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

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ADVISORY BOARD ON RADIATION AND
WORKER HEALTH

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WORK GROUP ON FERNALD

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THURSDAY
FEBRUARY 9, 2012

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The Work Group convened, in the Brussels Room of the Cincinnati Airport Marriott, 2395 Progress Drive, Hebron, Kentucky, at 9:00 a.m., Bradley Clawson, Chairman, presiding.

PRESENT:

BRADLEY P. CLAWSON, Chairman
JAMES M. MELIUS, Member *
PHILLIP SCHOFIELD, Member
PAUL L. ZIEMER, Member

ALSO PRESENT:

TED KATZ, Designated Federal Official
SANDRA BALDRIDGE
BOB BARTON, SC&A
EVERETT "RAY" BEATTY, SR.
CAROL CAINE *
ALLEN CALLAWAY
MEL CHEW, ORAU *
HARRY CHMELYNSKI, SC&A *
CHRIS ELLISON, NIOSH
SAM GLOVER, NIOSH
KARIN JESSIN, ORAU *
TOM LaBONE, ORAU *
JOYCE LIPZSTEIN, SC&A *
JOHN MAURO, SC&A *
ROBERT MORRIS, ORAU *
GENE POTTER, ORAU *
MICHAEL RAFKY, HHS *
BRYCE RICH, ORAU *
MARK ROLFES, NIOSH
BILLY SMITH, ORAU *
JOHN STIVER, SC&A

*Participating via telephone

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

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

9:00 a.m.

MR. KATZ: Good morning, everyone in the room and on the line.

This is the Advisory Board on Radiation and Worker Health, Fernald Work Group.

We are just getting started, as usual, with roll call. We are speaking about a site, so please speak to conflict of interest when you say your name.

(Roll call.)

MR. KATZ: Okay. Then, let me note a number of things.

One, everyone on the line, please mute your phone except for when you are addressing the group. If you don't have a mute button on your phone, you can press *6. That will mute your phone. And then, if you want to come off of mute, just press * and then 6 again, and that will take you off of mute.

And also for people on the phone, please do not at any point put the call on hold, but hang up and dial back in if you need to leave for a piece of time. That would be great.

A couple of other things to note: there is a long agenda for this meeting, and it is posted on the NIOSH website under the Board section.

Also, there are a good many documents that are being discussed here. This is a very heavy agenda today. Those documents are also posted on the Board website, all but one that was just recently delivered from SC&A and it hasn't been Privacy Act-cleared yet. But everything else, most of the documents being discussed today are PA-cleared, Privacy Act-cleared, and on the website, so you can follow along, if you want, with those documents.

And the last thing I would just note for everyone to keep in mind today, given

we have a lot of documents that we are going over, there has been a lot of good, hard work on all sides done, so please try to be efficient in your comments when you are commenting on your technical material, and so on, because we have a lot to do and it is only one day.

Thank you.

And, Brad, it's your meeting.

CHAIR CLAWSON: Thank you, everybody, for coming.

Like I said before, at Fernald we are kind of starting to wrap this up. We have brought it before the Advisory Board I believe twice now. We are kind of coming to the end.

I would like to tell everybody I appreciate the work that they have put into it. There has been a lot of time.

With that, I am going to turn it over to John Stiver and let him start.

MR. STIVER: Okay. This is John Stiver from SC&A.

We have basically four major issues we are going to go through today. These are things that have been discussed, and we really are getting to a point on most of these where we are closing the loop on a lot of the issues.

The first issue, which is an open SEC issue, really it is not an SEC issue. That SEC issue has been resolved in terms of the uranium bioassay coworker model for Fernald. A data completeness and analysis report has been developed and analyzed in detail, I believe over the course of over a year ago.

What remained was really an analysis of whether subcontractors' construction workers were adequately represented in that uranium bioassay coworker model. NIOSH was supposed to provide that type of analysis, which they, indeed, did.

We have reviewed that. We are prepared to discuss that today.

Mark, since you have presented your last paper and we are responding to that, if you could kind of give us the broad brush stroke view of where you guys stand? And then, we can respond.

MEMBER ZIEMER: Just a quick question procedurally. We have so many papers with this meeting. I think it would be helpful if each of you, when you discuss, like Mark now, identify which of the papers it is. Otherwise, I am shuffling through them like mad, and, likewise, on the responses.

MR. STIVER: Absolutely. Okay.

MEMBER ZIEMER: Which document are we using here?

MR. BARTON: I believe the title is "NIOSH Evaluation of Fernald's Subcontractor Bioassay Data".

MEMBER ZIEMER: Thank you.

MR. ROLFES: That is correct, Bob. Thank you. That is correct. This was from October 7th, 2011.

Basically, what was done, we had gone through the HIS-20 database and found that, prior to 1985, subcontractor bioassay data was not included in the electronic HIS-20 database.

So, we had initially believed that several subcontractors were not monitored for uranium in urine during their employment or following their employment at Fernald. What we did to determine whether or not these individuals were monitored is went back to hard-copy records stored, I believe it was in Morgantown, West Virginia, under DOE's Legacy Management.

We had requested all records which might have uranalysis data in them for subcontractors. We went through several -- correct me if I'm wrong, Gene -- we went through several thousand pages of reference material to determine if there were additional bioassay data for subcontractors.

We found quite an extensive amount

of subcontractor bioassay data. So, what we had done, we had compared the subcontractor uranium urinalysis results, excretion results, to the HIS-20 data and the coworker intake model that we had developed. We did find that there were some higher concentrations of uranium in urine in some of the subcontractors' hard-copy records.

So, we went back and developed correction factors to adjust intakes based upon the differences between the hard-copy records and our coworker intake based upon HIS-20.

Now although the uranalyses for some subcontractors could have been higher, it appears that, based upon the sample type of these urine samples, most of these samples were labeled as they were spot-samples, basically, or special samples. They had a code of Code 50 or Code 59.

It turns out most of these samples were likely taken during the day or following

a short-duration exposure at the end of a job. And so, we believed that some of these results were higher because of the time period of the sampling. When you compare a spot-sample to an annual sample or a Monday morning uranium urinalysis sample, you are typically going to find a higher result of excretion.

That could be because the person had just been exposed to uranium and just stepped off the job. It could also be because of sample contamination, for example.

So, although the uranium urinalysis excretion could be higher, that doesn't necessarily equate to a higher exposure because you have to consider the duration of that exposure.

Now, based upon information that we have for a limited number of subcontractors, it appears that you would have to look at the case details for a specific individual to determine what their actual dose would be and whether or not their internal

dose would be higher or lower than the internal dose for another Fernald employee that wasn't a subcontractor. Just because the uranium urinalysis result is higher doesn't necessarily mean that their internal dose was higher.

So, that is the summarization of our work on this topic.

MR. STIVER: Okay. thank you, Mark.

This is John Stiver.

We had reviewed the NIOSH response, and we put together our report based on our analysis of the available data. This report is entitled, "SC&A Review of NIOSH Evaluation of Fernald Subcontractor Bioassay Data, Revision 1," by Bob Barton and Harry Chmelynski.

I am just going to give the 10,000-foot view of what we did. Bob Barton, who is involved in the detailed analysis, is going to provide a more in-depth review.

But, basically, what we did is we looked at the data in a couple of different ways. We did a side-by-side comparison as opposed to looking at the pooled data. What we looked at was just the raw data itself. There are approximately 10,000 of these Type 50 urinalysis samples, and combined with about 107,000 overall. So, you end up with about 117,000. So, you are looking at about 10 percent of the overall databases due to this Type 50 data.

We saw that, even at 10 percent, the effect is really quite remarkable, being anywhere, when they are pooled, from about 1.2 up to 1.6, depending on whether you look at annual or quarterly data. And so, we looked at this side-by-side comparison both in terms of raw data and, also, looked at the log-normal transforms of the data and did comparisons by year for select years, and tried to determine what is the real difference when you look at the data as a direct

comparison, as opposed to this pooled system.

We also had some questions regarding some of the assertions and assumptions that went into the NIOSH report, primarily because a lot of the analysis was not presented. This is part of the summary paper. Typically, NIOSH would provide us with statistical analysis, tables, and a description of what was done. In this case, we just had kind of a summary graph which showed those ratios over time. So, we had some outstanding questions regarding those comparisons.

Bob, if you would like to go ahead and fill in some of the details here?

MR. BARTON: Sure. Thanks, John.

This is Bob Barton with SC&A.

I guess, as John pointed out, our major concern here was a sort of lack of quantitative information, because, clearly, there was a lot of work that went into this, diving into hard-copy records. I mean, it is

not easy to look at these handwritten things and try to form some conclusions about them.

