if we can prepare a report that has no names in it or a summary at least that has no names in it, but that can be maybe with your permission and with Emily and Liz agreeing, circulate it.

And then whatever pieces that may have names in it or individuals mentioned other than, you know, we've had these conversations, of course, with Roger that are expert conversations that are essentially public conversations, what he's going to say today or, you know, what he said on those calls as experts which I understand are allowed.

Except for those, I think we can keep names out of the report. Maybe just publish the report, send the rest of it for Privacy Act review like the spreadsheets. And I don't, that might be a way to proceed that might put things in the hands of the Board and the petitioner rapidly.

MS. HOWELL: Arjun, this is Emily. Obviously, as usual, if you have something put together the Board members and members of the OCAS and ORAU teams can see that, but we cannot make anything public on the OCAS web page or available to the Congressional staffers, who I'm sure are very interested in this, until a full privacy review has had time to take place. Can anybody give me a rough estimate of how large you anticipate this document being?

DR. MAKHIJANI (by Telephone): I had a question about that. Privacy Act review is necessary even if it has no names in it? Or no individual, no names and no -- I just want some clarity because this is something that we revisit quite a bit, and that's not my understanding of my instructions from our project manager at SC&A. Is that if there's no individual data and no individual names and no workers that --

MS. HOWELL: Arjun, we can discuss that offline perhaps. I think that it doesn't necessarily sound like this supplement will have no Privacy Act information in it. I

mean, like I said, it can go to the Board members who obviously need the most time to spend with it prior to the meeting, but --

MR. FITZGERALD: I guess from the standpoint of back engineering this, and we'll talk this through in offline work with the work group, but it sounds like we should aim to enable NIOSH to post this for the Congressional delegations and petitioners by a week from this Friday I would think as at the minimal, which would mean that back engineering from that we would need to get it to General Counsel by --

MS. MUNN: Tomorrow.

MS. HOWELL: Well, how, do you have any, can you estimate a page number for me? I need to know how much.

MR. FITZGERALD: I think that certainly it's going to be about 25, 30 --

DR. MAKHIJANI (by Telephone): Fifty pages.

MR. FITZGERALD: Fifty pages.

MS. HOWELL: That'll be fine. If you could give us 48 hours from the time that, 48 hours not including a weekend, 48 hours on week days.

MR. FITZGERALD: Okay, it does look like we're talking middle of next week at the very outside. So we'll have to talk about that and make it happen. I mean I certainly with a meeting coming up there's no ground to make it later than that. So I think we're talking about no later than Wednesday to get it --

MS. HOWELL: Close of business Wednesday.

MR. FITZGERALD: -- to get it to General Counsel to enable it to go out on Friday.

DR. ULSH: Can I make a request? When you send it to General Counsel, also send it to me.

MR. FITZGERALD: We've been trying to do that.

DR. MAKHIJANI (by Telephone): But some pieces of paper you already have that are the two -- I don't know if you have them, Emily -- but certainly NIOSH, OCAS and the working group has them because Brant and I sent them out, two pieces of the minutes of the conference calls with Roger. And also, I understand three spreadsheets, Joe?

MR. FITZGERALD: Yeah, listen, we don't have to solve all this.

DR. MAKHIJANI (by Telephone): Okay.

MR. FITZGERALD: But certainly, as we did with the main draft of the report, we will make available as many pieces of that report in advance to General Counsel and to Brant as possible. And that way you'll have a head start on this. By the time you get the finished product on Wednesday next week, you would have seen maybe half of it hopefully.

DR. WADE: It's a plan.

MR. GRIFFON: Yes.

MR. ELLIOTT: Could I ask a question for clarification? Joe, in your delivery of the executive summary what I did not hear were specific words that go to whether one of these five issues or all five of these issues are SEC related or are they a mix of site profile issues and SEC issues? Can you clarify that for me?

MR. FITZGERALD: Yes, these would have what we believe would be SEC implications from the standpoint in some cases of, not so much whether the approach seems to have sufficient data, which is normally one test of something with SEC significance, but it comes down to

demonstrating that, in fact, an upper bound can be established with the data.

And that's a little bit of a variant in that we think the approach seems to be adequate. We do believe that in general there's enough data, but we have not crossed the T to actually see a demonstration. Or if there was a demonstration, and this gets to the NUREG-1400, we feel the results were equivocal, meaning that there were some sense that some of the calculations were not bounding.

So we wanted to be very complete in terms of the issues where even though we felt the approach was adequate, we felt that in general there was enough data, we were still concerned that we never got to the point of seeing an upper bound demonstrated which would be, I think, the final step in that.

And there's two or three instances in that where we felt that was the case. And I think we've kind of indicated that in that language in the report that certainly that was the context of providing that.

MR. ELLIOTT: Thank you.

DR. WADE: For the record the judgment relative to the Board's vote will be the Board's judgment as to whether these are issues, and then it will go on to the NIOSH Director and the Secretary. Those are where the judgments really need to be made.

NEUTRON ISSUES

MR. GRIFFON: Okay, and I think we also prioritized these items as we often do, frontloaded the, at least what I think have higher SEC implications on the front of this agenda, so with that in mind I think we should go right into the neutron question. Again, I'm not sure if I can describe in enough detail, I didn't write enough detailed notes out here, but we had, as Joe indicated, we had some outstanding neutron questions that were action items for several months, probably six or seven months out. And NIOSH had been working on these issues, and I think it was probably what, three weeks ago, four weeks ago?

MR. FITZGERALD: About four weeks ago.

MR. GRIFFON: Four weeks ago when the data was provided on some of this. One thing I'm

remembering is an Excel spreadsheet which examined the question on the N/P ratios that's some paired data that Ron had requested to do some of his follow up, and that was provided. And that led to some further questions of, on the N/P ratios themselves but also then we realized that the data provided was on claimant data.

So we asked, and Brant provided the NDRP database on the O drive, and we actually, it raised more questions on the actual NDRP database. And I guess that's what led to these two technical calls. So just with that backdrop I think I'd ask Joe to sort of frame, there's a lot of, for those of us who were on the technical calls, there's a lot of details and twists and turns in understanding the NDRP project. Just when I think I have a handle on it I found out something new that I didn't necessarily know what was going on.

So the background was very useful for us to understand, but for the sake of this work group though I think it would be useful if, Joe, you can sort of frame preliminary conclusions and sort of a brief description of

your basis for those. Of if you want to do it or if Arjun's going to do it, but rather than get into, we got into very specific discussions on everything from quality control of the NDRP Project to some very specific issues within the NDRP Project, I'd rather at least give us sort of the big picture right now. And then as we need to go into some of the details we can, but I think that'd be a way to start this off.

MR. FITZGERALD: Yeah, and Arjun Makhijani and Ron Buchanan, Ron's here and Arjun's on the phone, I think have been the two principals in revealing the NDRP database, looking at this particular issue. And I'd like to defer to, I guess, Arjun.

If you can just give that overview, Arjun, as opposed, at first, as opposed to maybe getting into maybe the nitty gritty at this point, but certainly just to put things in perspective.

DR. MAKHIJANI (by Telephone): Sure. Basically, in looking at the NDRP, we're looking at -- and Roger, you can jump in and, you know, put a nuance on the facts if I'm not

getting them exactly right. We're looking at the workers who are considered at risk of plutonium exposure in the '52 to '70 period.

And because not everyone was monitored or those who were monitored had some gaps in the different periods, those gaps were filled by certain methods that were developed in the NDRP and then an estimate of neutron dose was made for each individual worker which is in their dose file. And when there's a claim, then that's the thing that is used.

In understanding the NDRP we've split it up into three broad periods: '52 to '58, '59 to '66, and '67 to '70. There are some kind of sub-periods within those, but '52 to '58 is when there wasn't very much monitoring. Up to '56, Los Alamos was doing it. They had these glass track badges. They issued 20 per badge cycle, and all of them were issued in one building, Building 91. And Building 71 where there was also plutonium work was not monitored at the time. So everybody has kind of a calculated dose in Building 71 in the '52 to '58 period.

'Fifty-seven and '58 Los Alamos was no

longer doing the issuance of the badges and the reading. There was a private contractor and those badges were not mostly recovered for re-reading. Part of what the NDRP did was to re-read, recover as many of the original dosimetry records' badges and re-read them. And up to '66 at least the original readings were generally found to be systematic underestimates. And then those underestimates were sought to be corrected in the NDRP.

And unfortunately, for '52 to '58 there's not very much data so the vast majority of workers who were at risk of plutonium exposure have assigned doses, and they are not assigned basically from the data of the time because the data of the time are very sparse. They're back extrapolated from neutron/photon ratios from 1959. And so there are a number of problems with that.

First of all there's an assumption that you can multiply a gamma dose by a neutron to photon ratio from a ^ back extrapolate that. There are two assumptions in that. One is that neutron dose is proportional to gamma dose, relating to a

building. And the second assumption is that essentially the working conditions in relation to both the neutrons and photons were about the same for workers in buildings even though you're back extrapolating.

And when we looked at it, we found that in the period when you looked at the doses -- and also we were told by NIOSH that the highest, the workers at highest risk of exposure or judged to be at highest risk of exposure were badged in the '52 to '58 period.

So we did a check of various of these conclude ^ NIOSH, and we couldn't verify many of them. For instance, the assigned doses which are the calculated doses, not the measured doses, were often bigger than the measured doses which were supposed to be of workers most at risk in some years. And in some years it was not the case. So in '53 the top ten exposed workers to neutrons were all, all had measured doses so the NIOSH assertion is clearly correct for that year.

But in '55 and '56 the top ten, and more than ten actually, workers were in Building 71 where there were no measurements,

and also they were all assigned doses of calculated doses which were calculated as best estimates with some claimant favorable factors. So it's not at all clear that the most exposed people were badged. So there's no way to actually validate what is being done with these assigned doses and back extrapolation.

Also, it appears that when you do scatter plots of either the original doses or re-read doses, it doesn't appear that in the measured doses there is a very good correlation between gamma and neutron doses in the period. We haven't yet finished the statistical work. This is just a kind of visual inspection.

So there are actually a number of issues with respect to that. ^ that arose is, can the calculated doses be validated in some way to be bounding or to relate to the working conditions by, say, area measurements of neutron dose or similar data from the period.

And we were informed by Roger that the NDRP tried to find such data to validate the calculations for the '52 to '58 period but

could not. And we also have not been able to find it. So we have a lot of concerns about this. There's also finally some indications that job types to affect neutron/photon ratios, ^ some locations where gamma doses are high and neutrons are low and vice versa, you can have neutron doses but zero gamma dose.

And there is not really a serious analysis of job types and neutron to photon ratios. Although in a pilot study that was analyzed in a master's thesis, so the only real reference on neutron to photon ratios in the NDRP, such an analysis was recommended. So for the '52 to '58 period it seems difficult to characterize the calculated NDRP doses as bounding doses based on the available information.

MR. GRIFFON: Arjun, do you want to go through each time period and then we'll take questions or do you want to stop at this point?

DR. MAKHIJANI (by Telephone): Whatever your preference is.

And finally then also, you know, the NDRP report does say that the notional doses,

these doses that are calculated, they're calculated in two ways, but the doses that are calculated from neutron to photon ratios are, quote, somewhat speculative. And we discussed this with Roger on one of the calls, the call actually day before yesterday, and you have his statement in there, and I might just read it so I'm not mischaracterizing it in any way.

He said that "I agree that notional doses are basically estimates and they are a best shot at calculating the dose. They are more speculative the farther back we extrapolate. So the 1952 to '58 period would be the more speculative part. As you get more and more film in the 1960s, the second method of using the average neutron dose as the estimator of the notional dose becomes more important. Then notional dose becomes less and less speculative.

And we would agree with that, and so the '52 to '58 period really has, relatively speaking at least, the most speculative components in it and also not well characterized by the data. I could go on to the other periods if you like, Mark.

MR. GRIFFON: Just briefly, describe the other periods and then let's go back. I think '52 through '58 will be a long discussion, but I think just the sense that the issues change in the different time periods.

DR. MAKHIJANI (by Telephone): Yeah, they do change.

MR. BUCHANAN: Could I make a clarification before we go on? Arjun stated that you could have neutron dose and no gamma dose, and I want to clarify that. That means that the gamma dose could be low, below the detectable limit, and in the database that we looked at, here is zero. That was changed to one so you wouldn't have to divide by zero, and then your neutron dose would be some value. And so what to clarify that the gamma would be below the detectable limit and the neutron would be above.

DR. MAKHIJANI (by Telephone): Thank you, Ron, yes. Zero means below the LOD.

MR. GRIFFON: Okay, go ahead on to the next time period, Arjun.

DR. MAKHIJANI (by Telephone): The next time period is useful to consider also in two sub-

periods: ’59 to ’64 and ’65-’66. And it’s clear in looking at the data that more and more people were badged. And starting from ’59 there were people who were badged, well, in this period there were people who were badged basically by the end of the period in all the plutonium buildings.

^ and by the end of the period it’s very clear to see that the workers at highest risk were the ones who were being badged so you can see this very clearly in 1966 if I remember right. ^ look at whether the assigned doses are higher or the measured doses are higher. The measured doses are consistently higher.

That’s not the case in the early period. Also, in the early period not all the badges were recovered for re-reading, and so you have a kind of peculiar problem wherein this period you had considerable systematic underestimates of dose in the original readings. I’ll give you an idea. These are not QA’d so they may change, but the re-read dose could be several times or an order of magnitude bigger than the original dose.

Roger, am I characterizing that correctly?

MR. FALK (by Telephone): Arjun, based on the conflict of the interest rules, I'm not able to really participate in this --

MR. ELLIOTT: No, you can answer the question. He's asking you a direct question, and you can answer it.

DR. MAKHIJANI (by Telephone): Yeah, just asking for your recollections so I'm not mischaracterizing it for the working group here. The re-read doses in the '59 to '66 period were often many times greater than the original readings.

MR. FALK (by Telephone): Yes.

DR. MAKHIJANI (by Telephone): Up to an order of, maybe an order of magnitude bigger?

MR. FALK (by Telephone): That is a possibility.

DR. MAKHIJANI (by Telephone): So in this period when we looked at the data, we found that quite a lot of more systematic underestimates, say, than in the Los Alamos period where there were also underestimates, or in the later 1960s period where they had

gone back and tried to read the badges more carefully.

One of the problems here is that not all of the badges were recovered. And so there was a portion of the original readings that could not be re-read. And the NDRP decided that they could not correct a badge reading that they could not find. So they simply added the original badge reading which is very likely to be a systematic underestimate to the re-read dose.

So now you have a final neutron dose that has three components: an assigned dose which is calculated to fill the gaps when there was no monitoring, a re-read dose from the badges that were recovered and re-read, and then a component from the badges that were not recovered and not re-read which is as best as we know known to be in substantial error and statistically speaking very, very likely to be in substantial error and an underestimate.

So now we’ve got a final dose estimate that is very difficult to characterize as a bounding dose in those cases where the badges

of the workers were not all recovered. Now that particular problem varies from year to year. In some years almost all the badges seem to have been recovered. In other years there were maybe 15 percent of the original reading could not be re-read because for some reason the badges were not available. And so there are significant numbers of workers whose badges could not be recovered.

Also, not everyone was monitored, and we did check in this period whether the most at risk were monitored, and we found that up to 1964 many of the workers who had completely assigned doses from neutron to photon ratios, a hundred percent, were among those who had the highest doses. So we could not verify that the highest risk workers were actually being monitored in this period, and in fact, there's evidence to the contrary.

That's not the case for '65 and '66 if I remember correctly. And all of this analysis is in process, and I'm giving you the best status based on what we've done so far. We also tried to check whether the NDRP notional dose is a best estimate compared to

the measured dose.

So we took one randomly selected worker from Building 71 and one from Building 91 for each year from ’59 to ’64, and we pretended that they didn’t have a neutron measurement even though they did, and calculated it by the NDRP method and then compared it to the measured dose. And for Building 71 it was less than the measured dose in all cases. In two cases it was in the 90 percent. But in four out of six cases it was substantially less than the measured dose. And Building 91 --

MR. GRIFFON: Arjun, one second, is this Table 2 that you --

DR. MAKHIJANI (by Telephone): No, it’s in the --

MR. GRIFFON: In Ron’s spreadsheets though, didn’t --

DR. MAKHIJANI (by Telephone): In Ron’s spreadsheets I don’t know.

Ron, will you say which spreadsheet it is from because I’m not looking from your spreadsheet.

MR. BUCHANAN: I think it’s four, but that’

not --

DR. MAKHIJANI (by Telephone): Yeah, it hasn't been published yet because this hasn't been QA'd yet. This thing needs a check, and so I'm giving you the numbers that we have so far that could be changed, but I'm giving you the best judgment.

MR. GRIFFON: Just to step back from that for a second, Arjun, because there was some shaking heads on this one so I just want to clarify. I think you took one individual from each year from each building, 71 and 91, right?

DR. MAKHIJANI (by Telephone): That's right, so there are ten individuals.

MR. GRIFFON: And where they had a period of measured dose, they summed the measured dose, and then they basically said, okay, let's pretend that this person wasn't monitored. And according to Table 11.1, is it?

DR. MAKHIJANI (by Telephone): Yes.

MR. GRIFFON: You would use the photon dose to use that factor to calculate a notional dose and said, okay, how does a notional dose compare for that time period to the actual

measured dose for that individual and see if it was, in fact, a bounding approach, right? Is that --

DR. MAKHIJANI (by Telephone): Right, and/or a best estimate approach. In a best estimate approach it should be comparable because the bounding can then be calculated by applying a variant.

The question that is being asked here in this analysis, is there some factor that could be creating systematic underestimates of neutron dose in the NDRP notional dose assignments as they are calculated from neutron to photon ratios? And the answer indicated in this analysis, and we chose workers who had at least six months of neutron monitoring, who had six months and 12 months of neutron monitoring, for this.

And we found that in 10 of 12 cases that the answer was less than the measured dose. The notional dose was less than the measured dose -- four, five, six, seven cases it was considerably less than the measured dose. That is more than ten percent less than the measured dose. The lowest value was 22

percent of the measured dose. So this test of the adequacy of the neutron notional dose to be comparable to the measured dose was not successful.

The other problem that we found was in the validation of the re-reading which becomes more important in this period. It was found that the people who were re-reading the badges were tending to under read the actual dose and correction factors were developed for each individual reader because it tended to vary by reader and their experience.

And the correction factors were essentially developed assuming that one reader, who was Roger Falk, had, as he described, was the gold standard, that he had the perfect reading. Now his reading was never independently that statistically very difficult, as independent of the quality of his reading. Everybody's going to have some error, and those errors were not characterized.

And so all of the re-reading was calibrated against one person's, a one person reading. Now there was some validation

exercise which was done against badges that Roger, himself, has had calibrated.

MR. HOLEN (by Telephone): This is Bill Holen with Congressman Perlmutter's office. ^ any comparative analysis of the difference between the contractor's badge reading and those that were done by the government in the early '50s?

DR. MAKHIJANI (by Telephone): Well, the NDRP, itself, was that comparative analysis. The government's readings, especially up to 1966 were found to be systematic underestimations. And the NDRP, and that was found to be so at the time as you'll see from the minutes of the 17th April conference call is one or 12th April.

One of the reasons that a review of neutron doses was undertaken in 1967 was that -- by Roger -- was that the earlier dose readings were found to be not of adequate quality, and so they decided to read the ones of those considered at highest risk more carefully so that they would cut down on the errors. And they did succeed in that.

They didn't eliminate all the errors,

but the percentage error markedly decreased in 1967 to ’70. And so that goal was mostly but not fully accomplished. So the government’s readings were not sound from the time so that’s why the badges were re-read.

MR. HOLEN (by Telephone): Thank you.

MR. GRIFFON: Arjun, can you give us a brief description of that last time period then? Are you --

DR. MAKHIJANI (by Telephone): Yeah, the last time period we actually haven’t finished all of our looking at all the data so there’s quite a bit of work remaining to do. And part of the reason that there is so much work remaining to do is that we did not understand until the last few days that there had been significant changes in the neutron dosimetry program in 1967 in that fewer badges were read, but they were read more carefully. And so we have to take a look at that period.

We did verify that they were read more carefully. Almost all the badges were recovered in ’67, but in ’69 and ’70 the badges were not archived, or at least some of the badges were not archived, and so not

available for re-reading. And so there's quite a few gaps in terms of the re-reading. At least in 1970 only 48 percent of the original dose was re-read. And in 1969 82 percent was re-read.

Now the significance of those gaps may be less in this period because of the quality, improved quality, of the readings since the indicated errors in the cumulative dose are lower. And also in this period, I think from the mid-'60s -- and we haven't been able to pin down the date yet -- but from the mid-'60s most of the gaps are not filled by the neutron to photon ratio method, but they're filled from looking at the worker's own nearby dose in the badged periods which is a more reliable method of filling gap and more normal certainly corresponding to other practices of filling gaps in a worker's dose.

MS. MUNN: Arjun, this is Wanda. I apologize for not having been able to be on that phone call on the 19th. Perhaps this would be more -- I mean on the 17th -- I might be more clear. But I'm a little confused about a couple of things that are being said,

and I'd appreciate some help clarifying it for me.

For one thing the question was asked, and I'd like to define terms a little bit here. The question was asked about the differences between badge readings from the contractor as opposed to by the government. Now to me the government incorporates all kinds of agencies and I guess I'd like to be very clear about what we're talking about when we say government readings as opposed to contractor readings.

And the other question that I have for you, which I think is a simple one, you talk about variances of as much as an order of magnitude difference. When we talk about an order of magnitude difference, are we talking about the difference between 20 millirem and 200 millirem or are we talking about the difference between 200 millirem and 2,000 millirem?

DR. MAKHIJANI (by Telephone): Well, let me take the second question first because it is simpler. The cases would vary by worker and by reading. So sometimes you'd be in the 40

to 400 millirem or 40 to 100 millirem, and sometimes you would be from a couple of hundred millirem into two millirem.

The doses do go up into the several rem range. The doses are in the thousands of millirem for the most exposed workers. So the re-readings do materially change the doses for many workers. For the most exposed workers certainly they put them into significant dose levels.

MS. MUNN: And since I have not seen the tables with which you've been working, the spreadsheets that I guess Ron put together, how many actual individuals, how many doses are we talking about per year? You spoke in terms of having looked at various years, but I'm trying to identify how many actual individuals were incorporated in those two, so far you've talked about two separate time periods.

DR. MAKHIJANI (by Telephone): Three actually.

MS. MUNN: Well, yeah, but primarily you talked about pretty much the '50s and pretty much the '60s.

DR. MAKHIJANI (by Telephone): Right.

MR. BUCHANAN: Hey, Arjun, do you want me to address that?

DR. MAKHIJANI (by Telephone): Sure, please.

MR. BUCHANAN: In the ’59 to ’64 era, we’re talking one to 2,000 annual doses. And in the ’65 to ’69 era we’re looking at just about 2,000 doses per year, workers. And if you looked at how much the original dose that was not re-read compares to the, if you was to adjust it, say, you went in and adjusted it like you did the other re-reads, some years it would not have much effect on the overall dose and some years it would increase it by 50 or 100 percent.

It would increase it some years, double it, if you went in and made the same adjustment to the non-re-read as you did to the re-read. So that gives you an order of magnitude of how many workers would be affected, a thousand to two thousand workers each year and the order of magnitude is from zero percent to about increasing it by 100 percent.

MS. MUNN: Do you have a rationale for why

there would be that much variance from one year to the next?

MR. BUCHANAN: It would depend on how many of them were found and re-read, and if those constituted the high dose workers or the low dose workers. If most of the high dose workers' badges were recovered and re-read, then you would have very little impact on the overall dose.

MS. MUNN: So you're talking about only reworks here in this particular discussion right now?

MR. BUCHANAN: We're comparing those that were re-read and those that could not be re-read and how the ones that couldn't be re-read would influence your overall dose assignments.

MS. MUNN: Right, got it. Thank you.

DR. MAKHIJANI (by Telephone): So in regard to the first question, you know, as I understand it all the dose readings were done by contractors, or essentially all, and part I was answering was comparing the original readings done by the government's contractors basically was running Rocky Flats in the '50s and '60s compared to the re-reading of the

same badges done in the neutron dose --

MS. MUNN: NDRP, okay.

DR. MAKHIJANI (by Telephone): -- reconstruction project which was also done by a contractor, which was done by ORAU actually.

MR. GRIFFON: I think for me looking at this data, and I was on these technical calls so I have a little advantage having looked at some of this data, but I'm still, you know, there are some details that are, we're all sort of sorting out. But I noticed, and Roger said this on one of the calls -- I forget if it was the 17th or last week -- but the year's not perfectly defined but '64, '65, somewhere in that time period, they started phasing in and more of the highly exposed workers were monitored in full.

And just looking at some of the data from the NDRP database, it does support that argument. But you can see instead of, I just did some plots on my own that look at the percentage of notional, which is basically an estimated dose. And when you have 100 percent notional, it was basically all, as Arjun's pointed out, assigned dose versus the final

neutron dose. And as you get out to like ’65, it’s clear that, like in ’55 I have up on the screen the top 40 or so final neutron doses as far as magnitude, the top 40 are all 100 percent notional dose, so they had no, either they didn’t find the film badges or they had no film badges, all 100 percent notional.

