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 August 8, 2007.

STEVEN RAY GREEN AND ASSOCIATES
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C O N T E N T S

August 8, 2007

TRANSCRIPT LEGEND

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-- "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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ELLIOTT, LARRY, NIOSH
FAUST, LEO
HILL, STEPHEN, CONG. CHABOT
HINNEFELD, STUART, NIOSH
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MAURO, JOHN, SC&A
MORRIS, ROBERT, CAI
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SHARFI, MUTTY, ORAU

P R O C E E D I N G S

(8:00 a.m.)

WELCOME AND OPENING COMMENTS

DR. LEWIS WADE, DFO

DR. WADE: Hello, this is the work group conference room. This is Lew Wade. Is there anyone out there on the telephone this morning?

(no response)

DR. WADE: Anyone on the telephone?

(no response)

DR. WADE: It could be that there'll be no one but us. Was John Mauro expected?

DR. BEHLING: No, he's not, but he did say he will be on the telephone. He's probably going to join us a little later.

DR. WADE: Good morning all. This is the work group conference room, Lew Wade. John Mauro, are you out there?

DR. MAURO (by Telephone): Yes, I am, Lew.

DR. WADE: Can you hear me?

DR. MAURO (by Telephone): I hear you perfectly well.

DR. WADE: We're going to do some

introductions and maybe you could be our sound monitor. If anyone's introduction is not clear to you just sort of shout out, and we'll make the necessary physical adjustments.

DR. MAURO (by Telephone): I'll do that.

DR. WADE: Thank you.

We're just waiting for the court reporter to make several final adjustments, and then we will be ready to begin. It is hard to get good help anymore so we do with what we have.

This is Lew Wade, and I have the privilege of serving as the designated federal official for the Advisory Board. And this is a meeting of a work group of the Advisory Board. This particular work group is looking at the Fernald site profile and SEC petition, so it's looking at both.

That work group is chaired by Brad Clawson, members Griffon, Ziemer, Presley and Schofield. All of the members of the work group are here around the table.

Let me first ask if there are any other Board members who are on the telephone. Are there other Board members who are on the telephone with us this morning?

(no response)

DR. WADE: Any other Board members?

(no response)

DR. WADE: Okay, well, we have five Board members present. That's not a quorum, and that's appropriate for a work group meeting. If we did have a quorum of the Board, we'd have to make adjustments. So we're in good shape on that stead.

What I'd like to do now is make some introductions. First, we're honored to have in the room with us Stephen Hill who represents Congressman Chabot from the First District of Ohio. Stephen, thank you for being here, and we appreciate it. And if you have any questions or things to say during the proceedings, please let us know. We're very much honored to have a representative of a congressman with us.

What I do now is go through the introductions. The court reporter is up and functioning, and everything is working correctly, right? We're going to do a little bit of adjustment for the court reporter.

Okay, now what I'll do is go around the table, and ask everyone here in the room to introduce themselves. Then we'll go on the telephone and we'll ask for members of the NIOSH and ORAU teams to introduce themselves, then members of the SC&A team to introduce themselves, other federal employees who are on the phone, members of Congress or their representatives, petitioners, claimants, people who are expert with regard to the site, and then any others who would like to identify themselves.

When Board members or NIOSH/ORAU team members or SC&A team members identify themselves, I would like you to very briefly address whether or not you have any conflict of interest relative to the Fernald site. Once we complete the introductions, we'll talk a little bit about telephone courtesy, and then I'll turn it over to the chairman who will begin the proceedings.

Again, this is Lew Wade. I serve the Advisory Board. I am also an employee of NIOSH, and I have no conflicts relative to the Fernald site.

MR. PRESLEY: Robert Presley, working group member, and I have conflicts with the Fernald site.

DR. ZIEMER: I'm Paul Ziemer, working group member, and I have no conflicts.

MS. KENT: I'm Karen Kent, part of the ORAU dose reconstruction team, all with no conflicts.

MR. SHARFI: I'm Mutty Sharfi for the ORAU team, no personal conflicts.

MS. HOFF: I'm Jennifer Hoff. I'm with the ORAU team, and I have no conflicts.

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

MR. ROLFES: Mark Rolfes, NIOSH Health Physicist, no conflict of interest.

MR. CHEW: Mel Chew, the ORAU team. I have no conflict with Fernald.

MS. JESSEN: Karin Jessen, O-R-A-U, no conflicts.

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

MR. GRIFFON: Mark Griffon, work group member, no conflict.

MR. BEATTY: Ray Beatty, former worker.

MR. CLAWSON: Brad Clawson, working chair for Fernald, no conflict.

MR. HINNIFELD: I'm Stu Hinnifeld from the NIOSH staff, and I do have a conflict or potential bias associated with Fernald that I worked in the Radiation Detection Department there.

DR. MAKHIJANI: Arjun Makhijani, I declare I have a conflict that my work was cited in the SEC petition.

MR. SCHOFIELD: Phil Schofield, working group member, no conflicts.

MS. HOWELL: Emily Howell, HHS, no conflicts.

DR. WADE: Okay, let's go out then to those on the telephone, and I'll start with members of the NIOSH or ORAU team. Anyone representing NIOSH or the ORAU team on the telephone?

(no response)

DR. WADE: SC&A team?

DR. MAURO (by Telephone): John Mauro, SC&A, no conflict.

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

(no response)

DR. WADE: Other federal employees who are participating by virtue of their employment?

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

DR. WADE: Welcome, Jeff.

Other federal employees?

(no response)

DR. WADE: Any other members of Congress or their representatives?

(no response)

DR. WADE: Again, we're honored to have Stephen Hill with us here in the room.

Petitioners, claimants, those familiar with the Fernald site?

(no response)

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

(no response)

DR. WADE: I would say that the way the working groups have functioned if there are people with site expertise in the room, workers or representatives, you should feel free to comment as you would like. I think it's important that as much knowledge be brought to the table as possible so consider yourself a part of these deliberations.

By way of phone etiquette I'd each of you connected by the phone to remember that noise from your site can be very distracting so if you're not speaking or are about the speak, mute your instrument if at all possible. When you do speak, speak into a handset. Don't use a speaker phone. And be extremely mindful of background noise so that you don't disrupt the ability of this group to use its time productively.

We have one new addition to the table. If you could.

MR. RICH: Bryce Rich with CAI.

DR. WADE: And do you have a conflict relative to the Fernald site, Bryce?

MR. RICH: I do not.

DR. MAURO (by Telephone): Lew, this is John Mauro. The last person that introduced himself I could not quite hear.

MR. RICH: Bryce Rich.

DR. WADE: Did you hear Bryce?

DR. MAURO (by Telephone): Yes, I did, thank you. Hi, Bryce.

DR. WADE: Bryce had just walked into the room and hadn't made his way fully to the table yet. But please, John, again, do us the service if we tend to fade, let us know. We would appreciate that.

No more introductory materials. Brad, it's yours.

INTRODUCTION BY CHAIR

MR. CLAWSON: I appreciate that for the fine introductions. First of all, this is my first working group so please forgive me on a lot of this stuff. I don't know a lot of the etiquettes. But one of the things I wanted to start out with is I wanted to ask that Hans, being the SC&A person, have we looked at the petition that has been filed, and have we covered all of the petitioner's issues with Fernald and with the paper that has been written? Have we covered all those?

DR. BEHLING: Yeah, if you read the report, there is a section that is dedicated to addressing issues raised by the petitioner. And I believe I have addressed in different ways all of the issues that were raised by the petitioner.

MR. CLAWSON: Okay, I appreciate that.

Who wrote this matrix for NIOSH?

MR. ROLFES: That would be myself as well as (inaudible) associates.

MR. CLAWSON: Okay, I wanted to get that. I did notice one thing right off the bat. I thought at this board, we were all to look to see if these were SEC issues. I didn't realize that you guys would make these assumptions right off the front of this. But it was kind of interesting for me to be able to see that. But I guess what I'd like to be able to see right now is if you could kind of give us an overview of what Fernald did and kind of the timeframe that we are looking at if you could.

DR. WADE: Before you begin, is there anyone who needs a copy of the matrix? A hard copy of the matrix?

DR. ZIEMER: What is the date on this matrix? We've gotten several versions.

MR. ROLFES: August 3rd. Yes, this is just the matrix so this shouldn't have, this should be the one and only that was sent out to the Advisory Board.

MR. PRESLEY: I got mine on the sixth, August 6^{th} is what date's on mine, August 6^{th}, at 11:18 p.m.

MR. ROLFES: Yeah, the paper is dated the third, and the e-mail was sent out on the sixth.

DR. WADE: Just to follow up on a comment Brad made, in the Draft NIOSH Response column, NIOSH will occasionally say this is not an SEC issue. That's NIOSH's opinion. Board can do whatever it wishes with it obviously.

DR. MAKHIJANI: Mark, where's the date on the paper?

MR. ROLFES: On the bottom left-hand corner. I don't have a hard copy in front of me at the moment, but --

DR. MAKHIJANI: My copy doesn't have a date.

MR. ROLFES: Right here it says Matrix from Fernald SEC Issues, August 3^{rd}.

DR. MAKHIJANI: Can I have another hard copy because I'm not sure I have the latest version.

MR. ROLFES: I just gave my last one out. I apologize.

So there anything else before we begin, Brad, that you'd like to --

MR. CLAWSON: No, not at this time.

FERNALD OVERVIEW

MR. ROLFES: Well, I guess I will give you a brief overview of what Fernald did, and then give you an update on our changes to the technical basis document that we use for dose reconstruction.

To be brief if you remember in the very beginning, I believe this document, the initial technical basis document, was dated from late 2003 or early 2004. And we had a big push to get some answers out to claimants in a timely manner. We wanted to get a technical basis document that we could use for making scientific decisions with claimant favorability incorporated.

So we took as much information as we had at the time to assemble this technical basis document to cover as much as we could in the limited amount of time that we had. And so we realized that we didn't incorporate everything at that time and these documents are living documents, and when we received public comments, we update the documents as well as when we receive additional reports and information.

So we have begun working on the technical basis document as a result of the SEC evaluation and SEC evaluation process, I guess. I'd like to give you a brief update on the changes that are in progress to the Fernald technical basis document used for dose reconstructions.

One of the first issues that we looked at was the ingot rider. We received a picture of an individual working, I believe, in Plant 9 who was straddling a large uranium ingot. We realized that there was a possibility that some stampers experienced unmeasured full body and skin doses while straddling ingots during the stamping operations. So we took evaluation time, motion and frequency based on worker interviews, and we performed calculations to estimate dose rates that the worker was exposed to.

We also took a look at neutron-to-photon ratios. We know that neutron dosimetry was not implemented due to the near absence of neutrons at Fernald. We had results of a neutron dose rate survey that were conducted in Building 4B where there were over 12,000 drums of uranium hexafluoride present. Two percent of the drums contained enriched material.

Now, keep in mind natural uranium is approximately .71 weight percent. So two percent of the drums were enriched to 1.25 percent up to two percent. Twenty-three percent of the drums were enriched from natural up to 1.25 percent. And 75 percent of the drums contained natural or depleted uranium. The highest neutron dose rate that we observed was .089 millirem per hour. And it gave a calculated neutron-to-photon ration of less than 0.1.

What we have in the technical basis document at this time is a neutron-to-photon ratio of 0.23, and so this report confirmed that what we have is claimant favorable for dose reconstructions.

DR. MAKHIJANI: Mark, where was this dose rate measured?

MR. ROLFES: Where was the dose rate measured? It was in Warehouse 4B.

DR. ZIEMER: Surface of the drums or what?

MR. ROLFES: There were multiple measurements taken. I'd have to take a look at the hard copy report to tell you the exact locations.

DR. BEHLING: And how were these measurements made both for the neutrons and photons? Instruments? Using instruments?

MR. ROLFES: I believe there were survey instruments.

DR. BEHLING: And what source was used to calibrate those instruments?

MR. ROLFES: Did you happen to take a look at that?

MR. MORRIS: I didn't. I don't know the answer to that. It may be in the report.

DR. BEHLING: Because a lot of problems I've seen is that they used polonium, beryllium or plutonium-beryllium sources and then measured neutrons that they were very different in their energy spectrum; and therefore --

MR. MORRIS: REM meters tend to over-respond in those regions and so the errors are to give you a higher neutron dose than a lower neutron dose.

MR. GRIFFON: Just hold on one second. The report you're referencing, is that available on the O drive?

MR. MORRIS: Sure.

MR. GRIFFON: It might be good just to, and as we go through the day I think I'm going to repeat that question. Let's make these documents available so we have them. So we don't at the end of the course --

MR. ROLFES: At this time it is not on the O drive. I will definitely make it available.

MR. GRIFFON: Yeah, put it in our AB system so we can find it easily.

MR. ROLFES: Sure, I certainly will.

DR. ZIEMER: Robert -- and this is Ziemer -- what surveys did you say they were using for that? You said it was a REM meter.

MR. MORRIS: I'm thinking it's a Snoopy.

DR. ZIEMER: Snoopy? Okay, fine.

MR. MORRIS: I don't know the model number off the top of my head on that. It may be in the report.

MR. CHEW: Leo Faust actually did this work for us.

DR. MAKHIJANI: Are there any neutron data for Building 7, Plant 7?

MR. ROLFES: As far as personnel dosimetry or area monitoring?

DR. MAKHIJANI: Any data at all.

MR. ROLFES: I would have to take a look at the records. We do have neutron dosimetry results in HIS-20 from more recent years, I believe. However, given the near absence of neutrons based on the surveys that they conducted, they really didn't see that many neutrons. And they basically took a look at them via observed exposure rates and determined that it was not something that would be detectable by a worker.

DR. MAKHIJANI: Plant 7 operated only for 18 months in the 1950s, and it had uranium hexafluoride so I don't think, I don't see how you can make the assumption then that there were negligible neutrons.

MR. ROLFES: I'm sorry. I couldn't hear all of what you said, Arjun.

DR. MAKHIJANI: Plant 7 operated only for 18 months in the 1950s, and they had uranium hexafluoride there. I don't see how you can assume they had negligible neutrons.

MR. ROLFES: That's very possible. We'll have to take a look into that since the work was similar to Portsmouth or Paducah. What we can do is evaluate the observed neutron-to-photon ratios there and possibly use that information in order to address unmonitored doses in the early days.

DR. MAKHIJANI: Have you taken into account the criticism of neutron-to-photon ratio that happened in the Rocky Flats?

MR. ROLFES: Fernald is a separate site, and I wouldn't compare Fernald to Rocky Flats given that there was no plutonium production going on at Fernald. Fernald was a uranium facility. Their major goal was to produce depleted uranium targets for shipment to the Savannah River site and Hanford where they were irradiated in reactors to produce plutonium. There were also some smaller for thorium to produce thorium metal for shipment to several different reactors to produce U-233. I don't think it's a credible comparison to take a look at the neutron doses from Rocky Flats and compare those to Fernald.

MR. GRIFFON: Just the approach.

DR. MAKHIJANI: That wasn't the question.

But the question was there’s a method of using neutron-to-photon ratios in buildings and areas that were generally evaluated in the specific context of Rocky Flats not the specific ratios at Rocky Flats to be used some place else. And there were a lot of problems that -- and maybe they can be overcome in your analysis at Fernald, but the problems that were discovered, for instance, that building neutron-to-photon ratios may not apply to job types. There’ll be a lot of variation over time and over workstations. Those kinds of observations -- anyway, the --

MR. ROLFES: The bottom line that we draw is that we’re assigning, the bottom line that we’re doing in dose reconstructions which we feel is claimant favorable unless we have information that indicates to the contrary, we’re assigning a 0.23-to-one neutron-to-photon ratio for everyone that worked in, there’s a couple of plants.

We can also take a look at Plant 7 that operated for a short amount of time in the early 1950s. But in comparison to all the reports that we have seen, the neutron-to-photon ratios that we are assigning are claimant favorable in comparison to the observed measurements.

DR. BEHLING: Let me also interrupt. In your original report the 0.23 N gamma ratio was defined in behalf of a single drum, and those were empirical measurements. Your revisiting of that issue involves another different study. In fact, in the original study that 0.23 was, in fact, the 95th percentile value of the N gamma ratio. You've now looked at another study and looked at different measurements, I assume, and you're sticking with the 0.23 N gamma ratio. Is that also the 95th percentile value?

MR. ROLFES: I'm not certain. Is that 0.1 or the .023?

MR. MORRIS: The .23 is the 95th percentile.

DR. BEHLING: Also in the second study? In the original it was in the 95th percentile value, and you said will be claimant favorable by assuming the 95th percentile value. This is a different study, the same value. In the second study was this value also defined as the 95th percentile value?

MR. MORRIS: No, it was the largest value.

MR. SHARFI: But your largest value is less than 0.1?

MR. ROLFES: Right, exactly. So there's 0.089.

MR. SHARFI: Point 23 is still bounding versus the largest value on the new study.

MR. ROLFES: Any other questions before I move on?

MR. SCHOFIELD: How well do you know the characterization of this material? Is this an assumption or is this by actual analysis?

MR. ROLFES: This is documented. This is documented information. The quantities of material, the green material. Two percent of the drums were enriched between 1.25 percent and two percent. Twenty-three percent of the drums were enriched between natural uranium and up to 1.25 percent, 75 percent of the drums were natural or depleted uranium.

We also have a lot of new information on thorium production. We've located multiple, multiple documents on thorium production information. The petitioners were very helpful in providing some documents that

NIOSH had not access to previously. We've conducted several interviews with some former Fernald managers and workers.

We basically put together a matrix, which I've handed out to you, documenting where thorium production occurred by plant and by year. We basically have documented that production occurred between 1954 and 1979 except for a couple of years in '57 and '58. Also, the plants that were involved were plants 1 and plants 2, 3, 4, 6, 8, 9 and the pilot plant. And this is a slide showing the handout that we passed out.

We located multiple thorium air samples spanning more than 20 years. We sorted these data by year and fitted them to a lognormal distribution. We calculated the 50th and 95th percentile values which we input into Atomic Weapons Employer thorium intake model which was developed by Battelle.

For the years where we do not have detailed information or we feel that information isn't sufficient, we are going to default to the exposure for the maximum year that we have documented. And we will assign the maximum year intake for the year where the data is not as strong as we would like it be. And we believe this is very claimant favorable as well.

MR. BEATTY: Excuse me, Mark. Can I ask a question, please?

MR. ROLFES: Sure.

MR. BEATTY: On this matrix are you only talking about the years of production with thorium? Are you not including the over packing and remediation effort with thorium?

MR. ROLFES: Well, as I understand that was done in the more recent years. The SEC petition is up to 1989, and so I understand that a lot of that work began in the late ‘80s or early ‘90s.

MR. BEATTY: I was just noticing it stopping at ’77 here, and I knew the petition went to ’89.

MR. MORRIS: Well, our rationale for this is that’s when production actually stopped, and we have, in the technical basis document there’s some special storage issues and repository issues versus production issues. And what we were really missing our data on was production years and so that’s where the focus was.

MR. ROLFES: The Atomic Weapons Employer model predicts both inhalation and ingestion intake rates. We can actually input the actual number of hours into the model. We factor intake rates by job title for operators, laborers, supervisors, and administrative clerical staff. We can validate that this is claimant favorable by comparing the intakes based on the air monitoring data to the coworker analyses, the Mobile In-Vivo Radiation Monitoring Lab results that we have and analyzed as well.

The ongoing coworker studies include in-vivo data for thorium from the Mobile In-Vivo Radiation Monitoring Lab which as lung count data that was transcribed from 1968 through 1988. We fitted this information to annual lognormal distributions and modeled intakes using the Integrated Modules for Bioassay Analysis.

Our uranium bioassay --

DR. MAKHIJANI: Mark, before you leave the thorium, how many in-vivo data points do you have in all?

MR. ROLFES: I believe we had a total of 3,000 measurements, I believe, is what it was for the thorium results. Now that's either thorium or thorium's daughter products. Sometimes it was reported as thorium mass, and in the more recent years it was reported as Actinium-228 and Lead-212, which are two thorium daughter products.

DR. MAKHIJANI: I couldn't tell when I looked at the thorium mass data what was actually being measured.

MR. ROLFES: I believe it was the same daughter products that were being measured in the earlier years, but they reported total mass based on calibrations that were done onsite.

From the uranium bioassay data that we have for Fernald workers almost all of the workers were individually monitored for uranium exposures. So the need for a coworker study is really marginal, but there was approximately, I believe, about seven percent of the workers that might not have been monitored and should have been monitored.

So what we are doing is taking the information from those who were monitored, completing a statistical analysis and coming up with a claimant-favorable coworker model for people that might have been exposed to uranium without bioassay data. And we will be assigning the recorded results of the urinalysis to those unmonitored workers. So once again this is another claimant-favorable assumption that we are making by assuming that a person that wasn't monitored could have been exposed, and we are, in fact, assuming that they were exposed.

MR. GRIFFON: Mark, I'm not sure where it makes sense the most to do this in the matrix or during your presentation, but I've got about five or six actions in my head already, and it all regards the data. I mean, you've mentioned that you have put all this thorium data together. You have your in-vivo count data. I haven't seen any of it. But I want to make sure we track the actions and say you're going to post next month --

MR. ROLFES: Yeah, definitely --

MR. GRIFFON: So maybe as we go through the matrix it would make more sense because I know these items come up.

MR. ROLFES: Sure, be happy to. Once again, I'll post all this information. Anything or any records that you would like to see, I will be happy to put those under the Advisory Board Review folder on the O drive.

MR. GRIFFON: Would that last thing you mentioned, the uranium urinalysis records, is there an electronic database or are you building one or what?

MR. ROLFES: We have, when we receive a DOE response from Fernald, it comes from the HIS-20 database. Now we also do receive some older, hard-copy records, but I believe many of those have been typed into the HIS-20 database as well.

MR. GRIFFON: HIS-20. So I think that's one item. I think the HIS-20 if you can post that database right off the bat.

MR. ROLFES: Anything else?

MR. CLAWSON: Just that the claimant is able to get this information, too. What they can.

MR. ROLFES: We're not going to be able to provide the, for an individual claim we can provide the claimant's dosimetry information based on a Freedom of Information Act request. However, we cannot provide much of the data, too, because of Privacy Act concerns, much of the data does have people's names on it. We can definitely do what we can to work with the claimants and/or petitioners to provide --

MR. CLAWSON: So what about the petitioner that filed this?

MR. ELLIOTT: This is Larry Elliott. Only the Board the contractors can have access to the information on the O drive. So if there's anything the petitioner feels they need, we would have to work with them through the Privacy Act laws.

MR. GRIFFON: I think Brad's point, I mean, just from our last process with Rocky, I think we want to make sure that anything that's publicly shared, we make sure we get it readily available to the petitioner, you know, at the same time that we all have it if it's publicly available.

MR. ROLFES: Another analysis that we worked on was the radon breath analysis results for evaluating radium exposures. And back in the early days Fernald, back in the early ‘50s, Fernald received approximately 1,300 drums of waste that they slurried and pumped into the K-65 silos, silos one and two. This material contained many of the radionuclides. Radium was one of those components in the silos. We have 449 valid radon breath samples located for the years 1952 through 1954 when the workers were transferring the material into the silos. We are using ORAU Technical Information Bulletin-0025 to interpret the radon breath analyses for bioassay data.

From the calculated radium body burdens, we are using the 95th percentile value, but we have calculated the 95th percentile value of 0.15 microcuries. From a known radium intake, we can then add in dose from other isotopes in the K-65 materials based on measured and documented activity ratios.

MR. CLAWSON: You said 1,300, but you’ve got 13,000.

MR. ROLFES: Thirteen thousand, thank you.

MR. CLAWSON: I just wanted to make sure we’re --

MR. MORRIS: Mark, yesterday we learned in an interview that these radon breath analyses samples also represented the workers who were in Plant 2, which was just Plant 2 identified at the time. It became Plant 2-3 at a later date. And so was on the ingestion and extraction side. And the raffinate part of Plant 3 including -- what was it called?

MR. RICH: Hot raffinate building.

MR. MORRIS: Hot raffinate building so there was more scope than just this 13,000 drum coverage. It was actually the raffinate stream at the same time.

MR. RICH: Some of the separation that they did at Rocky Flats, not at Rocky Flats, Fernald, and represent the same type of raffinate that were delivered from Mallinckrodt in the 13,000 drums plus other sites. They were all pitchblende which were high in radium and thorium. So they did sample throughout the plant and the raffinate during the raffinate period which is the pitchblende separation process.

MR. ROLFES: We’ve also taken a look at thoron exposures. Since we now have additional information on thorium processing and storage, we can assign thoron intakes based on some documented release factors. We also have located historical thoron-specific measurements that were made. These are not as detailed as we would have liked, but we are going to use these measurements to validate our analyses.

We have calculated working level months for exposure values for the storage and processing areas for all time periods now. And we are assigning claimant-favorable defaults of up to 20 working level months per year.

The recycled uranium first arrived at Fernald in February of 1961, and the primary contaminants were Plutonium-239, Neptunium-237 and Technesium-99. And the limiting radionuclide in there was Plutonium-239 which was controlled and maintained at less than ten parts per billion.

Historical average results for plutonium in the recycled uranium was approximately 0.9 parts per billion. There was a maximum concentration that ranged up to

97 parts per billion which was a shipment that came from Paducah Gaseous Diffusion Plant tower ash.

We have assigned a default correction to all urine bioassay based on 100 parts per billion of plutonium and other contaminants beginning with 1961 as well as for all periods following. And these defaults we feel are very conservative. The tower ash receipt operation was identified as a special case.

MR. RICH: Mark, it would be well to mention, I think, the tower ashes were as you indicated there was (inaudible) and (inaudible) we just found out yesterday.

MR. ROLFES: Yes, the two workers interviews we've done yesterday, we had found out that this operation where they received the Paducah tower ash was a special case where they wore respiratory protection, airline respirators, and they down-blended the material with material from Fernald in order to lower the concentrations of the recycled uranium contaminants.

For environmental dose we have also re-evaluated historical emission source terms.

MR. MORRIS: Can I just clarify to that. The 97 parts per billion average, was that after it was down-blended or -- you've got the numbers wrong there. Ninety-seven parts per billion was the highest observed in any subgroup process.

MR. RICH: Including concentrations so we defaulted to the highest plant-wide concentration of plutonium and contaminants.

MR. MORRIS: To proportion to that. What we didn't include was the tower ash because it was a special campaign.

MR. ROLFES: All right, we have used data from the RAC Report Number CDC-5 "Uranium Emission Estimates". Thorium emissions were estimated using the latest thorium production data based on the information that we have compiled in this handout.

DR. MAKHIJANI: Did you have some breakdown of episodic versus continuous releases in that source term?

MR. ROLFES: Episodic versus routine releases in the source term? I believe we did. I'm not familiar with the calculations at this time. I'll get you an answer in just a second here. We'd have to take a look at the report and get back to you.

MR. ELLIOTT: Were you thinking of examples, Arjun, of episodic releases that --

DR. MAKHIJANI: Well, in our review and a RAC review, there were many episodic releases that were documented. But in the '50s, which was the worst release period, it wasn't clear that the very large releases that happened then were documented.

But there are indications that they did have serious episodic releases. I don't know that they were measured. And so it's a kind of methodological problem at Fernald to have these extremely large releases some of which were very likely episodic and not well documented.

MR. MORRIS: Well, we used data from the RAC Report which, as I recall, was one of your recommendations at a prior review.

DR. MAKHIJANI: Right, it was a recommendation for the overall source term since the RAC Report and other work demonstrated that the Fernald official source term was wrong and omitted many important elements of the source term. However, I haven't looked at the RAC Report recently, but I don't think they did a very thorough job of looking at episodic releases, not because they weren't trying, but I think -- I've looked at this problem, and I think they looked at this problem -- and it is a difficult one. I don't know what we said about it in our review of this.

DR. BEHLING: Well, let me add a couple things because I looked at the RAC Report, and I believe they were coming up with numbers like 5,000, 6,000 curies per releases. But if you look at the radionuclide mixture and you realize the disequilibrium, you come up with values that I calculated to be about 90,000 curies per year. And so I just looked at the nuclide ratios, and on the basis of first principles, you have to conclude that the release quantities were probably a factor of ten to 20 too low.

MR. MORRIS: So are you saying we should have used something besides those reports?

DR. BEHLING: Well, if you just look at the ratio, and it's in one of my findings where I looked at the radionuclide mixture, and I said there's a disequilibrium here that cannot be justified on the basis of five or six thousand curies releases. Take a look at that finding, and I explained it very definitively.

MR. ROLFES: We'll take a look at it.

DR. BEHLING: And I think among other things was the fact that in the '80s there was basically the silos were sealed off. And so what you may have observed later on may not reflect the time period when the silos were essentially open to the free air. And I don't think that was taken into consideration by the RAC Report. Take a look at the finding.

MR. ROLFES: Anything else?

DR. ZIEMER: Yeah, is somebody tracking the items as we go? Who's tracking?

MR. HINNEFELD: I'm doing a relatively poor job of it.

MR. CLAWSON: Arjun, can I get you to --

DR. MAKHIJANI: I am. You asked me that yesterday but --

MR. GRIFFON: But I think as we go through the matrix it makes --

DR. BEHLING: Yeah, but I'm hoping that we can actually look at the matrix --

MR. CLAWSON: I thought we'd start in the matrix. But it's a good point.

MR. GRIFFON: We can finish the overview, I think, right? But then when we go through the matrix, I'm going to reiterate some of the actions I had with others --

DR. ZIEMER: Well, the overview is becoming a little detailed.

DR. MAKHIJANI: Brad, just so I get my charge right, should I start documenting the issues?

MR. CLAWSON: Well, what I was planning on doing was when we got to the matrix, we'd bring, we're probably going to reiterate most of this stuff, but we want to make sure that we haven't lost any of this information.

DR. MAKHIJANI: I'll make notes and send them to you.

MR. CLAWSON: Okay.

MR. ROLFES: Here is the answer to your question, Arjun. The new model incorporates evaluations for episodic releases that occurred. Calculated concentrations near buildings include building wake effects. And the annual joint wind rose data was also used for frequency, wind speed and wind direction.

Other radionuclides that in the emissions included uranium progeny, Radium-226, Thorium-230 are also added to the uranium emissions from the uranium ore processing. Thorium-232 progeny including Thorium-228 and Radium-224 are added to the thorium emissions from the storage areas.

Concentration fields for radon near the silos include building wake effects in our environmental calculations. And pitchblende ore storage from the Q-11 silos were identified in the Pinney Report, and these have been added to the radon source term as well.

Back to external doses again.

MR. MORRIS: This is environmental external.

MR. ROLFES: Okay, environmental external doses. The direct radiation from Radium-226 and the progeny in the K-65 silos were derived from environmental monitoring data after 1976. The annual doses prior to 1976 near the K-65 silos are extrapolated from dose measurements in the early 1950s and ‘60s.

And that is the update on the technical issues that we are incorporating into our revision of the Fernald site profile.

DR. ZIEMER: Mark, could you or one of the O-R-A-U team talk a little bit about the breath analysis capabilities in those days? What was the methodology and calibrations and also talk about same on the thoron and how were they distinguished?

MR. MORRIS: The radon breath analysis was done at University of Rochester under subcontract. Exhaled air volume was captured in a cylinder of some description. I think it was a round --

DR. ZIEMER: Charcoal or was it --

MR. MORRIS: No, it was actually --

DR. ZIEMER: Oh, they evacuated.

MR. MORRIS: -- evacuated some, I think it was they were given an evacuated sphere if I recall. And then it was shipped to the University of Rochester where it was analyzed. It turns out we have an OTIB on this method in the repertoire of the Oak Ridge Team. The analysis then was calibrated back to, was traced back through calibration to radium full-body burden. And from that the dose calculations are bounding from there. Yeah, there's certainly a question about --

DR. ZIEMER: And the thoron was done in a similar manner?

MR. RICH: No, the thoron breath analysis significance. These are purely theoretical.

MR. SCHOFIELD: Oh, okay, it was talking about thoron breath analysis as well.

MR. ROLFES: I apologize. Those were not thoron breath analyses that were conducted. Those were actual thoron measurements that were completed within the areas that were processing thorium. The thoron measurements that were conducted were air samples that were collected, counted, I believe immediately and then counted again after several minutes I think it was.

I'd have to take a look back at the analyses to determine the amount of time. But it is documented in the air samples that we do have to determine both the short-lived as well as the long-lived activity.

MR. SCHOFIELD: How frequent were these samples taken?

MR. ROLFES: I wouldn't be able to make a judgment without looking back at the records right now. These were very limited. There's probably a few tens of results as I recall.

MR. MORRIS: Well, you're talking about the thoron?

MR. ROLFES: The thoron, yes.

MR. SCHOFIELD: So they have the potential for missing a lot of dosage there.

MR. ROLFES: Well, that's true that thoron measurements were not conducted routinely, but what we have done is taken a thorium production, we taken the thorium production information. And we have calculated release fractions and used those thoron measurements to confirm our analysis. So we have come up with an analysis that's very claimant favorable.

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

MR. ROLFES: Yes.

DR. MAURO (by Telephone): I just have a, from a perspective, you're referring to a great deal of information. Just I wanted to confirm that the material that you're describing, is that material contained in a recent version of the site profile and/or in the evaluation report? Or is this material, the analysis that you're describing, this is material that has been developed relatively recently and is being incorporated into a new revision, an upcoming revision, of the site profile?

MR. ROLFES: This is information that was assembled and evaluated based on the SEC report and based on the SEC investigations that NIOSH conducted. This information is, in fact, being incorporated into a revision of the site profile for Fernald.

DR. MAURO (by Telephone): Okay, but it's not in the version that's currently available to us.

MR. ROLFES: Correct, it is not in an approved public version at this time.

DR. MAURO (by Telephone): I just wanted to be a little oriented because it's a lot of material that I wasn't aware of from my reading of the previous documents.

MR. ROLFES: Yeah, as I mentioned, this was one of the first few technical basis documents that was completed. NIOSH was trying to get some answers for claimants in a short amount of time. And we realized that the information that we had at that time was not complete and realized that we would, in fact, have to revisit this information. This is one of the many important source terms that we are adding into the technical basis document.

DR. MAURO (by Telephone): I appreciate that. And also as I understand it you're also then, the evaluation report that we recently reviewed and put a report out, that material is not contained or is it referred to in the evaluation report?

MR. ROLFES: What material is that, John?

DR. MAURO (by Telephone): The evaluation report for the SEC petition that was put out and that SC&A recently reviewed and submitted a report. I just wanted to get a little clarification of how much of the material that we're talking about right now, or the findings perhaps, has been incorporated into your evaluation report.

MR. ROLFES: I'm not sure I understand --

DR. WADE: The materials that you're --

MR. ELLIOTT: He wants to know if our evaluation report addressed any of this new information, and the answer is no.

DR. MAURO (by Telephone): Okay, that’s all I’m asking.

MR. CLAWSON: So when this information goes in the TBD, I realize that they’re a living document and so forth like that. There’s going to be page changes and so forth.

MR. ROLFES: Most definitely. This will be incorporated into the technical basis document for use in dose reconstructions, and that approved version will be made available to the public. This information has been informally documented in draft papers, and we’re in the process of getting revisions to the environmental section of the TBD in the internal primarily.

MR. CLAWSON: We’re going to get to the matrix in a minute, but you’ve handed out this thorium operation, and you’ve got Xs. What are they actually representing? Because I’m seeing a lot that have four, some have three.

MR. MORRIS: That looks like an old copy to me. I’d refer you to the one that’s in the handout itself. And let me describe what would be around that.

DR. ZIEMER: Which handout are we --

MR. ROLFES: I apologize. I didn't provide a copy of these slides.

DR. WADE: Do you want me to make copies of that before you -- I can make copies of that before you describe it to people.

MR. MORRIS: If you could visualize mass numbers in this line of Xs like 300 metric tons or 200 metric tons. It represents if we had individual year data for production, we put that in there. If not, we put the total that was listed for that thorium campaign over those years.

DR. MAKHIJANI: I'm lost. I cannot, I guess I need to near a piece of paper.

DR. WADE: Can you put that slide up?

MR. CLAWSON: Do you have that matrix in your slide show?

MR. GRIFFON: I think we need a copy of the whole --

MR. ROLFES: Yeah, I didn't provide a copy of the presentation. I apologize for that.

MR. MORRIS: You can see that there are numbers interspaced into there, and sometimes we have real production data available for an annual basis and sometimes we didn't. And when we didn't have production data annualized basis, we just said that that was to total mass through that campaign over the years.

DR. MAKHIJANI: And is the production geared to the dose reconstruction in some way?

MR. MORRIS: No, that will not gear to the, the air samples will drive the dose. It won't be the production data.

DR. WADE: Now what is your pleasure with regard to hard copy of the slides? Would you like those made and distributed as quickly as possible?

(affirmative responses)

DR. WADE: I need a copyable version.

MR. CLAWSON: I appreciate that. I was just trying to figure out what all that, what was the meaning. What was represented.

MATRIX DISCUSSION

So, Hans, I guess what we'd like to start is just start out with the first item on the matrix and start off our discussion.

DR. BEHLING: Let me make a couple

statements beforehand. First of all, my report was obviously geared towards the SEC evaluation report as well as the technical basis documents that define Fernald. And so we're dealing with issues that in part have been modified as a result of the more recent information that has been presented to you.

But I also want to make a couple comments here. In my report I identified 29 findings, and I know there's a certain subjective element to the finding what a finding is. In my way of thinking, in certain instances under different circumstances, some of the findings that I identified would not have been considered a finding.

When I looked at the totality of the picture, and I can give you sort of an analogy as a finding as being a spoke on a bicycle wheel. If you pull out one spoke, the bicycle rides just as nicely as it did with that spoke still in place. But if you take enough spokes out, the wheel fails to function. And I looked at the findings in a collective term in saying how many findings can you possibly have before the system starts to really be questionable.

