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

FERNALD

The verbatim transcript of the Working Group Meeting of the Advisory Board on Radiation and Worker Health held in Cincinnati, Ohio on November 13, 2007.

STEVEN RAY GREEN AND ASSOCIATES
NATIONALLY CERTIFIED COURT REPORTERS
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TRANSCRIPT LEGEND

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In the following transcript: a dash (--) indicates an unintentional or purposeful interruption of a sentence. An ellipsis (. . .) indicates halting speech or an unfinished sentence in dialogue or omission(s) of word(s) when reading written material.

-- (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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POTTER, GENE, ORAU
RICH, BRYCE L., CAI
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SHARFI, MUTTY, ORAU
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P R O C E E D I N G S

(9:00 a.m.)

WELCOME AND OPENING COMMENTS

DR. LEWIS WADE, DFO

DR. WADE: Can you hear me out there? Is Mark Griffon with us?

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

DR. WADE: And Robert Presley?

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

DR. WADE: Let's begin. This is the work group on the Fernald site profile and SEC petition. It's ably chaired by Brad Clawson, members: Griffon, Ziemer, Presley and Schofield. Clawson, Ziemer and Schofield are here in the room, and Griffon and Presley are on the telephone. So let's begin.

Again, this is Lew Wade. I have the privilege of serving as the Designated Federal Official for the Advisory Board. Before we do our normal introductions and have our little talk about telephone etiquette, I'll remind everyone that we are talking from a matrix that hasn't been necessarily scrubbed for

Privacy Act information. So we should talk; we should be guarded in our comments when we come to the potential of sharing personal identifiers. But I think everyone around the table is well schooled in that etiquette.

We'll begin by going around the table here for three purposes, introduction, if you have any conflicts, please so state, we'll also give people on the phone a chance to establish they can hear everyone around the table. So if during an introduction someone out there in telephone land has difficulty hearing, just shout out, and we'll make the necessary adjustments either in volume or in positioning of equipment.

This is Lew Wade, and I have the privilege of the Advisory Board, and I work for NIOSH.

MS. HOWELL: This is Emily Howell with HHS.

DR. ZIEMER: This is Paul Ziemer, member of the work group and no conflicts at Fernald.

DR. MAKHIJANI: Arjun Makhijani, SC&A and CDS -- CBCS said I have a conflict at Fernald.

MR. SCHOFIELD: Phillip Schofield, a Board member and no conflicts.

MR. ROLFES: Mark Rolfes, NIOSH, Health Physicist, no conflicts.

MR. CHEW: Mel Chew from the O-R-A-U team, no conflict.

MR. RICH: Bryce Rich from the O-R-A-U team, no conflicts.

MR. MORRIS: Robert Morris, O-R-A-U team, no conflict.

MR. CLAWSON: Brad Clawson, work group chair, no conflict.

DR. BEHLING: Hans Behling, SC&A, no conflict.

MS. BALDRIDGE: Sandra Baldridge, petitioner.

MR. BEATTY: Ray Beatty, former Fernald worker.

MS. KENT: Karen Kent, O-R-A-U, no conflicts.

MR. HINNEFELD: Stu Hinnefeld from NIOSH/OCAS, and I am conflicted having worked in management and salaried positions in Radiation Safety and Health and Safety departments at Fernald.

MS. HOFF: Jennifer Hoff, ORAU team, no conflicts.

MR. LEWIS: Mark Lewis, ATL International Outreach Specialist, no conflicts.

DR. WADE: Okay, let's go out to the telephone and start with members of the NIOSH and ORAU team. Please identify yourselves.

MR. FAUST (by Telephone): Leo Faust, ORAU team, no conflict.

MR. POTTER (by Telephone): Gene Potter, ORAU team, no conflict.

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

DR. WADE: Other NIOSH/ORAU team members?

MS. CHANG (by Telephone): Chia-Chia Chang, NIOSH Director's Office, no conflicts.

DR. WADE: SC&A team?

MS. BEHLING (by Telephone): Kathy Behling, SC&A, no conflict.

DR. WADE: Any other SC&A team members out on the phone?

(no response)

DR. WADE: How about other federal employees who are working on this call?

MR. WOOLS (ph): Jessie Wools, Senator Voinovich's office.

DR. WADE: Welcome.

MR. KOTSCH (by Telephone): Jeff Kotsch, Department of Labor.

DR. WADE: Welcome, Jeff, as always.

MS. HOMOKI-TITUS (by Telephone): Liz Homoki-Titus, HHS.

DR. WADE: Good morning, Liz.

Other feds working on this call?

(no response)

DR. WADE: Are there any other representatives of members of Congress on the call?

(no response)

DR. WADE: Workers, petitioners, their representatives?

(no response)

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

(no response)

DR. WADE: Okay, just a little bit about phone etiquette. We were doing real well, but we slipped on a call recently. So please again, mute if you’re not actively engaged in a discussion. If you are engaged in a discussion, try and use the hand set if at all possible. It cuts down on background noise.

And be very mindful of your environment and what happens if you put a call on hold. We were inundated with background music the last time.

And please be aware of the fact that what might be commonplace noise to you can be very disruptive to the work group in trying to do its business. I think it’s good that we open these calls up to any and all and give them telephone access, but please ensure that we can continue to do that by exercising due caution in terms of how you monitor the environment around you.

Let me ask if there are any other Board members on the call aside from Mark and Robert Presley.

(no response)

DR. WADE: Okay, we don’t have a quorum of the Board, and that’s appropriate for us to conduct the business of the work group.

I guess, Brad, it’s yours.

INTRODUCTION BY CHAIR

DR. MAURO (by Telephone): Brad, this is John Mauro. I just joined you late. I’m sorry for being a few minutes late, but I just want to let you know I'm on the line also.

MR. CLAWSON: Thank you, John, we appreciate that.

At the last meeting I believe that we finished up with 4.3, at 4.3. So we're going to start off in the matrix at 4.3-1, and I'll turn it over to Hans.

FINDING 4.3-1

DR. BEHLING: Let me ask Mark, since you're on the phone, if you had anything that you wanted to discuss prior to starting with 4.3. I know we've indicated on a couple issues, and I just wanted to be sure before we start whether or not you had some outstanding issues, action items that you wanted to discuss prior to starting at 4.3, Mark.

(no response)

DR. BEHLING: Mark Griffon, are you on the phone?

(no response)

DR. WADE: Might be muted.

DR. BEHLING: Hello?

(no response)

DR. BEHLING: Well, okay, let me just perhaps speak in his behalf. I think last time we talked about something that involved the very last finding, 4.2-3, which involved the radon release model for the K-65 silos. And I’m not sure whether or not we had asked NIOSH to revisit that issue looking at basically the disequilibrium between Radium-226 and the two daughter products and perhaps come up with a revised estimate about annual releases. And I’m not sure whether you’ve had a chance to look at that at this point.

MR. GRIFFON (by Telephone): Hey, Hans, this is Mark Griffon. I’m sorry. I went to pick up the handset and lost the call accidentally. You were asking me something.

DR. BEHLING: You and I had discussed a couple items in preparation for this meeting, and I wasn’t sure whether you wanted to perhaps discuss some of those, a couple of those items before we get started at 4.3 because that’s --

MR. GRIFFON (by Telephone): Well, my, I mentioned it to Brad, I talked to Brad earlier this morning, and my sense was why don’t we go through the rest of the matrix and get through it one time, and then maybe come back if we have time at the end, if that would be all right, Hans.

DR. BEHLING: That's fine. That's fine.

MR. GRIFFON (by Telephone): Just so we have one pass through the entire matrix.

DR. BEHLING: Okay then, I guess we'll start at 4.3 which in the report that I submitted starts at page 49. And the issue there is one of the model that assigns a maximized internal exposure of 1,050 MAC hours for thorium as a default value. Now I'm not sure, and I'm talking now to Mark Rolfes, whether or not that has been also changed.

MR. ROLFES: Well, that certainly is in the process of being changed. That was what we believed the claimant favorable default at the time when we had a push to get the technical basis document out to use for dose reconstructions. We now have a much more comprehensive dataset process knowledge of the thorium processes, the time periods that it was processed, the areas and the air concentrations associated with that.

So we are going to be using a more detailed approach to assigning thorium exposures to individuals rather than a default 1,050 MAC hours. So our approach that we will be using now for the early time period will be based on air monitoring data associated with the processes. And in the more recent time periods the mobile in vivo results will be used as well.

DR. BEHLING: And when will that revision, or in what form will that revision take place, that will be part of a revised TBD?

MR. ROLFES: I believe we can separate that out in a white paper. I believe that's what we've agreed to do. So we will --

MR. RICH: The TBD is in revision now.

MR. ROLFES: The TBD is, in fact, in revision as well as we speak. So I believe we had agreed to pull the thorium portion out as a white paper for the Advisory Board members.

DR. MAKHIJANI: Can you describe the conceptual approach at least that you're taking? How are you taking into account the high episodic air concentrations of thorium like half a gram per cubic meter that has been documented?

MR. ROLFES: Well, we have because we do have data for that. We will be giving credit to those high airborne concentrations. We have a distribution of thorium air concentrations for the early time period. I believe, is it about 3,000 air monitoring results that we have for the early time period. And so we have quite a bit of data that we feel is going to provide a good distribution of the air concentrations to which people were exposed.

Now, furthermore, when we do have these higher concentrations, we are not assuming any credit for respiratory protection. We are not reducing air concentrations based on particle size distributions because many of the particles which were airborne were not respirable size. And plus, we are also assigning a 2,000 hour per year exposure to thorium for an entire year as a chronic exposure. So we feel that what we are doing is certainly very claimant favorable and will be a bounding analysis of a person’s exposures to thorium.

DR. MAKHIJANI: I guess I didn’t address my question well. How does a distribution of

3,000 air samples take care of an episodic exposure to an individual?

MR. ROLFES: Well, we could look at that specific air sample, but we wouldn't assume that that person was exposed to that high air concentration for 2,000 hours per year.

DR. MAKHIJANI: Yeah, no, no. That's exactly the problem that I'm talking about. The distribution doesn't take care of that, and we don't know the names of the individuals who were involved. We should just wait for the --

DR. BEHLING: I think that's going to come up in the next finding essentially because it does raise questions that you are addressing here.

DR. MAKHIJANI: Okay.

DR. ZIEMER: One other question, this is Ziemer. What years are covered by this set of samples?

MR. ROLFES: This would be from, I believe, beginning in 1953 through -- well, for the air monitoring data? Is that what you're referring to specifically?

DR. ZIEMER: Well, this thorium data that you’re talking about.

MR. ROLFES: The thorium air monitoring data, the majority of it is in the early time period because that’s what we had focused on because that was prior to the time period where we had the mobile in vivo radiation monitoring lab results in ’68. So we focused our initial data gathering on the time period from, I believe, ’53 through ’68. And we are revisiting those records to make sure that we’ve captured all the thorium air monitoring data that we can. We are also looking for the more recent time period as well.

MR. CLAWSON: Well, this thorium data that you’ve got, this is Brad, is this general area or is this personnel or where was this sample on the O drive? I’m just questioning some of the, how were they being taken?

MR. ROLFES: There’s a sampling of both breathing zone samples as well as general area air monitoring results. So there’s a wide distribution of air monitoring results that are taken associated with the individual’s breathing zone while he was working with the materials. And there are also general area plant monitors distributed throughout the plants and processing areas.

MR. MORRIS: This is Bob Morris. There's a third set of data that we didn't use which is the process sampling where the intent was to get inside the (unintelligible) or right at the point of generation to actually capture the worst concentration that existed. And so that the plant had three sampling protocols, general area, breathing zone where the sample was held near the person's face, and then the process samples. We used the, we excluded the process samples from these calculations and used the general area and the breathing zone samples to represent the intake rates.

DR. MAURO (by Telephone): Mark, this is John Mauro. Quick question. When you provide the white paper, will there also be these datasets, the 3,000 air sampling measurements, put on the O drive as supporting material?

MR. ROLFES: They've already been provided.

DR. ZIEMER: They're there, John.

DR. MAURO (by Telephone): Thank you.

MR. RICH: This is Bryce Rich. We might just mention again that, I think Mark mentioned at the last meeting, that we have become aware through additional research that there were a number of careful attempts to look at intake or deposition through a thoron breath analysis and through accounting by Hirsch and others throughout this early period. And the results of those studies indicated that the body depositions of the highest exposed people were less than maximum permissible lung burdens at that time.

MR. ROLFES: Correct, thank you, Bryce. I did forget about that. I wanted to point that out, I guess, as we moved through the matrix. I figured we'd be discussing thorium for quite awhile, and it is very important to note that several individuals from Fernald who had been previously exposed or currently exposed to thorium in the process at Fernald were sent offsite because the Health and Safety Division or the Industrial Hygiene and Radiation Department wanted to characterize who had been exposed to thorium and how much thorium the body was retaining.

So I believe beginning in 1961 there were a couple of individuals that were sent to the University of Rochester to Dr. John Hirsch, I believe was his name. And he had done in vivo counts, lung counts, of the two individuals and was unable to detect any thorium in the individuals' bodies at that time.

So he had also requested thoron breath samples from the individuals, and he was able to detect some thorium progeny, some thoron. And the amount he interpreted was about I believe, let's see, was about 1.6 picocuries per liter of thoron which was the higher result. From that he said that that was approximately a ten percent of maximum permissible lung burden.

So that was the first of a series of studies where individuals from Fernald who had previously not been monitored in vivo for thorium exposures were, in fact, being monitored. Now the second was I believe a couple years later, and I'd have to take a look back at my notes, but another handful of individuals was sent to Wright Patterson Air Force Base who had an in vivo set up, and I believe those same individuals also gave urine samples to determine whether they could detect thorium in urine using neutron activation analysis of their urine. They were able to detect some thorium in one individual's urine.

I believe a handful of people were also sent to Y-12, and I believe some of the same individuals that had gone to Wright Patterson Air Force Base also were sent to Y-12. Those individuals at Y-12 were given lung counts as well, and I'd have to take a look back at the data, but I do believe they may have detected some thorium in the one individual, but it was still less than the maximum permissible lung burden.

MS. BALDRIDGE: Question, Mark. Did the Hirsch report say what means they used to try to determine whether there was thorium?

MR. ROLFES: Yes.

MS. BALDRIDGE: And are there other methods available today that would have been more precise? Is it possible that there was thorium present, that the means that they used to try to detect it didn't work?

MR. ROLFES: Sure. The NIOSH approach that would be taken if there was a sample result that didn’t have a detectable amount of thorium, NIOSH would assume that there was a missed intake that would have been incurred but not detectable by the equipment at the time. So it is very possible. We’re not saying that there was no thorium exposure. What we would interpret this as is that there could have been exposure, but it was less than the detection limit.

MR. RICH: Mark, you probably have to say in addition that we have a high level of confidence as a result of these studies that the air sampling default values for intake are enormously high.

MR. ROLFES: Yes. They certainly are.

DR. ZIEMER: Compared to the in vivo results?

MR. RICH: Yes.

MR. ROLFES: Correct, yes, probably a couple of orders of magnitude higher than the actual in vivo data.

DR. BEHLING: When we talk about the Hirsch study that he attempted to actually establish the in vivo body burdens of thorium, which kind of technique did he use? I mean, it wasn't until years later I assume that the mobile in vivo laboratory analysis was conducted, and that's a fairly sophisticated system which leaves me to question whether or not this individual, Dr. Hirsch, had the capability of doing in vivo studies.

MR. ROLFES: Well, he was at the University of Rochester. I'd have to take a look at the data to make a determination on what he did. But just given the fact that the workers were sent there for counts, and that there were no detectable quantities of thorium in the workers, he didn't believe that that was as sensitive a measurement as possible. So he used another method, the thoron breath analysis, and was able to detect a little bit of thoron indicative of a thorium exposure.

DR. BEHLING: But you mentioned that he had concluded that the body burden was less than ten percent of the maximum permissible lung burden. And that means he must have understood how to convert thorium, thoron in breath to body burden. And again, that requires a very detailed understanding of the biokinetics, retention and elimination rates and all these things which, again, I don't know if Dr. Hirsch is a medical doctor, and not to speak disparagingly of the medical profession, but my experience has been that they understand very little about these very esoteric items and issues that are coming into play here.

MR. ROLFES: Dr. John Hirsch was very involved in other radon breath analyses. He was very involved at the University of Rochester and had quite a detailed knowledge of bioassay methodologies. He did discuss some of his assumption in the report that I have regarding the thoron breath analyses, and I'd be happy to provide that. I actually do believe I have provided it to the Advisory Board members.

MR. RICH: They did have a major thorium study going on. That's where the genesis of this, the bioassay work was started there.

DR. MAKHIJANI: It might be useful to look at the caveats that they put on their own measurements. Here's what they said. I'm reading from a document, quote, "Results of these tests were interpreted as showing not more than five to ten percent of a permissible body concentration of thorium." That's what we were talking about, right?

Continuing with the quote, "In arriving at these results, certain assumptions had to be made concerning the presence of radium daughters with the thorium in the lung and the percentage of the total thoron generated in the lung which is exhaled in the breath. The breath thoron technique, if it is to be useful, clearly requires some refinement." So they're quite sure, this seems to indicate that they didn't think this was very useful, and that to be useful it would require some refinement. At least that's how I read the English. Maybe English is a complicated language so probably other people may read it differently.

They put together a pretty cheap apparatus for doing this which is described a little bit here. So I think it would be worthwhile to note, it's at least worthwhile to note that these people were skeptical about their own measurements and their utility in saying how much thorium was in the body.

DR. BEHLING: And am I correct in concluding that your assumption was that the source term was strictly radium in the lung as the source term for the thoron that’s being exhaled?

DR. MAKHIJANI: I haven’t read the whole report.

DR. BEHLING: And there was no other source term in the body?

MR. ROLFES: I think it’s very important to note also that in the same report it does say a second question arises as to the precision of the thoron measurement. An error here would be in the nature of an overestimate.

DR. MAKHIJANI: Well, clearly, this requires some very careful analysis, and it would be useful I think to have more details on their equipment and their own assessment of their accuracy.

MR. RICH: Could I just say this was a quality program at the leading edge of research and development in this particular area. We’re not indicating that that is going to be used for dose reconstruction. It just is a valid measurement to indicate that the deposition in the lung was not extreme as the air sampling, the use of air sampling results will, we're sure, will give a bounding that will probably be in the couple of orders of magnitude high. That's all we're saying.

DR. BEHLING: And how would these individuals who were assessed by Dr. Hirsch selected for this evaluation? Is there, I haven't seen the paper so I'm talking basically on a blind level, but how were these individuals selected? Were these people who were considered at the top end of the exposure group?

MR. ROLFES: Yes, I do believe so. One was a chemical operator, and I believe the both of them may have been chemical operators. I know the one individual was involved in the early production runs in 1954 in Plant 9.

DR. MAKHIJANI: Well, this paper is from 1965 and to the extent that I'm familiar with the thorium air data, and I went through all 3,000 samples, the highest measurements of thorium were in the '50s and early '60s by far to my recollection. So it could be easily two orders of magnitude higher.

So you're talking about, I think we're

comparing apples and oranges. We have two individuals from 1965 with a method that was regarded as questionable by the people who did it themselves that are being talked about bounding doses for air concentrations for the ‘50s and ‘60s that were one or two orders of magnitude higher than were measured in the mid- to late ‘60s. Well, it might be useful to see the white paper and what element of temporal analysis there is in the various periods within the air monitoring.

DR. ZIEMER: I agree we should look at the paper, but I also would point out that those caveats are not any different than almost any scientist puts in this kind of a paper because you always have those issues. It’s always based on what assumptions you’re using so that does not imply that these are not useful. I think your conclusion, Arjun, may be questionable there. It does not imply that they did not believe that their method was useful. I think it’s the normal caveat scientists put in. I would do that. Hans would do that. You would do that.

DR. MAKHIJANI: I don’t think I had a conclusion. I more put a question on the table that as to what interpretation of a paper in which people had reservations about their own measurements. That was point number one.

DR. ZIEMER: No, no, I'm saying that does not imply reservations. That's what I'm saying. That's the usual caveat.

DR. MAKHIJANI: If it is to be useful, I would not put, you said what I would do in a paper that was I was writing. If I had made a good measurement, I would not put a caveat if it is to be useful if I were confident that the --

DR. ZIEMER: No, no, I think you would put the limitations of that reading. All readings have limitations. All readings do.

DR. MAKHIJANI: Right, but if the limitation if it is to be useful, I believe that it deserves careful scrutiny (unintelligible) its utility.

DR. ZIEMER: Oh, I don't disagree with that. I'm just saying all measurements have their limitations and you need to know what they are. And then you can determine whether you can use that for bounding.

DR. MAKHIJANI: And the more important point, conclusion, that I'm putting forward, I'm not putting forward a conclusion about the utility of this, is that you've got people from 1965, two individuals, and a claim has been made --

MR. ROLFES: Nineteen sixty-two, I wanted to correct.

DR. MAKHIJANI: -- claim has been made that they are bounding. And we need to know what air concentrations they were exposed to because the air concentration variations in the '50s, from '54 to '61 as I remember in that period were much, much higher than in later periods. Now if we can establish what these people were exposed to in the workplace, there would be some basis to say that these results show that the air concentrations are bounding. But I think unless that can be done, I would question whether these two individual measurements, even if they were valid, meant anything.

DR. BEHLING: I think the issue is one of timing, too. The fact that these potentially large exposures may have taken place years earlier, to what extent can you assess that years later by a exhalation of thoron. Obviously, even a highly insoluble material will ultimately be purged from the lung; and therefore, the question is how do you account for the time element here. If their exposures took place in the middle '50s or early '50s, what is the validity of a 1961, '62 measurement, and how do you account for that time gap?

MR. ROLFES: Was that a question or --

DR. BEHLING: No, it's just a question that again, what is the validity of using that data in trying to establish a bounding value?

MR. ROLFES: Our objective from this is just to show that we feel that there was a concern in the workplace that these individuals had previously been exposed to thorium. And this is an attempt, and it was a good attempt in my opinion, to determine if there were any significant exposures to thorium previously.

MR. RICH: They did make an effort to choose those that had been exposed, before the special study, to choose those that had been the highest exposed during the 1952 period.

DR. MAKHIJANI: 'Sixty-two.

MR. RICH: 'Fifty-two, those that had been exposed at the highest levels. The people at the plant made the decision to send people, operators, chemical operators, for analysis that had the highest potential exposure stemming back from the early period.

DR. MAKHIJANI: I didn't understand the reference to '52. I thought thorium started in '54.

MR. ROLFES: We're referring to '54, '55 time period is what we're referring to.

MR. RICH: I misspoke, '54.

And in addition to that at the very early time it was well known that thorium, where it was inhaled, it did not, it's very difficult to use urinalysis. It did not become systemic to the standpoint of being excreted as uranium and other materials were. They knew that, and as a consequence they knew, Hirsch and others at the university had, the front end of the studies in relationship to thorium bioassay and other processes. That's where it began.

DR. MAKHIJANI: I compiled the air concentration data in the review of the Fernald TBD that I did. Obviously, you have a lot more data, but in the 1950s you had some data in Plant 9 that were as low as one times MAC, four times MAC and then a hundred, 353, 3,500 times MAC. It's all over the map.

Whereas, the range in the '60s was a little bit lower. There were some high samples in the '60s, but as I say, you have to know, you can't just presume that they were sending the people who were exposed to the highest concentrations because the concentrations varied from four to 1,260. You have to know which worker it was, and when they were exposed.

MR. RICH: They were a relatively small group of workers that were working in the thorium processes.

MR. ROLFES: A very small population of workers.

DR. MAKHIJANI: Do we know who they are from the '54 to '68 period?

MR. RICH: We have listings of them that identifies them as potential thorium workers.

MR. ROLFES: And there was a study that was done before the mobile in vivo radiation monitoring lab came in at Fernald in 1968. There was an attempt to determine who had previously been exposed to thorium. And those individuals that had been exposed to thorium were some of the very first people to be counted in the in vivo lab.

DR. MAKHIJANI: Well, I guess I just have to look at the paper.

MR. SCHOFIELD: What about did they account for the crafts that might be in the vicinity during these high concentrations? Were they also monitored?

MR. ROLFES: During the time period when the mobile in vivo unit did come in, yes, there were some individuals from crafts who were potentially exposed. They were counted. Not as routinely as the chemical operators, but, yes, they still were counted.

There are some notes on some of the air monitoring results from the early time period that, for example, our general area air monitoring results associated with like a guard station. And they said that this is a general area air monitoring result for the guard, but it’s probably representative of the guard’s breathing zone.

So he wasn’t directly involved in the processing but was potentially exposed to background, I guess elevated air concentrations. They just note that on a lot of the air monitoring results.

MR. MORRIS: Just a second. I’ll give you a site research database reference number for this document I’m going to refer to. Bob Morris, here.

It’s a letter from R.C. Heatherton to all employees. It’s essentially introducing the first use of the mobile in vivo lung counting system, and he says, “Each employees’ potential for inhaling uranium or thorium determines if and how often they will be counted. For example, a water plant worker’s potential for exposure is practically nil, and they are not included in the routine in vivo counting program.

Chemical operators who work daily with uranium or thorium have the greatest chance of accidentally inhaling these materials and are counted at least once each year. Workers with only a slight possibility for such exposure such as mechanical employees are monitored about every other year.

Of course, any employee regardless of classification would be counted if air dust data or urine results indicated elevated exposure levels. If an employee was involved in an incident which might have caused significant exposure to uranium, airborne uranium or thorium, they would also be counted."

He goes on to cite the recounting protocol, what would prompt a second recount. But this was a letter to all employees, introducing a new technology into the system. So I think you can find more details and for the record in just a minute I'll tell you what the site research database number was on that.

MR. KISPERT: What's the date of that letter?

MR. MORRIS: It's not dated specifically, but it's clear that it was early in the process, '67, '68.

MR. RICH: It was preliminary to the mobile laboratory, so it would be in the mid- to late '60s.

MR. ROLFES: Arjun, you can address your question about the episodic (unintelligible).

DR. BEHLING: The episodic issue we can address it in Finding 4.3-1, and if we're ready to go on we can discuss Finding 4.3-1 which is described on page 52 of SC&A's report. And it is basically a discussion that is not confined to thorium, but it's a generic issue which at this point obviously has some real implications because before the in vivo measurements were taken, it is the air monitoring that is, in fact, going to be used for dose reconstruction.

And 4.3-1 discusses the generic limitations on certainly associated with air sampling. And I provide a significant number of attachments and examples that define the variability of air sampling data as a function of time and location, time and space. And also the issue of using general air sampling as opposed to breathing zone. And on page 52 of the report I provide some data. These are empirical data that were procured at the

Nuclear Materials and Equipment Corporation, NUMEC, back in that timeframe.

And what it shows is that the ratio between general air and BZ is one that is not a static ratio but actually increases as the air concentration increases. And that's likely due to the fact that when you have discrete source terms, Mark had previously mentioned the issue of breathing zone for guard shacks.

Obviously, one could make a reasonable assumption that when the source term is at a great distance from the people that you're trying to monitor, a general air sample is very accurate in defining a breathing zone because obviously it's not going to vary as a function of (unintelligible)square or some other value which is obviously the case when you deal with work location such as a hopper and so forth that is a source term for breathing zone air sample and having a general air sample that's 20 feet removed.

We know from empirical studies that even five feet can make a several fold difference in air concentration. What this whole discussion’s about, and you will see some examples that I cite on page 52, 54 and in attachments, is how variable these things are. And it also touches on episodic events such as obviously radiological incidents.

And I give examples where on page 53, for instance, where you have air concentrations for various discrete locations over a 30-minute time interval. And it goes from 355 dpm per cubic meter to 140,000 over a half hour timeframe. And these are things that you see throughout when you look at the air monitoring data.

And even when you look at BZ samples, oftentimes a person will take three consecutive measurements at the same location for the same individual, and we’ll see a high and low for three samples that are orders of magnitude apart. And so the question comes into play which numbers are accurate and how do you apply that to people whose work location or job location you’re not even sure.

We have roving maintenance people. We have people who go from one location to the other. And how do you assign air sampling data under these circumstances where we have already problems associated with even defining what the air concentration is in time and space, but you don't even know who these people are especially when we're dealing with roving maintenance people and laborers who were assigned almost on a daily basis to different jobs.

So the generic question here is how well do you define an internal exposure based on thorium air sampling data that mixes BZ sampling with general air sampling. And over time and space you have to somehow or other get your hands around a bounding estimate when you don't have a clue as to who these people were and where they worked in time and space.

MR. ROLFES: We certainly feel that our use of these air sampling results will provide a bounding analysis of the potential exposures given the additional process knowledge that we've obtained based on records retrievals, document searches, worker interviews. We also know that based on the processes these were typically campaign-type processes rather than continuous production operations.

So by us using these higher concentrations for 2,000 hours per year without respiratory protection factors being credited and without any other adjustments to the air sampling data, we feel that we are assigning a very claimant favorable intake by assuming that this concentration existed for 2,000 hours per year. Based on information some of these operations lasted a day, so by us assuming that an individual was exposed to 2,000 hours per year at a high air concentration is very claimant favorable.

Getting back to what we were discussing before about the thorium bioassay investigations I have a letter dated November 2nd, 1965, and this was in regards to the people that were chosen for quantifying thorium exposures in vivo. It’s titled “Thorium Bioassay Investigations”, and it says, “During the past year there has been a small thorium operation at the pilot plant in which about 25 people have been receiving exposure to airborne thorium. It’s understood that there is a good possibility that there will be a larger operation in the refinery...”

and there's some discussion about crushing thorium in the plant.

Let's see, let me get on to, they had chosen two of the foremen who were employed -- that's the earlier time period. This refers back to the earlier total body counting that was completed. The two individuals that were sent to the University of Rochester, two foremen, were employed in Plant 9 thorium operations were counted in the whole body counter at the University of Rochester on November 30th, 1962. At that time Dr. Hirsch interpreted the body results to show that there was not a permissible concentration of thorium in either of the employees.

It goes on to say if the results show that these persons had a detectible quantity of thorium in the body, we would want to have other persons with thorium exposures counted with the possibility that we would eventually count all of our employees who are exposed to thorium. This would involve a total of about 30 people, it appears, or 80 I think. But it appears to be 80 people. So it does show there was some attempt to quantify historical thorium and current thorium exposures.

MR. BEATTY: May I ask a question, please? This is Ray Beatty. You mention in that story that this was two supervisors tested. I really question why would they send administrative personnel when the chemical operators and maintenance people would have been the most highly likely to be involved in an incident at work.

MR. ROLFES: The one individual I know was in Plant 9 and was associated with the thorium blender operation, the explosion that occurred in Plant 9. He was one of the first individuals that had gone back into the area.

MR. BEATTY: My concern would be that the persons that were right there at the incident would be the ones that should have been tested. And I hear this a lot from claimants that supervisory personnel being in an office setting most of the time or administration areas, well, I just see a little problem with it.

DR. BEHLING: I guess, again, the question is obviously in the process of revising the TBD and I guess we’re going to be looking to you to tell us when you finalize some of these issues in establishing a new database and drawing your conclusions from some of that data. And can we expect to see the revision to the TBD any time in the near future?

MR. ROLFES: Bryce, do you have a feeling for the time period that we'll be able to produce the white paper?

MR. RICH: The time period, a couple weeks I would imagine. We're in the final phases I think even though we're still developing information. That's the reason I hesitate just a little bit.

MR. ROLFES: But, yes, as soon as it's available we will make it available to the Advisory Board work group members.

DR. MAKHIJANI: Including the whole model and all the information. Are you still researching information and doing interim white paper or --

MR. RICH: The answer is yes.

DR. MAURO (by Telephone): Mark, this is John. I recently reviewed some work dealing with Bridgeport Brass. And I was looking at a similar situation where you have air sampling data, and you’re going to use that data as your primary basis for constructing exposure matrix. I bring this up just because I notice certain approaches that were taken there have raised some questions in my mind, and I thought may be valuable to alert you at this time some of the things that I and we would be looking at.

And I noticed in the Bridgeport Brass there was a lot of pooling of air sampling data whereby you may have taken data from several years and across operations and from that built a distribution and selected the upper 95th percentile.