When you go out to ’65, it’s reversed. It’s all the higher end final doses actually have their own film data. And Roger indicated that this is kind of phased in. They took the highest risk workers, which makes sense, they took the highest risk workers and badged them, but it wasn’t all done in one year.

It was kind of phased in over a couple of years from ’64 so that their time is unclear. But the data does support that. And I would say in those cases you’re on more solid grounds with regard to being able to bound that dose because you have the actual film data from the high-end people, for the highest exposed. That was an important take away for me and the data sort of shows what Roger said was happening in the field, and they support each other.

But maybe, Arjun, is it a good point to maybe then go back to the first timeframe and --

DR. MAKHIJANI (by Telephone): Sure.

MR. GRIFFON: -- I'm sure there are several questions, so let's maybe turn it over to Brant and have some discussion.

DR. ULSH: Good morning everybody. Mark, I do have a lot to say about this. I don't know in terms of logistics when you want to take a break, but what I'm going to do is since certainly the people on the call and the people who were not involved directly in the conference calls may not have as good a picture of the NDRP as those of us who were, I'm going to start with an overview of NDRP, and that's going to take a little while.

MR. GRIFFON: I'm getting a nod from our court reporter that we should have a break. So let's take a ten-minute break and then reconvene.

DR. WADE: We're going to take a ten-minute break. We'll mute the phone, and we'll turn it back on in approximately ten minutes.

(Whereupon a break was taken from 10:47 a.m.

until 11:00 a.m.)

MR. GRIFFON: Okay, for everyone on the phone we're ready to start up again, and I think Brant's going to give us a little background on NDRP and get into some questions for Arjun I'm sure.

DR. WADE: Can we just verify for the record, Mike, are you still with us?

MR. GIBSON (by Telephone): Yeah, I'm still here, Lew.

DR. WADE: Thank you.

MR. GRIFFON: And everyone on the phone just let us know if you're not picking up Brant well or any of us. We'll make sure we adjust.

DR. ULSH: All right, thanks, Mark.

As I mentioned before the break I think it's worthwhile to take a step back here for those of you out there on the phone and for the people around the table here who were not directly involved in the conference calls that we had over the past couple of weeks and the process that we've been involved with for months now.

I'd like to start with just a little bit of background on how SC&A and NIOSH have

approached these neutron questions. And please don't read anything into what I'm about to say. I just think it's important to understand how we've been interacting and what data has been provided and when and what that all might mean.

We provided the neutron dose reconstruction protocol. It was months ago. I don't know the exact date, a long time ago though, and we also provided de-identified data months ago. So that stuff has been available to the working group and to SC&A for some months.

We had a process initiated where we've had several exchanges of questions and responses primarily with Ron Buchanan of SC&A and some folks on the ORAU side and that has been a very productive process I think. I hope, at least, that everyone involved with it would agree with that. And there've been numerous iterations, and questions would be presented; we'd answer them and then follow up questions would be presented.

We've also had numerous conference calls over the past several months. At least,

well, two within the past week I think, and at least two before that, and that's just going from my memory. It is true that we provided matched neutron gamma readings on the 14th of March to SC&A. And those were de-identified I believe. Yeah, I believe so, yeah.

Once SC&A had some time to spend with that it became clear that they still wanted to see the NDRP database, and I discussed that with Mark. And as Mark mentioned, we provided the complete NDRP database on the 23rd of March. So there's been an ongoing interaction. I mean, it hasn't all just happened in the past couple of weeks. It's not like these action items have been, there's not been a lot of items hanging out. There have been a couple that have been provided within the last few weeks.

Now for some background on the NDRP itself. This is a very big, a very complex study that was done so I think it's worthwhile to point out a couple of things. And some of this is going to be some tough hoeing, so I hope that you can just bear with me.

The NDRP was undertaken by the

Department of Energy because it was recognized that there were some problems with the neutron dosimetry as it was done at the time. And we're talking about the early '90s. And the impetus, at least as I understand it, was an epidemiological study that was done by Dr. Ruttenber from the Colorado Department of Health. And at that time it was recognized with input from former workers, I'm thinking of you know, the NDRP staff, that the weakest link in the dosimetry as it stood in the early '90s was the neutron dosimetry.

And so as Arjun mentioned, there was a study by a researcher at Colorado State, kind of an original study, and then a pilot study quickly followed just to demonstrate that, yes, this was a worthwhile thing to do. And then the NDRP was launched following on those.

Now the NDRP itself, and when I say NDRP, I'm talking about Neutron Dose Reconstruction Project, was conducted by ORAU. The scientists involved were former workers. Roger Falk was a primary scientist. Joe Aldrich was the project director. It did have oversight by not only by DOE individuals, Bob

Bistline and Bruce Wallen, but it also was overseen by an advisory board similar to the way our program is overseen by an advisory board. And the advisory board for the NDRP included a number of very knowledgeable, pre-eminent scientists in the field.

Now in terms of the history of neutron monitoring at Rocky, you've heard some of this from Arjun's description, but I'm going to cover it in maybe a little more detail. From 1952, that was the start of operations, through 1956, workers who were judged at the time by the health physicists to be at highest risk of neutron exposure were issued neutron track plates. And those were supplied by Los Alamos. A total of 757 neutron track plates were retrieved from Los Alamos for the NDRP. So that covers the '52 to '56 time period.

Now you might be a little confused because I said -- and I very carefully worded what I said -- the health physicists at the time judged that the people who were issued plates were the ones at highest risk. In retrospect it became obvious that not all of the people at significant risk of neutron

exposure were monitored.

So I think it's fair to say that their judgment at the time, while it was based on understandable reasons which we can't really discuss at the moment, it was in error. There were people in Building 71 who were getting significant neutron exposures and who were not issued plates. It is a fact that those people in Building 71 were not monitored from '52 to '56.

The people who were monitored were in primarily Building 91. That is where plutonium was received from offsite and where final assembly was performed I believe. And during that time like I said, Building 76, we have to be careful when we say that they were not monitored. People in Building 76 were monitored for beta and gamma. They were not monitored for neutron. So it's not really accurate to say that they were not monitored.

Now from 1957 to '70, through 1970, workers were monitored with NTA film. And this is where I think a little perspective is in order about the magnitude of the NDRP. The NDRP retrieved almost 90,000 films for the

NDRP. And of those 87,000, a little over 87,000, were matched to workers.

So that's the population of films that was looked at. So I think that's a little bit of perspective on the magnitude. We're not talking 20 films or 100 films. We're talking almost 90,000 films involved.

Now for in terms of the NDRP methodology and hang with me on this. This is a very complicated topic, that I'm still grappling with, too. The NDRP re-evaluated over 76,000 matched neutron-gamma pairs. Now let me tell you what I mean by that. A matched pair consists of a gamma measurement and a corresponding neutron measurement. So that is a matched pair, and there were 76,000 approximately of that.

And now the NDRP re-evaluated all the films that were available, and here's an important fact. They didn't just go back and re-read the films. They went back and re-read the films in most cases several times because from a statistical standpoint that will give you a better estimate. If I take a film and I read it once, well, that's a number, but if I

read it four or five times, then I have a much more statistically robust estimate from that film.

So you multiply that 90,000 by I don't know. I would just be guessing what the average number of re-reads is. Let's say three, you get an even larger scale project. So this was not a trivial project. It was very rigorous in terms of the QA that was applied.

Arjun mentioned, and I'll get to it at some point here, there were individual specific calibration factors. So each individual person who was re-reading films had a calibration factor that was specific to that person, and that was to eliminate errors from differences between readers. Looking around the table I might read a film differently than Wanda would read a film. And the NDRP didn't want that to have an impact on the estimate of the dose.

So they were, and the individual specific calibration factor's applied to each reader. And it's a little, I think the description that was given before was a little

bit incomplete. They were compared against the senior scientist readings, Roger Falk, but they were also compared to two sets of calibration films with known doses, and Roger's readings were also compared to calibration films with known doses.

So Roger was simply the normalizer. It wasn't as if these, if there was a disagreement, Roger was right and the reader was wrong. They were both compared to these calibration films.

And there were two sets. One set was exposed to a bare, unmoderated plutonium fluoride source. That's the source of the plutonium at Rocky Flats. That's the source term there. So it's the same type of plutonium. That was one set of films, and there were, I think, maybe four different doses that were evaluated.

And then a second configuration was used. That was completely moderated plutonium fluoride source. So you had plutonium fluoride source encased in seven centimeters of polyethylene, completely moderated to spectrum, and a second set of calibration

films was used. And these are the films that the readers' calibration factors were calculated against.

MR. GRIFFON: These were Rocky Flats calibration films or --

DR. ULSH: Yes, they were.

MR. GRIFFON: -- were they developed outside of --

DR. ULSH: I'm trying to recall, Mark. These were films that Roger Falk did in the 1960s. I don't know the exact year. I'm not sure if this was the Los Alamos source. I think it was. Yes, I think it was the Los Alamos --

MR. GRIFFON: So Roger Falk set up the films and exposed them and made these calibrations?

DR. ULSH: Yes, exactly. I think 1960-something. I don't know the exact year.

Now prior to the, now this really astounded me, prior to reading any films on any given day, readers were required to read films and pass an initial qualification test every day that they were reading, every day. And a separate Quality Control Program was implemented where they had to re-read at least

ten percent of the films that were read the previous day by each reader just to make sure that they were being consistent. The point that I'm making is not the details here, but the point that I'm making is there were rigorous QA protocols in place at the time.

Now, we've talked a lot about notional doses, and I know this might be a bit confusing. If you think about the total dose that would be calculated for an individual, if you can think in terms of an equation. Total dose equals the measured dose as re-evaluated by NDRP plus the notional dose. And I'm going to talk about each of those terms in a minute.

Now what is a notional dose? Well, notional doses are neutron doses that were assigned to workers who may have been exposed to neutrons in a plutonium building at Rocky Flats, but for one reason or another they were not monitored. I'm sorry. There is no monitoring data for that person. Now that could have happened because the person was not monitored. It could have happened because he was monitored, but the doses couldn't be re-evaluated from the film. And it could also be

the case that the worker was not likely to have been exposed during that period of time.

Now everything I'm saying comes from the NDRP protocol. This is not a late breaking development. All of this information is available in the NDRP. So a notional dose covers periods for when there is no neutron monitoring data for whatever reason for a particular individual. And you add that to the time when there was monitoring, and you come up with a total re-evaluated neutron dose.

So let's talk a little bit more about that notional dose term. Well, you start with the recorded gamma doses. And this is an important point. It is not true to say that every person who was in a plutonium building at Rocky Flats was included in the NDRP. The policy in place at the time was that if a worker was judged to have an exposure potential greater than ten percent of the limit, then they were required to be monitored. If it was less than that, they were not required to be monitored.

So there were people who were judged

not to have significant exposure potential who were not monitored for gamma. Those people aren't in the NDRP. The trigger for getting into the NDRP was that you were gamma monitored, and that is a surrogate for having significant exposure potential.

So let's start with the gamma. We've got a person, let's say he's monitored for gamma, but he's not monitored for neutrons. The whole focus of the NDRP, well, that's too strong a statement. One of the big focuses of the NDRP was to come up with a method to calculate notional neutron dose that, well, the best method. And simulation studies were performed to come up with the best method of calculating notional dose.

Now these studies as part of the NDRP compared the predicted neutron dose, the predictions from various alternative methods -- and these are all described in the NDRP -- and they compared them to actual measured doses for matched pairs.

So you've heard a little bit about SC&A trying to do some of these comparisons. There was a rigorous effort in the NDRP itself

to do exactly these kind of comparisons and find out which methods yielded predictions with the smallest error. So that was, the details are in the NDRP. I don't want to get too far down in the weeds any further than I already am.

Now let me tell you about notional dose, the method that was finally determined to be the superior method to do it. It was a weighted combination. Now this is where it might get a little difficult. It's a weighted combination of estimates that are determined from two methods.

The first method is based on a worker's average neutron dose per day, and that is attained from actual neutron measurements. The second method is an estimate based on a common neutron to gamma ratio for a particular building and for a particular year.

So let's say I was a person in the NDRP, and let's say I was monitored; I was measured for neutrons for four months out of the year. I don't know. I can't do math now. Let's make it six months out of the year. For

the other six months I was not monitored. My total neutron dose is going to be my measured neutron dose plus my notional.

And my notional is going to be a weighted average of the dose that is calculated for that gap period when I was not monitored by my average neutron dose per day or the common neutron to photon ratio. So some of the concerns that you've heard expressed here deal with using neutron to photon ratios to calculate notional doses, that's the part of the dose that we're talking about. And if you want additional details, I would always refer you to the NDRP protocol.

Now note that when a worker had only small gaps, in other words he was monitored most of the time, then his neutron dose estimate would be heavily weighted towards his actual measured neutron doses. And conversely, if a worker was only monitored a little bit of the time, his estimate would be heavily weighted towards the notional methodology.

So I mentioned that one of the big concerns that has been expressed or questions

that have been raised concern neutron to photon ratios. Well, how were these ratios calculated? Well, you started with a population of matched neutron and gamma doses. And I told you there were about 76,000 of those I believe. And the ratio was determined by dividing the sum of all the neutron doses for a particular building, for a particular year by the sum of the gamma doses for a particular building, for a particular year.

Now that method was evaluated as well to determine which gave acceptable results, stable results, and results that matched what was observed. And one of the big conclusions from those studies were the estimates that are based on neutron to gamma ratios are generally more accurate than the common neutron to gamma ratio was used rather than individual derived neutron to gamma ratios. Those were extremely variable over the course of the year. And I'm going to talk about why that makes perfect sense that they were variable.

So rigorous statistical studies were performed to determine which method of estimating the neutron to gamma ratios gave

stable and accurate results. And in the years where there were no matched neutron to gamma pairs, they were extrapolated from neighboring years. And I think this is also a source of concern. You heard Mark maybe or Arjun or somebody mention they had questions about extrapolating back from the ratio observed in ’59 back to earlier years.

And that is certainly true since there were very, there were relatively few neutron measurements in the early ‘50s. There wasn’t enough data to generate year and building-specific neutron to photon ratios for the ‘50s. And so what was done was the ratio that was observed in 1959, when we did have significant data available, that was back extrapolated to 1952.

So it’s always, you always have to be cautious when you’re making those kinds of extrapolations. That was one of the things that was hammered into me in school was you don’t extrapolate beyond the range of your data without having very good reasons to do so.

And so we considered this question,

the validity of extrapolating back from ’59, and there were a couple of questions that we evaluated. And this dealt with what factors could occur that would make 1959, the ratio in 1959, different from earlier years. What are the possibilities, and do they compromise our ability to make this extrapolation, or really the NDRP’s ability to do that.

Well, if there was a significant change in the source term you would expect that extrapolation might be questionable. In other words if you had plutonium fluoride in 1959, but earlier than that you had some other form of plutonium which generated a different neutron to photon ratio, well, obviously you wouldn’t want to back extrapolate.

However, the only change that occurred in the source term was an increase in the batch size. They started out with a batch size of 200 rems. They increased to 1,200 rems. I believe that occurred in 1957. Well, now obviously that increase in batch size would increase the amount of neutron radiation coming off. It would increase the amount of gamma radiation coming off, but it would not

be expected to change the ratio, and that's what we're talking about, the ratio.

So we didn't, and the form of the plutonium, plutonium fluoride, was constant across that time period. So we in our evaluation, we did not see that there was a factor related to the source term that would compromise that extrapolation. Well, there are a couple of other things that might crop up.

One is what if there were changes in the configurations of the buildings or in the neutron shielding that was involved that occurred prior to 1959. I mean, let's say you installed a bunch of shielding in 1958 that changes the neutron to photon ratio. Obviously, then you wouldn't want to extrapolate back from '59. That didn't occur.

If you look at the building histories that's available on the HAER. I think that stands for historical -- well, that's as much of the acronym as I can come up with, but that is certainly available on the internet if you do an HAER search. There were no major building configuration changes until the '60s.

They added significant neutron shielding in the ‘60s but not before ’59. So we don’t see any changes in terms of those kinds of factors which would compromise the ability to back extrapolate.

And that leaves you with one other factor that might affect the ability to back extrapolate, and that is if there were changes in the way workers did their jobs you might question the ability to back extrapolate. But the philosophy that was in place at the time in the ‘50s and well into the ‘60s was that a particular group of workers would be assigned to a batch of plutonium, and they would follow it through the process from start to finish. And that did not change over the course of our extrapolation. That was well into the ’60s.

Now in the ‘60s the philosophy did change. You had workers who did specific tasks and the batches passed them by, you might want to think of maybe in an assembly line fashion. But that didn’t happen until in the ‘60s. And we don’t know of any other factors that changed the way workers did their jobs.

So we did consider all of these questions when we evaluated the advisability of back extrapolating from ’59. And we just did not see anything that would compromise that extrapolation, to be honest, over the discussions over the past couple of weeks, I haven’t heard of anything. I haven’t heard a technical argument that says that that extrapolation would be invalid.

Now it is true, and I know, Mark, this is one of your big concerns, about notional doses, the highest doses in early years being notional rather than measured. And as I mentioned, the health physicists at the time made a judgment that people in Building 91 were at highest risk of exposure, and that’s why they got the plates.

As I said, in retrospect that was probably not in this individual, in this particular instance a good judgment because what you see is that the notional doses are higher in the earlier years. That is certainly true. And you could say, well, that, obviously, I think it’s fair to say that the health physicists at the time didn’t have

an accurate judgment about who should be monitored if you're trying to monitor the highest people.

But the relevant question is can we bound, or really the question is can we bound or come up with a more accurate methodology for calculating neutron doses. And the fact that the doses are notional, highest doses are notional does not in and of itself in any way compromise our ability to estimate those doses.

Now, I'm almost done. I had a feeling that the speculative language would become a sound bite, and I appreciate Arjun reading what Roger said about that. I think taken out of the scientific contexts, members of the public or others could be forgiven for reading more into that comment than is really appropriate. When you say speculative, I mean, scientists, health physicists, abbreviation protection people, always prefer directly measured doses over calculated doses even in a situation like this where the calculated doses were done very rigorously.

So, of course, it would be preferable

if the workers monitored in the ‘50s had been directly monitored for neutrons. And there is some degree of assumptions involved in calculating via notional dose methodology. And, of course, you have to evaluate those assumptions. I think they’ve been very thoroughly evaluated.

So with that I think I’ll finally be quiet and you ask, discuss questions or...

MR. GRIFFON: Joe or Arjun, I give it to you.

MR. FITZGERALD: I think Arjun’s chafing.

DR. MAKHIJANI (by Telephone): I’d just like to make one clarification about a point regarding a notional, what goes into the final dose. Brant said that the final dose has a two component equation. One is the re-read dose, and the other is a notional dose and ^ two components.

And I agree with all that, but there’s a third component which I mentioned. Doesn’t apply to all workers but is important for many workers, and it depends on the period and the year how many workers this applies to. But since not all the badges were recovered, the ^

re-read portion of the original dose.

So the original dose actually was split up into two pieces, the piece that could be re-read because the badge was available and in condition to be re-read and could be matched up with a specific worker, and the part that was not re-read. Now the part that was not re-read was simply added into the final dose without change.

And one of the essential methodological problems that we see is we know that the non-re-read portion ^ likely to be in significant error as an underestimate for the whole period but particularly for certain periods it would be a large underestimate most likely. So that piece of the total neutron dose is a problem for those workers who have that in their final dose.

DR. ULSH: Arjun, we agree with you that that particular piece of the total dose, in other words the piece of the original dose that was not able to be re-read could pose a problem, and that takes you up through the NDRP. Now the particular individual enters the NIOSH dose reconstruction process. And

we're very well aware of that problem so I think Mutty's going to have a few words to tell you how we address it in terms of a dose reconstruction.

MR. SHARFI: When we actually assessed the NDRP data, we didn't re-break it back up to any originally non-adjusted doses and the NDRP dose and the notional dose. The original doses then are the ones that have not been re-read are then readjusted for possible track errors.

And those are covered in the site profile and in a lot of cases are adjustment factors of up to a factor of two can be assigned to those original non-re-read doses to account for possible misreadings. And that's all covered in the site profile. So the original dose is actually pulled back out of the NDRP dose and re-analyzed for possible, and adjustment factors are given for that possible issue. And that is covered in the site profile and how to handle that and is addressed in the dose reconstruction.

DR. MAKHIJANI (by Telephone): The errors as we observed them for certain years as I

mentioned go up to an order of magnitude or more even in the cumulative dose and they are considerably bigger for individual workers. And a correction by a factor of two certainly would not in most or all years, except maybe ’67 to ’70, could be described as bounding in any way, at least as I’ve read the data.

DR. ULSH: Well, Arjun, I’m sorry, but I don’t have your analysis in my hands or maybe I do and I just haven’t had time to digest it yet. I’m not quite sure.

DR. MAKHIJANI (by Telephone): No, you don’t, Brant, because as I said, we’re still in the throes of producing this and as you understand, you don’t like to publish --

DR. ULSH: Oh, sure.

DR. MAKHIJANI (by Telephone): -- draft documents.

DR. ULSH: I would say that -- this is dangerous without seeing your actual data, but in my experience, limited experience, you tend to see a lot more variability in terms of the neutron to photon ratios when you’re very close to the limit of detection. And I’m looking at Mutty to give him an opportunity to

correct me. So I think you might see more of that kind of an issue at very low doses. But I would have to see the data that you're basing this on to say for sure.

DR. MAKHIJANI (by Telephone): Brant, I think you did not understand me. I'm not talking about neutron to photon ratio calculated portions of the notional dose. I'm just talking about the errors that were present and discovered, assuming that the re-reading, re-read dose was completely correct, and just accepting that, you try to estimate by how much the original reading was in error. And that error varied from one year to the next.

For instance, Los Alamos ^ seemed to do a fairly good job, in some of the time at least, and those errors are not very large in the '59 to '64 period. In the '66 period, the errors ^ some of the years, and I think as was discovered in the NDRP the individual reader errors were dependent on experience and at the time they were not corrected. So you tended to have a fluctuation in the errors of the original readings.

And what we're talking about is what can you do to correct the original readings, and as Roger mentioned and as is documented in the minutes, either of the one of the two calls in the last week, is that the ^ decided that they really couldn't do anything to correct the errors of the badges that they could not find. That's why they left them alone.

DR. ULSH: Yes, I think it's safe to say, Arjun, that that was done in the NDRP in an overabundance of caution because you have to realize that what they were trying to do here was recalculate, readjust a dose of record. So they were very conservative about messing with the original dose. I mean, if they had significant, if they had enough data to do it, they did it.

But in terms of what we do here, I mean, we might, you've given me some numbers. You said that it can be, I don't know, 100 percent or 50 percent. I think Ron put some numbers out there. So I mean, we might, I would encourage you to look at what we've done in the TBD if you haven't already and send

over what you guys have done, and we'll take a look at that.

But I think at the end of the day if it's just an argument about numbers, should it be a factor of two or a factor of ten or a factor of whatever, that's not necessarily an SEC issue. That is simply a TBD issue. What number should be applied? I can't go any farther than that without seeing your data.

MR. BUCHANAN: Could I ask for clarification? I believe in the TBD the factor of up to two is applied because of the NTA film's lack of response below the 700 keV threshold was the main reason for putting those in and building specific.

MR. SHARFI: That's a separate adjustment factor on top of that. There is an adjustment for the low energy less than 700 keV.

MR. BUCHANAN: Right, and that's around a factor of two.

MR. SHARFI: In addition to there's a -- it's in Section 6.7.3.4 which talks about the correction for neutron film reading deficiencies. And there is a separate issue for the low energy neutron energy range that

also gets applied on top of these original re-reads. So, yes, TA could do adjustments up to a factor of four to eight, depending on if they're falling into both categories certainly. I was describing just the deficiency part of it.

MR. GRIFFON: My feeling is this is news. I mean, I didn't know that there was this adjustment factor so I think we should, for that one issue I think it would be useful for SC&A to reflect on that in the TBD, and --

DR. ULSH: And send us your analysis so we can reflect on that.

MR. GRIFFON: Right, right, right, but that's --

MR. SHARFI: So a factor of ten ^ order of magnitude between recalculated and the original, unless you're talking about like go down in the ^ where you're talking about right near the LOD where I could see large --

When you get to the larger doses I don't know if I've seen factors of ten where the original dose was like 500 millirem, and I usually see five rem in re-read. But I could see that when the original dose was ten

millirem and now you see 100, you're getting down to the error of the dosimetry itself.

MR. BUCHANAN: Just for the records can you summarize briefly when the, what years and when the dose reconstructor would apply a --

MR. SHARFI: Anytime there was original, non-adjusted NDRP dose, then these factors then would be applied onto the original portion of the NDRP dose.

MR. BUCHANAN: For what years?

MR. GRIFFON: For all years.

MR. SHARFI: For all years.

MR. BUCHANAN: 'Fifty-two through '69?

MR. SHARFI: 'Sixty-nine, '70, yeah.

MR. BUCHANAN: But the neutron threshold was apparently corrected in the later years.