It really comes down to the decision of whether you are going to pool the data. And when we say that, basically, taking these contractor records, putting them with everybody else, and then comparing them with the original model, which was what was done originally. The other option is you could just look at the contractor records as a separate worker population and then compare it.

Now the decision was made by NIOSH to do the pooled system. There was a couple of rationale given. But, like I said, we were a little concerned because we couldn't really see the underlying quantitative logic behind making that decision.

Here is just one example, and this is in NIOSH's paper that was just discussed. It is on page 4. It says, "In the majority of cases evaluated the work occurred over a few

weeks or a few months with a series of samples requested. In the data captured, the subcontractor results appear to represent a series of acute intakes over periods of a few weeks or a few months. The coworker study was developed by assuming multiple chronic intake periods. Thus, it is very likely that the data presented in the coworker study would bound to the doses to unmonitored subcontractors."

Basically, what that is saying is, listen, I mean, these guys were only doing this job for a short period of time in the year. If we give them the full year's worth of coworker doses, that will bound the exposure.

It is a sound rationale, but we didn't see the numbers, as to how many cases were actually evaluated and, honestly, how you could tell that it was just a short-duration job. Later in that same report, it says, "The actual length of subcontractor employment was

not available to assess the potential missed and unmonitored intake period."

So, we actually don't know how long these workers were actually doing their contractor work. So, that is the kind of thing. I mean, maybe that kind of information was gleaned from the hard-copy records, but it is not in the report. So, we really don't have a basis to determine whether that is a really sound assumption for choosing pooling of the data versus the side-by-side comparison.

One of the other rationales was that contractor samples might have been contaminated. On page 5 of the NIOSH report, it says, "There were a number of cases where the sample taken at the end of a shift was a factor of two greater than the one taken the following morning," which would indicate a possible sample contamination.

Once again, we didn't see how many cases were evaluated that showed this type of

behavior, how many cases didn't show that type of behavior. Later in the report it says, "There were far more subcontractor samples designated 50 or start-of-shift sample than were 59, end-of-shift sample."

So, again, it is like, well, how many are we really looking here that were contaminated? We kind of build this weight-of-evidence argument to say, listen, it doesn't really make sense to do the side-by-side comparison because of these reasons. And the reasons are given, but we don't see any of the underlying work that went into it that would kind of quantitatively back that up.

And the reason we are concerned about the pooling versus the side-by-side comparison is you actually see quite different ratios develop when you do the side-by-side comparison, which you would imagine, if you are pooling the data together, you are kind of muddying the water a little bit. So, the comparison isn't going to show as great a

ratio as if you actually took the 50 series and compared it to the regular coworker.

So, what we did is we went into the HIS-20 database and we pulled out all the 50 series records. And then, we pulled out all the records that were originally used in the coworker model, excluding certain records like the first-day-of-employment sample or any sample that wasn't really used originally because it is not reflective of your normal, unmonitored exposure.

So, we did that and we actually, instead of pooling the data, like what was done, we did the side-by-side comparison. Here are the records for Type 50; here are the records for the rest of the coworkers.

We did a basic data analysis with the raw data. Depending on what basis you want to look at it, whether it is the arithmetic mean of the two groups, the geometric mean, or median, we find out that, if you are looking at the average,

essentially, in 50 percent of the time, 50 percent of the years, your ratio is going to be higher than that suggested value of two, which was in the NIOSH paper. Looking at the geometric mean, you are looking at 42 percent of the time above that value of two, and in the median it is 38 percent of the time.

So, when you do the side-by-side comparison, there is clearly a difference compared to the Type 50 records that appear to be significantly higher than the rest of the coworker models.

For those following along, I am looking at table 2 on page 8 of the SC&A paper. It actually shows sort of the raw data analysis. It has the values and the ratios in there, and they are also plotted in figures 1 through 3, which show each year what the ratio was. And then, it has the line there of two. So, you can see how it fluctuates.

And if you look at table 2, you can see that the ratio of the Type 50 records

to the original coworker model could actually be as high as seven, if you are looking at the average results, a little less than five for the geometric mean, a little over six for the median. So, I mean, these are significantly higher values than the proposed number of two.

So, this is why we are certainly concerned, not being able to see the work that went into it and all those quantitative rationales for choosing pooling the data versus actually doing the side-by-side comparison.

It gets even crazier if you look at it on a quarterly basis. Obviously, you are going to have a smaller dataset for the Type 50s. So, those ratios can get very high. When you only have a few Type 50 records that are significantly higher than the coworker model, those ratios can get even higher than that.

Of course, this was just looking at the raw data. I mean, we don't construct

coworker models based on raw data. We actually fit it to a log-normal distribution and calculate the appropriate parameters for assigning doses.

So, what SC&A did is we selected four years, just to kind of perform that scoping. We are going to do the log-normal transformation and see what it would like. We were just going to do a coworker model. We chose 1959, 1963, 1967, and 1972.

If Harry Chmelynski is on the phone, I would like to turn it over to you to explain what we did here statistically.

DR. CHMELYNSKI: Hi. This is Harry Chmelynski from SC&A.

Basically, Bob told you what we did. We had the data for the Type 50s and we had them in a separate pile from the data that was originally used in the coworker model.

These records, by the way, were not included in the original coworker model because, on the surface, they were called

special type records. And these were eliminated because they would not be representative.

NIOSH, on later inspection, found that a lot of these Type 50s were, in fact, contractor records. Now we don't claim that they are all contractor records, but they do, as a pool, serve as a good surrogate for the collection of contractor workers.

We fit four years to the log-normal distributions. In every case, the distributions for the Type 50 records lie substantially above -- well, in some cases more substantially than others -- above the typical coworker model records, which had been included in the original study.

From our purpose, this really brings up the question as to whether the coworker model is designed to cover the construction workers. That is really the issue here. We can give them a factor of two, and that gives everybody an extra dose to

account for perhaps these Type 50 records that should have been in the model. However, that still may not be sufficient to account for the differences we see for the contractors.

I think that is about it.

MR. BARTON: Thanks, Harry.

The one thing that I neglected to mention, and it is another piece of information that will be very useful in evaluating this is we know that Type 50 didn't mean you were absolutely a contractor. They could be other people at the site. They were called special, so maybe they were involved in some special operation. But, again, that information wasn't in there as to how many were actually seen in the hard-copy records of these Type 50s that were contractors versus site personnel.

It would also be helpful to know, I mean, in the NIOSH report there is table 1, which shows all the references that were captured and approximate number of records

that were in each document. Except for the first seven or so, it just gives the number of results, and we really don't have an idea of how many of those were actual contractors versus site personnel.

For example, the eighth reference down covers 1969 all the way to 1984, and it is 5,000 results. Well, I mean, how many of those were contracts. It is important to know how many per year do we have that we can look at and say these are contractors, because that could have a profound effect on it. I mean, you might have these really high values because you are looking at people who were involved in special projects on the site. Or these could really represent what the contractor intakes were.

So, again, what our concerns pretty much boil down to is we would really like to see the quantitative work and logic that went behind all these decisions and rationale. They are in here, but they are

kind of more anecdotal and it is hard to make a judgment on them without seeing what was actually done.

MR. ROLFES: Okay. There is a couple of points that I want to consider, and then I will ask Gene Potter to maybe provide some additional details here.

Yes, in the initial couple of years back, when we started looking into this issue, we were under the assumption that there were a lot more unmonitored subcontractors, because we didn't have electronic bioassay data for them.

Now, going back into the hard-copy records and looking for their specific urinalysis results, we determined that there were actually a lot more subcontractors that were monitored rather than not monitored. When all is said and done, there is probably a handful of claimants that we have, approximately 10 or 12 I think, that were actually and truly unmonitored for uranium.

In looking at some of their work history, I don't recall if any of those individuals had employment more than maybe a year or two in continuous duration. So, a lot of these employees might have come onsite to make a delivery or something or do a short job for a couple of weeks at a time possibly, and might have some intermittent exposure potential.

In order to get some understanding of what that person could have been exposed to on the site, they had to sample that employee, that subcontractor employee, while they were onsite, before they left, and Fernald didn't see them again possibly.

So, you have to take a look at the individual details within each individual claimant's case in order to determine what their true or more realistic internal dose would be.

There was one other point that I wanted to make, but I have forgotten what it

was.

Gene, could you possibly go through some additional details?

MR. POTTER: Yes, this is Gene Potter again.

I would like to refer the Working Group to the paper dated February 6th, 2012, "NIOSH Comments on the SC&A Review".

MR. ROLFES: Okay. This is something I just sent out. Unfortunately, I wasn't able to get it out earlier. I just sent it to everybody's email this morning and put it on the K: drive. It is approximately three pages, four pages long.

Gene, if you can just possibly go through that? I know some people might not be able to pull it up.