Now one of the concerns is that when you, these datasets that you work with -- and it sounds like that, Arjun, you’ve already looked at some of this -– to what degree do you try to construct some granularity where your distributions, the datasets, are grouped by time, campaign, location, type of operation, and have different distributions for these different, I guess, segments whether it’s in time and operation.

And then from there select the

percentile that you're going to treat as being your bounding number for different groups of workers. Could you give, you may have already covered this, but have you broken it down in a finer way or used pool data?

MR. ROLFES: Bob.

MR. MORRIS: Bob Morris. The data for thorium air sampling from in the '50s and '60s is broken down by individual calendar year. We could have in some years subdivided two different operational locations, but in most years the operations were focused at one operating location. So I suppose we could have subdivided two or potentially three of the years into different operating locations, but we chose not to. Now that's a choice that could easily be reversed if you think that's necessary, John.

DR. MAURO (by Telephone): Well, no. I only bring it up, I'm not saying it's necessary, but when you pool data, whether it's, you know, for example, in this case it sounds like you are breaking it down by time. But if you are taking data from a number of different operations and you pool it, what happens is that would work if you knew your workers that were working were, in fact, being cycling through these different operations.

So therefore, but if it turns out one particular operation might be a little bit more severe, and this goes without speaking to the incident issue. That's, of course, a separate issue. I'm looking at more of a chronic situation and you're going to try to place a bound. The degree to which your white paper addresses the need for granularity in different job locations and job types in a given time period, I'm not saying that you need to break it up that way, but if you don't break it up that way, a case needs to be made that you have, your distribution that you are using does, in fact, reflect what would be considered the upper end operation that took place.

So I just say that because later on we are going to come back to this when we see the white paper. I know this is going to be in my mind when I look at that data.

MR. CLAWSON: I had one question. When we're talking about the TBD is being rewritten and so forth and then the white paper, is this what we're going to receive back from it or is this two different --

MR. ROLFES: Can you repeat that one more time, please?

MR. CLAWSON: Well, we've been discussing that the TBD's going to be somewhat rewritten, and you're going to produce that to us in a white paper.

MR. ROLFES: That's correct. We're going to separate the thorium exposure model essentially out from the TBD so that we can get it to the Advisory Board members in advance of the technical basis document. It's typically a little bit easier to get pieces out to the Advisory Board rather than the entire document as a whole.

MR. CLAWSON: On the sampling of this -- and forgive my ignorance and so forth -- but whenever we pull air samples and so forth like that we have a calibrator that's telling us exactly what the air flow is. That's a prime thing, and we really haven't had calibrated instruments until about ten years ago. And if you go back and look at our data, it could be off. We were using a vacuum cleaner. What type of system was being set up that we're relying on these air samples to be correct? And where did they come up with the number for the air?

MR. RICH: Even in the very early days of the, you know, using the vacuum cleaners, we used Filter Queens, for example, in the very early days, but we did air flow measurements. In other words we had, and we were aware that the air flow at the beginning of the period was considerably less as the filter low. And so there was a measurement pre- and post-sample. And during the early days we averaged. We took the average flow. So even in these early days there was an awareness and a full measurement made on the air sampling devices.

DR. MAKHIJANI: Are you talking about Idaho or Fernald?

MR. RICH: Yes.

DR. MAKHIJANI: Both?

MR. RICH: Idaho I know about and Livermore and the other places I've worked at.

DR. MAKHIJANI: Well, I certainly recall stack monitors that had not been calibrated at Fernald for decades. This is from memory, but I would wager that it's pretty good memory, and I would produce the document.

MR. CLAWSON: The reason I was wondering is I've been through the O drive and so forth on that, and I was checking. And it seems like they used one data point for all the air flows. And you can go from one sampling process to another and it's going to be off.

MR. RICH: Normally what they tried to do, for example, for constant air monitors or other devices, they tried to set them so that they sample at five cubic feet per minute as the air sampling rate for general air samples or one cubic foot per minute or whatever the air flow measurement is, they tried to tune the restriction so you get the air flow measurement that you desire. It makes the calculations easier.

MR. CLAWSON: And I understand that. My issue and my point that I'm trying to get to and especially even on these air samplings, breathing zone, whatever and so forth like that, you've got to assure the data integrity of this. And just with my layman's brain going through it, we're using a set point there. And it seems like to me that it's an over-bounding one.

I'd like to be able to see where, you know, the post- and so forth because I've seen what you were talking about of checking what the air flow was. But I know that in my world it was not done that much. I guess I'm just looking at I want to make sure that if we're going to be using this air sample data that I want to be able to make sure that it is correct and how it was done.

MR. MORRIS: All you're really talking about is an uncertainty analysis. Isn't that right? This is just one of the parameters that goes into the final answer. It's two liters per minute plus or minus a half liter a minute or five cubic feet per minute plus or minus one cubic foot per minute. I mean, fundamentally you've got a vacuum cleaner that's got a fixed amperage going through it. And you've got a new filter because these were 30 minute long samples.

So the reproduce-ability of a lot of

these systems is in favor of low uncertainty because a vacuum cleaner can never pull more than a certain amount. And with a new filter it's going to have a certain amount of resistance to it. So the uncertainty may be a little bit less than you would begin to think off the top of your head.

MR. CLAWSON: I know you could take one, and you could take another one, and you could have a totally different sample.

MR. MORRIS: How different is totally? Is it a factor of two or a factor of a hundred or a factor of a thousand?

MR. CLAWSON: There, now you've brought exactly what it is.

MR. MORRIS: The answer is a factor of two. The physical hardware constrains that. It's not going to pull like a jet engine if it's got a 12-volt motor on it.

MR. CLAWSON: Let's take and say you use a Filter Queen, and you use a shop vac.

MR. MORRIS: Both pulling through the same filter? In fact they didn't do that though. They used the same sampling equipment. You have procedures that showed the sampling equipment they used.

MR. ROLFES: There are procedures documented, and there is indication in the Industrial Hygiene and the Radiation weekly reports in that early time period that the various pieces of equipment were tested, calibrated, and there were quality control procedures in place. Also, on the air sample results that we have, we also do have documentation of the air sample volume that is pooled, the time that the sample was drawn, as well as the total counts and the counts per minute, and then a conversion to (unintelligible) per minute per cubic meter.

MR. CLAWSON: So you do have documentation showing that these were calibrated so that they were checking the air flows.

MR. ROLFES: Yes. I don't know if we have every report, but there are early reports that do indicate that the air samplers and other laboratory equipment was, in fact, calibrated.

MR. CLAWSON: What timeframe are we talking, the early or --

MR. ROLFES: Back in the early '50s. I know of '53 off the top of my head so it's very close to the early time period.

MR. RICH: Could I add just one other point? Hans, you mentioned the uncertainties associated with doing intake based on air flow our air sampling data. That's well understood. Just as an example I remember we did a, we had a little contract to do some research and development for NRC. And one of the studies involved putting a breathing zone sample on both lapels of a welder that was doing some Plasmarc welding on a contaminated piece of equipment. And we got a factor of five difference on between the two lapels.

So and for that reason there are enormous uncertainties associated with doing dose reconstruction or determining the intake and then the deposition of what stays in the body using air sampling data. We recognize that that is the least accurate. There's enormous uncertainties. But -- and it's been mentioned here, we're not assuming taking credit for respiratory use.

If you used any type of a respirator that uses a high efficiency filter, you get 99.97 percent of filtering efficiency which is three times ten to the minus four, not ten. And that’s at the low point, 0.3 microns. The filtering efficiency is higher on either end. And so as a consequence you got four as a magnitude conservativism if you’re wearing a respirator. I’m not assuming that they wore a respirator.

And in addition to that the respirable particles in most operations you get a major share of the particulate distribution that’s non-respirable. And so as a consequence we will default to in vivo measurements where measurements are made that’s actually deposited in the body or it can be reconstructed by urine or fecal analysis which is a more direct measurement.

Now what I would say that in using air sampling data to reconstruct intake, you’re generally going to come up significantly on the conservative side. And that’s the only thing that we’re saying here is that we feel that in these very early time periods, if we have a decent distribution of air sampling data, we’re going to err on, in a very conservative fashion. And that’s fundamentally all I wanted to mention.

DR. MAKHIJANI: Well, your conservatism about respirators would only be true if they were wearing respirators.

MR. RICH: I said that already.

DR. MAKHIJANI: Well, let me review, but lots of evidence that a lot of people didn't wear respirators. Here's a document from 1954.

MR. RICH: You're assuming that they all ---

DR. MAKHIJANI: I'm not. This is an SEC thing that applies to any worker in the group, right? And here's a document from 1954 that deals with respirators and also a particle size. It talks about thorium fumes so you could have some micron particles there.

MR. RICH: Go on.

DR. MAKHIJANI: Quote, "cutting thorium derby in half by hacksaw, the operation is done dry and releases a considerable amount of fume in the area. Saw operator was without any type of respirator." Okay, so there is some evidence that respirators were used some of the time, and there's some evidence that they were not used some of the time. So that actually assuming no respirator use is not a conservatism, it's simply applied to the manner of work of many workers and in many periods.

Moreover, assuming that your applied micron particle size would not necessarily be conservative for thorium fumes when you're cutting dry by hacksaw, and you could have 0.3 or 0.4 micron particles. So I think, well, I guess I won't say more. Maybe it's necessary to see the white paper. How general area samples are handled. How fumes are handled. How particle sizes are --

DR. BEHLING: When respirators were used, obviously, I looked at a lot of memoranda that talked about the respirator issue, and again, I'm comparing it with today's methodologies and protocols for assigning respirators where you fit test, where you clean these things to specification.

And repeated memoranda talk about respirators that were so filthy that people didn't even bother using them, and they clearly were not fit tested. They were not monitored for good seals and any of those things. Today when you go into a radiological environment, a minor amount of one day or two days worth of facial hair is an issue that has you sending back to the bathroom for a shave. In those days I'm sure none of that existed. So you have to be careful about applying today's standards to periods of time when people simply didn't care.

DR. WADE: Question.

MR. GRIFFON (by Telephone): Hey, Brad?

DR. WADE: Hold on, Mark.

MS. BALDRIDGE: One of the documents that I read from old documents says since work practices are a primary determiner of exposure and is not a measurable factor, how are you taking into account the different work processes of people? I mean, if one man is really careful about how he does, you know, that he's conscious that he could get exposure and somebody else has been basically told there's no danger, he can just do whatever's necessary, how are you making the distinction there, and particularly when the two samplings that were evaluated were management? These are people who were not in there grinding, cutting, whatever, you know, their inhalation rate is going to be significantly different than somebody who's on a grinder.

MR. RICH: At that point we're assuming the worst in all cases, the maximum --

MS. BALDRIDGE: How can you make that assumption based on two people who weren't in there grinding, filing, inhaling the fumes that were basically, you know, in, if they were on the floor, they were in a mobile situation where they were moving, checking from one area to another or doing their paperwork.

MR. RICH: Those are just two people who were mentioned that were taken, but there were others.

MR. ROLFES: If an individual entered the area and was potentially exposed to high air concentrations and then left the area, our results would certainly be bounding for that individual by applying a 2,000 hour per year inhalation at that high air concentration.

So our approach, if a person was only casually or based on air monitoring data, if a person only casually entered the area without respiratory protection for a short amount of time, by us assuming that the individual was in there for the 2,000 hours per year without respiratory protection, our dose reconstruction results would be very claimant favorable for that individual.

MS. BALDRIDGE: Now the data that you have now, are there still gaps in that? I mean, initially when the technical basis document was developed, there were gaps where that was reconstructed. Are there still gaps that exist that you're using to reconstruct the data for?

MR. ROLFES: I think it's important to clarify that for a technical basis document there's not supposed to have every piece of information in it. There's always going to be things that we're not aware of. However, those pieces of information are frequently documented in a person's dosimetry files, and it's usually those dosimetry files that are the most important piece of information for us in a dose reconstruction. So if we would look into our dosimetry files when we looked at a dose reconstruction, if we saw that we had a gap in dosimetry data, for example, we could go to the technical basis document to learn how to interpret that gap and to assign a claimant favorable missed dose or unmonitored dose for that. And so we feel we have a very comprehensive approach to dose reconstruction based on the dosimetry results that we have for individuals in combination with our site profile documents.

MS. BALDRIDGE: Weren’t they measuring thorium with (inaudible)?

MR. ROLFES: External exposures from thorium would, in fact, have been measured by dosimetry.

MS. BALDRIDGE: When would that have started?

MR. ROLFES: Nineteen fifty-one, if there was thorium at the site in 1951 so it certainly would have been, it’s much easier to detect penetrating exposures from thorium than it is uranium.

MR. CLAWSON: That brings up a question of, I know that I bring up Rocky Flats, but have we checked into the information in each person’s dosimetry of, didn’t we go back at

Rocky Flats, have to kind of check the information on that? What's in each person's file for the reconstruction?

DR. MAKHIJANI: Brad, we did that when at first there was a discussion around the completeness of the HIS-20 database, the computerized data file. And then that was found to have some gaps, and then at that point we did, NIOSH stated that they were going to rely on the individual dosimetry files. And at that point we did an investigation of the completeness of the individual. But we have not done that.

MR. CLAWSON: We have not done that with Fernald.

DR. MAKHIJANI: So far as I know. Hans may have -- No, I don't believe we have done that.

MR. CLAWSON: I think to have something, you know, as we're going into this, but we, for our own reliability we better be sure that we check into that. I don't want to get down the road and have to revisit this issue. So that's something we may want to check into.

Did you hear that?

MR. GRIFFON (by Telephone): Yeah, Brad, I agree with that. I just think, you know, the last couple meetings I've been trying to in my mind scope how we would do that, and I'm not sure exactly until we see. We don't have all the pieces for NIOSH's approaches yet. That's what I've been trying to piece together in my mind.

And I was just, had a question for Mark on the statement he just made. I mean, maybe this is something, again, going back to the individual files versus the sort of database approach here. For thorium we have all this air sampling data which I've been flipping through here while you've been talking. And your response a few minutes ago suggested to me that some of this was in the individuals' files as well, or not the air sampling data you were referencing, just broad dosimetry data?

MR. ROLFES: Typically, the air monitoring data is not associated with specific individuals' files.

MR. GRIFFON (by Telephone): So even the BZ stuff wouldn't be --

MR. ROLFES: Correct, even the BZ --

MR. GRIFFON (by Telephone): No, I just wanted to clarify that. Yeah, I agree, Brad, in general. I think we need to consider having that --

MR. ROLFES: There are some exceptions to that, Mark. For incident reports there is sometimes some air sampling data associated with that.

MR. GRIFFON (by Telephone): Okay, thank you.

Can I just say one thing on this whole, I think, 43.1 through five actually. I'll track these actions again today. I think it's sort of already an action, but this white paper everyone seems to be bringing up again that we really have to wait to see the white paper. I think we can talk in circles on this until we have something more specific to react to.

But I would ask that in the, if it doesn't already address it, in the white paper I would hope the question of what buildings are covered and the one that John brought up are you going to break it out by building or is it going to be sort of a site, across the site approach where the one model fits all areas? But also just the, you know, what buildings are considered, quote/unquote, thorium buildings for the application of this model for assigning internal dose.

And the second question is, and I think it's already listed sort of in 43-2, or I'm sorry, three, the question of how you're going to deal with the different jobs. And I think you've already probably got that in your draft, but I think those things need to be in that white paper somewhere so we can understand those.

And the last question I would have and then I'll shut up for a little while because it's a little harder to participate on the phone here, but the last question I had, Mark, you mentioned again a few minutes ago that you have some Health Physics or whatever type of weekly or monthly reports from the early years?

MR. ROLFES: Yes.

MR. GRIFFON (by Telephone): You were responding to Brad's question about the calibration of the air samplers. I think we brought this up once before, and I was wondering if these, because at one point someone indicated there were all these monthly reports somewhere. And I think you had said that you were still looking for some of them.

MR. ROLFES: Yes.

MR. GRIFFON (by Telephone): Are those all on the O drive or are those, can those be, or even if they're not specifically put in the AB document area, if you can give us like the names so we can find them on the site research database.

MR. ROLFES: There's quite a number of them, Mark, but there have been some put on from the earlier years beginning in the '50s. We have, I don't want to say hundreds of them, but I've probably seen at least 50.

MR. GRIFFON (by Telephone): Are they all on the site research database?

MR. ROLFES: Correct.

MR. GRIFFON (by Telephone): And do they have a name that we could search by that would help find some of these? Are they Health Physics reports? Are they, you know.

MR. ROLFES: This one in front of me is IH&R

Department Monthly Report. So maybe IH&R might be the easiest way to find it in the site research database.

MR. RICH: There are several names.

MR. ROLFES: Yeah, they're also referred to as the Health and Safety Reports, Radiation Safety, Fire Safety --

MR. GRIFFON (by Telephone): So a broad number of names which always happens at these sites.

MR. RICH: Otherwise we'd have them all in front of us.

MR. ROLFES: It can be tricky locating them sometimes in the site research database.

MR. GRIFFON (by Telephone): And in your -- this is a little off the current discussion, but we had talked about some of these early reports sometimes have summary statistics that are helpful when you're looking at the validation of the electronic databases. Do any of these reports have that kind of like summary? You know, for this quarter we had 400 thorium samples taken?

MR. ROLFES: Yes. I don't believe I have one in front of me, but for example, in the early time period in, say, 1954, one of the Industrial Hygiene and Radiation Reports had the number of film badges that were assigned, the number that were read, the number of uranium urinalyses that were collected from employees, the number of clinical urinalysis results that were collected from employees, the number of accidents that occurred, the number of visits that were made to the clinic onsite for either occupational-related diseases or non-occupationally-related diseases. And they also had the number of radon breath analyses collected. So something else that I'm sure I've forgotten.

MR. CHEW: Brad, I want to make sure we don't lose your question because you asked about the data gaps here related to the call at Rocky Flats. Just to refresh our memory that was about the 1969 time period and Arjun was talking about the data gap that you were discussing that was missing at Rocky Flats was external and there were some issues of why some badges were not read and some people were not wearing badges because (inaudible). I want to make sure we don't mix up what we're talking about. We’re talking about internal versus external data gaps.

MR. CLAWSON: Well actually, what I was talking about is that we had been halfway through Rocky Flats and then we kind of had to back track to be able to go back and look at the claimants’ individual files for completeness, and it kind of seemed like we have to back track. It covered everything on that if I wasn’t correct. There was large gaps and so forth. Now I didn’t want to get three-fourths of the way down the road and end up having to come back and look at those things. I wanted to be able to address that right up front for the completeness of the individuals’ files.

MR. CHEW: I think we need to have the question exactly posed to searching for what you’re looking for.

MR. GRIFFON: Yeah, I think the notion here should be that we need to have the work group and the Board independently need to sort of have a sense of, you know, because we’ve heard, much like the Rocky Flats scenario, that you have a couple coworker models, you just provided the one for uranium. But you've indicated that there is probably very little reliance on that because most individuals have sufficient data in their files to be able to reconstruct their personal doses from their personal records.

And I think we want to do, we want just to verify that, and I would think it would be a worthwhile task for SC&A to do, again, emphasis on small but statistically significant. A small sample to sort of say, yes, we agree or, no, we don't agree that the data is there in the individuals' files to allow for internal and external DR.

MR. RICH: This is Bryce Rich again. Based on the record keeping requirements you would expect the personnel dosimetry files to be (inaudible) gaps. That'll be good data. All of the personnel dosimetry data will be in the file. To say that we don't have gaps in the air sampling data is incorrect. The air sampling data was not kept with the same regularity as the personnel dosimetry data. So there are gaps that we're trying to fill by the way. But we do have a large number of air sampling data with a wide distribution and a very conservative, I'm convinced, dose reconstruction.

DR. ZIEMER: But those are not associated with individual claimants --

MR. RICH: They are not.

DR. ZIEMER: -- as I understand it.

MR. ROLFES: They're not in the dosimetry records; however, there are some BZ samples listed as, you know, so-and-so was doing this operation, and this is a BZ sample from this operation for him.

DR. ZIEMER: For that person.

MR. RICH: An individual associated with a major incident, and then the report would be in his file.

MR. ROLFES: For an incident report that may be in the individual's file, but for, I was just making the point that oftentimes the BZ air sample results do have an individual's name associated with the sample results.

MR. CLAWSON: Well, I just want to make sure that, because I know that we're going to be discussed with this several different people as their data integrity and so forth for their individual dose.

DR. ZIEMER: Could you help me recall? Did we have SC&A –- did your statistician suggest a number of random samples from the Rocky database? Or how did we proceed on that? I'm trying to remember, or, Mark, do you recall that?

DR. MAKHIJANI: Mark, do you want to --

MR. GRIFFON (by Telephone): Arjun, you can describe it.

DR. MAKHIJANI: We took an initial look at a few and found there were periods where workers were not monitored, and then we went to our statistician to design a sampling program that would enable us to say with confidence that since we took, I think in all there were 32 cases, individual dosimetry records we looked at, internal and external.

DR. ZIEMER: In detail.

DR. MAKHIJANI: In detail. And then since there were periods for which some of the workers were not monitored, including some periods where many or most workers were not monitored for, as I remember, external (inaudible). Then we had, at Rocky Flats we had the particular situation of a group of workers that were retrospectively determined to be high exposed workers and were called back for review.

And we picked 20 of them to look for completeness of data in their files in order to judge whether a coworker model could be built from that. And so we did two different completeness investigations, one for workers that were thought to be highly exposed retrospectively looking back from the '90s cumulatively, and then one random sampling.

MR. CLAWSON: Well, I just wanted to bring that up. And somehow we've got to be able to look into that and capture it. I want to get this started now instead of halfway --

DR. MAKHIJANI: Is that something that you would want us to look into, the uranium paper? Because the thorium paper's obviously not complete.

MR. GRIFFON (by Telephone): Yeah, I think the thorium, and it's also clear that the thorium, you know, as far as personal files, it's not a completeness issue.

DR. MAKHIJANI: Right, true.

MR. GRIFFON (by Telephone): I mean, you've got a separate model going on. But I would say you want to do, I think, maybe a task, a good task would be to have SC&A come back with a sampling plan first before we just go hog wild into it. Let's get a sense of what you think, you know, again, Arjun, just like we did at Rocky, go back with your statistics folks.

And I would think we would focus on internal uranium and external and look at, and I would give you, I mean, my feeling is the similar factors that we outlined for Rocky apply here as well. Like I think we'd want to sample the workers from the different decades but also maybe workers from different job types, you know, operations, supervisory, administrative, maintenance.

But I think that would be a starting point as come back to the work group with an approach on how we might do that. And then we can talk through it more.

DR. MAKHIJANI: Yeah, Hans and I can certainly get together and think up something, talk with Harry about it. Now at Rocky Flats we did not do job types or sample by building. We did a more crude approach because, as you recall, there was a considerable amount of discussion and a desire by the working group to limit the amount of investigation that we did. So as a result the end product did not tell us lots of things and did tell us lots of things. And because the sample size was not defined by building, job type and, it was defined by two broad periods basically. That’s it.

DR. ZIEMER: I would think the plan itself would address whether we need to do it by job titles or by buildings or what. That could be part of the plan, could it not?

DR. MAKHIJANI: We could look at what it would take to do it by job types and building.

DR. ZIEMER: If that’s even needed.

MR. CLAWSON: I guess the thing is you need to --

DR. ZIEMER: Develop the plan.

MR. CLAWSON: -- develop the plan and let’s sit down and take a look at it. I guess because some of the points of interest to me is going through the TBD and so forth there was a lot of interesting information to me like the clothing worker, so forth like that of all of a sudden coming back. Also, how they issued a lot of different clothes. I think what I'd like to do is be able to have you guys bring us back a plan. We'll discuss that, and then we'll continue on. We'll make a decision at that time.

MR. GRIFFON (by Telephone): And I think we did -- I'm sorry -- I think we did for Rocky. We sort of, like you said, Arjun, we did one subset of production workers, but then we looked at a larger, and that was just random across, you know. And I guess the idea there was that, or the issue we were trying to get at there was that we wanted to see if the production workers certainly were all sampled or had complete data. But also we wanted to see if sort of all job types so that was the random --

DR. MAKHIJANI: And we did have some fine grain things show up even in this. We discovered that uranium workers were not monitored in a certain period whereas plutonium workers were more completely monitored. So we were able to tell a lot of things and because the sample size was --

MR. GRIFFON (by Telephone): Yeah, and in your plan if you can sort of think about the purpose of each piece that would be helpful, too. For instance, we have to think about how we're doing dose reconstruction for these individuals and if you have uranium, you may not have -- well, I guess I would leave it up to you.

But in my mind there's a couple questions for if I looked at an individual's file for their uranium samples, one thing I'd want to know is, okay, given their job type were they on the schedule that they were supposed to be. So if they were an operator in a certain building, and the procedure said they got monthly sampling, were they, in fact, getting monthly sampling?

But the other question is, and probably the sort of bottom line question would be did they have a uranium urinalysis toward the end of their career? Because in many ways you could really use that as a bounding measurement to bound their dose over their entire career if --

DR. ZIEMER: A close-out sample.

MR. GRIFFON (by Telephone): -- right, right, close-out sample kind of thing. That would be essential, and that's one thing we found in Rocky Flats, and they actually had to revise the coworker model because during the D&D period we did not have that urinalysis data for as many people as they did during the production years. So we had to extend that coworker model. I guess those are the questions maybe to incorporate in your plan.

DR. WADE: This is an important issue, and maybe we could talk about it just for a minute, but I think we really need to proceed in stages here to get this right. I would propose that the first thing that happens is that SC&A thinks about this and then comes back to the work group and says we're going to develop a plan to include these factors. And you sort of lay out the variety that you're going to --

DR. ZIEMER: In your plan, is that --

DR. WADE: -- then the work group can comment upon that. And then based upon the work group comments you develop the plan. If we don't do that, we're going to do that eventually anyway.

MR. CLAWSON: I just passed déjà vu or whatever. I just wanted to make sure we addressed this up front. But, Hans, --

DR. BEHLING: Yeah, I think Mark has already touched on it. We have Finding 4.3-2, 4.3-3, 4.3-4 and they all basically touch on the common theme, and that is we've come to the conclusion here that for thorium internal dose reconstruction there's likely to be very little, if any, data in the personal files. Mainly, we're going to have to rely on your white paper that will define what is the potential upper value that we may have to assign to these people in the absence of information.

And I guess I have multiple things that I want to discuss here on that issue when you develop this plan. One is obviously to always make a distinction between what do we assign to a person who is likely to be compensated, and we have to rely on a best estimate.

And frequently our approach to doing dose reconstruction based on generic information may differ significantly from one where we have a maximized dose estimate. So I would like to ask you to keep that in mind when you do a white paper and distinguish between best estimates as opposed to maximized dose.

And also, talk about the issues that are identified here as the various findings here. We've already discussed 4.3-2, and from what I gather we're going to dismiss or discard the issue of the 1,050 MAC hours as a default value for a bounding intake.

We are also in 4.3-3 I want to again make sure that when we talk about the issue of identifying a person even though he may have some cards in his file that says he was a certain process worker or chemical plant worker on a job location. If you look at 4.3-3, we identified problems with that, too, because I've identified numerous attachments that talk about maintenance people, project labor pool.

And so you're going to deal with a lot

of unknowns for a lot of people in terms of where were they working, and what were they doing. So when you develop a protocol that will probably have generic applications for all workers regarding the internal exposure to thorium, you're going to have to keep all these issues in mind.

Because as Arjun, and as the regulations require, you're to basically identify all individuals for their potential exposure. We're not looking for what is a best average exposure, but what is the potential maximum exposure because we're here to protect every person, not just the average person.

FINDING 4.3-4

And so I think we can probably go through the next several issues that go all the way including 4.3-4, the inability to account for internal exposures associated with radiological thorium intakes. And again here I had provided a significant amount of documentation. I talk about the frequency of these events, and they were not infrequent when we talk about, for instance, Attachment

4.3-4 on page 71 where we talk about fires and other incidences that would have given rise to significant elevations in exposures.

So rather than belabor all these issues at a time when we don't have your white paper, I would just like to at least make you aware that these issues will be looked at in the context of your white paper. And we will assess your white paper in the context of identifying the problems associated with where did this worker work. Was he a member of a project labor pool, a roving maintenance person? Was he potentially exposed to incidents that are not documented in the light of his personal file, et cetera, et cetera.

So if everyone agrees, we can probably postpone the next several attachments and postpone further discussion until we have your white paper. And then we will go back again and look at these specific issues to be sure that we are satisfied with the fact that they have been addressed.

MR. ROLFES: Hans, I did want to clarify that we do have thorium exposure information associated with individual claims beginning in

1968 with the mobile in vivo radiation monitoring lab results.

DR. BEHLING: Well, yes, that’s a new threshold, and maybe ‘65’s a little early because there were just a handful, but ’68 is really, marks a threshold for potentially applying a person-specific data that are obviously in that person’s file. But up until ’65, ’68 timeframe, obviously the application of the generic thorium air monitoring data may have to suffice.

And we just want to be sure that we’re talking about a comprehensive review of all of the variables that come into play here and assure ourselves that we’re not leaving people high and dry who may be very well at the upper end of that. We have, for instance, documentation as Arjun already pointed out in some of the memoranda where people were exposed to 1,200 MCGs that translate to more than 1,800 MAC hours who were not wearing a respirator.

And when you talk about what is his potential exposure for any given year, well, it’s going to be pretty high. We have no clue as to what he was exposed to even during that evolution. But it's clear that if we were even assured that he was taken to a lower exposure environment what that might be. It's likely that he was not transferred to mahogany row and taken a desk job. We just don't know what happened to that individual.

DR. WADE: Let's keep our issues straight. Hans, took us back to thorium. We want to close on the uranium issue in terms of the completeness of the data.

So are you comfortable now that the instruction has been given to SC&A that you want, Brad?

MR. CLAWSON: Yes, I want --

Is it clear to you?

DR. MAKHIJANI: What I wrote down was what Dr. Wade said at the end that you want this plan developed in two stages. First -- at least that's what Dr. Wade proposed. I didn't see an assent from --

DR. WADE: What I'm suggesting is SC&A comes back to us and says these are the factors we're going to look at. How we're dealing with uranium internal and external monitoring, the entire period at the facility.

DR. MAKHIJANI: External (unintelligible) and then uranium for internal.

DR. WADE: And then we come back and say these are the factors we're going to take into account as we develop our plan. The work group will modify that, give you instructions, then you'll develop the monitoring --

DR. MAKHIJANI: Then we'll go to the statistician. So the first step will be a kind of a technical review. I guess Hans and some of us can sit down and talk about what factors and work with you in developing that, and then we will go to the statistician.

MR. GRIFFON (by Telephone): Lew, I agree with your outline of the approach. The only one thing I might ask, and this is a question, it's uranium internal and the external. But I'm just listening for the, I also included thorium post-1968 because are you relying on individual data at that point, Mark? Is that correct?

MR. ROLFES: That's correct.

MR. GRIFFON (by Telephone): So I would add in thorium post -- and do I have the date right, 1968?

MR. ROLFES: 'Sixty-eight.

MR. GRIFFON (by Telephone): 'Sixty-eight, right.

DR. MAKHIJANI: I got that.

DR. WADE: So, Brad, would you expect to see something from SC&A and then you would convene a work group meeting to deal with it before this goes forward? Is that your --

MR. CLAWSON: Yes, that'd be my, we'd like to be able to sit down and discuss with being in the process of being able to look at it and make sure it's covering what --

DR. WADE: So then John or Hans or Arjun, a sense of when we might have that sort of preliminary thought piece that the work group could then react to before you develop your detail plan. You can think about that this morning and maybe after lunch --

DR. BEHLING: We need to talk with John and Arjun.

MR. CLAWSON: While we're thinking about that, more on that, there may be required a comfort break here. If we could have a kind of a comfort break and convene back in 15 minutes. If that'd be all right --

DR. WADE: We're not going to break the line. We're just going to mute the phone.

(Whereupon, the working group took a break from 10:45 a.m. until 11:00 a.m.)

DR. WADE: We're back in session.