MR. SHARFI: Correct, that's for certain years. I have to go back to look up on that issue when those apply.

DR. ULSH: Because that's a separate correction.

MR. SHARFI: Yeah, that's a separate correction. There are two separate corrections.

DR. MAKHIJANI (by Telephone): Let me throw

some numbers into this mix that are from real readings. So you have an original reading of, in 1965, of 517 millirem, and a re-read value of 3,267 millirem with a variance of 204. Then one right below it there's one of 515 with ^. You know, one is about a factor of six and one is a factor of five for that same original reading both of which are over the LOD. You've got a hundred and odd millirem that turns into 1,000 millirem. You've got 274 that translates into 1,249. You've got 43 that become 58, so those are some examples of order of magnitude type of changes in non-trivial doses. And there's not just a question of what ^ apply and picking something that would cover because ^ methodology.

MR. GRIFFON: Arjun, are you on a speaker phone?

DR. MAKHIJANI (by Telephone): No, I have headphones.

MR. GRIFFON: Okay, because we're losing you. We had a better connection before and now you're crackling.

MS. MUNN: You're very staticky, and you're also very soft. We can't get the volume up

any higher.

DR. MAKHIJANI (by Telephone): I'm sorry. Maybe I will dial back in so I don't have headphones. I thought this would be easier because I can mute this, and I couldn't mute my other phone.

DR. WADE: Why don't you try dialing back in with a handset.

DR. MAKHIJANI (by Telephone): Okay, I'll just hang up and dial again. I'm sorry.

MR. GRIFFON: My sense is just that I wasn't aware.

I don't know if, Ron, if you were aware of it. I mean, I think we need to, we should look at that and the question of whether the factor's appropriate or not. I think you have to look at the factor first to determine that. So I think you need to look at that closer. But that's for the non-re-read. There's other issues that we need to --

DR. ULSH: Right, the non-re-read dose I think is a fairly small problem in terms of the number of people that it affects, the number of films.

MR. GRIFFON: Right, although there's a

couple of years when it gets pretty significant, but, yeah, overall that's, so that's related to that one issue.

MR. SHARFI: When he talks about a factor of four or five and I'm just talking about discrepancy, there is the other issue of the low energy photon I believe was corrected in the re-read that if you applied both of them, you might be looking in more of a factor total that will compensate --

DR. MAKHIJANI (by Telephone): Hello, I'm back I think.

MR. GRIFFON: That's much better.

MS. MUNN: That's so much better.

DR. MAKHIJANI (by Telephone): I'm sorry about that. I thought I was making it better, but I made it worse.

Anyway, I was ^ a magnitude ^ is a factor of five and seven changes in the mid-'60s in the re-read dose from a base that's non-trivial, that ^ rem. But the point I wanted to make is it's just not a question of calculating the ratios and picking a number if there's some scientific basis on which we can pick that number because these things vary

from one year to the next.

And I think that is a problem because I think the main source of this error, at least if the NDRP record I understand correctly, was individual reading errors and the experience of the individual reader. And at this time we have essentially no information about who was reading these badges originally, and what their errors might have been in the badges that are missing.

DR. ULSH: I just don't think we're able to say much more, Arjun, without getting your data and taking a look at it.

DR. MAKHIJANI (by Telephone): Yeah, I mean, I was reading directly from the O drive data.

MR. GRIFFON: I think, Mutty, you might want to repeat because Arjun didn't hear the one statement about the --

MR. SHARFI: The re-read I think also accounts for the low energy neutron that wasn't a capture, so when you're comparing the factors, I think you have to take both factors. I believe the low energy neutron's a factor of 2.5, and I guess that sort of combined together you could see, we could end

up multiplying a factor of five to the original neutron dose.

DR. MAKHIJANI (by Telephone): But then you should see a consistent ^ for the low energy neutron adjustment, but you don't see that.

MS. MUNN: A consistent what? You broke up.

DR. MAKHIJANI (by Telephone): If there's a piece of the adjustment factor that relates to the low energy neutron adjustment, then you should see a consistent factor of, say, two that is there at least in all the re-read doses, but you don't see that. You'll see re-read doses that have smaller corrections.

MR. SHARFI: Correct, because you have to consider that we take a factor 2.5 because that's the most conservative underestimate based off the percentages of neutrons you'd expect under 800 keV. In some cases you would expect a much lower correction factor if you went building by building and type of material specific which the NDRP project probably went into a more detailed scenario of where that person was working in the case like a plutonium ingot versus molten salt or something like that. That means you're going

to see different low energy neutron composition.

What we did is we took the worst case scenario and then applied the, which would have been the 2.5. In most cases we're attempting to put you at that upper bound instead of trying to find possibly a lower correction factor.

DR. MAKHIJANI (by Telephone): I think we would need Roger's help here. I do ^ NDRP did a job type analysis in the re-evaluation.

Roger?

MR. FALK (by Telephone): That is right.

MR. SHARFI: It's by building.

MR. GRIFFON: By building, right? Yeah, you're --

MR. SHARFI: Seventy-one out of ^ versus 707 had different lines. So based off the building --

MR. GRIFFON: Right, it's building not job type. You're right. You're right, Arjun. But I think at this point I don't know that we can take this much farther without just asking SC&A to address the factors that you discussed in 6.7.3.4 and see where you come out on that.

I mean --

DR. ULSH: And we've got to see what SC&A's going to produce, quickly.

MR. GRIFFON: Right, it's got to be quick.

DR. ULSH: It's a very technical issue, and we're going to need some time to respond to it.

MR. GRIFFON: I know, but I think they have to look at the numbers and how they're applied. I mean, doing a real-time out loud here, I'm not sure we're going to get much further.

DR. MAKHIJANI (by Telephone): And if I might ask Ron to give me a buzz on my cell phone at lunch we can make some progress because I need to talk about numbers with him. Sorry to say this on the record.

MR. GRIFFON: So then -- go ahead.

DR. ULSH: We talked about two issues. We talked about what to do about badges that were not re-read, and then I've given you a response on neutron to photon ratio. Do you want to talk about that some more?

MR. GRIFFON: Yeah, Arjun, the question of neutron to photon back extrapolation of the

ratio?

DR. MAKHIJANI (by Telephone): Yes. There are a lot different issues with that. First of all the underlying assumption is that you can calculate an average building ratio and apply it to an individual. And the second underlying assumption is that neutron doses are proportional to photon ratios; that's why you use a constant factor.

If you look at the evaluation of different methods of neutron to photon ratio calculation that could be done with the same data which is there in the Stanfield thesis which was done using the pilot ^ provided to Stanfield by Rocky Flats, you find that depending on how you calculate the neutron to photon ratio, you can get an order of magnitude difference in the ratio.

And there's no really clean way of aggregating this data. I'm not talking about differences in individual paired ^ badge readings. I'm talking about aggregated data. So for instance, if you look at the scatter plots of neutron to photon ratios, either annually aggregated or in matched pairs,

you'll find there's very little correlation.

In the pilot study that was done by Rocky Flats and where Roger was involved ^ stratify, if you stratify the data by gamma dose and say less than 50 people who had less than ^ what would the neutron/photon ratio be. And more than 50 millirem but less than 100, more than 100, you get very different results.

If you look at scatter plots, you find that neutron doses are concentrated in a rather low band of gamma doses, but then there are also people with very low neutron doses who have quite high gamma doses, and then you've mixed all of those things up. And it appears to me that building N/P ratios are useful for calculating population doses but not individual doses. I think the underlying methodology is open to question even for the years in which it was applied.

And I think the terminology was not, scientists don't use the word speculative very easily. They use the word uncertainty when they can realistically, actually put a number on it, and that defines a kind of scientific representation of what you can say about a

particular number. In a scientific context when you say speculation, there's a ^ which you're throwing up your hands.

And if you read the NDRP report, it does say that the piece of the notional dose that is calculated based on the neutron to photon ratio is the more variable part. And the back extrapolation is considered as the more speculative part the farther back you go.

I think that's, in my opinion, when I look at the sum total of everything, I'm not saying it isn't the best shot or a very well thought through scientific approach, but you have, you're dealing with thin gruel, and you can't get more calories out of than are there.

MR. SMITH (by Telephone): This is Matthew Smith on the phone with the ORAU project. Just to add to that, the final that was on that conclusion regarding the uncertainty of notional dose in the NDRP report, it states, "To reflect this uncertainty methodological choices have been made at every stage of the analysis that will tend to overstate ^ the claimed variability of the estimates."

And it's important to note that we do

take that uncertainty that's reported by the NDRP data, and we work it through our calculations and into the IREP input that we use to calculate POC. And that's all I have to say.

DR. ULSH: I don't think it's going to be worthwhile to get into a debate of what the meaning of speculative is. I mean, you read what Roger said he meant by that, and that stands.

DR. MAKHIJANI (by Telephone): And when we ^ so that's why we've tried to do these quantitative exercises, as I say, you know, we will publish this as soon as we possibly can, but I'm sharing with you, you know, these un-QA'd numbers. And we did try to verify whether the calculated doses from N/P ratios are comparable to the measured dose, and in eight out of 12 cases we found that they were considerably short. In some cases they were 22, 39, 49 percent.

I mean, those are the lowest numbers, and that is well short of the measured dose. Now this isn't a statistically significant analysis, but they were randomly chosen

workers with significant monitoring data. So we're not able to verify that the actual application of N/P ratios calculated on a building basis actually works.

DR. ULSH: Well, I appreciate your sharing your preliminary results with us, Arjun. I mean, obviously, I can't comment on in detail because I haven't seen what you've done. But I can say though that the NDRP itself did exactly this kind of analysis with a statistically significant population, and they picked the method of calculating N/P ratios that agreed, had the smallest error when you compared to actual measured neutron doses. Beyond that I can't really say much without seeing your analysis.

DR. MAKHIJANI (by Telephone): I'll also not say what is in process in terms of our statistical evaluation. But during one of the calls I did raise a question about the whole statistical model that was adopted by the NDRP. Now I sent off my reading of that to our statistician, Harry Chmelynski.

DR. ULSH: I think you said he sent off his reading of this model to the statistician.

MS. MUNN: Some statistician.

DR. WADE: Arjun, we've lost you.

DR. MAKHIJANI (by Telephone): I'm very sorry. I have a landline and a handset, and I don't know what I'm doing wrong here.

DR. WADE: Well, you're probably doing nothing wrong so just --

DR. MAKHIJANI (by Telephone): I'll try to speak as loudly as I can. I'm sitting in a room by myself with a closed door. Is that better?

MS. MUNN: Your volume is fine.

DR. WADE: There is interference periodically. I don't think it's you so please persevere with us.

MR. BROEHM (by Telephone): And this is Jason. I'm hearing Arjun just fine on my end.

DR. MAKHIJANI (by Telephone): Yeah, I believe that the interference is not coming from my phone.

DR. WADE: I think you're right.

MS. MUNN: Probably here.

DR. WADE: Go ahead.

DR. MAKHIJANI (by Telephone): As I said I have a question about the statistical model

both in relation to the way the errors, expected values of the errors in the true value of the dose and the measured value of the dose and in the model for how the variance was calculated for the notional doses with using these N/P ratios, that piece of the notional dose. Now I have some experience in statistics, but I'm not an expert and so I have sent it off to our statistician, Harry, and he has not yet had a chance to get back to me about that.

MR. BUCHANAN: I would like to make one question or clarification. The verification, Brant, when you said that they did do some comparison, that was the master's thesis, and he did 71 and then a group that was in the late '60s and compared 50 workers or something. But that was the qualifying I'd like to put on it, right? Is that correct?

DR. ULSH: I'm not sure that I agree entirely, and I might need to be corrected. The way you described Stanfield's thesis is, I think, accurate. But I'm talking about Appendix 4 of the NDRP where Dr. Chapman's analyses are presented. And I'm talking about

the simulation studies where they compared various different methods for predicting the neutron. They took matched pairs. You had a gamma; you had a neutron. And they compared several different ways of predicting a neutron dose and compared it to the actual measured neutron dose and picked the one that had the smallest relative error. So I'm not talking about Stanfield's thesis on that.

MR. BUCHANAN: That was for later years though, right?

DR. ULSH: Oh, I don't really remember the details, Ron. I'm not sure.

MR. GRIFFON: Can I just, I think Matt Smith was on the phone and the point you made, Matt, I think was when you're doing the dose reconstructions, you do add that 95th percentile or you consider it? I'm not sure I understood exactly how that's used in IREP --

DR. ULSH: Matt, are you still out there?

MR. SMITH (by Telephone): Yeah, I'll pick up the handset. When you look at those NDRP sheets, you'll see that there's an error value associated with the total dose. That kind of error is carried forward ^ and then it

eventually ends up in IREP as well.

DR. MAKHIJANI (by Telephone): But you use a full distribution, not the 95 percentile value.

MR. SMITH (by Telephone): That's correct. For someone who has NDRP data. It's being applied as a distribution.

DR. MAKHIJANI (by Telephone): So actually when you apply it as a distribution, don't you wind up with some negative dose values because you used, you're using a normal distribution with a plus or minus that carries you into the negative dose range?

MR. SMITH (by Telephone): That would be a question regarding how IREP operates under the hood, and that is not my area of expertise.

DR. NETON: It's quite possible.

DR. ULSH: Well, I don't think that we, Arjun, we never really go in and explicitly calculate individually the neutron dose by year. We put the neutron dose by year into IREP with the distribution along with all of the other doses in the IREP input spreadsheet, and it runs through the calculation. And then we pick the 99th percentile credibility limit.

DR. MAKHIJANI (by Telephone): Yeah, I realize that. I know that that's how it's done. I'm just mentioning this as this is a direct consequence of the statistical model that was selected because when you do it that way it is going to take you into negative dose ranges in some cases.

DR. NETON: I'm not sure, Arjun, whether IREP would truncate that as zero. It may well.

DR. MAKHIJANI (by Telephone): I don't know.

DR. NETON: I know it does with the risk models. It won't allow the risk to go below zero, but I'm not sure about what it does with the dose calculations. But nonetheless, I mean, if the distribution's a distribution, I mean, it's the range of expected values. And I don't see anything wrong necessarily with a negative dose.

DR. MAKHIJANI (by Telephone): Some of the range of negative expected values are physically impossible.

DR. NETON: Not when you can do measurements and you subtract background, no. I mean, if you put a number of measurements and average

them, if you take out the negatives, you're biasing your values.

DR. MAKHIJANI (by Telephone): This is not about measurements. This is about a model that is leading you into considerable negative dose territory, not --

DR. NETON: But I think in reality it is possible to measure a negative dose because of statistical distribution. So that's consistent with reality.

DR. ULSH: Yeah, it would not be considerable. It would be way out at the tail.

MR. SHARFI: You'd have to be way out at the tail to get to the negative values. I mean, you're talking three, four standard deviations out, 99th percentile, 99.9 percentile.

DR. ULSH: But anyway, I don't know if it's --

MR. GRIFFON: No, I think we're getting beyond our discussion here. But the only thing I was going to -- in your dose reconstructions though, you would enter this dose as a normal distribution?

MR. SHARFI: Correct.

MR. GRIFFON: Yeah, okay, so you don't modify that in any way.

MR. SHARFI: Correct.

MR. GRIFFON: Arjun, do you have anything more on --

DR. MAKHIJANI (by Telephone): No, you know, I'd be happy to answer questions. I mean, I've given sort of as much of a review of our analysis as I could in some detail, but I'd be happy to answer questions and invite Ron to talk more about the data. I mean, I don't know. We just have to complete our work and share it with all of you as soon as possible.

MR. GRIFFON: The only thing I think you might want to speak to is the back extrapolating the N/P ratio from '59. Brant gave some arguments about how the process and source term would not have affected that. I think you've said some different things on some of the technical calls so I just wanted to, I think you should share that for the record.

DR. MAKHIJANI (by Telephone): Yeah, let me find my draft here.

DR. ULSH: While he's looking, Mark, we

should probably talk about the validation question that just occurred to me. We haven't talked about that.

MR. GRIFFON: Right.

DR. MAKHIJANI (by Telephone): Do you want to go ahead while I find my stuff here.

MR. GRIFFON: I guess there was a question that came up on the technical calls that we had about, and Arjun raised it in his description of the work we've done, that prior to '59, and I think Roger sort of supported this, that while the NDRP Project made extensive efforts to kind of try to validate the neutron/photon ratio based on field-type data, and Roger basically concluded that it wasn't available.

DR. ULSH: Yeah, this question has been raised by SC&A in the conference calls and maybe in the draft report. I've seen it somewhere. And that question has to do with what they call validating or benchmarking the data. And I'm going to rely on Ron and Arjun to correct me if I've got a misinterpretation of what you mean by that.

I saw it in the context of using field

measurements, neutron and gamma surveys in particular areas to compare to the NDRP. Is that kind of what you're thinking about, those kinds of comparisons?

MR. BUCHANAN: That's what we'd like, but we found out that wasn't available.

DR. MAKHIJANI (by Telephone): Well, there are two types of validation, you know, like what we're doing in the '60s is we have data from exposed workers that have neutron and photon measured data in the various buildings. And so we can actually do the kind of the comparison that we talked about is assume we don't have a neutron dose and estimate it using the method and compare it, the kind of validation that was done by the NDRP and by Stanfield and so on.

For '52 to '58 the only available method is to compare it to Building 91, measured neutron doses, because there's very little data, almost no data from Building 71. There's a little bit but almost none.

And since the job types were different and since the maximum exposed workers in many cases were in Building 71 best as we can tell,

there's no, in terms of estimated dose, there's no actual measurements ^ against which you can validate your model. So that leaves you with area neutron measurements and those also do not exist.

And since the building ratio is an average ratio, yes, you have some workers following a badge, but not all workers are doing the same thing. And the Stanfield thesis which looked at this question, even for '59 to '66 in that pilot study, found that there is a difference by job type. So that back extrapolating from a limited set of '59 data which is, which has some questions even for 1959.

So it's not as if we're taking a 1959 dataset that is complete and that we know has a certain amount of integrity in terms of job coverage and back extrapolating that. We're not, we don't have by job analysis in 1959. We have a limited amount of data. Most of the doses in 1959 that are high were calculated notional doses for workers who were not monitored, and we're taking the limited data available for paired data in that year and

back extrapolating it into a different time.

For workers whose exact jobs and how they followed, how much time they spent, for instance, in the early years, you would have workers with less experience. As they became more experienced, they would do different jobs differently, more efficiently. The amount of experience that they would get in different jobs would vary. So even if the process is the same, it's not at all guaranteed that when you've got a start up plant, that you're not going to have many workers who have difficulties or following through the batch or the processes may take longer.

So none of these things can actually be validated by any piece of measurement that we have, and that makes back extrapolation very iffy even leaving aside all the methodological questions like assuming a constant proportionality for a period of seven years between neutron and photon exposure for all workers.

DR. ULSH: Well, let me talk about 1959 first because you said that there wasn't much data there.

DR. MAKHIJANI (by Telephone): I said it was limited.

DR. ULSH: Okay, sorry. It was limited.

In 1969 in Building 71 --

MR. GRIFFON: 'Fifty-nine you mean?

DR. ULSH: 'Fifty-nine. I'm sorry. Thank you. Nineteen Fifty-nine in Building 71 I see 310 people counted for gamma and 160 counted for neutron. And in 91, I'm sorry, Building 91 in 1959 I see 216 people counted for gamma and 88 counted for neutron. So that is what it is. I'm not going to offer a qualitative judgment on this. That's just, those are the numbers.

Now, yet we did talk about, I mean, I talked about the job philosophy in terms of people following the batches through the process, all the way through the process and that did not change. Now could there be some changes because workers learned their job? Yeah, that's not quantifiable. I mean, that's true not only for the '50s, that's true for any time when a worker starts.

Those are the kinds of factors that I don't think you can really get a quantitative

estimate on. We don't do it anywhere else in the program. That's why we use the 99th percentile credibility limit to cover situations, those types of factors that are not quantifiable.

So, yeah, Arjun, I agree with you. I mean, you can't, there's no way to evaluate that, but that's not limited to this particular situation. That's true everywhere.

DR. MAKHIJANI (by Telephone): No, I disagree with that. I think it's not just any time period we're talking about. We're talking about the first time period when Rocky Flats went into operation so that you had workers who were doing industrial manufacturing processes for nuclear weapons on an assembly line basis for the first time ever. This had not been done anywhere so yet you're taking processes that were custom processes at Los Alamos and Hanford and translating them into mass manufacturing processes.

And so you have workers who are necessarily doing something that was unique, where you don't have an experienced

population. And then ^ a relatively experienced population because as I understand it there are no ^ very high from 1959 and back extrapolating that. Now it is not quantifiable, and that's exactly part of the problem here.

I do not believe that the appeal to the 99th percentile in IREP has anything to do with it. That's simply part of the law. This has something to do, once the dose is calculated, this has something to do with how the dose is calculated. It has nothing to do with the 99th percentile.

DR. ULSH: Well, it's also not clear to me how the -- well, first of all, how long does it take to learn a job. I mean, okay, you can maybe make an argument for '52, maybe '53, but the next thing is how does getting more experience change the neutron to photon ratio? I just don't see that.

DR. MAKHIJANI (by Telephone): The number of workers was going up, and I have looked at many of the job cards as part of our evaluation of the gamma and beta dose completeness. And as Roger had said, there

were people applying to work there. The workforce was growing, and very often people would start in non-uranium work or janitorial work, and they would be promoted. And this happened within a period of months very often, and a promotion was very often into the plutonium area. So this was happening throughout the ‘50s. So it’s, I don’t have an analysis of this problem, and if NIOSH has an analysis of the problem, you know, we should have it.

DR. ULSH: No, I --

DR. MAKHIJANI (by Telephone): It’s a difficulty with, it’s one of the difficulties with back extrapolation. The other ^ don’t have a job type analysis. Not everybody was following the batch in the same way. They were different, and the Stanfield thesis when it looked at these nine different methods, came up with nine different answers for neutron to photon ratios.

And minimizing variance in a particular model doesn’t guarantee you that you’re going to calculate a bounding dose. That just says that the error is smallest for

the method you've chosen. It says absolutely nothing about the adequacy of the method for producing a bounding dose.

DR. ULSH: We're not required to produce a bounding dose.

DR. MAURO (by Telephone): Brant, this is John Mauro. Can you folks hear me okay?

DR. ULSH: Yes, sir.

DR. MAURO (by Telephone): I've just got a factual question. In those 100 and so paired neutron to photon ratios collected for the two buildings in 1959, let's say we have one building, and you have a set of, you mentioned on the order of about 100 or so, individual measurements. And if when you take those individual measurements, you get a range of neutron to photon ratios from a low to a high.

Then for that building when you go ahead and back extrapolate, do you use the full distribution of the neutron to photon ratio to apply to the gamma dose, let's say for the 1952 person, or do you use the upper 95th percentile or in other words, mechanistically, when you are going to apply a neutron to photon ratio using the 1959

experience to an earlier time period, do you work with the full distribution, the median, the upper 95th percentile of those individual paired values?

DR. ULSH: John, I'm going to take a shot at answering your question and rely on other people to correct me if I'm wrong. But I believe that the NDRP provides distributions of the neutron to photon ratio. Now, am I, anyone want to correct me?

DR. MAKHIJANI (by Telephone): As I read it a constant number is used and is not calculated in the way that John suggested.

Roger, correct me if I'm wrong, but the way I believe NDRP wound up calculating it was summing all the neutron doses and summing all the gamma doses and taking a single ratio. Roger?

MR. FALK (by Telephone): Yes, that is the way that the ratios were determined.

DR. ULSH: Okay, I stand corrected. Thank you.

DR. MAURO (by Telephone): No, that's very useful because then, now I understand. So in effect the ratio that was selected is sort of

an aggregate. Now, in the way I look at a problem like this is in the aggregate, that number might work.

What I mean by that is let's say we have a person that worked in 1952 through 1956, and there's reason to believe he worked in a lot of different functions which means that his experience in each change out of his gamma dose is going to reflect the cross-section of neutron to photon ratios that, and not any one extreme.

We've had this theme before. So the way I look at it is if, in fact, you have an aggregate ratio, and then you're going to apply it to an individual, and I understand Arjun's concern in going from an aggregate number which is really a population number to now I want to apply it to an individual.

Now I would agree entirely with Arjun that if you were going to try to apply that aggregate ratio to a single change out for one person in let's say one month in 1952, there would be a real problem with that. But if you're going to apply that aggregate ratio to a person's, let's say to several hundred

change outs that he may have experienced over the course of many years, and there's reason to believe that he had very varied experience by way of the nature of his exposures in those early years, then all of a sudden ^ the ring a little more true.

But if it's plausible that the person in the early years that you're trying to reconstruct ^ may have worked at a single location in a particular job where he consistently was exposed to let's say more of the higher end of the neutron to photon ratio as observed in your 1959 data, then I would say that then we've got a problem.

So we're back to a question that we have encountered before. There are times when using an aggregate ratio will serve us well. If the person that you're assigning it to we have a good reason to believe he's experienced a cross-section of the exposures. But if we don't know that, and it's possible that that individual over the time period for those early years worked in one location or one job function where it's not inconceivable that he could have experienced the high end ratio,

then I think we've got a problem.