One of these, or even several of them, would have probably been regarded as an observation that says, yeah, you can fill in the gaps. You can easily accommodate that deficiency. But when there are so many findings, and so many things that are potentially amiss, then I start to look back and say, no, this has to be a finding because it’s part of the larger problem. A single crack in the wall makes no difference to the integrity, but if you have a crisscross or a spider web of cracks, the wall crumbles, and that’s how I viewed this.

And the other thing I wanted to point out is an issue that has been raised numerous times in the past with regard to Fernald, and I believe some of the petitioners raised that question. And that is we hear an awful lot about what we can do, but the real question is, is it plausible?

There’s a lot of things that in theory can be done. And you heard again today a tremendous amount of new information, and we have radon breath data and so forth. But the question is can we necessarily mate certain data with people, and what happens when you don’t have data. We have default values.

For instance in the case of radon breath samples I hear that, oh, yes, we do have radon breath samples for some, but obviously, not everyone. Are we going to use 20 working level months per year as a default value? And will that be used for a person who may be a potential claimant that has to be compensated? Or is this a default value, once again, that is only used to maximize the dose and to say, no, sorry, even 20 working level months per year assignment won’t get you over the 50 percent.

There are a lot of unanswered questions I have with regard to the complexity of this issue, and the ability to apply these complexities out in the field. I know there’s a lot of experts here. Mark and Stu and Jim Neton and others, they’re always a party to these discussions, and they always know the answer that could be used to satisfy a certain deficiency. But the question is they’re not the people who will be doing the dose reconstruction.

And the people out there who are not party to this, may not have any clue that when there is no radon breath data, that their potential exposures should go to a default value of 20 working level months per year. That I don't know, and I always question the ability of the dose reconstructors to actually make use of the information that we're hearing about today and in the past.

MR. ROLFES: Well, Hans, I can say that I've probably done more Fernald dose reconstructions than anyone within NIOSH and OCAS outside of the contractors. I know Mutty Sharfi. I'd like to have him go ahead and make a comment about that.

MR. SHARFI: Actually, one of the reasons why me and Karin are here is we represent the dose reconstruction group, so we can play a role in any additional information that provided more fundamental changes in the approaches in how we assign doses, that there is a dose reconstruction understanding of all the new aspects or any changes to the site profile.

So it's not just a blanket change to the site profile where it's not clearly defined into the dose reconstruction side. So we do try to take an active role. And the same thing in Rocky Flats where we would take an active role in to making sure that the dose reconstruction side is in agreement and consistent with what the findings are from this group.

DR. BEHLING: Now let me ask the question here because one of the previous meetings you showed a slide that says to date we have somehow close to 700 claims that haven't been completed of which -- no, 90 percent of the claims that had been submitted were completed. And that was months ago back in early of this year, February. To date I assume we're probably closer to 95 percent of the claims that have been submitted have been completed.

And, of course, I've looked at some of the claims. I haven't done an exhaustive search, but I realize that many of these claims have been completed on a basis of TIB-0002. And a lot of the information that is obviously at this point only in the process of being formulated, let alone get implemented. And so we're 95 percent probably home free in claims, and we're still in the process of modifying the TBD. We're still in the process of establishing a Patel* dose model that involves a generic AWE procedure.

And I'm just questioning. We're going to be still talking about modifying when there are all the claims have been done. And they were done by old methods, and methods that at this point have been abandoned including the, for instance, the K-65 silo --

DR. WADE: Okay, let's let NIOSH answer that.

MR. ELLIOTT: I'd like to speak to another level of this though, Hans. There's another level that we didn't talk about here just a moment ago, and that is the reviews that goes on with regard to dose reconstructions completed under a specific approach, and any changes that occur regarding that approach. So you have that as another level, I hope, of assurance that these things are getting attended to properly in the claims.

The other thing I want to speak to is

that, yes, we made calculated decisions on when to put a technical approach into dose reconstruction play knowing full well that there were aspects that hadn't been fully developed in that approach or a full, best-estimate dose reconstruction.

Our regulations enable us to employ efficiency measures in our dose reconstruction approaches, and this is one of those ways we employ an efficiency measure. To use a tool as soon as we possibly can to give people answers in a timely fashion.

A rule also enables us to go back and look at denied claims and re-examine them with new understanding, new tools, new approaches and better designs in order to make sure that the compensation decision is correct. We see this as working to the benefit of the claimant population.

DR. BEHLING: Well, as I said, I clearly understand the efficiency. Most of you know that I've been very much involved in this project from day one, and I clearly appreciate the need for a new efficiency measure. But when I see a TIB-0002 protocol where a person gets assigned 28 radionuclides on day one of his employment, and he’s there for 30 years, to what extend have we verified that the actual doses that the individual may have received far exceed what might otherwise --

And I realize TIB-0002 is intended for people who were never even monitored. People who have no reason to be exposed. It is strictly an efficiency tool. And I fully grant you the fact that when, under those conditions, that model is used it is likely always, probably 99 percent plus, likely to overestimate the real dose.

But in this case, when I see a TIB-0002 being applied with a 28 radionuclides on day one of this occupational involvement employment at Fernald, and assume that he’s necessarily going to supercede or transcend his actual, I have to really question it.

MR. HINNEFELD: Well, Hans, in cases -- I’m sorry, Stu Hinnifeld from NIOSH. In cases where TIB-0002 is used and a person, for instance, had monitoring data. It’s only used in a case where it can be demonstrated from his monitoring data that his exposure based on monitoring data is lower than the TIB-0002 dose. That'd be the only cases when a TIB-0002 approach should be used on personal monitoring data.

So, I mean, it has to be demonstrated in order to use that approach on that claim. So a TIB-0002 approach, something over a hundred years of exposure at the MDC, something over a hundred MDC years. So, I mean, it is a huge, huge intake given all at once. But it's equivalent to hundreds of years at the maximum dose concentration, so a huge amount. And it would be very hard to conceive of an actual exposure situation where someone would exceed a TIB-0002 intake.

DR. MAKHIJANI: We actually, this is a finding in our site profile review. It's finding 5.2.1. It refers to earlier work that we did on Mallinckrodt. Earlier work that we did on Mallinckrodt in which we had pointed out that in some cases the TIB-0002 doses at Mallinckrodt where people were exposed to a certain raffinate stream for not all organs generally, but for instance, for the bone surface, may be exceeded and that the recommendation was that NIOSH actually verified this in the case of Mallinckrodt and the recommendation in our site profile review. And that finding is that NIOSH verify this in regard to certain raffinate streams for Fernald. Because I am not confident that TIB-0002 will result in a conservative dose. And in doing the site profile review, I did look at some dose reconstructions, and I am not confident that what you are claiming to be a maximum dose would survive a close scrutiny for raffinate stream. In fact, there aren't good data for certain raffinate streams so I don't know how you could even go about verifying it. We'll cover it during the matrix. I think maybe we should get to the --

DR. WADE: Right, what we should do is get to the --

MR. HINNEFELD: This is pretty far afield but it's --

MR. ROLFES: To comment on what Arjun said, we wouldn't be using TIB-0002 to calculate a bone surface dose. That is not one of the organs that we would use TIB-0002 for. In the case of a bone cancer, as you're referring to, for the target organ would be the bone surfaces, because of the number of people that have bioassay data from Fernald, we would use the uranium bioassay as well as exposure from thorium based on the air monitoring data that we have. And those two components are usually sufficient to make a compensation decision.

DR. WADE: I think it's also important that we stick with the matrix. I think general discussion is good, but I think the grist of this really comes with the discussion of the issues in the matrix.

MR. CLAWSON: Right, there's only one point that I want to make before we start in the matrix. You know, we all work to procedures and so forth, and this is why it's so critical that Board, one of the things we're tasked with is data integrity and also if the process works. So this is why getting this information on the O drive or so forth is so critical to us. And that's why I know that I sometimes beat on it so much. It's so that we can actually verify what's out there and so forth.

DR. WADE: Can I make one other observation?

I think it needs to be said for the record though I think everybody around the table probably understands it. I mean, NIOSH might well have undertaken dose reconstructions early in the process and now the science has evolved to a new point. NIOSH is bound to go back and re-do those dose reconstructions, and I think everyone understands that, but I think it's important to say that.

MR. GRIFFON: And one more thing before we get into the matrix. This is really for Mutty. I have "Basic Guideline for Fernald Dose Reconstructions".

MR. ELLIOTT: It's probably old.

MR. GRIFFON: Yeah, it's probably old

MR. SHARFI: It's not going to include any of the --

MR. GRIFFON: That's what I was going to say. I have a 6-13-0-6. If you could provide the latest draft to him, that would be useful. It wouldn't even include this new stuff.

MR. SHARFI: Correct, it still would not include the newer stuff.

MR. ELLIOTT: I'm sorry. I missed that. You're asking for what?

MR. GRIFFON: The DR Guidelines that are currently being used, but they wouldn't even include these updates, no.

MR. HINNEFELD: It wouldn't address this new information.

MR. ELLIOTT: We won't. We want it as a matter of logistics. We won't update those until these discussions are done and whatever decisions are arrived at.

MR. HINNEFELD: It probably will be consistent because the site profile hasn't changed since almost it came out in 2003, so probably little information as we work with Task 3 to get clarification and make sure we fully understood areas.

MR. GRIFFON: Probably one of the first things you read in this one is that if there's no external or bioassay results, use environmental dose. And what Mark presented was we got 70 percent of the people without bioassay results, but we're going to develop a coworker model and use that. So already --

MR. SCHOFIELD: And that would be something we'd go back and have to reassess those claims if that's the way we did them.

MR. GRIFFON: Right. But we did that in Rocky.

MR. SCHOFIELD: Yeah, there'd be no --

MR. GRIFFON: -- retract those and make sure for like Super-S and for the other things that --

MR. ELLIOTT: It was part of the program evaluation, yes.

FLUOROPHOTOMETRIC URINALYSIS DATA

MR. CLAWSON: Okay, if we'll start into the matrix, limitations associated with the use of fluorophotometric urinalysis data.

DR. BEHLING: Arjun, let me, I'd like to introduce the issue, and then maybe you can respond. The issue is really one, and I've heard it before that our principle approach for dose reconstruction will rely on urine data. And, of course, a urinalysis was limited fluorophotometric method which only establishes the amount of uranium. It does not distinguish between different isotopes of uranium nor does it define the activity.

So when you have, obviously, a mixture of uranium plus, of course, all the contaminants that might have come from the raw source term that involved Congo ore as well as the recycled uranium. We don’t have any of that data. We don’t have solubility, and yet somehow or other we’re going to, I’m led to believe we’re going to use a very claimant-favorable assumption in just finding a quantity of uranium in urine.

So if you have, let’s say, 50 or 100 micrograms of uranium in a liter of urine, you’re going to somehow or other convert that into an activity that also not only defines the total activity of uranium and assume that that total activity is U-234, but you’ll also make assumptions regarding the solubility, et cetera, et cetera. And I guess I have to question what is it that you’re going to use here.

Obviously, with urine you always have to be aware of the fact that the most claimant-favorable assumption is that it’s always insoluble even if it’s a non-metabolic tissue that in question. And is this an assumption that will be made so that every time you have a urine sample, that the assumption is that it is an inside form of urine and that you have to somehow or other make a default value as to what the radionuclide mix is.

I've heard two percent enrichment because that's a critical issue here to convert mass into activity. And yet I know we have information out there that large quantities of seven percent uranium enrichment was done. So to what extent are we going to accommodate all these variables into a single format that says we don't know anything other than quantity in a 24-hour urine sample, but somehow or other we want to be claimant favorable in assuming that it is the right solubility and there is no variable.

It's only insoluble that is always regardless of what the tissue is most claimant favorable. And, of course, also the issue of converting --

MR. HINNEFELD: That's not, that's not --

DR. BEHLING: Well, we've done that before. You always assume that if it's an air sample --

MR. HINNEFELD: The intake was bigger.

DR. BEHLING: -- if it's an air sample, clearly, it would be much more favorable to assume that any other tissue than the lungs would be a proper. But we're dealing with urine now. Let's remember that. And if something is very insoluble and still shows up, that just means you've taken in a lot more than if it were soluble. I've done these calculations --

MR. HINNEFELD: Oh, sure, the intake's much bigger.

DR. BEHLING: And the dose to an organ based on a given value is always higher for insoluble.

MR. GRIFFON: The intake's higher.

DR. BEHLING: That's what you're trying to find out from a urine sample. You're going to have to convert --

MR. GRIFFON: But the next step is not necessarily intuitively obvious to me. The dose may not be higher to the organ because you've got to assume the same solubility when you carry it through for your dose calculations.

MR. HINNEFELD: Once it's in the bloodstream. Once it's in the bloodstream --

MR. GRIFFON: We have on many work groups.

MR. HINNEFELD: Yeah, we've been through this many times.

DR. BEHLING: In the calculations I've done it always shows that insoluble is the most claimant favorable.

MR. ROLFES: But not necessarily the dose.

MR. SHARFI: When we do a dose reconstruction, we always look at all solubilities and assign which ever will give the biggest dose to any, whichever organ is of interest anyway. I mean, we don't default to any particular solubility. If a soluble form would give a larger dose, then we'd use that. If an insoluble would give a larger dose, then we would use that. It's not bounded by a set solubility. We will find the most claimant-favorable solubility, and that's what is assigned.

DR. BEHLING: Okay, that's, the starting point is urine.

MR. ROLFES: So anyway, the NIOSH response to the issue of the fluorophotometric or fluorophotometric urinalysis data, we believe that this is not an SEC issue. What we are doing with the bioassay data that we have, the urinalysis data, we are converting the uranium mass to an activity excreted on a 24 hour basis.

And in order to complete this calculation, we take the mass value observed in urine, correct it to an amount of urine excreted for 24 hours, multiply that value of mass times the specific activity of the uranium enrichment. And then we assign intakes of that material based on claimant-favorable solubility information. And we calculate the internal dose from that intake assuming that all uranium that was inhaled was from, the internal dose that is calculated is all U-234 because that has the highest dose conversion factor.

So there are very, there are several claimant-favorable assumptions within there that really don’t make the issue on enriched uranium or low enriched uranium as big of an issue as it might appear to be. Because we are not assigning, we’re not doing best estimate claims for the greatest amount of the population at Fernald.

Our estimates are typically very claimant favorable. We are assigning chronic intakes over the entire employment history based on a person's urinalysis data rather than reconstructing specific, episodic intakes. Generally, when we are calculating intakes for a person, it is much more claimant favorable to assume the chronic exposure than an acute intake.

MR. HINNEFELD: Well, the key element here, Mark, is the enrichment.

DR. BEHLING: The principle element is the enrichment and what is the default value.

MR. HINNEFELD: Because that drives the specific activity, and that drives the whole thing.

MR. ROLFES: Exactly. I'll have to ask Bryce for the, for support on this, but I believe after 1961 we are assuming a one percent enrichment at this time, and after -- is it two? Two percent. I apologize, two percent enrichment.

DR. MAKHIJANI: Nineteen sixty-four or '61? The TBD says '64.

MR. ROLFES: Okay, I apologize and --

MR. SHARFI: 'Sixty-one when the type of uranium starts, and then '64 is when enriched uranium, enriched recycled uranium starts.

DR. MAKHIJANI: The reason I ask as in our review we actually said that that was not correct. That enriched uranium began, if you look at the materials accounting data at Fernald, you will see that enriched uranium began to appear at Fernald in 1950s. And the entire set of production data in the TBD is full of internal contradictions.

And I don't know if you've sorted this out in the new work that you've done, but it doesn't correspond to the materials accounting data either in any of the streams for recycled uranium for the various enrichments. So I don't believe that until these contradictions are sorted out you can actually assign, what one can agree as we did in the reviews that if you assign two percent for everybody from '64 on, that it would likely be claimant favorable for most workers. But in the context of an SEC where you have to have a more rigorous standard, you actually haven't addressed the five percent, the ten percent or more than two percent even though it wasn't a vast proportion of the material.

And secondly, the materials accounting, the materials flow from Fernald was very different than what you're assuming, and enriched uranium was present at Fernald in the '50s. And so I don't know where you got your information, but certainly the materials accounting data at Fernald are not, do not support what is being done in the TBD.

MR. ROLFES: The great amount of material at Fernald in the early time period was naturally uranium, and --

DR. MAKHIJANI: This is correct.

MR. ROLFES: -- and there may be, there may have been a very small amount of enriched uranium --

DR. MAKHIJANI: This is not correct. You have not looked at the materials data carefully. I pointed out that actually there are internal contradictions. Your recycled uranium amount is bigger than your total uranium process amount. You're off in your total production by a factor of two when you're saying 200 or more. You're saying

200,000 where the total at Fernald was about 600,000 metric tons according to the materials account data.

So I think you have a number of problems that we pointed out in the site profile review that apparently haven't yet been addressed. And the very material to the SEC discussion because unless you're willing to assign an arbitrarily high enrichment up to the maximum that was every assigned, you have to have the materials flow for various enrichments and who was working with what. And I haven't seen any information that allowed you to do that.

MR. ROLFES: Well, NIOSH would like to request the same data that you have available to you.

DR. MAKHIJANI: Well, we've given citations to the plant documentation, and I'd be happy to, they are in the review. They're memos, and they're filed every year with the Department of Energy.

MR. ROLFES: Well, if you could be helpful to us and provide that, we would appreciate it.

However, the enrichment issue we do not feel is an SEC issue because it is a selection of, we can basically assume exposure to any level enrichment that occurred at the site. Like I said, this issue is not a significant issue for the great majority of the claims. And actually, when we process a claim, when we complete a dose reconstruction, this issue, based on our approach, we are assigning very claimant-favorable doses.

Now this is an internal dose issue, and I'd be happy to run through an example or provide an example to the Advisory Board and SC&A on how we would reconstruct internal dose for Fernald to basically show that this issue is not going to be a significant issue for the great, great majority of the claimants that we are completing dose reconstructions for.

DR. MAKHIJANI: Could I ask the two Board members for some guidance in regard to how we are thinking about SEC issues under 22-CFR-83? Whether we are supposed to discuss all the members of a class and all the covered cancers or whether we're discussing claimant favorable for the majority of the workers. Because a lot of the comments are going to be the same, and unless we have some common understanding of what we're discussing, we're going to be repeating the same comments.

Whether something is claimant favorable for a vast majority of workers, which I would agree to and already written in the site profile review, but whether you have information to cover the class of workers is a very, very different and more rigorous question. And so I'd like to know what we're commenting on, whether we're actually in an SEC discussion or dose reconstructions.

MR. GRIFFON: Well, we're in an SEC discussion, and it is all members of the class, all the stuff. So that's my take on this. And so I would say, I mean, I think we have to have some fall backs and one might be an example related to this.

Another action I wrote down was that we need to have more information on NIOSH's assumptions regarding which levels. And then SC&A's action is to provide those references that they have so that we can get that clear. I think, Mark, you're probably saying that even if we find out that the level was higher for a certain time period, unless there's an adjustment, that's not really, and we can bound it.

On the other hand we do have this, well, in our procedures we say proof of principle. So we want to sort of nail it down like when are you going to apply, if we decide it was a higher percentage for a certain time period or for a subset of workers.

We want to understand that a little better. So I think we need to understand those assumptions and then maybe get a sample on the table as well of how you're going --

MR. ROLFES: Based on some interviews that we've done with some former workers, we know that the area where the higher enriched materials were, in fact, blended, and so we would look into that. From the records that I've reviewed, I have seen indications in documentation of higher assay material being worked with and air sampling, breathing zone air samples taken during that time period as well. So we could look at that as well.

MR. GRIFFON: So I'll try to track these actions as we're going through because I think, and then maybe at the end of the day we can summarize these because I think we tend to, we want to make sure we stay on them, right? We don't want to let NDRP slip, right?

MR. CLAWSON: So you'll help me track some of these?

MR. GRIFFON: Yeah, I will, yeah.

DR. ZIEMER: Arjun, did your original report include those references? I'm just looking at the report now, and they're in the reference list?

DR. MAKHIJANI: There's at least one reference to an incident in 1986. I'll check.

MR. CLAWSON: Mark, I've also got one question. How much uranium did Fernald actually produce?

DR. MAKHIJANI: Nineteen eighty-five, I'm sorry.

MR. ROLFES: Off the top of my head, I don't want to throw a number out there. Bryce or Mel?

MR. CHEW: Ask the question again.

MR. CLAWSON: How much uranium did actually Fernald produce in their life?

MR. CHEW: I don't have that.

MR. CLAWSON: Let me tell you why. Because I go into the TBD, and I see one reference. And it go to the DOE site, and I see three times that amount. And in several different other positions one of my questions and why I'm bringing this up is I see that I can't get a clear, I believe your TBD -- I can't remember how many thousands of tons it was. It was 30,000 or something like that, and I see on a DOE site that it was actually 120,000 that was produced. So there's a difference of almost three percent right there.

And actually, I went to one of their little videos of the clean up of it, and they said that they had basically about the same amount as what you guys were saying it produced over the life sitting there that they had to dispose of. So one of the things that I see in this, and I know the TBD is a living document. We understand that. But there is a clear disconnect in what was actually produced.

MR. ROLFES: Keep in mind that Fernald didn't just produce uranium metal. They also received shipments of uranium metal from other sites, so those could be some of the issues why the numbers don't match up. It may be an issue of the actual amount produced for shipment, you know, to Savannah River site and Hanford and other locations or produced specifically for the AEC. Because there was some work in the later years that was conducted for the Department of Defense as well. So I'd have to take a look at the numbers in order to make a judgment.

MR. CLAWSON: Well, and I'd like that to be an action item because one of the things, it's like with me. I realize, and I'm a person that's always said this about every one of the sites. We're all intertwined. We get an awful lot of stuff from Savannah River. I think in my data right now I've got Savannah River, Rocky Flats, Hanford, all this different stuff.

But one of the things about uranium metal that I've found, or uranium product that I've found that's different is being a nuclear material custodian when I have fuel come in from another facility, it doesn't go on my books. The only thing it goes into me for is criticality concerns and to assure that I'm not in a critical state and so forth. And I produced an awful lot of it through, but I never take responsibility. That is always on the other companies' books.

MR. RICH: May I say something?

MR. ROLFES: Sure.

MR. RICH: Let me make just a couple comments about inventories and material flow through Fernald. In the technical basis document, for example, there was an extensive study done by, for recycled uranium material flows. It was recognized that there were some conflicts between the various sites. When we did the recycled uranium study, for example, didn't all add up until three years later -- it took them three years -- to do another study, a follow-on study in 2003.

The only problem with that was an incomplete study that only dealt with the primary shipments from the primary recycled uranium shipment which was Savannah River and Idaho. And so that did not include the secondary shipments. So clearly, even within the recycled uranium material mass flow area, that ore, some disconnects as you pointed out, Brad. When you’d get it in for a certain purpose, you keep it on a separate inventory tracking system.

Now as far as the total mass flow at Fernald, see, they did the pitchblende, which is a natural. They also took material, they had a contract to take all of the yellowcake from all of the United States processing centers. And for a period of about five years, they processed that, which was a natural uranium, high volume, high mass flow. Now the point being that there are differences in mass flow for different programs.

And the technical basis document does not address all the mass flows. The mass flows that are in the technical basis document are primarily recycled uranium in an attempt to do not only the primary, because that secondary flowed into Fernald, and it’ll be different than what you can find in other publications.

Now if we go to get total mass flows of all uranium from all sources, that’s a different challenge. And probably doesn't relate directly to does reconstruction. And so if that statement has any clarification, it is related to what you're seeing on the reports now, I think there's a justification for it.

MR. CLAWSON: So you're telling me that none of your dose reconstruction is based on the amount of uranium ore product that they have?

MR. SCHOFIELD: Bioassay data.

MR. RICH: It's strictly on bioassay data, but what we tried to keep track of total types of material in the system and looked at, for example, the average enrichment in the back house filler, for example, over an extended period of time to get a feeling -- and by the way, that averaged out 0.7 enrichment. It averages out natural uranium because that's primarily the bulk of the material that was processed. And then what we've said is that to default to a two percent enrichment is at the level that would cover all but a few, a minor exceptions.

MR. CLAWSON: I think it's something that, I guess personally for me looking at the TBD and probably, I guess I've got to look at it like the common person looking at that, there is a disconnect there and might be something we may want --

MR. GRIFFON: Clarified.

MR. CLAWSON: -- clarified.

MR. RICH: There possibly could be a clarification even in defining the fact that if you compare this with other material flow sources that there will be this discrepancy. We did that in the technical basis document by pointing out the difference between the recycled uranium study in 2000 and the one that was done in 2003 to explain why we defaulted to different levels than what was in 2003.

The 2003 document was important, but it was not complete in terms of defining all of the material flow, recycled uranium, because gaseous diffusion recycled uranium came in. There's a lot of different sites, secondary sites.

MR. CLAWSON: Well, I wanted to bring it up because --

MR. RICH: You're right from a first-time reading. It can be a disconnect.

MR. CLAWSON: Well, and you start getting into a little of what Fernald actually did, and, you know, when you start looking at outside, even outside studies that were done by other groups, that -- I can't remember the name, but they called the group that was just outside Fernald, the locals there.

MR. ROLFES: Fresh.

MR. CLAWSON: Fresh, that's what it was. I couldn't remember. There seemed to be kind of a disconnect of part of this, and I just, mainly for clarification, we may look into that a little bit. We're basing everything on urinalysis and bioassay. How many --

MR. RICH: For uranium.

MR. CLAWSON: For uranium. How many bioassays and uranium samples do we have?

MR. ROLFES: Uranium urinalysis results?

MR. CLAWSON: Yes.

MR. RICH: Several hundred thousand. We have a lot.

MR. ROLFES: Off the top of my head I know that the latest number I had saw and reported at the Advisory Board meeting was about

180,000 results. However, I believe there are some additional ones as well in HIS-20 that, so the number's at least 180,000 results.

DR. MAKHIJANI: Dr. Ziemer, just for your reference I was wrong about (inaudible). It's Bogar 1986.

DR. ZIEMER: It's what now?

DR. MAKHIJANI: The material accounting reference is Bogar, B-O-G-A-R, 1986.

DR. BEHLING: Are we finished with this?

MR. CLAWSON: Yes.

DR. MAURO (by Telephone): Before we leave that -- this is John Mauro. In listening to the discussion I'm thinking about something that Arjun mentioned earlier and I think we touched upon, but I'd like to hear a little more on an issue. Let me pose my question.

Let's say we have a worker, and we have a bioassay sample in terms of micrograms per liter. We have that information regarding him, and perhaps we have a number of measurements for that worker. And we need to reconstruct a dose to one of his organs. And what I'm hearing is that there's some possibility that, well, we don't know whether that worker predominantly worked with natural uranium or perhaps enriched uranium. I heard numbers as high as five percent.

Also, there was some question about whether or not that material might have been recycled uranium that could contain ten parts per billion of plutonium. Where I’m going with this is something I guess I’m not quite, it’s almost more of an interpretation of the regs. If I have the worker, and I say, well, we’re really not quite sure whether he was working with a lot of enriched uranium or primarily for natural uranium and how much of it might have been of a particular chemical form and how much of it may have been recycled.

In theory, in theory -- and I understand, Mark, what you had said. In theory, certainly, you could make assumptions that would maximize the dose in terms of the degree of enrichment, chemical form and whether it was recycled or not. Now, I guess I have an SEC question that I could use a little help on.

Is it considered to be sufficient

accuracy to say, well, we'll default to those worst case assumptions when we really don't know for this particular worker or there's some uncertainty regarding this particular worker and what he did where he worked, et cetera, and just default to that which would drive his particular dose considerably much higher than, let's say, if we knew exactly what he did, and we know the circumstances were different.

So I think what I was hearing before when this matter of, is that considered to be, if you do take that strategy -- I'm not quite sure if, in fact, that's the strategy you plan to use, but it sounds like you might be leaning that way. If you do take that strategy, my question, I guess, is one of does that represent an approach from the SEC world that would be considered sufficiently accurate?

DR. WADE: Maybe I can read from the SEC rule and I think it goes to your question, John. These things are always subject to the interpretation of the listener, but I'm going to read from 83-13.c.1.

Is it feasible to estimate the level of radiation dose of individual members of the class with sufficient accuracy, question mark.

Small i, radiation doses can be estimated with sufficient accuracy if NIOSH has established that it has access to sufficient information to estimate the maximum radiation dose for every type of cancer for which radiation doses are reconstructed that could have been incurred in plausible circumstance by any member of the class or if NIOSH has established that it has access to sufficient information to estimate radiation doses of members of the class more precisely than an estimate of the maximum radiation dose.

So, I mean, I think that answers the question, but again, you always have to leave that supposition to the ear of the listener.

MR. ROLFES: These are plausible circumstances, and the issue of sufficient accuracy, we're making compensation decisions. We're not doing best estimates for regulatory compliance reasons. We are doing claimant favorable dose estimates for claimants. And when we have uncertainties associated with plausible circumstances, those uncertainties are always given to the benefit of the claimant in our dose reconstructions.

DR. MAURO (by Telephone): And I, Lew and Mark, I appreciate that answer because I think you’ve answered my question. The answer is, yes, that since it’s plausible that this particular worker in theory could have handled as high as five percent enrichment for some period of time, and it could have been recycled uranium -- this is a hypothetical now I created -- and since all of those are plausible scenarios, if, in fact, they’re considered plausible, then even though the only information you have is milligrams per liter of uranium in the urine, it would be considered to be of sufficient accuracy and plausible to make these what I would call worst case assumptions since they do fall within the realm of a possible scenario.

And I think you’ve answered the question. The answer is, yes, that would be considered to be sufficiently accurate. It’s something I’ve been thinking about, and I think I was looking for an answer. And am I correct? There's a general consensus that that is a proper interpretation. That is, the scenario I just described would be considered to be, yes, that would be a reasonable way in which to deal with that particular worker.

MR. ROLFES: I'm sorry. Could you repeat the question for me?

DR. MAURO (by Telephone): Well, it just had to do with, you know, if all you have is fluorometric results in micrograms per liter urine analysis, and then you're in a position, and this is more of an SEC question now. And I ask myself the question can I reconstruct this worker's dose with sufficient accuracy.

Now I have before me a lots of options of assumptions I could make because remember, my starting point is milligrams or micrograms per liter of uranium. And then I have to say, well, what am I going to assume is the type of uranium. In other words how do I convert that into activity. And I also want to factor in that where perhaps there may have been also recycled uranium or plutonium in there.

And if we don't know, we give him the

benefit of the doubt, and we assign that to him. And I could understand why that would be a way of making sure you're claimant favorable. And my question was is that something that one would consider to be of sufficient accuracy for that worker. And I think the language that Lew just read says, yes, that would be considered to be within the definition of sufficient accuracy. And that was the question I asked.

MR. HINNEFELD: I believe that would be NIOSH's interpretation.

DR. MAURO (by Telephone): Okay, I appreciate that.

DR. WADE: But again, it is also left to the Board to make its judgment of that interpretation when it makes a recommendation to the Secretary. There are four parts to what I read I think are important to remember. The one is that NIOSH as established has access to sufficient information to estimate the maximum radiation dose for every type of cancer incurred in plausible circumstance by any member of the class.

So to go back to Arjun's question,

there was a time when, Mark, you said for most members of the class. The test is for any member of the class. But I think when you look at the range of those tests, the Board then can understand what's in front of it.

DR. BEHLING: But, John, the question, I raised that very question that you were asking. And as a starting point I said, you know, what are the assumptions regarding solubility, enrichment, et cetera. And what you were basically asking which, for instance, five or seven percent in their documentation that seven percent enrichment was, in fact, used at least for certain periods of time in restricted quantities. Now, the question is, is a default value of two percent something that will satisfy your concerns, John?

DR. MAURO (by Telephone): That's why I asked the question, yes. And I heard that the selection was based on the time period you might use two percent. But then I also heard at the same time that there's some evidence that there were time periods, or at least situations when the concentrations may have been as high as five percent. And I think that goes toward some judgment. In other words the judgment is, is it sufficiently accurate to assume a default of two percent --

MR. GRIFFON: Well, that's, yeah, that's where we have a (inaudible), I think.

MR. ROLFES: Yes, if you take a look at the data the numbers of, in one of my slides I had from the approximately 12,000 drums that were stored in Warehouse 4-B I believe it was. If you took a look at the amount of material that was there, the great majority of that material was either depleted or natural uranium, approximately 76 percent of the material.

Now, the other components that were in fact in that warehouse were between natural uranium and 1.25 percent. So between 0.71 percent U-235 and 1.25 percent. And then there was another group of, I believe, 1.25 percent up to two percent enriched. That was a very small quantity. So when you take a look at the mass values of the uranium that was processed, it's very obvious that the great majority of the products coming from Fernald over time was natural or very, very slightly enriched material.

MR. HINNEFELD: But for SEC purposes the point is, is it plausible the members of the class, some employees, had an exposure, and if you're going to break this down by maybe a year or whatever increment you're going to talk about, that their exposure that year exceeded the two percent, some group, some small group of employees. That's an SEC question. It's completely irrelevant that the place dumps out mainly depleted uranium at the end. So that's completely irrelevant to the SEC.

What's relevant to the SEC is, is there a way to demonstrate that some enrichment value -- whatever you choose. Right now it's at two percent, but some other enrichment value, really provides an upper bound for what some small group of people might plausibly have been exposed to in a particular year if you want to break down by year.

DR. MAKHIJANI: And that data request, the TBD is volume two on page 15, paragraph one. The current TBD in volume two on page 15 says that 1,500 (inaudible) mass batches of up to ten percent U-235 materials were prepared for drum digestion. And it also said this was recycled uranium. So we've got actually potentially, you know, an example of many batches of uranium over time from '66 upward limit possibly of the uranium enrichment plus recycled uranium contaminants.

Do you have examples of worker DOE files that contain information that said which workers worked with this data. Now, this is in the refinery I think. Which workers were in the refinery or whether maintenance workers who went there to do this work, it is in their records. So some way of identifying the workers who worked with these 1,500 batches of ten percent.

MR. ROLFES: This operation they used material of up to ten percent enrichment to sweeten other batches of uranium metal. We know where this operation was conducted, and some of the interviews that we conducted were focused on this specific issue.

We know that some of the air monitoring data that we have from this area has documented higher enrichments of material.

And it also does have employees' names but not consistently. So we will have to take a look at that area and the exposures associated with, well, potential exposures to high enriched material in those areas where they did the blending.

DR. MAKHIJANI: I guess you missed my question. The question was can you give us examples of DOE employees' individual files that would establish that you know who worked with this material or would the proposal then be to assign everybody if, you know what I mean? If you were in that SEC mode, you have to be able to identify the workers who got, who worked with ten percent recycled uranium.

DR. ZIEMER: I think he's saying they're going to go back and look at that issue.

DR. MAKHIJANI: So it would be useful for us just as an action item, if the working group agrees, it would be useful for us to have claim numbers or employee files that contain information about who worked with this. Or in the alternative --

DR. ZIEMER: There's the other side of the question you'd have to ask, and that's can you show somebody didn't work with it. I think for the SEC you have to be able to establish that either on a time basis or a location basis probably.

DR. BEHLING: Can I just make a comment here? In finding number four I included excerpts from a Health Protection Appraisal report dated September 1968. And it states here that action has been initiated for hanging Uranium-235 enrichments about five percent, current plans include installation in Plant 1 of a continuous digester for enrichments up to ten percent.

And on the next page it makes reference to significant portions of the fuel will range from three to seven percent U-235 enrichment. And so there are documents here that lead you to believe that up to at least seven percent and possibly ten percent enrichment was processed at Fernald.

MR. CLAWSON: Excuse me. Go ahead.

MS. BALDRIDGE: I'll identify, Sandra Baldridge, a petitioner. I have a question. You stated that about 180,000 pieces of uranium urinalysis data. Of those data, is it identified which of those are from employees who had renal damage who would be retaining certain levels of uranium that were not being excreted?

People with exposure to uranium hexafluoride in one of the documents submitted showed everybody who had that exposure had renal damage. Now when I was going over my father's papers, I noticed in his medical infirmary records that there was a notation that he had renal damage. When I checked online about the condition and so forth it says that that type of renal damages causes a retention of uranium salts.

So if you are assuming that everyone was excreting at a hundred percent efficiency rate for the kidney, you know, and someone has a 50 percent or 70 percent or 80 percent damage, you don't know what their retention rate is so to measure what their excretion is and assume their dose based on that, you are eliminating the potential for undetected exposure and dose.

MR. ROLFES: Well, I'd like to clarify. If we suspect that the urinalysis data might not be adequate, we are developing a coworker intake model based on urinalysis data for the entire plant. The urinalysis data is not the only bioassay data that we have. We also have lung count data which we could use. We could take a look at the intakes that we're assigning from the urinalysis data and then compare those intakes to the intakes measured by the chest counter at Fernald. So that we wouldn't have any indication that --

MR. GRIFFON: Chest counting wouldn't be until a later period.

MR. ROLFES: Until 1968, that's correct.

There are indications in reports of renal damage that occurred from exposures to uranium hexafluoride, and that's, in fact, why uranium was being monitored for in order to control people's urine concentrations below a standard level to prevent nephrotoxicity. Have I answered what you're asking?

MS. BALDRIDGE: I think it just shows that even the data you're using can't give a definite comparison unless you know how many of these people were only excreting a portion of what they were being exposed to.

MR. ROLFES: When we're actually using the solubility that is the most claimant favorable. So --

MS. BALDRIDGE: Solubility doesn't reflect excretion --

DR. ZIEMER: I think it's an interesting question. I don't know that any of the models, the ICRP doesn't take that into consideration, and it seems to me it's an interesting question. Somebody ought to look at it. I think it's an --

DR. WADE: Well, I think it's a very interesting question.