MR. CLAWSON: First of all you wanted to read into the minutes the reference document that you had used.

MR. MORRIS: Yeah, this is Bob Morris. I referred to a document by Heatherington (ph) regarding the mobile in vivo radiation monitoring laboratory first use. It's SRDB reference number 2932.

MR. CLAWSON: Great. Now several of these are going to be covered with the new white paper coming out, so we're going to kind of skip over some of these in the matrix.

DR. BEHLING: Yeah, I think what I'd like to do is just again, continue.

FINDING 4.3-6

We talked about 4-3.2, three, four and five, and also just briefly touch on 4.2 (sic)-6 which talks about the need to potentially address thorium exposures during post-production periods, meaning that there were discrete periods of time during which thorium was processed at a given plant.

However, one has to come to a realization that residual contamination of thorium after the last production takes place will remain in that facility and there’ll be resuspension and inhalation and to what extent one may have to look at that and sort of say post-production air monitoring.

And since you’re really only measuring gross alpha, how do you know whether that gross alpha is a uranium daughter or uranium or is it a thorium, and to what extent during this transition period between production of thorium and resumption of uranium are there potential data points where you may have to be careful about assigning that gross alpha as strictly uranium as opposed to thorium? And that’s really due to the fact that the dose conversion values are considerably different between thorium and uranium. And that’s just a point that I wanted to bring up here.

DR. MAURO (by Telephone): Brad and Hans, this is John. Before we move on, during the break I was thinking a little bit about the conversation we just had regarding the white paper, and I just had an idea. And I'd like to just put it on the table. It seems to me that a recurring theme that we always run into is the construction of these coworker models and their granularity in terms of time and space, et cetera, et cetera.

And it's not until we get to this point in the process where we engage that issue whereby we have all these data. And then we look at the data and see, okay, how well does the data serve us and be able to build these coworker models at the level of granularity necessary to support some decision regarding SEC.

What I was thinking about was it seems to me that recognizing that we're always going to have to deal with this kind of issue, it seems to me that when the site profile is prepared, and you're preparing chapter two which describes the site, right now there's a ton of information that summarizes activities at the site.

I would suggest that while that

section is being prepared it is prepared with an eye toward what are the different sub-compartments of activities, job responsibilities, time periods, campaigns, et cetera, where you would say these represent different cohorts of people. I use the term cohort not in the SEC sense, but in just the sense of what a cohort is.

Whereby if you are going to create a coworker approach because you don’t have complete data, you need to build it around these different cohorts because the nature of the activities, the nature of the exposures were of substantially different -- Now we’re dealing with different populations of workers in effect. And so it’s almost like we should front-end this question and to be dealt with before you actually get into the data.

When you’re at the front end in principle you should be able to say, you know, we understand the operations and how the high level of resolution, and we also understand that if you are going to develop some type of coworker model, it has to be developed at this level of granularity in order for it to be functional for you to do dose reconstruction.

So I guess I just want to put that on, it’s almost as if that issue could be addressed in the front end before you actually get to the data, then when you get to the data you could pose questions to the data. That is, are the data of sufficient resolution that allows you to build the coworker models at the level that you need.

So I’d like to just put that on the table because I think we’re going to run into this time and time again, and I realize it doesn’t really apply, it applies to everything we’re doing. And I wanted to unload that because I was thinking about that during the break.

MR. CLAWSON: I understand and appreciate that, John.

DR. BEHLING: I do want to make a comment, John. This is really not a coworker model since obviously this applies to just about everybody. In the case of the thorium air monitoring data, we don’t really have worker specific air monitoring data. We have BZ and GA air sampling data without necessarily identifying specific worker groups. We may have some job locations and plant locations but not really individual-specific data. So this is really not a coworker model.

DR. MAURO (by Telephone): Maybe I used the wrong term. I guess I was thinking in terms of you've got a group of workers, and you're going to try to characterize the distribution of the exposure in that group of workers. Whether it's bioassay data, air sampling data, breathing zone data, external dosimetry measurements, film badge readings, all I'm really saying is that when you try to understand the exposures that any given group of people have been exposed to, the idea of pooling the data is, you want to avoid that.

You want to try to create a dataset, in this case air sampling data, at a level of resolution that you feel confident that the distribution you create captures the range for a particular group and is not pooled to such an extent that it's diluted to the extent that you really don't know whether you've captured the high end or not. And I can almost see that being done early in the process, you know, not after you've gathered the data but while you're actually characterizing the operations and preparing the site profile, that particular chapter two is prepared with that in mind.

I think right now there's a lot of information of that type, but I don't think that chapter is prepared with that issue in mind. Because in the end that's how you use chapter two. It should lead you to how much granularity do you need in order to understand the differing population groups and activities that took place that will need to be characterized whether it's a coworker model or whether, as in this case, we're talking air sampling data that's going to be used from which you'll pick off a distribution.

Anyway, forgive me. It was sort of in my head, and I wanted to get that out.

MR. CLAWSON: I understand. So, John, let me just try to capture what you just told us. You're talking about when they're developing the site profile, correct?

DR. MAURO (by Telephone): I think that's the right time to do this because you're not already immersed in the data. In other words you're basically, it's almost like a clean way to do it. In a perfect world given the site we would like to have data at a level of resolution, certainly all the individual data that's your perfect world, but you never have that.

And you may have to resort to what we're doing right now, drawing upon air sampling data. But then again if you're going to do that, right up from the beginning you need to appreciate how much resolution do you need in characterizing the workforce? Where they were. When they were. What they were doing.

And that should be done before you're looking at the data so that later when you do collect your data, you could actually evaluate that data from the perspective of will it serve our purposes to characterize the exposures that workers experienced given the granularity that we defined in chapter two.

DR. ZIEMER: It sounds to me like this is a suggestion for NIOSH, maybe two years too late or something.

MR. ELLIOTT: This is Larry Elliott. I appreciate hearing your thoughts, John, and it certainly needs to be considered in the context of how we have approached our work. As Dr. Ziemer points out I think that our strategy has been to look at sites where we have a large number of claims and datasets to apply to those claims and move a technical approach into play, into dose reconstruction practice as soon as we could so that we could use that to the best advantage of a majority of the claims. In some cases we would not be able to complete a given claim because we didn’t have all the information assembled. So you could say maybe we put our cart before the horse here, but that’s why we’re going through this today I think.

DR. MAURO (by Telephone): Yeah, I think so, too.

MR. ELLIOTT: We’re going through this now, and we’re saying, okay, how do we fill these holes? How do we fill these gaps? What are the questions that our overall strategic, general approach didn’t really address for each individual claim? The majority of the claims we feel we're working with a document or set of documents that give us the correct answer for compensation.

MR. CLAWSON: And we appreciate that, John. I'm sure NIOSH will take that into consideration.

We'd also like to welcome Larry Elliott here. He just arrived.

So I'm going to turn this back over to Hans, and he can proceed on.

FINDING 4.3-7

DR. BEHLING: The next one, 4.3-7 on page 86 of the report again makes reference to perhaps a fairly significant program that involved redrumming of thorium.

MR. GRIFFON (by Telephone): Hey, Hans, before you move on to 4.3-7, on 4.3-6 I just wanted to clarify in the previous action we had said NIOSH would post thorium in vivo data, and I underlined here, and associated model.

I'm pretty sure, Mark, that you've posted the data, but is there any coworker model associated with that for people that wouldn't have their own individual data during that time period?

MR. ROLFES: I don't believe the final version has been put out there. The data has been --

MR. GRIFFON (by Telephone): Okay, I just don't want to lose that because your response says done, but I think the second part of that action isn't necessarily completed. Is that correct?

MR. ROLFES: The descriptive information I believe is probably still being finalized. But the white paper that we had completed for the in vivo, the assignment of thorium intakes based on in vivo data, we've put together some documentation of the intakes and everything. There was a descriptive report I believe for that.

MR. MORRIS: Yeah.

MR. ROLFES: I do not believe I have put that back on or I do not believe that it is on the X drive or O drive at this time, but I will make sure it is available.

MR. GRIFFON (by Telephone): Okay.

MR. MORRIS: And probably as a result of this meeting and the comments that we've heard you say today that you'd like us to specifically consider, we'll make one more rev on it before we give it away.

MR. ROLFES: Probably a good idea.

DR. MAKHIJANI: This is the coworker model you're talking about, for thorium?

MR. ROLFES: For thorium, correct.

DR. MAKHIJANI: In this 4.3-6?

MR. ROLFES: Yes, and were we going to separate that out from the, are we going to put one general white paper for thorium out or are we going to divide it into the early time period and the more recent time period? Are we going to have two separate white papers, one for the early time period and one for the more recent time --

MR. MORRIS: We've got a coworker model for, based on in vivo chest counts starting with '68 that goes through '88. And we've got an air sample-based intake model that uses the Battelle TBD-6000 documentation, the equation that's in Battelle's 6000 which is, I think, has been reviewed by SC&A in a Procedures working group. So I think the only issues that could be left is who would be applied to the operator category versus the laborer category versus the --

MR. GRIFFON (by Telephone): I think that TIB-6000 is still kind of under review. I mean it's in the resolution phase in the work group, Procedures work group.

MR. MORRIS: Possibly, I know SC&A has provided a draft.

DR. MAURO (by Telephone): Mark, but bear in mind that it looks like what's more applicable here is 6001, which is the process. We did review 6000 which is metalworking, and that certainly is, we completed our draft. It has not entered into the issue resolution process, but I think that we did not review 6001 which has to do with processing with the thorium issues come in.

MR. MORRIS: Well, essentially the model is repeated in 6001, John.

DR. MAURO (by Telephone): Oh, okay, okay, then in that regard many of the comments in 6000 may very well apply to 6001.

MR. GRIFFON (by Telephone): And as far as one white paper or two separate, it sounds like you already kind of have two separate --

MR. MORRIS: We have two separate --

MR. GRIFFON (by Telephone): -- that's fine.

MS. BALDRIDGE: I have a question. How do you plan to approach the Plant 6 three and a half years where we didn't know that they were processing the thorium because you didn't have access to the records? So obviously the worker records didn't show any, there wouldn't be any worker records specific to thorium in that timeframe so you would have to rely on air sampling. But how do you develop a model where a group of people that you really don't know what they were dealing with since they were handling raffinates?

MR. ROLFES: Well, these individuals were not handling raffinates. They were handling materials that had been through the Plant 9 process in the 1950s. These were leftover scraps, thorium contaminated material such as thorium oxide, incomplete fires, incomplete, so there was a lot of high volume of contaminated scrap.

It was contaminated with thorium, and the idea was to reduce the volume of the materials that were contaminated for storage.

And so what they did is converted the Plant 6 sludge furnace over to handle thorium, and they essentially reduced the volume of the thorium contaminated waste from the earlier production time period. And so we're going to use the air sampling data for that time period for that operation to assign intakes.

DR. BEHLING: Let me just briefly introduce Finding 4.3-7 and that, as I started to say, was an issue that surrounds the redrumming of thorium and there were large, large numbers of drums that on a repeated basis were decaying and corroding, had to be repackaged.

And one of the things that, or at least we were not able to find any BZ air dust data or air sampling data on behalf of that operation, and we know very well that that is likely to be a fairly high airborne environment in this whole process. And the question is to what extent will that also, that particular evolution, be considered as part of your white paper in assessing air concentration intakes.

MR. ROLFES: The redrumming operation was typically done on every couple of year basis.

We do have BZ air sampling data for the three individuals that were involved in redrumming during one of the redrumming operations. That information has been provided to the Advisory Board.

DR. BEHLING: And that was three individuals?

MR. ROLFES: Correct.

DR. BEHLING: And do you have any idea as to how many individuals -- I would assume much of that effort was done by people who were declared as labor pool members of the labor pool. Do we have any understanding as to how many people we might be looking at who were exposed to airborne environments during this redrumming?

I mean, that's an awful lot of drums when I consider, for instance, the issue of the 13,000 drums and the number of years it took for the transfer of that material into silos one and two. When we're talking about periodic redrumming, I'm sure we're talking about a significant number of people. Do we have any idea who they were?

MR. ROLFES: The 13,000 drums of material were the raffinate wastes that were shipped from Mallinckrodt and from Lake Ontario --

DR. BEHLING: I realize that. I'm just giving that as --

MR. ROLFES: -- separate operation --

DR. BEHLING: I realize that.

MR. ROLFES: If you take a look at the quantities of thorium that were handled on the site, the typical production from the early time period was about a metric ton per day. So it was a very low quantity of material --

MR. RICH: Less than.

MR. ROLFES: -- less than a metric ton per day. So I do have some inventory data for some thorium here, and I'd have to take a look at it. But it was typically handled by a small number of people and we do have some breathing zone air sampling results associated with those individuals completing the redrumming operations. So that is something that we will elaborate on in our white paper as well.

MR. CLAWSON: So, Mark, let me make sure that I'm clear on this. When you're talking about the redrumming process in the white paper coming out, you're going to kind of cover how and what people are covered by that?

MR. ROLFES: I would suspect if an individual was, in fact, involved in a job where he was exposed to thorium, I believe he would have been one of the individuals that would have been counted by the in vivo lab at Fernald. In the earlier time periods we're going to have to rely on air sampling data to reconstruct exposures from this pathway.

MR. CLAWSON: Well, the reason why I'm just trying to get my hands around what people were involved in this because I do agree, that's an awful lot of drums to be able to... And I'm just, I'm trying to just figure out how we can (inaudible).

DR. ZIEMER: How many drums are we talking about?

MR. CLAWSON: I see here in the (unintelligible) materials have been redrummed, there's approximately 2,000 drums of material. This is just another report.

DR. MAKHIJANI: There were a lot of thorium drums onsite that involved stored materials because Fernald became a storage site, and a lot of the thorium was not processed there, but it had to be redrummed because the drums corroded.

MR. ROLFES: That’s true.

MR. BEATTY: Just from the campaign record of 2,000. Silo three was also full of thorium.

MR. CLAWSON: And I guess I’m looking at this a little bit different, Mark, and I apologize for my ignorance. But a lot of our processes -- we run an awful lot of people through, and I’m just wondering how we can get our hands around what people were going to be covered by this.

MR. ROLFES: Well, the time period that we’re referring to with the Building 64, 65, the thorium storage buildings at Fernald. Thorium was sent from across the entire DOE complex to Fernald beginning in about I think it was 1972. And this is the time period that the mobile in vivo lab was monitoring people.

So for an individual that was potentially exposed to airborne thorium, had intakes of thorium, these individuals were likely counted by the mobile in vivo radiation monitoring lab at the site. So there would be information in that individual's file that would allow us to reconstruct in a claimant favorable manner his thorium exposures associated with redrumming.

But the mobile in vivo results would be independent of the actual process because we have measurements indicating how much thorium is in the body. But really, you know, how it got there, we can make assumptions about inhalation, ingestion that result in a claimant favorable dose estimate. So the fact is that the data are in the dosimetry files for the individual.

DR. MAKHIJANI: The thorium in vivo stopped in '78, right?

MR. ROLFES: No.

MR. MORRIS: 'Eighty-eight, mobile in vivo monitoring laboratory went through '88.

DR. MAKHIJANI: Yeah, but for thorium I thought your in vivo only went --

MR. MORRIS: No --

MR. ROLFES: That's not the way it was reported.

DR. BEHLING: The next issue I think is something that perhaps Arjun is more qualified to --

FINDING 4.3-8

DR. MAKHIJANI: The fugitive emissions. This came up last time I did cite the memo which is quoted in review and maybe also, in the TBD review and maybe also in the evaluation report. There were quite large fugitive emissions, at least from the qualitative description in the memo. I read it out last time where trays were left in a doorway to dry and then there’s a lot of loose, suspended, or suspended thorium in the air both inside and outside the plant. And the atmosphere is described as very dusty.

And so not only production workers but others would also have been exposed to thorium. I wondered whether and how you were including them in the model. This doesn’t involve just inside the plant. It involves also workers who would have been there outside. I believe the memorandum is in the TBD review. I can try to bring it up. Yeah, it’s on page 41 of the TBD review. And it is in relation to thorium metal production housekeeping, and it’s from 1970.

So this is a period where the actual production air monitoring data, well, the ones that I’ve seen, were not as high as in the earlier period. But the processing operations were leading a huge –- well, the words indicate large fugitive emissions. There were no numbers that I’m aware of.

MR. ROLFES: In what form are they? Arjun, are they referring to air dust or are they referring to --

DR. MAKHIJANI: No, no, air dust. One is probably the worst housekeeping problem in the facility, the (unintelligible) mill equipment leaks at practically every joint. All horizontal surfaces have a thick covering of dust, ventilation is inadequate and so on. So this --

MR. ROLFES: Is that not referring to uranium though?

DR. MAKHIJANI: No, it is thorium metal production housekeeping, Ross 1970. And then the same memorandum explicitly talks about thorium tetrafluoride. During operation of removing calcine thorium tetrachloride and calcium fluoride from the retorts. A stack of trays was left standing on a skid near the south annex door. The door is left open to aid in cooling the trays. The wind coming through the doors blows the loose powder from the trays and spreads it generously through the annex, and so on. So I don't know which door this refers to. I'm assuming it's a door between the building and the outside, but it's not explicit here.

MR. MORRIS: So it spreads it through the annex. That's not out backwards through the door.

DR. MAKHIJANI: But if the door is to the outside, if it's a door between the inside and out, it's an access door, then it would also, it will be on the outside and non-thorium workers on the inside would also be at some risk.

MR. MORRIS: There's no doubt about that.

DR. MAKHIJANI: So this is the problem of fugitive emissions in a period where you're relying on in vivo data, but you may have a lot of workers who were exposed to thorium who don't have any in vivo data because they were not considered to be at risk.

MR. ROLFES: It's always very possible that an individual was exposed to thorium; however, because we do have mobile in vivo radiation monitoring results for the individuals, we can simply assign a missed intake. Because if an individual was exposed to thorium, and we have indication that he was in a thorium area, we could assign a missed intake of thorium based on his non-positive or positive mobile in vivo results.

DR. MAKHIJANI: Now these thorium areas are the same plants as the uranium areas. You're talking about a lot of workers, but you indicated that the thorium workers were actually very few. Well, we have to look at your in vivo database to see how many workers are involved in it, and whether workers who were in a particular building were all monitored in the in vivo or not.

But it's a question as to how workers who were not designated as thorium workers would be subject to this kind of emission. And how are you going to assign the, how are you going to know to assign a thorium dose to them? Or are you going to assign a thorium dose to everybody?

MR. ROLFES: We would, based on an individual’s mobile in vivo results --

DR. BEHLING: But he wasn’t, that’s the point, he was not monitored.

DR. MAKHIJANI: This gets to another question. Fugitive emissions means workers who were not designated thorium workers were at risk of thorium exposure. If they’re not being monitored for thorium because they’re not at risk of thorium exposure but are still at some risk of considerable thorium exposure, how would you know they’re thorium workers, and how would you know to assign them a thorium dose?

MR. ROLFES: Everyone that had a mobile in vivo radiation monitoring lab result was monitored for both uranium and thorium.

DR. BEHLING: But, Mark, that’s the point Arjun’s trying to make, that not everyone was monitored by the in vivo system, meaning that there will be people who for whom there is no record that they were given chest counts for either thorium or uranium.

It may have been a secretary who was obviously considered not at risk who was obviously subject to fugitive emissions but has no record of ever being monitored. How would you know to assign her missed thorium dose based on fugitive emissions? That's Arjun's question.

MR. ROLFES: That will be documented in the thorium coworker model.

DR. MAKHIJANI: Who are you going to apply the coworker model to? Are you going to apply it to secretaries? That's the question.

MR. ROLFES: A secretary certainly does not have the same exposure potential for a chemical operator directly involved with the thorium processing.

DR. MAKHIJANI: That's not the question. How are you going to determine who was subject to fugitive emissions, and therefore should be assigned a thorium dose? I guess we're not talking to each other.

MR. ELLIOTT: No, you're not, you're not. You're coming at it, Mark, from what environmental dose is going to be assigned. That's the way we would view it, an environmental dose. And first I'd have to understand to even enter into this what is considered a considerable thorium fugitive emission and whether or not there are enough of those that merits risk.

MR. RICH: That's the sticking point.

DR. MAKHIJANI: Well, I agree that that's the key point. I mean we just have, I mean, there may be other documentation. I happen to know of this one. It's from 1970, and it describes a routine operation. This is not an incident. This is a piece of equipment that's leaking and a procedure, there's two things. A piece of equipment is leaking and spraying dust. And an operation that was routine that was used to dry thorium tetrafluoride.

So we're talking about operations that were done there as part of their method of dealing with thorium. And so I don't know how long these things were done. I don't know how long this equipment was leaking. That's part of the problem is that we don't have any numbers, but obviously it was a pretty big concern because the surfaces had a thick covering of dust of thorium, that means you're leaking a lot of thorium.

DR. BEHLING: And also it parallels the issue that identifies those four individuals who had an unexpected high uranium intake. Had, for instance, on behalf of those individuals you're fortunate enough to give annual physicals that included a urinalysis that also looked for uranium in the urine.

In which case I believe those four individuals, and we can go back to that particular issue, were obviously identified and identified as unexpected. The point here is that we haven't monitored for thorium; and therefore, we're questioning whether or not they were people who on a parallel level were exposed to thorium. But in this case you have no chance of capturing them as you did in the case of the uranium.

DR. MAKHIJANI: That's right.

MR. CLAWSON: Was somebody trying to speak on the phone? We were getting a lot of static there.

(no response)

MR. ROLFES: For the early time period the Battelle model does allow us to assign a fraction of the intake or a fraction of the air monitoring results to a person based on their job category. And if you fold in the Battelle model for individuals that were potentially exposed to non-processing areas, you know, to general background areas or occasional entrants into the plant such as a secretary or something, we default to one percent of the intakes based on air monitoring data for the earlier time period. So the information for the later time period, the more recent time period, can be documented or will be documented in our white paper.

MS. BALDRIDGE: I have a question. In one of the documents included in the petition it states concerning thorium that there are improperly coded items that at times have exploded and burned. There were large losses into the storm sewer, and that 240,000 pounds of residue was sent to Plant 6 for oxidation. How are you going to determine who would have been exposed during an explosion or a fire concerning thorium? Not only the people assigned to the area but observers, the fire department that may have come to assist in getting this under control. Are these people identified?

MR. ROLFES: Yes, they are. For example, in 1954 when the thorium processes were starting up, there was a very bad incident, an explosion, there was a partial reduction of the thorium in process. Some calcium metal that was used to reduce the thorium was being blended, and there was a little bit of moisture I believe in the materials being blended. The calcium reacted with the moisture and caused a large fire and explosion involving about -- off the top of my head -- around 100 pounds of thorium I believe it was.

The individuals involved with that incident, two of them died because of the incident because of burns. There were other individuals that came to the first aid of, to give first aid to the individuals that had been burned. There is a very detailed incident report and investigation of this occurrence in Plant 9.

There are other documented incidents where there is indication that some thorium sludge or materials contaminated with thorium were burned. And it is documented in documents that we have available to us in the site research database, both the numbers and the names of the individuals that were involved.

MS. BALDRIDGE: Now my point, there have been other incidents concerning uranium, other locations, where in excess of 20 people have been involved in the incident but only five people had it accounted for in their records because only five people were examined for the uranium excretion levels. Now if they didn't have a practice of even recording all the individuals involved in an incident, how can you be sure that the ones who were recorded are the only ones who had been involved?

MR. ROLFES: Individuals that were involved in an incident were required to report to the medical office for giving a urine sample. That was part of the procedures that were documented at the site.

MS. BALDRIDGE: But they weren't always all tested.

MR. ROLFES: That's not true.

MS. BALDRIDGE: Well, there's documentation to support my opinion.

MR. ROLFES: There is, for example, there was a large uranium hexafluoride release in 1966 I believe it was. There is a report documenting what happened, what led up to the event of the release, the number of urine samples collected, the number of urine samples tested, et cetera. I believe there were more than 1,200 urine samples taken as a result of this event from I believe in excess of 200 people.

They did slightly change their procedure because they did not have the capabilities to do as many counts as they typically did. They normally count each urine sample with the barometer in triplicate. However, because of this incident and given the large volume of incident urinalyses, they reduced it to only duplicate counting of the sample.

So there was a documented change. However, it appears that all of the urinalysis results were, in fact, counted. All of them that were collected were counted. Now there could have been some laboratory errors associated with that, just like any other operation. There could have been samples that were lost, contaminated samples. Those are all possibilities.

MS. BALDRIDGE: I just want to make the point that everybody involved in an incident isn't always accounted for.

MR. ROLFES: It's very unlikely, but there may be an unusual case. However, I haven't seen a case where, now it usually follows that if there's an incident, that an individual had to report to Medical and provide a urine sample. And also --

MR. ELLIOTT: Was it within the discretion of Medical to say you don't need to give us a urine sample?

MR. ROLFES: I've never seen such a statement being made or documented.

MR. RICH: Could I just make a statement? Based on a lot of years of experience at a number of different plants, when you have an incident, that is the time when you actually look at the most likely exposed people, those that are in the immediate vicinity of what happened. You sample those people extensively, and that is (unintelligible). Those are the highest exposed people. There may be people in the building or in adjacent laboratories and if you don't get significant exposures in the initial responders or those that were involved in the initial, you don't sample everybody. It's not necessary.

MS. BALDRIDGE: I realize that.

MR. RICH: And so you could have people that said I was there. I was in the building, but I was not sampled, and there's a reason why they weren't sampled because the maximum exposures were bounded by the sampling that was done as a result.

MS. BALDRIDGE: My point is it's not, when dose reconstructions were done, the information that I received even from the evaluation that SC&A did on the initial way that NIOSH was handling things was that they did not attribute that type of dosage associated with an incident except to the people whose file indicated that they were involved in the incident.

In the case where five people were captured and in excess of 20 were involved, only five people’s dose reconstructions hypothetically would have included a dose attributed to an incident. The other 15, 16, whatever, would not have been given that dose consideration.

MR. ROLFES: I think what you may be referring to is our approach that we use for dose reconstructions based on our Technical Information Bulletin 0002. And this is a large intake that we assign to an individual on the first day of his employment as a demonstration of a large and very unlikely exposure to that individual. And this is a gross overestimate of the potential radiation exposures that an individual received, but it is done to provide a timely and quick response for the claim.

So typically when we go back and revisit those types of dose reconstructions, the actual dose that the person could have received was typically much lower than what we assigned in our efficiency process. And I believe that may be what you’re referring to.

DR. BEHLING: I think I understand very well what Sandra is taking about, and I fully understand what Bryce was talking about. If you have an incident, and you realize given the circumstances investigative people would look and say among the 20 people who are the five most likely to receive the highest dose. You assess them, and that’s fine.

And let’s assume that for the five highest exposed people they each get assigned 100 MAC hours. But you realize there were 15 others, and for those five people you will have in their personal files an assignment of 100 MAC hours for that particular incidence. But you also realize that there were people who were in the periphery whose exposure may have been half of that. We know reasonably well that their exposure was less than a hundred, but for whom you have no data.

And they’re not going to be covered by your TIB-0002 because that’s strictly used for the estimation of a person’s exposure who you know you’re not going to compensate. So that’s not going to cover that particular issue that we mentioned with Sandra. TIB-0002 does not address the issue that Sandra raised here.

And that is among the 20 people, the hypothetical case of 20 people involved in the incident, five were assessed. Five people have in their personal records information that would allow a dose reconstructor to take into consideration that radiological incident. But the other 15 whose exposures were less than those five for whom data is available have no data, and therefore, will not have any reference or no accounting for that particular incident. I think that’s an issue.

MR. RICH: Could I just add a comment to what you said? And that is that typically in an incident of this kind you sample people until you get results approaching nothing detected. In other words if you sample people in the immediate vicinity and you get -– and normally it’s recorded in (unintelligible) recording of CEDE, which is cumulative millirem dose, if you get down into the levels at which you cannot see them, then you don’t sample everybody else. If it’s less than 100 millirem or something like that.

DR. BEHLING: I would buy into that, but in the case I think that Sandra pointed out, it may involve some things such as uranium hexafluoride, a very soluble or relatively -- yeah, hexafluoride, a very soluble material. And if when you sample the five maximally exposed individuals that you assume were maximally exposed, by the time you get the data back, you may not have any chance to re-sample the people who are at the periphery. So that you can't do this on a concurrent basis.

MR. RICH: But you know you can take that maximum release of uranium hexafluoride whether the visible cloud that went for long periods of time, it is sampling (inaudible). Everybody in the plant was potentially exposed.

DR. BEHLING: Yes.

MR. RICH: And so they sampled to the point where they were assured that no one else, at least the exposures would be below permissible or detectable levels.

DR. BEHLING: But what you're saying is the fifth person that would have been sampled in this hypothetical case of 20 individuals, would represent a value between not detectable.

MR. RICH: Yes.

DR. BEHLING: But you wouldn’t know that until you get the sample back, would it? All five had very, very high exposures, and now it’s days later and you’re looking at it and saying, oh my god, those other 15 probably had a high dose. It’s kind of late in the day to worry about this.

MR. MORRIS: There are other indicators, too.

MR. SHARFI: From the dose perspective, if they sample them later all you’re going to end up doing is increasing your minimum detectable dose. So they did have a routine program so even if they didn’t do an instant (unintelligible) sample, they do have a routine program that if it was a, by extending out the bioassay to anything longer, it just ends up from our program’s perspective assigning larger doses.

So if their routine would have been positive, which would likely be due to the incident that is not in the record, you would have to, you would do bits which would result in very large doses. So the end people who were associated with instant samples would be easier to bound on a much smaller dose level than the people who are much farther out, and you're relying on their routine samples. So this might go more towards your completeness of data question of does everybody have bioassay data. And if they do, if they weren't captured under the incident main (unintelligible), they still have monitoring data that we can base the dose on.

DR. BEHLING: Well, I agree to a certain extent that even a year later if you have a monitored individual, and there's evidence of an exposure, but then you have to make a decision is this chronic exposure or was this a year ago during the episodic event. And the two are quite different.

MR. ROLFES: I'd like to read this into the record. It's titled, "Bioassay Aspects of UF-6 Fume Release". And it's NLCO-986. And this is in regards to the incident that I was referring to. There are some things that I thought were worth mentioning.

There's indication that, let's see,

"all employees were instructed that if they had noticed any peculiar odor or had any reason to believe they may have inhaled some of the material, they should report to the dispensary. All involved in emergency actions were also asked to report to the dispensary. Urine samples were collected from all these individuals within a few hours of the incident. Follow-up urine samples were collected at the beginning of the workday for several days after the incident."

The activities in the bioassay laboratory. "There was no need for evacuation of the Health and Safety Building since it and the site of the UF-6 release are on opposite ends of the project. Most of the personnel at the bioassay lab continued their routine duties during the release. Urine samples began arriving soon after the release was stopped.

"During the week following the incident, 280 employees and four visitors submitted 1,024 urine samples which were analyzed for uranium by fluorometric technique. In the usual procedure samples are analyzed in triplicate and only (sic) one technician can analyze 60 to 80 samples per eight-hour day. Because of the large sample load and the need for rapid analysis, the procedure was changed to duplicate analyses."

It goes on to say regarding the results, "six employees voided urine samples in which the uranium concentration exceeded one milligram per liter. Their exposures and results are briefly discussed below. No albumin was found in any of the samples from these employees. There was no clinical evidence that any employee suffered damage as a result of this uranium exposure. Elimination of uranium was rapid. During the first few hours after exposure the biological half-life for most employees was four-to-six hours. After 24 hours most employees were up to their pre-incident level."

And it goes on to various case studies of the highest exposed individuals associated with this. So I thought it was important.

DR. BEHLING: It dramatizes the exact point I'm making, and I'm not sure when that particular incident took place. But the situation that Sandra discussed may have occurred many years earlier. When was that particular incident?

MR. ROLFES: This was from, let's see, it was 1966 is when it occurred. It was on, I believe, Valentine's Day.

DR. BEHLING: There were certain incidents involving a high exposures to uranium hexafluoride in the '50s. And again, the question is if you postponed monitoring people that you retrospectively realize should have been monitored based on your biological half-time for excretion, you may miss the point in time when you have a data point that is really something that you want to look at.