So that's how in listening to the conversation here, that's where I come out as what we need to discuss and what needs to be addressed. Do you folks, I don't know, I guess I'd like to hear a little perspective on that way of looking at things. Am I looking at it correctly?

DR. NETON: John, this is Jim. I think I sense a slight shift here in the logic behind the arguments. You know, at one point I was getting the sense that SC&A was arguing that the data are not sufficiently robust to do anything. That's kind of the sense I've been getting this morning.

But your suggestion that, you know, it's a matter of picking the right dataset to use to reconstruct the doses sort of shifts the emphasis to imply that the data may be sufficient to do dose reconstructions just that maybe we're not going about it the right way.

DR. MAURO (by Telephone): I don't want to cut short the other aspect. In other words I'm not saying, now there may be more issues,

there are multiple layers of issues.

DR. NETON: Right, but I'll tell you, I'm not sure this discussion, the previous discussion needs to be finished and agreed upon before we go to whether it's the 95th or the 50th percentile. We've been down that path many times.

DR. MAURO (by Telephone): I just wanted to get that clear in my mind. Now what I'm hearing is notwithstanding that issue, and I'm agreeing with you, Jim. What I'm hearing then is that's not really where the issue lies because that is a tractable problem if it's just a matter of judging whether we should operate off the 95th percent. Now what I guess I'm not fully understanding is that there are other issues that are ^ go beyond the matter of whether we should be picking off the 95th percentile off a full distribution of paired numbers. There are other aspects to what has transpired that undermine that. And I guess for my own benefit if I feel I suspect other people have experienced it also, there are other aspects or nuances to this extrapolation problem that ^ ^ just raised.

DR. MAKHIJANI (by Telephone): John, we don't even have a 95th percentile for neutron to photon ratios in the way things were done. So, yeah, there are a lot of prior questions.

DR. NETON: Well, it could be calculated, Arjun.

MR. SHARFI: The NDRP is --

DR. MAKHIJANI (by Telephone): We're just addressing what was done in the NDRP project and how ^ was set up, and what the critique of that is and the centrality of this. Now, they chose this one method out of, I don't know, nine different methods that are mentioned here that I'm looking at. Let me see how many there are. I can tell you how many there were. One, two, three, four, five, six, seven ^, nine different methods that were tried to calculate the same number with nine quite different results.

And so there are a lot of prior questions, and there is a problem in that I didn't say ^. It seemed to me that N/P ratios that are aggregated by building would give you a good idea of what was the typical average dose in that building. But under 42-CFR-83,

that's not the question you're trying to answer.

MR. GRIFFON: Mutty had something to say here.

MR. SHARFI: Arjun, the NDRP study did provide a 95th percentile notional dose.

DR. MAKHIJANI (by Telephone): Yes, they did provide ^ percentile on the notional dose.

MR. SHARFI: On the upper end of the --

DR. MAKHIJANI (by Telephone): Right, but based on a ^ the N/P ratio was calculated as a single number based on a particular aggregation of neutron and photon doses.

DR. ULSH: Building specific, I mean --

DR. MAKHIJANI (by Telephone): No, no, I'm not saying, I'm just trying to be clear about what was done. That's all.

DR. MAURO (by Telephone): For clarity purposes just so I think to understand where we are, what I'm hearing is in 1959 for two different buildings we have 100 or on that order individual paired measurements that we believe are robust. They're reliable ratios. In other words those 100 or so measurements for a given building are, those individual

measurements are something that are robust numbers for that year.

Now the idea being now, and the question I guess I want to put on the table is, given that we have those numbers for that building, those 100 numbers for 1959, I guess I'd like to hear a little bit more about, and maybe this has been covered, but what is it about that that creates a situation that we may still not necessarily be able to reconstruct the neutron to photon ratios that might, that one individual might have experienced in a given year.

In other words what I'm hearing is that there might be a problem being able to do that. That is, taking that full distribution of 100 paired numbers and say here's the range of neutron to photon ratios, and now somehow I'm going to use that data to somehow figure out a claimant favorable, but still plausible, upper bound on what the neutron to photon ratio may have been for a given worker in, let's say, 1952.

Could ^ or Brant, what are the challenges associated with that because the

layers of problems the way ^ I've been listening ^ complex that I'd like to hear just a simple like what would it be about that situation that would make it very difficult to reconstruct the neutron exposure that that particular person experienced, let's say, in 1952.

DR. ULSH: Well, I mentioned earlier the questions that we considered when we were evaluating whether or not this extrapolation was a good thing to do. I'm not sure if that's what you're talking about or not. Those --

DR. MAURO (by Telephone): I'm asking a very simple question that goes right to the, I've got 100 paired measurements in 1959 for people that worked in this building, given building, and which give me my neutron to photon ratio distribution for those 100 or so workers. Now all of a sudden I have another worker that worked in 1952 and all I'm really hearing is that, well, let's see, can we use that data somehow to figure out what the neutron dose was to the person that worked in 1952 in that building.

And what I'm really hearing is a disagreement or there's some concern. In one case someone says, yeah, I think we can do it. And the other person's saying, well, there are a lot of things that are still uncertain here that I'm not sure whether you can do it.

And I guess I'm really bringing it down to if I were about ^ understand where the problem lies in being able to go from the 1959 distribution of neutron to photon ratios back to the 1952 given that I do have ^ exposure for that fellow in 1952.

DR. NETON: The only argument I've heard so far against that is Arjun's supposition or assertion that workers would gain more experience over time; and therefore, their time at any different station might be different between '59 and the earlier years although I might argue that the time would be slow and be ramped up equally among each of the stations.

DR. MAKHIJANI (by Telephone): Well, that's not the only argument. I mean, I have a whole list of bullet points.

And John, you have an incomplete draft

in your e-mail to look at.

DR. MAURO (by Telephone): I understand. I mean, I think that I am listening as everyone else is around the table, and I'm starting to understand what the -- I guess I'd like to understand more given that starting point, but I ^ really fundamental and you'd understand. I'd like to get a better appreciation of the other dynamics at work here.

Jim had just mentioned one, and you had mentioned one, Arjun, the fact that there's experience changes. What I heard is that there were some, I'd like to hear a little bit more of what are the other challenges to be enabled to do that that are before us, that when we engage in this ongoing dialogue that will continue certainly after this meeting, what will that be about? What are these other challenges?

DR. MAKHIJANI (by Telephone): Let me try to give you an idea of, repeat some of the things that I've said. I talked about the validation of these doses and some way to find whether the calculated doses are claimant favorable, much less bounding or not. If for a period in

which you have measurements of real workers, say, with particular job types, or even an average, say, following a badge through, if you define it as one composite job, when you have neutron and photon measurements, you can take the workers with paired measurements, and you can also calculate doses using the notional dose approach for those workers. And you can validate whether your model is good or not. For from ’52 to ’58 we have no measurements for Building 71, and so it’s not possible to validate it that ^ measurements. And so it is not possible to validate it that way.

There’s a question about the whole neutron to photon ratio approach that is raised in the NDRP itself is that this approach has been chosen ^ out of one of many different approaches that were examined, none of which, some of which have advantages over this one and disadvantages over this one. This was selected to minimize the variance or uncertainty in the results in a particular statistical model --

DR. NETON: And I think we’re not talking

about the NDRP model now. We're talking about why we couldn't use these 80 or 100 data points to back extrapolate.

DR. MAKHIJANI (by Telephone): I'm just giving John the picture of why I think -- well, there's the underlying problem with the model itself which is compounded by the back extrapolation. That's basically what I'm trying to explain.

DR. NETON: But John has suggested if you threw away the model and you've got 80 to 100 data points, now what can you do? I mean, we don't need to talk, rehash, what's wrong with the NDRP model at this point is what John's saying.

DR. MAKHIJANI (by Telephone): You don't have any way to validate what you're going to, whether the doses you're going to calculate are bounding doses under the rule as I read it.

DR. MAURO (by Telephone): This is very helpful to me, I'll tell you why, because what I'm hearing is that ^ have 100 measurements in 1959. And let's say someone would say, well, let's just take the highest ratio ^ that turns

out to be or something at the 95th percentile, and ^ that would place a plausible upper bound on what some earlier worker might get.

What I'm hearing, Arjun, and I'm not disagreeing with you; don't get me wrong. I just want to understand what you're saying is, well, that's not good enough. You've got to do, in other words, we don't know if that distribution that was captured in 1959 has, in fact, any resemblance to the exposure distribution that a given worker may have experienced in an earlier year. In other words you need a little bit more way of validating that distribution that somehow it has applicability to the earlier years. Did I say that correctly?

DR. MAKHIJANI (by Telephone): John, we're in the process of critiquing something that hasn't even been created. The issue on the table that we're evaluating is NIOSH's SEC evaluation report in which it says it can calculate doses in thus and such a way; and therefore, it's not a problem. And that method is the NDRP method, and that for '52 to '58 involves a particular approach. And as I

understand what we're doing is we're writing a review of that.

DR. MAURO (by Telephone): Okay, I guess what I just raised is, you're right, Arjun. I posed, I guess, a more, the question was related to is there a way to ^ you have lots of concerns about the method that was selected. I guess I'm trying to get to the point where, notwithstanding the method that was built, the bridge that was built, recognizing that we may have lots of disagreement regarding that particular methodology, I guess I'm ^ --

DR. NETON: This I don't think is very dissimilar from where we ended up with external doses at Y-12 in the SEC evaluation.

DR. MAURO (by Telephone): I think you're right.

DR. NETON: We had a lot of disagreement internally among us about the maximum likelihood estimation methods, but at the end of the day we all looked at the data and agreed that based on the data that we did have, some method of bounding the doses could be, was plausible. So I don't see that too

much different here. I don't know.

MR. FITZGERALD: Except I don't think we've reached closure on sort of the first issue is that whether or not there's a problem with the '52 to '58. I think that's what we --

DR. NETON: Exactly.

DR. ULSH: We do have, I don't think anyone's disagreeing that we do have --

(Whereupon, telephonic transmission interrupted the proceedings.)

MR. GRIFFON: I mean, it might be almost time for a break. Let us all reflect on because we're getting deep into the weeds. I think we need to take a break, step back and see where we're at.

DR. WADE: If you can hear us, we're going to take a break.

MR. GRIFFON: We're going to take a lunch break. So we'll reconvene at 1:30 if that's okay.

(Whereupon a lunch break was taken from 12:30 p.m. until 1:45 p.m.)

DR. WADE: Mike, are you with us?

MR. GIBSON (by Telephone): Yeah, I'm here.

DR. WADE: Are there any other Board members

on the line other than Mike?

(no response)

DR. WADE: Any other Board members other than Mike?

(no response)

DR. WADE: David, why don't you ask your question of the work group Chair?

MR. HILLER (by Telephone): Thanks, this is David Hiller of Senator Salazar's Office. While we were waiting to reconvene I had asked Lew kind of what the burden of proof is. I'm not sure that's the right term in this context, but that's my legal history creeping up on me.

I'm trying to figure out if in this context the burden is on the government or NIOSH or the appropriate agency to demonstrate the validity of its methodology and accuracy of its dose reconstructions, or if the burden is really on the petitioners here and kind of in the context of today's discussion in a sense the burden is on SC&A to demonstrate that the methodology is unreliable and produces inaccurate results.

MR. GRIFFON: Lew, did you want to --

DR. WADE: Well, I've answered it.

MR. GRIFFON: -- take a crack first? I didn't hear your answer to I'll listen to Lew's and then I'll ponder that.

DR. WADE: I'll give you a briefer version than what I did before. I really approached the answer by looking at the sequence of things. First, the petitioners in their petition present their arguments why they think this class should be added to the special exposure cohort.

And this special exposure cohort rule goes to issues of the ability to cap dose or bound dose with sufficient accuracy. There are all kinds of concepts we could talk about with that, so the argument is presented by the petitioners. NIOSH responds with a petition evaluation report. NIOSH gives its logic as to why they agree with the petitioners or why they don't agree with the petitioners.

And those two data points then make their way to the Board, and the Board has to consider both arguments and decide upon its recommendation as guided by the statute. The Board will often as it has in this case asked

for further deliberations to take place and assign it to a work group and dig through issues and sift through issues to try and come to a clear understanding.

The one thing, David, that I will point out to you that SC&A really is a participant but not an active participant in this. SC&A is a contractor that does what the Board or the working group asks it to do. SC&A is not presenting final arguments. SC&A is just informing the process by their scientific deliberations.

So again, the Board will look at the petition, will look at NIOSH's evaluation report. It will look at what the work group brings to it. It will deliberate and debate and then make a recommendation.

MR. GRIFFON: And I think just to add, I don't disagree with that. As the work group has gone along, I think it's important to point out that we always turn to NIOSH for, to do sort of the follow up investigations. So the Board isn't really going to the site and investigating these issues independently that way.

But we're getting data back from NIOSH to support their arguments or, and then we're, we consider that data that's put on the table that way. So in that sense I think, you know, the burden there is for NIOSH to demonstrate that, I think the burden falls to NIOSH there to demonstrate that they can, in fact, do the dose reconstructions.

But it also is we have to, I think we've started to consider sort of the weight of the evidence in our deliberations. The Board has to lean to some extent, look at sort of the weight of the evidence issues because sometimes there's not a bright line on some things. But sometimes we can come to a place where we are pretty or very confident that we can get a plausible upper bound. So that's the terms Lew was referring to.

MS. MUNN: And if the science is sound.

MR. GRIFFON: Yeah, and then the science is sound. So does that sort of answer your question?

MR. HILLER (by Telephone): Yeah, I guess one of the things it suggests is when I think I detected from this morning's conversation

which is it's very tough when you are trying to look back at old issues, and you don't have the data you'd like to have from that time period to give you independent validation.

MR. GRIFFON: Right, right, and that's why we have these deliberations about were there process changes? How did you determine this? And without those independent measurements necessarily are there other ways to sort of corroborate that method. So, you're right.

Anything else to add, Lew, or --

DR. WADE: Well, just to look to our process. The one thing I think we all try and do is make the process open, let everyone participate and really expend a great deal of effort of trying to get to the core of issues. And again, the Board and its work group really deliberates and works hard towards getting to the core of the issues and tries really to see that the concerns of the petitioners are heard and investigated. And that's why sometimes the process takes so long.

MR. GRIFFON: Okay.

MR. HILLER (by Telephone): Thank you.

MR. GRIFFON: Thank you.

MR. HILLER (by Telephone): And I'll point out that no one is going to accuse you of rushing through this.

DR. WADE: We'll take that as an endorsement of our practice.

MR. GRIFFON: I think we want to, well, we definitely want to close out the neutron issue, but I'm not sure exactly where we left off. We had, I mean, my sense of the end of the discussion before lunch was that we have this question of the back extrapolation, but then we were starting to get into some dialogue of how to correct it.

But I'm not sure that there's any, I mean, I'm not sure that I've heard clearly that there's a problem with it. So I was getting a little confused that some people were throwing out proposals for how we might bound it, but I think the current proposal on the table is that this approach is what we're using, and it works. There's no reason to look at another model.

So maybe starting there, Brant, I'll let you --

DR. ULSH: Mark, I think that's certainly

our position at this point in time. We've heard SC&A's concerns that Arjun expressed. We really can't come to any conclusion about those concerns until we see the analysis. As soon as it comes over we certainly will give it due consideration.

There was some discussion as I recall about -- and this is probably where it got confusing -- was trying to determine whether even if we have these disagreements about the back extrapolation, is it a tractable issue or is it an SEC issue. I think that was some of the discussion that was going on. And Jim, I think, was talking about source term.

DR. NETON: Well, yeah, I was just trying to point out that we do need to look at the SC&A analysis and see if there's any issues with the NDRP model that would affect our ability to do dose reconstructions. But, in fact, at the end of the day we believe these can be reconstructed. There's a source term involved here that's pretty well defined.

We know they started with around 200 grams of plutonium in the early '50s and worked their way up to something like 1,200

grams. And physics is physics. I mean, you can model the neutron/photon ratio or calculate it very precisely actually coming off of a source term such as that.

And whether one chooses the moderated or in a worst case an unmoderated source term, you can come up with a ratio from that activity. And in our estimation that would be a worst case analysis of ^ neutron/photon ratio. In fact, that could be used to in some sense be used to look at the ratios that were derived in the NDRP study to, in effect, maybe not a validation in the traditional sense, but at least a sanity check against some of those numbers.

And we're prepared to do any, we're prepared to look at the data and look at the plans. And then also if we do have to change our position, which I'm not saying we would at this point, but there are other techniques that are available to us.

MR. GRIFFON: Timing obviously is of the essence here, but we also have to consider this question, don't we, of the aggregate nature of the N/P ratios being applied to

individuals in the prior time period?

DR. NETON: Yeah, I think that's right. We need to look at this whole set up, and I'm not --

MR. GRIFFON: I'm not saying you can do it real-time here, but I'm just --

DR. NETON: I'm not convinced in my mind. I'm not close enough to it to know exactly how they, what the aggregate ratios, how they were applied. In some sense I don't know whether, did they take exactly that or I've heard a couple excerpts where they were extremely conservative approaches, where they believe that they're almost like bounding calculations. So we can just take a closer look at the NDRP analysis itself to see where they ended up. They do provide 95th percentile dose estimates. We need to look at that and see how those were derived, what factors were included. So there's a, we need to do a little more homework on this.

DR. ULSH: Yeah, and these issues were just discussed within the past week, so I mean, we do need to --

MR. GRIFFON: Right, right, right. And

we've all been, I mean, to everybody's credit we've been working real hard in the last two weeks to come to closure, but we've got to get this right.

But I guess the other observation that I had with these N/P ratios -- and this gets back to some of the discussions about the production and whether it was modified or anything -- and I don't even have the exact years, but I, we were discussing a little at the break. I think it was '63, '4 and '5 where they have, they sort of used a ten, a ratio of ten for the N/P ratios. And it has an asterisk saying it's kind of an upper bound. It was just administratively said. It wasn't necessarily the average value, the calculated value.

But I was just trying to understand because when Jim was talking about, well, we could use just a pure source term approach and calculate it, it seems to me that those years were when the americium was coming in, and you'd actually have lower neutron to photon ratios. And why were the tens coming up in that time period and the ratios lower in the

early -- I'm not necessarily asking for an answer. I'm just trying to figure that out myself.

DR. ULSH: I think I have an answer.

MR. GRIFFON: All right.

DR. ULSH: I'm recalling from our conference call two days ago that those tens with asterisk that you're talking about were for Building 91 in the years that you said, three, four, five.

MR. BUCHANAN: 'Sixty-two, three and four.

DR. ULSH: 'Sixty-two, three and four, okay. But the americium line is not in Building 91.

MR. GRIFFON: Oh, okay.

DR. ULSH: So I'm thinking that those two are unrelated. Now I can't tell you why the tens, but I don't think it has anything to do with the americium.

MR. GRIFFON: So it was just for 91. I didn't realize that. So where do we stand overall on this? We're going to have, you're going to look closer, SC&A's going to, are you going to wait for some materials from SC&A? This doesn't have to be privacy reviewed to be exchanged.

MR. FITZGERALD: I think, I don't know. Arjun's given me a very, very tight timeframe. When would be the reasonable time to provide something to Brant?

DR. MAKHIJANI (by Telephone): Well, you know, Joe, it just depends on how much of an internal check you want. We're in the process of preparing this. As you know, last night when Ron and I talked, some of the numbers I came up with weren't the same as some of his. And so I don't think we should be providing NIOSH and the working group with numbers that we don't have, you know, we have to make sure that the data is properly sorted and that we were internally talking about the same sets of data. So I can't really see that we're going to be able to digest all of this and address all the periods until Wednesday. Now if you want to chop that up and say let's serve up the '52 to '58 first, and then postpone the other stuff. I mean, that presumably we could focus on the '52 to '58 and send it off by Monday or something.

MR. GRIFFON: Would that save time?

DR. MAKHIJANI (by Telephone): But if you

prefer that, because then you get a piecemeal report.

MR. GRIFFON: I think we might want to do that, Arjun. And we might even have to be in a position of either another technical call or another work group call because I don't want to travel in the next week. But we might want to, yeah, if you can do the '52 to '58 by Monday, then maybe have a call on that on Wednesday or something.

DR. MAKHIJANI (by Telephone): I'll try to have it out by Monday close of business. And I guess, Ron, this will mean -- Ron, are you there?

MR. BUCHANAN: Yeah, I'm here.

DR. MAKHIJANI (by Telephone): Will we be able to talk about data between now and Sunday and for you to be able to review what I've done, what I write up, because I'm really relying on you, you know, you're familiarity with the data much more than mine.

MR. BUCHANAN: Yeah, okay, well, I think we can have, I can review your write up for '52 through '58 tomorrow, try to have you something by close of business tomorrow that

you can --

DR. MAKHIJANI (by Telephone): Yeah, well, we need to go over a lot of the data, too. So anyway we can talk about that offline.

So we'll deliver the '52 to '58 before the close of business on Monday.

MR. GRIFFON: And then we'll, Brant and Joe, I'll coordinate it for you. We have a technical call. I think we can keep it to technical calls at this point, and we'll certainly notify all work group members and everything. But if we need a work group, one final work group call before the May meeting, I mean, we're running out of space here.

DR. WADE: It's your call.

MR. GRIFFON: But I mean if we were to look at this '52 to '58, and we have some technical sort of exchange that needs to go on, I think we could do that in the next couple days after that. And then I'll certainly notify Lew and get the word out if we're going to have a full work group call it'd probably be the next --

MS. MUNN: The Monday before the meeting.

MR. GRIFFON: The Monday before the meeting might be the day we might want a work group

call.

MS. MUNN: Might be a wise idea.

MR. GRIFFON: I know. I know. But we're running out of days so we really do want to push for --

DR. WADE: Really the only difference between the technical and the work group call is we have the court reporter.

MR. GRIFFON: Exactly, and maybe a few less people involved. And then the other time periods we'll just assume you're going get out your full report by Wednesday, right? Or whatever.

MR. FITZGERALD: Yeah.

MR. GRIFFON: All right, everybody's on a tight timeframe. I know it's, and just for the record, I mean I think I don't think we're at this point where the neutron stuff and, you know, I don't think we anticipated this much in-depth work at this point, but sort of here we are. I think we had this neutron issue on the table for awhile, and I think we might have underestimated the depth that we were going to have to go into a few of them, but we'll just have to do our best from this point

forward.

DR. MAKHIJANI (by Telephone): Mark, may I also say one thing regarding a comparison that was made before lunch just so at least, you know, I think there are quite a few differences between Y-12 and this dataset and actual situation and the job descriptions and so on. So if there's an impression that it might be sort of simple to say we did it at Y-12 and so we can do it here, from what I know of the data that are available, I think it's a much more complicated question here.

DR. NETON: Arjun, this is Jim. I think I made that comparison. I wasn't trying to say your comment on the simplicity of it, I was trying to comment on the process.

DR. MAKHIJANI (by Telephone): Yeah, right, no, no, I'm just kind of worried about, I know last night by the time I talked to Joe I was so tired that I could not talk about what I had written. So I don't want to get to that point and be working in that mode.

MR. GRIFFON: All right, I think we're leaving neutrons unless there's anything else. Obviously, we'll be talking in the near future

about these issues.

THORIUM

Then the next agenda item I think, as I'm opening my computer, was thorium. Let me just attempt to give a little introduction to the thorium question then. The way I remember this, the way I remember it from the last work group meeting is that the work group and SC&A have sort of, have accepted NIOSH's definition of the source terms, the various source terms.

So we pursued this for awhile. We had some questions of other source terms of potential significance. And we basically feel that NIOSH has turned over every rock they can find, and we have a good handle on the source term of thorium. Where SC&A has some disagreement is the method. The method on the table I think is the NUREG-1400 approach.

And I think that SC&A in their report that was recently provided had some concerns that that does not bound some of the potential exposures. And now my understanding also was that there was this other sort of set of, or other data available, and we had discussed this a little bit at the meetings, and we have

some references.

And I may have this wrong, but Adley comes to mind as one of the reports. I don't know if that was uranium or thorium or, anyway there were some other references that were recently found by NIOSH or SC&A or both parties that had, that looked at sort of several different processes and work places that involved either thorium or uranium exposures.

And they looked at sort of the general airborne concentrations from various processes and things like that. And we had said, and I think NIOSH offered, that they would consider this data and see if, in fact, it was, could be used as a bounding or to validate their approach ^ approach. And I guess that's the question is, you know, if the NUREG-1400 approach, there's some question whether it bounds the exposures.

If there is other data there that we all agree is in a form that can be used to bound the exposures, then I think it comes down to a sort of a dialogue between the two parties of sorting out the method that's going

to be used going forward. And it may not be an SEC issue. But if, you know, I have to hear a little bit more about what this other data, you know, how appropriate it is for application at Rocky Flats and the operations you have.

And I think SC&A may have more questions than I do, but that's sort of where I stand with the thorium question. So the source term we agree with. The methodology there's questions about whether it bounds, but at least speaking for myself, I believe you have other data there that if you chose to use that, it'd probably be used to bound those exposures. So, I'll turn it to Joe first, then Brant.