MS. BALDRIDGE: I wouldn't have realized that it was a problem if I hadn't been --

DR. WADE: Yeah, excellent question.

DR. ZIEMER: I don't know if we have a way to handle that, but certainly --

MR. GRIFFON: I think the fundamental answer to your, the first part of your question, right now the data that you have, you don't necessarily have anything that implies that the person had renal damage, I'm pretty sure.

MR. ROLFES: Well, there are some reports that have documented some overexposures to uranium hexafluoride in the early time period.

DR. ZIEMER: Would that be in the medical record of the claimant?

MR. ROLFES: It is, in fact, documented in some reports. I do not know if it would be provided to us within the DOE dosimetry response.

MS. BALDRIDGE: My father's records didn't show that he had an overexposure. It just showed up and said, well, obviously he has been exposed to it at some point that has resulted in this damage. So it wouldn't have flagged his file to say there's been an incident here where this man was exposed. This was something that occurred without their knowledge, and they, after the fact, put the pieces together.

MR. ROLFES: The deterministic effects from uranium exposure associated with uranium hexafluoride, uranium hexafluoride is one of those more soluble compounds. And when we would do a dose reconstruction, it could affect, you know, an injured kidney could affect excretion. However, the material is generally a very soluble material.

So, in fact, that material rather than being excreted over a few day period, could be excreted over say a week or a month period. So it may extend the period which the uranium is being cleared from the body. And it's likely something that we definitely, I'd have to take a look at the case and the urinalysis data in order to make a judgment about a situation like that.

DR. WADE: I think it's a valid issue that needs to be addressed and reported back to the work group.

MR. CLAWSON: And you've written that down, Mark?

MR. GRIFFON: Yeah.

DR. WADE: Thank you.

DR. ZIEMER: Can I follow up briefly?

MR. GRIFFON: It might have wider ranging affects, too, on other sites as well.

DR. ZIEMER: On the issue of the discrepancy on some of the source terms, the reference that Arjun mentioned references by Bogar '86, it's a document in a litigation file. I just want to ask, is that available --

DR. MAKHIJANI: I will call the law firm and

--

DR. ZIEMER: It's a Cincinnati law firm.

DR. MAKHIJANI: Or you can call them. I mean, it would be better --

DR. ZIEMER: I mean, it's a reference, but it's not clear that it's available.

MR. RICH: Did that come out of a class action suit?

DR. MAKHIJANI: Yes.

MR. HINNEFELD: That's from a class action suit, but I mean, that reference in that time period should be available from Fernald. We should be able to get that from DOE.

DR. MAKHIJANI: I believe there's a full set of documents every year -- and Stu would know that better than me -- every year there was a report filed at least once a year. And I think at some period there was a monthly report that was filed. It contains DU, NU and EU. I don't believe it actually contained to my memory the level of enrichment. But it does specify the three screens and quite specific and quite detailed.

MR. HINNEFELD: Yeah, there was production control. There were, you know, routine production controls.

DR. ZIEMER: But they have access to different documents than you did?

MR. HINNEFELD: This document here, this Bogar document should be available from the Department of Energy. That's got to be available from the Department of Energy. That's, so that's got to be available.

DR. MAKHIJANI: That's not the only document that the lawyers got from DOE.

MR. HINNEFELD: Yeah, I know the author or knew the author.

MR. CLAWSON: Okay, well, one of the perks of being the Chair, I think we need a comfort break. For those on the phone we're going to take a ten or 15 minute break, and then we'll resume.

DR. WADE: Just stay on the line so we won't break contact.

(Whereupon, the working group took a break from 10:05 a.m. until 10:25 a.m.)

DR. WADE: Ready to go, so please --

MR. CLAWSON: Has it been unmuted?

DR. WADE: Yes, it's unmuted.

MR. CLAWSON: Is there any more discussion?

One of the requests that's come up to me as the Chair is that there's a lot of issues we need to try and get through, but we don't want to miss anything in the action. I feel that the first finding, there's been several addressed. But before I proceed on I would like to review the action items, if we could, Mark, and just make sure that we've got everything down.

MR. GRIFFON: You want to do these that we do so far?

MR. CLAWSON: Yeah, just before we go on to the next one because we had several issues.

MR. GRIFFON: Yeah, I have seven issues actually. NIOSH to review assumptions on enrichment level. This is all related to action item finding number one so it's related to uranium. Second, SC&A to provide references regarding enrichment levels. Bogar 1986 I think is the one --

DR. MAKHIJANI: Now is Stu going to get that from DOE?

MR. HINNEFELD: We should be able to get that from DOE. If we have a problem, I'll let you know. But I don't see how we cannot get that from DOE.

DR. ZIEMER: And the reference is in the SC&A report.

MR. HINNEFELD: It's a Bogar '86 document. I don't see how DOE cannot have that, but we'll try to get that.

MR. GRIFFON: Okay, so NIOSH to get this reference I think is the way I'll say that.

Third is NIOSH to provide sample DR to demonstrate approach for doing internal DR for uranium. That was what Mark had brought up.

MR. ROLFES: Mutty, do you know, do you recall -- I haven't looked at the sample dose reconstructions that we completed. We may have already done something very similar for uranium.

MR. GRIFFON: You can review it. See if they meet that.

The fourth one is NIOSH to examine whether the approach is appropriate for all members of the class. Parentheses, is there a subset of workers or areas where different assumptions should be made is the question of your sample. Does it fit all? As we've said all members of the class.

DR. MAKHIJANI: Could I supplement that in terms of the request for specific worker data?

MR. GRIFFON: Well, I have that, I have that in another action. I just kept them sequentially so they might overlap a little bit.

Five is NIOSH to review the total production numbers for uranium, provide -- and I think Bryce provided a good response to this, but maybe a written response, provide a written response to clarify differences in numbers in the TBD versus other documentation. Write it out.

MR. RICH: We can address the expected discrepancies and for what purpose.

MR. GRIFFON: Six was NIOSH to provide claim numbers for workers that worked in the blending areas, I said, involving the high enrichment levels. Is that where you said you'd like to see some of the high enrichment levels?

MR. ROLFES: We definitely have air samples identified with individuals' names on them. It might take a little bit of work to, because somebody might have been monitored that isn't a claimant so we'll see what we can do to respond to that. So it may not be claim numbers --

MR. GRIFFON: You may come back and say we couldn't find any claims that fit in it.

MR. RICH: Define the operations associated with the high enrichment.

MR. ROLFES: Yes, exactly, exactly. The process information we can get, the additional information can be provided on --

MR. RICH: Which is not directly related to dose reconstruction although it has some implications.

MR. GRIFFON: And the last one is NIOSH will examine the issue related to renal failure and the effect on uranium excretion and on the DR approach. And that was one of the same.

MR. CLAWSON: Well, one other thing I'd like to request from NIOSH, and I know this isn't onto this, is yesterday we came up with one of the things. These TBDs and so forth, when we add pages and so forth like that, could we kind of highlight those so that we know where they went, where they were placed in there? Because for us to feed through, like we did at the Nevada Test Site, what areas were changed or so forth --

MR. ELLIOTT: A matrix, you want a specification of where we made the change in the document.

MR. CLAWSON: Yes, if you would. That'd just make it a little bit --

MR. ROLFES: Sure. I understand for like a page change. I think our internal dose section is going to be, it's going to have so much additional supplemental information from three years ago, I think it would be a significant amount that would be highlighted, so --

MR. CLAWSON: Well, yeah, just, like we did with the Nevada Test Site where they were changed --

MR. ELLIOTT: It will simply say the section number.

MR. CLAWSON: And so forth like that, I'd appreciate it.

MR. ELLIOTT: Sure.

MR. CLAWSON: Arjun, if you want to continue on with --

DR. MAKHIJANI: I think Mark has already covered what I have.

MR. CLAWSON: Hans?

QUESTIONABLE INTEGRITY OF FLUOROPHOTOMETRIC URINALYSIS DATA

DR. BEHLING: Yeah, let's just go to the next finding, and the finding that you may see in your matrix is simply identified as questionable integrity of the fluorophotometric urinalysis data.

And we've already discussed the limits of it based the fact it only gives you quantities rather than isotopic (inaudible). But in addition to that there is something of a near absence regarding formal records that define the protocols that were used or any quality showing some quality controls that were exercised to ensure that the data was, in fact, reasonable and scientifically sound.

But one of the things that also bothers me is the issue of how the people who actually ran the program perceived urinalysis. And let me quote a couple things that came from people who were in charge of the program, and what their statements were in memos. And I've identified these memos as part of the attachments.

I won’t go identify the names because we’re trying to obviously shield people from being identified here, but they are reputable sources. And he says, “We use urinary uranium excretion information along with air survey information to be sure that we are controlling airborne exposures to amounts that will not be harmful.”

And then he goes on to say, “We do not consider the urinary uranium excretion measurements as an accurate method for estimating either body burden or any method for exposure.” And it goes on and on. And there are several of these documents that consistently make reference to that.

On another date the statement goes on, “We have pointed out on previous occasions we have little confidence in the reliability of any method for assessing dose from depleted, normal or recycled enriched uranium as levels,” et cetera. “...and believe that uranium assay results are of no value for this purpose.” And there’s on and on.

I’d cite multiple documents by people

who represented the Industrial Hygiene and Safety who claim that they have little or no faith in urine data, but it was really a screening technique for ensuring that the air concentrations. So it’s almost the reverse of how we perceive the data for doing dose reconstruction. NIOSH at this point is looking at urine data as the principle means for dose reconstruction and essentially ignoring air concentration data. And here the people whose job it was to essentially monitor people who say we have no faith in it. It’s useless.

Now, I realize there’s still information out there that says we have John Doe’s urine, and it contains 300 micrograms. And if one could reasonably conclude that these assays were done with meticulous precision and analytical protocols that we can, at this point, look at, yes, they’re useful.

But when I read these statements by the very people who were in charge of the program who actually questioned the usefulness of this data, then I have to question to what extent were the technicians informed you will do this based on this procedure. You will do this accurately. You will calibrate your instrumentation, et cetera, et cetera.

It gives me a very less than warm feeling about the accuracy of data when I read these comments that this data is virtually useless. And I bring that up because it's repeatedly stated in these documents.

DR. ZIEMER: Let me ask a question related to that because part of this may have to do with time period. One of the issues on use of data is always the model. Models have changed over the years. We can take the same data now and get much better output than people could in the '50s and '60s.

So I'm sort of asking the context of the statement. Are they saying that we don't trust the data or we don't have models that are good enough to take the data and predict body burden? Which 40 years ago I would have made a statement of that sort, too. I'm trying to get a context --

DR. BEHLING: I agree. It's a little bit of both that obviously they didn't have the benefit of current ICRP models that would say, okay, based on excretion and various assumptions we can now back-fit this and essentially identify what the body burden is and do dose modeling. I agree with you, Dr. Ziemer.

But the question also is if you don't have that level of usefulness, which they clearly did not, then the question is to what extent did that affect the technicians in the laboratory running these assets? And I think you have a combination of effect. They didn't have much use for it because the ICRP models didn't exist.

But on the other hand their limited use may have impacted their sense of importance that will come in the year 2007 when NIOSH will then look at the data and say, you know what, that's the best we've got, and let's use it. The question is did they have that understanding that some day, maybe, some day we would make use of this and we better be very good in doing what we're doing even though we at this point can't interpret it. And I just raise that as an issue.

DR. MAKHIJANI: One additional point, Dr. Ziemer, about that. This is on pages, page 27, 28 of SC&A review. And this is throughout the period. I think the latest document that Hans has cited is from '84 --

DR. BEHLING: 'Eighty-four.

DR. MAKHIJANI: -- where it says, "Excretion urinalysis data recorded, but this cannot be used for calculating internal dose." So it's not post-ICRP-60. But it's fairly recent.

MR. HINNEFELD: If I can offer. This was the historical opinion of the people who ran Fernald who were still running Fernald in 1984. And in point of fact the DOE order which was the equivalent of the regulatory requirement at this time didn't really require you to do dosimetry from your bioassay program, and Fernald didn't.

So the fact that it goes into 1984, I don't think you should read too much into that. The really good models came out in '76, you know, the 30, the real change in the model from ICRP-2 where you could really make some judgments about where the uranium ended up came out in '76. Didn't make it, you know, Fernald by ’84 had not adopted using that and didn’t make it into the regulatory scheme at DOE until I think about ’89.

So this reflects that attitude of with ICRP-2 which is what your requirements tell us to do. We can’t do this. So that’s it. Now, that’s the point. That explains the timeliness of it. Han’s point is interesting is if they felt like this was a screening were they that careful. Were the analysts that careful? I don’t know what exists of the records or of the operations and procedures from that period. I don’t know if anything exists from that period.

MR. GRIFFON: I think that may --

MR. HINNEFELD: It pre-dates me, you know, if you get back before, probably before ’83. I started in ’81, but I didn’t really work in radiation detection until ’83, from that time forward the people who ran the laboratory were pretty conscientious about giving a good laboratory result. Tom Dugan, who ran the lab, is still alive and lives in the area, and they were pretty conscientious.

DR. ZIEMER: Well, you know, even there,

there’s no reason why a technician would suddenly say, well, I don’t have to use care in counting. I go back to the, most of you who have been in Health Physics have done smears, thousands of smears over the years. And we all know that smears have almost no analytical value, but they’re always carefully counted.

MR. RICH: To the second decimal place.

DR. ZIEMER: Yeah, even though they’re simply indicators. There’s no -- it seems to me it doesn’t make sense to say -- we never had this situation where, well, I don’t care what the count come out because it’s not that accurate or something. You always counted it carefully and got your statistics.

MR. RICH: There’s one more issue, too. This is Bryce Rich. In the very early days the urine samples were rigorously and religiously taken because the controls were based on a toxicology basis. They used those, and they restricted the people from the work place on the basis of meeting certain criteria from a toxicology standpoint. They were very careful. And they were used for that purpose.

And the fact that they were going to be used later for radiological determinations was not a consideration for them at that point. They didn't anticipate that they would use them for radiological dose determinations. And so I'm not surprised, as Stu indicates, particularly in later years, they were still expressing doubt that they could be used accurately for dose determinations.

DR. BEHLING: I just raised it as an issue that may define a wider margin of uncertainty with regard to the accuracy of such data.

MR. RICH: And just one more thing. We've talked to professional people associated with the analytical work that was done at that time. They started in '54. They started in '54 at the very earliest, and they are quick to say that they were, they had procedures. They were detailed procedures at the outset, and we're in the process of trying to recover some of those very early documents. That's tough to do, but they had, there were procedural (inaudible) as a matter of fact. So they were very disciplined in what they did -- at least from our interviews -- just yesterday.

MR. CLAWSON: Well, I'd like to bring up one because everybody's brought up something. There's always the human factor in everything, but what Hans has brought up because I know it still today. There are readings that I take that are totally bogus, and they offer nothing to the process. But it's to what point of enthusiasm do I do them. It's like a cast to be able to get out. I've watched them (inaudible) that things many times and take two days to get out of there.

But when we're up against the gun watch them take one swipe and not even count it and you're going out the door. I think this is what Hans is kind of bringing up is when you're taking bogus data, to what level do you really go to. And I'm not saying that they did or anything else, but it's something that we need to kind of think about, too, and what their comments are.

MR. ROLFES: For example, to sort of address what you said if the lab observed an unusually high result, an unusually high urinalysis result, they would have typically prompted that with a follow-up bioassay request to see what the problem might have been and determine whether that first sample was, in fact, valid or not.

DR. BEHLING: This is the bioassay. You mean a second bioassay.

MR. ROLFES: Yes, a second bioassay.

MR. SCHOFIELD: Do you know how often these bioassays were actually done on the workers?

MR. ROLFES: Yes, anywhere from daily, multiple times per day, up to annual for people that were working outside of radiological production areas.

MR. GRIFFON: This gets into --

MR. CLAWSON: This gets into a lot of different things. We could debate this one for about a week, but let's -- Hans, if we could --

DR. BEHLING: And as I said, I don't expect any action things. Just sort of a mental note that says don't always believe everything or assume 100 percent accuracy. Consider the fact that the likelihood is that uncertainty margin is maybe wider than you would like to believe.

MR. GRIFFON: Well, I do see some actions here maybe. I just want to reflect back to the Board procedures on SEC reviews, and one thing that we specify is data integrity. So this gets a little off your finding, but the question of, earlier I think you said that we have yet another HIS-20 database out, uranium data. So I would ask that be one action is that'd be posted. I mean, I mentioned it before, but now that we're capturing all, and if you could just post all that data, that would be very useful.

The other question I think we have to examine to some extent anyway is the issue that comes up at many of these sites from workers that we've heard testify again and again is just the question that you kind of alluded to, Mark, is that, you know, I went in and I had a real high urine sample. And they said, oh, it must have been a contaminated sample. We need to follow up. We'll take a follow up, and that's the one that gets in the record and that high one went away. I think we need to verify that that kind of thing didn't happen. That the data integrity is good from that standpoint.

MR. ROLFES: We have no indication to, there have been reports indicating that, you know, samples could have been contaminated, but we generally see those in peoples' records. I don't believe there's any indication. I don't see proof in front of me that, but it is something we'll take a look at.

MR. GRIFFON: The one way certainly to examine this is if we have laboratory logbooks along with the database and all the records show up in both. Then we're, you know, then everybody's comfortable that those values weren't dropped.

DR. ZIEMER: What year did we start to get other kinds of bioassay, this whole body count?

DR. BEHLING: 'Sixty-eight.

DR. ZIEMER: So that's much earlier than in '84 when people are still not confident. I'd like to see can you cross-calibrate and say, okay, can you confirm -- maybe you've done this -- lung data and bioassay --

MR. RICH: In your comment, when they started to take lung count as a bioassay method, they did establish percent of maximum permissible lung burden for a period of time based on lung counting data and did restriction of workers on that basis in addition to the toxicological determination from urine sample data.

DR. ZIEMER: But they should be able to cross-calibrate those.

MR. RICH: Yes, yes. And they also did their AEC reporting on the basis of radiological issues in terms of maximum permissible.

DR. ZIEMER: I think, Mark, on the integrity issue perhaps at least on those points or those later ones where we have both kinds of data, that would help us. It doesn't definitively speak to the early years, but at least if there's some indication that there's consistency between urine analysis and other types of internal assessments, it would be useful it seems to me.

MR. MORRIS: I transcribed a lot of that data, that lung count data, in order to use it in an electronic format. And there are probably 90 to 95 percent of the people who had unremarkable lung count. There might be five percent or fewer that had many lung counts in the same year, and they were obviously --

DR. ZIEMER: They were tracking something, yeah.

MR. MORRIS: -- tracking some specific intake. I would think it would be completely useless to follow the 95 percent of the people who had one lung count a year.

DR. ZIEMER: Oh, yeah, I wasn't suggesting you track all these people. I would select a few and see if you get correlation between urine analysis and lung data.

MR. CLAWSON: And I think also the procedure for the urinalyses and how they were done, and I know at a couple of the other sites with the earlier lung counts I remember that they used a different type I believe, that come up to be a little bit of a problem, but maybe these are some of the things we may be able to look into on that.

Is there anything else, Hans?

DR. MAKHIJANI: Yeah, I had a problem. I tried to do some of this stuff in relation to

Fernald, and the complication you run into in the lung counting data and correlating it with the bioassay, of course, was the solubility. And they had all kinds of solubility at Fernald, and one thing that I found useful is to take the air monitoring data from a plant and to focus on workers, in the example you're doing, to focus on workers in a particular plant at a particular time so that you have three different pieces of information. And that --

DR. ZIEMER: The urine, the air sample and the lung.

DR. MAKHIJANI: -- and that I believe will give you, you know, within a factor of two, some confidence that you're in the right ballpark. It doesn't resolve all the issues.

MR. GRIFFON: These kind of reality checks.

DR. MAKHIJANI: Yeah, look at this as a reality check.

MR. GRIFFON: Do you even have any kind of air sampling data database?

MR. ROLFES: Database? No, but --

MR. GRIFFON: Do we have raw?

MR. ROLFES: Most of it is raw data.

MR. GRIFFON: So it may be an uphill battle to use that as a comparison.

MR. RICH: Most of the air sampling data is uranium.

DR. MAKHIJANI: Could we ask for the interview documentation also because a number of interviews are being done, and it could be useful for us. I mean, just as an action item.

MR. GRIFFON: That would cover a lot of these. That wasn't just related to this.

DR. MAKHIJANI: No, the prior referenced interviews but also (inaudible) interview.

MR. RICH: And they're all, Arjun. But they will be formally documented.

MR. GRIFFON: Maybe that's a general action item.

MR. CLAWSON: Go ahead.

MS. BALDRIDGE: I would like to bring up the point when I reviewed my father's records, I noticed that he had approximately 55 urinalysis tests done. When I looked at the uranium urinalysis sheet that was provided with his files only 21 of those test appeared on that sheet. I had asked Mark if he knew why they would have been testing and not recording, and he didn't have an answer.

MR. ROLFES: That's correct. Yeah, we did discuss that. And I don't know what Privacy Act concerns I have here Larry about discussing specifics of her father's claim.

DR. ZIEMER: Well, why don't you discuss it in general terms. What would you do in a case, or do you use all the data points.

MR. ROLFES: Within the medical records that were kept at Fernald, there were blood tests that were taken for reasons other than for determining uranium concentrations. There were also urine samples that were provided during annual physicals where they would take characteristics of the urine other than for radiological or chemical analyses. They would take a look at white blood cell count to determine if there was any concerns about the person, if they had any kidney problems which would me like, for example, they may have a urinary tract infection. And in that case they would find white blood cells in the urine. For lead being excreted they would find red blood cells. There were also casts, and based on the different types of casts and specific gravity of urine, they could infer different medical things. Those wouldn't be indicative directly of radiological exposures and wouldn't be used by NIOSH. Those also, I don't believe, are routinely reported to NIOSH; however, the uranium urinalysis results are. That is one of the differences between the medical records that you received as well as the dosimetry records.

FAILURE TO MONITOR ALL PERSONNEL WITH POTENTIAL INTERNAL EXPOSURE TO URANIUM

DR. BEHLING: Let me go on to finding number three, and if you have a hard copy on your computer, I mean an electronic copy, it's on page 28 of the report. And just briefly the finding is failure to monitor all personnel with potential internal exposure to uranium. And in Section 7.2.1.2 of SEC Evaluation Report from NIOSH it stated that nearly FMPC workers were monitored for uranium in urine. No coworker analysis has been deemed necessary for uranium intakes.

So in the context of that statement I looked at some of the documents that were part of the petition, and in one of the attachments that I included was one in which -- this was dated May 13th, 1955 -- and it is a memorandum that was issued that involved urinary uranium investigations and involved four individuals.

And I looked at the data and just as a background urine results that are greater than 0.025 milligrams per liter would, according to the people who were running the program, would suggest that there was a moderate uranium exposure. And at levels of 0.04 milligram per liter these are considered in their terms excessive exposures.

Well, when I looked at these individuals, one of them had 0.543 milligrams which is 13 times higher than what is considered an excessive exposure, and it involves a person that was described as a person who had little or no possibility of being exposed to uranium. And they provide no other information.

And that first question that would come to mind is why were they even monitored, and that is an unanswered question. But under worst-case assumption they may have been monitored as a way of getting control values. Maybe they should have selected spouses of people or members of the general population, but it’s also possible that these four individuals, none of whom had reasons to have any uranium in their urine, may have been asked to submit a sample as a baseline that says, this is what ordinary people excrete based on consumptions of foods that may contain trace amounts of uranium and this is what we may even subtract from those who are workers in order to get a net value.

I have no idea what these people represent. All it stated in the document is that there was no justifiable reason for them to have uranium. Now whether these were people who were exposed to fugitive emissions around the plant from contamination, I don’t know.

But it’s disturbing to me to read that there were four individuals in a single memo that had concentrations 13 times higher than the 0.04 milligrams per liter that is considered excessive. And at this point I have no explanation as to what to do with that data other than assume that they were people exposed who were probably not monitored. That’s my conservative assumption.

MR. ROLFES: Sure. So would definitely in a dose reconstruction, that’s why we are assembling a coworker model for coworker intakes now. And these intakes are, excuse me, these urinalysis data are documented. And so if we have those in a file, we would use those to estimate an intake of uranium. And even if it was a false positive, if we have no information but we have the urinalysis results such as this, we may not know the reason that this high bioassay result occurred, but we would assume that it was, in fact, a valid sample and assign an intake based on these data.

DR. BEHLING: That’s clear for this person, but for every person that was serendipitously diagnosed with uranium in the urine, there may be people for whom there is no data.

MR. RICH: Can I offer some operational experience? It’s not unusual in a large operation when you’re sampling a lot of different people to have some false positives for one reason or another, cross-contamination or a glitch in the laboratory. And then this stimulates an investigation.

And I would interpret this memo as one of those, as an investigation of some unusual air samplings which would normally call for re-sampling and an investigation of the work place. And they say we don't have any idea why this person would have, deliver that kind of a urine sample.

So after re-sampling and evaluation, you go to your laboratory to see if there's contamination or, you know, that would give you an indication how to look at your laboratory. This is not unusual in a standard operation situation.

DR. BEHLING: I'm just looking at the first sentence here that says the following urinary uranium results were investigated first because there were no apparent reasons for the high uranium results. So something triggered this investigation.

MR. MORRIS: An annual physical would have prompted the --

MR. RICH: Everybody gave a sample.

DR. BEHLING: Yeah, but that would be very disturbing to me as a result of an annual physical for people who were not radiological workers who would have --

MR. MORRIS: That is evidence of quality assurance. You know, they may not have called it that contemporarily, but it was evidence of a self-assessment going on.

DR. MAKHIJANI: That might be an explanation. There could also be a different explanation. I think that fugitive emissions at Fernald were very high, and they're not covered by your environmental TBD. We pointed this out in our review of the one that was published. The one that we reviewed.

Essentially, 5.1.3 we talked about thorium fugitive emissions, and this is from 1970. The worst housekeeping problem in the facility was in the mill. Equipment leaks excessively at practically every joint. And they had a kind of bucket brigade over there catching the stuff in buckets. Perhaps they had quality control in taking your example, but they didn't have quality control in maintaining the equipment certainly.

And this is not the only example of its type. In the petition, and I pointed this out several times in various situations for the last two years, that Fernald has the distinction of having had a job that actually was done that had 97,000 time maximum allowable concentration averaged over that job. And in the next year it included the 16,000 time maximum allowable concentration.

This memo is in the SEC petition. Please do look at it, and these kind of operations were into the area of plausibility, could certainly give you plausible high exposures. And it's plausible that it could be the kind of issue we're talking about, cross-contamination and all that. But it's certainly at least equally plausible that it would be fugitive emission exposure, especially -- well, this is a 1970 memo, and we all know that conditions, and there's ample documentation that conditions in the '50s were far from sanitary, let's say.

It's documented very, very amply, and I think the 97,000 time MAC is actually, if I remember right, maybe from around 1960. So this stuff extends into time, and I don't believe that you can assume that non-monitored personnel had less than the average exposures because 97,000 times MAC is an annual exposure in 1.2 minutes.

MR. ROLFES: Once again, for uranium exposures what we are relying on is the bioassay data within the person's file. That's the most important thing that we have. In greater than 90 percent of the people that we have in our claimant population at NIOSH for whom we need to do a dose reconstruction for have bioassay data within their file. And for the unmonitored, I believe it's about seven percent. So seven percent may not have bioassay data, and that is why we are, in fact, developing a coworker intake model to address unmonitored exposures.

MR. HINNEFELD: Do we know if the people cited in that memo are claimants? If any of them are claimants?

MR. ROLFES: The names were redacted when they were provided to us so --

MR. HINNEFELD: So we don't know then.

MR. ROLFES: We would have to take a look to find out whether they are, in fact, claimants or not.

MR. HINNEFELD: I was just wondering. If they were claimants, we would have their record, and we could see subsequent samples to these. But with this level of excretion on a particular day, if you were to take a follow-up sample within a couple days, you would expect an elevated excretion rate on that day as well. So, I mean, there would be a way to evaluate whether this was an excretion, if any of these people were claimants they would be evaluated, whether this was an excretion rate or a laboratory contamination event.

If I'm not mistaken, these samples date from the time when the bioassay was done in the analytical laboratory, the same laboratory building where the process samples were analyzed for the various things analyzed those for. I don't think the Health and Safety building was built until the late '50s. And so that's when the bioassay analysis then moved from the analytical lab to the Health and Safety building which ostensibly was a cleaner environment to do those samples.

DR. MAKHIJANI: Three of these four people were women who didn't have any external dosimetry --

MR. HINNEFELD: Right.

DR. MAKHIJANI: -- in that period, and that's really when I read Hans' report, that is the thing that leaked out of me and that caused me to have a lot of doubt about the questions regarding who was being monitored, what their exposures were, and to stress the idea that the problem of fugitive emissions at Fernald for worker exposure could be much bigger --

MR. HINNEFELD: If I'm not mistaken, women weren't even allowed in the production area at that time. So they would have had to have received this exposure on the, in the analytical laboratory or the administration building or the services building.

MR. CHEW: This is dated May 13th, 1955.

MR. HINNEFELD: Yeah, and I don't remember what date exactly they let women actually go into the production area. I actually know the name of the first woman who did, Marge Kane*.

DR. WADE: Here's an observation.

MS. BALDRIDGE: I think what was disturbing to this memo when I saw it was that Fernald personnel relied on their ability to predict which groups of people were at risk for exposure, and they missed it on these four and how many others. How many others were like these four people but because management thought they weren't at risk, they were never tested or checked.

MR. RICH: They were monitored annually. And if you get a major problem, you're going to see routine non-monitored people show up with high urine. The way I would read this is that this is unusual. This is an investigation of an unusual event and a reporting of an investigation.

MR. SCHOFIELD: The trouble is if these people give an in vivo sample annually at what point did they receive this dose? Was it three months, six months? So then we go and do their calculation dose reconstruction it's like where are you going to set that timeline for their dosage construction?

DR. ZIEMER: Well, we set it at the maximum point. You assume it was a year ago.

MR. SCHOFIELD: Okay, so --

DR. ZIEMER: That's the default assumption.

MR. SHARFI: You have to be careful because with a positive result this big, you're probably going to have some kind of follow up. So you can model what potentially was the intake date by back extrapolating looking at the follow-up samples and the positive and trying to fit bioassay data. If this was the only value that they had in their record, obviously you'd be looking at a much more claimant favorable, I mean, much more assumptions you're going to have to take on when the intake date occurred.

But generally, if you're seeing someone at the 0.5, and obviously this report came out less than a month after they got the sample, the obviously had the ability to turn around and ask for follow ups. So without having the names and actually looking at the records, I can't say there were follow ups, but I'd be highly surprised to see someone who's so much larger than what they consider a significant exposure or significant bioassay result and not see a follow up. And once you have the follow ups, you can use that to back extrapolate what the potential intake date, at least the range would have been that would fit those bioassay results.

MR. RICH: But like Paul says, you'd extrapolate.

MR. SHARFI: Yes, these would be sizeable doses depending obviously the organ of interest that we're talking about.

DR. MAKHIJANI: For purposes of information, do you actually, if this were the only sample would you in practice systematically choose the intake data the day after the sample? Is that what you do in compensable cases?

MR. HINNEFELD: You mean the day after the previous sample? We go all the way back to the previous sample or we go mid-way.

MR. SHARFI: Default is the mid-point depending on obviously scenarios. I mean, you could use possibly a chronic, in a scenario like this it might be you'd have to, I mean, I hate to make generalizations about what I would always do because if this was my only point I hopefully would have more information I may be able to request more information or try to find more information. And there's also information that possibly might be in CATI or something like that. So I hate to, the telephone interview.

MR. RICH: I'll just tell you in the case of plutonium facilities when we got a significant and detectable activity in the urine on an annual sample, we extrapolated back to the beginning period, the year. And it comes out --

DR. MAKHIJANI: In compensable cases.

MR. RICH: Yeah, -- and it comes out a very high dose.

MR. SHARFI: The dose size would be organ dependent.

MR. RICH: What I'm saying, Arjun, is not in the compensable program, but in the period of the operational program when we were determining doses by which to restrict people --

DR. MAKHIJANI: Oh, yeah, no, I'm asking a different question. I'm asking just for purposes because this has been kind of a different confusion, and so I just want. I want my own confusion to be cleared up.

MR. SHARFI: The default if you assume an acute intake would be the mid-point.

DR. MAKHIJANI: Because this came up at Rocky Flats, and it's coming up again. And I've twice put on the record that it is the day after the previous sample and I don't believe that that's correct.

MR. GRIFFON: Certainly not the standard case obviously.

DR. BEHLING: And even at the mid-point, and we'll hear probably from Kathy, my wife, when she discusses some of the aforesaid cases where they used the day before consistently in five consecutive samples that were done. They took the day before of the bioassay as the day of intake. And I raised, that is an issue. I said why don't you at least use the mid-point, and they came back and says, no, because it would be inconsistent if you took the mid-point because a subsequent data point would not fit the observed information. So again, it was again, well, we use the mid-point, but if it doesn't fit --

MR. GRIFFON: That's where they need more data.

MR. SHARFI: The difference of a single point assessment versus having a sizeable amount of data that you can actually, like I said, do the fits, and you can pick the curves. And at that point you want a mid-point that would fit this high point and then show that every subsequent result should have been in this, too.

DR. BEHLING: So it’s a floating value or approach --

MR. HINNEFELD: The intake data is floating depending on the strength of bioassay record. That is true.

DR. ZIEMER: Well, what do we do with this issue? It’s raised the question of these four cases. Can you run more from these four cases? Was this truly a follow-up issue or what?

MR. GRIFFON: I would suggest another action item, that we follow up on to get the identifiers from that memo. See if any are claimants.

DR. ZIEMER: What’s the situation for this?

MR. GRIFFON: If you have the claimant file, do what Stu suggested which was to follow up and see if there was subsequent sampling.

DR. BEHLING: But as a minimum and for all you heard this morning that a coworker model will be developed. Based on my opening statement up front was that this issue was raised by me because the statement I read is that there's no coworker analysis has been deemed necessary for uranium intakes. What this tells us is that people who were perhaps not monitored should be given some assignment and perhaps a coworker model is appropriate for those for whom there is no data.

DR. ZIEMER: Depending on what you learn from --

MR. GRIFFON: The second action item I have.

DR. BEHLING: In a way you've answered the issue.

MR. GRIFFON: The second action was that NIOSH will provide coworker model along with all analytical files on the O drive. I guess I should say as soon as possible because I think you're still finishing that, right?

MR. ROLFES: I'm sorry, the coworker --

MR. GRIFFON: Yeah, the coworker models.

MR. ROLFES: Yes, that's in process.

MR. GRIFFON: And when do you expect to have that in final form?

MR. ROLFES: I couldn't give you a certain date.

MR. MORRIS: It's hard to predict that.

MR. GRIFFON: I know, but we're also up against petitioners, too.

MR. CHEW: It's high on the priority for the RU team to do that.

MR. CLAWSON: I have another question, and forgive me for my ignorance and so forth. You have a pretty good idea of what to be able to do with these situations, but what do the other dose reconstructors, do we have a workbook? Do we have a process that when these abnormal ones come up, do we have a process or procedure to address this? I know some of the other ones we've got a workbook or something like that we can go to.

MR. SHARFI: We do try to take like we had guidelines that we used to try to just kind of help bulletize, make sure that there are obvious points that you want to make sure that you, you know, kind of summarize the site profile. But obviously, the site profile is the leading document. And then we do have, obviously, a support staff. We have a principal internal dosimetrist that you can bring in on any case that is probably a higher level expertise when it comes to either whether it internal or external issues. And a very large work staff, we have site leads that will help answer questions. Dose reconstructors are not only just given a case and said you're off on your own and good luck. We have a whole support staff that built in --

MR. CLAWSON: I was wondering if there was anything of documentation of how when we get this situation how do we know we handle it.

MR. SHARFI: There are internal dosimetry procedures.

MR. GRIFFON: Well, (inaudible) be our guide. They sort of step it through.

MR. SHARFI: An in general assessments, how you do internal dosimetry. I mean, there are procedures that just cover general internal dosimetry. There are separate, it has nothing to do with the site profile. All it has to do with how you do, how you use bioassay or how you look at dosimetry or external and those

IGs and stuff like that.

MR. GRIFFON: As we just discussed earlier, I mean, the DR guidelines first step for this would have been external, environmental monitoring, and now you're using a coworker model. So that's changed already.

MR. SHARFI: And probably the reason why there hasn't been a big push to develop a coworker, just like in the sense of Rocky was, really at the time we almost had no claims that required it. Almost every claim that we had at the time has had bioassay data.

Therefore, when you're looking at resource priorities there's no claims that are awaiting a coworker, not to say that there aren't possible future claimants that are unmonitored. But of the claims that we have to do at this time, they all had bioassay data. So the emphasis on developing a coworker was not as prioritized as other sites that have a larger need for coworker.

MR. CLAWSON: Okay.

DR. BEHLING: I think the next one we can skip because it really addresses an issue that we've talked at length this morning about, and that is what are the assumptions regarding uranium enrichment. And just here I quote one of the comments in Section 5.2.1.1 of the TBD, and there is even a reference to, and I'll quote, "During the following production year after 1964, the uranium was processed in a variety of enrichments ranging from depleted to as high as 20 percent."

Now, I'm not sure I know where 20 percent comes from, but that's certainly a high value. And but we discussed it this morning but it's to the credibility of using a single value, two percent enrichment, for a select worker population who may have been exposed to much higher enrichment quantities.

MR. MORRIS: Twenty percent is the value where it would have become a safeguard facility.

MR. RICH: So they never say 20 percent. It's 19.9.

DR. BEHLING: And this was in your TBD here so I'm just quoting.