And it addresses her issue saying that you may have incidents in early years where you only chose to monitor the maximally exposed individual who then have records for that incident, but not monitored people who were more at the periphery but still were significant exposures. For them you have no data. And I think that's the central issue.

MR. RICH: However, Hans, the normal sampling procedure was after a weekend off;

they normally sampled for long-term deposition and excretion as opposed to the initial (inaudible). So immediate sampling in response to an incident they deliberately looked for a maximum credible to have more sensitive, but to delay a few days would not hamper the total dose reconstruction.

MR. CHEW: I don’t want to get into a long -- your question really kind of poses really a generic question if you really think about it. And you’ve got to look at the practices. When you have an incident where more than just indicators (unintelligible) and we know that from the practice of just looking at the bioassay data. That the monitoring itself, the air sampling itself, the contamination levels, and so you know as an Operational Health Physicist who the right people should have been initially monitored. And if you have a bioassay result come back or it’s just your indicators and you know the extent of the incident itself tells you who you should really be monitoring. And so there’s a lot more than the question, gee, I just monitored five people who were closest.

MR. RICH: Even the area of contamination gives you the boundary.

MR. CHEW: You have to give some credit to the Safety people and the Operational people who say, yes, we feel comfortable we have adequately monitored all of the right people that had potentially a significant exposure due to this incident, okay, just a generic question.

MR. ROLFES: There is discussion of the urine sampling program that's well documented. I don't know what site research database document number this is, but it does describe the sampling frequency and procedures for the individuals for different work places.

FINDING 4.3-9

DR. BEHLING: The next finding is 4.3-9 on page 92, and it just addresses the issue of internal exposures that may have entered the body by way of ingestion as opposed to the more common pathway of inhalation. And we bring this up mainly because of all of the documentation that alludes to poor engineering designs, poor ventilation systems, high airborne concentrations, the lack of training of personnel, the lack of anti-cees and other measures that might have mitigated internal exposures by way of careless handling. And so this particular issue focuses on the ingestion of thorium as a potential exposure pathway that at this point we're uncertain as to how that will be addressed.

MR. ROLFES: Ingestion intakes of thorium will be based upon information documented in the Battelle model.

DR. MAURO (by Telephone): Mark, this is John Mauro. With regard to the Battelle ingestion model, that's one of the areas that we did have quite a bit to say. So I guess regarding basically the Battelle model recent TBD-6000 -- I assume 6001 is similar -- was based on a methodology that pre-dated the latest protocol that you folks have developed and applied for Bethlehem Steel.

So one of our commentaries on 6000 is that that method probably has certain deficiencies. Whether or not this issue then becomes something that we would transfer over to the ingestion model, you know, because that is a cross-cutting issue, and I believe what was used in Battelle was the generic approach that has been used across the board. But that approach has, in fact, been significantly modified with the method that was adopted for Bethlehem Steel.

MR. MORRIS: This is Bob Morris. I think that that's true, John. And if I recall your comments, you actually analyzed the impact of that change very closely, didn't you?

DR. MAURO (by Telephone): Yes, we did.

MR. MORRIS: And my recollection of that is that depending on whether the new model applies or the older model applies the answer is still consistent within a factor of 50 percent. Is that --

DR. MAURO (by Telephone): I'd have to go back and quantitatively, but I remember we did have quite a bit of commentary on the Battelle model and tested it against the, compared it to the Bethlehem Steel. And there was some differences, but I guess what I'm saying is that we have engaged this issue extensively, and it's really a generic issue that's cross-cutting.

MR. MORRIS: That's right, and we have no objection to following the lead that has developed on this. My point here is that it probably is just a parameter that's in the equation. It's not the equation itself. And also the ingestion pathway provides relatively low dose compared to inhalation.

DR. MAURO (by Telephone): Oh, I agree with that completely.

MR. MORRIS: Okay.

DR. BEHLING: The next issue of 4.3-10 --

DR. ZIEMER: Where are we leaving this one, 4.3.9?

MR. CLAWSON: Well, I was going to ask a question. So have we reviewed this OTIB?

DR. ZIEMER: Is this covered by the independent review of the Battelle model or --

DR. BEHLING: SC&A reviewed the 6000 OTIB.

MR. MORRIS: It's the Battelle Technical Basis Document 6000.

DR. BEHLING: And if that's to be applied we'll have to look at that again to be sure that that's something that's --

MR. MORRIS: It's the subject of a Procedures working group right now, and SC&A has provided comments on it. And I think it's in the process of being considered. So my suggestion is to take it in that form where it's already on the agenda.

MR. GRIFFON (by Telephone): The only thing is this might be the more pressing --

MR. HINNEFELD: I would prefer -- this is Stu Hinnefeld. I would propose that Brad talk to Wanda, and they decide between them which forum to solve it in because I don't know that this work group wants to be held hostage to the schedule of the Procedures working group.

MR. ELLIOTT: Or vice versa.

DR. ZIEMER: Or vice versa.

MR. HINNEFELD: I mean, this is a little more, this specific for a site and it may be better to resolve that one in this forum than in a different --

DR. ZIEMER: Than in the Procedures group.

MR. HINNEFELD: Right.

MR. CLAWSON: I guess I'm still kind of at a loss what to be able to do on an action on this. I guess we need to be able to look at, I guess I'd have to talk to SC&A about this. How do we want to proceed on this one? Because John's made a comment that this is something that they were --

DR. BEHLING: I would say this. Since we have no empirical data such as fecal analysis which would give us some handle as to whether or not the ingestion pathway was a significant contribution to internal exposure, we're going to have to rely on a generic model and the Battelle-6000 may be your option for making a default model that can be applied here. But at this point we haven't looked at it to be sure that that's the reasonable alternative for dealing with the unknown of ingested thorium.

DR. ZIEMER: Well, I'm trying to recall, and maybe John will remember, John, does the Procedures group have the SC&A comments on --

DR. MAURO (by Telephone): Yes, it does, but it was transferred to the, in other words as you recall in the Procedures group, whenever we came across an ingestion pathway issue such as we are doing right now for this case, for this site, what we did in the Procedures group was transfer it to a global issue. Namely, right now the ingestion model is the subject of a generic, complex-wide OTIB that's being in preparation.

And so designate it as, okay, we're not going to address it in the Procedures. It's being transferred over as a global issue. Now this brings up an interesting question. That's certainly appropriate in the Procedures group where we have the luxury to do that.

In this particular case here we have an SEC and time is important. Perhaps the appropriate thing to do at the next meeting is to look at the comments -- well, I guess it's a two-step process.

One is apparently the TBD-6001 has adopted the same methodology in 6000. We'll operate on the premise, TBD-6000. Two, it's probably a good idea to make sure that, okay, the comments that were made related to TBD-6000 that SC&A made, is there anything about this particular circumstance now at Fernald that is, you know, the comment also applies here and in the same way.

There may be something about the circumstances of exposure at Fernald whereby the methodology in 6000 may or may not be applicable. But assuming it is, okay, now I'm taking it now the third step would be, okay, we all agree then that the comments on TBD-6000 have applicability here. Then the question becomes let's resolve that here whereby we look at the comments, explore the significance of the comments that SC&A made.

And then at that point, of course, NIOSH could either say, yes, we agree with those comments, and we will revise the methodology in accordance with those comments which by and large say we like Bethlehem Steel, but we don't like TBD-6000. Now the magnitude of that difference as pointed out previously might be relatively small. I'm not quite sure. But it seems to me that's a path forward to resolve the issue here in a timely fashion.

DR. BEHLING: John, I haven't looked at the Battelle model but and just a quick question, is that model linked to something such as air concentration, surface concentration, (unintelligible) ingestion?

DR. MAURO (by Telephone): Yeah, it's an improvement over the old deposition velocity approach and then the assumption that ten percent -- I don't want to get into it. There's a lot of structure to it. There really were multiple developments of the ingestion model. One was the original method which we had some serious concerns about because we didn't think it was scientifically valid. Eventually all of those concerns were resolved in the Bethlehem Steel site profile.

And now in this TBD there is another method which I would say is some place in between the method that originally was developed and the method that was developed for Bethlehem Steel. So it's an improvement over the original one that we had some serious concerns with, but it's still somewhat different and less claimant favorable than the Bethlehem Steel method.

And I believe that NIOSH right now is in the process of perhaps formalizing the Bethlehem Steel method as an OTIB that would have cross-cutting operability everywhere. And my sense would be when and if that occurs, it would also be applied here to the TBD-6000, -6001 documents.

But again as I mentioned I don't think

we have the luxury to wait for -- I don't know. This is certainly your call -- to wait for that or should we try to deal with that at this time? That, of course, would be, the only problem with that is that we'd be moving ahead of the global investigations.

MR. GRIFFON (by Telephone): Well, my sense is -- this is Mark. My sense is let's have an action item that says SC&A to review TIB-6001 for application to Fernald workers. I can't -- and we can coordinate with Wanda. I mean I'm on the Procedures work group also so we can coordinate with that group. But I don't think we want to hold up an SEC work group for a Procedures, you know.

MR. ELLIOTT: This is Larry Elliott. If I can offer another suggestion. You have Mark's suggestion, Mr. Chairman. It seems to me that without Jim Neton here I'm at a disadvantage. I don't know where he's at on this across the complex ingestion model. But I do know that we liked the Bethlehem Steel approach.

I'm a little bit concerned about where TBD-6000 leads us and whether or not it's the right one or not. And we need to think through that. I think it needs to be NIOSH that comes forward with what we're going to do on Fernald and ingestion and which model we're going for and then you can mac (ph) to that. Does that take anything away from you, Mark?

MR. GRIFFON (by Telephone): No, no, no. I thought the decision was made that you were going with TIB-6001, so I was starting there. But if you --

MR. ELLIOTT: I'm asking Mark Rolfes if it helps him or not or it hurts him.

MR. ROLFES: We have documented right now that in the matrix that our approach will be to rely on the Battelle model. NIOSH does have a separate technical information bulletin on ingestion pathways as well. So we haven't finalized the methodology, and our finalized methodology will be in our thorium white paper.

So we did not believe that this was an SEC issue but rather a dose reconstruction issue as to how much we are assuming a person ingests rather than inhaled. So typically for most organs for which we complete dose reconstructions, the inhalation pathway results in a higher dose.

MR. GRIFFON (by Telephone): And, Mark, I was actually going to add on that it may be in the course of SC&A reviewing your model, now I guess I'll take back that action because maybe it's SC&A reviews the approach described in the white paper. I thought you were saying TIB-6001 was one.

Anyway, it may be that this is a site profile issue once they do that review, and we don't have to close on TIB-6001 if that's the model. We could just examine it enough to say clearly the data's there. They have sufficient data to bound, and we don't need to know exactly the how's and particulars for the SEC process. And the rest can go back to the Procedures work group review, and it becomes a site profile issue more than an SEC issue.

DR. ZIEMER: Well, then it would await the white paper still so that you would tell us what direction you're going and then --

MR. ROLFES: That probably would be the best.

DR. ZIEMER: So it becomes a white paper issue right at the moment.

MR. ELLIOTT: So it's back in our lap.

MR. GRIFFON (by Telephone): So it's back with NIOSH with the white paper.

MR. ELLIOTT: Tell you which way we're going. It bothers me that I don't know why we would have a variety of ingestion models out there unless they're circumstance driven and so we need to look at that. And I don't know if Mark and his folks have talked to Jim Neton about this specifically, but I think we need to make sure that Jim's included in this discussion so that we can move forward. So I'd ask that it be put back on NIOSH's shoulders to deal with here.

MR. CLAWSON: Okay, and as soon as we know then we can address it and go from there.

Hans.

FINDING 4.3-10

DR. BEHLING: Yeah, the next one is 4.3-10, and I'm not sure with Dr. Wade's commitment, are you, can you stay for a few more minutes before we break for lunch?

DR. WADE: Sure.

DR. BEHLING: So let's maybe try to go through 4.3-10, and then hopefully that will bring us into our lunch hour, and we can take a break.

But the issue of an unanswered question regarding data integrity, there’s two elements to that. In this one, we didn’t go through it extensively, but we do have one affidavit that was provided by a member of the team of hygienists who was there to assess obviously the work environments by air sampling, and he had some disparaging comments to make about some of the protocols and things that he felt were inappropriate, and obviously you may want to read that.

That’s provided as an Attachment 4.3-10. It’s the sworn affidavit by a person, and he goes through a whole series of things that lead one to question to what extent was the program that was designed to protect the workers compromised by overzealous management who was more concerned about maintaining production quantities over worker health. And you can draw your own conclusions.

In addition to this particular one which clearly does affect the individual workers, there were some other outstanding issues that Arjun had identified in his TBD that go beyond the working environment necessarily to perhaps other indiscretions that involved the use of not counting samples that involved stack monitors and elsewhere. And I'll let Arjun talk about those separately.

DR. MAKHIJANI: Well, the stack monitor samples that were not counted were I think in the '69 to '81 period or '68 to '81 period. We were talking about that at the break. This problem is cited in the petition along with some documentation if I remember right, Sandy. And I believe the person involved who made that decision to not count them regularly and entered zeros even when they were not counted actually has acknowledged that that was done.

So long as NIOSH is relying on -- this is an issue that concerns directly environmental dose. So long as NIOSH is relying on the national lead environmental dose data that was a problem. But I understand NIOSH is now relying on the John King environmental dose model.

MR. ROLFES: I'd like to clarify. The most important piece of information is the urinalysis data --

DR. MAKHIJANI: I understand that.

MR. ROLFES: -- in the person's file.

DR. MAKHIJANI: But environmental dose is assigned to people who don't have urinalysis.

MR. ROLFES: For a small fraction, a very small fraction of the workers at Fernald may not have had urine sampling results. If we have reason to believe that they were exposed to airborne uranium of significant amounts we are going to be using a coworker intake model for those individuals.

DR. MAKHIJANI: Okay. The other question that arises, I think maybe, Sandy, you raised this so maybe you want to state the issue in that regard.

MS. BALDRIDGE: Concerning the accuracy --

DR. MAKHIJANI: Well, reliance on data and documents for use by a particular individual.

MS. BALDRIDGE: At the last meeting it was brought up that the environmental portion of the technical basis document was almost finished. And my concern is whether they used the same references that this individual in question provided for the revised document as was for the original, the one that the individual acknowledged not looking at certain data before they even wrote the report. Without mentioning names, do you have any idea what I'm talking about?

MR. ROLFES: I'm not exactly sure what you're referring to.

DR. WADE: Is there a reference cited? I think you can mention that.

MS. BALDRIDGE: There were several references cited from Mr. Bobeck (ph) that were used in the original environmental portion of the technical basis document. The deposition that he gave at the trial. Before the trial he indicated that zeros had been put into the data in lieu of actual measurements which affects the credibility of the data used in the environmental doses based on stack releases.

I'm hoping that anything that would have his name attached to it either directly or other documents that might have used him as a reference would be eliminated from consideration for the revised environmental portion of the new technical basis document.

MR. ROLFES: We will not be eliminating his input because it has been very valuable to us. We have spoken with him in great detail actually in previous meetings to get a better picture of workers' exposures, effluent of the site. We do have documented interview notes. I haven't made them available yet to the Advisory Board members but documented probably -- what, about maybe five hours speaking with him? So we have about 30 pages of notes of interviews with him. And we can --

MS. BALDRIDGE: Do you have notes of what he did while he was there to know where he actually applied data and where he didn't apply data?

MR. ROLFES: I would have to ask Jim --

MS. BALDRIDGE: Or his recall from his recollection?

MR. ROLFES: We can take a look and see what it is exactly that's being referred to, and we can consult with him to help in clarification.

MS. BALDRIDGE: Because Arjun had mentioned like from 1968 through '81, well, that's, you know, 12 years. I would just question unless he actually had notes that he could rely on, anybody’s ability to remember, but why they put a certain number down at any particular time during a 13 year timeframe.

MR. ELLIOTT: Yeah, but your point is you’re questioning how we’re going to handle the testimony that Mr. Bobeck (ph) gave at the trials in regard to the documentation that his name is associated with elsewhere.

MS. BALDRIDGE: Right.

MR. ELLIOTT: That’s what your point is, okay. It’s something we will look at.

DR. BEHLING: And let me go back in the context with the discussion about this individual. I think that the affidavit that I included as Attachment 4.3-10, I think we briefly touched on it the last time. And, Larry, you mentioned that this individual you hired yourself.

MR. ELLIOTT: I did not hire him.

DR. BEHLING: Or NIOSH hired him.

MR. ELLIOTT: I worked with him.

DR. BEHLING: But I think your comment if I can recall was that he was a credible individual.

MR. ELLIOTT: Yes.

DR. BEHLING: And his comments are very strongly worded comments. And a person who was there, he's not a bystander. He's not looking at this from the viewpoint of a distant observer. He's not a person that I would define as a person with an axe to grind who has malice on his mind.

I have to look at this and question, therefore, to what extent was this whole process more pervasive than we're willing to admit, that it may have involved other industrial hygienists who should perhaps be looked at or contacted to see if they have similar stories to talk about or present in a sworn affidavit and let's go back.

After all, we are going to be using air monitoring data exclusively for thorium intakes prior to '65, '68. And for us to look at that data and say it's credible requires us to have some feeling or assurance that we're not looking at data that has been manipulated as he implies in his affidavit.

MR. ROLFES: Well, the affidavit was referring to a uranium process area, Plant 5, I believe, which our dose reconstruction approach for reconstructing uranium exposures would rely primarily on urinalysis data. So it really wouldn't be an issue for a uranium exposure. But for thorium it could be if this was a pervasive and commonplace practice.

However, based on interviews with industrial hygienists from Fernald this was not a pervasive practice. Industrial hygienists were told to sample the highest areas where a person could potentially be exposed. They were trying to collect samples that were representative of the individual's breathing zone. They were taught to resample high areas. So when there was a high exposure, that attracted much more attention.

There was nothing in the affidavit that I saw that indicated that the high air sample results were destroyed. It actually appears that there were approximately six high air samples followed by one low one that was taken to satisfy the individual's supervisor.

DR. BEHLING: Which one is the sample of record?

MR. ROLFES: All seven.

DR. BEHLING: Is that a fact?

MR. ROLFES: It appears to be. There was no indication that the information had been altered in any way.

DR. BEHLING: Well, you may log, or you may identify six or seven air samples and then decide, well, this one's the one that we're going to put into the records as the one that is credible, perhaps not worry about the other six that we didn't agree on were reasonable samples (inaudible) here.

MR. ROLFES: That's not accurate. If you take a look, they're all air sampling datasheets, industrial hygiene datasheets. It has multiple results typically from the same operation in various times as you had alluded to before. And all those sample results were in fact recorded. So all of them would have become part of the record.

DR. BEHLING: It's hard for me to accept. As I said, he doesn't go into any detail. He does not talk about the destruction of sample data, but the implication is that perhaps they were not documented.

MR. CLAWSON: Well, the one statement right here, "on several occasions during the term of my employment I got air dust sample results that were above the MAC. I was told by my supervisors that the results were in error. I was told to go back and resample."

MR. ROLFES: But it did attract additional attention because they didn't expect the air concentrations to be as high as what was recorded. So time and time again the individual was sent back into the work area to resample because they did not believe the air samples could have been that high. There's no indication that the high air sample results were deleted from the record or ignored. They attracted additional attention so that they could reduce the exposure in the workplace to the people.

DR. BEHLING: When you have multiple high values, you have to start to become a believer and not keep sending the guy back. I have a very different opinion that is not consistent with your opinion. But I would certainly in context with what was said previously about the air emissions into the off-site or the stack monitors, I would certainly want NIOSH to perhaps contact this guy and clarify some of these issues. What was done with these high sample results? Were they discarded?

MR. ROLFES: No.

MR. ELLIOTT: He's no longer with us. He passed away.

DR. BEHLING: Oh, he's passed away. I guess we forfeit that option.

MR. CLAWSON: And for us to be able to say what was done with these samples I think would be questionable, too. Because there's only one person that can tell us, and unfortunately, he's not with us.

DR. MAKHIJANI: Brad or Mark or working group, I mean, just in terms of what we are doing. Do we await, is NIOSH addressing this in the white papers and then we look at it or we are supposed to -- I'm unclear as to whether, who's doing what at this stage.

MR. GRIFFON (by Telephone): Yeah, I was just going to ask for 43-10, I thought I heard Larry offer a response to Sandy for an action or follow up, and I might have missed something. Do we have an action on this item, 4.3-10 for NIOSH to follow up on something?

MR. ELLIOTT: Well, I committed to Sandy that we would consider her comments or suggestion that this one individual's offering and reference material may be compromised by what he offered in testimony in a trial situation. That's what I committed to.

On this particular point that we're just discussing, however, I think what I'm hearing Mark say is that we feel we have this information for consideration in what we do. We didn't lose any data here. We have it. We had the testimony prior to it being included as part of the documentation in a petition. So we have, you're not the first ones to read this information from this one individual. We had it before the petition was presented to us.

So, is that correct, Mark?

MR. ROLFES: Yes, correct.

MR. ELLIOTT: We feel we do have the data that is mentioned in this affidavit that was submitted by this one individual.

MR. ROLFES: He does not document exactly what time period and what air sample numbers these were that he was referring to. However, we do have air sampling data associated with this individual, this industrial hygienist.

But furthermore, I'd like to reiterate that for the process where he was collecting the uranium air samples, we would not be relying on uranium air sampling data to reconstruct an individual's dose. We would rely on their bioassay data.

MR. ELLIOTT: And I would further that to say that we have, I believe we've not run across information that tells us that there was a pervasive set of actions here that need to be accounted for in how we go about doing dose reconstruction. Yes, we have this one source of concern that's provided, and we've looked at that. But we don't see in a broad sense of all the information that there's a pervasive problem.

MR. ROLFES: Correct.

DR. BEHLING: I do have to, not to beat a dead horse, but when I read the statement, "when I got the air dust survey results that were above the MAC, I was told by my supervisor the results were in error." The question is would you record a result that your supervisor said was in error?

I mean, wouldn’t you accept the fact that your supervisor knows more than you do or he has authority over you. And when he says you did something wrong, would you necessarily record something that your supervisor tells you is in error? My gut feeling is you would not.

And what he says is that when he reversed himself, and his back was to the flow of the air, he would get the much, much lower air concentration readings on his air sample. And that became the correct value.

Now I have a difficult time in accepting the notion that in this case when he said five or six times he had high dose and then he gets the low dose that all the samples for that particular assessment were, in fact, documented and are part of the record. If someone says to me you made an error here, I would say, well, thank you very much. If I believe him, I would probably discard those data and probably rely on the one data that’s obviously acceptable to a supervisor. And this is my opinion.

MR. GRIFFON (by Telephone): Yeah, my opinion, Mark, is I agree with what you said about the fact that you're not relying on the air sampling for the uranium. I guess this finding speaks more to the overall data integrity question.

And I wondered if as a follow up, you know, sometimes these Health and Safety reports or Health Physics reports that we talked about have summary stats including number of samples greater than a MAC value or number of, you know. I wonder if some of these summary reports would, during that time period or if they're available, would possibly shed some light on this.

In other words if it shows a number of values greater than the MAC consistent with this guy's sampling, then actually that supports your argument that not only did he go in and measure all these high values, they were translated into the quarterly report. I guess that would corroborate the fact that, yeah, it would support that those measurements were not just discarded and the low one was recorded.

I guess that's the question. It speaks to the data integrity. We know that you're going to rely on urinalysis, but this speaks to data integrity and just trying to think of a way to close it out. And one way might be to examine the quarterly report during that time period.

DR. BEHLING: And, Mark, I don't, I mean, I've looked at enough data to realize that there were plenty of air sampling for uranium and for thorium where the air concentrations were very high. And so you can look at those and say, see, they recorded high air samples.

But there may be selective incidents, and I suspect that in some instances these measurements that we're talking about or that this hygienist is referring to may have come as a result of modification to the plant. There were so many memoranda that I looked at where there was a consistent effort to improve the engineering controls regarding ventilation systems.

And I'm sure that there have been attempts where perhaps some effort was made at some significant cost and perhaps slowing production where there were changes made to ventilation systems and other things and the expectation was, oh my god, yeah, this is really going to improve or reduce the air concentrations, improve the working environment, and it turns out it didn't quite meet their expectation.

And perhaps this particular effort here that we're talking about here was not so much directed against cheating someone out of an air dose or intake, but to somehow or other to cover your butt with regard to having invested a tremendous amount of money, stopped work, introduced a lot of things involving ventilation systems and changes in work practices without no real significant benefit after the fact. And I suspect that this might be just one of those cases.

MR. ELLIOTT: Another possible scenario. Another possible, I mean, I'm an industrial hygienist, and when I'm told to go out and take air sampling, I ask, well, what's the purpose. Is this process point sampling or is this sampling to determine what potential exposure might be to an individual standing at a work station?

So this particular individual may have been sent out on a process point sampling effort to try to see if he could sniff out something that no one expected to be there. And when he found it, he was met with resistance.

DR. BEHLING: Yeah, it may have been a very episodic issue, not a pervasive, systemic problem, but one that involved the situation where significant monies had been spent on modifying the plant system engineering controls that turned out to be of little use. And the people didn’t like it. Who’s to say? We don’t know.

MR. ELLIOTT: Who knows? We could have an employee-supervisor issue going.

MS. BALDRIDGE: I’d like to add something here. I have a letter from 1985, Battelle’s letterhead, and it’s addressed to the Health and Safety Environmental Division at FMTC. And it goes on they’re requesting an open house for employees and their families. It says that the gentleman requesting the open house is unaware of the extent of the contamination problems at the site.

It goes on the last paragraph of the letter it says, “There is, however, yet a more compelling reason not to invite large numbers of people, particularly children, into the facility. As we have indicated, once friskers are placed in the change room and become readily available to workers, they will be very likely to learn that they have frequently been leaving the plant contaminated.”

Now to me this shows a mindset that they weren’t willing to have an open house for the purpose of the employees not finding out how contaminated they were. It just looks like as Hans said somebody’s trying to cover this up.

MR. CLAWSON: Where do we want to go with 4.3-10?

DR. BEHLING: 4.3-10. It’s just an issue that we have occasional an insight as to perhaps the mentality of people who were, whose charge was to protect the workers who at least in this case that the person with 17 years experience talks about things that are somewhat disturbing. And I just have to raise the whole thing.

MR. GRIFFON (by Telephone): Well, can I ask, you know, NIOSH indicated that they interviewed many other industrial hygienists and didn't, I mean, I think Larry is correct in saying that really our focus should be was this a systemic problem. Is this an isolated incident? We really have to try to determine whether there was some sort of systemic problem here. And I don't know if SC&A has looked at those interviews? I'm assuming they're on -- I haven't looked at all of them.

DR. MAKHIJANI: I don't believe they're posted. Are these interviews posted?

MR. ROLFES: Only, let's see, I've documented, let me point to the end of the matrix. I did identify a couple of interview transcripts, site research database document reference ID 26115 and 31023. There will be at least two additional ones added.

DR. MAKHIJANI: They're on the site research database you believe?

MR. ROLFES: Yeah, I have put a couple on, maybe one or two onto the O drive for the Advisory Board.

MR. GRIFFON (by Telephone): I'm just going to capture that as an action, Mark, just as reminder for all of us that NIOSH will post industrial hygienist interviews on the O drives for SC&A to review.

MR. ROLFES: Mark, we do have that on the last page of the matrix. I'm sorry, I didn't --

MR. GRIFFON (by Telephone): Oh, it is on the last page, okay.

MR. ELLIOTT: Just the two that have been posted out of perhaps three more coming or two more coming?

MR. ROLFES: I don't recall. I know I have two with me right now.

MR. MORRIS: Those two that I sent you in the e-mail with reference numbers, then there's two that we provided last week. And then one --

MR. ROLFES: One that had been done awhile back?

MR. MORRIS: Right.

MR. ROLFES: Okay, so there should be a total of about five, and there may have been a previous one. There's --

MR. MORRIS: But only one of those is an industrial hygienist interview.

MR. ROLFES: Yes, correct.

DR. BEHLING: The reason I do have to question whether or not the potential exists for a systemic problem is the fact that if this had been an individual who was fingered by let's say somebody else who said we know this guy doesn't like to go out there and sample so he sticks a sample in there, ends it. Then this would obviously be confined to a single individual.

But in this case he's the individual who's fingering his supervisors as he does on bullet seven. And if this supervisor was in charge of all of the industrial hygienists, then perhaps his corruptive mentality would impose certain feelings and directives to not just this particular hygienist, but to all. And so therefore, I have to question whether or not this is an isolated event or if it involves the entire group of hygienists whose collective job was to protect the worker.

MR. ROLFES: That's a good point, and --

MR. HINNEFELD: It's Stu Hinnefeld, if I could just make one comment because I knew some of the people. I didn't know the person who wrote the affidavit in the petition, but I knew some of the people who worked out there.

Arjun a while ago when we were talking about thorium and the fugitive emissions from the pilot plant, there was a rather condemnatory letter written about how terrible the conditions were in the pilot plant that he read from. That was written by an industrial hygienist, wrote that.

DR. MAKHIJANI: Right.

MR. HINNEFELD: But before we paint the entire department with a (unintelligible) brush, I think you should look at the volume of the things that were written. They're read a lot more like Keith* read. I mean there are plenty of stuff out there, I mean, like Arjun read. There are a lot of those correspondence like that out there before you want to make some sort of judgment about some sort of inherent supervisory-imposed brush on that.

DR. MAKHIJANI: I would support what Stu said in terms of the volume of documentation that shows high air sampling. Not that, it doesn’t negate what this person said, but I think that there is a lot of documentation with very high air samples, and some of it’s actually higher than anything recorded any place that I know of.

But there’s also this affidavit. I mean, it would be useful to see these interviews, but there’s only one with an industrial hygienist, and I guess is that the person you were talking about it?

MR. ROLFES: Yes. We also have attempted to contact the industrial hygienist of whom we are speaking. We’ve attempted to contact his supervisor, and we have not been able to contact him to date.

DR. ZIEMER: Mark, how about other logbooks? Have you done any spot checking? The reason I raise the logbooks is I don’t know what their practice was at this facility, but my experience at Oak Ridge was that the first thing you did after you did a survey, it went into a logbook long before a supervisor ever saw the results. And those logbooks typically had the results. You counted an air sample. It was in the logbook right then.

And so someone telling you that you got the wrong results isn't going to change what's in the logbook unless somebody rips a page out. They're there, and they're dated and signed and numbered. Are there some log, you talked about trying to recover some logbooks. Do we have some that you could spot check and say, yeah, here's the logbooks, and here's where it's showing up on the survey data?

MR. ROLFES: We had attempted, we contacted DOE to see if we could recover logbooks from industrial hygienists. They responded that they did not have any logbooks. Based on the query that we did it did not appear that there were any logbooks at Fernald. However, we do --

DR. ZIEMER: They didn't keep logbooks or they just can't find them?

MR. ROLFES: I'm not sure.

Stu, do you know if the industrial hygienists did in fact keep logbooks separate from the industrial hygiene datasheets?

MR. HINNEFELD: From memory I can't say definitively. My recollection is they carried record books around like I do, but I don't recall if they did.

MR. SCHOFIELD: Could they possibly have used survey sheets? I know this was a common practice at Los Alamos. Instead of being put in logbooks they had survey sheets they did.

MR. RICH: Did we ever have access to survey sheets?

MR. ROLFES: We do have the survey sheets.

MR. SCHOFIELD: Maybe that's what you're looking for to find was the survey sheet where they were logged the first thing.

MR. ROLFES: That's it. I'm still confused about that myself. I do have, I wanted to point out some, well, here's an example of an air dust analytical sheet, and this says National Lead Company of Ohio Analytical Data Sheet. And this has the sample number, the sample time, a description of the sample being taken, air volume sample time, sampling and the results as well.

And it's what I initially believed was that this air sample result, this would have been information that was recorded in a logbook. This would have been the raw data here. I'm not sure if that's true or not, but we have requested logbooks to see if there are logbooks that exist separate from the analytical data sheets. Without going through the records again I can't answer whether there were separate logbooks from the analytical data sheets.