MR. FITZGERALD: Yeah, I think that's a pretty good summary. I think this ^ category I had mentioned earlier where we think again there's enough data available, but the concern is the method and whether or not it would be bounding in terms of the potential exposures.

In this case I think there was some equivocalness introduced in some calculations that NIOSH provided in December where we felt

that it showed the opposite, but maybe there'd be instances where it would not necessarily bound it. And I think that's our concern on 1400. And I'll let Arjun jump in. It is really from there. It's not that we don't think it would be feasible to do a bounding approach, we just don't think NUREG-1400 has been shown in this particular instance to do that in all cases.

Is that a fair approximation, Arjun?

DR. MAKHIJANI (by Telephone): Yeah, yeah, I think it is. Just to be specific, in the December paper, leading up to the December paper, Jim Neton had suggested that there be a comparison with centerless grinding and so on to see whether NUREG-1400 was bounding. And as I read NIOSH's paper and the numbers presented in that paper, the intakes from the process numbers, the measurements from other plants were much higher than the NUREG numbers. And so the validation exercise for NUREG as a bounding dose failed, but NIOSH continues to say NUREG is bounding which I did not understand.

And then we had a separate validation

exercise using time weighted data in the next paper whose date I don't remember. And as we've pointed out in several different studies, and specifically made an analysis in Mallinckrodt in the context of an SEC, that you couldn't use time weighted averages for bounding individual doses because time weighted averages are just that; they're averages.

And when they involved two and three measurements you've got very big uncertainties, and then you have to try to calculate a bounding dose or a 95 percentile to see whether your dose is actually bounding. And in that exercise also NUREG-1400 did not hold up. And that is in our, the calculations are in our report if I remember right.

And so on both tries where a numerical test was applied to NUREG-1400 it actually failed so in my opinion it's not an equivocal question. I think the demonstration of the bounding nature of NUREG-1400 failed.

DR. ULSH: I have similar recollections to Mark, but my recollections go a little bit further. We did talk about at the March 7th

working group meeting that there were these other data sources available. Adley was one. A book by Albert on the industrial hygiene of thorium, those were both provided by NIOSH.

And John spoke very highly of them although SC&A needed more time to review. We also spoke about there was continuing disagreement between SC&A and NIOSH on the applicability of NUREG-1400 to a bounding analysis. So far we are in agreement in our recollection.

DR. MAKHIJANI (by Telephone): Right.

DR. ULSH: But I recall clearly that we had agreed to disagree and that this was categorized as a TBD issue and not an SEC issue. I've got to tell you I'm a little exasperated that we're talking about this in the context of an SEC issue.

DR. MAKHIJANI (by Telephone): I guess I have a procedural question in terms of how I understand things. Does NIOSH have to, just in terms of the criteria that we are expected to follow that the Board laid down, and what happens when the criteria are seen to fail, now the Board has said you have to give some

kind of a proof of principle, and the first step in that is you have to specify a method.

And when the specification of a method failed, then we've said there likely are data, but they belong to other sites, and there's some kind of demonstration that's needed to show that they can be properly applied. And in that context we all agreed that if that were done, it likely would not be an SEC issue.

But as I understand it, NIOSH is insisting that NUREG-1400 is bounding, and in my analysis and the analysis that we did in our report, both of those assertions of validation actually failed. And so we've got a factual, technical situation where we're making opposite assertions about the same set of numbers in which the Board's criteria are not being fulfilled.

And if I'm wrong, I'll just stop talking about it and it's certainly not my province to say how the Board's criteria should be applied. We're just doing the best we can in understanding what the Board's criteria are.

DR. WADE: Let's let Mark --

MR. GRIFFON: I think, Arjun, I think, and my recollection also, and I haven't looked at the minutes, but my recollection was that we, as in general we did sort of say, you know, with this Albert reference and this other reference, it was my sense that we had enough there to be able to bound. Now quite frankly I don't, and I still haven't looked in detail at the analysis that you just talked about, Arjun, that you're saying that the NUREG-1400 doesn't bound those two circumstances.

And whether what we said in the last meeting, irrespective of that, I guess, I would like to go into the next meeting saying that I don't think we want to go in as the Board having SC&A say we don't think that NIOSH's approach is bounding. So what I was hoping is that if we can hear from NIOSH that these other two references have this data, and we're not necessarily committing to this method, but we would deliberate further with SC&A on the NUREG-1400 versus this method. But if we choose to, we have this data here that could be used as well.

And let SC&A say, okay, if you went that direction, we're in agreement. And then sort out the details afterwards, and that would be more of a TBD issue. It gets a little bit in this gray area of this proof of process, and I think you have to show that it's feasible to reconstruct dose with sufficient accuracy.

And then the question of, in my mind I guess, is when we're looking at feasible, we have to at least know enough about do you have the data there, and do you know how you're going to, how you can apply it. Whether we need to see you commit to one method or another, you know, even if there's still disagreement.

And this is my opinion. I'm not sure I'm on very solid ground here, but even if there's a disagreement of methods, if at least one of them everybody agrees is bounding, then we can have further arguments on which one is correct as a TBD issue. I don't know how other people feel on that.

DR. ULSH: Well, let me respond to that, Mark. Certainly, we would agree to discuss

NUREG-1400 and these other data sources further. Certainly we would. But that would occur in the context of the TBD issue, TBD discussion. That was the consensus that was reached at the March 7th meeting.

MS. MUNN: Yes, it is.

DR. ULSH: We agree that this data looks promising, the Adley and the Albert, sure it does. Now in terms of NUREG-1400 --

MR. GRIFFON: But you're not in a good enough position right now to at least describe the Adley data, and how promising it looks, and how it would or would not be applicable to Rocky Flats?

DR. ULSH: Well, I do have to say that our primary position has been and remains the NUREG-1400 is bounding. We also think that Adley looks promising. Now, of course, we are in disagreement with SC&A about NUREG-1400, and since those issues have twice now been put on the record, SC&A's objection to that, I feel I do have to respond. However, I refrained last time because we had this consensus that it would be a TBD issue, and I did not want to disturb the consensus that we

had reached.

In retrospect I guess that was a mistake because SC&A has backed away from that consensus, and I would like to know, I mean, we knew that we were in disagreement about NUREG-1400 on March 7th. That's not a surprise. We knew that. But consensus was reached at that meeting that this was not an SEC issue. It was a TBD issue. So what new has come up between then and now to make SC&A walk away from that consensus?

DR. MAKHIJANI (by Telephone): Well, I think that this is not a very accurate or full, at least a full characterization. My understanding -- well, first of all at the last working group meeting we had not had a chance to analyze the weighted, time weighted average validation exercise that you had done. When it was actually put to the test of the numbers, it did not meet them so that necessarily happened after the meeting because we got the paper just before the meeting. So that work could not have been completed at the time of the meeting.

Secondly, my puzzlement on this

question is not relating to whether doses can be calculated, and therefore, whether it's a TBD issue. And I think that's the context in which I remember an agreement last time. My puzzlement is when you've got something before you that fails the criteria of the Board affirmatively that NIOSH could retain the option of using that as a bounding dose method and insist that it is a bounding dose method, and say, well, the disagreement disappears because this other thing is there, but we may never use it, and we may use a failed method. That's what is puzzling me.

DR. ULSH: I'll first talk about what was actually agreed to on March 7th, and then I'll get into your objections about NUREG-1400 since it's an SEC issue.

DR. MAKHIJANI (by Telephone): Well, I don't know. I cannot look at the minutes. I don't have them in front of me.

DR. ULSH: I can help you out with that because I have them.

DR. MAKHIJANI (by Telephone): Well, can we talk about the substance of the technical matter?

DR. ULSH: We certainly can, but first we have to talk about what was agreed to. These are from the draft minutes. Ray is finishing the official version.

MR. GRIFFON: What page?

DR. ULSH: Well, on the draft it's 142. I don't know what page it will be on on the final.

COURT REPORTER: The pages will stay the same.

DR. ULSH: The pages will stay the same, so it's page 142.

Mark Griffon says -- and this is in the context of the thorium discussion -- "But I think at least for those two things, like you said, the source term and the exposure model, and I think at least we're probably at the point where we can say that we may not agree with them all right now, but we think it can be. There are ways to model and bound the doses and when you know the source term. Is that a fair synopsis?"

And Dr. Makhijani says, "Yes, I think that's fair." And then Mark says, "And then I think the upshot of that is I think that it's

removed from our SEC sort of deliberations, at least that aspect of it. We want to still bring it to ground, but it's not on that urgent profile."

Page 150, Larry Elliott says, "Well, you know, SC&A's going to finalize their report, and I would hope that in that finalization of this point alone they would refer to the consensus that I think I hear today and designate the issue as being site profile related. And then we can take it up in that form. If you're explicit enough in what your concern is about NUREG, then we can react to it from this report in a site profile discussion form."

Dr. Makhijani says, "Yeah, we will do that."

MR. GRIFFON: But hold on. Let me just add on the bottom of page 142, after I stuck my neck out a little bit so to speak, Arjun did come back and in the last sentence there says, "The one Becquerel piece that comes out of the NUREG-1400 remains unconvincing.

DR. ULSH: Yes, that's on the thorium strikes.

MR. GRIFFON: Oh, the thorium strikes, yeah.

DR. ULSH: Right.

DR. MAKHIJANI (by Telephone): The context of this discussion, I believe, is the idea that NIOSH would not use NUREG-1400 if there were a disagreement about whether it was bounding or not. I don't disagree that you can calculate these doses. The data appear to be there. You haven't done it yet because you're sticking with NUREG-1400.

And subsequently when I looked at this issue, and when we talked about it internally with Joe and John and as a team, it seems to me that if -- and we did the calculations after the March 7th meeting, and they are in the report -- that the claim that NUREG 1400 met the test of bounding dose by that time weighted average calculation actually, in the calculations that we did, it did not meet the test.

And then when you're writing a report, you have the Board's criteria in front of you that you have to actually have some proof of principle. You have something in front of you that's actively failed the test every time it

has been put to the test, then unless you reject that method, I do not see how it can't remain an SEC issue. And if that method is rejected, then it no longer is an SEC issue, and I believe that's what our report said.

DR. ULSH: Okay, so I still didn't, at the March 7th meeting we knew that we were in disagreement about NUREG-1400. That is not something that was discovered afterwards.

DR. MAKHIJANI (by Telephone): But we had not checked your validation exercise, and I believe there was a discussion about that as well.

DR. ULSH: If you didn't agree with it on March 7th, and you don't agree with it now, but we agreed to have a consensus that it was a TBD issue, nothing has changed.

Now in terms of the substance because you've gone into your objections to NUREG-1400 several times, and like I've said, I've refrained from responding. Now I will respond.

The validations that were performed, first of all, we disagreed strongly with even the need to do a validation exercise of a

standard that is widely accepted by the regulatory community; however, we recognize that it is to no one's benefit to go before the Board in disagreement. So we agreed to do these validation exercises.

It was suggested by you, Arjun, that we look at Simond's Saw and Steel, and that we also look at Rocky Flats' application. That is exactly what we did. At Simond's Saw and Steel we took the highest of 20 monitored workers.

Let me back up a little bit. You suggested Simond's and Rocky, and we took a look at that, considered your suggestion and it actually turned out to be a very good suggestion because it had the advantage of having a well-defined source term, a well-defined processing time, and air monitoring to go with it. So those were very good criteria and that allows you to do a good comparison.

So we did that comparison on your suggestion. At Simond's Saw and Steel there were 20 monitored workers. We took the highest of the 20. Yes, it was a time weighted average. I think that you're

misinterpreting the applicability of time weighted averages, and perhaps Jim can jump in on that. We took the highest, and we found that even with the highest worker, NUREG-1400 was bounding.

Then we looked at the ingot operation at Rocky Flats which is described in the Callabra report. This is all in our February 28th report. And in that case as well you had a well-defined source term. You had a well-defined processing time, and you had some air monitoring. That's the best we're going to get at Rocky Flats, but that's what you suggested. That's what we did. And in that case it turned out to be bounding.

Now you've mentioned the previous method of looking at uranium operations. As we pointed out in our report, that methodology incorporates a time factor, so we had to estimate how much processing time. And that's why we thought your suggestion to actually look at thorium processing operations with well-defined parameters in terms of source term and processing time was a good one because you didn't have to estimate that time

factor. That is the explanation for the difference that you see on the uranium comparison.

So even when we took your suggestion and did the validations you suggested, and they turned out to be bounding, now all of a sudden it's not any good any more. Jim, will talk about, I think, time weighted averages.

MR. GRIFFON: Let me hear, because I heard from Joe actually in the December report --

MR. FITZGERALD: I think it was February.

MR. GRIFFON: Okay, February. I'm hearing an indication that in their own analysis that the NUREG-1400 wasn't bounding, but I'm not gathering that you said that.

DR. MAKHIJANI (by Telephone): That's correct. If the table --

MR. GRIFFON: I want to understand that.

DR. MAKHIJANI (by Telephone): Sorry, Joe.

MR. FITZGERALD: Go ahead, Arjun.

DR. MAKHIJANI (by Telephone): It's on page 167 of our report where NIOSH's table from the December report, I believe --

DR. ULSH: From the December report.

DR. MAKHIJANI (by Telephone): Yes, it's

from the December 21st report is reproduced. It's an attachment, the whole thing is in Attachment 20 of our report. So if you look at that, the time factor is actually taken into account. The number of hours per year which are assumed for machining and grinding are ten hours per year, and the Becquerel intake is assumed based on only ten hours per year. So there isn't, like we're not comparing like 250 days per year in some place with a few hours per year at Rocky Flats. We're comparing ten hours per year with ten hours per year. And the intakes and doses that were calculated were in both cases bigger than what -- and in one case considerably bigger than what was calculated by NUREG-1400. That was in the December 21st report. That's why I believe the need arose for a second round. Now, we didn't agree then. I still cannot understand how you can do a calculation like this with a limited number of hours and then say that there is a time factor that hasn't been taken into account when it's right here in the table that it has already been factored into the result. And the result was

much bigger.

I can't, what was the NUREG-1400 Becquerel intake? It was a fraction of a Becquerel if I remember. And the grinding, the centerless grinding suggested by Jim Neton as a validation exercise, not by me, came out to be a considerably bigger, at least an order maybe two orders of magnitude bigger, than the prior estimate. And I do not believe, I do not understand actually how this can be characterized as a validation that NUREG-1400 is bounding.

DR. ULSH: All right, let me clarify. You referred to a page in your report that referenced our December report.

DR. MAKHIJANI (by Telephone): Yes.

DR. ULSH: The December report contained the methodology that Jim suggested, you're correct on that, about looking at uranium numbers and converting based on equal mass basis to thorium. And involved in that process, that incorporated the estimate of ten hours which you mentioned. The source of that estimate was Bryce Rich because we proposed it earlier to give a bounding estimate of the thorium

machining at Rocky Flats.

DR. MAKHIJANI (by Telephone): Yeah, it was the same time as you used in NUREG-1400. So it's a factor that just cancels out.

DR. ULSH: I would have to go back and look at that. I don't think I --

DR. MAKHIJANI (by Telephone): If you use a larger or smaller time in NUREG-1400 and the same time in this new calculation, you'll get exactly the same ratio. You won't get a bounding dose.

DR. ULSH: Arjun, NUREG-1400 does not have a time term in it.

DR. MAKHIJANI (by Telephone): No, I know, but you used a time term with NUREG-1400 to calculate the intake, and you're using the same time here. So if you get this being 100 times the previous one, the time factor doesn't enter into it.

DR. NETON: No, Arjun, you could pick any time factor and change that analysis.

DR. MAKHIJANI (by Telephone): Yes, and the ratio will be the same.

DR. NETON: No, it won't.

DR. ULSH: No, it won't.

DR. MAKHIJANI (by Telephone): If you use the same time in both calculations --

DR. ULSH: Arjun, time does not come into the NUREG-1400 calculation. It comes into what you're comparing it against, and that is an air concentration times the time in that air concentration gives you an estimated intake along with all the other factors in terms of inhalation rate. Now Jim --

DR. MAKHIJANI (by Telephone): That's right, you do have to have a time to calculate the intake.

DR. ULSH: Yes, and let me finish. Jim suggested this method. This was one of the brainstorming sessions at the end of one of our previous working group meetings. Once we got into it, and we realized that the results are completely dependent on the time that you choose, then we realized that that was a weakness of that approach.

And in concert with that we had a conference call with SC&A, with you and I can't remember who else, and you suggested that we look at actual thorium operations. And you even suggested by name Simond's Saw

and Steel and Rocky Flats. And we did that. And in both cases NUREG-1400 yielded higher estimates than what was actually observed.

DR. MAKHIJANI (by Telephone): I believe that our analysis of that which has been reviewed internally is before you in our report so I don't know that it is useful to go through why we arrived at a different conclusion from you about those data because how many workers were there at Simond's Saw that you were looking at time weighted averages?

DR. ULSH: There were 20 people.

DR. MAKHIJANI (by Telephone): I believe --

DR. ULSH: Twenty workers.

DR. MAKHIJANI (by Telephone): -- sorry?

DR. ULSH: There were 20 workers at Simond's Saw and Steel. Twenty monitored workers.

DR. MAKHIJANI (by Telephone): And there were air concentration data if I remember. Then we're not talking their monitoring data.

DR. ULSH: They were B-Z air data I believe.

DR. MAKHIJANI (by Telephone): Right, well, I stated that we looked at the time weighted average and the methodology to calculate

bounding doses, and this is time weighted average for a particular job which would correspond to some job presumably at Rocky Flats. And we've stated the time weighted averages when you do sporadic work do not correspond to a bounding dose. That is the analysis that we've presented. Now, we've got a disagreement about that, and I don't know, I mean --

DR. NETON: I think, Arjun, this is Jim. On the time weighted averages I think you're misrepresenting the case. We agreed early on at Simond's Saw that Simond's Saw and Steel time weighted averages were not applicable to Bethlehem Steel because we could not determine that the work practices were similar. That was the only reason.

We did not say time weighted averages weren't useful for calculating intakes. And so what we have here is a job-specific time weighted average which is indeed applicable for this analysis. I mean, it's the best analysis you could have.

MR. GRIFFON: In other words, you're saying the job's comparable to what would be at

Rocky?

DR. NETON: ^ comparability job. It's a comparability, it's to demonstrate that the NUREG-1400 calculation would be bounding in that application at Simond's Saw and Steel. It has no validity to compare it to Rocky.

DR. MAKHIJANI (by Telephone): Yeah, no, I agree with Jim on that. The question of whether time weighted averages should be used to calculate bounding dose in the context of an SEC was discussed in detail in one of our reports on Mallinckrodt, and all that has been done here is to really do a short form of exactly that same analysis and much more briefly --

DR. NETON: No.

DR. MAKHIJANI (by Telephone): -- and refer you to that.

Now if, Joe, I believe that you and John and maybe Mark need to take over this because what's in the report is very clear. We've said this before in the context of Mallinckrodt. We've said it again in the context of Rocky Flats, and I don't believe that there's any difference in the technical

position. Now if there's a policy decision, that is not in my province.

DR. NETON: Arjun, this is not the same issue. We are not calculating bounding doses here for dose reconstruction purposes. We're trying to demonstrate that the NUREG-1400 concept accurately portrays what the intakes would have been for a job.

DR. MAKHIJANI (by Telephone): That's not what has been represented. What has been represented is NUREG-1400 provides a bounding estimate of the intakes for thorium using a source term.

DR. NETON: It does for that job. For that particular job, NUREG-1400 bounded that person's exposure.

DR. MAKHIJANI (by Telephone): I don't believe you can compare that to a time weighted average because time weighted average doesn't provide you with a bounding intake.

DR. ULSH: All right, time weighted average does not bound an instantaneous intake. That is true.

DR. MAKHIJANI (by Telephone): You've got two or three samples in a work station.

DR. NETON: Oh, now you're challenging the validity of the time weighted average --

DR. MAKHIJANI (by Telephone): No, I'm not challenging the validity of it as a time weighted average, I'm challenging the validity of it to represent the intakes for that work station as a bounding dose. That's two completely different things. If you have 1,000 workers doing the same job, that would likely be the average. But then you would have significant uncertainty still because you've got very few measurements at each station.

DR. NETON: This is the best estimate of the intake for that worker at that time.

DR. MAKHIJANI (by Telephone): But maybe it's not good enough.

DR. NETON: Why?

DR. MAKHIJANI (by Telephone): For the reason that I've told you. There are a couple of measurements at each work station for each job, and when you have got a piece of that job that may even last only 15 minutes, if you've got two measurements that are highly variable one from the other, the uncertainty of the

intake for that particular 15 minutes is going to be huge.

And then from one day to the next, from one hour to the next, and certainly over a cumulative of a few hours and a year, you could have two workers getting very, very different doses. And that is the objection to applying a time weighted average in this context for producing a bounding dose comparison. I don't think it's that complicated.

DR. ULSH: I don't either.

DR. NETON: I don't think so either.

DR. ULSH: Arjun, it's true that a time weighted average will not allow you to calculate, to bound an instantaneous dose that a worker might experience. But in terms of the intake that could result from a job that is more than instantaneous, and in this case at Simond's Saw and Steel we're talking about I think it was eight hours. The only way to get an accurate estimate of intake is from a time weighted average.

DR. MAKHIJANI (by Telephone): Are we talking a best estimate of intake or a

bounding estimate of intake? What does NUREG produce?

DR. ULSH: NUREG-1400 produces a bounding estimate, and we showed that --

DR. MAKHIJANI (by Telephone): Well then, you have to try to calculate a bounding estimate from Simond's Saw to compare it to.

DR. NETON: No, no, no, no, no, you're wrong, Arjun.

DR. MAKHIJANI (by Telephone): Well, this is my understanding, and this is how the report is written. And so that's why I'm saying this is beyond my purview at this stage. If we're not comparing a bounding dose to a bounding dose, then I don't think it's an apples-to-apples comparison. And then somebody else has to define what kind of comparison we have to make because that's ^ calculation I know how to do.

DR. NETON: All right.

MR. GRIFFON: Hold on. Let Jim --

Go ahead, Jim.

DR. NETON: All I was going to say is our position was that NUREG-1400 provided a bounding dose, and we took an estimate of the

worker's intake which was based on breathing zone air samples and probably a G-A sample locally, and we demonstrated that it bounded his exposure.

DR. ULSH: For the highest of 20 workers.

DR. NETON: For the highest of 20 workers. I'm not sure why you wouldn't accept that as demonstrating that this approach would bound that worker's dose.

DR. MAKHIJANI (by Telephone): I have stated the reason, and for the record, in my opinion and my reading of the document, the sample that was represented as a breathing zone sample for Rocky Flats was not representative of a breathing zone sample in the document. I do not believe that it is.

DR. ULSH: We're not talking Rocky Flats. We're talking Simond's Saw and Steel.

DR. MAKHIJANI (by Telephone): I'm just saying that for the record because the term breathing zone came up. I've said what I believe. I don't believe that a time weighted average can give you a bounding dose, and you have to calculate a bounding dose to compare it to another dose that's claimed to be a

bounding dose.

And I just think that's not a mathematical matter anymore, it's how we understand the terms. And I think it's maybe for the Board to define the terms, not for me certainly. As Dr. Wade has said, we do the calculations and the process as directed, but this is the best of our understanding.

DR. LITTLE: Arjun, this is Craig Little. You keep talking about time weighted averages, and it seems to me that their breathing zone sample is exactly that. It's a time weighted average. A person wearing a breathing zone sampler wears it for the period of time that he's in the atmosphere that he's breathing, and it integrates over the period of time that you have it. And you calculate his input per hour based on how many hours he wore the breathing zone sampler and what the intake was over that period. That is a weighted average.

DR. MAKHIJANI (by Telephone): Yeah, Dr. Little, I do understand that. What I am putting forward is that when you have a couple of measurements, for instance, when you have a very high concentration in a certain work

operation, you have to have some confidence that you know what that concentration is. When you have two or three measurements of that that are very different from each other, you're going to have a very high uncertainty.

And therefore, when you try to calculate a bounding dose for that person as to what they might inhale if they only worked a few hours a year, you're not going to get a bounding dose estimate by using the time weighted average. I'm not arguing about what a time weighted average is and whether it's a legitimate number to calculate.

DR. ULSH: I think it's safe to say that we have a technical disagreement that's not going to be resolved today about this time weighted average. So I only have one question for, I guess for Joe.

MR. FITZGERALD: Yeah.

DR. ULSH: About what is the SC&A final report going to say about thorium in terms of whether or not it is an SEC issue? And then I have the same question for the working group. Because we're not going to settle the technical --

MR. FITZGERALD: First off I think certainly we'll take responsibility for what was said on the March 7th meeting. Clearly, we needed to have this exchange that was truncated when apparently we felt we had consensus when we did not have consensus. So I think that's pretty clear just from this exchange that at that particular work group meeting on this issue we did not, in fact, reach closure even though I think at that time, maybe it was late in the day, we thought we were done.

So for the benefit of the work group, your benefit, too, I think we certainly owe, not at this session perhaps, but some resolution so that, you know, this is a technical issue. And I agree, it's a technical issue unlike maybe some of the other more subjective questions that we're addressing. This is a technical issue. I would hope that we would put the Board in the position in two weeks that they would have a resolved technical answer to this issue.