MR. POTTER: Sure. Anyway, I guess the first point to make is that NIOSH's discovery of the lack of subcontractor results in HIS-20 was the reason that this issue surfaced in the first place.

What SC&A did was actually look at the data in HIS-20 and that did not include all of the data that we had entered from hard copy for subcontractors. But they did look at the Code 50s.

Now the subcontractors were in the HIS-20 database after 1985, I believe. So, any analysis for those years would include subcontractors as Code 50s.

As time went on after 1985, the subcontractors were more heavily sampled. They looked more like the general site population. So, it is really those earlier years that present this issue.

The Code 50 samples that SC&A did look at are similar to subcontractors in that they were special samples. I believe, from what I have read, these would be samples that were collected shortly after a potential intake. In other words, the site was interested in, if they changed a procedure or doing a new evolution of some type, they

wanted to sample that evolution or procedure change to see what the effect might have been on the intakes that people would have.

And so, what they have in common with subcontractors is that samples were taken relatively close to the potential intake. In that paper, if you are able to access it, I have plotted at the end a graph of just uranium urine excretion with time after an acute intake. You can see that, just within a period of like five days, the difference between sampling at day one and day five, the urine excretion drops by like an order of magnitude.

So, what we would really be interested in is the distribution of individual intakes for workers rather than just the distribution of bioassay results, because you can see that a bioassay result does not mean the same thing for two different intake times.

So, we actually investigated to

see if we could do a distribution like this for the subcontractors. We found that there wasn't enough information on their exact employment periods, but you could see from the periods when they were sampled that it looks like, in general, the constructions types, in particular, came onsite, worked for a few weeks, a month or so, and they may have gone away and come back and worked again. But this was an intermittent exposure in general. Of course, you can probably find exceptions.

So, anyway, while SC&A didn't analyze the subcontractor data that we had, we don't dispute that this comparison, direct comparison between Code 50s and subcontractors to the remainder of the results, would produce a result similar to what SC&A has presented.

I don't think that Bob mentioned that they came up with a factor of five to eight, or something in that range, if you look at the direct comparison.

We originally proposed a factor of

two as a claimant-favorable approach due to the fact that there is uncertainty in what the exact exposure periods would be, and it would be very difficult to quantitate this on an intake basis. If you look at the bioassay results of the two groups side-by-side, you see what SC&A has presented.

Another point is that, now that we have entered all this hard copy, have captured the records and entered the hard copy, we have the data for a bunch of the subcontractors. And so, we only have a need to use a coworker factor of two for unmonitored subcontractors. These are the folks that you are probably going to find were the delivery people, the people that were there for very short duration, and that sort of thing. Because it looks like from the hard-copy data, the people doing the heavy-duty rad work, we were able to find data on them.

Looking at some of SC&A's specific comments, we did present only minimal details

of our analysis. There was a spreadsheet, but I guess SC&A never received it.

SC&A's analysis did present a range of factors that they saw. It would probably be very similar if they had analyzed the subcontractor data. So, it appears that between the factor of two and what they have presented, there is some sort of technical agreement that could be reached on this in this area, and NIOSH could possibly address this by a modification to the TBD or the coworker study. One possibility would be to just include all of the results in the coworker study, the hard copy, and the Code 50s, which would increase the intakes assigned to all workers, not just the subcontractors.

SC&A made a specific comment about they didn't have any Type 59s. That is another indication that they did not look at the hard copy. That was the samples taken at the end of the shifts, and they could not have seen the contamination, and so forth, effect

by looking at the 59 taken the previous day to the 50 taken the following day.

I guess that, again, we don't dispute that, if SC&A had looked at the actual subcontractor data, chances are before `85 they looked at very few, if any, subcontractors. Mostly, the Type 50s for those years were site employees, and this could be verified by looking at the hard copy. There is no way of verifying it by just looking at HIS-20.

So, again, it looks like some agreement could be reached on a path forward on this issue. We have proposed a factor of two; SC&A has a higher number for the direct comparison, but that may not be appropriate for the time course of the urine samples compared to when the intakes occurred.

It looks like this is an issue which could be moved to the TBD/coworker arena, rather than the SEC.

Anything else I can present, Mark,

that I have missed?

MR. ROLFES: No, thank you, Gene. I appreciate your summary response.

MR. BARTON: If I could comment here, Gene, we didn't want to give the impression that we were throwing out numbers that we believed to be more appropriate for use. The reason we did the side-by-side comparison is to show that, without the information to justify doing the pooled data, there are some concerns there.

Now rationale is given for why the pooled data might be more appropriate, but, again, quantitatively, we do not go and compile from the hard-copy record. Again, you are correct, we only looked at the HIS-20 from that 1960-to-1985 period. We didn't do any analysis past 1985 where it was apparent that the contractors were actually being recorded in the HIS-20 database.

So, I just wanted to make that clear. We are not proposing a number here.

We are just stating our concern of why we would like more information on the subject.

For instance, you said that it appeared that most people were only working a few weeks to a few months, but, then, it also says in the paper you don't have sufficient information on employment period. So, I mean, it is those kinds of things where we were like, well, can we see what was actually done? You said there is a spreadsheet. That is something that would be very helpful in trying to sort this thing out.

Again, we looked at this paper and we saw what you did of the end results, but we didn't see all the steps in between to get there.

MR. STIVER: This is John Stiver.

I would like to reiterate what Bob said, but also I agree with Gene that this is certainly a technical problem that is solvable. I believe it is a TBD issue at this point.

I have a few problems. I wouldn't feel comfortable in buying off on things as they stand now without actually seeing the rest of the data. And this spreadsheet would be very helpful for us.

I also have some questions about post-`85 versus pre-`85, where you can see just the number of samples for the number of coworker or construction workers decreases in the later years, as presumably there is less of that activity going on. And so, one has to wonder about the exposure potential in the earlier years, say, compared to the later years and whether it would have been substantially higher.

The issue that really kind of stuck in my mind as being very important is this idea of intermittent exposures versus a chronic exposure. I fully understand that, if we are looking at the Type 50 records, which are spot-samples, certainly, if you get a preponderance of those, you are going to have

much higher urine excretion rates. So, to the extent that we could actually be looking at derived intakes, and maybe the assumptions that went into those intakes based on those records, and also it kind of troubles me that so many of these Type 50 records are really not necessarily for contractors, but a large proportion of them may be for onsite workers. There are all these uncertainties there that are kind of hard to unravel at this point.

So, I think at this point SC&A would be more comfortable if we could actually look at this spreadsheet and some of the data, and maybe some of the assumptions and bases for the determination that these samples really represent short-term intakes without any kind of corroborating evidence of employment period.

MR. ROLFES: John, this is Mark.

We can definitely get the spreadsheet to you. But I would also suggest taking a look at some of the hard-copy data

because --

MR. STIVER: Yes, absolutely.

MR. ROLFES: -- you would have to take a look through those special samples, the hard-copy data, because there are handwritten notes for identifying which person was a subcontractor. It will list usually in a handwritten or a typed line, you know, this individual worked for such-and-such company.

We had actually also looked back at some of the historical contracts. It appears in the earlier years there weren't many subcontractors employed by Fernald. It appears that -- now correct me if I am wrong, Gene -- from what I recall, it appears that most of the work that was done by subcontractors in the later years was actually done by type, NLO employees in the earlier years. So, we don't have the same issues. There weren't as many subcontracts in those earlier years.

And then, also, if you take a look

at the production history and changes at the Fernald site, and the need for uranium, there were periods when they thought that they were going to shut the Fernald site down. So, some of the work that was done by the subcontractors in the later years to maintain the facilities and build new buildings wasn't being done perhaps during the 1970s because of the lowering of the production rate and possibility of shutting the site down.

So, Gene, did I misstate anything there?

MR. POTTER: No, you are correct, we did look at the distribution, tried to look at the distribution of contracts, and guess which ones were construction subcontractors from the names of the companies, and so forth.

The other point, to go back a little bit earlier to what John was saying there, what you see when you look at the hard copy is that a company will come in, and half a dozen to a dozen folks will be sampled on a

daily or every-other-day basis for some period of time, and they will go away.

What I looked at trying to do was do individual employees with bioassay data and determine what their intakes were. But when you look at an individual, you cannot say for sure exactly what day he started work. You can only say when he was bioassayed, and probably get a pretty good idea of the last day because, if it is at-the-end-of-a-shift sample, then he has no more samples. But you don't know if he came in the day before or two days before, or what. That is the type of uncertainty we are talking about on sort of an individual basis.

MR. ROLFES: Correct, Gene.

This is Mark again.

We don't have that information for people who are not claimants. We do have that information for claimants. That is the uncertainty in trying to develop a model for individuals who are not claimants. We are

able to do this for individuals where we have the employment data.