MR. CLAWSON: You know, it basically comes back to data integrity, and I think that's the root of the whole issue here. And we've got conflicting affidavit to what other stuff is. I guess I'm at a loss of what we need to look into on this, and what we're going to do with this affidavit because --

DR. BEHLING: Well, it adds to the collective uncertainty that started out with the issue of how good is the air sampling. How good is general air sampling relative to BZ, and even we heard earlier from Mel that you mount the BZ air samplers on the same person and may have five different values.

So you add to the issue to the uncertainty as to where a person worked, what his job description was and then add to that the issue of potential air sampling that raises questions about these select samples, and you have to come to the conclusion that air sampling's not a very accurate measure of what a person was exposed to and take that into consideration when we use it for dose reconstruction.

You cannot look at these data in a definitive way and say this is precisely. You have to recognize that the uncertainty associated with air sampling data is very, very large.

MR. ROLFES: Correct, that's a good point. And all uncertainties go to the benefit of the claimant.

MR. CLAWSON: Right, and so this basically comes back to the person's personal file on their bioassay or whatever and so forth.

DR. MAKHIJANI: Oh, no, Brad, actually the thing that this throws into question is can we trust the thorium air sampling data. If you have high values recorded and somewhere, I think we're not talking about the uranium data. Mark Rolfes is right about that, but if you're using coworker models and bioassay that then the specific uranium air data are not in question. I would suggest that maybe --

MR. ELLIOTT: But this affidavit speaks to uranium air sampling, correct?

DR. MAKHIJANI: Yes, and it raises a question whether it was a pervasive practice. We do have what Stu said is high values were clearly recorded so it was not happening all the time. We know that. But accepting this affidavit means it happened at least once. And so where the actual matter is in regard to a particular supervisor or particular employee or particular period we don't know. And it may be useful to do a couple of independent interviews. Maybe the working group wants to do it or the industrial hygienists from -- we don't know the period from which this happened.

DR. BEHLING: Yeah, it was a 17-year period. Also, and he alleged --

DR. MAKHIJANI: And he alleged that it was a routine thing in that period.

MR. CLAWSON: And whatever timeframe that we had --

DR. BEHLING: And his second bullet here identifies the employment period.

DR. MAKHIJANI: What was the period?

DR. BEHLING: It's blanked out actually. It's just in the '50s --

DR. MAKHIJANI: But we have access to that.

DR. BEHLING: Yes. I have the original so I know.

MR. CLAWSON: 'Fifty-three to '71. Well, I guess for an action item I really don't know which way we're going to go, but we will have to look into this. I think at this time we're going to have to break for lunch, and almost quarter to one right now, ten to one. We'll probably return at about two o'clock if that's all right. And we'll go ahead and break the phone line.

MR. PRESLEY (by Telephone): Hey, Brad, this is Bob Presley. I'll see you at two.

MR. CLAWSON: Okay, thank you.

(Whereupon, the work group recessed for lunch from 12:50 p.m. until 2:00 p.m.)

DR. WADE: We're back. We're prepared to be back in session once the chairman lowers his hand.

MR. CLAWSON: First of all Larry Elliott would like to make a comment on the record, asked for a moment here, and then we'll proceed back in.

MR. ELLIOTT: Thank you, Brad. I just wanted to react to at the last working group meeting for Fernald, Dr. Makhijani asserted that NIOSH selectively relies on worker input. The implication was that NIOSH listens to workers when their input supports positions that is taken by NIOSH but ignores workers when their input contradicts with NIOSH positions.

And in his assertion Dr. Makhijani offered an example that related to the thorium strikes that occurred in Rocky Flats and the potential for an intake during those thorium strikes. Specifically, he said that we relied wholesalely (sic) on one individual, a health physicist who was involved in that particular work, without listening to others.

It is not clear why Dr. Makhijani again raised the issue of these thorium strikes and this issue since the Advisory Board has already opined upon the subject with regard to Rocky Flats. However, in a more general context I want to speak specifically to what NIOSH does and how they value, how we value worker input.

A review of the transcript on the June 12, 2007 Advisory Board meeting reveals this question was explicitly raised by David Hiller of Senator Salazar’s office. You can find that on page 143 of the transcript. And it was also answered by Brant Ulsh of NIOSH on page 143 to 147 of that same transcript.

As explained at that time NIOSH examined the redacted pages from a classified document that mentioned in passing that the thorium strikes occurred in Building 771 rather than Building 881. The subject of this investigative report was a later contamination incident involving Uranium-233 rather than the thorium strikes themselves.

Furthermore, the report was written by an independent committee convened to investigate the Uranium-233 contamination incident. One of the criteria for selecting committee members was that they were not directly involved in the project which helped assure their independence. Balanced against this report we had the explicit and detailed account of the thorium strike operation as provided by the health physicist in charge of the project.

He clarified that the Uranium-233 solution had been received in Building 771 from offsite and transferred out of the shipment container in that building. The Uranium-233 was then transferred to Building 881 for the thorium strike and subsequent Uranium-233 metallurgical and machining operations.

The health physicist had direct hands-on involvement with the thorium strikes, provided a list of employees involved in the project from memory which was confirmed later by the health physics logbook covering the project, and provided a detailed sketch of the room where the thorium strike occurred. That would have been Room 266 in Building 881, from memory.

And we further confirmed that by examining that drawing, the sketch, against building blueprints. He also provided the approximate dates of the operation from memory which was later confirmed by his own logbook and the logbook of his supervisor.

On balance we considered the detailed recollection most of which was independently confirmed of the health physicist with direct involvement in the project to be more reliable than an incidental mention in an investigative report dealing with a different subject and authored by individuals with no involvement in the thorium strikes.

It is noteworthy that this question was raised in the larger context of evaluating the potential for thorium strikes at Rocky Flats. Over the course of this investigation NIOSH interviewed 12 workers including one site expert currently retained by SC&A and to an individual they stated their belief that there was no potential for thorium intakes at Rocky Flats.

SC&A was apparently unconvinced by this unanimous opinion from the workers and repeatedly has asserted that there were potential intakes even though they could provide no monitoring data, worker statements, incident reports or any other documentation supporting their contention that such intakes could have occurred during thorium strikes or any other thorium operation at Rocky Flats.

In conclusion, NIOSH stands by its position that there were no thorium intakes during the thorium strikes or any other thorium operation at Rocky Flats for that matter. This position is supported not only by the information provided by the health physicist directly involved in the project but also by the testimony from numerous other workers, logbooks covering the operation and other written documentation.

So we in essence take full account of all of the information that is presented to us on a topic, and we take from that a balanced opinion as to where we need to go forward and what approach we would use specifically in that set of circumstances. So I just wanted to get that on the record since it came up in this working group.

I know it’s Fernald, and we talk about Rocky Flats here in this example, but I do see a lot of consternation in the claimant community right now about how much value NIOSH puts into worker input, worker interviews, worker comments. And I just want it to be known that we value a worker's input as much as we do, we consider them to be site experts.

So we're reaching out, and we're trying to improve our interactions and the ways that we interact with workers. So I just wanted to make this comment for your record.

MR. CLAWSON: Appreciate that.

DR. MAKHIJANI: Might I have a chance to clarify. I don't want to get into Rocky Flats stuff, but I brought this up not in a Fernald meeting but in a Nevada Test Site.

MR. ELLIOTT: I missed the meeting.

DR. MAKHIJANI: Yes.

MR. ELLIOTT: Thanks for correcting me. I'm sorry.

DR. MAKHIJANI: I believe that you were there.

MR. ELLIOTT: I was there.

DR. MAKHIJANI: It was at the last Nevada Test Site working group meeting. I brought it up because I felt that there was not a clear and full acceptance of the statement that the fellow, retired health physicist, made, kind of regarding badge practices. I did not bring it up to revise any Rocky Flats issues. I realize that the Board has voted on that and made a decision on that.

I did not bring this up in any context that involved thorium exposures at Rocky Flats. I believe Dr. Ziemer was there. I believe the Board actually took some action on that, and I have to revisit my notes from the Nevada Test Site. Jim Neton was there, and it was agreed that some investigation in regard to the badges not being worn in the forward areas needed to be done.

And that was as a result of the intervention that I made because I thought that not only a particular health physicist's statement was at least as much of a site expert as any other site expert at least that I'd interviewed or any evidence that I've seen brought forth from any site expert because he witnessed more than 900 nuclear weapon tests. And that was the context in which I brought it up.

I believe the record of the reports that we showed in which I played a significant part in preparing SC&A's, some of SC&A's Rocky

Flats report will not show that we had stated that there were significant thorium exposures during thorium strikes. But we had repeatedly questioned whether NIOSH has properly created a bounding dose or not. And we repeatedly rejected the use of NUREG-1400 as the approach for bounding dose and asked for evidence from the site regarding bounding doses.

It is simply not correct and a misrepresentation of things that I said last time and a misrepresentation of things that are there in the report to have read into the record the kinds of things that you have just done, and I do object to it.

MR. ELLIOTT: Well, I apologize for getting the wrong working group, but the issue was --

DR. MAKHIJANI: But a considerable portion of the other record is also not properly stated in that e-mail, and I do object to it. I have not fought an exercise about anything in three years, but this misrepresentation of what's in our written reports, it's a matter of record what's in our written reports.

We did reject NIOSH's repeated statements that NUREG-1400 was bounding dose as improper. And that was done by the whole team. It wasn't done by me. For your information every report that I have submitted has been reviewed so far as I can recall, has been reviewed by John Mauro and signed off by John Mauro including, and specially I asked him to go over every word in sensitive reports to make sure that he knows every detail of what's in them. And if he's on the line --

John, are you on the line?

DR. MAURO (by Telephone): Yes, I am, and I agree with what --

DR. MAKHIJANI: -- or deny or --

DR. MAURO (by Telephone): -- especially NUREG-1400.

DR. MAKHIJANI: Sorry?

DR. MAURO (by Telephone): Especially the NUREG-1400 work. In fact, that was my concern. I mean, just to hearken back I am familiar with that particular subject and being very much involved in that part of the review.

DR. MAKHIJANI: Thank you.

MR. ELLIOTT: I was not --

DR. MAKHIJANI: When you make ad hominem characterizations, I would request you to make correct ad hominem characterizations. I do try to avoid them here, and I am exercised that your team has chosen to make incorrect characterizations on the record in a way that I find objectionable.

MR. ELLIOTT: Well, I think that's the pot calling the kettle black because I'm sorry, it was the NTS --

DR. MAKHIJANI: And there you go again.

MR. ELLIOTT: -- working group meeting that you brought this up. And you brought it up in the context that NIOSH does not listen to worker input. And I have so many --

DR. MAKHIJANI: You might ask a whole bunch of workers about that. We've had this complaint from workers repeatedly.

DR. WADE: We need to go back now to the business of this work group.

MR. CLAWSON: We appreciate the input, and we'll proceed on with this, Hans.

DR. BEHLING: The next section deals with the in vivo monitoring of workers for uranium and thorium that started with the introduction in 1965 in mobile in vivo radiation laboratory monitoring unit that was brought onsite, a discrete timing of people were assessed for lung burdens involving uranium and thorium exposures.

And starting on page 103 of the report I talk about some generic aspects, and I preface my statement on that page by saying not considered a finding by SC&A. A critical component of the MIVR lung counting system, however, was the radiation detection system was perhaps not the one that should have been used.

And I stand not alone in that particular criticism because recently I reviewed the Portsmouth TBD and some of the supporting documents that surround the Portsmouth TBD, one of which was a DOE document that had some very, very critical comments to say about the Y-12 mobile in vivo laboratory system. And principally the consideration here is the level of sensitivity that comes with the very, very large crystals that were used, the four-inch thick crystals.

As we know if you're looking for lower energy photons, you tend to go with the thin crystals in order to avoid the (inaudible) background against which you have to discern your signal. And if you read that particular report from the DOE that relates to the Portsmouth, you will see similar statements that I make here. Again, they’re not findings. It’s just the level of sensitivity that says the system was not intended to be used for lower energy photons.

And in the case of uranium you’re really looking at U-235, the 186 keV photon, and if you look at the conventional gamma spectroscopy issue involving the backscatter photon, then that particular 186 keV for uranium coincides with, if you look at, for instance, the 183 backscatter photons for cesium or for cobalt, they all lie somewhere between 180 to 210 to 120 keV.

So you have a real problem here when you’re potentially dealing with other radionuclides. And I realize cesium was not one of the major radionuclides, but we all have, especially in the early days, cesium from atmospheric fallout. And so I just brought that up as an issue that the four-inch thick crystal compares very poorly in terms of discerning low energy photons for chest counting when you compare it to the crystals that are conventionally used at other DOE facilities that involve four millimeters instead of four inches. And so I just brought that up.

DR. ZIEMER: Well, it still is possible to use them with proper calibration.

DR. BEHLING: Of course.

DR. ZIEMER: And, in fact, and I wouldn't know in this case, but it's interesting to note that sometimes the higher background is not an issue. Your sort of coefficient of performance goes to sample squared to background. So if your sample count gets high enough, you can put up a terrific background.

That's why Los Alamos for many years was able to use those large whole-body liquid simulation counters with terrific backgrounds because the sample count was so high, the sample squared background ratios were good. And I don't know what they would be in this case. My guess it's very inefficient to use a thick crystal because the particles only penetrate in the outer surface. So you probably don't have good samples squared to background here. And so it's not optimum.

And I agree with your point. I think it's not optimum, but it's usable.

COURT REPORTER: 4.4-1?

DR. BEHLING: Yes. We haven't really gotten to 4.4-1 because I'm only talking about a generic statement.

DR. ZIEMER: Right, right.

DR. BEHLING: And I meant to bring with me --

DR. ZIEMER: That wasn't a finding but an observation.

DR. BEHLING: No, no, that wasn't a finding, just an observation.

DR. ZIEMER: Yeah, that's the best way to do it.

DR. BEHLING: Be careful about how much accuracy you assign to these measurements and realize that perhaps by design this particular counting system was not optimal.

DR. ZIEMER: It's not optimum, yes.

DR. BEHLING: And was clearly not the one of choice that, because other DOE facilities used it for millimeter inch thick crystals for counting these lower energy photons. But having said that --

MR. MORRIS: I don't think you should say be careful of the accuracy. You should say be careful of the detection limits.

DR. BEHLING: Detection limits.

DR. ZIEMER: Yeah. We can be very accurate, but the detection limit is much higher.

MR. ROLFES: Right, and the net result from that I think is important to clarify that a higher detection limit will result in a higher dose estimate for a claimant. So once again this is another example of the uncertainties associated with a measurement being credited to the claimants in a dose reconstruction.

FINDINGS 4.4-1, 4.4-2

DR. BEHLING: There are a number of other findings. In part they were due, and I will ask you to look at page 110 of the report which is a reproduction of one of the tables that were identified in the original TBD. And I will say that because initially that is what I was working on in making some of my comments that relate to finding number 4.4-1. Actually not so much that but the other ones, finding 4.4-2.

And if you look at page 110, you will see a table that talks about the different measurements that were taken on a yearly basis as you see on that page for the year 1965. They were two Uranium-235 counts, two uranium total, and zero thorium although it says there were two Lead-212 and two Actinium-228 which by default I assume are there for thorium. And as you go down the list over the years, it isn’t until you get to the time period of 1978 or really 1979 that you see a large number of citations that reflect Lead-212 and Actinium-228.

And prior to 1978 you see an awful lot of data that involves thorium. And having looked at that I was very much confused. And I make all these statements in preface to what you’re about to tell me is that this is how the data was reported. And I think we can go through some discussion and come to terms with what the data really represents. But at the time when I looked at this, this was the table I was making reference to.

And quite frankly I could not make heads or tails with the notion that up to 1978 all of the thorium was reported in terms milligrams and for Lead-212 and Actinium-229, which are your two indicator radionuclides. There was no data there. And I think since that first discussion you informed me that this was an issue of reporting the method by which the mobile in vivo data was reported.

And that brings us to a couple of other issues, but having said that, we can discard perhaps a couple of the first findings here, 4.4-2. At this point I have looked at some of the data, and it is clear that the thorium data reported in milligrams between '68 where you have 310 all the way to '78 where you have 161 are reflections of data that involve Thorium-232.

One of my concerns was what thorium are you really referring to, but it's clear these data do reflect Thorium-232 and Thorium-228. So I have at this point accepted the notion that these data do, in fact, involve Thorium-232 as opposed to Thorium-230 or something else.

But having said that we can now go and look at a couple of the other issues that I guess I would like some clarification on because when you don’t have any data that involves a definitive assessment for Lead-210 -- and I guess for those who may still be somewhat unsure as to what these mean, I enclosed for the benefit of the reader, Exhibit 4.4-2 on page 106.

And there you have a citation of the radionuclides that start with Thorium-232 and the intermediate products between 232 and Thorium-228 and the two indicator radionuclides defined by Actinium-228 and Lead-212. And you realize, and I explain here briefly what assumptions are reasonable here.

Obviously, if you were to deal with pure thorium material that is just harvested for the first time, ore, you can reasonably expect every one of these radionuclides -- let’s put it this way. It’s not unreasonable not to expect them to be in exact equilibrium. However, once you segregate the thorium chemically again you may start out at time zero with total equilibrium between 232 and

228, not equilibrium but equal quantities.

They won't be in equilibrium long because what you've done is essentially removed Radium-228 which has a half life of 6.7 years and in the process if you remove Radium-228 you also remove actinium. And of course, that's your indicator for Thorium-232. At that point and moment in time when you chemically segregate thorium and you isolate Thorium-232 and 228, you start to, as a function of time, lose Thorium-228 because you have a half life of 1.9 years. So if you segregate the thorium chemically at 1.9 years, your Thorium-228 will be exactly half of the Thorium-232.

And in fact, I think somewhere along the line we do have a map that identifies how these values coincide over time, and I can pass it around here. And you see obviously that this equilibrium that occurs with these two radionuclides, and I guess the question now I have is what happens when you deal with data that you no longer have the individual measurement.

Obviously, Actinium-228 is your

indicator for Thorium-232, and Lead-212 is your indicator for Thorium-228. And now you have to make a decision. What is it that I'm looking at when it's reported in thorium milligrams? What were the assumptions on the basis of which the original data as you see in that table on page 110 where you have thorium reported in milligram quantities or microgram quantities, how do you segregate the two thoriums?

MR. MORRIS: It's a fair question, but it's obviously a dose reconstruction issue, not an SEC issue.

DR. BEHLING: Well, it's a dose reconstruction because you can certainly -- let's assume you rely on Lead-212 and say, well, they're in equilibrium. But you can certainly have Lead-212 that after a period of years has decayed off and it's going to be, you will have an accurate assessment if you rely on Lead-212. You have an accurate assessment for Thorium-228, but you can also realize that Thorium-232 has been grossly underestimated if you assume that Lead-212 provides you with an indicator. Unless you use both, and you have reasonable assumption --

MR. RICH: Again, you're using daughters of both isotopes.

DR. BEHLING: Yes.

MR. RICH: And they also were very careful to determine the age since separation of the material that they were operating from.

DR. BEHLING: I would care less about the age. If you have accurate measurements for both Lead-212 --

MR. RICH: Well, it makes a difference, Hans, in terms of the ratio that the activity of the 232 to 228, depending on the age since they've been separated.

DR. BEHLING: Of course, I know that. But you could, if you had accurate measurement for Lead-212 and Actinium-228, the 212, Lead-212, tells you how much Thorium-228 you have. And the 228 tells you how much Thorium-232 you have.

MR. RICH: They made assumptions for both in order to do the, to come to the conclusion of how much Thorium-232.

DR. BEHLING: Because I have looked at data in subsequent years following 1978. And we do have data that tells you how much Lead-212 was measured in the chest count and how much Actinium-228. And they're clearly not, frequently they're just a factor of two or three higher different meaning that there is disequilibrium between the two thoriums.

Now to what extent was that considered or was a larger number or value used to account for any uncertainty? I guess I would like, I'll turn to you and tell me how it is that you interpret data prior to 1978 when all the data was only issued to you in units of thorium mass as opposed to subsequent data when you may have two values, one for Lead-212, one for Actinium-228, and how do you assess your lung burden based on these two different sets of data, one lead involves just thorium measurement, the other one for two daughter products but they're not necessarily in equilibrium?

MR. RICH: The data that's listed in the claimant file prior to when they started to simply record the two daughter products. Those same two daughter products were used in the analysis to come to the milligram quantities prior, and it was just a matter of change recording.

DR. BEHLING: I mean, do we have data that says that for let's say in the years 1972 or so prior to the change --

MR. RICH: Yeah, there are some transition years where we have both the daughter activity and the milligram.

DR. BEHLING: I've seen it. But are we comfortable in understanding what it is that they did to be sure that they didn't do things that potentially are not claimant favorable?

MR. RICH: I'm comfortable, yes.

DR. BEHLING: Are you?

MR. ROLFES: We've also pointed the Advisory Board members to a couple of documents that discuss the assumptions that went into measuring Thorium-232 in the body. And there's a record of, let's see, one report is the evaluation of health physics problems from thorium and its daughters in a thorium purification and fabrication process. This is in the Health Physics Journal, Volume 8, pages 279 through 297 for 1962. And also we have radioactivity of thorium and the feasibility of in vivo thorium measurements from the Oak Ridge Y-12 plant, Report Y-1280, 1959.

DR. BEHLING: Again, I just want to be very careful about interpreting some of the data. As we know and we're fully aware because I'm preaching to the choir here, that disequilibrium is an issue here that you can't avoid. And on page 107 I give an extreme case. If you had something that is at this point thorium ore that has been just separated, what you don't have at that point is Radium-228 or Actinium-228. Meaning that you have no indication as to the fact that Thorium-232 is there potentially in large amounts, but you have no way of verifying that because your indicator, Actinium-228, simply isn't there.

MR. RICH: That was well understood by those people who were doing the work.

DR. BEHLING: I mean I just want to be sure that we're not caught off guard here by people who don't understand the mechanics and the biokinetics of all these radionuclides and how indicator radionuclides have some limitations regarding the interpretation.

DR. MAKHIJANI: Bryce, is there a procedure for the in vivo counter that shows that this was well understood?

MR. RICH: Those documents that Mark just referred to. The documents themselves were explanatory.

DR. MAKHIJANI: I've looked at the in vivo procedure, but I didn't see, I'll have to go back and revisit it because I don't remember.

MR. RICH: There are a number of documents, more than what we have here.

DR. BEHLING: If we go to, and if everyone's comfortable. As I said there's no real resolution other than to go back and assess what were the methods used to interpret thorium data prior to '78 when there (unintelligible) not reported for two indicators. And if we're comfortable with that then I think we can --

MR. RICH: It was standard industry practice and with the best minds that in vivo counting for thorium (unintelligible).

DR. BEHLING: I was just somewhat taken back because I did look at a couple memoranda that talked about the early years when the mobile unit was brought onsite and was operated by Y-12 personnel. And I have to say on a relative scale I would trust their ability to, since they designed the system, understood its limitations. And when they operated it, they clearly understood what they needed to do to compensate certain deficiency of the system and how to interpret data.

What did cause me some concern, and I quote one memoranda, is that the people after the first two years at, Fernald took over and it’s a question of did they understand the nuances? Did they understand what needed to be done here? And there were a couple of memoranda that I looked at that raised a question about the qualifications of people who ran the mobile in vivo lab.

And so again it’s an issue that from this point has a limited chance to be resolved, but the qualifications of people who were not necessarily trained on them to the extent that the Y-12 people were, raises some questions.

FINDINGS 4.4-3, 4.4-4

These next two issues relate to the worker selection and the frequencies by which the in vivo counts were conducted. And I know I’ve read enough documents to suggest that on average people at the high end of their exposure potential were at least counted once a year, but there were clearly indications that some people were skipped and there may have been two years. And, of course, the question was there for let’s say two years, just after the unit left and the next time it showed up obviously he would not have been part of the monitoring program. And I assume at this point if there’s indication that he was subjected to thorium, you’re going to tell me that there’s going to be a coworker model, is that correct?

MR. MORRIS: That is correct.

MR. GRIFFON (by Telephone): Are you on 44-2, Hans?

DR. BEHLING: Yeah, 44-3 and 4.

MR. GRIFFON (by Telephone): And for 4.4-2 it says NIOSH will provide a coworker model, and you indicate you’ve done that. Is that on the O drive, that coworker model yet? Or is that --

MR. MORRIS: No, it's not on the O drive right now. It's in final review right now.

MR. GRIFFON (by Telephone): Okay, so it's done, but it's not to us yet.

MR. MORRIS: Right.

MR. GRIFFON (by Telephone): Okay.

MR. CLAWSON: Pending.

DR. BEHLING: Again the issue of 4.4-4 on page 109, again, raises some questions again based on the memorandum that talks about worker selection. You know, you use good judgments and you rely on your process knowledge, your work environment and select workers. Again there are certain suggestions here in one of the memoranda that are (unintelligible). Air monitoring data for certain locations within the plant did not coincide with the highest empirical exposure data as evidenced by the mobile in vivo lab data.

And so the question comes to mind is, are there people there that should have been monitored (unintelligible) as opposed to people who may not have been at the high end but who were monitored. And are we potentially finding ourselves in a situation where people with potential high exposures were simply ignored? And I can't answer that.

MR. ROLFES: That's the opposite of the truth. In 1959, I have a letter here dated October 21st, 1959, in regards to thorium bioassay. It goes on to say "our interest in the subject concerning," excuse me. "Our interest in the subject stems from the thorium operations we had at the plant a few years ago. Although this operation has been shut down, many of the thorium workers are still employed at this plant, and we are still interested in performing tests to determine if retention of thorium from exposure to airborne material was appreciable. Recent efforts along this line have included the analysis of urine samples for radium daughter" -- and it also, this was one of the precursor letters to the individuals being sent outside to the University of Rochester.

So to me this indicates to me that for individuals that were potentially exposed to thorium, they were interested in determining historical thorium exposures. But --

DR. BEHLING: And that may be fine, but I'm also looking at on page 108, and I'll read it for those who may not have access to this report. And it's a direct quote from one of the memorandum that I selected and it's phrased as follows: "Recent in vivo monitoring (unintelligible) employs utilizing the IVRML indicated (unintelligible) of currently sustained 70 percent to 100 percent of a permissible lung burden of uranium." And then it continues. "A serious question has been raised regarding the validity of the job (unintelligible) and air dust sampling approach used by NLO since that data would not suggest lung exposure (unintelligible) at the in vivo indicated level." And again, --

DR. MAKHIJANI: Which page are you one?

DR. BEHLING: This is on page 108 of the report.

And it goes back to the similar issue we observed for uranium. We had people that just simply didn't expect to have had a high urinary excretion rate of uranium. Here you have people who show high test burdens, lung burdens, who were at 70 to 80 percent of the permissible lung burden who were not expected based on weighted air dust sampling data to have been exposed.

And then the question is since you selected these people on that premise, to what extent are there people for whom the in vivo data simply doesn't exist? Now again if there is a coworker model that elects to use a fairly conservative upper-end value, we can accommodate. But the question is what would we do for people for whom perhaps data is not there or is very, very sparse.

MR. MORRIS: There is the coworker model in final review.

MR. ROLFES: That's it. We do have a coworker model that will be made available to the Advisory Board working group members.

DR. BEHLING: And the next finding, 4.4-4, is something that perhaps you can clarify here. I think at one of the meetings that we had -- I'm really referring to you, Mark. You had mentioned that there was going to be some correlation between lung count data with thorium air sampling data. And is that still a process that you're going to look at in trying to establish some correlation between air monitoring data and in vivo chest counting?

MR. ROLFES: It's a potential approach, but I don't believe we would be doing that at this time.

MR. RICH: We wouldn't be applying the ratio back to the early times.

DR. BEHLING: Yeah, my concern was that obviously post-'79 we're not dealing with processing of thorium any more; and therefore, air monitoring data that correlates during that timeframe may be poorly correlated. So I just was looking to make sure that we understand what the limitations of the intakes exist when you compare two time periods that may or may not necessarily apply to earlier years when thorium was processed.

MR. RICH: However, we have taken a look at the relationship between internal uptake in the '68 period of time when they brought in vivo and air samplings. We find the in vivo results always significant.

DR. BEHLING: Now I would hope that if such correlation exists, that we would exclude post-'79 data because at that time air monitoring data may have very limited value to relating to body burdens.

MR. RICH: And because of the differences in operational and circumstances in the very early times where you're not going to tie that ratio back to the early time. That'll be done with pure air sampling results.

DR. ZIEMER: Yeah, you'd have no guarantee that that ratio which is sort of a modern day ratio held for the earlier days. It may, but there's no guarantee.

MR. MORRIS: They were totally different plants and different processes.

DR. ZIEMER: The net result is that earlier days are going to look like higher exposures.

MR. RICH: Well, they do have --

DR. ZIEMER: They may or may not be. You don't know, but you have to assume they were.

MR. RICH: Well, we have some assurance because of the experience in the period of time when both sets of data are there that the air sampling data will give us a very conservative result in the earlier time.

DR. ZIEMER: It tends to overestimate.

MR. RICH: Yeah, it tends to overestimate for a variety of reasons which are (unintelligible).

FINDING 4.4-5

DR. BEHLING: The last finding on that issue is Finding 4.4-5 on page 110, and we've raised it before. Perhaps we need a clarification. Based on one of the (unintelligible) on page 111, at a previous meeting I believe Mark had identified certain statistics regarding the number of cases that had been completed or adjudicated to date at Fernald.

And I know that in many of these cases for efficiency's sake, ORAU/OTIB-0002, had been used to essentially say what kind of exposure did you receive from the 12 or 28 radionuclides defined in TIB-0002 at the first day of employment. And would that result in if you're not going to be compensated on that premise. Chances are that you're not going to be compensated using your data.

I have yet to see, for instance, a comparison between OTIB-0002 and compare that to perhaps someone who had perhaps as many as

30 years experience at working at Fernald and apprise certain data, some of which you may have for uranium exposure from urine bioassay. And now that you have perhaps coworker data for thorium and others, and I'd like to see a comparison to see if that statement is, in fact, a true statement.

I mean, it's clear that if you had a person who worked for one year, but to assume that that is, that particular model transcends all other options for saying we're going to clear the slate by assuming that we can run your dose model using (inaudible) and thereby determine whether or not you're going to reach the 50 percent mark is something that I haven't convinced myself truly holds.

MR. ROLFES: This is not an SEC issue. It is how NIOSH does dose reconstruction. That is the issue. The TIB-0002 methodology we can show you a comparison of TIB-0002 intakes versus actual data.

DR. BEHLING: I would like to see, now that we're in the process of revising the TBD we're developing white papers. We're developing coworker models. I'd like to see someone for whom that TIB-0002 was used to say, okay, you're finished. We're done with you.

I'd like to be sure that under the most extreme case a long-term employee, a worker who is at the forefront of some of the exposures and determine whether or not the current assumptions as you're proposing here would still hold up in the sense where the TIB-0002 data would transcend all other exposures that you just pointed to by two workers.

MR. ROLFES: Has SC&A seen, have you looked at the data and seen any results where an individual's actual dosimetry records would have exceeded TIB-0002 intakes?

DR. BEHLING: Well, we've already concluded that dosimeter data for external is one parameter, that uranium bioassay, there are certain loopholes there. We don't know what thorium data to apply and other things that at this point are still part of your ongoing revision.

And so we're hard pressed to say once all of the dust settles, and you tell us that you were finished with providing this particular approach and this model, that’s the time when we would want to look at and take a limiting case, a worker who worked from the early ‘50s through the end of his employment career, maybe 30, 40 years later and determine whether or not the application of TIB-0002 is, in fact, a limiting exposure scenario.

DR. MAKHIJANI: I haven’t checked the Fernald situation, and I agree with Hans that we have to await your formulation of these models. But I did do some back of the envelope work in relation to Mallinckrodt, and I believe SC&A raised questions about TIB-0002 in the context of Mallinckrodt not with uranium but with the other radionuclides, with Actinium-231, the thorium.