And I would commit that we will certainly work with you to do that. I mean, let the chips fall where they may on the

discussion of time weighted average, whatever. We didn't have this discussion. I feel responsible that maybe we covered too much ground and didn't get as much of this out as we should have at that time because I think there is clearly some missed communication, misunderstanding, maybe some disagreements on the facts that I think and hope we can resolve over the next week or so.

But I think we ought to go back and finish that conversation we started on March 7th, and just put the Board in the position if, in fact, this is, as we put it earlier, a TBD issue after all. I would just as soon not see this as a standing question to be presented on May 2nd.

MS. MUNN: It was a surprise to me to see it in this report because my understanding was that we were going to approach it as a TBD issue, and from this individual perspective it was off the table for our SEC discussions. So to have it appear as it has in this current report was a surprise for me. I was taken aback. As soon as I saw thorium, I --

MR. FITZGERALD: Yeah, I think we probably

talked about some of the reasons why there was some sense of not leaving this question there. But nonetheless I think it's one that we have to resolve and not put you and the rest of the Board in the position of having any ambiguities about this technical question. We certainly have other ambiguities.

MS. MUNN: That's true.

MR. FITZGERALD: We don't want to have any more than that. So I would propose that we address it that way. And again, I certainly apologize for the confusion over the statements made. I think we did truncate that conversation quicker than we should have, and we should have had the benefit of some of this discussion on the time weighted average and what have you. It did not take place --

MR. GRIFFON: Yeah, I can't remember. And part of it might have been that we had your report but SC&A hadn't really had a chance to assess it, and we kind of assumed. And I probably got ahead of myself, too. But I'll finish this discussion where I started which is that if, you know, my feeling is that if -- and it sounds like everyone agrees that the

data's out there that could be used to bound whether you agree with NUREG-1400 or not -- and there's some question on the application or the comparison of this data.

But if we're in a good position to say that we think we have data for these type op -- that are applicable to these type operations at Rocky that could be used to bound, I think we can leave the methodology dispute. We can just say we've got the data.

DR. NETON: Well, I think we did a calculation the first time around. I mean, it didn't agree with NUREG-1400, but that certainly in my mind was a bounding calculation. I don't know what else --

DR. WADE: And the good news from a procedural point of view is that this work group deals not only with the SEC question, but also with the site profile question so this work group will have to deal with that.

MR. GRIFFON: And all I'm asking is that even if Arjun and SC&A are in disagreement of the way you did that bounding analysis with the other data, I don't think he disagrees that there's data there to be used, and it

could be a, you know --

DR. NETON: Well, that's my point though. The first analysis had some arbitrary time factor which we thought was unusually large.

MR. GRIFFON: Well, I'm not saying that's the analysis we would use, but --

DR. NETON: But if one agrees that that time factor's appropriate, then there's an analysis, and I don't see what the issue is.

MR. GRIFFON: Then there's an analysis.

DR. NETON: I'm not saying that's the analysis we would use, but --

MR. GRIFFON: The only reason it comes up this way is because the final, I guess, the, it's sort of like where we were at with the neutron. The proposal on the table was NUREG-1400. So that's what Arjun was arguing against. But I think if we all agree that the data's there, and as Jim just stated, you can choose to use that, but you have that out there. Then I think it goes to a methodological review.

And I mean I won't, SC&A is going present what they're going to present. I would say from my perspective I think at that

point, as a voting member of the Board if I was in a position where I could feel like there's solid data there. I have some questions about NUREG-1400 still because of some things that the contractor's raised, but I feel pretty solid that there's this safety net in the back that could certainly be used to bound. And I'd be comfortable with saying it's a non-SEC. That's sort of where I stand.

DR. MAURO (by Telephone): John Mauro. I have another, maybe a little bit more to add that might be helpful. When we discussed the fact that something is a TBD issue, it's always been my understanding that we leave the door open. That means both SC&A and NIOSH agree that we, at this current time we may not entirely agree on the methodology by which best to reconstruct this dose, but we both agree that it can be reconstructed.

Now that usually means that the door is still open, that the day will come that we're going to revisit this issue again as part of the site profile review. And I think the reason for the dilemma that we have here is it sounds as if that door's been closed.

Namely, that no, we're going to go with 1400, and this is our procedure, and it's not a TBD issue either.

DR. ULSH: No, no, no.

DR. MAURO (by Telephone): And I think that's really where the dilemma lies unfortunately. It's just really a matter of are we right now in the position to agree that, yes, the door is still open. We will certainly be, at some time in the future, discuss this. And if we can do that, then I think we can put it behind us as an SEC issue.

MS. MUNN: Yes.

MR. GRIFFON: NIOSH is saying yes, that the door's still open and it's a TBD issue.

DR. ULSH: Absolutely. We still have confidence in NUREG-1400 but we recognize that you don't. We recognize that there are other data. I think this is probably a heated discussion for another day.

DR. MAURO (by Telephone): Good, because I think that we've been arguing when we didn't have to. As long as the door is open related to the TBD, I think that we're all in the same place.

MR. GRIFFON: I think part of it, and Brant's right. I hate to go back to transfer some ^ for future meetings. I've held back a few past times in the Rocky Flats meetings to be honest, but I mean, we did say, and part of it is this, you know, I think we all feel the pressure to narrow this list.

And I certainly said that it was a non-SEC issue, probably did not have SC&A's full analysis. At that point I'm not sure if we had it or not. But anyway, I think we're all saying now the door's still open for the TBD process.

DR. WADE: And it's this work group that looks at it.

MR. GRIFFON: Right.

All right, are we done with the agenda? No, we're not. I feel like we should be done. I'm kind of --

MS. MUNN: How will this appear in the final report? How's this going, is this going to change? It should change if we're going to present this --

MR. FITZGERALD: Well, I think in the addendum I'd like to take a crack at it in

terms of capturing what we're talking about here. Putting in perspective that this is not a debate on NUREG yes or no, but an agreement that even though there continues to be some technical differences on NUREG-1400 that there are alternate means available, and certainly they'll be explored on the basis, a TBD basis. Just kind of lay that out for the record.

We'll also indicate that consensus was reached in March 7th, but clearly there was a misunderstanding; and therefore, we're at this point. Just something that will put things in perspective and make sure it's very clear.

DR. MAURO (by Telephone): I'd like to add a little bit. I think to a degree this was triggered by one of the criteria that we operate within, within the SEC guidance that the working group put together. One of the items is an example of proof of principle. And I think that's what really triggered this.

In effect, what we're really saying is we certainly can construct at this time several examples of the proof of principle which covers a range of different approaches that theoretically could be used. One of

which we would all agree upon or at some time in the future we believe that we -- how do I say this? The fact that we don't actually have, right now, in the evaluation report, an example of the proof of principle that we all agree upon, yup, that's the way to do it.

I think that's what the problem is with the conversation we're having. And as a result, SC&A is put in the difficult position of saying, you know, you didn't meet that one criterion by providing a proof of principle because that hasn't been done yet. So I'm trying to find a way to say that, well, the conversation that we just had implies that, well, we probably could, if we wanted to right now, develop other methods that would also be other ways in which we could reconstruct or be a little bit more conservative that SC&A believes would be more consistent with what we believe is the right way to do it.

So in a way the problem we're going to have, Joe, is writing our language in such a way in our report that it's able to concur, yes, in principle, that criteria in principle has been met because we believe that such a

model can be applied. But we've got the strange situation we're in because we're on the horns of a dilemma. We want to say it's behind us, but we sort of have in front of us this criteria of proof of principle. I'm not quite sure what to do about that.

And maybe, Mark, do you have some thoughts on that?

MR. GRIFFON: No, that's why I said, I think we're sort of in this middle ground, and I'm trying to, you know, I think part of the reason the Board went down this whole path of proof of principle was that we needed to hear more than we have a lot of data. And so, and then we sort of went through this hypothetical phase when we said, oh, that's getting us in trouble for a number of reasons so we wanted proof of principle.

But I think in this case, I think we've had enough discussion around the other data that is available. It's more of the application of it that there still remains some disputes over it. So it's not just a matter of, so I think in a sense, you know, we haven't completely had a proof of principle

because NIOSH is still sticking with the original model. But –-

(Whereupon, telephonic background noise interrupted the proceedings.)

MR. GRIFFON: You okay, John?

DR. WADE: Somebody's going to have to look at their phone situation. There's a race car sound. We're hearing all kinds of noises.

MS. MUNN: I thought it was a Harley.

DR. WADE: So please mute your phone. If you can't control background noises, hang up.

MR. GRIFFON: But we have the, you know, we have this, I guess my position is that NIOSH has gone further than just saying that we have other data out there. They've looked at it. We've seen what kind of data it is, and now there's just a dispute about application. So I think we have a sense that there is data there that could be used to bound. If you take the position that you don't want to agree with NUREG-1400, there is this other data that could certainly be used or that's my sense anyway right now.

MR. FITZGERALD: I think what John though is pointing out, this is a dilemma I referred to

earlier where we get into proof of principle which is how to gauge the application of that additional data without actually going through a demonstration. And I think that's a general challenge when you get into proof of principle.

DR. WADE: I think the only answer is common sense. I mean, the reality is when the Board votes on an SEC petition not all the dose reconstructions will be done, and that's a reality. So you're going to have look at reasonableness of what is undone and make some judgment. I applaud the Board's desire to see as much as it can, but in point of fact not everything will be done.

MR. GRIFFON: And what I was trying to say here is that we have more than just NIOSH coming in and saying we've got lots of data for that. They've actually examined it further than that and broken it down further than that. So we have more than just data there that, you know, well, we have this data there. We could use it if we want. They've examined it to some extent. So there's been analysis on it.

MR. FITZGERALD: I think certainly we want to go ahead and attempt to frame it up in that context and just see what we can do.

DR. ULSH: Yeah, more than one way to prove a principle I guess.

MS. MUNN: But the first week of May, the only issue with respect to thorium, that's going to be before us, is whether or not our lack of information and lack of ability forces an SEC cover on the thorium issue. And that's not the case. That's simply not the case. The data is there. The dose can be reconstructed. The thorium issue is not an SEC issue.

MR. GRIFFON: The last question I have on thorium, and this should close it out I hope, is -- and I talked to Brant about this. The question of how do you determine who gets assigned thorium dose. And I think you've got a good handle on that. I did ask him earlier if he could say something for the record so we have a good sense of that. But then otherwise I think it's --

DR. ULSH: Right. Mark, you've got to consider or recall that there were a couple

different types of thorium operations at Rocky Flats. The first was the ingot operation that occurred in I believe 1960. I'd have to look and make sure. There is a notation in one of the health physics log books that covers that operation that lists, I believe it's nine individuals by name, and for some of them by badge number. So we know pretty well who was involved in that operation. So that's the thorium ingot operation.

Now, for the thorium strikes I'll just give you a couple of sentences of context here. The thorium strikes were in terms of physical extent of the operations, were a lot like a chemistry experiment in that it was a very small physical operation. In other words the thorium strike was performed inside a reaction vessel which was in turn inside a high integrity glove box.

So there were only a couple of people that were directly involved with the hands on part of the thorium strikes. And those two individuals wrote a detailed report on the chemistry and the process. I'm not going to say their names on the record just because I'm

not sure if I can, in an overabundance of caution.

One of the people we have interviewed, and he told us directly the details of that operation. So those are the two people that you would expect to be at highest risk in, to the extent that there was any for internal exposure in a thorium strike. There were also some additional people who were monitoring health physics coverage because that type of operation has a very high external exposure hazard. So it did receive a lot of attention. But it's primarily going to be those two people directly involved in that.

And then finally, you have the final operation which was the, receiving the finished parts, thorium metal parts, from Y-12. There, there's really no external exposure potential. Now let me go a little further on that. The finished parts were used in models, and I don't really want to go a whole lot farther than that. Originally, and I regret saying this to this day, I said I can't tell you that if there was a high spot, they didn't go grind it off.

Well, at that time that was true. I couldn't say that. But since then, and I think this is in our report, our latest report, we have interviewed the research machinists who, if they had done any grinding or machining of those parts, these are the guys that would have done it. And I can't remember exactly how many individuals that we talked to, but more than one for sure. And they said we just don't recall doing that. I mean, we didn't machine these parts.

So therefore, I don't think there's an internal exposure hazard for that particular operation. It's not so much of an issue. But the other two, the thorium strikes and the ingot operation got a pretty good handle on who was involved. So I think that is how I would answer Mark's question. Unfortunately, for those of you on the phone, Mark has left the room briefly.

DR. WADE: Why don't we take a five-minute stretch break. Mark is not with us. Five minutes we'll be back. We're going to, we won't break the contact line. We will mute the phone for five minutes. Just a quick

break, thank you.

(Whereupon a break was taken from 3:00 p.m. until 3:13 p.m.)

DR. WADE: Mike and the three in the room? Okay.

MR. GRIFFON: I think, since I left a little early, I think we closed out thorium, correct?

DR. ULSH: I'll go for that.

MR. GRIFFON: Closed out again. I know, second meeting in a row.

DR. ULSH: I just repeated our earlier discussion on the record.

MR. GRIFFON: I apologize. I wanted to catch, Jim Neton was leaving and I had to ask him something.

DATA COMPLETENESS

So now we have data completeness, and I'll just turn it over to Joe and ask you to give us an update on this. This is one of the five, right? We're going back to your original four.

MR. FITZGERALD: Really given the span of issues that we addressed in data completeness, and it was quite extensive, I think that was quite a bit of progress to get it down to just

the question revolving around, I believe it's 441. Am I correct, Arjun?

DR. MAKHIJANI (by Telephone): No, 4-4-4.

MR. FITZGERALD: Four-four-four, I'll get it right, non-plutonium workers and the questions of a bounding analysis. And again, I think we tend to in these other issues talk about proof of principle or try to demonstrate if the approach is bounding more than whether the concept is, itself, valid. And I think this is one of those cases.

Arjun, do you want to add?

DR. MAKHIJANI (by Telephone): Yeah, I mean, we looked at the data that Brant gave and all the other data that they put up, that NIOSH put up on the O drive, and Brant is right that there was a lot of data on Building 44. Now we didn't see any job types identified in that data.

Brant, do you have any job types identified with Building 44 data?

DR. ULSH: Well, Arjun, I think to get that type of information you would have to link the individuals' hard copy file and look at their cards.

DR. MAKHIJANI (by Telephone): So we couldn't discern right away how that data would be used for bounding dose. There certainly does seem to be extensive data for the period about which we were concerned in terms of data completeness for that building. So presumably something could be put together, but there is a proof of principle issue that's why it is still mentioned.

I can't remember now exactly the words in our report, but I think that is the context. Let me just go there. That is the context in which it is brought up for Building... I think all it says is NIOSH would either show that foundry workers are the workers maybe at risk of high shallow dose were routinely badged or sufficient data are available. There are certainly quite a lot of data, but we haven't been able to parse it. That's why it's still here.

And there's a not a dissimilar issue for Building 81 where there are actually not data, but where the risk of exposure, potential for exposure, appears to be lower, but similarly, proof of principle is not

there.

MR. GRIFFON: Are there other data completeness questions, Joe, or should we take them one at a time here and let Brant --

MR. FITZGERALD: Well, the ’69, ’70, I think, we probably should address separately. I mean, it’s a separate issue, but it’s data completeness as well.

MR. GRIFFON: And I still haven’t probably haven’t read every detail of the report. There’s the question of the pre-’69, I don’t know if we ever, I mean, I think we’ve kind of attempted to get our hands around this, but the question of there were some workers in your analysis that didn’t have data from, say, ’64 to ’69. They had no records, and it was the period when we thought that everybody was going to be monitored. But then apparently, that wasn’t necessarily the policy going forward.

And then I had asked as a follow up can we examine some of these cases to see, in fact, if they had blanks in their record and zeros in the database. And I think you did look at that, right? And you didn’t find --

MR. FITZGERALD: No.

MR. GRIFFON: -- anymore years with that kind of problem.

MR. FITZGERALD: No, no, it's a span that we're talking about

DR. MAKHIJANI (by Telephone): Yeah, that's right, Mark. I mean, I don't think that we did any sort of extensive or statistical analysis. But we did, I certainly looked at a couple of them. I don't know, other members of our team looked at more, and we did not, I did not find that the gaps had been replaced by zeros as there were in '69.

MR. GRIFFON: I guess we have this odd problem in '69 and '70 which NIOSH has taken an action to correct or at least to mitigate. And we have no evidence that that has occurred in any of the other years or not as far as you can find.

MR. FITZGERALD: Not as far as the sampling that we've done nor has NIOSH done. I mean, I think it's clear right now it's just the situation for those two years although we still have not established a firm endpoint, but nonetheless, it looks like it's combined

in ’69 and ’70.

DR. ULSH: We’ve got a couple of different issues, I think, that we might be mixing up --

MR. GRIFFON: Start off with Building 44, the early year issues, I guess.

DR. ULSH: There was the question -- I’m trying to think of how it went at the last working group meeting. SC&A has raised the question, I think this -- Ron, you can correct me if I’m wrong, but I think originally the motivation for the question was that workers in the uranium foundry would be expected to have a significant shallow dose potential. And so you all were concerned about whether or not OTIB-58 would bound those workers. I think that’s where it originally came from. Am I right about that?

DR. MAKHIJANI (by Telephone): Right, yeah, that’s correct, Brant. That is correct.

DR. ULSH: At the last working group meeting, as Arjun mentioned, I handed out some data that showed that the Building 44 workers were indeed monitored. There was an example that I handed out. And we’re talking about in the ‘50s now. And I think that the concern

was that since SC&A had found gaps or periods without monitoring in the ‘50s for Plant B workers, they were concerned about whether or not the foundry workers would be monitored.

And I pointed out that the foundry was, in fact, not in Plant B. It was in Building 44, and then so the question became, well, okay, fine, but were they monitored. And I handed out that example, and then it was requested at the last work group meeting, it was an action item for us to supply the monitoring data for other years in the ‘50s.

MS. MUNN: A few foundry worker dose sheets.

DR. ULSH: Now one thing that’s going to be difficult, and Arjun has alluded to it, and that is that what we have on the data that I supplied is the dosimetry results for people who worked in Building 44. And I’ve purposefully refrained from calling them foundry workers because that is really an artificial construct.

Building 44 is where the DU foundry was. That is certainly true. But it’s not a defined job title of foundry worker. That’s not a job title. Like for instance in

Building 76 you have process operators. Well, there isn't a corresponding foundry worker job title. So there is some degree of difficulty in determining from the Building 44 results who was doing foundry-type operations.

And that's not the only thing that occurred in Building 44. I mean, certainly some of those operations were of the type that would lead you to high shallow dose potential. And I think one that was mentioned in Putzier's memoir and also SC&A has mentioned it I think from that source was the breakout molds from DU. And the contact dose rates could be very high on those. So I think that's kind of the background of where it all, where the issue came from.

When I provided, after the working group meeting, the last working group meeting, when I provided the depleted uranium -- I'm sorry, the Building 44 dosimetry results, I think that spanned most of the '50s although I'd have to go back and look because that was a concern that was expressed about those years.

Now in terms of peeling out the

foundry workers, well, like I said, I mean, that's an artificial construct anyway. You could kind of make a guess by looking at the dosimetry results who had the highest ones. Then you could identify who was foundry and who wasn't.

MR. GRIFFON: Assuming the foundry workers were monitored, right? I mean, that assumes that they were at least in the cohort.

DR. ULSH: Yes, exactly. And we have to, I mean, we have found individuals whom we know because they're people just through personal knowledge, I mean, not me but other people on the ORAU team know those people and know what they did. And so for those individuals we could peel them out, and they were monitored, but it's a big site.

There's a lot of people there. There is some degree of assumption here. And I think you have to go with common sense. I mean, why would they monitor Building 44 workers and not monitor the people with the highest exposure. And you might ask that question --

I know, Mark, you're giving me the

look.

MR. GRIFFON: I didn't give any look. I just wanted to use your words back at you. Seventy-one sound familiar?

DR. ULSH: But there is some evidence that at least some of those people were monitored. If you look at the Building 44 dosimetry results, some of them are substantial. So it would certainly make sense, and I can tell you that the badging policy in place at the time was -- you know, we've heard this before -- if they had a significant exposure potential defined as greater than ten percent of the regulatory limit at the time, they were to be monitored. And that relies a bit on the judgment of the radiation protection staff in place at the time. That is certainly true.

So what we have established is maybe not a definitive piece of paper that says we monitored foundry workers, and I've told you why we wouldn't expect to find that, but what we have established is that there were a significant number of people in Building 44 who were monitored. Some of them received very, well, some of them received some

appreciable external doses, and certainly you can see the types of ratios of shallow-to-deep that you would expect from these foundry-type operations. So as Mark mentioned earlier about a weight of evidence approach, I put on the table that the weight of evidence certainly suggests that they were monitored. We have monitoring for all the years in the ‘50s I think. I have to look, but, so to that extent I think we’re left with trying to differentiate out the people who may not have been involved in foundry operations in Building 44, and I would go to the dosimetry results. People who have low dosimetry results would be good candidates for that.

DR. MAKHIJANI (by Telephone): Low dosimetry results?

DR. ULSH: The people who, Arjun, I would suggest to you that if I were trying to separate out people who did hands on work with the DU metal who might be expected to receive appreciable shallow doses, I would use the dosimeter results as the source. I mean, if you find people who have a high shallow-to-deep ratio, those would be good candidates.

And if you find people who have zeros, they're probably not good candidates.

DR. MAKHIJANI (by Telephone): So, okay, I didn't understand what you said.

DR. ULSH: So, I don't know. That's where I am with this. Now that's for the '50s. Is that --

MR. GRIFFON: Just to refresh my memory, Joe or Arjun, the individuals that we looked at with the gaps or the questionable periods in the '50s, were they identified in Building 44 or are we, another question came up out of that review?

DR. ULSH: Plant B. They were in Plant B.

DR. MAKHIJANI (by Telephone): They were primarily in Plant B. There were a few scattered in other places. I do not believe that we actually found anyone with a history in Building 44 that was part of those gaps. I'm doing this from memory, but I think that Brant is right about that.

DR. ULSH: I think that's the way I remember it, too.

DR. MAKHIJANI (by Telephone): There were other places than Plant B, but I don't believe

that any of them were in Plant A. Now, well, I had a question, but I don't know where you were headed.

MR. GRIFFON: Did we conclude -- yeah, just let me finish my thought here. Did we conclude that the gaps in those workers seemed appropriate given their job type in the plant? I mean, that's a question that I had just aside from this Building 44 question.

DR. ULSH: Yes, that's what we concluded. I can't speak for SC&A.

MR. GRIFFON: Right. Well, I'm asking if SC&A was in agreement with that.

DR. MAKHIJANI (by Telephone): We found, we agreed that we did not find any gaps at that time in the -- and Ron, correct me if I'm wrong. We did not find any gaps among the plutonium buildings in terms of the way we define the gaps, that is, one full year of no monitoring data available. And so the --

MR. GRIFFON: But to my question --

DR. MAKHIJANI (by Telephone): -- yeah, it was confined, it was in the non-plutonium areas generally. And within the non-plutonium areas it was mostly in Plant B.

MR. GRIFFON: But where do you come down on that is the question.

DR. MAKHIJANI (by Telephone): Yeah, we looked, I agree with Brant that, I looked at the O drive. I don't know, Ron, did you, there are data I believe in most or all years for Building 44, and that had been the center of our concern in terms of if it were the non-plutonium buildings, you know, and the size of our sample was overall statistically significant. But then we were then taking out a small piece of it for the '50s, and so --

MR. GRIFFON: But I'm still asking about these ones we reviewed. None of the ones we reviewed were in 44, were they?

DR. MAKHIJANI (by Telephone): No, no.

MR. GRIFFON: So I'm still asking about the ones that you reviewed that were non-plutonium in the '50s. There were gaps. Brant gave an explanation as to why. I mean, basically I think because of the ten percent and the job title information or --

DR. ULSH: That is true, Mark; however, Arjun mentioned the gaps that were primarily seen, not exclusively, but primarily in Plant

B. That's Building 881. And we talked last time, and we will again this time, how those workers were not monitored externally until the fourth quarter of I believe 1960. And this is an issue that I'm sure we're going to get into about how we cover those people.

DR. MAKHIJANI (by Telephone): Yes, that's a separate, that was a separate question.

MR. GRIFFON: Okay, so let's stick with 44, but I just wanted to understand --

DR. ULSH: Yeah, but in terms of the 52 cases that we looked at for the data completeness evaluation, it was our conclusion -- now this is only NIOSH -- that the gaps that you see in the periods with no monitoring are explainable primarily by two factors. One, in several instances the people weren't onsite, or in some instances, so you wouldn't expect there to be monitoring results.

And two -- and I'm not sure which was the more common. I think maybe this one was the more common -- the jobs that they were doing as discerned from their job history cards. For instance, if they were a janitor and you know --

DR. LITTLE: Administrative secretary.