MR. CLAWSON: Okay, so I think we've worked that one pretty good, so let's go on to the --

RADIONUCLIDE CONTAMINANTS IN RU, INADEQUATELY CONSIDERED

DR. BEHLING: I'm going to pass the next one on to Arjun because this one involves radionuclide contaminants in RU that are not adequately considered. And I think Arjun can address that.

DR. MAKHIJANI: Well, we kind of reviewed this at some length in the TBD. You put up a slide there this morning of where you said the average plutonium contamination of recycled uranium was 0.9, and you had some other numbers. And the 2003 DOE report, which revised the 2000 report, even though it was partial had higher numbers for the average. Let me see if I can pull up some of the numbers.

So anyway the first point is that I think there's documentation to indicate that the values that NIOSH are using are not based on complete information. And there's information showing that average values are higher and maximum values were higher. The maximum value cited undiluted, unmixed for the Paducah tower ash in the TBD is 412 ppb. I think that's also indicated not to be the highest value. I cited a value of 1,000 ppb.

And there are other values also.

I am not at all sure that any DOE investigation to date is seriously complete and has the necessary information about the levels of contamination of RU with plutonium; and therefore, all the other contaminating materials. But certainly I think there’s documentation to show that the existing TBD is not correct. I mean, maybe I’ll just make that first point.

There are a lot of points in regard to raffinate. I don’t think the NIOSH response in the matrix is responsive at all to the raffinate because raffinates don’t involve radon breath. They don’t involve Radium-226 and isotopic analysis of the silo contents. So the response that NIOSH has given about K-65 raffinate drums, what’s in the silos does not contain significant data on the RU streams and the plutonium and neptunium contamination.

And so far as I’m aware, I have not found any information on the plutonium contamination in the raffinate stream. But I think it is important. There’s something that Stu wrote in 1988.

MR. HINNEFELD: That's really dirty pool is quoting something I wrote.

DR. MAKHIJANI: Let me find it, and actually --

MR. CLAWSON: Maybe Stu could quote it for us.

MR. HINNEFELD: I know what the issue is. The issue is in the refinery when -- the little bit it operated when I was there -- the feed in the refinery were not high in radium. So it's not a radium issue. There was some Thorium-230, a little bit, it's all been, this stuff's all been purified once before. So it's only about Thorium-230 going back in. There's not even very much of that.

But the recycled uranium in the feed may have gone in at ten parts per billion or something or some of it was as high as maybe 30 parts per billion on occasion, would go into the feed, and the refining process would purify the uranium and take impurities out, impurities including these radiological contaminants.

So on the raffinate stream which is the discharge stream from the refinery, you have very, very small amounts of uranium. I mean a little bit did leak through, but most of the uranium went to the product stream. But the impurities preferentially went to the raffinate stream. And so the proportions that were used for feed materials and product materials in order to bracket those numbers can’t really be applied to raffinate numbers because the uranium’s all gone.

And since you’re basing on a ratio of say plutonium to uranium, uranium’s, that ratio goes way up. And as I recall, we approached control on the raffinate on essentially a mass basis. You know, it was not very radioactive at all because uranium’s pretty much gone. You’ve got a little bit of contaminants. It’s not very radioactive at all, but the components were there was not uranium, and you couldn’t really scale on uranium.

So I think the issue might be if bioassay here is depending upon uranium bioassay, that person’s exposure environment is raffinate, you know, he was exposed to raffinate, then uranium bioassay and the kinds of ratios that you're using for plutonium to uranium that are based on feeds and products, those ratios aren't applicable to uranium bioassay in a raffinate exposure environment.

DR. MAKHIJANI: I could not have said it better. So this is exactly --

MR. HINNEFELD: That scares me so much.

DR. MAKHIJANI: -- that's exactly the point.

MR. CLAWSON: Ray, you've got that written down. They agreed.

DR. MAKHIJANI: And you said this back then.

MR. HINNEFELD: Yeah, I read what I said back then, and I couldn't think of a reason to say something different today.

DR. MAKHIJANI: And the whole NIOSH analysis is based on the ratio. And so far as I know, I mean, Stu, are there any measurements --

MR. HINNEFELD: Well, I haven't participated in this product because I'm conflicted at Fernald.

DR. MAKHIJANI: But just from your knowledge.

MR. HINNEFELD: There are some measurements of raffinate materials that were collected in circa '85 give or take a little bit timeframe.

And there are some evaluations of those ratios in raffinate materials. I don't know how extensive they were, but they were out there, taken during that '85 period.

DR. MAKHIJANI: And how would you, so the question is recycled uranium comes to the site around 1960. You've got 25, 29 years of processing, however many years there were, and you have one data point for raffinates in the mid-'80s. Now, I haven't seen that data, but this is exactly the issue that we had at Mallinckrodt when NIOSH said, okay, actually uranium bioassay data are not suitable for this kind of situation, raffinates, and went to radon breath data.

But there we didn't have the complication of trace contaminants with transuranics. There we were only talking about everything that's in natural uranium, and so we had a different situation. Here you have a more complicated problem I think where characterizing these raffinates is going to be much more difficult. And if uranium bioassay is not suitable, the question is what's the data that's going to be used in order to reconstruct these doses.

So there's two problems. One is the RU data itself and the feed material characterization, which I think is inadequate, at least so far as we reviewed it. And then the raffinate problem which I think is actually a more serious problem.

MR. ROLFES: One thing to keep in mind is that the extraction of the raffinates was a wet process, and it was also enclosed in process piping. We have found some confirmatory air samples to indicate that the measure to air concentrations were relative low. I know we have reviewed multiple samples. The total number in years off the top of my head I couldn't provide to you at this time.

I believe we're going to be addressing additional exposures to recycled uranium contaminants within our updated internal dose technical basis document which we're in the process of revising at this time.

MR. GRIFFON: Would you have a way to identify raffinate workers or workers that were in that? I'm assuming it was only one area of the plant, right?

MR. RICH: It's Plants 2 and 3, and Plant 3 was the, and the raffinates were not just raffinates. There were hot raffinates, and there were cool raffinates --

DR. ZIEMER: Radiologically or thermally?

MR. RICH: Radiologically.

MR. HINNEFELD: Radiologically.

MR. RICH: The hot raffinates are high in Radium-226. The cold raffinates are just other trace materials and fundamentally natural uranium that came and was processed through Fernald but had already gone through the mill operation where the daughters were removed. And so as a consequence, the cold raffinates had very little uranium daughters and essentially cold in comparison with the first-time pitchblende ores.

And there's a little twist that we've discovered also, and that is it turns out that the primary recycled uranium that was received at the plant came from Hanford, as you agree, did not go through the plant. It went directly to Plant 4 and was blended there. And so there wasn't a concentrating mechanism for a good share of it.

Scrap materials from the processes were then processed through the plant, but that is a reduction in the total amount of recycled uranium contaminants that actually went through the extraction plant.

MR. MORRIS: That's only ten percent by mass they said.

MR. RICH: And as a consequence, as Stu indicated, the contaminant levels sampled at a much later time were low, but where we are developing with air sampling and with improved knowledge of material flows a default. Right now we're defaulting at 100 parts per million for everyone in the plant based on uranium.

MR. HINNEFELD: One hundred parts per billion.

DR. BEHLING: What's a thousand --

DR. MAKHIJANI: I guess you put all the raffinate issues on the table. I would like comment on the recycled uranium raffinates, but since you have discussed all of the --

MR. RICH: This Board is going to be considerably upgraded in upcoming technical basis document.

DR. MAKHIJANI: But the one comment I had about the cold raffinates that were from ore concentrates that were processed at Fernald. The radium was left behind at the concentrating plant, I agree. And those wastes were sent to Silo 3. If you look at Silo 3 data, you see that the Thorium-230 content at Silo 3 is very high relative to radium. I think I have the data right here.

MR. RICH: It becomes the controlling --

DR. MAKHIJANI: Yes, but the thorium in Silo 3 averaged 51 nanocuries per gram and the radium's only about three nanocuries per gram, almost 20 times bigger. And there's a lot of reliance on that silo isotopic ratios, but I think that's easier with the pitchblende because you know pitchblende is a better characterized material.

I think ore concentrates came over a period of time, probably from different places and different mills and different ores. And I think the Thorium-230, Radium-226, uranium ratios would not be expected to be constant.

So from Silo 3 characterization to have an average ore concentration, ore concentrate processing information that I don’t, that I think would be applied to a population of workers, I haven’t seen anything that applied it to an individual worker.

MR. RICH: And as a general rule, Arjun, if you take the analytical data in the silos from a later time, you’re going to maximize, it would be claimant favorable because the long-lived isotopes are going to increase in ratio.

DR. MAKHIJANI: No, no, I’m only talking about radium and thorium where the (inaudible) doesn’t enter into it because it hasn’t changed in the time period that we’re talking about and --

MR. RICH: But when you compare to gross alpha activity, for example, then the ratio on the air sampling data, and we have some air sampling data that we’re going to be folding into this analysis.

DR. MAKHIJANI: I guess I’m maybe not being clear. As I understand it these ratios are to be applied to urinalysis data. That’s the preferred method of dose reconstruction if you have a certain isotopic ratios, and to calculate the radium and Thorium-230 doses, you're going to apply uranium and then use these ratios to calculate the intakes of radium and thorium.

MR. RICH: I'm going to do it a little bit differently on that because of the fact that the uranium, it really doesn't. You can't do a ratio for exposure in the plant areas.

DR. MAKHIJANI: I understand for the pitchblende workers and the Silo 1 workers you're using radon breath data. But I don't think that is a, my focus in making this comment on the cold raffinate is that radon breath is not relevant to that.

MR. RICH: Correct, it's true.

DR. MAKHIJANI: And so, well, let me just pose a question. How are you going to identify the workers who worked with ore concentrates, and how are you going to assign a Thorium-230 dose to them?

MR. ROLFES: That will be based on the information that have within our technical basis document. It's still in draft form; however, as Bryce as mentioned, we do have air monitoring data associated with those processes.

DR. ZIEMER: And you can identify where the people worked in these cases or not?

MR. RICH: According to the managers, Plant 2 and 3 have an up and down period of time. They didn't operate full blast for the whole period, and so during the peak of operations they had about 100 people that were operating that plant. Can we identify the individuals? I doubt it.

MR. CHEW: Sometimes.

MR. SHARFI: Yeah, sometimes. It kind of depends on what's in the claimant files.

MR. RICH: And what period of time.

MR. CLAWSON: We have a comment here.

MR. BEATTY: Yeah, just a clarification on those work assignments as a former worker. That, yes, people back in the early years were assigned a building normally, and those, especially chemical operations, and that was for security reasons. However, maintenance was a different ballgame. They had an assigned building, but then on the, like overtime, they moved around, all around.

MR. RICH: And that's because of the ebb and flow of operations at Plants 2 and 3. And that changed because of the fact they were shut down.

MR. BEATTY: Yeah, it seems like the metal side would get a peak time where they would be more active than the chemical side; you're right. However, the time that the people put in those buildings, I think there should be a point of emphasis made in the interview process to emphasize how important it is to capture all the buildings they were in.

DR. BEHLING: I think that's one of the findings that we discussed. Hopefully, we'll get there. It's the issue of associating people with specific work locations.

MR. GRIFFON: Can I ask? So I know we're waiting for this and that's an action I have here is an update on the site profile, but did I just hear you say that the cold raffinate, the answer is going to be based on thorium air sampling? Are you going to default to air sampling data?

MR. RICH: It would be dose air sampling.

MR. GRIFFON: Instead of any kind of ratio.

MR. ROLFES: Yeah, the ratio wouldn't apply in this scenario because of the low concentration of uranium associated with the silo.

MR. RICH: It just doesn't fly.

MR. GRIFFON: So we're waiting on thorium air sampling data. I mean, we don't have that either, do we? Do we have that posted anywhere?

MR. ROLFES: We've got quite a bit of thorium air sampling data, and I know I haven't reviewed all of it.

MR. GRIFFON: But, I mean, is it in a --

DR. MAKHIJANI: Two thirty?

MR. GRIFFON: -- spreadsheet format or is it in a --

MR. ROLFES: Yes, yes, we do have 230. We do have thorium air sample data that is directly associated with this raffinate process. We also have gross alpha analyses for thorium. So we'll get that posted.

DR. MAKHIJANI: Thorium-230 is --

MR. ROLFES: Yes, there are air samples labeled specifically as Thorium-230 at Fernald.

MR. GRIFFON: Is this in spreadsheet format or --

MR. ROLFES: No, these are not transcribed yet I don't believe. These came out of the data capture that was conducted at the federal records center in Dayton, I believe.

MR. GRIFFON: I mean, I would suggest that these things be posted even if, you know, even before the site profile finished so we can have a chance to digest this.

MR. ROLFES: I understand. There's just an overwhelming amount of data. And some of these documents may not have been named Thorium-230 samples yet. They may still have like a, you know, several numbers, and I 'd like to try to organize them a little bit so they're presentable so that you can look through and find them in some reasonable manner, I guess, without hunting.

MR. RICH: As I'm looking.

MR. ROLFES: Yeah, yeah, true.

DR. MAKHIJANI: May I make a request in that regard? Did the documents have some kind of brief on this title because a lot of Fernald documents references for the evaluation report that are posted just had numbers, and that makes a review extremely difficult.

MR. ROLFES: I agree. I couldn't agree more.

MR. RICH: We can agree with you on that, Arjun.

MR. CLAWSON: That's two today.

One other thing I'd like to make a point. I may not mean anything, but, Mark, you made a comment that the raffinates were in a liquid form and so that it wasn't quite as much of a problem. Be sure to remember this is a maintenance process. We do have a lot of leaks in the process that dry themselves out.

Usually where the leaks at, it's also by air moving systems or whatever else like that. So just because it was in the dry form, and I look at from a maintenance standpoint because even when you take one of these systems out or so forth like that, you've got to dry the system out before you can get in there. So now you're getting into a whole other issue that now it is dry and airborne.

MR. RICH: And then part of the raffinates, Brad, also were extracted on a rotor-to-jump filter and knifed off, and that was dried into a filter cake and then airlifted. So it was a dry material when it was actually went out to the silo.

MR. CLAWSON: Well, the --

DR. BEHLING: The next one is also one that I'm going to defer to Arjun, Finding 4.1-6.

DR. MAKHIJANI: We may have covered that. Yeah, I think we've covered that in, this is sort of a feed material for RU data. We could review. And I think I've given you all the references that I have.

MR. GRIFFON: And I assume, in capturing the actions, I assume that that's going to be captured in the site profile update, right?

DR. BEHLING: Yes.

K-65 DEFAULT MODEL

The next one is on page 36 of the report and it addresses the issue of the K-65 default model. And my statement that you will see in the matrix is strictly defined as the K-65 default model is inappropriate. And I analyze that. In fact, if you look through the TBD it is heralded as a very claimant favorable model. And from what I gathered this morning, it is a model that will not be used in the future or will it be used?

MR. ROLFES: No, this is also one of the changes that has taken place as well. Would you like to --

DR. BEHLING: I will go through as to what I believe were some serious flaws to it that are clearly not claimant favorable because it's based on, to a large extent, external doses. Here we're trying to assess internal exposures, and we're trying to contain the internal exposure model by means of external doses that were monitored.

And if you go through my write-up, you will see a series of assumptions that are clearly not appropriate in terms of confining it to a certain period of time based on administrative dose minutes that were imposed, et cetera, et cetera. And you end up with a six-week period which is clearly inappropriate.

And I question, for instance the whole issue of a three shift. I know that there was a document that references three shifts, but it may very well have been people who work with raffinates that were being processed at Fernald as opposed to the 13,000 drums. I have a difficult time in getting to believe that there were people staying an extra conveyor belt, shoving the contents of drums onto a conveyor belt that's being lifted up into the silos in the middle of the night.

I mean, it makes no sense. And so this whole model as far as I'm concerned is based on assumptions that I cannot agree with. They're broad assumptions, and assumptions that are counter-intuitive.

MR. RICH: A number of things. You're right. It turns out that some of the drums were slurried, taken to a location, slurried and transferred out the dumping-off place and was carefully monitored. It was monitored for gross alpha. And then, of course, that was the basis for the original default in the original technical basis document. We're modifying that now, but that data is still available in terms of actually bounding, making sure that it's bounding. So they're sampling radon breath analysis.

MR. ROLFES: Yes, NIOSH feels that this may have been an SEC issue, but because the additional data that we have located, this has allowed us to basically supplement our approach for dose reconstruction. And we feel that it's no longer an SEC issue based on the additional data that we do have because of the radon breath analyses, the air sample results and updated information.

DR. BEHLING: So you're not going to use this model I take it. Because like I said, I find faults right down the line, and I identified each of the elements --

MR. CHEW: We're not going to use the model that was in the environmental.

MR. RICH: Hans was saying that he had a problem with the breath analysis --

DR. BEHLING: No, no, I have problems with the assumption that, for instance, the period of time was restricted to ten weeks, then to six weeks, and it was all based on external doses involving 13 of the 22, and ultimately there were dose restrictions or administrative dose that don't fly with the data that I have that says during that time it was 300 millirem per week, and 15 millirem per year, et cetera, et cetera. And so all these assumptions that are artificially introduced here to reduce the time period for exposure had no scientific basis.

MR. RICH: Probably a waste of time to justify the original technical base document that we're not going use that precisely. We may use some similar analyses but not those, so we'd probably just drop that.

DR. BEHLING: Okay. If the new model is a facsimile of the old, I would certainly want to look at it again because there were just flaws after flaws after flaws introduced.

MR. RICH: Well, it appeared at the time that it was going to be a conservative default.

DR. BEHLING: Well, it is in my estimation anything but conservative.

MR. CLAWSON: You know, one of the comments that was made here was the closely monitored and so forth like that. Have we extracted any of the DOE reports on Fernald? I'm talking like Tiger Teams and reports. The reason I bring this up is when we were here in Cincinnati and just starting into Fernald, I know that several of the former workers and so forth questioned that I know that Fernald was beat up very, very severely for a very poor Health Physics program or RAD program or whatever like that. A lot of stuff came into this.

I’m thinking even in the mid-‘80s there were some reports that were put out of this. So are we gathering any of this? Because one of the petitioners -- well, not petitioners, but one of the former workers made the comments of DOE coming in and totally having to reconstruct or re-put together their RAD monitoring program because of fallacies in it.

And I guess the point that I’m trying to get to is we’re basing everything off of this. We’re basing that all this information is in there, and if it’s flawed data, you know, this is all like a big computer. If you put good stuff in, you get good stuff out. You put garbage, you get garbage back out. And I just wanted to see are we addressing any of the reconfigurations of their air sampling programs for flaws. Are we looking at any of these DOE reports, the Tiger Teams, the so forth like this? Because I know they got ate up pretty bad.

MR. ROLFES: Sure. For example, like the bioassay data, the urine samples were evaluated using a document and accepted practice. Air samples as well were taken based on a document, documented in procedural practice.

These weren't things that were new to Fernald but had been around since the '40s. Many of the procedures for evaluating worker exposures had not just been invented at Fernald. They had been carried on from a previous experience, for example, at Oak Ridge. And there may have been some shortcomings and a control of contamination and things, but the records that we have received, we had no indication that the records are suspect or falsified if that's where you're --

MR. CLAWSON: No, I'm not --

MR. HINNEFELD: But your question hints to there were Tiger Team reports which are, I think, goes late '80s. There was a report written essentially at the end of the NLO year, which would have been '84, '85 that took to task pretty significantly the radiation protection program, and how we pulled out those reports and said of these findings that were identified in these reports, do these relate to this data that we intend to use. I mean, do they impeach the bioassay data. Do they impeach the dosimetry data? So that's the question is can we go get those reports and make that evaluation.

MR. CLAWSON: And the reason I bring this up is because I know it's stated in public comments many times about this. And I want to make sure that the former workers and so forth that we are addressing these issues, and would pull up, and I'm just roughing off what was said, but it was clearly portrayed to me that they had a new way of mossing, let's put it that way, because of a flawed process. And I just want to make sure that we're looking at that.

MR. GRIFFON: Stu, what were those report references again?

MR. HINNEFELD: Well, the Tiger Teams were the late '80s.

MR. GRIFFON: The Tiger Team, and then what was the other one?

DR. ZIEMER: Well, it was in the early ‘90s --

MR. HINNEFELD: I’m going to say -- was it early ‘90s? Okay, early ‘90s for the Tiger Team.

I’m going to say there was a report called the Gilbert report. Gilbert was the author, and that was written -- have we seen that?

MR. ROLFES: Off the top of my head I don’t recall seeing it.

MR. HINNEFELD: If I’m not mistaken, the Gilbert report was written, would have been probably been ’84 or ’85 that sort of assessed NLO’s operation of the Fernald site. And I believe it was pretty critical. I remember it being pretty tightly held when it came out. I mean, they didn’t just show it to everybody. And so that contained a lot of these comments.

I mean, the early-on comments I think it led largely to contractor change. You know, DOE’s recognition of how Fernald or how NLO was operating at Fernald’s plant, particularly, you know, probably health and safety, but probably other things as well, led to re-bid of the contract. Up until then they'd always just re-awarded it to NLO. It led to a re-bid of the contract and change of the contract.

So that's the kind of report that's being asked about here. And so I think it's our responsibility to make sure we've looked at those documents and see do any of these findings affect how we consider this data that we're relying on.

DR. WADE: Does it impeach any of the data that we're building --

MR. CLAWSON: And you've got to understand from our standpoint, as a Board member I'm tasked to assure that the data integrity is good, and this is why I'm bringing this up.

DR. WADE: So it would be wise to get that report posted and then offer an opinion as to whether the data foundation is impeached by it, but let the Board members and others offer their own opinion.

MR. CLAWSON: Well, and also, if that can be put on the web because I know it has come up several times at the site. I want the workers to realize that we are looking at this, that just because they've made these comments that we are trying to address them.

MR. ROLFES: NIOSH takes, you know, we are, I believe, very responsive to workers. When workers -- I know I started off doing telephone interviews with several workers, and if they had something on their mind, they'd tell you. We didn't just ignore these issues. We do consider these issues.

These are public documents and workers' input is important to NIOSH so we do take these issues seriously. And we want to make sure -- we're getting into great details with each, with these discussions, and we want to make sure that we are adequately addressing any corporate concerns or issues. And I want any workers that are on the line also to make sure that if they have questions about what we're discussing, we will be happy to spend as much time as we need to discuss these issues with them.

MR. CLAWSON: And I understand that, Mark, and I've never in any way questioned NIOSH's or anything else integrity. This is just one of the things that kept coming up to me in reading this report here and stuff like that. It really didn't address anything like that, and I wanted to assure that we're looking at that because there was a change in the process. There was a changing of the guard, and there was a changing of the guard for a reason.

DR. MAKHIJANI: There's also volume 4 of the Westinghouse Transition Report --

MR. HINNEFELD: Yeah, there is the Westinghouse Transition Report.

DR. MAKHIJANI: -- that covers this. I referred to it, but it should be accessible to you.

MR. HINNEFELD: It should be.

DR. MAKHIJANI: I have a copy of it if you don't have that.

DR. BEHLING: I think the next finding again is something that Arjun will address, Finding 4.2-2.

DR. MAKHIJANI: I think we've covered this. I mentioned that the cold raffinate question was a separate item. This is the cold raffinate item basically. The Thorium-230, basically, the radon breath analysis is now going to be, leaving aside the question of adequacy of radon breath analysis for where you have radium, it's now going to do the job for the cold raffinates. And so I think we've already discussed that, and you're going to present a different method for that.

RAC 1995 REPORT

DR. BEHLING: That brings us to Finding 4.2-3 on page 47. And I think again this may be an issue that you can resolve fairly quickly, but my concern, or my finding, really addresses the RAC 1995 report and the model that came from it. In that report it was stated that about five -- and I quote it here in the report. It says, "During the 1953 to 1978 period, five to six thousand curies per year of Radon-222 were released from the silos," and so forth.

And I looked at that, and that translates to 15-to-20 curies per day. And I looked at the actual radionuclide mixture that were categorized for Silos 1 and 2, and specifically I looked at the Radium-226, the Polonium-210 and the Lead-210, and looked at the ratios. And I realized the degree of dis-equilibrium, and then I also looked at the total quantity. And then on the basis of mass balance, I calculated probably a release of closer to 90,000 curies per year.

And that's strictly based on the fact that Lead-210 would be there if radon didn't escape the silos. And I believe the difference is that there was no dome cap for a long period of time until the '80s that would have retarded the escape of radon. And so based on first principles and simple mass balance, I calculated a value that's ten to 18 times higher than that assumed by the RAC Report. And I just throw that out as an issue that you may want to look at.

MR. ROLFES: This is another issue that we don't believe is an SEC issue at this time. We're also revising the environmental internal dose or the environmental section of the technical basis document. And we've also adopted a new methodology that will be detailed in this technical basis document revision.

And this is part of the Pinney Report

that was conducted, and let’s see. I guess, I’ve spoken with Susan Pinney once or twice regarding this model, and it basically is employing very claimant-favorable assumptions regarding potential worker exposures. And I believe she basically has modeled worse-case scenarios essentially for workers where there was uncertainty where the worker was, in fact, working at the plant. Now, her model incorporates radon emissions from the K-65 silos as well as from some of the other areas such as the bins, I believe, that was the Q-11 source term, the bins that were outside of Plant 2, 3 if I recall.

MR. BEATTY: Ore silos, too, Mark?

MR. HINNEFELD: They call them the ore silos. They were up on the side of Plant 1.

MR. BEATTY: South of Plant 1.

DR. MAKHIJANI: Do you have the interviews with Dr Pinney documented?

MR. ROLFES: They were short interviews. The documents which she provided to us have thousands of data points, and we can definitely make that available to the Advisory Board as well. So I would have to take a look. I spoke maybe ten minutes with her on the telephone several months back, and I didn't --

DR. MAKHIJANI: No, no, it's not, this isn't some kind of pro forma thing. It's just if there's, if the information she gave you is contained in a document so the (inaudible) that she did.

MR. ROLFES: Yes, yeah, we do have several reports, and we have a slideshow that she has prepared. There's quite a bit of information that she has provided to us.

DR. ZIEMER: The bottom line is that you're not using this model any longer.

MR. ROLFES: Correct.

DR. ZIEMER: And there'll be a new TBD out that will cover it.

DR. MAURO (by Telephone): Mark, this is John Mauro, just a real quick question. The model as I recall that was used in RAC basically measured the radon concentrations in the head space of the silos, and then it had a way of predicting diurnally due to pressure changes from day to night, venting through cracks in the silo as means of coming up with that 5,000 curie number. So that method is no longer being used. Is that correct?

MR. ROLFES: I believe the differences in emissions is accounted for based on the shift that the worker was onsite. I believe those considerations were evaluated in Dr. Pinney's model. That methodology -- excuse me, the original ORAU Technical Basis Document methodology though, based on the RAC Report, is not going to be used at this time.

DR. MAURO (by Telephone): Okay.

MR. CLAWSON: Ray, did you have something you wanted to --

MR. BEATTY: Yeah, I'd like to make a comment first of all. The mention Hans makes of the levels of radon emissions coming from K-65, yeah, they did diminish greatly when the bentonite* clay was applied and the berm was put around the silos from the cracking. In the later years there was even other processes done, as in the remediation years for foam, a foam spray was applied and the manholes were double-sealed and various things.

My point is that there was still a large amount of emissions coming off the silos, and as late as '96, and I have this documented on calendar, that we were warned to stay indoors on a certain day due to high levels of radon.

MR. ROLFES: Yes, during certain atmospheric conditions when there was an adiabatic inversion, that's what it was called. Basically, when the clouds dropped down really low, and basically you can see a ceiling, a very low ceiling of clouds. The radon that was being released out of the silos would, in fact, be trapped down below that cloud layer. And so there were some times when the radon concentrations did not dilute as rapidly as normal. And so, yeah, that is a good point, so I'm sure you're right.

MR. BEATTY: If I may, just as some help to the Board or working group especially, I have a copy of this Pinney's Report and the Q-11, K-65 studies as well as a letter personally from Dr. Pinney as to the findings. If they'd find that beneficial, I'd sure be able to supply that to you.

DR. MAKHIJANI: If you have a copy here, maybe you could just get it done at the hotel during the lunch break.

DR. ZIEMER: Is this what you were referring to already or is this something that --

MR. GRIFFON: It might be similar or it might overlap, but --

MR. BEATTY: It's the actual presentation by Dr. Pinney. It's the one that showed the peaks of the two and three area. It showed like a CAD description, time to its higher, yeah, I'm talking about the smoking and radon.

MR. ROLFES: It's probably the same thing or very similar to what --

MR. CLAWSON: Why don't we at least take a look at that?

Due to the time right now --

MR. GRIFFON: I just have something before if you're going to break for lunch or something. I'm trying to track these action, and I noticed that on Finding 4.2.1 in the matrix you have ORAUT-TBKS-0017-5 revision in draft?

MR. ROLFES: Uh-huh.

MR. GRIFFON: And then the one we just looked at is -4?

MR. CHEW: Environmental.

MR. GRIFFON: Environmental, that's the environmental section, okay. So they're both updating drafts of the -- all right, I wanted to make sure I had the numbering right. That was it.

DR. MAKHIJANI: Are you going to read the action items now or later?

MR. GRIFFON: I'll read them all later. I mean, I only read those first ones from the first finding, so we've had several more.

MR. CHEW: Within our team we're having a constant battle within ourselves because the environmental TBD was to try to address ambient environmental exposure. This is really a worker that's working outside. And so does it really fall into the internal side or is it more fall under the environmental side? We have lots of data in the environmental report, and so I think I'm trying to make a decision right now how to word --

MR. GRIFFON: I guess that's why I was confused because I thought it was, could have been the same one.

MR. MORRIS: Is the environmental section going to be this big or this big?

MR. CHEW: And also a person working outside next to a silo --

MR. GRIFFON: I thought at first it was a typo, maybe that's why.

MR. CLAWSON: After they had the fire with the release out of the stack, whatever you want to put it to, wasn't there an outside group with Fresh or so forth like that, that did actual monitoring outside of the Fernald site? Mark, wasn't there an independent group that pulled air sample data?

MR. ROLFES: There may have been. I know that Fernald employees didn't travel offsite to take measurements. Back in the early days we have documented air samples from distant locations --

MR. HINNEFELD: I think the State Board of Health.

DR. ZIEMER: The State Board of Health.

MR. HINNEFELD: I think the State Board of Health does the sampling. ASTDR, the agency for --

MR. ELLIOTT: ATSDR.

MR. HINNEFELD: ATSDR, Agency for Toxic

Substances and Disease Registry.

DR. ZIEMER: Didn't they get involved somehow --

MR. HINNEFELD: They had a citizens' advisory group. That was related mainly to exposures to the neighbors. ATSDR was mainly (inaudible) by exposures to the neighbors to evaluate those, and the (inaudible) came out of that.

MR. CLAWSON: Well, I just wondered. It might be a problem.

MR. HINNEFELD: Yeah, I don't think they took any samples. I don't think the ATSDR took any samples.

MR. ELLIOTT: They did not, and it was NCEH that had an advisory subcommittee out there. And NCEH looking at pathways out there.

MR. CLAWSON: I was just wondering. I know that it was addressed at one of the meetings that we looked in the comparing it to what the actual site profile was. I was just throwing that out for an informational thing of if we have compared this to anything that was --

MR. MORRIS: Well, at some level it's probably not productive. You know, the fence line access is pretty far away from the operational cases. And turbulence and assumptions about air sample location all make that a hard to compare dataset. Maybe you could have found it in some comparable data for a very high emission action that both were monitoring at the same time, but those are going to be rare to actually find comparable data I think.

MR. HINNEFELD: Yeah, you should ask John Burn, works for the ORAU team. Ask John Burn if he knows about whether the State Board of Health did that sampling, and, if so, where would that data be.

MR. CLAWSON: Okay.

DR. WADE: Ready for lunch?

MR. CLAWSON: We're ready for lunch.

DR. WADE: You want to go 'til one?

MR. CLAWSON: Yes, if you would, please.

DR. WADE: For those of you on the phone we're going to break for lunch. We're going to break the phone contact. We'll call back in about five minutes before one. Thank you.

(Whereupon, the working group meeting took a lunch break at 12:02 p.m. and returned at 1:05

p.m.)

DR. WADE: We're about to go back in session. John Mauro, are you with us?

DR. MAURO (by Telephone): Yes, I am.

DR. WADE: Good, okay.

Brad?

MR. CLAWSON: Hans, if I remember right, we were stopped at 4.2-2?

INTERNAL DOSE ESTIMATES FOR THORIUM

DR. BEHLING: Yeah, we were up to page 49 of the report which starts with findings associated with internal dose estimates for thorium, and in those couple pages I provide some background information and introduce the assumption about the model that had been identified in the original TBD.

I'm not sure it's still, it's a model that is expected to be used. But the model involves a hypothetical intake of 1,050 MAC hours that was derived -- and I won't go into the details. You can quickly scan through. It's on page 50, the report, what that particular model was based on.

And if you go to page 52, the report is really the first finding. And I wanted to just kind of look at the basic limitations that you experience when you rely on air monitoring data. And I brought up in that particular finding a study that was done at NUMEC that was, that compared the lapel air sample to general air sample data, and just to show that there are severe limitations associated with air sampling data, specifically general air samples.

And on that graph you will see obviously the ratio between breathing zone air samples and general air samples. At the point where you start to look at that it’s the MPC level, you realize there’s a 70-some old discrepancy meaning that the BZ air samples will underestimate -- or the general air sample will underestimate a BZ air sample. And that’s just to give you an understanding of how rapidly an air concentration can change when you have very questionable source terms.

Obviously, if we’re dealing with a nuclear weapon test like at NTS, the source term may be ground zero, and if you’re down wind by miles, the difference between position one that may be a few hundred feet and position two, is not going to be affected. But when you have a very, very discrete source term, even five feet, ten feet can make a monumental difference.

And that is expressed in one of the examples that I cite where I think they took a sample six feet from a locations and it was a factor of five lower. But in this particular finding, 4.3-1, I also talk about the difference in air sampling that I looked at over a period of time and space, in time and space. For instance, in Attachment 4.3-1A you will see multiple samples that were taken at a single location, a single location and probably in a rapid succession.

And on page 55, for instance, in that attachment you’ll see on the top page there were three samples taken. And among the three samples the high was 4,400 DPM per cubic meter, and the low was 170. And so you see in a single location over probably a very, very short time this huge difference that you can get in terms of air concentration.

And I provide multiple examples that involve differences in air concentrations at a single location over a very brief time over a period of weeks, over a period of months or years, et cetera, et cetera, for common locations. And you get to understand the difficulty in trying to assign a single value to a person even when you understand what his job was and where he was stationed.

And this is just a series of examples that I bring out here that defines the variability. We’re not talking percent value; we’re talking orders of magnitude values that will differentiate an air concentration.

In fact, one of them was curious where -- I think it’s on page, I’m not sure. This is Attachment 4.1-A on the second page. I have actual values that are given in increments of minutes. And for this one was the location of west separation booth area, and you’ll see air concentrations taken at 8:35, 9:05, 9:35, 9:50, et cetera. And you will see all of a sudden air concentrations that go from 42 to 333 to 140,000.

You obviously realize that it’s a question of when were these spot samples taken that will define a person’s potential exposure to a certain air concentration. And I realize that at this point we're looking air concentrations as the principal means of doing assessment of thorium exposures. And these particular attachments highlight the high degree of variability that you have to deal with in trying to define even when you do know a person's job, and you also know where he was actually located in a given facility.

MR. ROLFES: We are aware of the uncertainties associated with air sampling, but we feel that these uncertainties result in claimant-favorable intakes basically significant as overestimates in internal deposition. Given the fact that we're not using any respiratory protection factors we've actually taken both breathing zone samples and general air, general area air samples.

We've combined those basically to increase the data spread of the values. We're using a distribution of those values to assign worker intakes based on an atomic weapons employer thorium intake model with information that has been analyzed by year for Fernald.

Do you have anything to add to this?

MR. RICH: That's what I understood within the Health Physics community. As a matter of fact the number of reports, the little research and development little group I had did it one time. We took the breathing zone samples, the lapel samples on both lapels of a guy doing a (inaudible) cut of a, and the difference in the lapel sample breathing was a factor of five.

And that's the reason why AEC/DOE policy was that you would never use air sampling results as the primary result if you had anything else. Now what we're talking about here is that you can be high as well as low in estimating results from samples here or there. You can be sampling, and so over a long, a database, a large database of air samples, particularly if it's a lognormal distribution, and then default at the 95 percent level, it's always going to be conservative, always going to be conservative.

And then one other thing. Based on long experience in the field we would take urine sampling, for example, based on the fact that there could be an intake based on air sampling data. And I don't remember a case where -- well, I shouldn't say a case. Occasionally there would be a case where you find urine activity that would be above what you'd predict with air sampling results.

But at the 95th percent level, it would be, the air sampling results would predict uptake way above what was actually demonstrated by bioassay. So we're aware of that. That's all I'm wanting to say.

DR. BEHLING: Well, the concern was also stated on context with the 1,050 MAC-hours as a model. And I'm sure you've looked at the attachments. There were a couple people who were cited for the air concentration, and it was noted he was not wearing a respirator where the air concentration was 1,260 NCGs. That translates to 1,000 rads. This guy would have gotten his yearly dose in less than a half hour or thereabouts. And so I just question the value of 1,050 as a default maximized intake value.

MR. RICH: I think that we've already agreed that that approach may not be uniformly and assuredly conservative. We're working that now.

MR. ROLFES: Yes, exactly. Our previous default in the technical basis document was to assign 1,050 MAC-hours of exposure per year for a worker at Fernald. And we are actually reviewing, and I believe much of the work is already done in draft form.