And I wasn’t convinced that the TIB-0002 -- at the time I think you’ve since revised it somewhat, and I’m not aware whether, how many and what numbers have changed or how its application has changed. But in the context of Mallinckrodt certainly there seemed to be situations where TIB-0002 would not be bounding or would not be a --

MR. ROLFES: For monitored workers? Is that what you’re referring to?

DR. MAKHIJANI: Well, we --

MR. SHARFI: OTIB-0002 was not allowed to be used on every case. It does have limitations inside the TIB. It does point out that there are cases where you can run into a situation that you may see larger doses by assessing bioassay than you would have OTIB-0002. So you do have to consider before the monitoring data whether or not OTIB-0002 would (unintelligible).

DR. MAKHIJANI: Right.

MR. SHARFI: I don’t want to believe that we always say OTIB-0002 is an overestimate, but usually we do some analysis and make sure for that particular claim the OTIB-0002 would result in larger doses than would be if we assessed the individual data.

DR. MAKHIJANI: I was speaking in the context where you didn’t have bioassay data for those radionuclides, and that’s why there was sort of an extended discussion about the SEC at Mallinckrodt. And here also we’re talking about thorium where you don’t have bioassay data for the first 16 years.

MR. SHARFI: And that might (unintelligible) for OTIB-0002 when we go back and --

DR. MAKHIJANI: And in that context NIOSH had applied OTIB-0002 in Mallinckrodt and also noted that NIOSH has frequently applied OTIB-0002 in Fernald when, in fact, there are no thorium bioassay data. And so the fact remains to be demonstrated, and in a few cases that I, I did review some cases at Fernald when I drafted the site profile review for our team, and I did not find, you know, I didn't look at every scrap of paper in your dose reconstruction files, but I did not find an attempt to calculate whether the thorium doses are based on air concentration data. Of course, the air concentration models you were using, were they correct?

MR. SHARFI: Yeah, you've compared that they were concerned to the thorium at the 110 MAC. There was 1,050 MAC which is different than the new proposed. So it may be in the revision of the site profile that now it may not be applicable, and we may have to then go back and redo it, which we'd have to redo those cases. (Unintelligible).

DR. MAKHIJANI: I agree.

MR. SHARFI: So there's a lot of potential where a lot of these cases they might be reworked. The thorium they may still be bounding.

MR. ROLFES: That's correct. We'll go back and look at previously done claims in a formal program evaluation report for Fernald based on document changes to the site profile.

DR. MAKHIJANI: Right, I agree. The Fernald question is still on the table. I just wanted to point out that this issue had been raised earlier in the context of radionuclides that were not monitored --

MR. SHARFI: That was never dose per unit.

DR. MAKHIJANI: Yeah, that was never brought to a conclusion because it was rendered moot by the decision of the Board.

FINDING 4.5-1

DR. BEHLING: If there are no other comments, I guess we can go to the next topic starting on page 112, Section 4.5 and we're still with external exposure monitoring at Fernald. Finding 4.5-1 states the absence of performance standards quality assurance for personal dosimeters, and you see in the write up that there were a number of issues here that deal with the training of the people who processed the film, the certain practices involving film dosimeters that were left in cars were experiencing heat damage and other things.

And apparently there was very little in the way of controlling how these badges were used by the individual person to whom the dosimeter was assigned to as well as perhaps some of the qualifications for the people who ran the program in terms of their training qualifications in using calibration standards appropriate for the energies to which workers were exposed and a number of other things.

And I found very little that would give us a warm feeling about the quality controls and the QA program that was in place especially in the very early years. And again, I refer to a couple of attachments that make reference to that. Among the other things that was missing was obviously extremity exposures that were not monitored properly.

MR. ROLFES: NIOSH has located inter-comparison studies from Herb Parker, dated in 1945. It was inter-comparisons of the (unintelligible) lab, the Oak Ridge Clinton lab and Hanford Works. The three badges were inter-compared. The Oak Ridge dosimeter was the one that was used at Fernald during start up, and we've provided that on the site research database, reference ID 439.

DR. MAKHIJANI: And the relevance of a 1945 study at Fernald?

MR. ROLFES: It was the same dosimeter.

DR. MAKHIJANI: The same three badges that were compared?

MR. ROLFES: The inter-comparison study was done by Herb Parker in 1945. It was an inter-comparison of the Oak Ridge dosimeter, the Argonne National Laboratory dosimeter and the Hanford dosimeters.

DR. MAKHIJANI: (Unintelligible) dosimeters?

MR. ROLFES: It was the Oak Ridge dosimeter that was used at Fernald.

DR. BEHLING: I don't question the validity, obviously the key component in the integrity of a dosimeter is the people who manufactured the film and how does it respond and how is it processed. And when you obviously go through the exercise you will find that there's a fair degree of consistency when you have such a test as you mention.

But the question is when these dosimeters were, in fact, processed by in-house people who were perhaps not properly trained, who did not or may not have understood the need for calibrating these dosimeters to energies, photon energies, that were applicable to the facility, then these are issues that you can't really assess by this inter-comparison that you mention.

So I still have questions about the quality of the program because there's regular documentation that would give you this feeling that there was a high degree of emphasis placed on the processing of these dosimeters and an assurance program that said these things are always done by procedure. I didn't see such, especially for the early years.

MR. ROLFES: The calibration curves for the dosimetry that was used at Fernald is in fact on the site research database. There are several calibration curves reported. As far as procedures I would have to take a look back to see what we do have.

MR. GRIFFON (by Telephone): Hey, Mark, these reports, these Health and Safety Radiation Safety reports, whatever these are, these monthly or quarterly reports, do they have any section on quality, assurance quality control? Because this issue came up related to the bioassay I think related to the urinalysis results, too. And you had mentioned that you were going to look for QA reports but were unable at that point to locate any.

MR. ROLFES: I'm looking through a stack of papers on the table in front of me here, and...

DR. BEHLING: The ones I've seen are obviously later years and clearly again when you talk about later years, the question is to what extent were similar procedures applicable to earlier years. And that's always an unanswered question. Obviously, as we all know in the health physics field things were fairly questionable early on in the '50s.

They obviously significantly steadily improved over time. And so when you look at something that was the health physics people put out in the ‘80s, you have to obviously be very mindful of the fact that what existed in the ‘80s may not have existed in the ‘70s, ‘60s and ‘50s.

MR. ROLFES: Let’s see, I believe we had spoken with a couple of employees from Fernald. We did receive indication that there were procedures for some badge calibrations. We haven’t located those procedures to this date I believe. We’re still looking for additional procedures and if Leo Faust is on the line I wondered if he could please elaborate on what I’ve just stated.

Leo, are you on the line with us?

MR. FAUST (by Telephone): Yes, I am. You have to remember, Mark, that the first year and a half of operation at Fernald, the dosimeters were actually read out by HASL. And they didn’t start their own read out until I would say 1953, about 18 months after start up. As far as calibration is concerned they used uranium and uranium slab of which the surface dose rate is well known, and they used radium as their photon source which everybody else did, too.

That badge, the Oak Ridge badge, or as it's referred to in the Parker study as the Clinton Laboratory, that badge was used for a long time and any changes that were made at Oak Ridge were incorporated in the same dosimeter that was employed at Fernald until Fernald decided to go with a TLD system which was in the '80s.

So we also found some procedures that were employed. I did not find any that related to the actual calibration itself other than the calibration curves and the timing that was used to establish a certain number of doses to calibration film.

They also handled their film just like everyone else did. They had controls and they had backgrounds. And they stored their new film in refrigerators which was the standard practice.

DR. BEHLING: Well, I'm going back to page 112 of our report, and I made a number of statements which actually are quotations from the 1981 response to dosimetry assessment fact sheet that follows, that's included in the report. And the statement says test dosimeters, i.e., control badges, are not routinely processed. And these are again things that a good quality assurance program would do obviously on a routine basis.

You would obviously zero in your densitometer and make sure your densitometer is working. You would have various protocols that says on measuring things that I can reliably assign to an individual as an exposure. And there seems to be questionable data that would support that this was in fact done.

MR. FAUST (by Telephone): What time period was that?

DR. BEHLING: Well, I don't know if you have access to --

MR. FAUST (by Telephone): No, I do not.

DR. BEHLING: Okay, then I'm sorry, but in my report as Attachment 4.5-1 on page 113 is a response to dosimetry assessment fact sheet. Apparently the National Lead Company of Ohio was asked to perhaps support a reconstruction program. And they went back to historical records.

And so you get some assessment of what they felt in 1981 were limitations that would allow them to do so. And in that report they make certain statements that lead you to believe that they didn't really have a lot of faith in some of that historical dosimetry data.

MR. FAUST (by Telephone): Yeah, I'm familiar with that report. I don't have it with me. I'm sitting in Branson, Missouri, as a matter of fact. But the person that, well, we're intimately familiar with that particular report, but from our interviews with at least three different individuals, they all claim that there are not exact responses to the questions that were asked in that particular survey. And that's about all I can say to it at this stage.

We also recognize that during the changeover from the film to the TLD that there were some discrepancies in the algorithm that was used. And those discrepancies were in fact taken care of and changes were incorporated into the algorithm. However, we haven't found anything that would discredit the film dosimetry program at the site. Everything that we have found supported a good dosimetry program. And that's been verified by interviews with people that were responsible for the program including an individual that actually operated it.

DR. BEHLING: Well, apparently there was, but the single individual, I will read to you from one of the pages in paragraph G that states in this particular report, "There were no specific training requirements for the film badge technicians when this program began in 1951. The technicians received on-the-job training. The technician now performing all film badge processing began this work" -- and then it's been blanked out for Privacy Act reasons -- and has been the only technician doing this work since whatever.

So we have but one individual who apparently was responsible for the issue of assessing personal dosimeters, the film badge dosimeters. And apparently, he was not necessarily trained, formally trained, in this area.

MR. MORRIS: I have a question for you. Bob Morris. Excuse me, Leo.

On page 112 your finding, quotes, “test dosimeters,” and I assume that’s your parenthetical addition, i.e., control badges?

DR. BEHLING: Yes, yes.

MR. MORRIS: Are not routinely processed. I don’t think that was control badges that they’re talking about there. If you go into the actual text you quote that from, I think you’re out of context.

DR. BEHLING: I’m not sure, where did you --

MR. MORRIS: Just go down one more page, right below there, and you’ll see where you quoted that. And it doesn’t say control badges. That was your interpretation of it. And I think that’s in response to a specific question, do you use test badges. And I don’t think that was in their vocabulary, but he does, whoever replies, goes on to say “test dosimeters are not routinely processed; however, five or ten gamma and six or seven beta and gamma calibration films are processed.” So I think that you’re out of context on that.

DR. ZIEMER: There's two kinds of possibilities here I think. One is where you're giving known doses and you're confirming your calibration. Another would be a blind test. Some groups do that where somebody gives a dose, but the reader doesn't know which it is which is different from a calibration.

MR. MORRIS: A round robin perhaps.

DR. ZIEMER: Yeah, it's something like an inner-calibration study where the person reading it out does not know the dose that's been given to a, quote, test badge. As I say that's different than a control badge where you give it a known dose and confirm that you get some density reading on the film. So those two --

Hans, I'm wondering if those two issues got intertwined here.

DR. BEHLING: You know, in fact I'm kind of trying to search where that statement was extracted from.

MR. MORRIS: Okay, let me get it for you exactly.

DR. BEHLING: Give me the number.

MR. MORRIS: In your attachment there's a little page number two at the bottom of it, and it's item D, page 114 at the bottom.

DR. BEHLING: D, okay, now I see.

MR. CLAWSON: This is Brad. We used a control badge, and I believe they use that for general background.

MR. MORRIS: That's right.

DR. ZIEMER: Yeah, a control badge that gives you a background reading.

MR. FAUST (by Telephone): Correct.

DR. ZIEMER: Badges which are given known doses to establish your calibration curve. And then the third thing is many places used a blind test badge where somebody has given that badge some dose and the reader doesn't know what it is in advance.

MR. RICH: You express those in your sequence so that periodically it just validates --

DR. ZIEMER: It looks like another user badge and whoever's reading it out doesn't know that it's --

MR. CLAWSON: Checked before that it was --

DR. ZIEMER: I'm wondering if they didn't do that.

DR. BEHLING: Well, I guess I don't really understand what a test dosimeter represents.

MR. MORRIS: Well, I think he, we don't know what the --

DR. ZIEMER: Sort of like a blind review for dose reconstruction.

DR. BEHLING: On the next page, item E again, "test dosimeters were not routinely evaluated." I don't have --

MR. MORRIS: Well, first of all they didn't process them, and then they didn't evaluate them.

MR. FAUST (by Telephone): That's correct. They were not processed unless there was some mix-up or a question regarding a particular result of a particular dosimeter. Then a control may have been processed just to answer that particular.

MR. MORRIS: The way I interpret this is, are you involved in a round robin where you're trading dosimeters with other groups.

MR. FAUST (by Telephone): That we would terminate, we would call that a test program, and they would be readily identified as a separate test from the ordinary day-to-day operation of the dosimetry program.

MR. MORRIS: But I think the important part of this quote is where the responder says test dosimeters weren't done, but we did do five of these and six of those.

MR. FAUST (by Telephone): That's correct. They did a whole bunch. I mean if I remember correctly it's some 15. They also used a so-called fast dosimeters for the calibration of their densitometers.

DR. BEHLING: We can strike number three then. As I said I was not, and I'm still not certain I understand what test dosimeters are, but I will accept the notion that a certain number of badges were processed with this badge to establish the fact that a system was properly functioning, at least the densitometer.

DR. MAKHIJANI: Number three.

DR. BEHLING: Three on page --

DR. ZIEMER: In the report.

DR. MAKHIJANI: Not in the matrix.

DR. ZIEMER: No.

DR. ZIEMER: Could I ask here? I'm not sure anybody was using sort of you mention the quality factors here. Somewhere I thought I saw that. Virtually everyone in the early '50s was using a, well, I'm not sure anybody was using the rem even.

DR. BEHLING: No, they were interchangeably.

DR. ZIEMER: Well, I think in the early '50s I'm not sure the rem was even --

MR. RICH: Invented yet.

DR. ZIEMER: Well, probably invented, but I think almost all badges were in Roentgen units in those early days. Even the rad probably wasn't, in fact, they were using reps and --

MR. FAUST (by Telephone): And we recognize that, and we take the easy route out, and we equate them all.

DR. ZIEMER: Well, I think within the accuracy of a film badge, a rep, rad, rem, it's probably the same thing.

MR. FAUST (by Telephone): It didn't matter all that much.

MR. RICH: For gammas.

DR. ZIEMER: For gammas I'm talking.

MR. FAUST (by Telephone): Yeah, for gammas.

A rep was like 94 erds* per gram and a Roentgen is give or take a little bit about 88.

DR. ZIEMER: Eighty-seven point six.

MR. FAUST (by Telephone): Yeah, right. So for all practical purposes it doesn't really matter whether, you can interchange them without worrying too much about it. And they finally, everybody did. But as far as I can tell the rem didn't come into play until probably early '50s, somewhere around '55 I would guess.

DR. ZIEMER: Well, I don't think it was that early. I'd say '59 or '60.

MR. FAUST (by Telephone): Well, I was thinking of ICRP-2 they were using the rem.

DR. BEHLING: Well, that wasn't published until '59.

DR. ZIEMER: Roughly, yeah.

MR. FAUST (by Telephone): That wouldn't matter here anyway.

DR. BEHLING: In fact, if you look at (unintelligible) 20 in the late '80s before they converted or revised the NCR (unintelligible) standards, the statements say that Roentgen is the rem for gamma and beta. That’s the statement. I don’t know what to say other than to again mention the fact that I didn’t come across any kind of QA of procedures or anything that for the early years would let you know that there was a quality program here that had certain procedures that had to be adhered to in processing and read out of dosimeters.

I obviously identified that as a finding, but at this point you’re going to find anything that would support your statement that there was some very, very well defined procedures and protocols in place. I didn’t see any. And the 1981 document reports that statement that there is very little data so we really (unintelligible) which film dosimeters were used.

Finding 4.5-2, unaccounted doses to --

MR. GRIFFON (by Telephone): Hans, back to 4.5-1, I just wanted to say I have one action down there which is just that, and this has been a kind of ongoing thing, that NIOSH will also attempt to identify procedures in their QA reports from the early time period related to that topic. So it's just sort of a follow up. If we can find any procedures or QA reports from that '53 to '85 time period that might help close this out.

The only other thing I wanted to note in that in your report page 115 interested me. As we were looking through those other areas I looked at page 115 on the bottom, and this part, I don't know who added the emphasis with the underlining, but it might have been you, Hans.

Under number one there at the bottom of the page it says employees have always worn badges; however, exposures were not always determined for all employees. I think we might want to keep this in mind as we look at the data completeness question when we look at these individual files. I don't know if it's going to even, are we going to be able to pick that up though because I'm not sure what years we might have annual summary data as opposed to cycle data.

Can NIOSH respond to this? I mean, you're probably aware of this issue or this statement, but does this result in gaps in the external monitoring data or have you examined this at all?

MR. ROLFES: NIOSH is aware that not everyone was monitored.

MR. GRIFFON (by Telephone): This says even the people who were wearing their badges, not all of them, exposures weren't determined for all of them.

DR. ZIEMER: This is Ziemer. I'm not sure you should interpret that word badges as film badges.

MR. GRIFFON (by Telephone): Security badges, I know, yeah.

DR. ZIEMER: I mean many facilities everybody wore badges, but not everybody wore film badges. The ones that did those were built into the security badges.

MR. GRIFFON (by Telephone): Since badges were always a combination security/dosimeter badge is the first line in that paragraph, that's what I was going on.

DR. ZIEMER: Yes, but if you weren't required to wear a film badge, it wasn't loaded as it were.

MR. GRIFFON (by Telephone): Right, right,

right, so, yeah, I understand. And also, if they were in a, they could have made an educated determination to not measure, like I think at Rocky Flats we found that they made a decision; it was in memos, that these people that were on quarterlies we weren't going to read the badges in these years because they were likely to have very limited exposure anyway. And so they made a determination that certain people even though they had the badge in there, they weren't going to bother reading it.

DR. ZIEMER: Well, you wouldn't read it unless there was some kind of an incident?

MR. GRIFFON (by Telephone): Yeah, if something came up, then they might, you know, so they only, but I just wondered whether this, you know, I'm not sure exactly how to, I just noticed this statement as I was reading through.

MR. FAUST (by Telephone): This is Leo Faust. We have to remember that the dosimeter in the security credential weren't necessarily incorporated into a single unit until later on. I can't tell you what Fernald did, but a lot of other sites the security credential and the dosimeter were two separate things. And finally they were incorporated probably in the mid-'50s for the most part.

MR. GRIFFON (by Telephone): Yeah, that's what I, we've certainly seen that at other sites. And this statement in the survey, maybe I'm misinterpreting it or it might be slightly inaccurate. Who knows? But I just think we might want to, but I think this question can be further examined in our review of the data completeness question really. I don't know that --

DR. ZIEMER: Another piece of that is it looks like they had criticality dosimeters incorporated into those. So that might be a reason why everyone would wear it.

MR. GRIFFON (by Telephone): Right, right.

DR. ZIEMER: But you wouldn't look at any of those unless you had a criticality accident.

MR. HINNEFELD: This is Stu Hinnefeld from NIOSH. I'm just reading this note off Brad's screen here, and it sounds to me like they describe the people who were not, whose badges were not read and were women through two periods of time. And I was told anecdotally while I was there that there was a period of time when women weren't allowed to go to the process areas so they didn't wear a dosimeter.

MR. GRIFFON (by Telephone): Yeah, maybe it is just a female.

MR. HINNEFELD: I believe the people who were -- wore a badge who were not monitored are depicted right there in the following four lines. It's the two periods when women weren't.

MR. FAUST (by Telephone): Well, yeah, there was two periods in time when females were not, did not wear a dosimeter.

MR. HINNEFELD: I believe that's the entire interpretation of that statement that not everybody who wore a badge was monitored.

MR. FAUST (by Telephone): That could very well be, Stu.

MR. GRIFFON (by Telephone): It could be, Stu. You could be right, yeah, yeah.

MR. SMITH (by Telephone): This is Billy Smith. I was the health physicist in charge of dosimetry at the Nevada Test Site for years, and I processed thousands of film dosimeters. The terminology that I’ve been listening to relative to the quality control process that may have been in place, at NTS we processed film in batches of 100 badges each.

And in each batch we had two controls which were dosimeters, film badges, that had no dose on them that we used to determine what the background was going to be that we were going to subtract from any readings from any film that we read.

Also in that batch we had five standards. The five standards were film that had been exposed to 30 millirem, 100 millirem, 500 millirem, 1,000 millirem and 2,000 millirem. Those were processed prior to reading any dosimeters for dose purposes from people to determine that the densitometers were working properly, and then the individual film dosimeters were read.

Now if the same process that Oak Ridge had previously established that Fernald was following, then they would process controls and what I call standards at the same time. And that to me indicates a quality control process that’s going on in the reading process.

MR. FAUST (by Telephone): I think they looked at it that way, Billy, because that was pretty much the standard practice throughout the industry. And there's no reason to believe that Fernald was any different than anybody else in my opinion anyway.

MR. CLAWSON: Well, we can all project our opinions into what they mean, but bottom line is we're trying to get to the bottom of a determination if we could find this is evidence of this, then it brings this to a head. And as Mark stated into this that he's tried to check for a procedure or so forth that was being followed to be able to say that this is how it was done.

MR. FAUST (by Telephone): And we're continuing trying to find those procedures. We found a lot of different procedures, but we haven't found one for that yet. But that doesn't say it doesn't exist.

MR. CLAWSON: That's true.

DR. ZIEMER: So they're continuing to look for that then. Well, who read about the five and four? Was that from there? Was that a procedure?

DR. BEHLING: Yes.

MR. MORRIS: Well, right here on page -- you got your file open?

DR. ZIEMER: Oh, yeah.

MR. MORRIS: The SC&A report page 114 at the bottom of that page, item D.

DR. ZIEMER: Now is that a quote from --

DR. BEHLING: You know, the response, from the 1981 response report.

DR. ZIEMER: Okay.

MR. GRIFFON (by Telephone): And that sort of, I mean, Paul, I think that says they're doing calibration films but whether it was each batch just like we heard happened in Nevada. But if we can get a --

DR. ZIEMER: Well, a formal procedure would help.

MR. GRIFFON (by Telephone): But I think that's as far as we can take it really is to try to identify that.

DR. ZIEMER: They're doing that which would, if they're doing what would be considered good practice at the time, that's important.

MR. GRIFFON (by Telephone): Right, but I think this survey, to me anyway, it says they were doing calibration runs along but not necessarily what people are calling tests or round robin tests or whatever.

MR. FAUST (by Telephone): By the way that was recommended in that Parker report that continued inter-comparisons between the sites was recommended by Herb. Whether or not it was practiced I can’t say for certain, but I do know that at the Hanford site we did exchange dosimeters with various other organizations on a cooperative kind of a basis. There was nothing required to do that, but we just did it as a good practice.

DR. ZIEMER: Do we know whether later on after they got into TLDs and so on, did they get involved, would they have been eligible for DOELAP?

MR. HINNEFELD: They were certified by DOELAP.

MR. FAUST (by Telephone): They were the very first organization to be DOELAP accredited.

DR. ZIEMER: Okay, well, that’s important to know.

MR. FAUST (by Telephone): And they've kept it up or they've kept it up through their operating time.

DR. ZIEMER: Well, that doesn't come out of the blue either. There had to be some --

MR. CLAWSON: Other reason.

DR. ZIEMER: -- well, some basis for achieving that.

MR. MORRIS: This is Morris. One of the things, Hans, that you had mentioned in your preliminary remarks was matching the energy spectrum of the radionuclides to the challenge spectrum that is used for calibration. And radium and uranium combination is quite a reasonable approach to matching that spectra I would think.

MR. FAUST (by Telephone): Well, they couldn't get any better quite frankly. And later on it was DOELAP. DOELAP dictated what calibration energies they had to perform by. So that kind of took it out of the individual site's hands so to speak.

MR. CLAWSON: Well, this is a rousing conversation. I do think we're going to take probably a ten-to-15 minute break. We're just going to mute the phone, but we're going to have a quick comfort break, and then we'll come back.

(Whereupon, the working group took a break from 3:30 p.m. until 3:40 p.m.)

DR. WADE: We're back in session, the last leg of a journey.

FINDING 4.5-2

DR. BEHLING: I guess we're down to the item finding 4.5-2 on page 119 and that is the exposures to extremities. And I guess there were certain studies done, and I quote them in the first quotation there on page 119 that talked about fairly high ratios.

And I can only gather that the ratios that are defined therein in 1963 where they identified 22.9 rem that represents beta and gamma versus 4.4 rem penetrating only in providing a fairly high ratio was based on a whole body dose in the one on the chest. And at that point they compared it to earlier data in the 1960s when that same ratio was 20.7 to 1.

And so it's clear that there were significant skin exposures, and I believe those measurements represent a film badge worn at the chest. Now the question that comes to mind is what would have been the potential exposures to extremities that were not monitored.

And it's clear that obviously that ratio for a whole body dosimeter may have significantly underestimated skin exposures experienced by your extremities based on strictly the geometry and/or distance to source term especially when you deal with uranium and its radioactive daughters, Protactinium-234. So I raised some questions about the potential for extremity exposures that may not have been properly monitored or not monitored at all.

Because in the next paragraph down there we talk about, and I quote, "The results of the study showed projected annual forearm exposures from about 14,000 to 46,000 millirems." And there was a subsequent time period during which wrist exposures were used, and again, they may or may not necessarily reflect hand exposures which were estimated to be two-to-three times the wrist exposure.

So this whole issue deals with extremity exposures to the skin and in particular hand exposures that even during the time of wrist monitors may not have been properly assessed. So I guess I'll ask Mark to, give your opinion as to what you intend to do to deal with skin exposures and specifically skin exposures involving the extremities.

MR. ROLFES: We at NIOSH typically don't receive many claims for a skin cancer of the extremities. I can think of one. We do have extremity monitoring for many individuals that may not be complete in certain years.

What we have done typically is used recorded results to the time, day, the ratio for -- to interpolate between years where the individual didn't have an extremity monitor. We can use a ratio of the dose received by the whole body badge to assign a ratio to the extremities, or assign a dose to the extremities, excuse me.

We don't consider this to be an SEC issue but rather an issue that is how we go about doing dose reconstructions. What assumption we make regarding the dose that's being assigned. And once again this is for a very, very low number of individuals whose dose is being reconstructed.

DR. BEHLING: I accept the notion that skin cancer of the extremities are probably not a very common occurrence, and you may not even have one. But if you did have one, you would have a difficult time in reconstructing exposures during a timeframe when skin exposures to the extremities was not the issue. And so if it's a contractor would you say I'll reconsider that the potential issue that involves (inaudible) because you really have no way of properly addressing that particular exposure.

DR. MAKHIJANI: I have a question.

MR. ROLFES: The approach that we use in that case it would be done on a case-by-case basis. We would have to take a look at the extremity dosimetry results for the employee. And if there was a year or two years that the individual was not monitored for extremity doses to the skin, on skin we could use a previously documented ratio of the recorded wrist dosimeter to the whole body badge and assign that ratio for the unmonitored periods. There are other methodologies that could be used.

DR. MAKHIJANI: Mark, just a question of I thought wrist dosimetry started in 1970. Was there wrist dosimetry before 1970 at Fernald?

MR. ROLFES: I don't believe a routine program prior to that.

DR. BEHLING: No, and, Arjun, what I've mentioned was the earlier ratios that were developed were based on the shallow dose to deep dose worn at the chest so we don't really have an understanding other than certain measurements that were done, I guess, on an experimental level later on that would suggest a fairly large ratio.

DR. MAKHIJANI: Yeah, so that's right. I mean, it's not a question of a year or two gap where somebody was monitored, and then they were not monitored so you can interpolate something, the whole first 19-year period I think. There may have been some experimental badging, but I don't believe there was extremity badging until 1970.

MS. BALDRIDGE: Mark, I have a question. Have you addressed exposure to private contractors who came in? Because the petition was filed for employees and subcontractors. I personally met a gentleman who had skin cancer on his arm who was a private contractor. He wouldn't have monitoring. He was allowed to work in his street clothes.

MR. ROLFES: Was he in a radiation area?

MS. BALDRIDGE: He was cleaning and painting. He's had two types of skin cancer.

MR. ROLFES: Was he in the process area or was he outside of the area?

MS. BALDRIDGE: I don't know where he was, but the point is subcontractors who didn't have files, they're not considered employees to be monitored the same way someone on National Lead's payroll would be. What type of provision is there for them in the evaluation of their extremity exposures?

MR. ROLFES: What time period was the individual on site?

MS. BALDRIDGE: Probably late '80s, '90s.

MR. ROLFES: Well, if it's the '90s, we haven't specifically looked outside of the SEC time period. During the '90s I haven't looked in detail at the procedures involved for individuals that were brought into the site.

MS. BALDRIDGE: I believe his father was a sheet metal worker, and he's been dead for years. So there wouldn't have been a claim filed for him unless he filed for his father in addition to himself.

MR. ROLFES: I would have to take a look at the information that we have and separate from dose to the extremities for a painter or for someone who would have brought in casually into the area. It's very unlikely that those individuals would have spent a significant amount of time in a position where they were directly handling uranium metals.

MS. BALDRIDGE: Except that they would have had to be cleaning and prepping areas that would have been contaminated.

MR. ROLFES: Sure, they may have --

MS. BALDRIDGE: Releasing that as a dust factor. If they're sweaty, then you have an adhesive factor there where the perspiration just causes the dust to cling to the skin.

MR. ROLFES: We actually did a little research experiment to bound this scenario. I can have Billy if he's on the line and available to discuss what we did, or Bob?

MR. MORRIS: I don't have my memory right now.

MR. ROLFES: Okay, Billy, are you available to discuss what we did with the experiment that was conducted to bound skin contamination?

MR. SMITH (by Telephone): Yes, I'm on the line. I conducted an experiment at ULD using Whatman filter paper, both wet and dry filter paper, and to see how much contamination could actually be deposited, retained on the filter paper.

I used chalk dust, that is the construction material that's used to mark chalk lines, and sprinkled it on square pieces of paper that were about three centimeters square. And then shook it off of the dry and the wet and then weighed the individual pieces of paper. And there were 20 pieces of paper in each category.

And then after the weighing took place, I converted that math to what the uranium, I assumed that that mass was uranium and converted it to a uranium number and came up with what kind of contamination would be retained on a person's skin. It's interesting to note that it takes a very, very large amount of contamination to be seen on a person's skin, on his contamination clothing.

And right now I'm in the process of finishing up a white paper of this experiment that will be provided to NIOSH so they'll be able to put it out on the O drive so you can get a feel for what the exposures would be. But the exposures came up to be very, very low from the amount of contamination showing up on these particular experimental papers.

MR. FAUST (by Telephone): This is Leo. In addition to that there were some actual measurements of contaminated gloves at Mallinckrodt. And the maximum dose rate was measured, if my memory serves me correctly, was 45 mRads per hour. And the smallest or the minimum dose rate, and these are now contaminated gloves, was like 23 mRads per hour. So the people that we interviewed were adamant in their insistence on clothing changes at least twice a day and glove changes as often as once an hour. So in our estimation and in the records if you look at the exposure records, there is no one that exceeded the administrative levels for skin contamination.

MR. MORRIS: Skin dose.

MR. FAUST (by Telephone): It’s highly improbable that a painter, particularly late in the ‘80s or early ‘90s, would have had anywhere near that kind of an exposure.

MR. ROLFES: In addition to what Leo has said we also do have some surveys that were taken of personnel clothing, coveralls. There were several measurements made in various areas of several different employees’ coveralls and shoe covers. There are results --

MR. FAUST (by Telephone): It’s item 41-36.

MR. ROLFES: Approximately 15 measurements of the chest area on coveralls, the stomach area on coveralls, the thigh area and the leg area of coveralls, and then also additionally there are radiation survey results of the shoe covers. The results range from, the highest result that I see here is one and a half millirep per hour.