DR. ULSH: -- yeah, those would be consistent with them not being monitored. And you're right, Mark, this is another thing that you mentioned in your original question I think. Originally we had assumed that everyone was monitored after '64. And that turns, that's not the case throughout the history. It appears that there was at least a small number of Rocky Flats workers who had low exposure potential were not monitored.

So that's kind of what we found. I don't want to go further than that and speak for SC&A.

MR. GRIFFON: Just sticking with the '50s still, do we, SC&A, does SC&A agree that there's gaps? Do you accept the explanation given by NIOSH for those gaps I guess is the --

DR. MAKHIJANI (by Telephone): You know, we didn't find any different explanation. The question that arose from the existence of those gaps since there were gaps for full years wasn't about what the policies were at the time or the judgments of the health

physicists which, as we saw this morning, weren't 100 percent correct.

The question that arose from the fact that there were these gaps was does the, can the coworker model be demonstrated to cover those gaps in a bounding way or is there another set of data that can do the same job and can that be demonstrated. So as I mentioned, this wasn't a question about monitoring practices. It was simply left at the proof of principle level just following the Board's criteria of what is required to review as we review NIOSH's work on SEC.

MR. GRIFFON: And just to stay with that thread, are we convinced that the coworker model bounds those years?

DR. MAKHIJANI (by Telephone): Well, you know, we have not, as I said, that's why I looked, when I looked at Building 44, I saw lots of data there. We did verify that so that is correct. I believe that Brant is right. I think there are data, my memory is the same as his, there are data for every year and quite copious, but no job types.

And since there's a particular types

of jobs that had the high exposure potential, you know, the only question remaining reviewing that data since the last meeting was how are we going to identify those people and make sure there's an approach to covering them and/or make sure that they were all monitored since their job types are not mentioned.

I mean we've felt all along that, you know, people made the best judgments that they could at the time, but they're not infallible and that some data are preferable before you affirm an idea that somebody was doing things right all the time. And I think as it's turned out with neutron doses this morning finally, we have an idea that maybe an initial judgment was not quite right when it tried to be verified by numbers. And so it's the same concern here is that can you show it.

DR. ULSH: Well, I think that's true, and let me be more specific here. In this situation the thing that originally I think maybe caught SC&A's attention was the memoirs of Ed Putzier. And if you look at his memoirs, he identified that this was recognized as an issue back in the '50s. And

they had housekeeping issues; they had pretty high contact dose rates.

And I would certainly agree that we're talking about, when we're talking about the radiation protection staff, I agree with Arjun's characterization. They were doing as good a job as they could. I think I would go a little further and say in general they did a pretty good job, but they're not infallible, and we saw that this morning, and that is true.

However, I don't think you can necessarily equate that to the situation in Building 444 because it's clear from Putzier's memoir that it was recognized early on that there was significant exposure potential here. Now again, I have to go back to it. This is just the weight of the evidence, and you certainly see that in the dosimetry results there are people who would fit the profile you would expect if they're doing the type of work that we're concerned about here. You know, high shallow-to-deep ratios.

So at least some of them I would say are there. I mean, I would say that the

weight of evidence suggests that they were monitored. I mean, if the hazard was recognized as Putzier's memoir indicates that it was, and you have dosimetry results for workers who were in Building 44 who may not be able to say that Worker X was a foundry worker, and let's go look and see if he was monitored. But the weight of the evidence certainly points in that direction.

It's just a question of for the working group and the Board, do you feel the weight of the evidence is strong enough.

DR. MAKHIJANI (by Telephone): Brant, could I ask, did you actually, is there a table or something of the ones with high shallow-to-deep ratios? I mean, you seem to have examined it from that point of view.

DR. ULSH: No, Arjun, I'm speaking more anecdotally from when I was looking through the dosimetry results. I haven't done a formal analysis or anything.

MR. BUCHANAN: What years of operation looked at that or what years of operation that the uranium foundry workers ^.

DR. ULSH: Ron, let me go on from Putzier's

memoirs. I think he referred to the ‘50s. I’m not sure if he was more specific in that. And to be honest with you, Ron, I don’t know if that would extend up into the ‘60s and, I mean, I don’t know. You’d have to look at the building history, but --

DR. MAKHIJANI (by Telephone): Well, Putzier’s concerns about it, you know, until the time he wrote, but his main concerns were about the early years. I don’t believe that he actually wrote down an interval of years for which he was mostly concerned. I think he said early years.

MR. GRIFFON: This data in the worksheets, is it part of your TIB-58 spreadsheets now? Is it, would it be included in that annual data or is this kind of new data that you’ve uncovered in response to this question?

DR. ULSH: No.

MR. GRIFFON: No, it’s been out there.

MS. MUNN: It’s individual dose data, right?

MR. GRIFFON: Yeah, is it?

DR. ULSH: The worksheets that I provided are individual dose data, Wanda. And, yes, they would be reflected in OTIB-58. Keep in

mind that OTIB-58 prior to 1970-something has a penetrating dose and a non-penetrating dose. And, yes, we certainly expect them to be in HIS-20.

DR. LITTLE: As a lump sum?

DR. ULSH: As a lump sum.

DR. LITTLE: Prior to ’76.

DR. ULSH: Thank you, that’s what I was reaching for.

So, yes, Mark, they are in there.

MS. MUNN: So the weight of that evidence would lead us to the conclusion that we do not have an SEC issue here.

(Whereupon, hotel staff interrupted the proceedings via telephone.)

MR. GRIFFON: So Wanda’s conclusion is --

MS. MUNN: That the weight of the evidence would lead us to believe that dose reconstructions are in fact possible from the data that’s there, and this, therefore, is not an SEC issue.

MR. GRIFFON: Yeah, I think the final, is there any, there’s no way to, I forget these ^. I was just looking for these worksheets on my drive. I know I have them somewhere. Are

there exposure IDs or any way to link these to individuals? You said --

DR. ULSH: I'm trying to remember, Mark. I know that they were given by name, but we might have main number in there. I'm not sure.

DR. LITTLE: Are you talking about the worksheets?

DR. ULSH: I'm talking about the worksheets that we put, we mailed out to the working group for '50s, Building 44.

DR. LITTLE: I think they have names. They have mang numbers, too. I'm 99 percent sure that's true.

MR. GRIFFON: Because you say even if we, I'm just wondering if I cross-checked some of these names with their cards -- they may not be called a foundry worker anyway. Is that what you're telling me?

MS. MUNN: They aren't.

MR. GRIFFON: There's no such job title so it's not, it's going to be --

DR. LITTLE: They're probably a chemical operator or a assistant operator or something of that nature.

DR. ULSH: You've got to remember, Mark, that the dosimetry results that I provided to you are not limited to the claimants, and we have the job history cards for claimants.

MR. GRIFFON: So there may not even be any matches.

DR. ULSH: Yeah, you just have to, needle in a haystack.

MR. FITZGERALD: Would those be considered for, you know, just as on external dose, it's attributed to those working radiation areas ^ I would think that would be ^ that category, right?

DR. ULSH: Yeah, I mean, if we had an unmonitored person who did foundry-type operations, I would say -- Mutty's not going to jump across the table and reach me -- but I think that we would certainly give 95th percentile to people like that.

DR. WADE: Mike, are you still with us?

MR. GIBSON (by Telephone): Yes.

MR. FITZGERALD: Arjun, are you still with us?

DR. MAKHIJANI (by Telephone): Yup.

MR. FITZGERALD: Any reaction on that before

we get to --

DR. MAKHIJANI (by Telephone): Well, no, I think, you know, a decision about whether something's SEC at this kind of stage and whether the Board's criteria is being fulfilled clearly belongs to the working group. The comment that we made is there that certainly the data, there are plenty of data. They're not identified by work type, you know.

I take Brant at his word that there are some data with high shallow-to-deep doses, and I think that then certainly the rest is for the working group to say how much of a proof of principle they want.

MR. GRIFFON: And I think if we're reasonably convinced, and I think Wanda is, that the most exposed were monitored, you know are in this population, I might amuse myself if I had time to check some of these cards. But I think it's going to be kind of fruitless because I think it's probably, you're not going to find a foundry operator or whatever foundry worker.

So as long as, and I have no reason to believe they wouldn't have had some operators

in that population, I think they probably have gotten the bounding job types covered. So I guess that's the question is if we've got the gaps and they apply. And I also, looking at the procedures, I have no reason to believe that they wouldn't assign a 95th in this scenario for the external coworker model because that's different than the internal coworker model.

And Mutty's nodding his head, let the record show. And I think that's the general rule is that if it's in a radiation operating area, then you --

MR. FITZGERALD: That would make a difference from my standpoint. One can't close that loop on this so-called proof of principle. It's almost the, what's the overall available cautions or conservatism. So I think that will make a big difference.

MR. GRIFFON: So I think from that standpoint it's, I think we're satisfied with that, at least I am personally.

MR. FITZGERALD: What's next?

MR. GRIFFON: I mean when we, let's see. I just want to go back to that Building 81

question that you mentioned. Is that part of this discussion or is it another item? I think bring it up now I guess.

MR. FITZGERALD: Well, the 81 issue devolved from discussions on the other issues of the last couple of work group meetings. I guess I don't have a bin for it. We certainly cite it in the completeness analysis of it, didn't provide a bottom line conclusion as to whether it was an issue or implication there.

Arjun, do you remember how that came up in terms of part of this discussion and it just kind of fell out of it.

DR. MAKHIJANI (by Telephone): Well, Building 81 is distinguished in two ways from what we've just been talking about in that in Building 44 there are data. They've been identified, and there are data throughout the period, their names attached to them if I remember correctly and so on.

In Building 81 the '52 to '59 period does have lots of workers who were not monitored. They were working mainly with uranium. There were some other areas besides Plant B where there are significant gaps in

the sense of not being monitored for whatever reason. And we didn't see that the exposure potential had been quantitatively characterized so as to be sure that it was covered by the coworker model.

And so that is the issue. It's a different kind of proof of principle issue is that there is a coworker model, but there's no demonstration that it applies to this group of workers who were production workers who were not monitored and whether it covers them adequately or not.

So, I mean, that's where we left it. We haven't seen anything more specific than that. And I think we do have agreement that in those areas there were workers who were not monitored because they were felt to be not at high exposure potential.

DR. ULSH: Do you want me to go ahead?

MR. GRIFFON: Uh-huh.

DR. ULSH: You are correct, Roger -- Roger, Arjun -- we do have agreement that there were, there was a time period when the workers, the uranium workers in Building 81 were not monitored. That time period is prior to the

fourth quarter of 1960, I believe.

DR. MAKHIJANI (by Telephone): That's correct.

DR. ULSH: Okay, thanks. And that was discussed by the ORAU team, that was pointed out in a conference call that we had with SC&A. And that came up in that that you mentioned that you had found a lot of unmonitored periods or gaps in the Plant B workers in the '50s. And so that's when we said, yeah, that's because they weren't monitored until the fourth quarter of 1960.

DR. MAKHIJANI (by Telephone): Right.

DR. ULSH: Now I don't think it's, I don't think I would characterize that we have not performed an analysis to show that they were bounding. I would say that we have presented an analysis and maybe SC&A is not convinced by that, but we certainly have presented an analysis.

I'm looking at our response to SC&A's draft report on this chapter, Chapter 8, the "Data Completeness Evaluation," and we presented an analysis that shows that once they were, okay, first of all the reason that

they were not monitored --

DR. MAKHIJANI (by Telephone): Can you give me the date of the paper you're talking about? There are a lot of papers here. Is that 2/26/07?

DR. ULSH: Sounds right. I don't have a date on my copy here but it sounds about right. And I'm on page five if that's the right report.

So the reason they were not monitored is because they were judged by the health physicists at the time to be at less than ten percent of the exposure potential. So what we see is that when they were monitored in 1960, and we also have results for 1961, that certainly turned out to be the case. They were at less than ten percent of the exposure potential.

But now we're into a situation where we have to back extrapolate again because we have some monitoring data here, and it certainly shows that in the years when they were monitored, 1960 and '61, number one, they were at less than ten percent of the exposure potential, but number two, and more

importantly and more relevant for what we're talking about here, the coworker dose that we would apply was bounding in those years.

Now, we're back to this back extrapolation. What do you do about the period when these workers were not monitored? Well, again, you have to be cautious when we do this, when we back extrapolate. And we have to consider whether or not there were changes in the source term that would have compromised our conclusion.

Now, I do want to be clear that we're not back extrapolating data. What we're doing is back extrapolating the presumption that our coworker model is shown to be overestimating or bounding in '60 and '61, and that assumption we are going back and saying that applies also to this earlier period when these workers were not monitored.

And we presented an analysis that showed that when they were, it was overestimated by pretty large factors. Factors of, and we're talking only about the positive doses that were measured, factors of 13, factors of 24, factors of, well, there's a

three; there's a four. So we're not talking by a little bit, by ten percent. We're talking about the coworker model overestimated by a significant, and would be bounding by a significant margin.

And so what we considered were, were there source term changes that would you would expect to make this assumption suspect. Well, yes, as a matter of fact, there were source term changes. If you look at the amount of uranium that was processed at Rocky Flats beginning in the '50s and going up into the '60s -- I can't give you exact amounts because that is classified.

I can tell you the trend though. And the trend was that they started low in the '50s, low inventory, low through-put of uranium, and that increased steadily throughout the '50s up into the early '60s, up into the middle '60s where it kind of plateaued right there in the '60s. And then in 19 --

Maybe you can help me out, Bob, 1965 was it?

Nineteen Sixty-five those operations

were transferred to Y-12. So it goes away in 1965 at Rocky. So what we see here is that, yes, there were source term changes.

If you look at a graph, and you see a steadily increasing line, but that gives us confidence that at least on this source term question, if we use this assumption, or I mean this phenomenon that we've seen in the '60s, well, it doesn't make sense if you consider that factor that they would have been at higher exposure potential based on the source term because the source term was higher in the '60s than in the '50s.

Now, if that had been the reverse situation, if they had been higher in the '50s and gone down, well, you would say, well, when they were monitored, they were at lower exposure potential. But not based on the source term for what we see at Rocky Flats. This is all presented in our response here that I've referenced.

We also don't see any changes in the building configuration similar to the discussion this morning in terms of improvements in shielding or anything like

that that would have depressed the doses that you see in the ‘60s compared to the ‘50s. So based on that we just didn’t see anything that would call into question this fact, well, what we observed that our coworker dose was, in fact, bounding and not by a little, by a lot.

So in order for this not to be acceptable, in order for someone to say that, well, the coworker dose isn’t bounding for these workers back in the ‘50s, you would have to say, number one, their doses were going up when the source term was going down and not by just a little bit but by factors of three, 13, 24, a lot. It just doesn’t sound plausible to us.

DR. MAKHIJANI (by Telephone): All right, thank you for reminding me. I remember this, and I believe there is a discussion of this some paper of ours, probably also in the report that we’ve given you.

I have the table that you’re referring to in terms of the coworker comparison in front of me, and I think this back extrapolation issue in terms of the source term has been an issue of some discussion.

Certainly, we're not arguing about whether the source term was going up in the '50s because that's a matter of record for you looking at the classified data.

The thing that we've said is that the source term quantity matters only in one context on a priori basis in terms of exposure potential is that if you've got workers who are going from full time to part time because the source term is going down, then you've got a lower exposure potential. So long as you've got some workers who are doing full-time work, the source term is irrelevant because then you're simply multiplying the number of workers who are exposed and it doesn't matter for individual dose whether the source term is going up or down at that point.

What matters are the industrial hygiene conditions, the ventilation conditions, the area dust doses. And there are certainly plenty of examples where you have got relatively small source terms, for instance, typical with AWEs where you have gotten much, much larger intakes than with much larger operations because the industrial

hygiene conditions are much worse. So I think this point should be clear that unless there's part-time work, the source term is not relevant.

It's the industrial hygiene conditions that are relevant, and that's why the back extrapolation issues are kind raised here. Is there some data to validate this back extrapolation from the time, like dust data and so on. And I believe this question is raised in our report if I remember correctly.

DR. ULSH: Well, let me talk to that. When you're talking about dust, external dose from airborne material that, I mean, I assume that you're talking about airborne material that settles out on surfaces, not on people's skin.

(no response)

DR. ULSH: And hearing no objection I'll go with that. So what you have to, let's try to put together a scenario where our assumptions would not work. What you would have to assume -- now remember, starting in the '50s, at some point, '52, I don't know the exact year that the uranium operation started.

But operations are just getting

started so industrial hygiene operations at worst, what you're worried about is an accumulation of airborne material on equipment and surfaces. And that's going to take some time to accumulate. And if you're doing good hygiene, good industrial hygiene, cleaning up after every shift or every week or whatever, then really that accumulation factor is not going to be too much of an issue, and --

MR. GRIFFON: I think that's the general point though. I think, I mean, every site I've been at, work practices and hygiene improve with time, and I'm not sure. I think the source term is sort of irrelevant because your workforce could go up with the source term increasing, and you know, personal exposures may not be affected by that. I guess the, we're at the same sort of question of do we have some hypothesis or do we have evidence that supports, you know.

DR. ULSH: Well, what I'm saying, Mark, is that it would certainly be an issue if the trend were going the other way. If you had higher inventories in the '50s going down into the '60s, then we might be concerned. No, I'm

not saying that it's sufficient to say that the source term was increasing up into the '60s. I'm just saying that's one piece in the weight of the evidence.

Now in terms of the industrial hygiene and this question of dust that accumulates and what not, you have to take into account here that the primary source of external dose is going to be the hands on work with the uranium metal. I'm not saying that the dust, the dose from the dust that accumulates is insignificant, but it's certainly less than the operations that involve the hands-on metal work. So that's not going to be an issue in terms of changing hygiene conditions in the '50s or '60s or whatever.

I just, what you have to say here is that the, at the same time that the source term was increasing into the '60s, the hygiene conditions were so different in the '50s that they led to such an increase in external dose from this accumulated material on surfaces that it would counteract not only the source term, if you even think that that's an issue, but it would have to overcompensate the fact

that we're overestimating by factors of, by large margins.

And I just don't think that that's plausible. I mean by factors of 13, factors of three, factors of four, and you have to also realize that these factors are underestimate because we only considered workers with positive doses in this analysis. It doesn't count anybody with zeros. So, yes, could you have had a little bit of higher exposure from dust on the surfaces in the '50s? Sure you could, but is it going to be so much that it would make our coworker analysis --

MR. GRIFFON: The dust on surfaces makes it --

DR. MAKHIJANI (by Telephone): Is it a dust --

MR. GRIFFON: Arjun, hold on.

The dust on surfaces makes a good sort of sell in this situation, to sell your model. But I think it's more than just the dust differences over time. I think, I question whether work practices from '52 through '60, I doubt they were stagnant. I think they

learned a lot about uranium handling in that period from ’52 up through ’60.

And maybe shielding, I mean, you’re saying we have no evidence that shielding didn’t significantly change. You know, you see some of these papers, and they just say, you get suggestions over time that we’ve learned that, we modified the work practice here so that the workers aren’t handling this directly. They’re using this approach instead, or whatever because we found high hand exposures. I mean, they were constantly doing that in the early years.

So I guess that’s my question, not so much to harp on one particular item or another, it’s just that trend. And even if the production values were going the other way, I think the production source term is, I’m not sure it’s completely relevant because I think if you’re scaling down production, you’re scaling out workers. And some individuals may just be doing, their particular personal experience may not be changing at all. You’re just modifying the workforce in the area maybe. I don’t know.

Maybe. So I can hypothesize that way as well.

DR. ULSH: If that's not a factor of concern to you, the source term, then ignore addressing ^.

MR. GRIFFON: I'm just saying. I'm just saying.

DR. ULSH: I understand. I want to address the questions that you consider compelling. And if that's not one of them, that's okay. That's fine.

But in terms of industrial hygiene, I again have to go back to the fact that, yes, it is certainly possible that industrial hygiene practices improved from the '50s to the '60s. Is that maybe enough to counteract the fact, number one, you have to realize that the health physicists at the time were judging that these people were at low exposure potential. Now might there have been one, two, a couple, some that exceeded that? Sure, there might have been. I can't say that there weren't, but we just don't see people exceeding the 95th percentile of the coworker doses that we're assigning when they were monitored. I mean, we're overestimating by a

factor, by large factors. And the question I --

MR. GRIFFON: And I don't remember that analysis to be honest with you. I mean, I looked at it initially, but I don't even find it.

DR. ULSH: The question that I think you would have to wrestle with is the possible effects from possible changes in industrial hygiene practices, would they be enough to make our coworker model not bounding? That's the question you have to wrestle with.

And also I know that people have varying confidence in the judgment of the health physicists at the time. We know that they were not infallible. We do know that, but they judged at the time that these people were less than ten percent of the exposure limit, and when they were monitored that is certainly borne out. Might they have missed a couple? Yeah, they might have. But would that have put them over 95 percent of the people that were monitored?

MS. MUNN: One has to use common sense in approaching these things and use the personal

knowledge that we have of the individuals and their motivations. And certainly the health physicists of the time were extremely concerned over the health and welfare of the workers. We know that.

MR. GRIFFON: Oh, sure, but I believe they probably had priorities, too --

MS. MUNN: Oh, undoubtedly.

MR. GRIFFON: -- and this type of facility I don't know if uranium was necessarily a big problem. In the early years I don't think people were so real hepped up over external exposures to uranium, and probably rightly so, I mean, so I'm not sure they were thinking let's worry about who might exceed ten, you know, that's my question.

MS. MUNN: That's probably true, but we do have some data to look at.

MR. GRIFFON: Yeah, so not for these people we don't. So that's the question. I think we just have to grapple with those figures you gave. There might be differences. Are they really going to throw it off by a factor of three to ten or whatever the examples you gave.

MR. FITZGERALD: It seems like we have two or three variables at work side by side, so I don't know if there's a way to know how they offset, I think your point's well taken, a source term. But then the question of whether practices improved, it seems to me --

DR. ULSH: They might have.

MR. FITZGERALD: They might have.

DR. ULSH: It seems plausible to me that they did.

MR. FITZGERALD: I think whether or not the measured doses compare favorably with ^.

DR. ULSH: It would be nice to lay monitoring data for these people in the '50s before you. That doesn't exist. I've put the weight of the evidence that is available on the table, and I think it's up to you guys now to decide what you think of that. I mean, I don't have a lot more to add to it.

MR. GRIFFON: Arjun, do you have anything else?

DR. MAKHIJANI (by Telephone): No, I think Brant is right about that. I think that he's properly characterized the numbers. The 1960 data are as they are. And as I said, the

point, at least as we have discussed internally, and as somebody that's written a lot of the stuff that's been discussed today, the principle I've tried to follow is to look at the Board's criteria and our operating procedures on what we're supposed to evaluate and present that evaluation to you.

So I think, you know, at that point I agree with Brant. The numbers are, we have no dispute about, the numbers, no argument. It is true that the 95th percentile is several-fold the doses that were measured in the fourth quarter of 1960. And if that is being compared to the coworker model, the coworker model at that time was as characterized at the 95th percentile. So then it's the Board's judgment call about back extrapolation because these other factors we don't know.

MR. GRIFFON: I think the only other piece we have to consider in this is how, if we have these different groups or this one group, I guess, that was not included in the monitoring, one question is would a 95th bound and I'm leaning toward a yes as that answer.

The second question, I guess, is for

those years, ’52 through ’59 --

DR. ULSH: Third quarter of 1960.

MR. GRIFFON: ‘Sixty, okay. How does this population of workers not being included in the full dataset affect the coworker model itself. Because you use not only the 95th, but you’re going to use the 50th sometimes for some workers.

DR. ULSH: Well, that’s an interesting question, Mark.

MR. GRIFFON: Sort of a secondary one, but --

DR. ULSH: I want to think about how to answer that. I can tell you that they were judged to be a low exposure potential. When they were monitored, that judgment was supported. When they weren’t monitored, you can make your own conclusion. In general, as a general principle, if I exclude a group of workers who were at low exposure potential, you accept that they were, that would be claimant favorable. That would shift my distribution to the right.

MR. GRIFFON: No, no, I agree with that, but in looking at the 44 data or if I recall what

you said about the 44 data, like I said I couldn't find it in real-time here on my computer. But going back to 44 for a second, for those you indicated that maybe it was by your personal knowledge of the names or whatever.

You said some of them were management, and some of them were operators, some of them were, so it makes me wonder was this judgment of ten percent of the RPG based on like building or was it on job or, you know. And managers were included in this population that was monitored in the early years. It makes me wonder if, in fact, --

DR. ULSH: I think I recall -- yeah, I understand your question. I think I recall our discussion at the last working group meeting about this because the question came up. What was the unit of decision. Was it an individual-based question or was it a building-based question? I think the answer that we heard, I'll have to defer to people who were actually there, but I think the answer that we heard was it was by work group, by org code.

Now that conversation occurred in the context of Building 44. I don't have any reason to think it would be different in other places. Is anyone out there on the ORAU team that can speak to that?

(no response)

DR. ULSH: No one's going to take the bait, huh?

DR. LITTLE: Is anybody out there from ORAU?

DR. ULSH: I can get back to you with an answer on that, Mark.

DR. LITTLE: Well, I think to some extent I can speak to that because the, certainly we did find one indication of people who were in the uranium foundry, and it was in a personnel strength report. It was from 1962 or '3 it seems to me. And those, a strength report is essentially a compilation of how many people worked in various org codes in various buildings.