The amount of thorium exposure has changed based on the actual production and air measurements that we have recovered. And that is broken down by year and will be put into a model basically based on job, or worker category to assign annual intakes.

MR. MORRIS: It doesn't contain production data.

MR. ROLFES: Okay, no production data. I apologize. It's just air monitoring data.

MR. RICH: It's his work place assignment.

MR. ROLFES: Exactly. We have it associated with plant and year.

MR. RICH: Well, that's at the craft level, too.

DR. BEHLING: Because again, in one of the attachments, 4.3-1E, you see that there were two comparisons. The first data point involves May 17^{th} through October 31^{st}, and the other one was November 4^{th} through November 23^{rd}.

So two relatively brief time periods for the same location and the same area of job function, and you realize how different they are. I mean, just compare the two sets of data and you will be absolutely stunned by how things can change for a given worker, location and job function.

MR. RICH: The initial effort in data capture for the initial technical basis document appeared to bound high, and as we uncovered more data, why, we agree.

DR. MAURO (by Telephone): Mark, this is John Mauro. From your response in the matrix, it’s not clear given what I just heard you have data from different locations, perhaps at different times, and are you planning in your model to use the full distribution for a given location or building? Or are you planning to use the upper 95^{th} percentile as your default value for intake?

MR. ROLFES: We are using a Patel model that was put together. Let’s see, I’ll let Bob

Morris comment on this also.

MR. MORRIS: With regard to what air sample data we’re going to use, we’ve annualized the data and taken a lognormal distribution assumption around it and fitted the data. It’ll be, there’ll be parameters available at the 50th percentile, 84th percentile and 95th percentile, available for dose reconstructors’ selection based on where they believe the appropriate model is for the maximizing or best estimated work used for that.

The Patel model then allows input on the number of hours that the person worked, the job category that they had, whether they were an operator, maintainer, supervisor or in some other role, and I think that’s the set of parameters that (inaudible).

DR. MAURO (by Telephone): So as I understand it, I did read 6001, so as I understand it, it’s up to the dose reconstructor to use his best judgment where within that distribution of values is the most appropriate for that particular case.

MR. MORRIS: We’ll publish three values for each distribution, that’s right.

DR. MAURO (by Telephone): And they'll make that choice, I guess, based on some guidance provided.

MR. MORRIS: Yeah, also note that the reason our data spread so much and in a claimant favorable way increased the geometric standard deviation for the lognormal distribution is that we are combining breathing zone data and general area air sampling data.

So it's essentially two populations of data we're treating as though they were one. An effect of that will be to spread the data and increase the geometric standard deviation and make the tail end of the lognormal distribution go higher than it might otherwise.

DR. MAURO (by Telephone): Just one observation, since there is a substantial difference between whether you use, which percentile you use could change rather dramatically the assumed intake, and you'd like to make sure that those guidelines are used in a consistent manner, I don't recall whether there's any direction given on how does the dose reconstructor make that judgment for a particular case. Is there any general guidelines or it's really left to his personal judgment on which of those three values are the one that is most applicable to a particular case?

MR. ROLFES: Those guidelines will be published in the approved revision to the site profile. I don't know if those, that verbiage is --

MR. RICH: The data's in a tabular form so they can take it off the table.

MR. ROLFES: The data as Bryce is saying, the data's in tabular form and the dose reconstructor would have the option of basically choosing from a table. More details on this will be in the site profile document.

MR. MORRIS: It just hasn't been approved yet.

MR. ROLFES: Yes, exactly.

DR. MAKHIJANI: A couple of questions, a couple of observations first. One is in Hans', in the document in review, you have the wet area. This came up earlier. This is a reminder for those of you who were not there, it also came up at Mallinckrodt where the initial position was, oh, raffinates wet, low dose, don't worry. And then the dose number that came out of NIOSH were actually quite high.

The other point is that assuming respirator not used is not a claimant favorable thing. It's just a factual thing. It says so in the document, no respirator worn at least twice that I've seen. And that's just two points.

The last observation that I have that I have a question is I don't think mixing breathing zone samples and general air samples is a good idea. They all belong in the same distribution so methodologically it's, you don't have any distribution all you have is a collection of numbers. I don't think you can call breathing zone samples and general air samples mixed up together a distribution in any rigorous in any statistical sense of the word. They're two different sets of numbers. They're taken in two different circumstances --

MR. MORRIS: Well, also consider that we are proving these are lognormally distributed anyway.

DR. MAKHIJANI: I understand all that. Just from the two different populations of numbers, they're not the same thing. We just have gone through, you know, there've been lots of studies even where breathing zone samples can belong in some distribution, but at least you can say they're in the same distribution because the measurements are the same thing.

In statistics you cannot mix up numbers in distribution that are known to be from different populations. Moreover, within this process, we started, the very first thing we did was Bethlehem Steel. We had a long process in which NIOSH actually agreed not to mix breathing zone samples and general air samples, and agreed the general air samples actually needed an adjustment factor and that you could not mix these two things up. So just as a kind of a heads up that this procedure, even if it's in an approved document, is a contradiction to other approved documents that NIOSH has approved.

And my final question is, so I can kind of round this out, are you using the raw data with all of these numbers, high, low mixed in from all the different stations, or is there some daily weighted average proceeding?

MR. MORRIS: We used every number that was available in the air sample database.

DR. MAKHIJANI: Threw all the numbers into the pot without any consideration of how much time a worker spent in the operation?

MR. MORRIS: That's what biases the best year to the worst is that only the dataset we found for 1970 only had the levels of high values in it.

DR. MAKHIJANI: Sorry? These are not the data that I'm looking at. On page --

MR. GRIFFON: Do we have this data yet?

DR. MAKHIJANI: -- 55, I don't know that we have the data, but we have quite a lot of air sampling data, and these are clearly data from various processes, and without a little bit more information you don't know whether throwing in all the numbers into the same pot is going to be claimant favorable. I don't know what the process is.

Let me ask a question again. I mean,

we've gone through a lot of these things in previous reviews, and I don't know what the process is to, whether the previous review matters in the new process. During Mallinckrodt we pointed out that if you have three measurements at a particular work location, and you try to create a daily weighted average out the average you're going to find a wildly claimant unfavorable number because 95 percentile of the three measurements are going to be very high.

If you throw all the numbers into one pot, you'll get a very different result than trying to calculate 95 percentile at a job location and then weighting that with the time spent over there. So unless you have knowledge of the time spent actually you won't know whether your result is claimant favorable or not in my opinion.

MR. ROLFES: Let me give you an alternate scenario. Take, for example, a chemical operator that has a, that's working, say, at a station working with green salt, and there's a general area monitor right next to him. Say he's doing his job and working for a couple hours, and he goes and takes a break. He's away for 15 minutes. The meantime the air sampler is running so it's going to continue to record air activity.

Then again he's going to be leaving for lunch, taking a shower, eating lunch, returning. Still that air monitor is going to be recording elevated levels of air contamination. So essentially, even though that worker isn't being exposed during that time period, that air sampler is still running and recording data.

So we feel that the distribution of both general area air monitoring as well as BZ data are, you know, all worth analyzing together. So we feel that both are, in fact, representative of worker exposures.

DR. BEHLING: I would have to modify that because most of these sampling data are not controlled air samples. They are slot samples, and they will run for a matter of minutes.

DR. MAKHIJANI: We have lunchroom data. You have all of that mixed in.

MR. ROLFES: Sure, that worker could have been exposed in another area at an area of lower concentrations.

MR. GRIFFON: Well, it sounds like all of this has been the spreadsheet. You've been analyzing it. I mean, once this is complete or is it complete and can it be posted on the O drive? I mean, I'd like to look and see. And I assume the descriptive part of it is kept in the spreadsheet so that we know which ones are BZ samples, which ones are general area. I think it might be useful for some of us to sort that out and see if we agree with your conclusions, you know.

DR. WADE: Do you have a question?

MS. BALDRIDGE: I have a question. How do you address the issue like with the thorium levels being, the general air levels being three times the maximum allowable levels for a period of over three years continuously?

MR. ROLFES: We would address that in dose reconstruction. We're not making any argument to say that Fernald was a clean place at all. We realize that there were --

MS. BALDRIDGE: Well, I think Arjun had said about, you know, the time of exposure and all, I was just wondering how that high a level over a continuous day after day after day over a three year period, what type of effect that has and how that is being factored into --

MR. ROLFES: Sure. We fully acknowledge that there were elevated air samples, and many of the air samples, they were very high. That's very true. We're not disputing that in any way, shape or form. And we're basically using that information to credit workers with that exposure. So we're not saying the Fernald was clean. There was no contamination at all. I don't want to, you know, I don't want to convey that message at all.

MS. BALDRIDGE: I didn't have that message, but I was just wondering about the extended, you know, when you're talking about acute exposure, chronic exposure, that type of --

MR. ROLFES: Sure, exactly. If we have information saying that for three years this job was routinely a dirtier job that released more contamination into the atmosphere, we want to make sure that we are crediting the worker with that exposure. And essentially, it's going to be a chronic exposure for those three years, so that's what we've tried to do.

In this data that we've collected for thorium, we've taken all these samples, put them together by year and run a statistical analyses of these data points to come up with a likely value but also uncertainties associated with that most likely value. And we want to make sure that we are claimant favorable in assigning intakes because we know that respirators were supposed to be used, but they weren't routinely.

So we're not going to, what we see in the air, we're going to assume that that air concentration is what the worker was exposed to. We are not going to apply any respiratory protection factors, and we will, in fact, assume that the worker was exposed to what was measured. Did I answer --

MS. BALDRIDGE: That was fine.

MR. GRIFFON: I don't know that we can go much further without seeing the model itself, but I did have one follow up on 6001 because I haven't looked at that procedure. You mentioned that two factors could be added in from the Battelle model, one was the hours worked, but also the job type. And is that really referencing back to what was in? You've got a table of different types of job categories with different -- I don't understand how you entered the job type into this model. I'm trying to --

MR. MORRIS: It's just a number factor. One hundred percent of the doses assigned are the intake. It's assigned if you're an operator.

MR. GRIFFON: Okay, so it's based on maintenance operator versus administrative or --

MR. MORRIS: That's right.

MR. GRIFFON: -- and some fraction applied.

MR. MORRIS: Yes.

DR. BEHLING: The finding number 4.3-2, I think, has been addressed because it also raises the issue of the 1,050 MAC-hours as a default value.

So to Finding 4.3-3, and that one is titled limitations associated with the use job tasks, job locations for the assignment of thorium intakes. And we just, in fact, Bryce has just mentioned that the new model will try to define by year the job function and base air concentrations and intakes on those two parameters, job function and by year.

In going through the documents I identified a number of references to a project labor pool, rolling maintenance crew, roving operators and also enclosed a couple memos that were submitted by the Director of the Health and Safety Division in ’53 who complains about the fact that when he gets a person in there, he doesn’t always know. He thinks he knows, but then it turns out that the card or the data that he has is incorrect, and I quote here, and he makes reference to a roving maintenance man.

He said, the department of the job location is where they present themselves for medical care. The man then reveals that he’s working in a different area from the one noted on his medical records. In a subsequent memo it’s written that another serious problem in determining internal exposure is the difficulty in good work records which show how long an individual worked in the various jobs.

So again, we may have information that would designate a person to a different location, a different job function, but he may not necessarily be there. And then there are people, and they're not small in numbers, that are labeled as project labor pool.

And they may have had some of the dirtiest jobs including repackaging drums. They were constantly involved in some of the, probably the most difficult and highest airborne environments. And do we have any clue as to who these people were? Are they identified as members of the labor pool, members of the roving maintenance or roving operators? And when there is no such designation in their file, what do we do about these people?

MR. ROLFES: I think we sort of addressed that a little bit before, that we don't feel this is an SEC issue because we have a model to essentially assign intakes based on the Battelle AWE model. With real data from Fernald we're using a model for different classes of workers, for operators, for laborers, for supervisors.

DR. BEHLING: But you will have to obviously make some decision as to which category the

95^{th} percentile comes from.

MR. HINNEFELD: Part of the process, I think, that has to come out in our next response to this is not only the basis for the model we intend to use, but some idea that what can we select of the worker population to which this model would be used for. I don't think we can just say that, well, we have a way to do it to take some people and assign them to put them in this population that we're going to assess their dose in this manner without accompanying that with a set of decision criteria for what employees fit with that. I think that's part of the same analysis we've talked about.

DR. MAKHIJANI: I'd just like to mention fugitive emissions again. I think if you just take a look at that one memo from 1970 which is quite late, and try to infer the kind of dust levels that would have motivated the writing of that memo.

MR. ROLFES: (Inaudible) version?

DR. MAKHIJANI: TBD review. I'll just read it. I read the bucket brigade piece earlier, and then there's another piece where the ball mill was leaking, and there was dust everywhere. And then the second piece to that memo, Ross, 1970.

"During the operation of removing the calcine, thorium, tetrachloride and calcium fluoride from the retorts, the stack-up tray is left standing on a skid near the south annex door. The door is left open to aid in pulling the trays. The winds coming through the door blows the loose powder from the trays and spreads it generously through the annex."

And, you know, while we say we are doing generous dose reconstructions, there's no measurement of what this generously through the annex means. You've got this blowing inside and outside, and this is why I said that you can have non-production personnel get quite high exposures in very short periods of time. You walk through something like that and a gust of wind, and you're essentially in a little bit of a thorium dust storm.

And because the stuff was there at open doors as late as 1970, and you wonder what happened in 1956 and 1955 and 1954 when stack emissions were at least -- you know, I can't remember the order of magnitude higher, but it was a lot higher. I don't see how you're going to use any of these models which have to do with production data to take into account fugitive emissions or who was exposed or put a limit on this.

MR. ROLFES: It doesn't sound like that was necessarily blowing outside. To me it doesn't differentiate whether the materials were blowing back into a production area --

DR. MAKHIJANI: It would depend on whether the wind was coming from the inside to the outside or the outside to the in. And I don't think you have the measurements to say that, and so you have to assume it was in both places some of the time.

MR. ROLFES: So if the material was blowing back into the production area, it would have contributed to the observed air monitoring data that we have.

DR. MAKHIJANI: And the other way about?

MR. ROLFES: And the other way about? If we don't have information, we're actually going to assign the highest annual intakes in our model.

DR. MAKHIJANI: The question is how do you know that the highest assigned intake covers a situation for which you have absolutely no evidence that you have any data?

MR. MORRIS: It's possible that the concentration outdoors is lower than the concentration indoors.

DR. MAKHIJANI: Well, you know, it's a question. What plausibility in the scientific sense has to be buttressed by at least a few data points? And I'm not aware of data points, at least in regard to thorium, that are there for fugitive emissions, and you can say that this is a pure speculation that there was a sampler near where the trays are being dried.

And I have not seen any reference to a sampler near a door where trays are being dried. So you don't have any evidence that you have an indoor air sample. I've looked at a lot of air samples, and I have not seen evidence of any.

I readily grant you if they were on the other side of it. You don't have any outside air samples. Do, in fact, do we know of a high dust operation with indoor and outdoor contamination for which you have no samples at the time it was documented in 1970, not to speak of the time in 1950s when such things may not have been regarded as worth documenting.

MR. ROLFES: It’s important to differentiate where we’re going with this because for uranium exposures, for example, we wouldn’t be relying on the air monitoring data. That wouldn’t be as important to us. The urinalysis data would be the most important piece of information.

DR. MAKHIJANI: I agree.

MR. ROLFES: Thorium is slightly different though because they did, in fact, have different attempts to take thorium bioassay in the early days through urine. It wasn’t a very good method so they didn’t follow through with it. What we have done I believe is very claimant favorable because we are accounting for production of thorium by year, and I would have a hard time believing that the outside thorium air concentrations were in excess of the actual production operation.

DR. MAKHIJANI: Inside. Do you have any evidence that there was a single air sample taken near the door where these trays were being left to dry and it says, “removing the trays from the support requires heavy effort and this dislodges more powder to be spread by the wind.” There’s no evidence that there ever was a single air sample over 20 years.

MR. ROLFES: We can discuss it either way, but, you know, we can’t go on asking questions about what data we don’t have. You know, that’s, we are focused on the data that we do have, and that is what we have analyzed. And we do feel that this is claimant favorable to assign intakes based on the recorded data associated with the production operations.

MR. HINNEFELD: Well, I mean, the air sampling data has to be compiled and presented to the work group. And it will either be convincing or not as to whether it has covered the appropriate places and is of sufficient number. So, I mean, we can talk here all day, but until the working group sees the data, it’s not going to matter.

DR. MAKHIJANI: I wasn’t talking about the sufficiency of the data. I just am flagging this as being a very remarkable thing from the first time I looked at Fernald data which is about 20 years ago actually the first time. And this has been a very remarkable thing about this site is that the ambient, what is normally called ambient environmental contamination I believe at Fernald in many places was dominated by this kind of fugitive emissions.

We had blowouts, you know, and stuff coming out of the windows. You had stuff drying in the doors, and so the stack emissions even though they were high, may not even describe a fraction of this kind of dose. And I just think that methodologically it's extremely difficult and should be flagged and attended to because I have not seen any other site with this kind of problem except, you know, in the context of nuclear testing or something.

RADIOLOGICAL THORIUM INCIDENTS

DR. BEHLING: We'll go to Finding 4.3-4 on page 70. And this is basically a continuation of the issue surrounding the difficulty in quantifying the air sampling data. And this particular finding is entitled the inability to account for internal exposures associated with radiological thorium incidents.

And it's well documented, and it's also accepted by NIOSH that small fires, spills, explosions were commonplace. And yet it is unlikely that most of the air sampling data that you're compiling will necessarily reflect them, those radiological incidents. So that you have a large number of readings from air sampling that you may have at specific work locations.

But those were spot samples, some as short as a few minutes at a time. You don't have any kind of understanding of radiological incidents and what airborne concentrations they may have contributed to. And as part of the attachments there was one that first you talk about the number of known fires and all the different, the (inaudible) nature.

And let me just recall that much of the work at Fernald was very much similar to what had taken place at Ames, that is, the reduction of thorium. And we all know how dangerous that particular process was in terms of the exothermic reaction that resulted in blowouts and large releases of thorium.

But the one particular attachment I wanted to look at was Attachment 4.3-4D. It's on page 76 of the report, and it just caught my eye when I looked at that because it turned out that perhaps just a, there were air samples taken that were at a location where thorium was being processed. And the first general air sample that we see as the first entry, I believe -- I may have marked those with arrows -- were basically background. And you have a high, low and, I guess, average value here.

And in the next one it says, "same as above except" -- it's hard to read -- derby on fire, "one derby on fire." And they took two air samples. And it goes from, I believe, yeah, it goes from an average of 2.1 MAC as background before the fire to 458 MACs. And it happens obviously in an instant.

And in this case there was somebody there to observe what the air concentrations were at the time of this one derby fire.

Further on I think there was another instance where there were two derby fires occurring simultaneously.

And it just demonstrates the ubiquitous nature of radiological incidence and the very rapid rise in air concentrations to which a person may have been exposed to that are probably not likely to be captured by spot samples that are normally taken based on the fact that industrial hygienists in today's job is to go down there and just routinely go through there.

And it's not always likely that he would catch these radiological incidents that we know will raise the air concentrations by orders of magnitude. So this is just another variable that is probably not going to be accounted for in trying to model air concentrations for dose reconstruction.

MR. ROLFES: Hans, this appears to me to be a uranium derby rather than a thorium metal product. And for uranium this isn't of concern to us because of the bioassay data.

DR. BEHLING: Well, okay, if it was, I wasn't really sure.

MR. CHEW: It does not say thorium. I'm looking at it now. It just says derby fires.

DR. BEHLING: Yeah, it just says derby fires. But again, the question is that would it matter? It's likely that derby fires involving thorium also occurred for such exposures.

MR. CHEW: But there was no such thing as thorium derbies, right?

MR. CLAWSON: No, uranium derbies.

DR. BEHLING: Only uranium?

MR. CHEW: Derbies are related to uranium.

DR. MAKHIJANI: There were 30 drum fires, at least 30 known fires until 1959 in materials involved in thorium residue. I don't know. Do we have any data for those thorium fires? This is on page 44 of the review.

MR. ROLFES: A big fire that occurred was an accident that resulted in the death of two employees. Two employees received severe burns in 1954, I believe, at Plant 9 during a blending operation where they were combining a calcium metal with some thorium tetrachloride, I believe it was. And I guess there was a little bit of excess moisture in the thorium material and it reacted with the calcium metal and caused an explosion.

We recognize that events like this did occur, and I'm hesitant to say I don't recall seeing air sampling data specifically associated with that occurrence. But I would have to take a look.

DR. MAKHIJANI: I was asking about the fires actually, Mark. They were documented from 1959. It says, "During the past four years there have been 30 known fires with these materials." Thorium and -- "some of which burned for several days. Clean up after these fires is a difficult job. In one case the fire burned through a concrete storage pad," et cetera. Housekeeping problem, hazards, with residues and unoxidized (inaudible).

So you've got a systemic problem here for a number of years that has gone on, and these drums were presumably stored outside. Correct me if I'm wrong. These things were stored outside at Fernald to my knowledge. And so you've got workers probably involved in putting out these fires and cleaning up the residues that would have been exposed to thorium.

First of all it would be good to know if we have some data on who these workers were. And secondly, if there are any data to support the dose reconstruction with respect to incidents like this with thorium.

MR. ROLFES: Sure. I'll address this generally at first. I don't know for a fact whether we have air sampling associated with a short-term excursion or a short-term episodic release for thorium outdoors. I haven't taken a look, and I can't recall from the thousands of records that we've recently catalogued and recovered.

However, when we're discussing intakes from acute scenarios, NIOSH is not intending to do intakes of this approach in a dose reconstruction for thorium. What we'll be doing is a chronic intake, and I think in almost all cases that we've discussed with SC&A, we've been able to demonstrate that these chronic intakes are generally more claimant favorable by assuming that the worker was continuously exposed over a full 2,000-plus hours per year rather than breaking it down for a short duration exposure to a very high air concentration. I believe that our methodology has been claimant favorable in assigning intakes from these scenarios.

DR. MAKHIJANI: That’s not always the case, and moreover, you have to be able to identify the worker in a production situation where you have (inaudible) and a record of an incident and continual exposure you can do something. But if you don’t know who the worker is, and you don’t have a record of any continuous exposure, and you have a single incident intake, and you don’t know when to assign it, this is more of a problem.

MR. ROLFES: If we have indication that a worker was involved in thorium operations based on information from a telephone interview, based on information from a report, based on dosimetry records which would indicate which plants the individual was working in, then we can certainly associate that worker with potential exposures that were ongoing in that plant or that area during that time. So the more data that we have, obviously, the better detailed, more accurate and precise approach that we can take for a specific claim. However, typically, when we have less information, we are more claimant favorable in assigning dose.

DR. MAURO (by Telephone): Mark, this is John Mauro. The data that's collected, the air sampling data, I would say for a given building or room of thorium, was that a continuous air sample that was continuously collecting air particulates over the course of the day, day-in, day-out throughout the course of a year or was this some type of spot samples that were taken at different time periods?

I guess the only reason I ask that is that a human being is for all intents and purposes a continuous air sampler. So in effect if you have a continuous air sampler always collecting it so you get a time integrated accumulation of what was the airborne activity over the course of a year. I know you might pull the sample after it gets loaded up and replace it with another one, and I understand that over a long period of time there may be these short-term spike that we've been talking about, if they are short-term. They all sort of average out.

So I guess I want to get a better feel of the air sampling data that was collected for thorium. When was that? Were those continuous air samples?

DR. BEHLING: No.

DR. MAURO (by Telephone): They were not.

MR. ROLFES: That was Hans, but I'd like Morris to answer this, please.

MR. MORRIS: No, they were generally 30-minute air samples that were taken in triplicate by Industrial Hygiene technicians. There was a standard operating procedure published in 1960 that clears what we think HASL imprinted on the plant in the early '50s as a method. And it looks as though that was probably the procedure that was followed through the duration.

DR. MAURO (by Telephone): For example, this 30-minute air samples that were collected now. They were collected once a day? Were they collected just a few times during the course of a year? Just trying to capture, given what I heard as variable air concentrations from place-to-place and time-to-time, and then someone comes in and grabs a 30-minute air sample let's say once a day. That might be okay.

MR. MORRIS: John, I don't think it was as clear cut as that. In 1954 we had 530 samples recorded, 750 the next year and 225 the next year. 'Fifty-seven, '58, '59 I found no data. But, of course, in those years there was very, maybe no production at all going on in thorium. 'Fifty-seven there probably was. I'm not sure.

MR. RICH: Let me add just a note, too, and that is that the uranium production involved thousands of metric tons and large amounts of, large masses of uranium going to the plant all the time. In the case of thorium, however, it averaged considerably less than a metric ton per day.

And so the process was not only short-term -- and by the way, a metric ton is a piece about like so. It's very dense material. Now, it's a bigger volume because if you get thorium oxide then, of course, the average density is considerably less. But for a perspective standpoint, the thorium operation was not like uranium by several orders of magnitude. And so when we talk about continuous samples the operation was probably not continuous. It was a batch-type operation in general.

And so these samples, although they may not sound like much, and the general air samples of 30 minutes may not sound like a continuous air or a very good general air sampling for this particular operation, they very well could have been appropriate for general or breathing zone samples and monitored as the process was in place.

DR. ZIEMER: Were they systematic --

MR. RICH: By the way, we're going to find out a little bit more about that in some interviews we have scheduled with some professional people.

DR. MAKHIJANI: From the bone dose point of view if you just want to take the kilograms and move from kilograms to per Becquerels, the bone surface dose for Thorium-232 is nearly three orders of magnitude bigger per Becquerel. So the production is two orders of magnitude less of the dose per Becquerel.

MR. RICH: What we're talking about though, Arjun, is not that conversion factor but the definition and the concept of general air sampling or how you're monitoring a given operation.

DR. MAKHIJANI: What you're saying actually makes it more difficult to do dose reconstruction because if you've got a small volume of material with very high dose consequences, three orders of magnitude bigger almost for one organ at least, then you're sampling network has to be considerably more dense than when you have a large volume of material going through the same big building. Because thorium was going through the same buildings as uranium, and the buildings were designed for uranium.

There's no question that uranium was the main thing, and it was two orders of magnitude more than thorium, but you have a sampling network and a sampling protocol. And buildings which are designed for a mass volume of material, and then you're dealing with a smaller mass of material with much higher dose consequences. So how are you, you know, these general air samples, even if you accept the breathing zone designation at face value, I think the problem of general air samples with thorium is going to be much more complex.

MR. RICH: The sense we have from looking at the air sampling data at this point is that they were taken operationally specific, specific to the individual operation, a breathing zone of a person actually doing a job or general air sample in the vicinity of the specific operation that was being conducted as opposed to a continuous operation for two shift, you know, or whatever.

DR. ZIEMER: Well, that answers my question. They're systematic in terms of jobs rather than time of day.

MR. MORRIS: And they're spread over a first and second shift.

DR. ZIEMER: Although the mass is much smaller, your specific activities are much higher.

MR. RICH: Specific activity for thorium is much lower.

DR. MAKHIJANI: Lower by about a factor of three, but the dose conversion factors are much higher.

DR. ZIEMER: For the dose conversion factors, yeah.

DR. BEHLING: You mentioned something that we may get in later if we get that far, but in one of the affidavits that was a sworn statement given by an industrial hygienist. And it's included in here, he made mention of the fact that the industrial hygienists never worked other than the first shift Monday through Friday not on weekends, second and third shift. And it was known to people that they would postpone the dirtiest jobs when the industrial hygienists weren't there. You mentioned that there are air sampling data that identify the second shift. Is that a fact?

MR. RICH: Yes.

DR. BEHLING: Do we have that for most of the years?

MR. MORRIS: When you look at air sample datasets, you see that there's a lot of them that start at eight or nine o'clock in the morning, and there's a lot of them that start at four or five o'clock in the afternoon. It's as though the system, the second shift crew came on and got their equipment ready and started the air samples. So I would almost guess that there's as many second shift as first shift.

MR. GRIFFON: So the, I mean, the data you provide, the spreadsheets going to have all this information, location, time, time of sample, volume, culture.

MR. MORRIS: I think so. I'm not going to know what the spreadsheet says.

MR. GRIFFON: As much detail as you have anyway.

MR. MORRIS: Certainly the raw datasheets will show the time of day that it was taken.

DR. BEHLING: It would certainly conflict with the testimonial statements given by that individual I made reference to because he distinctly made reference to the fact that industrial hygienists worked only Monday through Friday on first shift. It would be very helpful to dispel that if you have data that would contradict his comments.

MR. GRIFFON: But air sampling is so

(inaudible).

MR. MORRIS: All the air sampling records are available to see hard copies.

MR. CLAWSON: A lot of this, we can debate this for quite awhile, but a lot of this until we get to be able to see the data we're going to have to be able to do our own thing. So unless there's some critical -- I don't want to stop anybody, but if we can go on.

THORIUM PRODUCTION

DR. BEHLING: Let's go on to 4.3-5 on page 77. And I just, Arjun will take that one.

DR. MAKHIJANI: I think we've already discussed it, and from what I read in your response that you have a lot more data on thorium production than you did in the facility years because at this point there are lots of gaps in the data. So I guess there's more data that we need to look at.

MR. ROLFES: Yeah, we initially thought this could be an SEC issue, but we feel that the additional data we've collected and analyzed consequently no longer make it an SEC issue.

DR. MAKHIJANI: Yeah, so I guess we just need to see the data.

RE-DRUMMING

DR. BEHLING: The next one is on page 86, 4.3-6.

DR. MAKHIJANI: The post-production period, well, what happened in the third period, the re-drumming.

MR. RICH: That was even during operational periods.

DR. MAKHIJANI: Well, there was a lot of re-drumming during operations.

MR. MORRIS: Three years from what we understand from the report.

DR. MAKHIJANI: There's a question of the re-drumming during the operations, and then there's this gap between '77 and '86 when you have lapel sampling. And I did not see any information as to how that dose reconstruction was going to be done. At least we had residue of contamination, you have re-drumming operations, you have, you know, you have a lot of different, you have stuff that we dumped into the pits, stuff we've shipped in and out as part of Fernald being a repository for thorium. Or shipped in maybe. But I don't know, I have not seen any data from that period separate from the re-drumming question.

MR. SHARFI: The post-production period is after the in vivo, the thorium was up and running, right? So you would have thorium in vivo counts for the workers for the post-production periods. So you can use actual monitoring data rather than air monitoring data.

DR. MAKHIJANI: In vivo counts for the thorium did not stop in ’78 or whenever --

MR. ROLFES: From ’68 through ’88 and then on after as well.

MR. MORRIS: And then with a new system that was installed at the plant in ’88 or ’89.

DR. MAKHIJANI: So I guess with that, too, we have to just wait for that.

MR. ROLFES: Sure, that is an important point that we sort of skipped over a little bit. We do have thorium air monitoring data that we’re going to use; however, we also have the mobile and giga-radiation monitoring laboratory results from 1968 through 1988. Those have all been transcribed and analyzed, and we can actually basically take a look at those in vivo data and ensure that our thorium air monitoring data is in fact claimant favorable and also reasonable.

DR. MAKHIJANI: And how about the re-drumming operations? Do you have for the early period air concentration data for that?

MR. MORRIS: We may. It's hard to know for sure whether we've got enough. You know, we're only now getting focused in with the right people to tell us when the re-drumming happened. That was kind of a detail that we didn't understand, so we're correlating when they said something happened now and going back to try to find any air sampling records is something we're working on right now.

DR. MAKHIJANI: The next one is re-drumming (inaudible).

DR. BEHLING: Yeah, and I guess I'm not so sure in looking at this, when a facility goes from thorium production back to uranium, are people at that point monitored principally by urinalysis, which is now your focus regarding their uranium exposure?

MR. MORRIS: No, the equipment was cleaned in between the campaigns.

DR. BEHLING: Because one of the things that was introduced here was the transition period where, okay, today we stop processing thorium, and we're now back into uranium production. The question now is what do you monitor for, uranium by way of urine analysis or thorium by way of air monitoring? Because clearly residual contamination must have or persistent contamination must have continued for some period of time.

MR. ROLFES: There were limits on the amount of contaminants that could be contained within uranium metal. There are documentation of any contaminants in the thorium metal so they would have wanted to clean the machines if they were used for the same, or used for thorium then for uranium.

I'm sorry, what was the other part of your question then, Hans?

DR. BEHLING: Well, the question is how do you monitor people during this time period where yesterday you did thorium; today we did uranium? Did they monitor for urinalysis or do we monitor continual air monitoring for a period of time? Because we know very well there's persistent thorium levels, contamination levels that people were exposed to during this period of time. And the question is what do you do?

MR. ROLFES: For the production years, are we going to be assigning an entire year intake --

So the entire year of intake will be assigned by year. So we won't be addressing a lower intake potential for residual contamination but rather a production-level intake for the entire year.

MS. BALDRIDGE: I have a question or statement. In, I don't recall which document, but when the auditors came in to check, I think there were some came in from Oak Ridge. And in those documents it talks about the questioned whether some of this in vivo testing that was being done on the individuals were being done correctly. They also said, you know, then, I guess, this transition time from one product to another, they came in five years later. There was still contamination that had never been dealt with.

MR. ROLFES: Sure, that's, once again, we do understand that Fernald had contamination. We understand that. We, we --

MS. BALDRIDGE: I just think it puts a question on the reliability of the data that they're presenting from their in vivos if the auditors questioned how competent they were to even administer or evaluate the information. And it was all done in-house so no one was ever checking what was done.

MR. ROLFES: We've spoken with the people that operated the mobile in vivo radiation monitoring laboratory equipment. And, yes, they did have procedures to calibrate the equipment. They did do routine quality assurance checks on the equipment. I don't believe we have the procedures at this time. I know that a couple of the people that we have, in fact, spoken with though could verify that there were quality assurances to ensure that they were getting good data essentially.

MR. GRIFFON: I think I just captured that as an action item. Maybe that you should look back at the audit report that --

MR. HINNEFELD: Is this document in the petition?

MR. GRIFFON: -- just as a reference to that.

What is it?

MR. HINNEFELD: I just asked if this document was with the petition and so we can go find it, and we can address what's in that document.

MR. GRIFFON: Yeah, address that.

THORIUM INGESTION

DR. BEHLING: We're going to skip the next two findings because in discussing it between Arjun and I, I think we've discussed enough issues surrounding Finding 4.2-7 and 8. So I think we'll go to Finding 4.2-9 on page 93. And the title of that finding is the inability to assess internal exposures from the ingestion of thorium.

And we kind of thought about what are the potentials for exposure due to ingestion pathway given the fact that repeatedly we see things such as one of the words housekeeping situations that were encountered. We have people who were not properly trained about the avoidance of certain practices such as touching your mouth or certain other things. We know that they were not given anti-cees*.

They were probably never really monitored for fecal analysis that might have perhaps assessed their intake by way of ingestion, especially for insoluble materials that would nevertheless expose the cells of the GI tract during the transit time. So the question is there are gaps here with regard to how do we model the ingestion of thorium exposures in the absence of data that might provide us some clue.

MR. ROLFES: And we've alluded to this a little bit in our discussion of the atomic weapons employer thorium exposure model developed by Battelle. We're going to be using thorium air monitoring data within this Battelle model. And it also evaluates, or also included intakes from ingestion, from the ingestion pathway.

MR. MORRIS: That's based on the OCAS guidance that came out of mode two and mode three in testing. Battelle incorporated the OCAS directives.

DR. BEHLING: So the new model will address ingestion?

MR. MORRIS: Explicitly.

DATA INTEGRITY FOR AIR MONITORING

DR. BEHLING: The last one on this one is the issue of data integrity for air monitoring. And I did make reference to, and briefly touched on moments ago, the affidavit, the sworn affidavit that was provided by an industrial hygienist regarding what he recalls during the 17 years of employment there. And then he cites a number of issues here that obviously you speak disparagingly about some of the practices inclusive of things that he was asked to do by his superiors.

And I always look at statements like this, and I'm currently, and I won't go beyond what I'm about to say, and I always look at the source. And it's like a crime investigation. You sort of say who's got reasons to say what. And sometimes you realize you're dealing with disgruntled employees for one reason or another, and it's unreasonable to assume that in some instances this is strictly very biased at best and an outright lie at worst on the part of that individual.

But in this case I have to look at it

and sort of say how much truth is there. We've already discussed the issues where he is going on record and stating that they never took air samples on the second and third shift, neither that or on weekends. If you can prove that, certainly that would be one of the issues that could be put to rest. But he talks about air sampling protocols where he was asked to go back again and again and again until he came up with air sample data that somehow or other met the expectation of his superiors because they were under the gun to clean up the act and keep production rolling.

And so I guess I have to look at this guy's statement and dismiss it and take it very seriously that after all, it's not a moment in time. It's not a single incident. It's 17 years worth of employment, and he has some very critical statements to make here.

MR. ROLFES: In the case what he had described was that he had taken a couple of air samples, reported them back to his supervisor, and he supervisor said, you know, those couldn't be that high, go take more samples. So it essentially attracted the supervisor's attention to those high airborne results. So the individual went back, took a couple more samples, still got some high results, reported them to his supervisor. No, those can't be right, you know, something's going on and attracted his attention once again. So this individual, you know, rather than walking away from an observed high air concentration value where they might have a problem, the individual was continuously sent back to that, to take additional samples to determine what the problem essentially was. Keep in mind that the data, we don't have any indication that the data was destroyed. I don't know what specific set of air sample data this individual was referring to or if there's some specific results, but there's no indication that the results were not reported in the record or that NIOSH couldn't get them.

MR. GRIFFON: That may be one of the things I was talking about earlier. If this individual had logbooks, then if we could find the logbooks related to the time period that he worked or his logbooks or whatever and compare them back to the data you have. And if all the data is there then I guess it shows that they weren't, you know, just trying to get a clean result. They were --

DR. ZIEMER: Well, certainly a follow-up survey would make sense, and I guess the issue now is --

DR. BEHLING: Well, it's who do you believe.