MR. FAUST (by Telephone): There was also the Fernald used the wrist dosimeter prior to the advent of reasonable TLDs, and they used a wrist-to-extremity ratio. And the ratio that was employed was three. So whatever the wrist dosimeter results were, the extremities were given three times that. And in about 19 -- I believe it was, well, I don’t know, ’87 I’m going to say, a study was made by an independent person.

And she concluded from the results of her study that the ratio, wrist-to-extremity, should have been somewhere around 2.1. I think it’s actually 2.09 is what she came up with. But they accepted that, but they did not change any of their doses of records to account for the fact that they were very, very claimant favorable.

MR. GRIFFON (by Telephone): But I guess I’m having a little trouble following on the phone here. I think we just went back to Finding 4.5-2. We delved into 4.5-3 for awhile there. I think for 4.5-3, I don’t know how much further we want to discuss it, you have an ongoing action. And it says that in the NIOSH response.

But 4.5-2 I think, I mean in my mind my question is similar to what Arjun had raised which is this, you know, there's the other question about this wrist ratio is that prior to 19 -- I forget the date -- 70 or whatever, you didn't have any wrist monitoring at all. So you're proposing that that ratio was consistent through all the early years, too, I guess is what you're saying.

MR. FAUST (by Telephone): Yeah, and I'm not certain I believe that they actually did use wrist dosimeters pre-1970.

MR. GRIFFON (by Telephone): Okay.

MR. FAUST (by Telephone): They didn't use extremity dosimeters, but they used a wrist dosimeter and used that multiplier.

DR. MAKHIJANI: Well, is there documentation on the site research database about wrist and finger dosimetry? I think Stu might know.

MR. HINNEFELD: I don't know when the --

DR. MAKHIJANI: It's my recollection. I might be wrong.

MR. HINNEFELD: I don't know when the wrist dosimetry started. I believe it was in use when I got there, but I don't know when it started. The ratio, you know, I remember the ratio, the wrist badge being applied to generate the hand or the extremity dose. I remember there was a ratio used for that.

DR. MAKHIJANI: I agree. I remember the same thing.

MR. HINNEFELD: But I don't know what the years when wrist monitoring was done. I believe there's a presumption here that wrist monitoring didn't start at the beginning, that whatever ratios could be determined from the monitoring occurred later on would be applicable backward or there may be, you know, I don't know if they're saying they're universally applicable backward or applicable backward with some caveat or some --

MR. GRIFFON (by Telephone): But if you don't have wrist monitoring in the early years you have nothing to apply the ratio to.

MR. HINNEFELD: So what I'm saying, no, I'm saying in addition to the hand-to-wrist ratio there's also theoretically a ratio to be developed from a wrist to a whole body.

MR. GRIFFON (by Telephone): Yeah, right.

MR. HINNEFELD: So it would be combination of those two ratios if, in fact, it was extrapolateable, backwards.

DR. MAKHIJANI: Mark and Brad, one of the things we could do since we're looking at external completeness is to look at when the wrist dosimetry started.

MR. GRIFFON (by Telephone): Yeah, okay, that can be --

DR. ZIEMER: Well, there's an additional issue. Did they have any operations at Fernald where the body was actually shielded such as you have in nuclear pharmacy where there is reaching around a shield and a whole body dosimeter will read essentially zero and you can have high doses to the hand?

That's very different than working, painting and so on where you're moving your arms around and the actual extremity doses are not very different from the whole body for a person just working around in a general radiation field. Their body moves around. Their arms move around, and the integrated doses are probably about the same.

The real times when extremity becomes important is when the body itself is shielded or if you have something like the fraction units where you can stick your finger in a beam and that's very different.

MR. HINNEFELD: I don't recall any, but it's been awhile.

DR. ZIEMER: Because unless they're doing something like that I don't see extremity dosimetry as being important.

MR. FAUST (by Telephone): I don't either. Well, to answer your question, Paul, they didn't use a whole body shield as a radiologist might use. They did use some face shields though for eye protection.

DR. ZIEMER: That's for lens of the eye.

MR. FAUST (by Telephone): Yeah, right. They did use some rubber matting to cover finished product when it was located near a place where, well, located near an occupied area. They used rubber matting of some kind or another to reduce the exposures that might be obtained from that stockpile, if you will. But other than that I don't believe they used any protective clothing other than anti-cees.

MR. GRIFFON (by Telephone): But didn't they, I mean, you've got to think about the operation itself not necessarily the protective gear but the operation itself. Didn't they have furnace operations where they would have been reaching into, you know, I'm thinking about like cask cleanouts or cask cleanout operations, those kind of things. That would be the more, where I would expect more and where you have --

DR. ZIEMER: That's similar to a shielding.

MR. GRIFFON (by Telephone): -- where you also have the daughters separating, and you have high concentrations of the radionuclides of concern.

MR. FAUST (by Telephone): Well, they encountered that. There was no doubt about that.

MR. GRIFFON (by Telephone): Yeah, so that would be more the issue.

MR. FAUST (by Telephone): The slag was the concentrator of many of the daughter products.

MR. GRIFFON (by Telephone): Right.

MR. FAUST (by Telephone): And contaminants of other kinds, too.

MR. GRIFFON (by Telephone): And that's where I would be concerned about the whole body ratio being representative of what your arms, your forearms, would be getting. I mean, that's why the document Hans has in his report specifically talks about the forearms, and I think that's probably why they're looking at that.

MR. FAUST (by Telephone): But my point is that the whole body dosimeter would be exposed in those kinds of situations, too.

DR. ZIEMER: It's reaching in and cleaning up something that was heavily beta in there you might --

MR. FAUST (by Telephone): Yeah, if it was inside of something, yeah.

DR. ZIEMER: But I think for dose reconstruction if you looked at the individual case and find out what the person, what kind of things they did, you would end up having to model that in some way I would think.

MR. ROLFES: You can certainly learn a lot by looking at an individual's dosimetry records, and you can identify those people who would have been in a job where a potential extremity dose could have been significant. Typically, those individuals weren't monitored especially in the more recent years.

There were surveys taken in the earlier years prior to extremity monitoring. That is typically something that is done on a case-by-case basis based on the specifics of the case. That's how it's previously been handled. And it's been handled for a very, it's been a low number of individuals who are potentially affected by this issue.

MS. BALDRIDGE: I have a question. It's not exactly an extremity, but how do you deal with the set-up man, the set-up people that worked with their heads inside the machinery?

MR. ROLFES: The head is considered an extremity so it's not the same extremity that we're referring as the hand or a foot, but it is once again --

MS. BALDRIDGE: Special consideration for that type of exposure?

DR. ZIEMER: There's a different dose limit for the lens of the eye because that's a critical organ for the head.

DR. BEHLING: In those early days they didn't have a 300 milligram dose to worry about.

DR. ZIEMER: They didn't worry about it, but we would worry about it now for dose reconstruction.

DR. BEHLING: Either shallow dose or 1,000 milligram dose.

DR. MAURO (by Telephone): This is John. Are we talking about concern over skin cancer of the extremities?

DR. BEHLING: Yeah, I guess we're talking about skin cancer not only of the extremities but in the next finding, 4.5-3, we talk about shallow and deep dose resulting from skin and clothing contamination that now extends to the whole body skin that may have been underestimated by use of a whole body film or TLD that may or may not necessarily represent the skin exposures received to the head or the chest, the trunk or any place else.

So, yeah, we're talking about skin cancers at large, and then in Section 4.5-2 we're talking about potential skin cancers as they reflect the areas of the skin associated with the extremities, the hands and the forearms.

DR. MAURO (by Telephone): Now what about the, we know what the upper limit is for a contact dose with uranium. So in other words I think it’s 200 MR per hour in that order.

Does that have any play here or are we basically saying all dose reconstructions, for example, related to skin cancer would be based on some type of film badge reading either wrist or some kind of adjustment factors to a ratio of, let’s say, hand-to-wrist or does somehow this affect the upper theoretical limit, which would be the contact dose, have any play here in dose reconstruction? Do you believe that the person who was involved in actually handling -- in this case I’m using pure, natural uranium -- do any of your procedures factor that in as opposed to depending on the actual film badge reading?

MR. ROLFES: The first piece of information for a dose reconstruction, for example, for a skin cancer claim would be the individual’s dosimetry records. That was the most significant type of exposure that could have been received by an individual especially chemical operator working in direct proximity to uranium metal, especially aged metal.

We are aware that high skin dose results were recorded for many of the chemical operators. We would, in order to have an issue with the dose reconstruction for skin cancer, we would have to have a skin cancer that was a non-compensable claim. At that point we could take a look at the specifics of the case and determine whether the individual was in fact in an area where he was, could have been exposed in any abnormal geometrical, you know, we would take a look based on the location of the individual’s cancer to determine if any additional correction factors wouldn’t be needed to correct the whole body badge result to the area where the skin cancer was located.

I can’t give a more specific answer than that because I would need more specific information about the cancer location and the job duties that the individual was working in and also some information about the individual’s recorded dose. I’d be happy to explain more details for a specific case scenario.

I’d be happy to answer any questions that there are about any specific scenario and how we would handle dose reconstructions for that specific scenario. So if there are in fact a specific case such as --

DR. BEHLING: Let me point back to the statement that I quoted on page 119 where we had two timeframes, one 1961-1963. And during that time the ratio between penetrating dose and shallow dose as measured by, I assume, the whole body dosimeter mainly worn at the chest, and during that three-year time period it had changed, the ratio had changed from approximately five-to-one to 20-to-one ratio. In other words in 1960 if you measured one rem to the whole body for penetrating radiation, you would have potentially experienced 20 rem of shallow dose. That changed to only five-to-one for 1963. So it was a dynamic process and there was no single value, but it depended obviously on the material that was handled and the quantity of material handled. And so it did change over time. And the absence of monitoring really gives you some problem how to accommodate these skin doses especially to the extremities that were not monitored for these select periods of time early on. And as I said in the second statement, there was a study that said the results of the study showed projected annual forearm exposures of 14,000 to 46,000 millirem. You're talking about a substantial dose to the skin involving people who may have handled these materials. And as Sandra pointed out we had a person here with two skin cancers to the forearm, and this person apparently was not monitored. Is that correct?

MS. BALDRIDGE: (Unintelligible) monitored.

MR. ROLFES: Was that monitored at all or monitored for his extremities? This sounds more of a contamination issue than it does an extremity monitoring issue. I think they're two separate issues that we're discussing.

<u>FINDING 4.5-3</u>

DR. BEHLING: Well, that brings us to the next statement, and that is unmonitored shallow and deep dose resulting from skin-clothing contamination. And we briefly addressed it. I think Leo or somebody else had mentioned the issue of having done experimental studies with chalk and filter paper.

But I did go through it and obviously anti-contamination of clothing was not provided for a good number of years during the operation, and neither were people monitored by using the friskers or portable monitors for skin contamination. And, of course, in those days early on one can reasonably conclude that oftentimes people may have worn the same clothing over and over.

And the assumption that skin contamination is something that is confined to at most a 24-hour period between showers may or may not hold true. Certain skin contamination is very persistent, and therefore, in the absence of monitoring for skin and clothing contamination, you may have had a substantial skin dose that resulted from persistence of repetitive skin contamination that were clearly not monitored.

And early on Sandra had mentioned the issue about families being invited onsite and acknowledged the fact that they might in fact be now informed of the issue that they've carrying contamination home with them. And so that particular issue is raised here under Finding 4.5-3.

And I don't know when it was that you finally introduced the issue of frisking out people. Certainly it wasn't done for many years early on. In fact, I think it's the 1985 site (unintelligible) that identified, and I quote -- this is on page 124 -- "there are no contamination survey instruments kept at the work site for use in checking for skin and clothing contamination. Neither are there any hand and shoe counters available for use either before or after showering."

So as late as 1985 you make very little effort to assess people for skin and clothing contamination that may persist for days on end and repetitively expose people to fairly high skin doses.

MR. ROLFES: Thank you, Hans. Well, I'll reiterate what we do have from 1958 is the results of a clothing survey. It appears that the chest, stomach, thigh and leg area of 15 individuals’ coveralls were in fact surveyed. The highest contamination result was 1.5 millirep per hour. Shoe covers were also surveyed.

The highest result there was 0.5 millirep per hour, an average of 0.24 millirep per hour was documented. Individuals that were working in the process areas were required to shower before they left the area for lunch and then again before they left for the day. Employees were also encouraged that if they had visible contamination on their clothes, they were encouraged to shower during the day, and they were allowed by management to leave the process area to go take a shower and change into new clothes.

Management also encouraged the routine exchange of gloves by these individuals. There is documentation of the numbers of gloves that were sent offsite for laundering. These were in the thousands for one month. So it does indicate that the individuals were changing their gloves very frequently. We have documentation of individuals changing their clothes and showering very frequently.

So it's very unlikely that a large contamination, you know, a large unknown contamination problem existed.

We feel that based on the results of surveys that were conducted, information that we have in a person's claim files and various other sources, reports. We feel that we can bound a skin contamination dose, and also we do not feel that this is an SEC issue.

DR. BEHLING: Well, again, it's a subjective issue whether it's something -- if you don't monitor, you can't really assess the exposure. Again here there was a, in Attachment 4.1-3-A on page 123 is a memorandum issued by the senior person who was responsible for the Safety and Health or Hygiene program who in 1953 states that only those men involved in the cleaning of the (unintelligible) would be required to make a clothing change, again indicating that the majority of people were not given anti-cees and probably there was minimal effort to assess potential skin and clothing contamination during the early years and possibly as late as the 1980s.

MR. ROLFES: I can show you pictures of the individuals that worked in the workplace at Fernald showing that they all had anti-cees. They all wore coveralls.

MR. MORRIS: And it was hundreds of thousands of gloves per month, not thousands.

MR. ROLFES: There was quite a high number.

MR. RICH: That's a lot of thousands.

MS. BALDRIDGE: We have to watch about generalities because this petition covers 40 years.

MR. ROLFES: Correct.

MS. BALDRIDGE: When my father was working there, he was never issued gloves. I have his clothing issue and gloves was not on it, but yet he was an inspector, and he was handling the slugs for inspection. So when you say there were gloves, there really needs to be a timeline. When were there gloves? You know, there are periods I know there were no gloves.

There were periods that people were not required to have the monitoring badge. There were, you know, there's so many different issues that affect specific groups within this petition, but they can't be considered to be broad spectrum over the entire 40 years. They may apply to one part and not another part. When you do the rationale on this you have to be able to differentiate what timeframe, what group was given what protection, what provision, and what other groups were not.

MR. ROLFES: The report that we were referring to was from 1959, so I haven't completed an exhaustive search for the time periods when gloves may or may not have been worn and the numbers of gloves that were worn.

MS. BALDRIDGE: I mean gloves is just an example.

MR. ROLFES: Sure, sure.

MS. BALDRIDGE: Throughout the discussion, two meetings even before, there's just a lot of factors that each group differs --

MR. ROLFES: That's very true.

MS. BALDRIDGE: And it has to be sorted out.

MR. ROLFES: That's very true. I agree completely with that. There are very specific issues that we're discussing today that typically only affect a very, very small population of the claimants that we have.

Many of the issues that we are not

discussing, well, excuse me, on a broad basis we typically have 99 percent or greater than 99 percent of the information that we use for an individual's dose reconstruction available to us within that dosimetry file. The issues that we're talking about today are typically the less than one percent of the individual's dose or a very small fraction of the individual's work history that we're referring to.

It requires a lot of digging, a lot of investigation and these issues really, I don't want to say they don't affect, but we're getting into very specific small populations of workers involved in some of these projects and in some of these unique scenarios.

DR. MAURO (by Telephone): Mark, this is John. On a related matter we spent close to two or three hours last week talking about OTIB-0017 which is the OTIB dealing with reconstructing shallow doses. And I think a lot of what we're talking about today as applied to Fernald also is very much related to the discussions we had the other day. I think some of the members of this working group are also sit (sic) on the other working group.

And I just want to point out that the area that was most contentious with regard to OTIB-0017 was the dependence on your open window film badge reading worn on the lapel for shallow dose measurements and the fact that those measurements -- and I know we're talking about this though -- are really going to decide contact doses which could occur.

There's also the matter of particulate deposition. And I think that it sounds like that there's a great reliance on some film badge readings in a setting where there could be, the film badge readings could, may be very non-representative of the exposures to a particular location on the body, especially beta exposures localized to the skin.

And so we got into the discussion of particle deposition on skin, the ability to detect it when a person's leaving an area from a scan or a portable monitor. We got into discussions regarding VARSKIN and what type of doses could be experienced as these small particles are sitting, let's say pure uranium metal oxide, on your skin for a day before it's washed off.

Subjects that I guess we haven't, I haven't heard discussed here with respect to Fernald. To what extent do those kinds of issues come into play in the dose reconstructions, factoring in all these other matters that I just mentioned?

DR. BEHLING: John, I would assume that hot particles do not apply to Fernald.

DR. MAURO (by Telephone): So they don't have any air particulates of uranium depositing on people's skin.

DR. BEHLING: Well, you mentioned already that they did calculations using enriched, up to 20 percent or whatever, enrichment of uranium and then determined basically on the basis of skin contact what the dose would be.

DR. MAURO (by Telephone): Okay, I apologize. I didn't catch that. So you do use those techniques to place an upper bound on what some localized dose might have been?

DR. BEHLING: Yes, and hot particles do not apply to Fernald.

DR. MAURO (by Telephone): Okay, I'm sorry for bringing it up.

DR. ZIEMER: Well, another issue, it's not clear to me that the surface dose is really the limiting factor here. Usually on contamination you're really, the same amount of activity, put it in nanocuries or whatever, it's usually pretty low, maybe microcuries in this case.

But you can deliver much more if you ingest that than you will ever get to the skin I would think. Isn't the internal dose still going to be kind of the limiting or the driver on these for personal contamination? Is skin dose really the issue? I mean you can get cancer.

DR. BEHLING: (Unintelligible).

DR. ZIEMER: Yeah, but it takes a lot of dose to get an effect on the skin.

MR. ROLFES: Well, our first piece of information if there was a systemic contamination problem that typically skin contamination is not a large source of dose to the skin.

DR. ZIEMER: That's what I'm saying.

MR. ROLFES: Direct contact with uranium metal is in large quantities. And that would typically outweigh any potential exposure from skin contamination by orders of magnitude. That’s the bottom line. We do have approaches and methodologies that we can add what we’re essentially referring to as a very small amount of skin dose based on skin contamination in comparison to the large amount of dose recorded by the person’s dosimeter. It’s something that we have an approach for, but it’s not a very significant source of skin dose so that’s the bottom line. And we can do simple VARSKIN calculations to bound skin doses.

MR. CLAWSON: I just wanted a clarification. This is Brad. You kept bringing up that you sent off hundreds of thousands of gloves for (unintelligible). Was this to be laundered?

MR. ROLFES: For laundry, correct.

MR. CLAWSON: Well, we’re talking rubber gloves and stuff like that.

MR. ROLFES: They were leather.

MR. CLAWSON: Cotton gloves?

MR. CHEW: Example, one had processed 29,000 pairs of gloves in one month in August in

1959, something like that.

MR. CLAWSON: They were surveyed when they came back, too? Because I know that right now we have a problem with our scrubs coming back contaminated with europium and so forth like that.

MR. RICH: (Unintelligible).

MR. CLAWSON: I was just sitting there wondering if we had this much data on what was going out, we're monitoring what these were reading going out, coming back, it'd kind of give a better idea of what we had for the kind of contamination issues for that. I was just wondering what kind of documentation we had of that.

MR. ELLIOTT: I don't imagine they surveyed the activity level on the gloves going to the laundry. Do you think?

MR. CLAWSON: Ours does before it can leave the building. They've got to have a --

MR. ROLFES: There are some documents of surveys that contaminated gloves, but as far as them coming back from the laundry I wouldn't believe that.

MR. ELLIOTT: No.

MR. RICH: They would check and evaluate.

MR. ROLFES: They did, however, encourage employees who were doing hands-on work with uranium metal to change gloves, I believe, hourly or as soon as visible contamination was seen on the gloves. And I think that is supported by the number of gloves that were sent offsite in 1959 for laundering.

DR. ZIEMER: But I've seen overalls and so on come back uncontaminated, but almost always it's like a little spot somewhere. I don't think I've ever seen one that was uniformly contaminated come back from the laundry, and maybe it would read a half an MR per hour, a little spot or something. Well, how important is that in, if you're working in a hot area it's usually --

MR. ROLFES: Very trivial.

MR. SCHOFIELD: I've seen them come back with chips of uranium in the pocket.

MR. ROLFES: Sure, it's very possible.

MR. SCHOFIELD: Or it's fairly significant how it got out in the first place, shouldn't have been able to get out to the laundry at those levels.

MR. CLAWSON: I'm kind of at a loss where -- be able to do an action item on this.

MR. POTTER (by Telephone): Mark, this is Gene Potter.

MR. ROLFES: Yes, Gene.

MR. POTTER (by Telephone): I just looked in HIS-20 going back to a slightly earlier subject, and there are extremity dosimeter results in HIS-20 going all the way back to 1952, only just a few people. I'm only able to look at annual totals for those years, but right through the '50s there are some people who are being monitored with an extremity dosimeter it would appear up until the 1970s. Then they appear to come into much more wider use.

MR. ROLFES: Thank you, Gene. I did misspeak. There were some individuals that were monitored for extremity exposures earlier on back in 1952 as Gene has indicated.

So thank you, Gene.

MR. GRIFFON (by Telephone): Well, Gene, is it clear whether those were calculated values or actual readings?

MR. SHARFI: They were actual readings based, they have in HIS-20 a separate results when they actually mark them, either calculated or they give what hand it occurred on.

MR. GRIFFON (by Telephone): Got you, thanks.

DR. BEHLING: Can we get some kind of an assessment as to what the ratio is for individuals whose personal dosimetry data include whole body exposure, deep dose, shallow dose from a chest dosimeter as opposed to dosimeter monitoring data from the extremities. That would be extremely useful for those instances where it might be needed and establish a timeline for ratios that may have to be applied in the event of a skin cancer for whom we have no data.

MR. SHARFI: You have to be careful about drawing too much conclusion. I'm not sure that all these extremities that are listed for the entire year, where their external badge may cover them for the entire year, their extremity may only be from jobs that needed extremity dose monitoring.

DR. BEHLING: Well, so I'm sure that if there are individual wear periods during which that data's available you simply match those data sets for that wear period and not make it all inclusive for that year. But it would give you some kind of an understanding so that when you have a person who has no extremity monitoring but you have data for someone else for that time period that you can potentially find something that has more transportability than for subsequent years that may have no relationship for the time period in question.

MR. ROLFES: So you're recommending that we put some guidance into the site profile?

DR. BEHLING: Not necessarily, yeah, put some guidance without going through that. You may not have one single claim for which this is necessary, and there's no sense in spending energy for something that doesn't have to be done until the time comes where you have somehow feel compelled to do something that is credible. But right now if you say there are no claims for which this dose calculation needs to be done, there's no point investing a lot of effort and only realize it's not going to be used. But --

DR. ZIEMER: It's data that could be used.

DR. BEHLING: Yeah, could be used, and just put it in your back pocket and save it for that day when you may have to resort to that.

MR. RICH: I'm just, to add to what Mutty said. A number of different sites we through the years did some studies of this very issue. And if you look at the whole body through the extremity dosimetry, you'll get a lower value than what you can do experimentally.

If you look at a given job that may go on for a month or so we could get ratios as high as, depending on the job and the kind of material you're working with, up to a factor of seven higher including extremities but not generally much higher than a factor of seven, extremities to whole body. And again, depending on how you're working. Glove box operations are different than working with uranium metal pieces and the like.

DR. ZIEMER: But if you have enough data you can at least bound it then.

MR. RICH: Not to say that this review should be fairly easy to do because it's on a different database.

MR. GRIFFON (by Telephone): Well, that's an interesting statement though, not much higher than seven. You got some skin extremity doses in HIS-20 that range up to 36,000 millirem.

MR. RICH: As I said, the ratio was whole body to extremity. And that would be --

DR. ZIEMER: It would be about five rem whole body.

MR. GRIFFON (by Telephone): It would be about nine rem, right? Oh, five, five rem, right at the limit.

DR. ZIEMER: Well, in those days the limit was 15.

MR. GRIFFON (by Telephone): Right, right, right.

MR. RICH: And then as (inaudible) the reason that the permissible dose for extremities higher (unintelligible).

MR. GRIFFON (by Telephone): Yeah.

DR. BEHLING: But I will bring you back to the statement on page 119 where for the 1960 timeframe a study suggested that the ratio again for the chest dosimeter was the deep-to-shallow dose ratio was 20.7-to-1 so that's greater than five.

MR. RICH: That's very conservative.

DR. BEHLING: Well, this is apparently just quoting what was reported here in this study.

DR. ZIEMER: That might be enough for bounding.

MR. MORRIS: And it could be a scenario that Mutty just described which is that if one was, that they may not have matched monitoring periods.

MR. SHARFI: If you don't compare the data well on the --

DR. BEHLING: Yeah, but to answer your question if anything you would err on the unconservative side. If you monitored whole body but failed to monitor extremity, you're going to end up with a dose ratio that is less than what it should be, not the other way around.

MR. FAUST (by Telephone): When was that particular value, when did it occur?

DR. ZIEMER: Which one?

DR. BEHLING: Well, it was, I'll quote to you, and if you have a report -- am I talking to Leo here?

MR. FAUST (by Telephone): Yes.

DR. BEHLING: Leo, this was reported on page 119 of my report, and it identifies Attachment 4.5-2-A which is also part of the report on 120. And there it talks about the 1960 timeframe where they had a ratio of approximately 20-to-1, more than 20-to-1 ratio between deep dose and shallow dose.

MR. SHARFI: This is a ratio of deep-to-shallow not extremity.

DR. BEHLING: But it could be higher yet because obviously --

MR. SHARFI: I'm not disagreeing that you could have, we've seen very sizeable deep-to-shallow ratios. But it doesn't mean that the extremity-to-whole body ratio (inaudible). One's a material property, and one's a geometry issue. Two separate ratios that you're looking at.

DR. BEHLING: Well, I'm fully aware, but would you expect an even greater ratio that would have potentially separated the deep dose measured by the chest dosimeter to a wrist or femur dosimeter at the extremity? It's possible that even that ratio of 20.7-to-1 could have been even three times higher. It certainly couldn't be lower than --

DR. ZIEMER: Well, he was saying you might have both together.

MR. SHARFI: Yes. I think we're combining issues.

DR. BEHLING: But in this case you have at least the assurance that the timeframes were identical because the skin, the shallow dose and deep dose measured by one common dosimeter operate under the same timeframe. There's no discrepancy. But what it doesn't do is to account for the geometry difference which could enhance a 20-to-1 ratio to 40-to-1.

DR. ZIEMER: Well, you may have to look at the data to --

MR. SHARFI: Maybe I'm just not understanding your point going from deep to 40-to-1. What we care about is what the shallow level and whole body-to-shallow wrist ratio is and whether deep to -- we're not proposing to go from deep-to-shallow-to-wrist-to-hand. We're talking about going from shallow-to-wrist-to-hand.

DR. BEHLING: Well, as I said, there's reason to believe that the deep and shallow doses measured by the whole body dosimeter worn at the chest provides you with a clue. And it’s likely and it’s possible that the ratio that is observed in this case to be 20-to-1 could actually be significantly higher if the source term that this badge was measuring was right here and it’s measuring here to here, but my hands are here.

Which means that the skin dose to the extremities is going to be considerably higher than a skin dose monitored by my chest dosimeter. And so what was already measured as 20-to-1 ratio could easily be a factor of two, two times higher. That’s my point, depending on the geometry. I mean, look at it this way. If there’s a uranium block, and I’m wearing my TLD and that’s measuring shallow and deep dose, and it’s giving me a 20-to-1 ratio, but here are my hands that are not measured, they’re going to be exposed to a higher ratio yet.

DR. ZIEMER: I think Hans is saying it might be the combination of both of these.

DR. BEHLING: Yes.

DR. ZIEMER: The deep-to-shallow ratio in the badge may be different than the deep-to-shallow ratio as well as the --

DR. BEHLING: The extremity dose is usually defined by the seven milligram dose as a skin dose.

MR. HINNEFELD: I think they're all saying the same thing.

DR. WADE: It took us a long time to do it.

DR. BEHLING: You're the closer.

MR. HINNEFELD: No one else here seems to be.

DR. BEHLING: Anyway, I think we've exhausted that one. You say there are data that my be used in the event that such dose calculations would be done on the basis of time, and I trust that you will do that.

MR. CLAWSON: Well, I guess I need to just make sure that we're all on the right page on this because I think we're still talking 4.5.3 of the unmonitored shallow and deep dose. Do we have an action that we need to be able to look into? I understand that we do have data in the HIS-20.

MR. GRIFFON (by Telephone): Brad, I think there's an outstanding action still there. I mean --

DR. WADE: That study that was described. We're waiting for the results.

MR. CLAWSON: Okay.

DR. ZIEMER: Well, that covers 4-5.2 and 3?

DR. BEHLING: Well, the two are different.

DR. ZIEMER: I know they are.

DR. BEHLING: One is, as you mentioned the external beta component does not have to, is likely to be much larger than a skin contamination dose. Five point three really deals with the issue of residual skin and clothing contamination. And we said there are some study data to suggest that it's marginal at 1.5 reps per hour or something. On the other hand if it's a persistent one over a period of a year can still be a substantial dose.

MR. GRIFFON (by Telephone): Well, I mean I think for 4.5-2 if I'm understanding this right, NIOSH is saying it's a very limited number of people that would ever, you know, they'd run across this for, and they'd do like a case-by-case analysis if it came up as a best estimate issue. And I guess I would just maybe the action’s in SC&A’s court to say reviewing HIS-20. Do they believe there’s enough information there to do case-by-case all the way back to ’52?

DR. ZIEMER: Do you mean to get those ratios?

MR. GRIFFON (by Telephone): Well, to get an extremity dose, yeah, assuming, you know, if you only had whole body data as opposed to some people in there clearly have extremity data.

DR. ZIEMER: We have some ratios now, but are you asking what do you do if you don’t have a person with extremity numbers in their record?

MR. GRIFFON (by Telephone): Right.

DR. ZIEMER: I thought we were saying, Hans was suggesting we establish the ratios as a point during the time periods.

DR. BEHLING: The time from the data that existed.

DR. ZIEMER: And then NIOSH could do that if they got such a case, right?

MR. ROLFES: I think it’s important once again to reiterate that this is not an SEC issue, but it's related to how we would do dose reconstruction for a specific claim.

MR. GRIFFON (by Telephone): It's not an SEC issue. I mean, if you have to have the information to be able to do, reconstruct doses for all members of the class, right? So from that respect it is an SEC issue, isn't it?

MR. SHARFI: (Inaudible) is not (unintelligible).

DR. ZIEMER: You might say that louder, Mutty.

MR. GRIFFON (by Telephone): Yeah, I couldn't hear that.

MR. SHARFI: Mark, it's probably more of agreeing that the ratio that we choose is, or the approach we choose is conservative enough, not that it isn't really doable. There is data out there to say we can create one, it's whether or not it's conservative enough.

MR. GRIFFON (by Telephone): Yeah, I don't think there's any more action on NIOSH's behalf here. I'm glancing at this HIS-20 stuff myself, and there's some very interesting, I mean that one value I just saw with the 36,000 millirem extremity exposure has a deep dose for that year of 400 millirem. That’s a pretty high whole body-to-extremity ratio or pretty low, whatever, vice-versa. But I think SC&A should probably examine that whether, is there sufficient information there. If there is to do those ratios, then I would agree it becomes not an SEC issue but a DR issue.

DR. MAKHIJANI: Mark, earlier on we had talked about when wrist monitoring began, and I suggested maybe we’d look at that as one of the criteria of periods in the completeness investigation. You seemed to agree, but let me, I just want to be clear about that so we have a to-do list that’s good.

MS. BEHLING (by Telephone): This is Kathy Behling. Let me ask a question and maybe I missed something along the way. Is there any OTIBs or procedures or guidance available to the dose reconstructor if they were to come across a case like this to help to guide them as to what to do? Or is this something that is either in the Fernald site profile or will be put into the Fernald site profile?