MEMBER SCHOFIELD: May I just say something?

You mentioned earlier that there are 12 unmonitored workers. These would be 12 claimants?

MR. ROLFES: Claimants, correct.

MEMBER SCHOFIELD: And these hard-copy records, they are available for us to review?

MR. ROLFES: Yes, they are in our report from October. On the third page, we have listed the --

MEMBER SCHOFIELD: Okay. All right. Okay.

MR. ROLFES: -- database reference number.

MR. BARTON: This is Bob Barton.

We were talking about the spreadsheet. Presumably, that is a compilation of the hard-copy data, right?

MR. ROLFES: Yes. It has been a while since I looked back at that.

Gene?

MR. POTTER: Yes?

MR. ROLFES: I think you had identified the urinalysis results and which subcontractor these employees had worked for in your spreadsheet? Was that correct?

MR. POTTER: Right. Yes, that is generally how you could identify them in the hard-copy records, were by the subcontractor name written on the card. In some cases, the names may not have been written on the card, but you recognize the person from working on the contract and he was sampled a couple of days before, and he did have that subcontractor. So, there were a few that I could add that way.

And so, I will have to go back and look. I think it should be obvious, when you look at the spreadsheet, who the Code 50 site employees were and who the subcontractors

were.

I just used Excel to do the log-normal fits and stuff. So, some of them may be sorted. So, I will see what I can -- I may have to make a few changes to the spreadsheet, so it will be more obvious. I knew what was going on, but when another person looks at it and didn't generate it, it may be more difficult to understand all that. Maybe I should look at that.

MR. BARTON: Gene, just one more question. When you were giving your response, so we only took the contractor records out of the hard copy, and those were the ones that were added to the coworker model, except for years -- and it says here -- for years where there weren't any contractor records, we added some Type 50s.

But, other than that, if you had contractor records in the hard copy, those were the only ones that were added to the coworker model?

MR. POTTER: No. I looked at adding, as you did, all the Code 50s and the subcontractors. So, even on the pooled data, as you noted, that can make quite a difference.

MR. BARTON: Thank you, Gene.

CHAIR CLAWSON: This is Brad. I have got a question for Mark or you, Gene.

I am having a hard time understanding this 50 series because, on the one hand, you are telling me that you have got a construction worker bioassay, but, then, a majority of them are in, they are classified as a 50. I am having a hard time following here what --

MR. ROLFES: This is Mark.

The Type 50 sample was just a sample designation. It stood for a special sample, and those special samples were collected from both NLO employees as well as subcontractor employees. It just stood for like a spot-sample, like a sample in the

middle of the day where they would go out to just see what these people are exposed to at that moment.

And some of those samples might have been collected in radiological areas. So, it is possible that they could have had some sample contamination or they could have just had an exposure to uranium.

So, those Type 50 samples could potentially be elevated due to a more recent uranium exposure, sample contamination. So, comparing something like that to a sample that is collected, you know, a few days after an exposure, you are likely going to get a higher result.

Now the internal dose isn't necessarily higher because you don't know the entire duration of intake. And some people could have had an intake that was two weeks long; some people could have been chronically exposed the entire year. So, that is where the uncertainty is coming in.

CHAIR CLAWSON: Well, my one question, this is urinalysis, right?

MR. ROLFES: Correct.

CHAIR CLAWSON: You are not going to walk out to the floor and ask a guy to provide this much. I mean, when he would take a sample, it takes a little while to process enough for you to be able to do that.

So, I don't understand this spot-sample because it usually takes a day or so to work up enough for them to be able to have that.

MR. ROLFES: These are spot-samples, not 1500-milliliter daily. You know, these are like 100-millimeter samples. You get 100 millimeters of pee and that is what they test, rather than a full 24-hour excretion period of 1400 or 1500 milliliters.

MEMBER ZIEMER: But, then, they normalize it.

MR. ROLFES: Correct, they normalize it.

MEMBER ZIEMER: But could I interrupt there?

CHAIR CLAWSON: Sure.

MEMBER ZIEMER: Could you clarify? It appears that you are saying that, to some extent, these Type 50s were event-driven rather than random?

MR. ROLFES: That's possible.

MEMBER ZIEMER: Or both?

MR. ROLFES: That is possible. I think Gene could speak to that a little bit better.

MR. POTTER: Yes, I can try to respond. This is Gene Potter again.

There was a separate sample type for incidents. So, what I believe the bulk of these special samples that SC&A looked at were samples that were taken when a procedure was changed, when they were doing a new evolution of some sort, and that sort of thing, where they were wanting to check on a group's exposure immediately after some change or

there was some reason to collect a sample that was out of sequence from being a normal or a routine sample, and that was not considered an incident.

MEMBER ZIEMER: Okay, but it was event-driven in terms of, as you say, a new procedure or something like that. And you want to get an early indicator if there is going to be a problem with intakes. Is that what you are saying?

MR. POTTER: Yes, I believe that is so.

MEMBER ZIEMER: Yes, I've got you.

CHAIR CLAWSON: And how do we know that? Because I am looking at a lot of these different samples, and we have got everything from construction workers to house ones. What is your basis for saying that this was part of a new process or this is why we were using these as a special sample?

Because, if it was a new process going on, to me, that would have been built

into the process going into it. I am just wondering, what do you have that tells you, oh, yeah, this is why they have these 50 samples?

MR. POTTER: This is Gene Potter again.

I have reviewed, of course, thousands of documents in the course of this. I have read this somewhere, and I cannot give you a reference for it at this time. The title of this particular type of sample is a special sample. And I know I have read something that indicates what I just mentioned to Dr. Ziemer, that this was a sample taken when a procedure was changed or that sort of thing. But I don't know of a specific reference right now. We could work on getting you a reference for that.

MR. ROLFES: Brad, I can take a look during the meeting and see if I can pull that up.

If you take a look in some of the

DOE response files that Fernald sends to us for each claimant, there are sample codes included with some of the urinalyses. For example, in the earlier years you might see a Code 2 or 3. That meant Plant 2/3. There is Code 49 and 50, 5. There's probably about 20-something different codes. Some of them are defined and some of them are not, but there are different references. They changed a little bit over history, but there are some references that explain what those codes are.

As Gene said, we have seen that, and I will see if I can get that for you here sometime before the day is over.

MR. BARTON: This is Bob Barton.

If you are interested in all the different HIS-20 codes throughout the years, if you look at page 19 of SC&A's report, we pretty much break down what the codes mean, how many of them you will see in HIS-20, the first year of use, the last year of use. There is lots of information on that.

One other question I had, and I address this to Gene, do we have a rough idea of how many of these Code 50s were contractors versus -- I mean, obviously, this is going to vary year-by-year, but your general sense of how many of them actually are contractors versus site personnel?

MR. POTTER: Well, the amounts vary quite a bit year-to-year. I am not looking at that, unfortunately, right at the moment. Maybe we could go on and I could pull those numbers for you.

MS. BALDRIDGE: This is Sandra. I have a question for Mark.

Would you explain what you mean when you said that you had only checked for the claimants, the contractors who had already filed claims? Where does that put the non-claimants in the SEC if their data or information hasn't been factored in or isn't being considered in the SEC process?

MR. ROLFES: Well, one would first

have to look as to whether or not they were monitored. As far as an SEC, I mean, that would all depend on ultimately what was recommended, if an SEC Class was to be recommended for something. That would all depend on who was included in the Class. That is not something really that we are discussing today or something that I could answer for you.

At this time, NIOSH is not recommending an SEC for any Class of workers for the Fernald site. So, I couldn't really answer any better as to what would be done for construction workers who are not claimants.

CHAIR CLAWSON: But let me clarify something. If I understand how -- this is Brad again -- how Sandra is thinking, we have taken all of the data that Fernald has and it is put into the spreadsheet. Any data that we have is in there.

MR. ROLFES: Yes, correct.

CHAIR CLAWSON: Okay.

MR. ROLFES: Yes.

CHAIR CLAWSON: So, what this is for is, if we have a non-monitored person, this would be the coworker data, that they would be used to be able to do it.

MR. ROLFES: That is correct.

CHAIR CLAWSON: Does that help you, Sandra?

MS. BALDRIDGE: Right.

MR. ROLFES: Yes, we had used non-claimant data in our analysis to build a coworker intake and a coworker adjustment factor for subcontractors who were not monitored. So, we have considered non-claimant data.

However, getting into the details of what a non-claimant's actual exposure was, there is uncertainty associated with that because we didn't go and get their employment information details that we would have for a claimant. So, that is where it comes down.