DR. ZIEMER: -- is he being sent back to get better results or --

DR. BEHLING: Yes, well, that's the crux of the issue, I think. I sort of alluded to the fact that maybe the culprit here is the hygienist who then, in order not to go back a fifth time, decided, I'm going to give them a low dose and then they'll be happy.

And the statement that he incorporates if you read his verbatim statement is that the rejection of the high values were based on their unacceptability because the person as his superior did not want to acknowledge the fact that the air concentrations were that high. Mark sort of thinks that his superior was so concerned he kept sending him back again. It's a question of who's the culprit here.

MR. RICH: Well, you know, as Paul indicated, from my operational experience if you get a high sample, you normally want to investigate the source of the result, send back the, find out what the source is or to see if you can fix it.

MR. GRIFFON: Well, you can read it both ways.

MR. RICH: You can take a series of samples.

MR. GRIFFON: You can read it both ways. I mean, you could say I don't want to shut down the operation. Go back and get a clean sample. I'm not shutting things down.

DR. BEHLING: I agree with you, but repeatedly if you read these memoranda is that the issue over and over and over again from industrial hygienist says we need better engineering designs improving the ventilation system. And it's not up to the industrial hygienist to rectify the problem. He's only there to be the bearer of bad news. That's all he is. He's the messenger. He shouldn't be shot for bringing back the bad news.

The people who should have had the incentive to change the ventilation system or create barriers or do other things were people that were outside his purview. So I still look at his testimony in critical terms and say, well, I'm not going to dismiss his comments.

MR. HINNEFELD: Ma'am, you wanted to say something, right?

MS. BALDRIDGE: Yes, Mark can speculate on what he thinks. But when you read some of the other documents, when the Atomic Energy Commission comes in and says you've got to clean this up, and they're response in writing is tell them what they want to hear, and then they go on to say, you know, the situation's actually getting worse than, instead of better. That tells me that it's questionable whether their concern was to rectify the situation or just get the Atomic Energy Commission off their back.

MR. ROLFES: Once again, it's a matter of interpretation on how you read it. For example, if this were in fact in a uranium area, however, these results would not be of significance to us because we once again would be relying on the bioassay data that we have for the individual. We wouldn’t be using the air monitoring data that was recorded to assign intakes for those employees involved. We would be using their bioassay data which is the most representative approach of actual worker exposure. It’s the most precise, I guess, approach for estimating a worker’s true exposure.

DR. WADE: I mean, you can argue forever about the motivation, but it should become unimportant. The key question is was data destroyed or --

MR. GRIFFON: Or falsified.

DR. WADE: -- falsified, destroyed, in some way corrupted. That’s what needs to be investigated.

MR. GRIFFON: And I would say to that end if we have raw data to compare against these files you have, that’s one way to get at that question. Do we have logbooks from this individual or whatever.

MR. CHEW: (Inaudible).

MR. GRIFFON: Do you even have those available?

MR. ROLFES: I haven’t seen any logbooks. I know I’ve seen some of the raw data reported. Most of the information that I’ve had available to me would be the electronic versions after they’ve been scanned. I know some of the data capture team members have, that have scanned the actual data. I can ask someone in ORAU to see if we have come across any logbooks.

MR. GRIFFON: It looks like there’s Health and Safety or Health Physics reports anyway, monthly or quarterly. I’ve seen those referenced haven’t I? Health Physics reports? So that may have some information also.

DR. ZIEMER: I’d like to ask. Hans, did you get the impression from this gentleman that that was the sort of common practice versus maybe a single event? He worked 17 years.

DR. BEHLING: Yes.

DR. ZIEMER: Was he suggesting that this was fairly standard practice on the site for him or for other workers? Does this stand out in his mind as --

DR. BEHLING: I guess, I didn’t obviously interview this individual myself. It’s a sworn affidavit that is available, and I think

I took select pages starting with page 100 of the report that are direct statements that he submitted and are notarized. And so you can kind of look at those and draw your own conclusions. But I think it is not something that was an isolated event.

MS. BALDRIDGE: Well, the document was part of the evidence submitted in court in 1990.

MR. CLAWSON: How many industrial hygienists did they have at Fernald? Does anybody know? I mean --

MR. ROLFES: Stu, might you know the answer to --

MR. HINNEFELD: Well, I was time dependent. I mean, from 1970 to 1980 there weren't very many at all because there weren't very many people working there. Before 1970, I think, they had a little healthier staff, but I couldn't tell you. There were a couple in 1980.

MR. BEATTY: After '80 there was only one RAD tech. I know that.

MR. ELLIOTT: This individual, Mr. Rudy, was an industrial hygiene tech at the time. He actually came to NIOSH after he left Fernald.

He worked for me for awhile. I can tell you he was very ethical, responsible industrial hygienist.

DR. BEHLING: And to answer Paul's question, if you look, Paul, on page 101 of the report, item number seven, it's a statement that should answer, at least in part, your question about how prevalent this issue might have been. And I'll read it for everyone else who may not have the computer.

Statement seven it says, "On several occasions during the term of my employment when I got air dust survey results that were above the MAC, I was told by my supervisors that it the results were an error, and I was told to go back and re-sample."

And then he goes on about this one instance where he was, went back multiple times before he decided to turn around and be downwind from the direction of the air flow, took his air sample because he knew from experience that simply rotating his body and the air sample 180 degrees would reduce the air concentration as measured by his air sampler.

So that’s as much as I know about whether or not this was a prevalent issue or a very episodic and inconsequential issue. That’s all I have is that statement.

MR. CLAWSON: This air data, I know that, and I guess it’s kind of odd for me for an industrial hygienist to be pulling these samples because we have RAD techs pull them. But we have to have a calibrated instrument to be able to pull these samples so that we know that we’ve got the total flow. Do we know what were being used?

DR. BEHLING: He refers to it as a homemade device. Now to what extent that is a fair and accurate description is again open to subjective interpretation.

MR. HINNEFELD: Most of the devices must have had a flow rate indicator on it because most of the samples should have a flow rate recording. So it must have had some sort of anemometer or some sort of flow rate indicator. If you want to talk about the calibration of the anemometer in the ‘50s and ‘60s, I’ll bet you’re not going to find any kind of calibration record for an anemometer in the ‘50s and ‘60s.

DR. MAKHIJANI: Dr. Ziemer brought up the document destruction thing, and that reminded me that thorium documents were destroyed at Fernald if I’m remembering correctly in the early ‘70s. Do you have any idea --

MR. RICH: Process data.

DR. MAKHIJANI: Process data?

MR. RICH: Not air sampling.

DR. MAKHIJANI: How do you know that?

MR. RICH: Well, we have some. We don’t have it all.

DR. MAKHIJANI: I mean, do we have some idea what was destroyed and what kind of production and process information might have been destroyed and what was retained?

MR. RICH: Well, the major reconstruction process for the thorium operations was primarily in the process area. We have a team put together to reconstruct what had been lost. The equipment, the process equipment had been removed and that was gone plus the fact that during the declassification period some of the process data had been, they were unable to recover data in any other repository. So they put the team together to reconstruct what they primarily processed.

DR. MAKHIJANI: Is there a record of that reconstruction?

MR. RICH: Yes, yes.

DR. MAKHIJANI: Can we have that?

MR. RICH: You have it.

DR. MAKHIJANI: We have it?

MR. RICH: Yes. It's, that processing's described in -- I'm trying to remember the author right now. I'll think of it. I'll think of it in just a minute.

MR. GRIFFON: When you say process data was destroyed, was this table you handed out earlier based on reconstructed thorium information or was it --

MR. MORRIS: I'd say new interviews.

MR. GRIFFON: New interviews, okay.

MR. RICH: Yeah, and I guess that is Dolan and Hill.

DR. MAKHIJANI: I have looked at Dolan and Hill.

MR. RICH: And Dolan and Hill, part of that is described, part of this process and part of the disposal was described in that report.

DR. MAKHIJANI: I have looked at Dolan and Hill. I saw that that was in your TBD --

MR. RICH: And there may be another -- if I come across the, there's at least a couple of references that talk about this -- I'll --

DR. MAKHIJANI: But Dolan and Hill was based on interviews that were at least not available to us. I remember I asked because it said we reconstructed this from interviews, and here, there's going to be a kind of an issue as to --

MR. RICH: They describe the interview process, but I've not seen a formal record of the interviews. They probably did not document it that way.

DR. MAKHIJANI: There was a document destruction in the '70s, and then Dolan and Hill -- I'm just trying to figure out what happened here. Dolan and Hill did some interviews and put something together about production --

MR. RICH: It was more than a set of interviews. They put together a team of professional engineers that had been there at the plant during the operation, and they collectively, as a reconstruction team, put together, based on best recollection and what information that they could assemble which included both the effluent data and the process descriptions.

DR. MAKHIJANI: Did that team produce a discrete report or was it just, did they just talk to, Dolan and Hill and the -- because Dolan and Hill had hardly any underlying information about how the thorium data, where the thorium data came from. It just has the data.

MR. RICH: Well, it's the results of the committee's work were reported in Dolan and Hill.

DR. MAKHIJANI: The committee itself didn't file like a report that was then -- because Dolan and Hill covered everything, right? It covered uranium.

MR. RICH: Right.

DR. MAKHIJANI: It covered thorium. It covered, and only a small part of Dolan and Hill is devoted to thorium; whereas, the destruction of the records is specific to thorium. So obviously some considerable effort must have gone into that small piece of Dolan and Hill which relates to thorium. And I'm not at all confident that Dolan and Hill captured the thorium operation. But there must have been some report from this committee to Dolan and Hill who had a much bigger job.

MR. HINNEFELD: I remember Dolan, and I remember Hill. But I don't remember this activity so I'm afraid I can't answer that.

MR. GRIFFON: Is this committee listed in the references in Dolan and Hill?

DR. MAKHIJANI: No, no, there's no record. I was not able to find any underlying -- I may be wrong, but this is just my own, our little, small team's review. But we were not able to find any underlying information, and I remember asking about it and came up with nothing.

MR. HINNEFELD: Is the record destruction really strictly just thorium though? I mean, Fernald had a records retention. They followed the Department of Energy's records retention schedule pretty carefully and threw things away when they go to their lifetime, and not every site did that. But Fernald, from my recollection, was pretty careful about throwing things away when the DOE said they could. And so I would think that there would be a large category of records that were dispositioned in accordance with those what were called the retention, retention schedules is what they were called.

MR. RICH: They just mentioned the thorium discussion because evidently it was complete enough that they had to put together a committee to actually reconstruct, to answer questions that came as a result of some other issues.

MR. HINNEFELD: Okay, because I don't remember that task to do that reconstruct (inaudible) the thorium. Records were destroyed routinely when they have reached the end of their retention time. Now, none of the records related to exposure should have been in that. They had a much longer retention time. So they should not have been destroyed.

DR. MAKHIJANI: And maybe you're right. I mean, I don't know. The only thing I've come across is a reference to the destruction of thorium records. And Bryce may be right in that those have become relevant because --

MR. RICH: That would not have been destroyed. There was no authorization to destroy a bioassay record or anything related to dose itself. Now, that did not include field operating data like air sampling. So frankly, I don't know if there was some, because my impression is that we don't have all of the air sampling data yet. We have a significant body, but I'm not satisfied that we have everything that was taken.

DR. MAKHIJANI: You have done new interviews though after Dolan and Hill. Now you're going through that.

MR. GRIFFON: So this matrix including, I think you have some numbers on the one that you presented, but --

MR. MORRIS: To be clear about where I got that. There are a lot of documents and some that were cited in the SEC petition that had production data in them. When those were available, I picked those up. Sometimes I had three different documents that had three different numbers in them, and I just had to choose. That's available in the annotation that you'll see eventually on there. And then we did do additional interviews that clarified a lot of the uncertainty about this.

MR. GRIFFON: Go back to the matrix.

MOBILE IN VIVO RADIATION MONITORING LAB

DR. BEHLING: The next topic that we want to discuss is on page 104, and it deals with the mobile in vivo radiation monitoring laboratory. And I have just a couple comments that are not, and I'll say it up front, this is not considered a finding by SC&A, but I did have some questions about the lung counting systems, and it's been something that's bothered me from the days where I reviewed some of the Oak Ridge team, and that is the use of a lung counter that's defined by a nine-inch- by four-inch-thick sodium iodide crystal.

And, of course, I would consider that a very unsuitable device for doing lung counting. It's great for doing the whole body counting if you want to look at CCM of Cobalt-60. But certainly not very suitable for counting 60 or 93 keV photons from uranium which was obviously the central reason for introducing the mobile counting system there.

And so having said that my first -- and I show a couple things that look at the spectrum and you realize you get a lot of backscattering at the left-hand side which reduces your signal-to-noise ratio and limits your sensitivity by orders of magnitude. In fact, many of the other lung counters that have been in use whether it's at Hanford or (inaudible), they used, instead of four inch, they used four millimeters. And, of course, that would be one-twenty-fifth the thickness, and that would be the most desirable detection system for doing chest counting. And so I couldn't quite understand why --

MR. MORRIS: That might be for plutonium typically where you're looking at much lower energies than that, 60 keV.

DR. BEHLING: But here they also looked at the Thorium-234 daughter as a surrogate for Uranium-238. And that has 63, and it's 93 keV, so --

MR. RICH: But that's Thorium-234, plus it's shown as 235. And 235 had got a --

DR. BEHLING: Hundred and eighty-six keV.

But that, too, is also a problem because it coincides at the 180 backscatter photon that you get from high energy photons. So it, too, has a problem even though it's much higher in energy, it coincides with the 180 backscatter from cesium and cobalt which fall in between 180 to 210 keV backscatter.

MR. RICH: As you know, if you get cesium and cobalt, it's a problem.

DR. BEHLING: It's a very big problem.

MR. RICH: But when you don't have cesium and cobalt, why, you can do a better job. The MBL is a little bit higher. That's true.

DR. BEHLING: And I guess I just couldn't understand why they would select that particular system both for Oak Ridge as well as for Fernald as a mobile unit.

MR. RICH: It's your only game in town.

MR. MORRIS: Probably.

DR. BEHLING: And the other thing that I wanted to, brought it up here, when you look at Thorium-234 as a surrogate for 238, you also have to make some assumptions about 234 because in most instances, that's the radionuclide you're going to assign the highest PCF to. And therefore, it is that particular radionuclide that you're more interested in.

And, of course, that dominates when you start to have an enrichment or at the end if you have a highly enriched, it's U-234 that dominates the activity. And where were the assumptions here regarding, since you didn't look for anything that involved 234, but you used 235 which gives you some indication if you're dealing with enrichment, admittedly.

But it's a fairly complicated process by which you say, okay, I have Thorium-234, and that has a very weak photon energy and a very low yield, and I have a fairly high yield in 186 keV photons from U-235. Now in order for you to understand what's in there in terms of 234, you would have to then weigh those two balanced Thorium-234 against the Uranium-235 photons and get some estimate as to how much 234 is in there.

MR. RICH: Some of these are not done in a vacuum. You've got to know something about the material that you have been exposed do. So you start with some field data and know a little bit about the source of exposure, and then you're able to do it.

DR. BEHLING: And it brings us back to the issue at Fernald where you had everything from depleted uranium up to seven percent and possibly even higher. And so the question is how do we account for 234.

MR. RICH: But they're generally no higher energy emitters in the (inaudible) except for Potassium --

DR. BEHLING: Yeah, and cesium.

MR. RICH: -- a little bit of Cesium-137.

MR. ROLFES: The bottom line is that because we have urine bioassay data, that's going to be our first, most important piece of information or data within the Health Physics hierarchy for reconstructing an internal dose for a person.

MR. RICH: The same thing's true of thorium. You have to know a little bit about the relative equilibrium.

DR. BEHLING: We're going to get into that.

MR. RICH: Oh, you are. Maybe we solved the problem here now.

DR. BEHLING: There are some serious

problems here, and I guess I'm going back just as an opening statement here and said this is not a finding. It's just a comment I want to make here when I talked about the issue of the design system that is not very suitable for low energy photon detection based on the thickness of the sodium iodide crystal.

But the second issue I raised was operator experience. And in one of the memo I remember reading, and I looked at these carefully. The memo stated many lung counts that were made for screening purposes are made under circumstances which require the interpretation of the count results by someone familiar with the vagaries of in vivo measurements. While all count data are contained in the employee's file, not all results are useful as an expression of the true lung burden.

And it's when I gathered the initial year during which the mobile unit was introduced, it was operated by personnel from the Oak Ridge.

MR. RICH: Yes.

DR. BEHLING: After that it was turned over to the people and say you're on your own now. The question is, and I think this is where this statement alludes to, is perhaps the in-house people who at that time took over the operation of the mobile unit were, in fact, properly schooled in operating this systems as well as in interpreting the data.

MR. RICH: I think, Stu, you may be able to comment more on that, but my impression is that that they were, the responsibility for the training was Oak Ridge, and my impression is at least that they were adequately trained.

MR. HINNEFELD: Well, I'm trying to recall. Never operated it myself. People who operated it were trained. They relied on Oak Ridge for the training and the knowledge for, you know, how to deal with the science. You talked about certain exams being called screening exams.

As I recall, any exam where the person had gone to work that day and then come out and had got a count while he had already been in the process area was considered screening, meaning given the contamination environment at Fernald, and it was a contaminated environment, there's a decent chance that a guy could be contaminated when he got in the chamber from his work that day. And so a record count either had to be like a first day back after a weekend off or maybe first thing in the morning, when you came in in the morning after getting back.

That was kind of like some, I think the screening count was one like that where you didn't worry so much about the subject's pedigree. It's what he'd been doing that day before he got in the chamber. That's my recollection. Now, this is more than 20 years ago I'm talking about. I could be wrong on that.

MR. RICH: But the records indicate also they didn't do monitoring for, which was incident driven. In other words if they're involved in something, they didn't count on Monday.

MR. HINNEFELD: Yeah, if the counter happened to be there, and there was an incident, they'd bring people over to the counter, sure.

MR. RICH: And that's another point. This is, was a mobile van that was not there all of the time. It came frequently, at least once a month.

MR. HINNEFELD: Usually, I think it came twice a year normally, and they would count as many people as they could essentially.

MR. ROLFES: The highest exposed personnel like the chemical operators, et cetera, were generally moved to the top of the list or those people that had been involved in an incident --

MR. HINNEFELD: Had a burden, people who had an identified lung burden in the last count, they were normally counted every visit. And so, yeah, those were kind of the selection criteria on who got counted.

MR. RICH: I think a little bit later on the frequency was greater than that, but I --

MR. HINNEFELD: Well, maybe, I don't remember for sure how often it showed up.

MR. RICH: It served a number of facilities, but I think they were maybe down to once a month or so.

MR. HINNEFELD: Well, you had it for a certain amount of -- when it came, it didn't just come for a week and leave. I mean, it was there for weeks, and the counting was done for weeks, and then it left. And I was thinking it came at roughly six-month intervals. It wasn't exactly six months, but I was thinking roughly six-month intervals at least when I started.

But in terms of the operators' ability to use the system, I believe they knew how to use the system because they were taught by Y-12 staff, this is how you use the system and this is what you do. But the system design and really understanding the system, I think, was mainly the Y-12 folks who really understood the system other than a few things that the operators knew locally and going so far as a front-to-back ratio because there were detectors above and below the counting table.

And the front-to-back ratio if a person has a lung burden, should be close to some value, should be actually a little higher I think in the back. The back count, I think, should be a little higher than the front count if it's a true lung burden.

If a person comes in with contamination more likely on the front of their body, and so you can have an extraordinarily high front-to-back ratio which is an indication this is probably a contaminated person who was out in the process area. We need to get him showered and get a record count over here to see if, in fact, that was a burden that we measured or just contamination on his skin.

So were things like that. I mean, that's some of the vagaries of interpretation that they were talking about. But other than that I don't think that Fernald tried to interpret things very much because the whole system is a little bit of a black box that Fernald operated. You know, you put in the number, and it counted the specific regions of interest, and it calculated what was called the expectation value. How many counts they expected to have in that region because of the K-40 peak and the person's size. And then the difference was what the result came out.

And so it pretty much was black box, and even knowing what the region of interest was or what they called the prediction equation was, how did you predict those count, even that was, the Fernald operators by my recollection weren't too well versed in that. That was all provided by Y-12, and it was a sort of a black box sort of thing. That's my recollection.

DR. ZIEMER: Well, I think you're right. It clearly isn't an optimum system, but this is true of many whole body counting systems which were some of the, like all around the country. And for most systems it's the optimum counting, it goes with the sample squared count over background. The background clearly is too big here with the big crystal. And you compensate for that by longer counts and then the front-back business. Also, to do this right you have to have a background for each person. The K-40 peak is different for every person. It's based on your muscle mass. Some people have big K-40 peaks. And, of course, this is probably a cesium peak in here during those years, right?

DR. BEHLING: Yeah, you had, obviously, a fallout that would even for a non-occupational person be --

DR. ZIEMER: Yeah, and the cesium distributes like potassium in the body so that also is a very personal one variable person-to-person. But if you have the person's background and count long enough, you could optimize it even though it's not the best system.

The problem is your low limit of detection is the problem. What you can really see becomes more and more difficult if you have this high background that you're fighting. But I've seen counters with terrific backgrounds that if you count long enough, you can get pretty good results.

DR. BEHLING: Yeah, as I said --

DR. ZIEMER: But you have to have, you've got to take care of the background, the geometry and people have to know how to strip, you're doing a spectrum strip.

MR. RICH: And Hans, (inaudible) came on a little bit lower, and then the jelly detectors came after that. This was the front end of the camel. Whole body counting, the large crystals were good for whole body. It was used as primarily lung counter in this situation, and it functioned with an MDL that was not quite as good as we can do today.

MR. CHEW: Hans, is there a real question?

DR. BEHLING: No, no, again, it was really an issue that says be careful of (inaudible) are the low limits of detection because it may be higher than you thing it is, and it should be.

MR. RICH: And that's right, plus the fact that it represented the state-of-the-art at that time as provided by Oak Ridge.

DR. BEHLING: The next finding is also on page, actually, it's 106 on my copy, the use of surrogate daughter products and unsupported assumptions for thorium exposures. And that is basically an issue here that I think we've just alluded to with Bryce. And that is what do we do with thorium? We have Thorium-232, and we have Thorium-228, and depending on where you are in the process you can make assumptions regarding the relationship between the two. If you start out with virgin ore, yes, you can assume that the two are in equilibrium along with all their daughter products. That's not an unreasonable assumption. But the minute you extract them chemically, you may still have at times zero in equilibrium condition, but in due time you're going to have decay of Thorium-228. It has a half-life of 1.9 years so that in less than two years you reduce it by radioactive decay by a factor of two. At the same time you have an in-growth of Radium-228 which is the daughter product of 232 that has a 6.7 year half-life, and it also now produces Actinium-228 which is your surrogate for 232. Now the question is --

MR. RICH: That's a 5.7 your half-life, building slower, and then with the Thorium-228, with the chain down to again maybe of Lead-212.

DR. BEHLING: Yes, I was. And here's where you have a problem in looking at the thorium. And later on the discussion is, well, we use either Actinium-228 or Lead-212. The question is which one did you use and what assumptions applied, and how old do you know the material was so that you can make a correction. Because at the worst, if you looked at -- you always know you're going to see Lead-212 because you're always going to see as a minimum 35 percent. The relationship between 232 and 228 bottoms out in about seven years or so when you get about 35 percent --

MR. RICH: Forty-seven percent.

DR. BEHLING: Whatever it is.

MR. RICH: Yeah, you look at Lead-212 which gives you a direct, and then you've got to assume that the thorium stays. And then you can get a pretty good estimate of the Thorium-228, but you're only halfway there then because of the fact you've got to know the history of the material at the last process.

So you apply a factor of 1.4 or 1.2, depending on the degree of equilibrium between Thorium-228 and 232. Well, they made a determined effort at Fernald to track and have a good feeling for the separation. And that was used in the determination of the -228 and Thorium-232. And then the mass quantities reported were Thorium-232.

DR. BEHLING: It's very critical because according to the statement here, and it's taken out directly here from Section 6.2 of the TBD. It says, “Thorium-232 and 228 activities were determined based on equilibrium assumptions. The detect was most likely Actinium-228, Beryllium-232, but Lead-212 may have been used for the assessment of both thorium isotopes.

MR. RICH: We used calibration.

MR. MORRIS: It was a calibrated system.

DR. BEHLING: Because if you allow yourself to limit yourself to Lead-212, you could be at the bottom of the curve, and that means you’re only measuring 43 percent present of 228 versus 232, which means you would underestimate --

MR. RICH: That’s just a calibration of the energy from, so that you’d know how much Lead-212 and how that comes out on your spectrum.

MR. MORRIS: Yeah, you need a stable calibration; it doesn’t change by month.

MR. RICH: Then at that point, then it’s a --

DR. BEHLING: But you would need both to assess a person. Suppose a person was counted, and he, at this point, had been exposed to purified thorium. You know very well at times zero the two should be in equilibrium. But unfortunately, Actinium-228 is there, so now you're stuck with 212 as your sole source, and you would have to now make an assumption. What is my Thorium-232 worth?

MR. RICH: If you get a very freshly separated one you're dead.

DR. BEHLING: You're dead because you have no way of knowing --

MR. RICH: You have no daughter product.

DR. BEHLING: That's right.

MR. RICH: You don't have any Lead-212.

DR. BEHLING: Well, you have Lead-212 because it's only a matter of days before the grows in.

MR. RICH: That'd be in a couple weeks.

DR. BEHLING: Couple weeks. I mean, we're not talking, when I say times zero, you could take a few months.

MR. RICH: You might not be completely there.

DR. BEHLING: But the truth is for a fairly long period of time your only indication of thorium present is Lead-212.

MR. RICH: And so admittedly it is, and it requires information related to the process history of the material we're dealing with.

MR. CHEW: It's so fresh the daughters could not contribute to the exposure.

DR. BEHLING: No, no, we're not worried about the daughter. We're worried about the thorium.

MR. RICH: Determining the mass quantity of thorium.

DR. BEHLING: No, I just had that as open-ended question because you have this wide variation in terms of what can be there, and based on what it is, whether it 212 or Actinium-228 that you're using as a means of assessing body burden.

MR. RICH: The process used at Fernald was developed at Y-12 because of the fact they were using large quantities of thorium there also. And the mobile laboratory was developed there and calibrated there and taken to Fernald. So it's an Oak Ridge technology that was used at Fernald.

DR. BEHLING: I guess the next one is Finding 4.4-3 --

MR. GRIFFON: Before we leave dash-two, what is there any action on this or, I mean, at what point do you rely on that data, your dose reconstruction process?

DR. MAKHIJANI: There are data you said on how old the thorium is and so on and you collected it?

MR. GRIFFON: Yeah, that's the question I had is do you have enough to determine the --

MR. RICH: That's not recorded in the calibration, and so it is part of the counting and the correction parameters that went into the determination. All we have is the data associated with the count.

DR. ZIEMER: What does the dose reconstructor do at that point though?

MR. RICH: He reports it in milligrams and records it. Or in later years it was recorded in activity units of Lead-212 and sometimes Actinium-228 which is kind of difficult to do well unless you've got a long-term source.

DR. MAKHIJANI: Well then, how do you translate it back?

DR. ZIEMER: Yeah, what does the dose reconstructor do with that?

MR. RICH: Based on the age of the material, there are correction factors to apply to the activity --

DR. ZIEMER: Does he know that? Does he know the age?

MR. RICH: Well, you have to --

DR. ZIEMER: Or based on the process he assumes a certain age.

MR. RICH: Yes, that has to be so.

MR. ROLFES: I believe our dose reconstruction approach will rely on the air monitoring data that we have primarily that would be the first order, the piece of information. And then if we have specific information in a claimant's file that indicates that their global in vivo results for thorium were greater than our air monitoring data, I think that that would then be our approach --

MR. RICH: However, in no way do we want to imply that the process is efficient. It was a standard accepted process. The fact that the data, the lung counting data, is fundamentally low, it demonstrates for the most part just a few individual that have significant body burdens.

MR. MORRIS: Lung burdens.

MR. RICH: Lung burdens. And as a consequence then in the use of air sampling data to calculate intake, that's much higher, much higher than would be indicated by the lung counting data which, based on where it came from and the procedures that are there, it's an acceptable process by then current standards.

MR. MORRIS: Specifically to the calibration and assumptions of the calibration, I've got a note from Tom LaBone last week regarding how he has modeled the in vivo coworker data using IMBA. He confirms that that was 100 percent equilibrium assumed for calibration purposes, and which would, and then for the modeling he assumes, I think, 42 percent value of the activity ratios if it dips down at four and a half years post separation. And that results in --

MR. RICH: I think the 1.42 is 70 percent. Seventy percent over, one over 70 percent of 1.42 --

MR. MORRIS: So we have a 1.42 adjustment factor that's --

MR. RICH: And that accounts for about a two year after, and it's conservative by a factor of 0.42 in addition to equilibrium.

MR. GRIFFON: And this is documented where or we're still waiting for, I mean, is this in your TBD yet or it's coming?

MR. MORRIS: This is one of those coworker studies that's in progress right now.

MR. CHEW: Thorium, it's a thorium coworker study.

MR. GRIFFON: And did I understand, Mark, correct that you're saying you're only going to use the in vivo coworker model if it results in a higher dose than the air sampling for thorium or -- I'd like to know the decision logic, too, on this. I think it's important.

MR. RICH: It's going to default high.

MR. GRIFFON: Default high.

MR. MORRIS: And I think what we'll really be using our in vivo data for is just to prove that our default values are bounding.

DR. MAKHIJANI: So you're using air monitoring data throughout the period even after 1968 as the primary dose reconstruction data?

MR. RICH: Yeah, basically because we have a significant database of air sampling data -- and check me if I'm wrong -- it's a lognormal distribution, and we're defaulting to the 95 percent.

MR. MORRIS: We are going to allow the dose reconstructor to interpret. We will provide intake rates based on 95th percentile, 50th, and 84th percentile.

MR. RICH: And assure ourselves that has not picked up anything higher than that. And as a consequence that data is there also so it's defaulting high all the way from, significantly high I might add.

MR. MORRIS: I mean, it's not high.

DR. MAKHIJANI: Sorry, which is default?

MR. RICH: The air sampling data, the intakes, by a large amount. Mutty's not here.

MR. SHARFI: They should have streaming chest counts.

MR. RICH: Primarily because there's some uncertainty involved in thorium.

DR. ZIEMER: If they were that high even a nine-by-four crystal would be a (inaudible).

MR. GRIFFON: And do we approach the question of plausibility. That's another factor you have here, I guess. If they were so high predicted, are these just real high numbers or are they actually plausible exposures? It's an SEC question.

MR. ROLFES: They're based on monitoring data.

MR. SHARFI: They're a bounding scenario and we're taking the upper end. You're giving them every day for an entire year when you're looking at the upper end. They were sampled for probably a short period of time. You're probably going to end up over assigning the overall intake over the course of a year.

MR. RICH: And this adds to the fact that the operation for thorium, and because of the limited amount of thorium handled, less than a metric ton per day, this is going to bias and default high because of the, we're assuming, full-time operation.

MR. SHARFI: Three sixty-five.

MR. RICH: And so all of it's going to come out large doses.

MR. CHEW: I think Mark commented it's so high it doesn't make sense.

MR. GRIFFON: Is this just a way to avoid the fact that you don't really have enough information to calculate a good dose, you know? I mean, you're just throwing a high number at the problem.

MR. ROLFES: Sure, when we complete a dose reconstruction keep in mind when we're assigning intakes to compensate people, say for example, if they have a positive uranium urinalysis result. Rather than reconstructing each individual acute intake, what NIOSH does, we can demonstrate pretty quickly that if a person has positive bioassay, rather then fitting each of those positive bioassay to separate, episodic events, we assume a chronic intake across the board. And that's an accepted method that we've used to compensate people. So in my opinion I think that these exposures are plausible and of sufficient accuracy.

MR. GRIFFON: I'm just throwing that out there for the work group to consider. We need to see that, the model, yes.

MR. SHARFI: Normally chest counts,

especially it's in the soluble form. I don't know if the body burden then becomes so outrageous that, the chest count, the chest burden would become so outrageous that way over predicting. The systemic organs would be using the air intakes and looking at the Type M which would be obviously more claimant favorable. And you're probably now looking at a gross overestimate of what the chest burdens should have been. Like a lung cancer. You'd look either some Type S, and your intakes are very large, you should consider this acute build up of thorium inside the lung.

MR. CLAWSON: So we should be expecting to see a coworker data for thorium and for uranium?

DR. BEHLING: Finding 4.4-3, it's a question about what the selection criteria --

MR. GRIFFON: Hans, I'm sorry. Just to go back to 4.4-1, the same, are you using the uranium in vivo for anything or the same sort of scenario? I've got the sense that you always the urinalysis for uranium, right?

MR. ROLFES: Yes.

MR. GRIFFON: Do you ever use the in vivo or just maybe to check or --

MR. ROLFES: Exactly, basically if we assign one of those chronic intakes, this isn't a typical dose reconstruction. It's probably more towards a best estimate-type dose reconstruction. What we would do when we would assign an intake based on urinalysis data, we might check to make sure we're in the correct ballpark by comparing that urinalysis data, or excuse me, the intakes estimated the urinalysis data to the actual lung burden observed just to give us confirmation that we're in the correct ballpark of the worker.

DR. ZIEMER: And if for some strange reason the lung burden gave a higher dose, then you would use that, right? Or would you?

MR. SHARFI: Are we talking about an individual case or --

DR. ZIEMER: Yeah, an individual case.

MR. SHARFI: I would assume you'd be looking to try to get both to agree whether it's, I mean, you might end up becoming where you're mixing intakes where you might be looking at an insoluble and a soluble form of intakes where you might use the chest counts to estimate your insoluble form, and the urinalysis to estimate your soluble form, very case specific.

MR. GRIFFON: And for the coworker I don't think they use it at all, right?

MR. ROLFES: For -- I'm sorry.

MR. GRIFFON: For coworker I don't think you're planning on using it at all, right?

MR. ROLFES: The in vivo data, I don't believe we are going to incorporate in vivo data into the uranium coworker model. I believe that's strictly urinalysis.

MR. CLAWSON: Would we like to take a comfort break? People on the phone, we'll be back in about 15 minutes.

(Whereupon, the working group took a break from 3:08 p.m. until 3:23 p.m.)

WORKER SELECTION CRITERIA AND INFREQUENT USE OF MIVRML

DR. BEHLING: Four-four-three. I guess there, there I was again questioning, and it goes back the early issues where we had these unexpected counts of uranium urine data for those four individuals. And here's a situation where in the first statement that's taken out of the TBD it says lung counting became available, it says, in ’68 in the form of a mobile unit and so forth.

And then it goes on to say workers were counted on the schedule that’s based on internal exposure potential in their urine sampling. So there was obviously selection criteria by which people were selected. Not everyone was counted but the attempt was to count the people with the highest maximal exposure potential. I take it as that.

But then I looked at a Health Protection Appraisal report that was issued in September of ’68 that had some second thoughts about it because it says in a recent in vivo monitoring of NLO employees utilizing the mobile unit, da-da-da-da-da, a serious question has been raised regarding the validity of the job-weighted air dust sampling approach long used by NLO since that data would not suggest lung exposure to these individuals at the in vivo indicated levels.

In other words they observed a disconnect between air monitoring data for people who were obviously monitored for thorium who had the high potential and then found that perhaps that correlation did not exist. And the question is, is there a potential that indicates where people who were not counted but should have been counted.

And I guess that's the issue here, the selection right here. If we count everyone, then there's no question. If we count a select one, the question is did we count the right people. And here's a question that was raised where air monitoring data for thorium people did not match the expectations for in vivo measurements.

And, obviously, it wouldn't matter as you said towards uranium since you're more or less relying on urine data as opposed to in vivo chest counting for uranium. But that was the issue here for this particular finding is that were the selection criteria necessarily good enough to say those who were not counted didn't have a potential for thorium exposure just because they weren't counted.

MR. RICH: I guess all we can say is that their stated intent was to count the very high people, and based on the people in the database that we got from, they were operators and the like --

DR. BEHLING: Apparently a lot of people because in that same memo further on it stated it is therefore, noted with concern that only about half of those potentially subject to exposure have been monitored by the RDRML during this year. Meaning that obviously 50 percent were not counted. And the question is were there people there that should have been counted but for reasons that they were not necessarily considered high-risk candidates were not counted. And so it's an issue of data, complete data.

MR. ROLFES: Also in the procedure that describes the people that were, in fact, monitored, if they weren't monitored during one trip, I believe they were pumped up a little bit on the list for the next trip that was made by the mobile in vivo lab if they were in one of those higher exposure categories. This is just purely from memory, and I'd have to look back into the record to get the exact procedure for selection criteria for those workers.

DR. BEHLING: But I would assume again here if a person was not necessarily monitored by in vivo measurements, the air monitoring data would still apply as a coworker model?

MR. ROLFES: Yeah, exactly.

DR. BEHLING: So as a minimum we use that as a default approach rather than saying you weren't monitored; therefore, you were not necessarily at risk, and therefore, you could not --

MR. ROLFES: Correct, correct.

DR. BEHLING: Yeah, the coworker model satisfies an awful lot of questions, open-ended questions.

MR. ROLFES: We certainly understand that.

THORIUM LUNG COUNT DATA

DR. BEHLING: Finding 4.4-4, this is something that you're probably going to answer, and I will withdraw this, and that is interpretation of Table 6-2 in the TBD that's been introduced in this document on page 111. And I probably should have contacted some of you. I may have got an answer before I actually wrote this up.