MR. ROLFES: If there’s a case where this information becomes necessary, we can add some discussion of it in the site profile. But typically --

MS. BEHLING (by Telephone): I guess what I’m wondering from a dose reconstructor’s point of view if they come across a case like this, how will they know how to proceed? We’re sitting here talking about these studies that have been conducted and these ratios, but is that something that the dose reconstructor is going to have some guidance on or will he be familiar with these types of studies that have been done?

MR. ROLFES: I would say that the dose reconstructor would typically be familiar with such a situation.

MS. BEHLING (by Telephone): Okay.

MR. SHARFI: I mean, we do have like principal external dosimetrists. We can bring in additional help on a situation like this. I do know of one site -- at Rocky Flats actually did publish inside the technical basis document whole body-to-wrist ratios. No, they had hand-to-wrist ratios. So I mean, there are, we have looked at this issue before.

This isn't a, extremity doses are not a new issue for us, but there's not, like OTIB-0017 is we had talked about before that covers skin cancers doesn't specifically cover every extremity situation that you could run into or otherwise it would become a 300 page document. So I mean some of it is handled on a more case-by-case basis.

MS. BEHLING (by Telephone): Okay, and I understand that. I'm just thinking from a dose reconstructor's point of view and realizing that they have a lot of cases to deal with and there are a lot of guidance documents out there. And sometimes even if there's just a generic guidance document that points them in the right direction, and I realize this might be sort of a unique situation that they would come across. But I just question whether that dose reconstructor would know where to go from here.

DR. MAURO (by Telephone): Kathy, this is John. During the OTIB, the Procedure close out meeting last week, I guess a fundamental concept was discussed and that is whether we're talking about a particular OTIB or a procedure or a site profile very often there are unique circumstances that could arise.

And one of our commentaries, findings for any particular document we happened to look at -- it could have been OTIB-0017 -- is that particular document only covers a certain range of scenarios. And one could conceive of numerous other scenarios that could be presented to a dose reconstructor that he's going to have to struggle with. And the concern we expressed was that the guidance regarding these other scenarios was not explicitly provided in a given procedure.

But the position taken by NIOSH, and I think was accepted by everyone present during the Procedure review, is that you really have to look at the collective set of procedures. That is, for example, OTIB-0017 may not address certain scenarios, but the site profile and there are other guidance documents that collectively are in place to allow that that guidance does in fact exist.

And I guess I'm just passing this on.

This came out of the Procedures discussion where it was agreed that we will not as a review group expect every procedure to address every scenario and that credit could be taken for the fact that the knowledge base exists collectively within the site profiles and the suite of procedures that are available to the dose reconstructor. And that the dose reconstructor has access to that expertise through training and through the other resources available to him.

So I mean it was an important precedent that said that, well, every procedure does not have to address every possible circumstance that might arise. The degree to which that same philosophy is embraced here is important, but I did want to pass on that this did emerge during the Procedure meeting, and that's how it was resolved.

MS. BEHLING (by Telephone): And I do agree that I understand that you can't account for every circumstance that the dose reconstructor might encounter. I guess I'm not familiar personally with any extremity OTIBs or procedures or really even how much it’s ever discussed in the site profiles. I’m just personally not aware of any guidance document whatsoever.

DR. MAURO (by Telephone): I would agree that that is important because the philosophy was that the scenarios that we were discussing last week, you know, were tractable because the know-how was either provided in the site profile or in another procedure. And some of these procedures were referenced actually during the course of our conversations. But if the reality is that the particular scenario or issues that are being raised here, there really is no guidance on it, I think that that is an issue.

DR. BEHLING: On that issue let me also, I looked at some of the example dose reconstruction that was submitted on behalf of the Fernald, and I’m looking at external dash three. And I have to be a little cynical here in saying does this really answer our question with regard to skin contamination because in example external three you have a situation where you have a contamination report indicating a worker’s hand was contaminated with uranium for one hour prior to decontamination.

And you give a precise assessment as to what the DPM per 100 square centimeters was. And then you say that was there for exactly one hour, and then using VARSKIN, and it was 100 percent decontaminated and VARSKIN calculates 1 millirem. Well, that’s nice and fine. It would make for a nice RADCON 101 quiz exam.

But in truth you don’t have any data where people were monitored, and certainly we don’t have a clean-cut situation where we know precisely when that contamination took place, 100 percent successful decontamination (unintelligible) VARSKIN. As we said before we don’t have any reason to assume that people were ever monitored for skin contamination. So in terms of this particular example has very little value in terms of answering our question.

MR. ROLFES: I’ve read into the record a couple of times that individuals were in fact monitored for clothing contamination. That would be I think representative of potential skin contamination. I don't want to read it into the record, the results of the clothing surveys that I just read twice, but it was in fact monitored.

MR. ELLIOTT: I don't think this example he's talking about was designed to answer this particular issue we've been discussing.

MR. SHARFI: (Unintelligible) white paper on dust loading on skin for dose we've already talked about.

DR. ZIEMER: Yeah, we talked about it this morning.

MR. SHARFI: That might be more to the level of what Hans is referring to, long-term skin contamination, dust associated with that and he was doing a wet and dry filter paper. He was going to write a white paper. I think that will address more of what Hans is asking.

MR. MORRIS: Well, short term or long term it's still a dozen hours or ten dozen hours, it's still less than 100 millirem, you know. It's not much.

MR. GRIFFON (by Telephone): Can I propose an action for 4.5-2? I think we've discussed these two items long enough actually. I was going to say that what I have here is that SC&A will review data in HIS-20 to consider whether sufficient information is available to estimate extremity doses for individuals who did not have extremity data and who may have had significant extremity exposures. And then additionally, SC&A will consider whether this should be considered in their review of data completeness, in other words in that plan. And the outline of their plan, should we be looking at this issue? If those are okay we can move on to the next set of items.

FINDING 4.5-4

DR. BEHLING: Well, the next item is 4.5-4, and that's neutron dose.

DR. ZIEMER: Well, that was two that you did. What about three?

MR. GRIFFON (by Telephone): Well, three there's an outstanding item, action, Paul, that this white paper that Mutty just referred to I think, you know. So we'll just wait on that.

DR. BEHLING: Okay, the next one involves the issue of neutron doses. We do know that people were not monitored for neutron doses. The TBD identifies a neutron-photon ratio of 0.23. The last time we met we discussed the issue, and I think we resolved it because we did a calculation that came up with a higher value. But we did, in fact, for deriving such a higher value assume that you would have multiple drums, three high, three deep and three wide. And Stu brought our attention to the fact that based on criticality considerations that could never happen, and we accept that as an explanation.

Am I correct, Stu?

MR. ELLIOTT: That's what he said.

DR. MAURO (by Telephone): I remember that discussion also, and we agree.

MR. GRIFFON (by Telephone): I'm reading from the matrix though, Hans. It indicates here NIOSH will provide neutron survey data.

That was apparently posted, right, Mark?

MR. ROLFES: That's correct. We provided that prior to last meeting and discussed it in quite a bit of detail.

MR. CLAWSON: That's also where Stu brought up the configuration --

MR. GRIFFON (by Telephone): Right. So did SC&A review that survey data or it was just in this last meeting, this discussion?

MR. ROLFES: It was prior to the last meeting we had provided the neutron survey information.

MR. GRIFFON (by Telephone): I guess I'm asking SC&A if they looked at the survey data.

DR. BEHLING: Mark, I did not look at this. I haven't had time to look at that.

DR. MAURO (by Telephone): This is John.

MR. GRIFFON (by Telephone): I mean, it sounds like we're okay. I just want to be complete that we probably should look at that survey data.

DR. MAURO (by Telephone): There were two aspects of that that we looked at, the person that did that is Bob Anigstein. He's not on the line. One aspect was the data itself and what it reported. And I recall that it did support this ratio. But Bob had pointed out also that, yes, you're correct. You're not going to have enriched uranium stacked up that way. But he did come up with higher ratios than this for stacked up natural, you know, unenriched uranium as being higher than that.

So the outcome of -- this is my recollection. I certainly could confirm this by checking with Bob -- was that, yes, the data did, in fact, the actual measurements taken, did, in fact, support the ratio proposed here. However, there were scenarios where you did have stored material that was not enriched, but it was a large pile of material where the neutron-to-photon ratio could be greater than was here, but I don't remember how much greater.

MR. FAUST (by Telephone): This is Leo. That report does not support that conclusion. It's just the opposite. It's much less than what we originally used in the TBD. As a matter of fact they used some 12,000, well, the actual number is 12,773 containers located in Building 4B with enrichments between, somewhere between 0.711 and two percent. And the maximum neutron dose rate measured was 0.089. And I'm trying to find the gamma dose rate.

MR. ROLFES: The MP ratio, Leo, that you're referring to. The MP ratio was less than 0.1-to-1.

MR. FAUST (by Telephone): Right, and the ratio used in the TBD was 0.23.

MR. ROLFES: Correct, so it is a bit higher.

MR. FAUST (by Telephone): It is higher. I mean it's much more claimant favorable than the actual measurements would conclude.

MR. ROLFES: Correct.

MR. GRIFFON (by Telephone): And I think John was referencing something that SC&A team did as far as --

DR. MAURO (by Telephone): Yeah, as I mentioned we looked at the actual measured values that you're making reference to. And the empirical data certainly support the conclusion that the 0.23 is, in fact, claimant favorable. But I recall that we did just a theoretical calculation. We ran I believe an MCNP calculation to see, okay, if you had stored uranium -- well, we did it for both natural and also two percent, I believe, enriched.

So we had two cases, and it was a white paper that we submitted. It's in the record somewhere. And it was pointed out at the time we discussed this white paper where SC&A did these calculations, we withdrew the enriched one because the argument was made that you would not have for criticality reasons that kind of configuration. But I believe our calculations for the unenriched uranium in storage, and I think they were working with uranium tetrafluoride.

MR. FAUST (by Telephone): That's what this was, too.

DR. MAURO (by Telephone): Yeah, and the calculations -- of course, I could confirm this. It's on the record. I don't have it in front of me, but I seem to recall that for that scenario the ratio was somewhat higher than 0.23. But I will need to go back and check that.

Now the fact is though that's a theoretical calculation. If you have empirical data that you feel represents reality that demonstrates that 0.23 is claimant favorable, then you've got to give the amount of importance you give to this theoretical calculation. You know, that's certainly --

MR. GRIFFON (by Telephone): Well, that's what I'm suggesting, John. Why doesn't SC&A just review the neutron survey data provided by NIOSH along with revisiting your own white paper and come back to us.

DR. MAURO (by Telephone): Fair enough, and we probably could take care of that fairly expeditiously because we have looked at. Okay, we'll take that as an action item.

DR. BEHLING: John, just for your information, you may be referring to a calculation I asked Bob to do that looked at for instance a pile of UF-4 salt just laying there in a conical pile.

DR. MAURO (by Telephone): Yes, that's correct. That's exactly what he did.

DR. BEHLING: So it's something that is realistically something that you might have encountered. And if I recall, and I don't remember offhand the precise numbers, but they're probably not significantly higher than 0.23.

DR. MAURO (by Telephone): And we can certainly confirm that. Right now we're speculating. It’s in the record. We have the information. We’ll just have to get back to you.

MR. GRIFFON (by Telephone): That’s fine. 4.5-5, right?

MR. CLAWSON: Yes.

FINDING 4.5-5

DR. BEHLING: Let’s see here. Oh, the only other thing I want to go back and that is on page 127 under the issue of neutron doses there was one reference that I saw, and I quoted this, that identified a neutron source. And I didn’t have a clue what to do with that. Apparently a neutron generator or neutron source that they talk about in this one, and it’s Attachment 4.5-4B that I’m quoting from.

And they talk about completion of the detailed survey of the neutron generator is needed for performing adequately Health and Safety operational procedures. And I don’t know what that’s a reference to, a neutron generator. Because no one has made mention of this before, and I don’t have any clue as to what that neutron generator involved, and what potential exposures may have resulted in the use of that neutron generator. I don't know if there's any comment here from anyone here that can shed light on that.

DR. ZIEMER: You also mention a Cockcroft-Walton machine and you use that with a deep (inaudible) reaction you get 14 meV neutrons. I'm wondering if that's what they had.

DR. BEHLING: I don't know. I'm just, you know, I came across a memo that identifies the assistance of a neutron generator, and I saw no other reference.

DR. ZIEMER: Were they doing activation work?

MR. ROLFES: Yes, there was a Cockcroft-Walton neutron generator at the site that was part of the analytical laboratory. It was used for the determination of mass quantities of uranium.

DR. BEHLING: And there's also a reference on the next page, 128, to a Californium-252 source as a potential source --

DR. ZIEMER: Apparently also for activation.

MR. ROLFES: I'm not aware of that source. I'm not aware of the californium source, but I am aware of the Cockcroft-Walton neutron generator.

MR. FAUST (by Telephone): I never, in anything that I've seen to date anyway, I haven't seen any reference to a californium source. That doesn't say it doesn't exist, but I haven't found it.

MR. CLAWSON: Well, now this was for checking for the -- I know we used to activate and find out how much unused uranium we had.

MR. ROLFES: It could have been the determination of mass, uranium mass.

MR. CLAWSON: Okay.

DR. BEHLING: Is there anything that needs to be looked at with regards to the use of these sources and --

MR. GRIFFON (by Telephone): I mean, I was just going to, in your document, Hans, on page 133, it does describe the californium source, 153 micrograms.

DR. BEHLING: Yeah, yeah.

MR. GRIFFON (by Telephone): Okay.

DR. BEHLING: Obviously we have no data that suggests people who used these facilities were ever monitored for neutrons.

MR. GRIFFON (by Telephone): This neutron activation facility, is there a building number associated with that or is it within one of the plants?

MR. ROLFES: I'd have to take a look back into my notes, but it was part of the analytical laboratory.

DR. ZIEMER: It does talk about a tritium target so they must be using a DT reaction for some neutrons. So it would be a little surprising if they didn't have some kind of neutron monitoring.

MR. ROLFES: Yeah, there are neutron monitoring results in HIS-20. I haven't looked to see if we have correlated that. There is neutron monitoring there. It is maybe one non-positive result in HIS-20. I don't recall what time period it was.

MR. FAUST (by Telephone): It's 20 millirem.

MR. ROLFES: Twenty millirem, sure. But there was an industrial hygiene and radiation survey. Off the top of my head I don't recall when it was, but there was a survey of the area of the analytical laboratory where they kept the check sources. And there wasn't very much detail in there, but they didn't note any problems associated with the storage of sources or use of sources.

MR. FAUST (by Telephone): They also, Mark, they also had a Snoopy monitoring instrument because they used one in that neutron survey study.

MR. ROLFES: Correct, that's true.

DR. ZIEMER: Well, Hans, were you asking about whether they had neutron monitoring?

DR. BEHLING: Yes.

DR. ZIEMER: You're saying yes.

MR. FAUST (by Telephone): No, they did not. As far as --

DR. ZIEMER: They did not?

MR. FAUST (by Telephone): -- personnel monitoring is concerned they did not.

DR. ZIEMER: Well, Mark was saying --

I thought you said --

MR. ROLFES: I said we do have neutron monitoring results in HIS-20.

MR. FAUST (by Telephone): We have zeros with one positive.

MR. ROLFES: Well, that would be indicative --

MR. FAUST (by Telephone): But that was late in the game, guys.

MR. ROLFES: Okay, okay.

MR. FAUST (by Telephone): It was post-TLD so it's 1977 or '78 forward.

DR. BEHLING: These were written, these memos come from --

MR. CHEW: This is '68.

DR. BEHLING: Well, '64.

MR. CHEW: 'Sixty-four.

DR. BEHLING: Nineteen sixty-four was the first report, and the second one is dated --

MR. CHEW: 'Seventy-two.

DR. BEHLING: -- dated '74.

MR. FAUST (by Telephone): It doesn't matter. They did not use NTA film at Fernald.

MR. CHEW: (Inaudible) operational TLD?

MR. FAUST (by Telephone): The original, the TLD system went into place either late '77 or '78. And it was DOELAP-certified by --

MR. CHEW: Here's what I'm getting, is that you may not have used NTA film, but you used a combination of TLDs to monitor neutrons. Is that what you're saying, Leo?

MR. FAUST (by Telephone): Yes, post-19 --

MR. CHEW: (Unintelligible) neutron

monitoring.

MR. FAUST (by Telephone): -- at the time that they put it into place. All I'm saying is that they had a Snoopy, which is a survey instrument. I don't know when they got it, or I don't know if they ever used it, but they did use it during this documentation that was or this document that were just referenced. And that was done, and I think the actual measurements were done in '98 to tell you the truth about it. So they had that instrument at that time anyway. When they got it, I don't know.

DR. ZIEMER: Well, Snoopies weren't made that late. Snoopies were made by Tracer Labs.

MR. FAUST (by Telephone): Yeah, we had one.

DR. ZIEMER: And Tracer Lab, they didn't exist after what, 1980?

MR. FAUST (by Telephone): Yeah, so they could have bought it prior to the time they went --

DR. ZIEMER: Well, that's what I'm saying. They wouldn't have bought it as late as '98 because --

MR. FAUST (by Telephone): Oh, no, no, huh-uh.

DR. ZIEMER: Or they could buy it on E-bay I guess.

MR. FAUST (by Telephone): Cheaper. No, I don't know when they bought it, but I do know they used it for this neutron study that they did.

DR. BEHLING: But there was no personnel monitoring. That's the issue.

MR. FAUST (by Telephone): They did not do any for neutrons. They didn't have to, and they didn't need to.

MR. GRIFFON (by Telephone): I guess, I mean, I don't know if it's hanging out there. Maybe as an action item we can just say NIOSH will determine whether other neutron exposure potentials existed. And, if so, whether the current approach would be bounding. In other words if these laboratory workers had a potential, albeit small, potential neutron exposure, does this 0.23 ratio, could that still be used and be bounding?

MR. ROLFES: Well, this is a separate, sort of a separate issue because we're talking about rather than a continuous source of potential neutrons, a very intermittent source that would only be a source of neutron exposure when electricity was applied to it. It was not a continuous source of neutron exposures.

MR. MORRIS: That's not true for the californium.

MR. ROLFES: Well, true, but in speaking for the Cockcroft-Walton neutron generator.

MR. GRIFFON (by Telephone): Right, right.

DR. ZIEMER: The californium source was only there for a few months if this is correct here. Well, that was limited. Well, we've got to know, I guess, but that's what it says. It was planned for four to five months.

MR. CHEW: You can read in there, I'm not sure they actually even used it.

MR. CLAWSON: I guess that brings up a question, you know, and this is where we need to come to a closure on it, too, is what went on with it and were the people monitored or not.

MR. MORRIS: That's a fair question, and we should interview the people that we --

DR. ZIEMER: Actually, there's probably a, this would be a very small number of people because Cockcroft-Walton, you're going to have an operator, and that's probably it.

MR. MORRIS: Right, and we have contacts with analytical laboratory people who could make statements about this if we specifically ask, I think.

MR. CHEW: I think we should do a follow up on it.

MR. ROLFES: We'll take another look.

MR. GRIFFON (by Telephone): Yeah, that's solved.

MR. ROLFES: We've asked around and, you know, I've asked around casually with individuals, not necessarily the same individuals, but --

DR. ZIEMER: Now this is not part of the matrix, right?

MR. CLAWSON: Now it is.

MR. CHEW: The neutron monitoring.

MR. GRIFFON (by Telephone): I'm just putting it under 4.5-4.

DR. BEHLING: Yeah, the second bullet.

MR. GRIFFON (by Telephone): Just looking at other neutron exposure potentials.

DR. BEHLING: No, it’s probably not in the matrix, Paul.

MR. GRIFFON (by Telephone): Yeah, it’s not really in the matrix, but it’s in the report, right?

MR. CLAWSON: Right.

DR. BEHLING: It’s five o’clock, and we’re down to the last finding, 4.5-5, unmonitored female workers. And the TBD fully acknowledges two time periods during which females were not monitored for external, but then the question comes into play as to whether or not 500 millirem may be a bounding default value.

And that also comes into question based on the fact that these women in the laundry facility were subject to exposures that come from dust collector bags and in today’s conversation we heard about thousands of gloves that were subject to laundering which were heavily contaminated in some instances.

And I guess that the discussion that I wanted to stimulate here is the external exposure of 500 millirem a year, a bounding value. And then does that also address issues that may come to, or that may involve internal exposures to whatever these dust collectors and gloves may have contained in the way of uranium, thorium and other radioactive materials.

Here we see in one of the attachments on page 135 I highlighted a statement here that some of these bags that were subject to laundering were reading up to five millirep per hour after cleaning. The question is, oh, yeah, 30 millirep before cleaning and five millirem after cleaning. And to what extent this would contribute a dose that might significantly exceed the default value of 500. Now I assume the millirep here is a contact reading involving photon and beta dose. I'm not sure.

MR. CHEW: Is that a question?

DR. BEHLING: Yeah, the question is is 500 millirem a year a default value that was a bounding value for women who were not monitored?

MR. ROLFES: I don't see the question how it relates to the laundry studies of the dust collector bags which were collecting uranium materials and unmonitored women that didn't enter the process area.

DR. BEHLING: Wait a minute. When you're in receipt of huge quantities of dust collector bags, they become your source of radiation exposure as was suggested here by these measurements. So they didn't enter a process area, but they were certainly exposed to external and potentially internal exposures.

MR. ROLFES: I'm still not clear where it's saying that women received, unmonitored women received dust collector bags.

DR. BEHLING: 4.5-5, laundry studies of dust collectors. I mean, who else would have been the recipients if not the women who ran the laundry?

MR. CHEW: You'd confine it to women who ran the laundry, and they were not monitored.

MR. ROLFES: So this is implied that the women were the individuals that were doing the laundry of dust collector bags.

MR. CHEW: And they were not monitored.

MR. ROLFES: And they were not monitored. So I honestly don't know if it was the women or men or both that were doing laundry studies. I would have to take a look at an individual's records who actually did the laundry to determine whether the individual was monitored or not monitored. I think it's a leap of faith to say only the women were doing the laundry and were not monitored.

DR. MAKHIJANI: I think the point is not that only women were doing laundry. The point is that women who were doing laundry, apart from any men who might have done it. We know according to the TBD no women were monitored in two periods at Fernald.

MR. CHEW: That's correct.

DR. MAKHIJANI: Unless you have some, and we know that women were doing laundry. Maybe not exclusively --

DR. ZIEMER: Do we know that?

DR. MAKHIJANI: There are pictures of it.

MR. FAUST (by Telephone): But it's got to be at the right time.

DR. MAKHIJANI: At the right time. We don't have pictures of it from every period, but we do have a picture of a woman in the laundry taken in the '80s. This would lead presumably to interviews. So I think there's a kind of, the logical set up of the question should be correct is all I'm saying. Some women did laundry. We have to fix the period and perhaps amenable to an interview. But we know that they were not monitored in two different periods of time.

MR. FAUST (by Telephone): Yeah, but the periods of time that they weren't monitored was males only, 1951 through 1960 or to '60. And again, from 1969 to 1978.

DR. MAKHIJANI: Correct.

MR. CHEW: And after that women were monitored?

MR. FAUST (by Telephone): Well, in between those times, in between those two times, yes, they were. And after '78 they were.

DR. ZIEMER: Why did they add them and then delete them again? Do we know?

MR. FAUST (by Telephone): I think it had, well, the thing that I read about this whole thing, the first time they didn't have to based on the rules and regulations at the time. The second time they did it because of cost and the fact that they concluded they didn’t have to. Anybody that received the less than I think ten percent of the allowable limit at the time didn’t have to be monitored.

MR. CHEW: Your reference in the document is from 1958, and I think you mentioned that there was a picture of women that was in 1980.

DR. MAKHIJANI: In the ‘80s.

MR. FAUST (by Telephone): Well, in 1980 they were monitored.

DR. MAKHIJANI: No, no, that’s what I’m saying. If we have a picture of it, women were doing the laundry but not from the right period.

MR. CHEW: (Unintelligible).

MR. ROLFES: Now, Leo, during 1958 would women have been monitored?

MR. FAUST (by Telephone): Nineteen fifty-eight? No.

MR. ROLFES: Okay. So if we have indication that a woman was working with laundry, the next question is could she have received in excess, working with laundry that was contaminated, could she have received in excess of the default that we have in our technical basis document? And I would like to read into the record again that we do not feel that this is an SEC issue.

Doses to female workers who were not monitored during two operating periods can be reconstructed by at least three methods. They are: if the worker in question is doing the same or very similar jobs during periods when she is monitored, that dose could be used to adjust the missing dose when she wasn't monitored.

The second methodology is workers who were doing the same job and were monitored at the time the female wasn't, could have an equivalent dose assigned to the unmonitored worker. And three, assignment of the missed dose as stated in the TBD, Volume 6, of 500 millirem per year for the missing time periods is known to be very claimant favorable.

So there are three different methodologies that are proposed, and therefore, we do not believe this is an SEC issue, more of a dose reconstruction issue that depends upon the specifics of the case.

MR. CLAWSON: Mark, in the back of this you kind of had some generic action items, and you had a Gilbert report.

(no response)

MR. CLAWSON: Did that get put onto the O drive? Did we ever get a hold of that Gilbert report?

MR. ROLFES: Oh, I'm sorry. I didn't know if you were talking to Mark Griffon or --

MR. CLAWSON: Oh, you, I'm sorry.

MR. ROLFES: Yes, we did have, the Gilbert report was made available to the Advisory Board members, and I did put that onto the O drive. Let's see. We reviewed the Gilbert report, the Tiger Team report and the Westinghouse Transition report to assure, we did review those, and we did not feel that there was any impact on our ability to do a dose reconstruction. Those reports are available also for the Advisory Board working group.

MR. CLAWSON: They're on the O drive?

MR. ROLFES: Correct.

MR. GRIFFON (by Telephone): And, Mark, did they, parenthetically there it said that this includes reviewing the data integrity but I forget exactly what I meant. Did this, reviewing those reports, did it shed any light on any data integrity issues?

MR. ROLFES: Gene, are you still with us? Gene Potter?

MR. POTTER (by Telephone): Yes, I am.

MR. ROLFES: Do you recall if there were any discussions? I believe it was you that had reviewed the mentioned reports in addition to some other individuals. Do you recall any data integrity issues from the various reports that we were asked to review?

MR. POTTER (by Telephone): No, I looked at the Gilbert report initially specifically for issues with the mobile in vivo counter and the qualifications of personnel. I believe the issue had supposedly been raised in a report. The Gilbert report did not contain any such information. That’s what I was looking for specifically, and I’m not recalling any other issues. I also looked at a series of other reports that are on that SRDB trying to find the appropriate report that may have raised issues with the mobile in vivo counter, and I was unable to find any information on that.

MR. ROLFES: Okay, so, Mark, it doesn’t appear that there were any data integrity issues within the reports that were reviewed.

MR. GRIFFON (by Telephone): I think as a follow up do we need SC&A, Brad, to look at these reports as well? I don't know if we've officially asked them to look at these.

MR. CLAWSON: No, I don't think we have. I think we need to follow up with that though.

MS. BALDRIDGE: Brad, my recollection may not be accurate, but I thought when you suggested getting the Gilbert report, it was to review it for the environmental impact, and that it really didn't have anything to do with the (inaudible).

MR. CLAWSON: Yeah, I brought that up, too, because I wanted to correlate it with some of the outside environmental ones that were brought up, so forth.

MS. BALDRIDGE: And I think there are multiple reports; they may not have gotten a hold of the right reports.

MR. CLAWSON: Well, we'll, I guess we could have SC&A take an action item to be able to look at the Gilbert reports.

And I guess what I'd throw out there

is, I was looking at more from the data integrity facts on that.

DR. MAKHIJANI: Brad, what did you mean by data integrity in relation to these reports?

MR. CLAWSON: Well, because there was a question of a Tiger Team report that went in there and really slammed Fernald especially from their Health Physics program, their air sampling data and so forth. And this was an internal one that was done. I just wanted to make sure that we weren't missing anything on that.

DR. BEHLING: What time period was that?

MR. CLAWSON: Nineteen eighty-five time era.

DR. ZIEMER: For which report?

MR. CLAWSON: My understanding was, I just understood it as a Tiger Team.

DR. ZIEMER: Tiger Teams didn't exist in '85.

MR. CHEW: Chuck Gilbert from EH --

DR. ZIEMER: Chuck Gilbert? Not Mark Gilbert?

MR. CHEW: No, Chuck Gilbert from the report, and what it was that there was a transition from National Lead Ohio to

Westinghouse.

MR. RICH: That's just prior to.

MR. CHEW: Yes, just prior to. And it was not part of the Tiger Team, Brad. It was right before.

MR. CLAWSON: Well, yeah, I guess I used the terminology that I'm using --

MR. CHEW: No, no, but it was a Tiger Team-like report.

MR. CLAWSON: Right, and I wanted to make sure that we had, and I think that's why we hit so many of them, Tiger Team, Gilbert report and Westinghouse transition was because we wanted to be able to review that.

MR. CHEW: We also did look at the Tiger Team.

Leo, I think you did that, right?

MR. FAUST (by Telephone): Yeah, I reviewed the Tiger Team report of Health Physics aspects of it. They were rather critical of the internal dosimetry program. They were complimentary of the external program for the most part, but the internal program they were very critical over. They were super critical over the lack of completed SARs. But there was nothing in it that I could find that would impact dose reconstructions.

MR. CHEW: Can you be more specific about the issue that they had with the internal program, Leo, any more specificity?

MR. FAUST (by Telephone): Well, I think that the time thing that they had with it was the fact that they didn't do enough bioassays nor did they do enough full body counting. That's what I recall anyway.

MR. CLAWSON: Well, it's well past five o'clock, which I apologize. I think I still would like SC&A to be able to look into that and just make sure that we're not missing anything. I'd like to look at it from the data integrity standpoint of it.

MR. CHEW: They tell me it was very difficult to locate, believe it or not, the Fernald Tiger Team report. Headquarters didn't have it. I think it was very, I mean, I'm just going to share that with SC&A not to look too hard. We'll get it to you because you'll spend another man-year looking for it, Hans.

DR. WADE: Brad, you did have this issue of

SC&A was going to develop a plan to develop a plan in terms of data completeness. Do you want to give some thought to timing on that now?

MR. CLAWSON: Well, I didn't know if that had time to be able to really sit down.

DR. MAKHIJANI: I tried to call John but couldn't reach him.

DR. WADE: Well, maybe then when SC&A gets its head together let Brad know, then you can decide when you want to reconvene the work group.

MR. CLAWSON: Maybe what we may be able to do is just have a conference call or something like that and be able to discuss, we'll evaluate that -- wait with everybody on the Board when we get that information when I get a time.

DR. WADE: Well, if SC&A gives you a sense, and even on the call on the 27th you could in your time use it to set a time for a work group call.

DR. MAKHIJANI: I guess we can do that. We can give a sense of when we'll produce this memo, and it shouldn't take too long.

MR. GRIFFON (by Telephone): Hey, Brad?

MR. CLAWSON: Yes, Mark.

MR. GRIFFON (by Telephone): Since it's so late I won't go back and read through all these actions, but I will, I'll revise the matrix based on what Mark had sent out recently. I'll add in the new actions and turn it around in a few days just so it's fresh on our minds still and circulate it if that's okay.

MR. CLAWSON: Okay, I appreciate that. I appreciate everybody's continued support on this.

DR. WADE: Well, thank you very much, all of you, for your work. It's a long day, but it needs to be done to do justice to the people whose lives we're reviewing, so thank you very much.

MR. ROLFES: Do we need to schedule another meeting before we leave here?

DR. WADE: Well, I'm not going to be able to do that. SC&A's going to get back to Mark with its timeline on the plan to develop a plan. And then maybe on the call on the 27th, we'll look at scheduling a meeting.

MR. SCHOFIELD: You're the only one's going to be back here. The rest of us get to leave.

MR. ROLFES: I'll be here.

MR. CLAWSON: With that said, we'll say goodbye and thank you.

(Whereupon, the work group meeting adjourned at 5:30 p.m.)

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I, Steven Ray Green, Certified Merit Court Reporter, do hereby certify that I reported the above and foregoing on the day of November 13, 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 3rd day of February, 2008.

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