CHAIR CLAWSON: I have got another

question here. Are we able to tell by the bioassay who is the subcontractor and who is a Fernald employee? Is it separated out that well? Because my understanding was that we really couldn't tell.

MR. ROLFES: In the hard-copy records you can. There are notes on the bioassay request cards indicating that this individual worked for Legge, like L-E-G-G-E was one of the subcontractors; another, a painting company like Stegeman Painters. Those notes are made on each of the hard-copy records that we reviewed.

The records that we reviewed are on page 3 of our October 7th, 2011 report. Where we have reported, this is page 3 of 7 from the NIOSH Evaluation of Fernald Subcontractor Bioassay Data".

CHAIR CLAWSON: Okay. So, back to my question, I guess it would be we are able to separate subcontractors out from the house?

MR. ROLFES: Yes, correct. We can

identify who was an unmonitored subcontractor versus who was an NLO employee, for example, or Westinghouse employees.

CHAIR CLAWSON: Okay. You made a statement earlier, too, about these Type 50 samples were used for subcontractors who were there for a short period of time. I guess my question that I have is that, in having the meetings and stuff here, we have had -- a lot of the subcontractors were out there like for 25 years. The only thing was, the name of the contractor just changed. We have numerous ones telling us that in a 25-year period they may have given four or five bioassays.

This is why, when you are telling me they have got a subcontractor in there for a short period of time and he has given five or six samples, daily, or whatever, I am wondering what the difference is. Because like Rust and all these that were out there, these people employed people for numerous years out there.

MR. ROLFES: Yes, I have heard the same concern. And I have spoken with some individuals in the past who have believed that they have not been routinely bioassayed. I have encouraged those individuals to submit FOIA requests, either from NIOSH or from DOE. It would be directed ultimately to DOE because it is DOE's data.

But I have some spoken with some individuals in the past about these concerns. From everything I have seen, it has turned out that those individuals did have monitoring data, and some individuals were surprised about how much monitoring data they actually did have. It was typically more than they had believed they had.

CHAIR CLAWSON: Well, this has been the thing with Fernald, and especially with the coworker, well, with the subcontractors, is numerous ones of them have been out there for years.

Now let me ask you the question.

If they are a claimant and they are a subcontractor, their data would be available to them, wouldn't it? Where you have done a dose reconstruction for them, that bioassay information would be available for them?

MR. ROLFES: That is correct. If they would submit a FOIA request to us for that data -- we might not discuss each individual bioassay sample in detail in the dose reconstruction, but that data is available to an individual, if they request it via the Freedom of Information Act.

CHAIR CLAWSON: Okay.

DR. MAURO: This is John Mauro. Can I ask a question and maybe even make a suggestion?

MR. KATZ: Sure, John.

DR. MAURO: What I am hearing is, the way SC&A approached this evaluation was to use these Type 50 data, which I am hearing now is really the Type 50 data may not be a good representation of the data for contractors,

and it may really be a sampling that is biased in an unusual way. As a result, we are seeing means of the bioassay data that are about eight times higher than the mean that is in a given set of coworker data.

The first question I have is, if you go to the coworker model as it currently is, and I believe you could get a given year, and you have lots and lots of bioassay data, you get a mean and a standard deviation on that.

What I heard Gene say is that, if you look at the bioassay data in the hard-copy records for the construction workers or contractors that were onsite, you actually have data. Have you plotted that for, let's say, a given year and compared it, the mean and the standard deviation, for that group, too?

Because I think, originally, this idea was to do that, the distribution for that year in your current coworker model, and has

that been done? It may very well have been in your report, but I didn't see it. If it has, do the means of the two different groups differ by a factor of two, three, four, eight, or are they the same?

MR. ROLFES: This is Mark.

John, yes, we have done that analysis. I can let Gene maybe elaborate on that a little bit further.

That was how we had derived -- we had actually compared the effect of adding in subcontractor data, and that is how we determined that the highest given year, the subcontractor data, the excretion rates were about a factor of 1.6 higher for the highest year that we had analyzed.

DR. MAURO: Yes, Mark, I understand what you are saying, but that would sort of blend in. I am interested in saying, listen, here is a group of a thousand construction workers for 1962 where we have bioassay data and make a distribution by

themselves, and here's the coworker model for 1962, and here is what we would use as the full distribution for the coworker model, the mean and standard deviation for intakes or for whatever the bioassay results are, and here is what we actually are seeing in this group of a thousand workers in 1962 that we know are construction workers.

I would like to know what the difference in the mean between those two are, not after you blended them in. Because if you blend them in, they could disappear. You may be blending in a small number into a very large number. And the small number that is a unique population that has its own distribution could be substantially different than this greatly aggregated group.

So, do you have the number for the separated distribution? And is there a large difference between the two? If not, that is really what we need. And if we don't have it, it sounds like it is available by going into

the hard-copy data and doing one of those.

MR. ROLFES: I am going to defer to Gene. I believe we had started an analysis similar to this.

Gene, is that correct? We did go back, I believe, and compare Type 50 subcontractor urinalysis results to the NLO employee Type 50 urinalysis results, is that correct?

MR. POTTER: Yes. Again, if you look at -- I think what I have at my fingertips here, anyway, is just all Type 50s. So, this includes your specials of site employees that SC&A did in their analysis, as well as our hard copy entered subcontractors.

As I said, if I am understanding John's question correctly, we see results very similar to what you have with the Type 50s alone that SC&A did. I am seeing a ratio of the geometric means for like 1970 is the maximum of 6.69, but that does include the specials and the subs, which I think is

logical to include the specials because they were site employees. If one were to modify the coworker study, it would make sense that, since these are site employees, there would be really no reason to exclude them.

MR. STIVER: Gene, this is John Stiver. I have got a quick question.

It seems like we are trying to address this secondary confounding factor here of the Type 50 really being these spot-type samples. So, even if you look at those and compare the contractors versus the NLO employees, you are still not really looking at a true representation of what an intake may have been because of the fact of the type of sample we are looking at, unless there is some kind of adjustment made for that.

It seems like there is also a set of data for the contractors which would not be in this Type 50. So, we have kind of got this mixing.

DR. MAURO: Yes, John, I'm sorry

to jump in.

I think the Type 50, unfortunately, is leading us down a road that is not helping us answer this question.

MR. STIVER: Yes, I think it is actually confounding the --

DR. MAURO: Yes, and we have got to walk away from that.

MR. POTTER: Yes, well, it is unfortunate that Fernald did things the way they did in naming. They should have had another type for just subcontractors. But they were considered, I guess, to be somewhat similar in the fact that this was not a normal evolution when someone comes in and removes a plumbing line, for instance. This would not have been a routine operation. So, they are similar in that respect.

MR. BARTON: This is Bob Barton.

And it sounds like, from these hard-copy records, that we can tell in the Type 50s which ones were contractors and which

ones were onsite personnel. So, it seems like if we did compile that hard-copy data for just the contractors, we could make that more meaningful comparison to the actual coworker model.

MR. POTTER: Yes, the only thing I would say is that you are going to be dealing with some lower numbers, lower total numbers. Some years you just don't have very many subcontractors in.

And I was still looking for that while trying to listen in here.

DR. MAURO: Yes, what I hear -- this is John again -- the Type 50 is a subset, if it is a special set of samples that may have relatively-short time periods between intake and sampling, what you are doing is -- and then, compare construction workers to all workers or non-construction workers. We are looking in the wrong place. It is almost like an unusual set that is not really going to help us answer the question.

If you could just go to the randomly-selected, a given year, from hard copy, people we know are construction workers, and we would do the same thing, and then we would put a distribution for that, not that they are Type 50, but just this is a random sample from a given year for people we know are construction workers. And just compare them to the same year that you are currently planning to use as your coworker model. If there is really the same distribution, we are done; the coworker model is fine. But if you do see a difference that could be a factor of two of three, well, there is your adjustment factor.

I guess am I asking something to be done that really can't be done? Because it seems to be pretty straightforward.

MR. ROLFES: John, this is Mark Rolfes.

This is something that can be done. However, keep in mind that our

coworker intake model is based upon a chronic routine exposure which we assume intakes are occurring chronically throughout a given year. Comparing that chronic intake to someone who worked a short duration and had one or two potential short-duration or acute intakes, typically, any chronic scenario, any chronic intake scenario is going to bound acute intake.

We encounter this uncertainty when we don't have a construction worker claimant's employment information. So, we don't necessarily know the intake duration. It could have only been a short-term, short-duration, two-week exposure possibly on the site, which would be more related to an acute intake rather than a chronic intake experienced by someone who is doing the same job every day at NLO.

So, that is where we get this uncertainty for people who are not claimants. We don't know their exact intake duration.

So, although they might have a higher bioassay result, a higher uranium excretion result, that doesn't necessarily equate to a higher total intake because --

DR. MAURO: I got you. Okay. That is a good point.