And that is the curious issue of converting thorium body burdens or chest burdens reported in milligram quantity as opposed to Lead-212 and Actinium-228 in activity values. And that transition, although, and what’s so strange here, if you look at that table that I incorporated, 6.2, and it’s introduced here in as Table 4.4-1 on page 111, you have as early as 1965, you have two counts that were recorded in terms of activity of Lead-212 and Actinium-228.

And after there is a sprinkle of (inaudible) there, two in 1968 and a couple more and so forth. But for the most part the assessment for chest counting involving thorium that made use of Lead-212 or Actinium-228 were very few. There’s only 15 for the time period of ’65 through ’77. On the other hand, if you look at the fourth column under thorium, you see in the year 1968 there were 310 classified as thorium counts.

Now, I wasn’t sure what that really represented. Why the conversion on your flip-flop between activity values expressed in units of activity for Lead-212 and Actinium-228 as opposed to milligram quantities of thorium? And I sort of interpreted this possibly I'm probably mistaken here. That they were not really looking at Thorium-232 and 228, but they were possibly looking at Thorium-234.

MR. RICH: It certainly wouldn't be recorded in milligrams.

DR. BEHLING: No, it wouldn't be because it would be in extremely small quantities.

MR. RICH: And if you're in the claimant file, your claimant record, you'll see frequently Thorium-212 and Actinium-228, but as a general rule in the initial records it was nearly all recorded as thorium milligrams. It should be interpreted as Thorium-232.

DR. BEHLING: Well, this is what confused me because I did pull up a couple records, and I brought one here, and I crossed out the name of the individual. And up to the timeframe of 1978, they were reported in terms of thorium milligrams, and your nanocuries for the daughter product. And I really was puzzled by what this really was. And I wasn't sure whether the earlier years, up to 1977, most of those assessments did not really reflect the thorium that we were concerned about, mainly

Thorium-232, 238.

MR. RICH: No, it's all 232.

DR. BEHLING: It was all 232. And is there any indication as to how those numbers came to be. I mean, it seems strange that, as I said, throughout that time period if you look at that table, there are just a handful that were expressed in activity units for the two daughter products. And the rest, the bulk of them, were expressed as thorium milligram, and it just doesn't seem --

MR. GRIFFON: Just the reporting convention at the time?

MR. MORRIS: Well, I think it was a reporting convention switch. My recollection from looking at a whole set of air sample, I mean, lung counting results is that there were occasionally people who were sent to Argonne, Argonne National Laboratories, and they came back with different recording conventions. And that may explain why we had some in nanocuries in earlier years. But the really vast majority of workers counted at the in vivo mobile laboratory, and so I think what you see is just a gear shift from reporting from Argonne.

DR. BEHLING: And I accept that. I just, I was puzzled, and I wrote it up because I felt, well, perhaps this is here where thorium was interpreted to mean something very different from what we thought it was.

MR. RICH: As Bob indicated, the coworker data is -- Tom LaBone is making the conversion from Lead-212, Actinium --

DR. BEHLING: Activity values.

MR. RICH: -- to compare with the --

DR. BEHLING: Right now it would be very troublesome to try to convert these.

MR. MORRIS: Tom is doing that.

MR. RICH: And for that reason it will all be consistent.

DR. BEHLING: And I will obviously acknowledge that issue here because, as I said, we were just puzzling to me and was my interpretation that the real bioassay for Thorium-232 and 228 did not really commence until about ’78 when you see all of a sudden where we talked of near conversion although thereafter, they’re still milligram reported again. It’s now flip-flopped, and it’s hard for me to understand how you could have a crew of people operating the mobile unit, and then in some instances reporting it one way, and in another it's another way, and the flip-flopping.

MR. GRIFFON: The flip-flop's harder to understand because ANL wouldn't have gone back to, you know.

MR. RICH: Well, ANL didn't count them all. They were counting them locally, but they just sent them down for the inner calibrations.

DR. BEHLING: Well, as I say, I accept your explanation, and the assumption is that somebody will look at these data and re-interpret them and convert them into common units of activity.

MR. MORRIS: It's certainly happening now on the coworker study, and I think largely that is what they're using this data for anyway. So that probably will suffice.

DR. BEHLING: The next one is one that we touched upon this morning --

MR. GRIFFON: Hans, does this address the whole Finding 4.4-4? It talks also about correlation with air sampling data. Am I reading this wrong? At least in the matrix it says --

DR. BEHLING: Now, the air sampling data is really for thorium, and the uranium data is for the, you know, when people were selected under 4.4, the statement here is that they were selected based on urine data and air monitoring data. The urine data was used, it says, okay, you had high urine data. We're going to assess you with chest burden for uranium. You had high air monitoring for thorium. We'll assess you for a chest burden of thorium and so forth. And so, yes, as we started out by saying we don't really care about the urine correlation because the primary source for dose reconstruction is always going to still be the urine data only as a back or up perhaps as a confirmatory way to assess the urine data will mobile in vivo data be used. But it's not really the primary data.

MR. GRIFFON: No, I understand that, but you were talking about the data discrepancy in the in vivo counts, but you didn't really talk back to this question of the correlation of air data versus in vivo. Or maybe we already covered that. We discussed that before. I just wanted to make sure we didn't miss anything.

DR. MAKHIJANI: Well, I don't know whether that remained as an action item after the --

MR. GRIFFON: Yeah, I think the action I have in the previous one was to, I think I had an action item. NIOSH was going to provide the in vivo coworker model. We've kind of got to wait and see that model.

OTIB-0002

DR. BEHLING: Finding 4.4-5 on page 111, again, we question the application of OTIB-0002 for efficiency reasons, and I think we discussed this morning. I'm still questioning whether or not the assignment of the 28 radionuclide mix on the first day of employment necessarily will cover all bases for all workers, especially those who were long-term workers and for all cancers.

I guess it would be at least some effort to assess, based on your new models and new assumptions regarding intake of uranium and thorium whether or not OTIB-0002 would, in fact, transcend any potential exposures assigned by those particular models. And I think it needs to be looked at.

MR. ROLFES: Once again, we don't really feel this is an SEC issue. OTIB-0002 was definitely used in the earlier days before we had detailed, site-specific information. And this was essentially an approach that NIOSH adopted to essentially provide the claimant a timely response and answer for their claim, basically yes or no as to whether the probability of causation would be greater or less than 50 percent. We do realize, now that we have additional data, this additional data can be used in lieu of OTIB-0002 so --

DR. BEHLING: I would assume that any person with a reasonable employment period but had cancers involving things such as lung cancers, lymphomas, bone cancers, liver cancers would not have been assigned OTIB-0002 as a way of, I mean, you must have had some screening methods for saying this should never be applied to certain types of cancers.

MR. ROLFES: And typically for those cancers that you mentioned, those are typically organs that tend to concentrate radioactive materials. And essentially, because materials are deposited within those organs, they receive more dose. And simply, you know, to complete it, the other side of the efficiency method that if we have an individual with a couple of positive bioassays, we can do a simple underestimate and compensate that person for a lung cancer based on --

DR. BEHLING: My concern was more towards a person who may have had a radiogenic cancer that's associated with uranium and thorium, but may have been a non-rad worker you may say, hey, we're going to be generous to this guy or this person and give him the OTIB-0002 treatment and see where we fall. And he may have had a cancer involving lymphoma or bone cancer or lung cancer or kidney or liver cancer. But on the basis of the fact that that person may not have been in his or her and the evidence that they were ever monitored, come to the conclusion that there was no exposure. Even though the cancer was the sort of cancer that might highly susceptible to an internal exposure to these two isotopes was dismissed and say, okay, we'll just use OTIB-0002. I don't know that that took --

MR. SHARFI: On this lung cancer and stuff, OTIB-0002 is very specific that it is assigned to soluble intakes. And so stuff like lung cancer that are more accessible to insoluble materials would not, cannot even be used for OTIB-0002. And OTIB-0002 is specific on what organ it does apply to, and really more of the systemic system for more of the organs that are more radiogenically sensitive like a bone surface and like that.

To assign OTIB-0002 would be to pay someone. And then I believe like the bone surface dose using OTIB-0002 is like 3,000 rem. It's so high you could never use it as an overestimate for a very sensitive organ. So it's more limited to you radiogenic-sensitive organs like the prostates and stuff like that that you can do these massively overestimates and not because radionuclides don't compile inside this organ you can give them these large intakes and not see large doses.

Or the more sensitive like the liver and kidney and those organs, red bone marrow, bone surface, OTIB-0002 would, it would be almost impossible to use an overestimate approach because they'd end up resulting in a compensable which you can't use an over-efficiency method for a cancer. We'd have to then go back in actual claimant information and do either a better or a best estimate.

MR. ROLFES: That's another important point. In dose reconstructions this is a simple, it's essentially a worse-case scenario that is applied. And, for example, for a prostate cancer there's, it's going to be very difficult to establish a probability causation of greater than 50 percent from internal dose for a prostate cancer.

MR. SHARFI: Tritium and stuff like that that has whole body --

MR. ROLFES: Sure, simply because of the biokinetic models. And even if, for example, if air monitoring data, I know we have a lot of discussion about air monitoring data. Even if the air monitoring data were orders of magnitude higher, still in most cases, certain organs are still not going to be, likely be compensated based on, based purely on biokinetic modeling.

However, organs such as the lung or respiratory tract, those are obviously much more affected by insoluble materials than, for example, a systemic organ such as the prostate. So the claims that would be most affected by a change in air concentration would be those claims that we're already compensating based on the bioassay data that we have. So we can debate the issue of the differences in observed air concentrations, but the net effect on claimants I don't see as being very significant.

Sandra.

MS. BALDRIDGE: Because OTIB-0002 was used on my father's claim we are locked into it until NIOSH gets their site profile revised. The Department of Labor will not send my father's claim back with all the additional information that I've provided on thorium to even consider his exposure for three and a half years. We are locked into it. Now I think the law says plainly that dose reconstruction has to be based on exposure at such site where you're exposed. The use of OTIB-0002 has been written into the regulation that has allowed NIOSH to use it. It is not a provision under the law because the law does not permit the substitution of data from one site to another site.

MS. HOWELL: Actually, it does. The law has been interpreted by the Department to allow values from other sites.

MS. BALDRIDGE: Interpreted.

MS. HOWELL: It's been interpreted. It's up to the Department of Health and Human Services General Counsel's Office and the Secretary himself to interpret how --

MS. BALDRIDGE: The data being allowed to be substituted for another site?

MS. HOWELL: There's a whole reason that we don't that the Board is aware of.

MS. BALDRIDGE: Yeah, but it should have been --

MR. GRIFFON: We've actually set up a work group, you might want to mention.

MS. HOWELL: They are looking into science behind the uses of data from other sites, but currently, they're allowed to do that.

DR. ZIEMER: As a general principle whether in a specific case it's appropriate might be subject to interpretation. As a general principle we can do that.

MS. HOWELL: As a general policy in legal matters, you can use it. The question of --

DR. ZIEMER: It's not an across the board thing.

MS. HOWELL: -- the Board is the science and the question of whether or not it's appropriate, and that's why we set up the working group.

DR. ZIEMER: We have a new working group that's looking at that issue.

MR. GRIFFON: But OTIB-0002 really isn't even another site. It's not data from another site so it's --

MR. SHARFI: It's based off like ten percent, I think, of the maximum --

MR. GRIFFON: It's a high number.

MR. SHARFI: Yeah, they've basically taken a huge intake and said --

MS. BALDRIDGE: I thought it was based on (inaudible).

MR. SHARFI: No, OTIB-0002, it's based off the legal, I think the --

MR. GRIFFON: Maximum limits of the time period.

MR. SHARFI: Time period, yeah, and assume that they basically gave them, you know, I believe it's ten percent of that for every single, 28 different radionuclides all at once, and by putting it in the first year you could maximize the dose that you're assigned over time.

MS. BADLDRIDGE: Then they're addressing the time limitations that are included in OTIB-0002 as well for applications outside the --

MR. SHARFI: The dose reconstruction should, there are some time limitations that they need to, if they're going use anything that obviously is outside I believe the 1970 OTIB-0002 they need to defend why they think it's still operable to that particular case.

MR. GRIFFON: But I think that is an interesting point that you make, but in trying to appeal this, they're bringing site-specific data, and their appeal is being rejected because it wasn't based on site-specific data.

MR. SHARFI: That would be the person at the Department of Labor. I can't speak for that side.

DR. BEHLING: It does seem to have a conflict in the sense where efficiency is encouraged under the regulations, but at the same time if you look at the hierarchy of data, there's no substitute to real data. And you're actually then substituting new data for hypothetical data that's not even applicable to any one site at all.

DR. WADE: And that's a tension that we all live with under law. We need to be complete, and we need to be timely.

MR. GRIFFON: I guess the one thing I said in this, at least in the matrix is that -- and I understand this from going through Rocky I think where we're going to end up with this is that any changes that are made through this process, if they result in the modification of a DR approach that may affect any of these claims that have been made and reassess them. That doesn't do much for your time of waiting, but it -- When a change is likely to -- so if the thorium model for a certain subset of workers ends up being very high, and it could affect OTIB-0002 rulings, then you would go back to those plans.

MS. BALDRIDGE: [Name redacted] going to be 94. You're talking timely.

MR. GRIFFON: I know. We do have the timely question.

MS. BALDRIDGE: And this has been going on for seven years.

PERSONNEL DOSIMETERS

DR. BEHLING: Let me go to the next section, Section 4.5 on page 113, and the first finding is stated as absence of performance standards/quality assurance for personnel dosimeters. It's truly accepted that Fernald provided external dosimeters for its employees. But the question is to what extent can we look at the data and say that they were sufficiently accurate in assessing external exposures.

And I took some of the statements out of the dosimetry assessment fact sheet that was dated September 11, 1981. And in there it basically says that all dosimeters values where in-house except for approximately the first 12 months of operation. And so it was a dosimeter system that was processed by in-house personnel. At the same time there are statements to the effect that there are no procedures available for how these dosimeters were processed.

And statement number three, test dosimeters were not routinely processed, meaning that calibrations was bypassed. There was also an issue about accountability for dosimeters that were at times not properly stored. They were kept in people's cars in heat weather and under environmental conditions that would obviously raise havoc with the response of these film dosimeters.

And there were no specific training requirements for the badge technicians unlike today where we obviously have very, very strict criteria under various accreditation programs where people have to be qualified to operate the equipment and the processing of TLDs of dosimeters. None of that really existed.

In fact, there was only one technician who had been assigned to this. And while he may have been qualified, but there's no documentation to that effect. So the question is one of the absence of performance standards and quality assurance for personal dosimeters.

And clearly by today's standards we would obviously have reasons to be concerned about the qualifications of these people who essentially were people who learned on the job as opposed to having some form of documents that we provided some proof that they were qualified to do the job they were asked to do. Again, there's not much we can do but accept that as a limitation in terms of accuracy for the dosimetry system.

The next issue --

MR. GRIFFON: Do we have any --

DR. ZIEMER: Just a question that they were using the Oak Ridge system. Is that the understanding?

DR. BEHLING: Yes.

DR. ZIEMER: And did Oak Ridge process the badges or did --

MR. HINNEFELD: Fernald processed the badges.

DR. ZIEMER: Fernald processed them. Using an Oak Ridge methodology or, I mean, you're talking about developing film and reading --

MR. HINNEFELD: Developing film and reading with a densitometer.

DR. ZIEMER: Did they calibrate with their own sources and so on?

MR. HINNEFELD: Yeah, it's in the report, you know, the continuation of the response to that questionnaire is that they shot calibration badges and read those and drew a densitometer curve using optimal density versus dose or generate a curve for each of the badges read.

So as they developed a set, they would then, they would also at the same time they were developing the personnel badges, they would develop their standard values, the calibration values for that batch. So they had a calibration per batch, per development batch. And so those were then, you know, that was a calibration then for that batch. I mean, I'm just reading from the report.

MR. ROLFES: As I was told in an interview by a former employee at Fernald was that the badges were calibrated to a slab of uranium metal, and the net result was that the dose recorded by a person wearing a film badge would have been higher than the actual, actuality is what I'm trying to get out, because of the criteria. Basically the dose that would have been recorded by the film badge would have been higher than what the employee would have actually received, and I thought that was of interest to relay.

MR. HINNEFELD: I didn't quite follow that.

MR. ROLFES: All right. I apologize. I'll try to clarify. I guess the badges were calibrated with uranium metal slabs. And I guess because of the age of the material, I guess to allow for Protactinium in-growth, I guess some of the beta dose for a person working with fresh uranium metal, I guess some of the beta dose would have been, I guess --

MR. RICH: It would be lower than the standard. The calibration curve would overestimate the --

MR. HINNEFELD: You can explain it to me later.

There was a point in time when the calibrations were done with radium, calibration films were shot with radium with this and so they did that for the photon calibration. And I think the uranium slab may have been the open window calibration.

MR. RICH: The skin dose.

MR. GRIFFON: Do we have a set of reports that discuss the QA?

DR. BEHLING: No, that issue is that we didn't see anything.

MR. GRIFFON: As an action is there anything that we can follow up on this to find more supporting documentation that would say there is a QA program going back to the early years. It might be worth us seeing more documentation to support that is all I'm saying.

MR. ROLFES: We've been told that instructions did exist, but we haven't been able to locate them. And we should probably look in Oak Ridge as well.

MR. GRIFFON: I mean, I would say as an action item, attempt to recover those kinds of supporting documents.

MR. HINNEFELD: What do you expect them to find along those lines, Mark, in terms of QA program? What would you think would be evidence of that?

MR. GRIFFON: I guess I would, wouldn't there be some sort of quality assurance reports or QA reports or sections of the Health Physics reports that might have a section on quality assurance?

MR. CHEW: How about in a comparison study?

MR. GRIFFON: Yeah, in a comparison study.

DR. ZIEMER: Of facilities?

MR. HINNEFELD: Well, the first ones I'm aware of were the preparatory evaluations for Golab*.

DR. ZIEMER: That would be much later.

MR. RICH: During the early days the Oak Ridge badge was used at most of the facilities. That was the first one out of the box, and so as a consequence I do know in the early days there was inter-comparisons between the laboratories. And I'm not sure that Fernald participated in those.

MR. HINNEFELD: Oh, yeah, I don't know about --

MR. RICH: I don't know about Fernald specifically, but I do know what was --

DR. ZIEMER: Internally many facilities will expose badges intentionally to see if the technicians who read it out get the right value. It's at least an internal check. They may be completely off compared to the rest of the world, but at least they're consistent internally. So you need both I think.

MR. CHEW: Mark, I think we understand what you're trying to go for. So maybe the action item is that we'll make an attempt to look for some control for the dosimetry badge process.

MR. RICH: Then again, it was the Oak Ridge technology that was used at Fernald just like other plants.

MR. FAUST (by Telephone): This is Leo.

DR. ZIEMER: Hi, Leo.

MR. FAUST (by Telephone): That dosimeter was the Oak Ridge dosimeter, and it was included in many inter-comparison studies with other sites including Hanford. And it did compare very, very favorably. And that's documented in some of the Parker papers.

The other thing that occurs is when the badge was calibrated, it was in fact calibrated to a uranium slab. And it was exposed on an individual that wore clothing, and the clothing actually attenuated the dose of the uranium by about 20 percent. The badge did not have the intervening clothing between it and its source. So the net result would be that the badge would actually give an exposure that was higher than what the individual actually received.

MR. ROLFES: A much better job of explaining that than myself. So thank you, Leo.

DR. ZIEMER: Leo, do you know the particular Herb Parker reports or are they Hanford reports or --

MR. FAUST (by Telephone): It's in the Herb Parker --

DR. ZIEMER: In the book?

MR. FAUST (by Telephone): That book on Parker. I've got it some place around here.

DR. ZIEMER: You can track it down.

MR. FAUST (by Telephone): Story I think. And we've referenced it at several different times, and it's on the, I think it's on the O drive quite frankly. It's called Herb Parker, Herbert M. Parker.

It's a compendium of a bunch of his personal papers and letters and speeches and that sort of thing put together by Baehr* and Kathryn* and somebody else.

MR. CHEW: Leo, did the years that Herb’s study or assessment, was it covered in the book there?

MR. FAUST (by Telephone): I didn’t get that. Please repeat it would you, please?

DR. ZIEMER: What years did he cover in his report?

MR. FAUST (by Telephone): I think the very first one was like 1948, and it goes up through --

DR. ZIEMER: Okay, the early years. That’s what we wanted.

MR. FAUST (by Telephone): I know, but it starts there around ’48 or ’49 and it goes up through the ‘50s and ‘60s.

MR. CHEW: We’ll take a look.

MR. FAUST (by Telephone): The other thing, I’m trying to track down some people that we interviewed and talked to by phone insisted that there were written instructions of one sort or another that governed the processing of the dosimeters. And I’ve got all of the Health and Safety laboratories because they did the first 15, 18 months of processing. And I’m trying to track down something out of the Oak Ridge organization that may have governed the use of that dosimeter.

MR. CHEW: Thanks, Leo.

DR. MAKHIJANI: I have a question about a later period that you raised about when TLDs were first introduced and they had that adjustment factor to account for the contamination of the TLDs and sometimes resulting in negative radiation doses. It’s in volume four of the Westinghouse Transition Report. It’s in our TBD review.

And I was told that these readings were never entered into the worker dose records, but I’m not convinced, by my reading of the Westinghouse Transition Report, I think they were, the corrected readings were entered. And when they had a correction of more than 50 percent, they said -- or negative radiation dose -- they referred them to Health Physics.

But there’s no indication of what happened. That’s a black box. And I think there’s an 18-month period in 1983 to ’85.

I've written it up in the TBD review, but it's nowhere addressed what happened to these correction factors that were obviously wrong. I mean, they were yielding results that were not physically possible in some cases.

MR. CHEW: Do you remember anything like that?

MR. HINNEFELD: I remember it. I was thinking it was for skin doses only, but I could be wrong.

DR. MAKHIJANI: I do believe so. I think it --

MR. HINNEFELD: Yeah, it was, well, the practice started -- gosh, a little history here. The practice started because when Fernald first switched to TLDs from film, they started getting skin dose-to-gamma ratios that were far larger than anything they'd seen and skin doses that were far larger than anything they'd seen on the film even though their film badge had performed well in the early Golab accreditation, you know, getting ready for Golab, and those inter-comparisons to film had really done pretty well. And so there was this puzzlement about what had happened here, and there was speculation that construction of the badge gave rise to a, there's a small ledge on the face of the badge right in front E-1, Element One. That's why I was thinking it was a skin dose adjustment. Where that became contaminated because Fernald was a contaminated environment, you would have an extraordinary large dose from that contamination on E-1, the first element of the TLD, and skin dose was derived from the ratio of Element One to Element Two, so you get a very high ratio and therefore, a very high dose that was incorrectly attributed to the dose to the skin when based on that little bit of contamination on the badge. So that was the speculation, and that's what gave rise to this contamination adjustment factor. It was contamination on the badge, and how we would adjust that. I think really what happened, the real problem with the dosimeter was that the algorithms were converting the E-1/E-2 ratio into skin dose was incorrect, and it took a few months to figure that out.

DR. ZIEMER: This was a commercial vendor and all?

MR. HINNEFELD: This was Panasonic TLD inside of a Fernald badge because it was still a Fernald security badge. So it was --

DR. ZIEMER: Read out here?

MR. HINNEFELD: Yeah, read at Fernald. But the algorithm for conversion was developed by the University of Michigan, and they did the preliminary testing of the badge, the Panasonic TLD in the Fernald badge, did the exposures, the radiations, and developed the algorithm for converting the E-1-to-E-2 ratio into skin dose.

And the error came there, you know, came back from the algorithm. Took a few months to sort out that this algorithm isn't right. And then that gave rise to some more with Idaho to come up with another, you know, what would be a better approximation algorithm for the E-1-to-E-2 ratio. So the error in the algorithm was that they put a polynomial with five data points, four of the data points were on one end of your data range, and the other one's at the top.

And so you've got this kind of a funny looking thing like this which should have been a uniformly assembled curve. So that was the evolution. That's how it started. That was the origin of that factor, and the end of the factor was sort of a recognition that, hey, you know, dosimetry results should be right on the individual case not on the average, whereas, there might be an average contribution.

I think the contamination adjustment was derived empirically, you know, get some bad news, to a certain extent, find out, you know, just leave them and read them and find out what dose you get on that badge based on contamination level. I think that's how the adjustment was developed, but and that's sort of an average approach to things. It just seemed like the dosimeters ought to be correct in the individual not in the overall average.

And so the practice was suspended before, shortly before the Westinghouse transition, before they took over. So that's my recollection of it. I really thought it was only a skin dose adjustment though.

DR. MAKHIJANI: Maybe, and I may not be remembering it right.

MR. HINNEFELD: That's my memory of what the evolution of it was, and it was strictly a skin dose, E-1/E-2 ratio explanation that gave rise to that. I think that's the case. I won't swear to that, but I think that's the case.

DR. MAKHIJANI: I may not be remembering it right, but some examples, actually, it doesn't say here. But some examples are given in Table 9 of our TBD review, and they're drawn from --

MR. HINNEFELD: Yeah, a contaminated badge, an unusually contaminated badge --

DR. MAKHIJANI: They're all over the map.

MR. HINNEFELD: -- and it would blow that adjustment factor. Clearly, it couldn't have been correct as you said. It was just the fact that it was bigger than the measured dose. So that did happen. In those cases the adjustment factor wasn't applied correctly, and there were probably maybe a dozen. And I don't really recall the resolution of that. As you said, above a certain fraction it was referred to somebody for investigation, but I don't really recall the outcome. How those investigations were conducted.

DR. MAKHIJANI: And when we raised this issue in a conversation with NIOSH, NIOSH said, oh, the doses were not entered into the dose record, but that's not the impression I got --

MR. HINNEFELD: See, I don't know whether that's true or not.

DR. MAKHIJANI: -- from reading the transition document to my knowledge the issue's never been resolved.

MR. HINNEFELD: I don't know. Originally, they were recorded I believe, as the adjusted doses I believe were originally recorded. They could have been backed out, you know, uncorrected later on, but I don't recall that they ever were.

MR. GRIFFON: So we need an action follow up on this?

DR. MAKHIJANI: Yes, I think we need to know --

MR. GRIFFON: This doesn't really fall under any of the findings, does it?

DR. ZIEMER: It's sort of performance standards of personnel dosimetry.

DR. MAKHIJANI: In the TBD finding, finding number 19, no, sorry. It's finding 20 in the TBD review. Correction factors used during an initial period of use of TLDs at Fernald are not scientifically appropriate. So --

MR. CLAWSON: And under our matrix that would be 4.5-1?

DR. BEHLING: No, it wasn't discussed.

DR. MAKHIJANI: No, it wasn't discussed in the matrix. It's just, it's covered under that umbrella item, but I think it sort of falls into the finding we've just been discussing except we're doing specifically (inaudible), but it should be, I think there should be some resolution for this question.

MR. FAUST (by Telephone): This is Leo again, and I could very well be mistaken, but it was my understanding that during that transitional period the Oak Ridge dosimeter was still used, and that that was the dose of record. That may or may not be correct, but that's my understanding of it.

MR. HINNEFELD: Now, Leo, the Oak Ridge dosimeter stopped, using the Oak Ridge dosimeter stopped when the film badge was adopted. There were maybe one or two months of overlap, but by the time you get into the Westinghouse transition period, they'd been on TLDs for about a year or so at that point.

Well, I mean, there was a very short period of time when people wore both, the TLD badge and the film badge that they'd worn before, a sort of inner comparison. And then after that it went straight to TLD.

DR. MAKHIJANI: My impression, if it had just been experimental, I think there would not have been this issue in the transition of what happened with all this with readings given and correction factors and so on. So that's why I say that it appears, although I'm not sure, but it appears to me that these were doses that were attributed to individuals.

MR. HINNEFELD: They originally -- I'm pretty confident -- originally there was some adjustment made before the dose was recorded. That's my understanding. I'm pretty sure that did happen. I don't know if later on they were unadjusted retroactively. I don't know if that happened or not.

DR. MAKHIJANI: I don't know. So this is something that obviously needs to be resolved.

MR. CHEW: Do you want to state the issue, Mark, so we all understand it?

MR. GRIFFON: I have general actions at the end of this, but I didn't tie it to any matrix item, and this is one of those. I said NIOSH would follow up on the doses assigned in the beginning years with the use of the TLD badge and what data was recorded, and I think that captures the question. And beginning years I'm saying '83 to '85. Is that the time period?

DR. MAKHIJANI: Yeah, I think that timeframe is given in the transition report. I think it was 18 months or two years or something like that.

MR. HINNEFELD: Sounds like it would have been, yeah, sounds like it would have been from early '83 to middle of '85.

DR. MAKHIJANI: I think so. I think it was something like that. Maybe it was 30 months.

MR. HINNEFELD: Yeah, it may have been. It may have been '84. When the heck did it change?

DR. MAKHIJANI: I don't remember.

MR. HINNEFELD: I don’t remember when. Somewhere in there, ’84, ’85.

MS. BALDRIDGE: There is some mention in one of the documents in the petition about them enclosing the badges in plastic bags, and why, the reasoning for that so there might be some insight.

MR. HINNEFELD: Yeah, the plastic bag was an attempt to keep the badge from getting contaminated so we wouldn’t have to worry about this adjustment. We didn’t have to worry about the badge getting contaminated. Throw away the plastic bag and --

MR. FAUST (by Telephone): That’s a non-issue anyway because the bag was, when the procedure was put into place, enclose the badge in a plastic bag, it was also calibrated in that plastic bag. And that would have taken care of any discrepancies between the unplastic bagged dosimeter and a bagged one.

UNACCOUNTED DOSES TO EXTREMITIES

DR. BEHLING: Finding 4.5-2 is unaccounted doses to extremities, and I know that, at least for some people, wrist badges were given. As was already mentioned, the ratio between skin dose and deep dose are the ratio varied considerably over time. And I've discussed some of the numbers that were cited. The ratios in some instances were as high as 20-to-one, and then they were reduced to five-to-one. So there were periods of time when skin doses were extremely high and probably due to the presence of Protactinium and exposure to that.

And in one of the documents that I enclosed as Attachment 4.5-2B, the following statement appears: "NRO has performed a study of exposures to the forearms of some Plant 5 employees. The results of the study showed projected annual forearm exposures from about 14,000 to 46,000 millirem. According to NRO estimates about 300 employees would require extremity monitoring because of potential exposures to their hands. It appears necessary that further attention be given by NRO to this matter."

And I guess the question I have is how many people may have been exposed to large extremity doses but were not monitored. And we can't necessarily rely on a ratio that is highly variable as a function of time. And I know that some people wore wrist badges, and we can make adjustments on behalf of those wrist badges. But did everyone who may have been exposed to their forearms handling uranium necessarily have wrist badges?

DR. MAKHIJANI: Well, just as an addition to that I think that wrist monitoring started in 1970. Is that right? I think that's the --

MR. HINNEFELD: 'Seventy-seven?

DR. MAKHIJANI: -- so before 1970 there was no extremity monitoring data to my --

MR. HINNEFELD: I think it was 1977. I don't think it was 1970. I think it was '77 just from the stuff I've read.

MR. ROLFES: Once again, this would be a limited subset of claimants that we would be doing dose reconstruction for. This would have to be essentially a skin cancer on the individual's hand, and anyway, we do have data for extremity doses recorded at Fernald.

And the obvious application of this data would be important for a skin cancer located on a person's extremity. That would be the application. Very few claims would be affected. The total number I could give you, but anyway we do have extremity doses that were made using those wrist dosimeters and a wrist-to-extremity ratio.

The ratio varied with the changes in the dosimeters. It actually did decrease with the introduction of the TLDs; however, we don't believe that there was an adjustment, a retrospective adjustment to actually correct the over-reported doses to the extremities. These are also things on a, these evaluations can be done on a case-by-case basis.

And we don't feel that this is an SEC issue because this can be bounded based on claimant-favorable assumptions and source term information as well.

DR. MAKHIJANI: Is there a model for this especially before 1977 or coworker model or how did you handle it?

MR. SHARFI: This is now really different than geometry which is essentially glove box work really.

MR. HINNEFELD: It's really not much different than that.

MR. SHARFI: We're basically talking about basically geometry.

MR. HINNEFELD: It’s a geometry adjustment.

MR. SHARFI: Right now like for Rocky we had to look at hand-to-wrist, and wrist-to-hand ratios. I don’t think this would be any different.

MR. FAUST (by Telephone): You guys, there was a big study done by Joan in determining what that ratio was, and the finding or the results of her study indicated that the ratio was actually less than what the ratio was that was being used to find extremity doses, but no adjustment was made to account for that lowering. It was left the way it was. I’m sure that happened while you were there, Stu, in the late ‘80s probably.

MR. HINNEFELD: I remember her study, and I don’t remember what all she investigated, but I was under, I did think that that had been sorted out. But there is a reasonable ratio, if someone does not have extremity monitoring, it does not mean that their extremities were not more heavily monitored and they were more heavily exposed on their whole bodies.

So if they have a cancer on the

extremity, you have to make an adjustment for the measured dose to account for the extremity to the ratio between the badge and the extremity. And I'm pretty sure it's available, if you say that Joan's study has it in there, I don't recall that specifically. It could very well have it in there.

It seems to be a pretty tractable issue. I mean, the jobs that gave rise to hand dose compared to whole body dose I think are pretty easily recognizable. And as long as you've got data from those jobs, I think you can bound that ratio.

MR. FAUST (by Telephone): This was actually a ratio between a wrist dosimeter the extremities rather than a whole body dosimeter and the extremity.

MR. HINNEFELD: I think even then in many cases you'll have to (inaudible) the ratio to the whole body badge because a lot of people only have a whole body reading, and you're going to need that ratio, but I think that is a tractable problem. I think if there are data available that allow you to do that from various time periods, they may be a later time period, but the physics of the radiation from the material isn't changed over the 40 years of the operation.

MR. ROLFES: From working on this project for, I guess, five years I've probably seen two cases where there have been extremity skin cancers. Other cases that I've reviewed I've probably seen two that I recall where we had indication that the person was monitored for extremity dose in a later time period, and what we did is actually use the rem from the time period, for the time period that he wasn't monitored. We had basically used his data from a later time period and basically made sure -- I believe Mutty may have been involved in --

MR. SHARFI: I also quit the case.

MR. ROLFES: Back and forth between us a little bit. We wanted to make sure that we filled in the gaps in the data with claimant-favorable extremity dose.

MR. SHARFI: I believe later in his career he did have extremity dose, and we could (inaudible) his personal (inaudible) of geometry, et cetera, (inaudible) since he had some extremity dose. We could look at the dose badges that he had, both full body and extremity, we could calculate his own ratio. And then at that point we could apply, we could back calculate that to a ratio to all his other full body dose to his extremities.

MR. GRIFFON: You don't have any procedure right now for Fernald?

MR. SHARFI: That would have been a case-by-case --

MR. GRIFFON: Case-by-case --

MR. SHARFI: It was such a rare situation when we do have an extremity cancer, not to say that we've done a --

MR. GRIFFON: I think there's a few of them. I've looked at a couple Fernald cases recently that there's cancers on the temple and neck and head. And it raises this question of the derby workers where we've heard testimony that they were going in these things cleaning them out, and if their whole body badge is representative of what their head getting to their upper extremity, you know?

DR. MAKHIJANI: There is that, yes. The workers put their heads in the graphite crucible --

MR. ROLFES: The difference in dose reported by the whole body dosimeter versus the head would in my opinion be much less than the factor between the whole body badge and the extremity.

DR. MAKHIJANI: Well, I don't know. In this situation --

MR. GRIFFON: It's a badge situation.

DR. MAKHIJANI: You'd have some shielding from the crucible itself because --

MR. GRIFFON: And it's really inside.

DR. MAKHIJANI: And then I think that there a quotation and a description of this particular problem in our TBD review. It came up in a worker interview. And it is in an appendix, the full interview is in the appendix to our TBD review. And it was explicitly culled out in the body of our analysis.

MR. GRIFFON: But I think I tend to agree with Stu. I think it's a tractable issue, and, I mean, what's our other recourse here. It's not a listed SEC cancer so realistically, we're going to --

DR. MAKHIJANI: That doesn’t matter --

MR. GRIFFON: That doesn’t matter, exposures exposure, I know.

DR. MAKHIJANI: No, no, but for SEC you’ve got to cover all the cancers even though they’re not among the...

SKIN/CLOTHING CONTAMINATION

DR. BEHLING: On the next one, this addresses the issue of perhaps shallow and even deep doses that are not necessarily monitored that could have resulted from skin/clothing contamination. I will accept the notion that people were monitored while they were at work.

But you also have to accept the notion that this was not a very clean environment in which they worked. Add to that the fact that they were not normally provided anti-cees and even in the, as late as a 1985 report, the observation was as following: “There are no contamination survey instruments kept at the work site for use in checking for skin and clothing contamination. Neither are there hand or shoe counters available to use before or after showering.”

And it goes on further to discuss other issues involving the limited effects of showering that were not necessarily abided by by our own people. Now the question is to what extent can a persistent skin contamination or even clothing if a person wears the same clothing day-in and day-out, it keeps it in a locker and the thing's just laced with contamination. Is he receiving a very high skin dose that is not necessarily monitored by his whole body badge?

And obviously, even if it is, during the time it's worn the fact is the badge stays home and he goes home and he wears the same clothing. And if it's a persistent skin contamination that may be there for days and days and days. And of course, that is not going to be monitored by a badge that's hanging some place else.

So the question is again, based on the fact that this was a fairly dirty environment, there's likely to be a significant number of skin exposures that will not be properly monitored because this simply, the data isn't there. In fact, what I have here was on one of my attachments early on.