And that's where we identified, we verified that these people that we found for the later '60s were actually in the foundry. What we, and those are all classified by org code and by building code as are most of the

data worksheets are not, but the so-called supervisor reports or the health physics exposure reports are all classified by org code, building and org code.

MR. GRIFFON: Org code can include a strata of job titles, I guess, right?

DR. LITTLE: Absolutely. And the unfortunate thing about org code is it was not a constant. It changed a lot.

MR. GRIFFON: Because the other thing though that now that I'm sitting here thinking about it, I know I looked through some of the data and noticed the percentages of non-detects in the early data. And I think part of it is related to I think there was a time period when it was a weekly or biweekly --

DR. ULSH: In 44?

MR. GRIFFON: Yeah.

-- turnover cycle so you'd have a higher percentage there, but it struck me that there was a lot of zeros, and if they were supposed to be over ten percent of the RPG, you know, so part of that might have been attributed to the short film cycle. But I was curious if --

DR. MAKHIJANI (by Telephone): There wasn't a foundry in '81, was there? You're referring to 44.

DR. LITTLE: I was referring to 44, sorry.

DR. MAKHIJANI (by Telephone): Okay, sorry, it just had a little disconnect there.

MR. GRIFFON: Okay, I think we have enough there. I think it's likely that, Arjun, do you have anything more on the --

DR. MAKHIJANI (by Telephone): No, no, Mark, I think that all the facts are before you, and NIOSH and --

Joe, I don't see any disagreement in how the facts have been characterized.

MR. FITZGERALD: No, I think on these proof of principles we do the best we can on both sides to provide the information and --

MR. GRIFFON: Then we used the weight of the --

MR. FITZGERALD: -- uncertainties. I do think they do come down to the weight of the evidence because there's no way to, the technical information that would ice it is not necessarily available so it is what it is.

MS. MUNN: And it's adequate.

MR. GRIFFON: Since I'm sure some people have flights to get to including myself --

DR. MAKHIJANI (by Telephone): Could I sign off if I might?

MR. GRIFFON: No. I guess so, Arjun.

DR. MAKHIJANI (by Telephone): If there are no more questions, I'd like to sign off.

MR. GRIFFON: If you've got to leave us, you've got to leave. Yeah, go ahead.

DR. MAKHIJANI (by Telephone): No, well, I'll wait if there are --

MR. GRIFFON: We're coming up to coworker models or '69, '70.

MR. FITZGERALD: We didn't quite --

DR. MAKHIJANI (by Telephone): Oh, '69, '70, okay.

MR. FITZGERALD: We really need to at least close that out.

DR. MAKHIJANI (by Telephone): Yeah, sorry, okay.

MR. GRIFFON: Go ahead, Joe.

'69 – '70

MR. FITZGERALD: Well, we started backing into it I guess to some extent. And actually I'm not reporting there was a disagreement

actually. There was a lot of agreement on the fact that the zeros that were introduced by the badging policy ought to be dropped out. That was something that was stated at the, I think, the last work group meeting, and I think we concurred with that.

And since then I believe last Friday there was a revised OTIB-58 that was circulated. I think, Brant, you sent it to us, and I've read the language. I don't think all of us have had a chance to let it soak in because it was just Friday, but in a sense I think that pretty much goes in the direction of resolving the issue at least from that standpoint.

Now there's only one other question. I'm afraid to say sort of a proof of principle statement, but does OTIB-58 given -- this gets to some of the questions -- we take those out essentially as data points. Can one actually apply, and I think, not to prejudice the answer, can you actually apply OTIB-58 and apply the data for those workers and is there any artifact to the distribution itself?

Again, I think it's similar to what

you were asking for because you're just withdrawing these people. I think the numbers are such that it wouldn't make much of a difference.

MR. GRIFFON: Or how does it shift to 95th is really what you're asking?

MR. FITZGERALD: That's one question but yet it's sort of a, the biggest issue I think we have resolved which we all agree that these data points should not remain. They should be removed. And I think that's what you've done with the OTIB. And I think that takes care of the big issue, but that certainly leaves this other implication similar to what you raised. Does it affect anything on the coworker application?

DR. ULSH: Well, to answer your question and to make your review simple in terms of this particular issue, '69 and '70, you could look at Table 7.1 and there's just a, the only thing that changed in that table was 1969 and '70. It's pretty easy to see. I can tell you qualitatively what the effect of removing the zeros was. The 95th percentile didn't move much as you would expect. The 50th percentile

moved more as you would expect. I don't have, unfortunately, the previous version to say it changed by this much, but --

MR. BUCHANAN: It changed by, I can --

DR. ULSH: Okay.

MR. BUCHANAN: The biggest change was in neutrons for 50 percent. It changed it by a factor of about four times, increased the dose about four times. In '69 and '70 it increased it about double. And for the 95th percentile it increased it by about 30 percent in '69 and about 20 percent in '70. So the biggest increase was in '69. Apparently there was more zeros in '69 than '70.

DR. ULSH: And that would make sense.

MR. BUCHANAN: Then on the gamma dose it didn't change the gamma dose as much as it did the neutron dose in the 95th and 50th percentile, but it changed it some.

MR. GRIFFON: Mutty looks like he has a question.

MR. SHARFI: You're talking about the actual doses, not the final coworker?

MR. BUCHANAN: In the Table 7.2. It's according to the new one compared to the old

one. In ’69 it changed the 50th percentile from 58 millirem to 270 millirem.

DR. ULSH: I can verify the 270. I got that.

MR. GRIFFON: What did it do to the --

MR. BUCHANAN: The old one’s 58.

MR. GRIFFON: What did it do to the 95th?

MR. BUCHANAN: Ninety-fifth it changed from 1,483 millirems to 2,015 millirems. That’s about a 30 percent change. And in ’70 it changed from .190 rem to .328 rem, about double for the 50th percentile. So there was a significant change in ’69 and some change in ’70 for the neutrons. The photons changed by a smaller amount.

DR. ULSH: So that’s the effect.

MR. FITZGERALD: I think from the preamble to what you sent us in the OTIB, you are considering a PER for the --

DR. ULSH: Well, we’re not only considering, we’re going to do it.

MR. FITZGERALD: For the past cases.

DR. ULSH: And that is an error that’s in the publication record for this. It says no PERs required, and that is not correct.

MR. FITZGERALD: I think that's very responsive to the issue, and I don't think we have any further comment on that.

MR. GRIFFON: And this question of going, whether the issue precedes or is that through the 1970 period? NIOSH has looked at that? SC&A has looked at that?

MR. FITZGERALD: We sampled, and again, we did look very comprehensively to see if we could find any other instances that would be a similar phenomena or a situation, and we couldn't find one that was the same so we did sample that.

DR. MAKHIJANI (by Telephone): Yeah, Joe, on the contrary, we did find that when there were gaps, they were represented as gaps.

MR. FITZGERALD: I'm sorry, okay, right. It wasn't the same as this situation where --

DR. MAKHIJANI (by Telephone): No, whatever we found was actually in the affirmative.

DR. ULSH: Thank you, Arjun.

MR. GRIFFON: Anything more on data completeness?

DR. ULSH: Arjun?

DR. MAKHIJANI (by Telephone): No, no, my

main concern and our main concern with ’69 has been the integration of bad data with good data. And Larry did say that last time it would be removed, and it was done. I think that settled that.

COWORKER MODELS

MR. GRIFFON: Moving on to coworker models. Let’s see, I think let’s do the TIB-38 first, the internal coworker model. Go ahead, Joe.

MR. FITZGERALD: Yeah, on that one I think it’s not an exaggeration to say that my colleague, Joyce Lipsztein, spent quite a bit of time on this issue, and we did have a number of opportunities with NIOSH to try to allay some of the concerns over the uncertainties. And let’s see if I can paraphrase uncertainties with the MDA and certainly some of the issues relative to HIS-20 and some of the gaps that certainly were identified in our completeness review of internal.

And we got to a point, and it was a fairly good point, where there was some acknowledgement that the 95th percentile was certainly available and could be applied to

address some of these uncertainties. And relative to the report we wanted to clarify that since in practice the 50th is applied for intake values in terms of a clarification of how and when the 95th would, in fact, be used given the standing uncertainties with the MDA and the internal coworker data.

I thought, and again this comes from some review that Joyce has done along the way, stands as a concern. We had left it as one where certainly the 95th would be satisfactory. However, there wasn't a good way to, since there was a concern over whether or not, well, it was a concern that the 50th, a full distribution, would not be sufficiently conservative in all instances so would not necessarily be bounding.

So therefore, in adopting the 95th, it wasn't clear when that would be applied and how. So in a way what we wanted to do here is -- and this may be in the vein of proof of principle -- given the uncertainties, given the issues that have been raised along the way, how would NIOSH apply the 95th given the fact that the 50th is routinely applied on the

internal side, and can that be reconciled with the fact that you have these uncertainties?

So in a way we did get to the point where I think we all agreed the 95th was available and would do the job given the uncertainties. However, in practice the 50th is used. So we're left with this sort of lack of reconciliation on the practice versus the availability of the 95th to satisfy some of these issues and uncertainties.

So I think we tried to lay that out, that we buy into the concept. We thought certainly the approach was fine. We agree certainly the 95th or higher would be satisfactory in the way of a conservative approach. However, we're still left mechanistically with when that would be applied, and if the 50th is applied in practice, how could that be reconciled with the uncertainties that have been expressed? That's I guess a not too helpful way of putting it, but that's where we were left at the end.

DR. ULSH: I've discussed this with Jim Neton, and there's a part of this that's an

overarching issue, but there may also be a part of this that is Rocky Flats specific in terms of some of the questions that, well, I know that Mark has about this HIS-20 database and the CEDR, you know, the various databases that were used for the internal coworker models. I can tell you that it is generating some discussions. Jim is taking the point on this in considering this issue, the 95th percentile, and if we decided to agree to that, when and how it would be applied as you said, Joe.

I can tell you that, for what it's worth and probably not much, the philosophy in terms of external coworker models is we apply the 95th when there is a person who was unmonitored but was routinely exposed. We apply the 50th when a person was unmonitored and might be only intermittently exposed. And we typically apply ambient for people who never went into the production area.

I don't want to say that that's exactly what we would do in terms of internal because I don't know. This is something that Jim is still wrestling with. I can tell you

that we've committed to get back to Mark and the rest of the working group with our position on that. And I think you've characterized it accurately that ^ there from ^. And I hear what you're saying that if that was applied that you'd be okay with that.

In terms of the details of the application of it and all that, we're just going to have to get back to you on that.

MR. FITZGERALD: Given the considerable effort on both sides to resolve that, I think that was sort of the mechanistic part we never really hammered out which it was okay, it's available, but like on the external side how and when would you actually ^. And I think that's fine. As long as we can be assured that there's a way to apply it, there's some guidance or criteria that would be enough.

DR. ULSH: We'll get back to you.

MR. GRIFFON: And I think it is, I discussed this a little with Brant and Jim, and I think it is a unique circumstance with the HIS-20 CER data because I had to use this approach at several sites, but they're going to, I think we'll leave it up to you to come back to us

with a reply on that.

DR. ULSH: And we'll do that quickly.

MR. GRIFFON: Yeah, that's ^.

MS. MUNN: So that's ^ conference call.

MR. GRIFFON: Yes.

And it says coworker models so I think we need to do 58. I think certainly it's in the ^ I think mainly, but I'll just as a sense of completeness here, Joe, maybe touch on that.

MR. FITZGERALD: I almost hesitate to wade into ^. Ron has spent a good part of a year in OTIB-58 land, and really I think the issues we've already discussed have surfaced the ones that have the SEC implications. If you want to just spend a minute just kind of overviewing where we ended up. Not so much the --

MR. GRIFFON: I think we sort of touched on them in there. We touched all the other items today. We kind of touched on them.

MR. FITZGERALD: Is there anything that we did not touch on, Ron?

MR. BUCHANAN: There's a couple clarifications on the ^. We've had two recent

additions of revisions to OTIB-58, one on the tail of another. And so we're still trying to get it all together. In '70 to '76 in the next to the last revision, you added Table 6.2 which before you were going to use the .42 derived from lighter TLD data.

And then you found the worksheets or something like that which gave individual neutron/photon data separate for '70 to '76. Now my question is are you going to use individual workers' data then for individual dose reconstruction or are you going to use, apply to Table 6.2 to all the workers that you do dose reconstruction on?

MR. GRIFFON: Because that situation you have the lump-sum doses, right? The aggregated neutron/photon doses during that period?

MR. SHARFI: No, we have reported pen doses. Those ratios to separate the pen dose out?

MR. BUCHANAN: Right, are you going to use this table or are you going to use the individual TLD datasheets for each individual worker to separate out their penetrating neutron and photon dose?

MR. SHARFI: I'm not sure when you refer to individual datasheets.

MR. BUCHANAN: Okay, this information here, this new table came from some new datasheets that you gave us, right?

MR. SHARFI: They were the quarterly, or in this case, monthly? Yeah, they were monthly data by building, and it was a printed out supervisory report. And what we did was enter every valid neutron and photon pair. If there was a zero in one of them, we didn't enter it. So we entered all 30,000 or whatever it turned out to be for the year, well, for several years. But we did not put in identifiers for individual people. They are individual readings, but --

MR. BUCHANAN: Right, no, what I've seen, the database you sent, I think you posted, was it had individual readings for each worker.

DR. ULSH: Yeah, it wasn't the neutron TLD worksheets. It was the supervisor reports.

MR. BUCHANAN: Okay.

DR. ULSH: I understand what you're saying. ^ data there.

MR. BUCHANAN: You had individual workers

and their neutron and their gamma, photon readings separately. And so this was going to be, so that data was used on a, you determined the total neutron dose and the total photon dose, and determined that --

DR. LITTLE: On that data we calculated individual pairs.

MR. BUCHANAN: Individual pairs, okay.

DR. LITTLE: So rather than taking the ratio of the means or the ratio of the sums, we took the ratios.

MR. BUCHANAN: And then averaged the ratios?

DR. LITTLE: Correct.

MR. BUCHANAN: To get this Table 6.2?

DR. LITTLE: Correct. Well, I wasn't involved in Table 6.2, but that's the raw data I can tell you.

MR. BUCHANAN: And so you averaged the individual ratios to get an overall ratio each year?

DR. LITTLE: Yes.

MR. BUCHANAN: But not according to building. So when you do dose reconstruction, if a person has a composite dose in '70 to '76, you will use, say 1971, you'll use a 1.61

to separate out their dose regardless of what building they was in or anything. Now, why was there, I noticed that several years, ’72 and, ’73 and ’70, you had not enough data. You had to use other, the year before or the year after.

DR. LITTLE: I can’t speak to Table 6.2.

MR. FITZGERALD: Yeah, Ron, the issue is just whether there’s anything that because of the fact that there’s new tables that might influence the bottom line. I --

MR. BUCHANAN: Yeah, I realize this isn’t the place to discuss, but what I’m trying to determine is, is there any missing data here that would determine any SEC --

MR. FITZGERALD: That would have the implication --

MR. BUCHANAN: Right, and that’s what I was trying to clarify, exactly how it was going to be, how it was attained, and how it was going to be used.

MR. FITZGERALD: And sometimes we might not have the right personnel, people who worked on this particular item.

MR. GRIFFON: I might just need to follow up

on that one, right? We're going to have a neutron call anyway. I think that's a neutron-related question, but just a follow up on that Table 6.2 related to de-convolution, I guess, is the question.

MR. BUCHANAN: How it will be applied and how it will be ^. And I don't see an SEC issue here, but I just wanted to clarify how it was done to make sure we don't have anything like we had doing '59 back to '52.

MR. FITZGERALD: So Table 6.2 and you said you had one more?

MR. BUCHANAN: Well, the other one is, I would characterize it as a site profile issue as far as Table 7.2.

MR. FITZGERALD: Six-two is the one --

MR. GRIFFON: We don't need to talk about that now.

MR. FITZGERALD: We'll have that on the neutron call.

WOUND ISSUE

MR. GRIFFON: One other thing I just added as we were doing our conversation, and the individual's who might likely respond to this is gone, but if you recall in the March 7th

meeting I brought up again my wound question.

DR. MAKHIJANI (by Telephone): Mark, could I sign off now?

MR. GRIFFON: Yeah, yeah.

DR. MAKHIJANI (by Telephone): There's some people waiting for me.

MR. GRIFFON: This time you really can, Arjun.

DR. MAKHIJANI (by Telephone): Thank you, bye.

MR. GRIFFON: I didn't think of it until Jim was gone.

MS. MUNN: It's on my list.

DR. ULSH: I know we've talked about it. I know I gave Jim a point on that.

MR. SHARFI: He had a thought process that he wanted --

MR. GRIFFON: Can you get something in writing to us maybe, a memo from Jim or something about this?

MR. SHARFI: ^ why we do what we do.

DR. WADE: And what's this?

DR. ULSH: What I can say, Mark, is that we have done some analysis, and Jim has been involved. We've had, there's been some

discussions about it. The bottom line, I think -- I have to speak very generally, from Jim, was that we had some pretty good reasons why we thought what we were doing is bounding especially with, when you consider compared to like Super-S. I can't go any deeper than that. I'll have to rely on Jim for that.

MR. GRIFFON: I've done some IMBA runs myself, and I can come up with scenarios that where the model's not bounding, but are they likely scenarios? I'd have to say they're probably not. Not only a huge wound intake, but also the likelihood that someone was wounded with a plutonium exposure and not monitored for a year.

The records seem to indicate that if they were in those plutonium areas, they likely fell into that monitoring program. So I would concede that I would like to see you guys at least run, give us some kind of analysis back on that just to close it out.

PROOF OF PRINCIPLE

The last item I had is proof of principle, and the only reason I had this on the agenda was you did provide some cases. I

think that's where, you know, we've brought up proof of principle several times today, but I think there were some specific cases that were posted for us, Joe, and I don't know if you specifically reviewed any of those or had an opportunity or we kind of put those on the side with the other issues?

MR. FITZGERALD: Well, I think there's been a, we looked at a few. I haven't look at them specifically, but I guess Arjun's already missing on the --

MR. GRIFFON: Yeah.

MR. FITZGERALD: Did you look at some of the proof of principle, the actual cases themselves?

MR. BUCHANAN: Yes, uh-huh. I had some of the hypothetical cases, you know, I looked at three external --

MR. FITZGERALD: These would be the more recent ones.

DR. ULSH: Yeah, we had an initial set that we put together around the time we presented the ER back in April of last year. But a lot of water's gone under the bridge since then.

MR. GRIFFON: Then you gave us some new --

DR. ULSH: Yeah, do you recall at the last working group meeting we decided that we needed to provide real cases that -- let me see, we had external coworker, preferably including ’69 and ’70. We had internal coworker, and we had Super-S. Those are the three specific categories of cases that were requested for proof of principle cases. I provided to the working group and Mark and SC&A -- let me think now. Was it some external case --

MR. GRIFFON: Case number, actual case number.

DR. ULSH: Yeah, and then it was a hypothetical case that covered internal coworker and Super-S. Following onto that I provided a list of all of the cases that we’ve done that included external coworker and internal coworker. That’s where we, I don’t know if you reviewed those, Ron.

MR. BUCHANAN: No, I got left out of the loop.

MR. FITZGERALD: I think that was, yeah, that came in. I think Joyce may have looked at the internal, but I think the external one

certainly came in the same time that this neutron --

MR. GRIFFON: It sounds like we've, but I think we sort of know the internal issue, and we've got that on the table. As far as the external, let's at least look at that case for the, I think we're already planning for a Monday conference call meeting. I would argue, we can do it on the phone, but on Monday --

MR. FITZGERALD: It may be Tuesday because the Monday, I think the notion was to get the '52-'58 out.

MR. GRIFFON: No, no, I'm not talking next Monday, I'm talking the Monday after, Monday the --

DR. WADE: The 30th of April.

MR. GRIFFON: For a work group call, Monday --

DR. WADE: What time, Mark?

DR. WADE: Eleven?

MS. MUNN: Eleven.

DR. WADE: Respect to Wanda?

MS. MUNN: Or suffer the wrath.

MR. GRIFFON: This could be good. I could

avoid an implant preparation at my dentist. What time did you say, Wanda?

MS. MUNN: Eleven.

MR. GRIFFON: Yeah, I could be a little numb by then. That might be good. Yeah, eleven o'clock's good for a conference call.

DR. WADE: This is going to be a work group call.

MS. HOWELL: Is this on the 30th?

MR. GRIFFON: Yes. Now next week we might have a couple technical calls, but at least one technical call.

MR. FITZGERALD: This is a work group call, right, right.

MR. BUCHANAN: Joe, will you send? I haven't received that.

MR. FITZGERALD: Yeah, well, we'll, again, we've been diverted, but yes, absolutely.

MR. GRIFFON: And in addition to the, so you have the wound thing on an action item, too. And SC&A will look at those other couple cases.

MR. FITZGERALD: Right, I think though, again, the external, since the internal ^.

MR. GRIFFON: Yeah, we kind of discussed the

external and internal in the broad sense. I don't know that we're going to see much more out of the cases, but we should look at them and --

MR. FITZGERALD: Probably the other thing, too, is, well, okay, we can talk about this offline, but the Super-S certainly ^.

MR. GRIFFON: Right, I think we had another hypothetical before in the Super-S, and it may not, but there were some changes, I think --

DR. ULSH: I think the situation there is that we don't yet have the Rocky Super-S cases.

DR. WADE: They're in adjudication.

MR. SHARFI: That's why we ended up doing a --

MR. GRIFFON: Adjudicated.

MR. FITZGERALD: Well, I think we did one before, but that's okay, we can do --

MR. GRIFFON: If you can provide us case numbers for Super-S that are non-adjudicated, I mean, we can do the same thing we did with the others.

MR. SHARFI: ^

DR. ULSH: No, that was then. This is now.

I'll have to check on that.

MR. GRIFFON: The understanding is if we can get case numbers that are non-adjudicated we can review them but we won't discuss them specifically on the record.

MR. FITZGERALD: But that would be a change from the one that we did before that which was another hypothetical Super-S.

MR. GRIFFON: So if you have a real case for Super-S.

DR. ULSH: I'll double check. I know that there's an answer back in my office. I just can't --

MR. GRIFFON: But otherwise the action's in SC&A's court.

MR. FITZGERALD: Yeah, yeah, we can certainly do that.

MR. GRIFFON: All right, and then the final process leading to the May meeting. I think we have meetings from now until the May meeting.

DR. WADE: Well, we certainly have a work group call at eleven a.m. on the 30th. We're looking at possible technical calls.

MR. GRIFFON: Yeah, and next Monday we're

getting a report from SC&A to NIOSH, a draft section.

DR. WADE: The neutron 1952 to 1958.

MR. GRIFFON: And then I would offer that maybe Wednesday or so, Brant, if we can get a technical call.

MR. FITZGERALD: ^.

DR. ULSH: Meeting on Wednesday.

MR. FITZGERALD: I was thinking Tuesday.

MS. MUNN: Sounds like you've got Tuesday.

DR. WADE: And Wednesday we're looking by close of business, the SC&A addendum, and then with Privacy Act review hopefully it will be available to the world by close of business Friday.

MR. GRIFFON: That takes care of everybody's next couple of weeks. We're getting there though I think.

DR. WADE: Deserves it, all the work you've put into this one.

MR. GRIFFON: And made good headway. Any final comments before we close?

DR. WADE: Only thank you.

MR. GRIFFON: Arjun? We know he's gone.

MR. PRESLEY: Are we going to meet before

the meeting?

DR. WADE: Wednesday we have a subcommittee meeting.

MR. GRIFFON: We have a subcommittee meeting, yeah, a subcommittee meeting. So I think the answer is no. I mean, I'm hoping that anything we have to finalize is on the 30th. I don't think a lot's going to change in two days.

DR. WADE: We won't be taking up Rocky Flats until Thursday so theoretically you have Wednesday.

MR. GRIFFON: We could have a late afternoon, yeah.

DR. WADE: Well, if we have also the public comment period Wednesday from five to whenever.

MS. MUNN: So we can skip dinner.

MR. PRESLEY: Yeah, I can see you skipping dinner.

MR. GRIFFON: Lew, before we close, I don't know if we asked, if anyone's still on the line if we have any final comments from anybody on the line? Petitioners or Congressional staff?

(no response)

MR. GRIFFON: Everybody lost track of us, huh?

Well we appreciate it. If you stayed on, we appreciate it, and I think we'll close out now.

DR. WADE: Thank you all very much.

(Whereupon, the working group meeting concluded at 4:40 p.m.)

CERTIFICATE OF COURT REPORTER

STATE OF GEORGIA

COUNTY OF FULTON

I, Steven Ray Green, Certified Merit Court Reporter, do hereby certify that I reported the above and foregoing on the day of April 19, 2007; and it is a true and accurate transcript of the testimony captioned herein.

I further certify that I am neither kin nor counsel to any of the parties herein, nor have any interest in the cause named herein.

WITNESS my hand and official seal this the 26th day of April, 2007.

_____

STEVEN RAY GREEN, CCR

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