Let's operate on the premise that, in general, construction workers may not have had the same type of exposure scenario in a given year. It may have been over a few months. And therefore, the coworker model really wouldn't apply appropriately to them.

So, then, you go ahead and you pull your sample, and you see you would end up, you are saying, overestimating, if you were to do that. That is, you do expect to see this difference, and not because there is a real difference in intake in a given year. It is because they are only there for a few weeks, and you pull a bioassay sample right at the end of their shift, and it is due to some maybe short-term intake. And a sample is

taken shortly thereafter. It would appear it would end up giving you a biased high estimate. So, I think I understand your dilemma.

But now when you are doing a real worker, if you have enough construction workers where you do have data, well, then, you are building a coworker model for construction workers from that. So, you are almost making a case why you need a separate coworker model for construction workers.

Does that make sense?

MR. POTTER: This is Gene Potter again.

As I mentioned, we actually tried to do that, but to model these as acute intakes, I could take a reasonable guess when the person first showed up onsite for this period of time, and maybe they showed up, you know, they came back a few months later. And I could make a reasonable guess.

But, depending on how conservative

you want to be with those guesses, you can come up with a whole range of answers. That is why we abandoned that.

DR. MAURO: One last suggestion or idea, and then I will step down from this. For those limited number of workers that do not have bioassay data, I realize that over 90 percent of all the workers, I think construction workers and all the workers, have bioassay data, certainly beginning around 1956. I remember the data. So, you have a very complete dataset.

We are really talking about along will come a claimant who you know is a construction worker, worked in a given time period, but he does not have any bioassay data. Historically, what is done on any of these coworker models is to decide, well, for this particular category of worker, are we going to assign the full distribution or the upper 95th percentile. Whether it is a construction worker or not, you always have

that question that you have to deal with.

It sounds to me that one of the simplifying approaches to dealing with this dilemma is for construction workers that come along, when you don't know the duration of exposure that he might have experienced, you don't have bioassay data for him, you want to assign a coworker intake to him, but you know the coworker model, if you used a full distribution, may or may not be appropriate. Why not apply the upper 95th percentile?

MEMBER MELIUS: This is Jim Melius. I would like to comment.

For the record, I am a Board Member. I am not conflicted.

You were doing well, John, until that last statement. But I think that you have to be able to have a coworker -- if you believe that the exposures or intake, whatever you want to call it, for construction workers has a different distribution than that for the production workers, and you have missing data

or unmonitored workers, whatever, I think you have to have a valid coworker model for them.

DR. MAURO: Jim, you are absolutely right.

MEMBER MELIUS: Judging that or showing that their distribution is similar to your other general production workers and they fit in, and so I think there is an obligation to demonstrate that. You may end up where John Mauro just suggested, but I think there has to be some sort of a statistical justification for that and ability to develop a coworker model to be able to evaluate that in some way.

DR. MAURO: Let me say I agree with that completely.

MEMBER MELIUS: Yes.

DR. MAURO: Because you can't just arbitrarily assume the 95th percentile will work for you, unless you have demonstrated that it will work for you.

MEMBER MELIUS: Yes. There may be

different ways of demonstrating that. I don't want to carry it too far.

DR. MAURO: Yes.

MEMBER MELIUS: But some of what I was hearing was, well, these data don't allow that. Then, I think you have got a problem.

MR. ROLFES: Thank you, Dr. Melius. This is Mark Rolfes.

This is something that can be done, we believe, but in order to do this comparison, you would need details of an individual's actual employment and exposure potential. We don't have that information for non-claimants for this comparison. We can use a limited number of claimants to compare. Ultimately, what we would need to compare would be the total intake experienced by subcontractors versus regular employees and compare the difference in total intake.

For example, that short-term subcontractor could have been exposed for two weeks and could have had a higher bioassay

result versus an NLO employee who was chronically exposed the entire year and had a slightly lower bioassay result. You have to compare all the facts.

MEMBER MELIUS: Yes. No, no, I understand that, Mark, and I don't disagree with that. But it seems to me that, and you are saying, well, you don't have their adequate work history records, and so forth, but you are going to have the first construction worker that comes along who wasn't monitored, or whatever, I mean, you have got to apply something there. I don't think you can do it arbitrarily.

Maybe eventually, after you have gotten enough information, then you will have a valid coworker model for them. That is prejudging what you have, and there may be other ways of approaching this. I don't want to jump too far ahead of you. But I think it is a significant issue you have got to address somehow.

MR. ROLFES: This is Mark again.

Just to clarify, we would receive that information, the actual employment history information, for any claimant who applies for compensation with the Department of Labor and requires a dose reconstruction.

MEMBER MELIUS: Yes.

MR. ROLFES: We don't have that information for people who are not claimants. We don't have their actual employment information. So, we don't have information on employment and exposure duration for people who are not claimants.

MEMBER MELIUS: Then, I think you are telling me you are unable to develop a coworker model. I mean, think about that. Think about different approaches. Again, I don't want to jump ahead too far. Many of you are more familiar with what data is available than I am.

But I think there has to be some way of showing that for construction workers

or some of these other subcontractors that either the distribution of their intakes, whatever you want to call it, is similar to the production workers, and, therefore, a general model applies. If you can't show that, then you would have to develop a valid coworker model for those specific groups in order to be able to do dose reconstructions.

DR. GLOVER: So in general -- this is Sam Glover.

MEMBER MELIUS: Yes?

DR. GLOVER: We don't know when the relationship between any -- in a coworker model, all we have is the bioassay data for thousands of people.

MEMBER MELIUS: Yes.

DR. GLOVER: We don't know when an acute intake may have happened in relationship to their bioassay. And so, we are using this overall large mass of samples to evaluate what is the general output from the exposure potentials experienced at Fernald. And so, we

don't know in general what this is.

Now we believe, if you have a short-term worker who maybe his employment and duration will be very closely tied to when that happened, you may bias high the results because you are closer to it, and I figure you are going to drive it high. So, if anything, you are being claimant-favorable, and the intake rate would be higher than what the normal population may be, if those assumptions hold true.

So, you know, you can still do the comparison. It doesn't invalidate all these things, but it is a potential reason why there may be a difference when you evaluate the results.

MEMBER MELIUS: Yes. This is Jim Melius again.

I think you need to work through this and see, but everything I hear, it is a significant problem.

MR. ROLFES: This is Mark again.

I think we can maybe prepare something, once again, to show, for example, a subcontractor who had a higher, say, 20 microgram-per-liter excretion rate following a two-week exposure period. We can compare something along those lines to someone who is chronically exposed for the entire year, but only had perhaps a 10-microgram-per-liter excretion rate. You could compare the total intakes, and you would find that the total intake would be higher for the person who had the chronic intake rate for the entire year. So, you would have to compare the total intake to total intake.

MEMBER MELIUS: Yes.

MR. ROLFES: At this point, we don't have any reason to believe that the subcontractor population is any different than the full NLO work population, just because this work done by subcontractors in the earlier years was actually done by NLO site employees. So, we have no reason to believe

that the employment duties or job duties changed over time between the two populations.

MEMBER MELIUS: Well, but my response to that, Mark, would be that I don't think you have demonstrated that they are the same. I think there needs to be some demonstration of that. Certainly, the SC&A report certainly suggests that there may be differences. I think, at least from my perspective, it behooves you, NIOSH, to address that issue. There may be different ways of addressing it. I don't know.

But I don't think you can just say, well, we have little differences in how they worked and how they were sampled, and so forth, and therefore, they are the same. You certainly haven't convinced me.

MR. ROLFES: Okay. We can do something along those lines, if the Work Group would like for us to do that or the Advisory Board.

CHAIR CLAWSON: This is Brad

again.

We have had a lot of discussions around this. We have been discussing this for numerous Work Groups. What it is basically going to come down to at the end of this, we have got to decide a path forward.

Paul, I know that you want to speak.

But I have got one thing that keeps popping out here. You keep talking as if the subcontractors are always short-term for two weeks there, or whatever. And that is true in a case, but you have got a whole other section of subcontractors that have been out there for years. I don't think that you can classify -- my question is now, so are we going to divide the subcontractors into the short-term ones and the long-term ones?

We have had a gentleman here for the last few Work Group meetings who was out there for 25 years. So, to say this was just a short-term exposure, I beg to differ for

that on the contractor. I don't know how you are going to be able to determine this.

I know that it was said earlier that we have got 90 percent of all NLO employees' and subcontractors' bioassay records. We are building this coworker model to be able to take care of the other 10 percent. Is that fairly correct?

MR. ROLFES: Correct.

CHAIR CLAWSON: Okay.