And this was in light of the issue surrounding thorium, but there a particular memo that I included here. This is on page 61 of my report that talked about the cleaning of the under burnout oxide conveyors in Plant 5. And it talks about something that really in this day and age would (inaudible) anybody out. It talks about up to about a year the operator had to position himself under the inspection plate to remove it for access under the oxide conveyor.

This caused much of the oxide to come down upon him. Breathing zone samples resulted from this operation were found to be 9.3 million DPM per cubic meter. So this is an incredible high air concentration that was measured by an air sampler. And this stuff obviously he was laying on his back face up, and this stuff would come down.

And so you can imagine the kind of skin contamination on his face, especially in his hair that he would have received from when this kind of operation took place. And I think it was one that wasn't necessarily monitored or dealt with in terms of decontaminating the individual.

So it's just an issue here that I wanted to bring out about skin cancers, and we have to be very mindful of potential skin cancers that will not be properly assessed based on whole body dosimeters that may not have been very effective in assessing exposures as a result of persistent skin and clothing contamination.

And as I said, there were no anti-cees, and there were no frisking of personnel at the end of a shift who were coming out of an RCA area. And so we have to deal with the unknown that says there may have been very, very profound skin contaminations.

MR. ROLFES: We don't feel like this is an SEC issue because we feel that we can bound this issue. We can bound the dose from skin contamination --

DR. BEHLING: But it's not monitored. If you have data, you can certainly make an attempt based on DPM per unit of area you can come up with some assessment of skin dose, but where you don't monitor it, and you don't document it, what do you have to work with?

MR. ROLFES: Well, we could look at the dosimetry results which we have because if the contamination was in proximity to the dosimeter being worn, that would, in fact, be recorded by the dosimeter.

DR. BEHLING: Partially.

MR. ROLFES: The other issue is we could do a VARSKIN calculation to determine a ballpark estimate and pretty much demonstrate that dose from skin contamination is relatively low. Dose rates from skin contamination is relatively low. The workers did typically take frequent showers before lunch and before going home so any physical skin contamination would have been observed and would have been removed at the time of taking a shower. So it's possible that some contamination, we know for a fact that if you review the historical photos that this occurrence did, in fact, it was routine, you know, the head skin contamination.

MR. GRIFFON: Is that true? They showered before lunch and going home?

DR. BEHLING: Let me read to you something on that issue.

MR. GRIFFON: That surprises me especially in the old days that that would have been a practice.

DR. BEHLING: In fact, this is Attachment 4.5-3A page 124. Let me read to you on page 124 of the report. It makes reference to the drum bailer in the drum reconditioning building only those men involved in the cleaning the bailer will be required to make a complete clothing change. Only those so obviously you were highly restrictive request for clothing change to people, certainly not, this was not a universal requirement.

MR. HINNEFELD: I think that pertains to a special clothing change mid-day, during while you’re out there. There was particular occasions -- this doesn’t speak well for the cleanliness of the plant -- there were occasions when people would get so dirty from whatever job they were doing that supervisors would send them or they would give them permission to go now, shower and change into a new set of clothes because they wore company-issued clothes. Go now shower and change and then come back out without waiting to go to lunch.

And there was a shower, in order to get through the locker room, you had to go through the shower. So you could intentionally avoid the shower, but to go from the locker room where you took off your company-issued clothes to the side of the locker room where your street clothes were, you had to go through the shower.

MS. BALDRIDGE: I believe there's a document in the petition where it describes them laundering the wool and the cotton filter bags from the air collectors in the same facility that they're laundering uniforms. I don't know what kind of --

MR. GRIFFON: Reissuing contaminated --

MS. BALDRIDGE: -- right.

MR. MORRIS: Every facility in America does that. They have a lower detection threshold cut out from recycled coveralls and I don't know of any reactor that doesn't have that.

MR. CLAWSON: It also came up with an awful lot of europium, lot of other isotopes even around coming back and giving them to other people. And we've got that today.

MR. GRIFFON: But if this was really the policy that they showered after their shift, for sure they showered before they went home, then I would see this as kind of a minimal potential here --

DR. BEHLING: Well, I've seen persistent steam contaminations that days and days and days of scrubbing wouldn't take off. So a simple shower is hardly adequate to ensure that there's 100 percent removal.

DR. MAKHIJANI: It may be useful to do a sample VARSKIN contamination. Mark, would it be useful to do a sample VARSKIN contamination for the case that --

MR. MORRIS: Yeah, we're in the process of doing that.

DR. MAKHIJANI: No, for the particular case that Hans read out which is that infamous 97.

MR. MORRIS: Well, obviously some of that is going to fall off. You know, it's not going to stick on like glue. It's not going to be --

DR. MAKHIJANI: I'm not telling you how to do the calculation. I'm just saying it would be interesting to see an example --

MR. GRIFFON: What kind of doses are we talking about?

DR. MAKHIJANI: Assuming that, I think the job lasted for five hours or something. I think it says in the first memo. The page of the memo is not in the report, but it actually says in this memo how long the job lasted. Well, you could do the calculations --

MR. FAUST (by Telephone): Somebody was just mentioning the therapist dose rate for an infinite slab of uranium is 230 plus or minus a few rads per hour. And if anybody's going to get any negligible dose, you should be able to see the uranium. It's inconceivable to me that anyone can have a dose of any concern whatsoever from residual contamination on his skin, and certainly not on his clothing because if it's any magnitude at all you can see it.

MR. CHEW: Well, Arjun is shaking his head positive so maybe we can stop there.

MR. CLAWSON: One thing that Hans says about the shower and so forth, this is from personal experience and wearing a glove for a week and a half, it doesn't all come off. So, you know, I've done the scrub. I've done the whole nine yards. There's still, you know, it may not be not much, but it's something that we need to be able to address because I think especially with this facility. I think it's something that we need to look at a little bit closer.

MS. BALDRIDGE: And not everyone wore a uniform. A lot of the contractors worked in their street clothes and left in their street clothes and took it home.

MR. HINNEFELD: That would be true of contractors. There were probably contractors who did not change out and probably wore their own clothes.

MR. GRIFFON: So they walked through that shower with their clothes on?

MR. HINNEFELD: They would not have gone through that shower. No,

MR. GRIFFON: So there was other ways to get out of there.

MR. HINNEFELD: If you didn't change into company clothing, you didn't have to go through that shower.

DR. ZIEMER: What about portal monitors?

MR. HINNEFELD: Not until mid- to late-’80s.

DR. MAKHIJANI: So what do we do about that one?

MR. GRIFFON: Still, you’ve got this uranium limitation. I mean, the physical limitation we still have, but I don’t think you have any way to address assigning additional dose to people that, you know, to contractors that may have, I mean, even though it would be small, and there’s no current method for assigning additional dose, missed dose sort of?

DR. MAKHIJANI: Yeah, that’s what I’m asking. Is there a procedure? I didn’t see it in the --

MR. GRIFFON: I’m sure there’s not.

DR. MAKHIJANI: I did not see it in the construction worker. I don’t remember.

MR. HINNEFELD: I don’t think the construction worker addresses it. I think NIOSH has an action here to kind of come up with some discussion about is there some sort of logically bounding or logical approach about this. Because there were certainly people got it on their skin and got it on their clothes. And clothes that came out of the laundry weren't necessarily completely decontaminated either. So there may be some necessity here to at least decide is this something we have to account for or not. And if not, why not?

DR. MAKHIJANI: As a helpful thing perhaps you might, we had this discussion at Bethlehem Steel, and there was a different facility with uranium and steel mixed in. You have to discount for that, but there a methodological discussion around, and it might be useful to revisit it.

DR. BEHLING: And while the dose rate even from a slab is a little, but I realize that some of these people worked there for years. And so even a modest dose integrated over a long period of time, you're not dealing with inconsequential skin doses.

DR. MAKHIJANI: I agree with Hans. I think if I'm recalling even at Bethlehem Steel after we were done assuming that people wore their clothes all, the kind of scenario that Sandy is talking about. I think once you get into people wearing the same clothes that were contaminated, then the doses became non-negligible although I'm saying this from memory. Jim Neton would know because he was involved in resolving that issue.

MR. CLAWSON: It was something that was they wore their clothes every two or three days and laundered and so forth?

DR. MAKHIJANI: Yes.

MR. CLAWSON: I just vaguely remember something like that.

MR. GRIFFON: Yeah, the details on that.

DR. MAKHIJANI: Ed Walker who supplied that information.

NEUTRON DOSES

DR. BEHLING: The next one I think we may have partially addressed this morning regarding the issue of neutron doses. And again, I'm going back to the original TBD where they assess neutron/photon ratios for a single using repeated measurements and came up with a 95^{th} percentile in gamma ratio 0.23. And I looked at that and said, well, I'm not going to contest empirical data. It's there, and if it's done properly that the value.

But the question we had is a single

necessarily a limiting factor in assigning neutron/photon ratio. And what we ended up doing was to run our own calculation. One of our people in-house, and some of you met him, ran a calculation using different configurations of drums. And what he found out -- and this is in Attachment 4.5-4A, and this is now on page 132 of the report. You can look at the n/p ratios that we calculated.

And for a two percent enriched uranium drum array, we had an n/p ratio of 0.42 as a deterministic value. And that's nearly twice the 95th percentile value that NIOSH had derived. So we're nearly double, but we're using a deterministic approach rather than the 95th percentile. So that's more an average.

And, of course, that significantly different from what you calculated. But then again you say you have empirical data that you have looked at that will support the earlier n/gamma ratio 0.23. Now, we haven't seen that data so this is an open-ended issue.

MR. ROLFES: It's one of our actions. We'll provide that information to you.

MR. MORRIS: I wanted to make a

clarification. Dr. Ziemer --

MR. GRIFFON: I'm sorry, let me capture that action before you say anything else.

MR. ROLFES: Earlier from our presentation we had been discussing the measured neutron dose rates, and then, but this was from Warehouse 4B these measurements were conducted.

MR. GRIFFON: So you're going to provide the data.

MR. ROLFES: Yes, we'll provide this information.

MR. MORRIS: To make that clarification, Dr. Ziemer has asked the question what kind of instrument was used to make the measurements and Leo Faust has told me that the record shows the instrument was a Nuclear Research Corporation model NP-2 which is the Snoopy that some of us know about. It had its own readout, but in low dose rate measurements it could be used with an integrating meter to select a variable period of monitoring time. And for these measurements a ten minute monitoring period was used. It was calibrated offsite to a plutonium-beryllium standard.

DR. BEHLING: Is that instrument energy sensitive?

MR. MORRIS: Yes, it is, just like United's Trim Meter. It's got a very similar energy response curve.

DR. BEHLING: And the plutonium-beryllium has what? A five meV average neutron energy?

MR. MORRIS: They tend to over-respond. United Trim Meters and Snoopies together alike tend to over-respond in the middle energies under keV up to one meV sometimes by a factor of two. The higher energy calibration will offset that to some extent compared to the californium calibration, but still you get an over-response than this would have been.

MR. CLAWSON: Arjun, before he leaves then, we've only got one more to go.

UNMONITORED FEMALE WORKERS

DR. BEHLING: Two more, yes, and the last one involved unmonitored female workers. We're at the last. I never thought we'd even come close. And the reason we brought this up is because there is an accepted statement in the TBD that women were not monitored for various periods of time. But one of the things that was also just brought up, the issue Sandra just brought up, was the commingling of perhaps laundry with collected dust bags.

And in my report as one of the attachments, we see some activity levels in dust bags of, in those days it was reported in terms of millirem, up to five millirem per hour of after cleaning and 30 millirem before cleaning. And these things were laundered by women who themselves were neither monitored internally nor externally.

And that also brings up the issue the came up subsequently. That is, what happens when you throw in those dust bags with other laundry that may be laundered and that people may wear as anti-cees. The question is, there are multiple aspects to this issue.

Women who were consistently not monitored internally and externally, bags that had a fairly high contamination level that would have exposed them and potentially contaminated, cross-contaminated, other things that people would wear the next day. So we have a series of potential open-ended issues here.

MR. ROLFES: Well, I think we addressed this in part in the current technical basis document by saying that if we have indication that a women was not monitored, we, by default, will assign 500 millirem per year to that individual, to that woman. And this actually exceeds by far the recorded doses received by many of the process operators at Fernald.

So I believe that's very defensible right off the bat. There's other approaches that we could adopt to address this issue as well. By looking at what the individual was doing, the area that she was working in and look to see what kinds of doses the coworkers were being received -- excuse me -- what kind of doses her coworkers were receiving. There's issues -- excuse me -- there's approaches to this issue that we can adopt in order to bound these doses and so we don't feel this is an SEC issue.

MR. GRIFFON: You don't have a coworker model for external right now.

MR. ROLFES: There's no coworker model for this.

MR. GRIFFON: So you wouldn't use the 50th or 95th because you don't have that data compiled.

MR. ROLFES: No, exactly, we've been assigning doses, like I said, that actually exceed the recorded doses by production personnel of 500 millirem per year.

DR. MAKHIJANI: I think a part of the resolution of this may be linked to the findings of the three women who were, who had the internal uranium burden --

MR. GRIFFON: Which you're going to follow up with that.

DR. MAKHIJANI: -- and you're going to follow up on that. So I think we may link the resolution of this to the findings because you have high, very high internal dose due to some exposure. Then this may also become an issue.

MR. FAUST (by Telephone): This is Leo again. The unmonitored females, I don't know whether that included the lack of bioassay data or not, but I would assume that it did. I think there are several ways of assigning a plausible dose to your workers and Mark has suggested a couple of them.

Another one would be the same female during the periods that she was monitored, whatever that, and was doing the same job, you could assign that dose then to those periods of time when she was not monitored. And I think it's pretty defensible.

MR. CLAWSON: That would be fine if all the processes were the same. Say (inaudible) issued them or whatever like that, it would be different filters. They may have started another process, and that means a little bit more background check into what had changed over the years if we were trying to use that.

MR. GRIFFON: And again, the 500 millirem you reviewed production worker raw workers and just sort of determined that this is higher than the maximum? Or did you --

MR. ROLFES: I believe this approach was likely adopted from the five rem per year and the justification that it wasn't necessary to monitor --

MR. GRIFFON: Sounds like it's one-tenth of it, yeah.

MR. ROLFES: -- someone if they didn't have the potential to exceed ten percent of the annual dose limits.

MR. GRIFFON: So it's going to the likely to be monitored if you exceeded the --

MR. SCHOFIELD: How much, was there a lot of in vivo done on any of these women? Any in vivo measurements, any urinalysis?

MR. ROLFES: Well, we have documented the urinalysis results that Arjun and Hans have out earlier. The women did, in fact, participate at least in a physical -- excuse me -- in an annual physical where a urine sample was, in fact, collected from them. As far as in vivo, I'm not certain.

In the later years it's very likely that they were in fact. But I think this issue is more gear towards I think right around the 1960s when females weren't routinely monitored. There's a couple of time periods that are documented in our site profile for Fernald that indicates the time periods where women weren't monitored. And in the more recent time period when women were working in the production area, those women, in fact, did have in vivo monitoring as well.

MR. HINNEFELD: Was there a time period when people, women, were not monitored but they were allowed to go into the production area?

MR. ROLFES: Not that I'm aware of.

MR. HINNEFELD: I don't even know. I mean, this predates me by a good bit, but I was always told that at the beginning of Fernald when they started up, women weren't even allowed to go in the production area and so they weren't badged. That's what I was always told.

MR. MORRIS: We heard in one interview that there were always exceptions that could be approved. If somebody wanted to visit for some specific reason that that could be arranged. But it was not a routine.

MR. GRIFFON: So that policy would seem to support the 500 millirem being very claimant favorable. Is there any action on this one? I'm not sure other than following up on those other cases.

DR. MAKHIJANI: I think the main action was to follow up on these two cases. Well, there were four, but one was a man.

DR. ZIEMER: We tied that in with the other.

MR. CLAWSON: Well, then we did it.

MR. GRIFFON: Can I go back before we close up. We've got plenty of time left.

The last item on the n/p ratio question, I just wanted to, I'm not sure it's an action, but I think maybe I need to look at the report a little closer. Maybe it's already been outlined. I haven't looked that closely at this issue for Fernald. But the question we raised, Arjun raised, I think I mentioned it earlier, our experience with Rocky.

And it's not so much the comparison of the operations but the comparison of the approach using the n/p ratio and the appropriateness of it if you are, and I don't know how. I've got to look. Maybe you've already outlined this, but it seems like you're applying one n/p ratio across the site for all time periods. Am I wrong on this?

MR. ROLFES: What we are assigning is the 95^{th} percentile --

MR. GRIFFON: Ninety-fifth, but it's not by year by building. It's for all time periods for all buildings or is it building-specific?

MR. ROLFES: That's correct. It's across the board, 95th percentile.

MR. GRIFFON: And is that, and that data, I mean, do you have any annualized data on this, the data that you're going to provide? The survey data was only --

MR. MORRIS: It was only 1998.

MR. GRIFFON: Nineteen ninety-eight.

MR. MORRIS: I think 4B was 1998.

MR. GRIFFON: So we don't have anything from early periods or early time periods. I'm looking at this.

MR. ROLFES: Off the top of my head I know that there are some other reports back in the '80s. I believe late '80s. As far as prior to that I'm not aware of any.

MR. GRIFFON: And I guess the one difference in, or one of the differences from what we were doing at Rocky is that at Rocky we had several different potential source terms for neutrons that complicated the matters for the ratios. So here you've got the one type of source term only. Is that pretty...

MR. ROLFES: Well, there are potentially other source terms; however, the total contribution from neutron dose in everything that we’re aware of is very miniscule.

MR. MORRIS: Thorium chloride was handled, but that’s such a low neutron emitter that it’s not even tabulated.

DR. MAKHIJANI: But mostly it would be the uranium tetrachloride and the uranium hexafluoride in that brief period. I think the n/p ratio complication may come in because there’s also radium and things onsite. So the Plant 2,3, the raffinates, from the pitchblende and, you know. I’m not talking about neutrons from radium. I’m talking about the denominator of the n/p ratio. If the denominator goes up, then your n/p ratio will go down.

MR. HINNEFELD: That would be relevant if data from there were used in developing the n/p ratio.

DR. MAKHIJANI: That’s right.

MR. HINNEFELD: If the data from somewhere else --

DR. MAKHIJANI: No, but I guess that only from the drum -- well, we just have to look at the way --

MR. GRIFFON: Yeah, we have to look at how you’re deriving --

MR. MORRIS: I think we understand that question.

DR. MAURO (by Telephone): Morris, as a reminder though that attachment -- this is John -- that you referred to I think does place an upper bound, theoretical upper bound, which basically give you, really could not get a greater neutron-to-photon ratio and the value derived using that mc-np calculation we ran in the attachment to your report.

DR. MAKHIJANI: For that physical arrangement.

DR. MAURO (by Telephone): Yeah, the reason we made that arrangement is to create a situation where you get the maximum amount of shielding of the gamma so that because there are multiple containers stacked, and as a result you get the highest neutron-to-photon ratio. I forget the number. What was the number? If it was one or two or something like that?

DR. BEHLING: Three four one.

DR. MAURO (by Telephone): That’s a high number without a doubt, and we deliberately constructed as a plausible scenario because I think there were large amounts of, I guess it was uranium hexafluoride stored. And that is what we believe to be the highest neutron-to-photon ratio that theoretically possible. Now it may not have existed anywhere at the site. It's important to note that though that there is a way to place an upper bound. And certainly, if you have some real measurements at real locations that show that, the reality is it's lower than that. But I think it's important to keep in mind that it is a tractable problem in terms of placing an upper bound on what it might be at the site.

MR. HINNEFELD: I don't know if this matters or not but in looking at the NP analysis in your report, the two percent array is a critically unsafe array.

DR. MAURO (by Telephone): Is that correct?

MR. HINNEFELD: Yeah.

DR. MAURO (by Telephone): There you go.

MR. HINNEFELD: Yeah, you wouldn't stack three 65-gallon drums with two percent UF-4 together.

DR. MAURO (by Telephone): Then the number would be even more --

MR. HINNEFELD: You probably wouldn’t stack, in fact, we normally put it in cans. Or they normally put it in ten-gallon cans, but this would be a critically unsafe array. Normal (inaudible) be stacked.

DR. ZIEMER: Then your neutrons are going to change.

MR. HINNEFELD: Yeah, if you’ve got a ratio, you don’t want to mess with it.

DR. MAURO (by Telephone): (Inaudible) change.

MR. HINNEFELD: You could have a normal array in that arrangement, but you wouldn’t have a two percent array in that arrangement.

DR. MAURO (by Telephone): Okay.

MR. CHEW: Do you want to revise your theoretical calculations?

DR. MAURO (by Telephone): I think I better fix that, right.

MR. HINNEFELD: Use the normal drum array value. That’s very close to what we have.

DR. ZIEMER: It’s good for a microsecond.

DR. MAURO (by Telephone): I can’t wait to tell Bob that, Anigstein. I finally got him on one.

MR. CLAWSON: Any other questions?

(no response)

MR. CLAWSON: Clarifications?

(no response)

MR. CLAWSON: Lew?

ACTION ITEMS

MR. GRIFFON: Do you want me to read through all these actions?

MR. HINNEFELD: Yeah.

MR. GRIFFON: In starting I listed all the actions with the findings so 4.1-1 I have the seven actions. And I read through these already, but I'll go through them again.

NIOSH to review assumptions on enrichment level. Two is NIOSH to provide references regarding enrichment levels. Originally I had SC&A but now we know that it's the Bogar 1986 reference. So I guess we're going to be able to track that back from DOE. Was that the idea, Stu?

MR. HINNEFELD: We should be, that should be easily findable, I say naively.

MR. GRIFFON: So NIOSH to recover this reference is what I changed that to.

Three, NIOSH to provide sample DR to demonstrate approach for doing internal DR for uranium. And Mark, you said you may have one of these already but adjust it if you need to or whatever and make sure we know where it is.

Four, NIOSH to examine whether approach is appropriate for all members of the class parentheses, is there a subset of workers or areas where a different assumption should be made? That's with regard to enrichment levels.

Five, NIOSH to review the total production numbers for uranium, paren, provide written responses clarifying differences in the numbers in the TBD versus other documentation.

Six, NIOSH to provide claim numbers of workers that worked in blending areas or high enrichment areas.

MR. CHEW: Worked in what areas?

MR. GRIFFON: Blending areas I think is what Mark, or other high enrichment areas.

And seven, NIOSH will examine issue related to renal failure and effects on uranium excretion and on DR approach.

And then I’m on to 4.1-2, and I can send all, I’ve got all of these in matrix. I can send it out so if you were frantically typing. 4.1-2, NIOSH is attempting to recover laboratory procedures and QA reports from the early time period, ’54 through ’80.

Two, NIOSH to post HIS-20 database. I put paren, with all identifiers, because I’ve been around this block before, on the O drive.

Three, NIOSH to recover urinalysis logs and/or Health Physics reports that can be used to verify HIS-20 database data and post on the O drive.

And on that one I said NIOSH to recover. I should say NIOSH will attempt to recover because I’m not sure they’re available as you said. Do you have a question on that?

MR. MORRIS: I thought you were asking us to delegate the HIS-20.

MR. GRIFFON: No.

Four, NIOSH to compare selective cases with lung count data and urinalysis data.

DR. MAKHIJANI: Would that include also --

MR. GRIFFON: I’m trying to remember what that meant.

DR. MAKHIJANI: I know what it --

MR. MORRIS: Let me tell you what my recollection of that was. We had a very small number of people with lung count, elevated lung count data, and those are the only people really that make sense to try to compare to urinalysis.

MR. GRIFFON: Elevated lung count cases. I'll put it in parentheses so I remember. Yeah, that was it.

DR. MAKHIJANI: And are we going to compare also with air monitoring data or not?

MR. MORRIS: We could, but I'm not sure it makes much, I would be surprised to find any really good results by that method. We can look.

MR. GRIFFON: Since there weren't, it didn't come up before. I mean that would be new.

DR. MAKHIJANI: So now is air not included?

MR. GRIFFON: My sense is you don't have air sampling data for uranium, right? I mean, you have it, but you haven't compiled it yet. You haven't compiled it yet, right, in any usable fashion.

Four-point-one-dash-three -- I have

two actions. NIOSH will provide coworker model along with all analytical files on the O drive. That's the coworker model for the --

MR. MORRIS: Urine analysis as it becomes available.

MR. GRIFFON: As it comes available, yeah.

Two, NIOSH will follow up on individuals identified in the memo cited in the SC&A report. If any are claimants, NIOSH will assess the elevated urinalysis results. This is the three women that we just discussed, right?

DR. MAKHIJANI: Yeah.

MR. GRIFFON: And then 4.1-4 it says see actions in 4.1-1. So we kind of covered the same thing.

Four-point-one-dash-five, NIOSH will provide update on RU feed and raffinate assumptions in the site profiles revision. So this is in your site profile revision. Including material flow information.

Two is NIOSH will post thorium air sampling data, paren, gross alpha and Thorium-230 data.

I think I captured everything, but if

I didn't, somebody feel free to chime in.

DR. MAKHIJANI: We're following along with you.

MR. GRIFFON: Four-point-one-dash-six I don't have any action on that currently. Now, at this point I don't know that that means that item's closed out, but we just don't have an action right now.

Four-point-two-dash-one, NIOSH will provide recently recovered data on the --

DR. ZIEMER: Four-one-six we said was, would be covered by the action in 4.1-5.

MR. GRIFFON: Did we? Okay.

DR. ZIEMER: At least that's the note I have.

MR. GRIFFON: So see 4.1-5.

DR. ZIEMER: Four-one-five is covered by 4.1-6.

MR. GRIFFON: Four-point-two-dash-one, NIOSH will provide recently recovered data on the O drive. And that's, paren, radon breath, thorium air, radium-slash-thorium activity ratio data, but you may have already given us that. I'm not sure. I just added that in. But it's in there if we didn't get it already.

The second one, NIOSH will provide new model along with supporting analytical files, and that TBKS-0017-5 Internal Dose Section.

Four-point-two-two, I don’t have anything for that. It may be that it --

DR. ZIEMER: It’s also covered by 4.1-5.

MR. GRIFFON: See 4.1-5.

DR. MAKHIJANI: There’s also the recovering the Gilbert Report, the Anigstein Report.

MR. GRIFFON: I’ve got that coming up somewhere. Keep that, Arjun, if I missed it, but I think I’ve got it in a later action.

Four-point-two-dash-three, NIOSH will provide Pinney data, I said, from the, that’s okay to reference her since it’s her report, right? Pinney data and reports on the O drive. The data and her reports if you have that. I think you have both, right?

Two, NIOSH will provide updated model for the Environmental Section, TBKS-0017-4.

Four-point-three-dash-one, NIOSH is revising the thorium model using air sampling data along with location, job and year. NIOSH will provide this model to the work group.

MR. MORRIS: I think we could just refer you to that Battelle Report 6000 or 6001, I think. It's in our --

MR. GRIFFON: So it's the same Battelle model? It doesn't even use the Fernald data in that model?

MR. MORRIS: We'll just put our air sample data in it.

MR. GRIFFON: In that model, okay.

MR. MORRIS: Yeah, but we did not change the model.

MR. GRIFFON: So the model's there, but the data we need to see, right.

MR. MORRIS: So do you want to just (inaudible) the action and (inaudible) to the data. Is that right?

MR. GRIFFON: I think so, yeah.

MR. HINNEFELD: Which I think we covered previously.

MR. GRIFFON: I thought it was adapting that model for Fernald, but you're using the same exact model.

MR. MORRIS: Exactly, I think we clarified how some of the coefficients were derived because it wasn't obvious in their write up.

MR. GRIFFON: Okay, is that in your TBD though?

MR. MORRIS: It's in our TBD draft, yes, but we didn't change any numbers.

MR. GRIFFON: So I guess there's no action here on the model.

MR. MORRIS: Right.

MR. GRIFFON: Then I have NIOSH will provide analytical data used for the model on the O drive. Okay, so that's the one that stays.

All right, 4.3-2, I say, see 4.3-1.

Four-point-three-dash-three, NIOSH will provide as part of the model mentioned in the response to 4.3-1 the decision criteria to be used to determine how workers will be placed into the model. This was from Stu's comment. So it's the decision criteria for how you're going to place workers, and that may be rolled into your TBD or wherever it falls. I don't care.

Four-point-three-dash-four, see previous actions.

Four-point-three-dash-five, see previous actions.

Four-point-three-dash-six, NIOSH will post thorium in vivo data. I have '68 to xx.

I wasn’t sure --

MR. MORRIS: ‘Eighty-eight.

MR. GRIFFON: To ’88, yeah, I couldn’t remember.

MR. MORRIS: We may have already done that.

MR. GRIFFON: Okay, if it’s done then you can just report back and say it’s there. Yeah, NIOSH will post thorium in vivo data and associated model is what I put. You have a coworker model with that, right?

MR. HINNEFELD: The coworker model will come out.

MR. MORRIS: That’s almost done. It just hasn’t been approved yet.

MR. GRIFFON: And two, NIOSH will review Oak Ridge audit report regarding findings related to the quality of in vivo data. This was from the comment that Sandy made about the audits that mentioned the concerns over the in vivo data. And I think it’s in the petition, right? So you can find that referenced audit report.

Four-point-three-dash-seven and eight, I don’t have anything on those two.

Arjun, I might have lost that one with

the Gilbert, but anyway, 4.3-9, NIOSH will post revised model which includes the Battelle model for ingestion. So maybe it's the same --

DR. MAKHIJANI: It's all the same thing.

MR. GRIFFON: It is the Battelle model. So we have the Battelle model which, I guess, SC&A needs to look because this is new information for us.

DR. MAKHIJANI: We have been assigned to review that.

MR. GRIFFON: Under another task, yeah.

Four-point-three-dash-ten, NIOSH will attempt to recover raw data, logbooks, Health Physics reports, air samples, survey reports, et cetera, which may be used for a comparison against thorium air sampling datasets. This is the attempt to validate against the raw basically is what this is asking.

DR. MAKHIJANI: Mark, I also have recovery of the logbooks for the individual who took the air samples.

MR. GRIFFON: So this individual cited, I guess, I was kind of including that in that same action.

DR. MAKHIJANI: Since it came up specifically regarding the --

MR. GRIFFON: Can we cite his name here? Wasn't his name in the --

MR. RICH: No, it was blanked out.

MR. GRIFFON: Oh, it was blanked out, okay.

NIOSH will attempt to recover --

DR. BEHLING: We can identify --

MR. RICH: Everybody knows who it is.

MR. CHEW: I-H's logbook, right?

MR. RICH: Or some other logbook associated with --

MR. GRIFFON: Well, that was in my first action was any log, and then specifically his log, right? His logs.

Four-point-four-dash-one, NIOSH intends on using urinalysis data for the coworker model. No further actions. That was a comment there more than an action.

Four-point-four-dash-two, NIOSH will provide coworker model developed from in vivo data and the underlying assumptions for the model. I think that might be duplicative to what I said before.

Four-point-four-dash-three, NIOSH will

review the selection criteria procedures and post to the O drive. This was basically if you can find how these people were selected for the monitoring program, any documentation to support your belief that the highest exposed were monitored.

And the next, 4.4-4, no further action is what I have.

Four-point-four-dash-five, NIOSH will re-evaluate cases which may be affected by, oh, that's just overall statement that --

Four-point-five-dash-one, NIOSH will attempt to recover QA inter comparison studies or internal studies, paren, Herb Parker Report and other reports.

Four-point-five-dash-two, I have nothing on.

Four-point-five-dash-three, NIOSH will examine whether an adjustment is necessary to account for this potential unmonitored dose. That's the beta contamination.

Four-point-five-dash-four, NIOSH will provide the neutron survey data along with the methods used in the survey. That's from your, relevant to your presentation.

And 4.5-5, it says, see action on 4.1-3. That's the three women we mentioned in 4.1-3.

MR. CLAWSON: Mark, I can't remember where we had it. Isn't that Baker Report a 1985 report?

MR. HINNEFELD: Gilbert.

MR. CLAWSON: Gilbert Report.

MR. GRIFFON: I missed that somehow.

MR. HINNEFELD: The Gilbert Report and the Westinghouse Transition Report.

MR. GRIFFON: And the Tiger Team.

MR. HINNEFELD: Yeah, Tiger Teams were later, but, yeah, the same thing with Tiger Teams.

MR. GRIFFON: Where did you have that, Arjun?

DR. MAKHIJANI: I didn't --

MR. HINNEFELD: Oh, you know what? I had that around 4.2-1

MR. GRIFFON: I'm sorry. I'm not finished. I have other general action items. That's where I've got that one.

MR. CHEW: Stu, your recollection of the Gilbert Report came out sort of right at the transition between National Lead and Westinghouse?

MR. HINNEFELD: I want to say it may have come out in ’84. I think it may have come out before the decision to rebid the contract. The contract was rebid and awarded in December of ’85.

DR. MAKHIJANI: There may be an excerpt from that in Hans’ report. It’s dated February, it looks like an evaluation.

DR. BEHLING: I may have to --

MR. GRIFFON: Here’s my other general action item before we lose, you know, people have got to catch planes. I couldn’t fit them into the matrix really, so there are five other general action items.

One, NIOSH will post all interview transcripts conducted in support of this review. Just something that came up earlier.

Two, NIOSH will review the Tiger Team, Gilbert Reports and Westinghouse Transition Report to assure that all findings related to the NLO operation of the Fernald plant did not affect NIOSH’s ability to reconstruct dose parameters and includes reviewing the data integrity.

Three, NIOSH will follow up on whether other groups or agencies did any offsite monitoring at Fernald. And it says, paren, contact John Burn to determine this?

MR. MORRIS: Well, John ran an extensive monitoring program over the last ten years I think, ten years, 15 years maybe.

MR. GRIFFON: Stu said he might have information regarding --

MR. HINNEFELD: He should know if there's another agency monitoring. He should know that.

MR. MORRIS: So the goal is inter-comparisons to other --

MR. HINNEFELD: No, actually, the goal is to find out where there other agencies monitoring in the vicinity, taking some air or whatever in the vicinity. I think John would know about those.

MR. MORRIS: I guess I'm not sure what the goal of that is.

MR. GRIFFON: To what end? I think it was brought up, the petitioner or you brought it up.

MR. CLAWSON: Well, I brought it up because one of the things was is that gives us a good opportunity to somewhat kind of check our air data or whatever for the outside. Granted that they may have been down a ways or whatever, but it just kind of gives us a little better of a check and balance.

MR. GRIFFON: A check on DOE's data to see if it's consistent for the use.

MR. MORRIS: Can we move that to the TBD issues instead of the SEC issues?

MR. HINNEFELD: We have to do them anyway.

MR. MORRIS: We've got to do them anyway, but the timeliness of the SEC petition is what I'm focused on.

MR. GRIFFON: I'm not sure if it's a low, I mean, it might be a lower priority than some of the other ones.

MR. CLAWSON: Well, it's just kind of a check and balance. So I don't see an issue with that unless you do, Hans, or --

DR. BEHLING: We all agree it's not an SEC issue, we can certainly shift it from here to the TBD.

DR. MAKHIJANI: Do we have any other QA documentation on the air sampling independent?

MR. HINNEFELD: I'm pretty sure there could be some produced in later years. Now the air sampling started before I did I believe. I think there were a few boundary station samplers. You're talking about Barmelle* air sampling or are you talking the other air sampling?

DR. MAKHIJANI: (Inaudible).

MR. HINNEFELD: Oh, I don't know. Was that in one of our actions? I don't know.

MR. GRIFFON: No, it wasn't in the actions.

DR. MAKHIJANI: Because this might provide some kind of checks from some periods.

DR. ZIEMER: I have visited so many labs over the years, I (inaudible).

MR. GRIFFON: I think it might be useful at least to keep a high priority to identify if other things were done, not necessarily to then find all that data and start working with it, but at least identify are there other studies at the time. And then come back and report and say, yeah, we found this. What do you want us to do with it?

MR. CHEW: You don't want this analyzed?

MR. GRIFFON: No, don't waste a lot of time with it yet. Just find out what's there and characterize it.

DR. MAKHIJANI: Yeah, I think that's good.

DR. ZIEMER: Wait until later to waste time.

MR. GRIFFON: We have plenty of time to waste now.

Anyway, NIOSH should follow up on committee formed to reconstruct thorium operational history. And this is the basis for one of the sections in the Dolan and Hill report, so when I say follow up, I mean did they have a separate report? What was on that committee? I think that needs to be followed up on and fleshed out a little bit. It seems to be an important piece that we might interested in. I know that we have, we're relying on the thorium air data, but the thorium processes might be very important in terms of what went on at what time and who was there.

MR. MORRIS: Could be, but we've got it fairly really well documented thorium processing stream at this point.

MR. GRIFFON: Well, that was one thing.

This mentioned, this committee --

MR. CLAWSON: Well, I think that's what Hans brought up that --

MR. GRIFFON: -- if it's a dead end, then it's a dead end.

MR. CLAWSON: -- lay it out in different liters, whatever.

DR. MAKHIJANI: If they feel they have complete documentation now, I mean, for me it would be a higher priority to see that documentation rather than try and find what some committee did.

MR. GRIFFON: I agree.

And last is NIOSH should follow up on doses assigned in the beginning years, '83 through '85, of the use of the TLD badge and what data was recorded likely limited to the skin dose correction issue is what I've got in parentheses.

DR. MAKHIJANI: You're so thorough. You've got everything. Everything I had anyway.

DR. WADE: Okay, Mr. Chairman, anything else?

MR. CLAWSON: No, I just want to say I appreciate everybody, their professionalism and it’s been fun.

DR. WADE: Thank you for your service, all of you. Thank you very much.

(Whereupon, the working group meeting concluded at 5:17 p.m.)

CERTIFICATE OF COURT REPORTER

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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 August 8, 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 17th day of October, 2007.

____

STEVEN RAY GREEN, CCR
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