MR. ROLFES: And to clarify, those long-term employees are typically not unmonitored employees. The longer the employee is there, the more likely it is that they are monitored in just about every case we reviewed. There may be exceptions, but what we are talking about is the unmonitored subcontractors. Those are the ones that had the short duration of employment and didn't provide bioassay data. That is what we are trying to develop, the correction factor for these short-term employees.

CHAIR CLAWSON: Well, and I understand that, but we have been to numerous ones of these meetings, and we have numerous subcontractors, especially that were there for years, and they say they weren't monitored. Now you say that they are. But I really haven't seen anything that ties down that they were, until we go through a FOIA request, and so forth.

So, when we build this model, this model is going to have to address everything on that. Because if they have been unmonitored, and maybe they were out there for, if they were out there a year, I classify them as a longer-term employee.

So, I am really having a hard time following what we have really got and what we don't.

DR. MAURO: This is John.

I have an idea. Let's say you grab 100 random samples of claimants that you know to be contractors for a given year. I'm

not sure if you can do that, but you just grab them and you say, okay, let's compile all of their bioassay data. Don't even talk about intakes because we realize it is going to be hard to predict what that intake is because of the timing.

But let's just compile their bioassay data. Maybe they have two or three urine samples per person per year. And make a distribution of what the picocuries per liter are in that group of contractors.

For this same time period, grab another set from the workers, the employees, the Fernald employees, and make a similar plot. See if there is a difference in the distribution of the concentration. Stay with me for a minute.

According to your theory that we are postulating here, you are saying you do expect to see a difference. That might result in your concluding that there was a higher intake amongst the contractors, for the

reasons we just discussed, that is not true. The higher bioassay that you might see may very well be a result of the patterns of exposure and how the samples are collected and, therefore, be a false difference. But that is what you are stuck with. Maybe you are stuck with that.

So, what happens, then, is if you get that mean of the construction workers, and you find that the mean in becquerels per liter in the urine is a factor of two higher, three higher, whatever, you are going to end up saying, well, lacking any other information, we are just going to assume that the intake for the construction workers in that year was a factor of two higher or three higher, or whatever it is, even though you recognize that it might be a false estimate because you really don't know what the pattern of intake was.

So, the worst thing you could do, the benefit of the doubt, would be, well,

let's just assume that it is real and that the concentrations in the urine that we are seeing in the construction workers are, in fact, on the average a factor of two higher, at least in that year, as compared to all the other workers.

And you end up assigning a higher dose, but it seems to me that it would not be implausible, first of all, if it was a chronic exposure over a year. But since you don't know whether it was chronic or some short-term thing, you would be giving them the benefit of the doubt and assigning a higher dose that perhaps is not, in fact, higher. But since you don't know, you have no choice but to do that.

In other words, I am sort of offering -- I often do this -- offering up a strategy that might work that would be plausible, but also, at the same time, give the benefit of the doubt that the construction workers may very well have experienced higher

intakes in a given year.

That idea that I just threw out, is that something that rings true with the other technical folks around the table? Or do you think that maybe that is not the right way to go?

MR. STIVER: John, this is John Stiver. May I can get a word in here.

I understand where you are going with this. I am just looking at kind of a timeline here and what might be practical from a dose reconstruction standpoint.

From 1985 on, they have bioassay data for the construction workers and for the NLO employees. From what I am hearing, they are not really all that different during that timeframe.

During the pre-1985, what I am hearing is that we have got this Type 50 data for both construction workers and non-construction workers, which represents this short-duration type of a sample where you are

probably going to overexaggerate or you are going to overestimate any given intake unless you are able to adjust for the time of intake. That is the kind of information they don't have. They don't have the information on the start dates and the end dates. You just have these samples, and you can kind of make some inferences, but you just are left with this open-ended range of potential intakes based on that data.

Therefore, I am also hearing from earlier in the discussions there are data for these subcontractors in that early period that are not the Type 50. That goes to, in my mind, would it be possible to identify construction workers who may not have had just the Type 50 or may have had some of these longer-term monitoring results, which would then allow us to compare, given that you had an adequate sample size. At that point, you would have all the data you would need to do some kind of a side-by-side comparison.

In this initial run that Harry and Bob did, we thought we were really looking at that very type of analysis. It turns out that we have kind of an apples-and-oranges thing here.

So, I don't know if it is intractable at this point in terms of doing a real comparison of like-type results that doesn't have this confounding factor of the short-term spot-intakes, or whether you are basically stuck with that. I guess that might be a question for Mark and Gene, if there are those types of data available that might fill in that gap for us.

MR. ROLFES: Before I respond, I wanted to offer Dr. Ziemer the opportunity to speak.

MEMBER ZIEMER: Well, I am not even sure I remember my original question.

(Laughter.)

A lot of things have been mulling around in my mind.

Just for clarity, NIOSH, what you guys are proposing now for your newest coworker model is to include the Type 50 samples, which you didn't include before, which actually drives the value upward because you are assuming it is chronic rather than these short-term exposures.

We know, in the way you do chronic, you assume a long-term exposure that led to that urine sample. So, that drives the coworker model up. And you are saying it might be a factor of two.

As I understand it, depending on how you utilize that data, I think SC&A is saying it may be five to eight times higher.

MR. STIVER: It can be, yes.

MEMBER ZIEMER: And my original question was, based on what you have heard today about the Type 50 samples, what would be needed? Because it is that factor of two versus five to eight is sort of the issue. Would what John Mauro is proposing answer the

question?

The problem I am having is, one of the problems is, that NIOSH has indicated that it appears that most of those contractor samples in the HIS-20 database were the Code 50 samples. I mean, there is a statement. NIOSH has concluded it meant -- well, this is actually your interpretation of NIOSH. This is SC&A's interpretation. "NIOSH has concluded that many of the contractor bioassay records in the HIS-20 database are denoted as sample Type 50."

That tells me that, even though you don't know all the jobs, you do know whether the samples are contractor or subcontractor versus what? When you say "contractor," what are you talking about here? You are not talking about construction necessarily.

MR. ROLFES: Correct me if I am wrong, Jim, but --

MEMBER ZIEMER: I mean the

operator of the site is the contractor.

MR. ROLFES: Right, right. We would be referring to a subcontractor such as a construction subcontractor.

MEMBER ZIEMER: So, your statement that many of the contractor records, are you really meaning subcontractors?

MR. STIVER: I meant subcontractor records.

MEMBER ZIEMER: Okay.

MR. STIVER: For these construction workers. I am trying to think, is there some, that missing piece of data there that might --

MEMBER ZIEMER: So, are you able to identify that much without knowing job categories?

MR. ROLFES: Yes, we can identify who the subcontractor employees were.

MEMBER ZIEMER: Without having a claim or anything?

MR. ROLFES: Correct, correct.

MEMBER ZIEMER: That you can do?

MR. ROLFES: That comes from the hard-copy records which we analyzed separate from the HIS-20 database.

MEMBER ZIEMER: And you have already identified that a large number of those are the Type 50 samples?

MR. ROLFES: Yes.

MEMBER ZIEMER: Most of them or --

MR. ROLFES: I think, essentially, all of the subcontractor urinalysis results were either Type 59 or Type 50 samples.

MEMBER ZIEMER: Up to a year when they sort of stopped doing that in the eighties?

CHAIR CLAWSON: `85, roughly.

MEMBER ZIEMER: Yes.

MR. ROLFES: But the Type 50 and 59 sample is not exclusive to subcontractors.

MEMBER ZIEMER: Right. I understand that. I understand that. Right.

What you did is you re-analyzed

everything putting the Type 50s back in, and that raises your --

MR. ROLFES: That would raise the intake --

MEMBER ZIEMER: -- your assigned coworker model data --

MR. ROLFES: Correct.

MEMBER ZIEMER: -- in your mind, about a factor of two?

MR. ROLFES: And SC&A's --

MEMBER ZIEMER: And SC&A's was about a factor of five to eight in certain cases.

I was trying to get a feel for what information was missing for you. Because early in the discussion today we talked about going back to some original records, the written records, that might help resolve that part of it. Is that something different than what John Mauro is suggesting?

MR. STIVER: It is a little different.

MEMBER ZIEMER: I think what John is suggesting is a good idea, but I am not sure, because if you sample the claims, it looks like the construction worker or the subcontractor data is going to be largely Type 50s anyway, and you are back to the original problem.

MR. STIVER: You are basically going to have what would be -- correct me if I'm wrong, John, or if I get this wrong -- but, yes, you would be able to look at strictly the subcontractors versus the NLO site employees.

MEMBER ZIEMER: Right.

MR. STIVER: But you still have the confounding factor that you have got predominantly these Type 50s for the subcontractors. So, really, what John was saying, if you couldn't get any greater detail on the periods of employment for those workers, what you could do would be just say, okay, even though we realize this distribution

for the subcontractors is biased, we are going to go ahead and assign to them to be claimant-favorable, to account for --

MEMBER ZIEMER: And you will assume that it is a chronic exposure?

MR. STIVER: And assume it would be a chronic exposure.

MEMBER ZIEMER: And compare it to the others. But isn't that sort of what you did in a way?

MR. ROLFES: What John Mauro has suggested is what we have completed already and we have proposed.

MEMBER ZIEMER: Yes. Right. That is where they are a factor of two.

DR. MAURO: No, I'm sorry to interrupt, but, no, you blended them in first. I am saying, no, let's just look at the contractors by themselves before you blend them in and see if, in fact, there is a difference in the mean concentration in the urine in that population of contractors as

compared to the big population of, I guess, the current database.

So, I don't know if I have heard that yet, you know, that that, in fact, was done and what that difference is.

DR. GLOVER: From a programmatic standpoint, I just sat in on a very long, internal NIOSH SRS discussion regarding coworkers versus the general population. We are going to separately analyze the coworkers for SRS, or the construction workers, and compare that to the bulk. And the intake rates, they were going to determine what is the coworker model for this guy as you do these quarterly breakouts, and does the distribution look any different than that?

So, I think that is what John said.

MEMBER ZIEMER: Particularly at the upper tail.

DR. GLOVER: So, we look at the 50th and 84th percentiles.

MEMBER ZIEMER: Yes.

DR. GLOVER: Just for internal. So, we are all going into different -- I know that is what SRS is going to put on. That is what the Board is going to see, and that is what SC&A is going to see from the SRS.

MR. STIVER: Okay. So, we kind of have a precedent established, then?

DR. MAURO: That is great. Then, this would be consistent with that.

MR. ROLFES: This is Mark again.

We have done this for Fernald. There are differences between the excretion rates of subcontractors versus non-subcontractors, but you need to compare the total intake. That is the key, because a subcontractor could have a higher excretion rate than an NLO employee.

DR. MAURO: And, Mark, I am sorry to interrupt, but I agree with you. But I heard you say that you may not have enough information for you to convert the excretion

rate to intake. So, you really don't know, and you are not going to be able to come up with the intake.

All I am saying is, well, then, just assume that excretion rate is from chronic, which would be certainly conservative and claimant-favorable, and assume it occurred continuously over the course of a year.

I mean, if you can't get to the intake rates from that, it seems to me you have no choice but to do that or claim you can't build a coworker model.

MR. ROLFES: One could do that comparison, if it was a chronic annual intake. We do have information for subcontractors who are claimants, and that is the clarification. We do not have it at this time for people who are not claimants.

You know, a person would have to file a claim in the first place for us to receive their data to do that analysis. Because we have maybe 1400 claimants, I think,

from the Fernald site. There were several more thousand individuals who worked at the site. So, it would take a lot of time, money, and effort to go and look for data on employment histories for people who are not claimants.

DR. GLOVER: Would it be a fair statement at this point to say that we could take what we have heard from the Board under advisement and respond back? I think I have heard from the Board something consistent with what we have heard at other sites.

CHAIR CLAWSON: I just want us, when we walk away from here, that we have a path forward that we are going to be able to track, not just, yes, we want you to go out there and reevaluate this. Because this has been to the Board for quite a while, and I just want to make sure that we get to finish sure where are we going with this. Because we have kind of been back and forth.

I will be honest, my thing was

that, if we have got all this information and we can separate out the subcontractors from the contractor, well, why are we even having a coworker model, bottom line, except for that 10 percent there?

So, I was under the impression that we couldn't really for sure separately out who was a subcontractor and who wasn't. And now, today, I am hearing that we can.

So, I just want to make sure that, when we leave from this discussion, that we have got a path forward. I understand what you are saying, Sam. So, my question is, from SC&A and NIOSH, what are we looking at for a path forward, to be able to bring this to an end?

MR. BARTON: This is Bob Barton with SC&A.

I think, through all these discussions, the major hurdle with all this is the information about the employment period. Because if these are acute intakes that we are

seeing with these Type 50s or these contractor records, how do you take that result and make it an intake?

And it seems like, from the electronic records and even the hard-copy bioassay request forms that we have looked at, we really can't figure out when these people worked, for how long, to make it a meaningful intake.

Now, if you go to the claimant files themselves, then you get that information all of a sudden. So, it seems like, if you really wanted to compare the intakes of the two groups, you would have to go in and sample claimant records.

MR. ROLFES: Correct.

MR. STIVER: And, Mark, how many? Do you have a feel for the number of subcontractor claimant files that are currently available, without going and --

MR. ROLFES: We had gone through a spreadsheet a while back looking at exposure

information for each claim. At that time, we might have had only 1100 or 1200 claims, I think, from the Fernald site. We had gone through and identified how many of those 1100 or 1200 claimants had no uranium urinalysis.

From my recollection, it was just under 100. So, it was a little less than the 10 percent that have referred to.

I think we identified approximately 10 of those cases that appeared to be subcontractors. Now we have to update the analysis, if that is something that you would like.

MR. STIVER: Sorry to interrupt, but how about the subset that actually do have the bioassays? You have the bioassay and you have employment periods that you could --

MR. ROLFES: I don't have a number for you right now. That would be something that we can definitely get back to you with.

MR. STIVER: That might give that hook that we really need to get a handle on

what a reasonable employment period would be. Or at least you could get intakes for those people.

MR. ROLFES: Right.

MR. STIVER: Or you might not be able to bound it otherwise.

MR. ROLFES: Gene, this is Mark. Is that something that we might be able to pull together quickly?

MR. POTTER: The claimants --

MR. ROLFES: Yes, out of our claimant population, could we identify how many subcontractors and what employment duration they had, and whether or not they had bioassay data in their files or in the hard-copy records that we've --

MR. POTTER: Yes, this is something we have done before, but have not updated recently.

MR. ROLFES: Correct. I think we might have done this maybe about two years back, when this initial issue was identified.

MR. POTTER: This is Gene again.

If I could just maybe go back to one of my original points, John Mauro has presented an idea which certainly would seem to represent an upper end for subcontractor intake estimates. We have presented one. And I believe John Stiver at the beginning of this discussion seemed to agree that these are all issues that could be worked out.

I am not in a position to make up NIOSH policy, obviously. But this is a set of circumstances we have here that could come to some resolution in the technical basis or coworker arena rather than taking the Working Group's time up discussing this as an SEC issue.

DR. GLOVER: But that is your decision.

CHAIR CLAWSON: That comes down to our decision. But I guess, as a Board Member, I want to be able to make sure that I can review this with scientific, sound

information. Because it could be an SEC if we can't get the information out there.

There gets to be a point to where, yes, this is bounding, but is it plausible, too? So, I want to make sure that we address this as clearly as we can.

And I understand what you are saying, but this, then, comes down to the Board's decision. I understand what you are saying, but we have also got to look at this from a plausibility standpoint. We just can't throw a number out there and say, "Well, yes, that's going to be bounding," because we have got to have some scientific validity to back that information.

MEMBER ZIEMER: If there are two different distributions, but let's just suppose there are, one for these subcontractors and one for the other folks, would NIOSH then have two coworker models? Or would you take the upper of the two and say that's the coworker model for everybody? Or

is that a decision that would have to yet be made?

MR. ROLFES: That is a policy decision --

MEMBER ZIEMER: A policy decision.

MR. ROLFES: -- that would have to be made. I mean, that is not a matter of the scientific ability to create the distribution.

MEMBER ZIEMER: Right, right. It is a matter of, okay, now that we have done it, how do we apply it then?

MR. ROLFES: Right.

MEMBER ZIEMER: Because you might have cases where you -- well, maybe not -- where you can't really identify. I guess if you can't identify, then you always use the upper one anyway. But, okay, that is premature then. I just wondered if there was a --

MR. ROLFES: Trying to focus back on reality, the number of claimants that we

complete dose reconstructions for for the Fernald site, in reality, there's very, very few that we actually need to apply --

MEMBER ZIEMER: Right.

MR. ROLFES: -- a coworker intake model.

MEMBER ZIEMER: Right.

MEMBER SCHOFIELD: Mark, I have got a question for you. You know, you talk about the subcontractors, and maybe you have some of the subcontractors come in and they do things like fencing. Maybe they do some of the painting on the outside of the building and stuff.

But, then, you have these other contractors like ABC Destruction that comes in on a regular basis over a period of years, but maybe they only may be there for days, weeks, or just a few months. They rotated their people in and out there constantly because maybe they need the tenders for a few days to rip out a bunch of stuff before they have the

heavy-equipment guys come in.

And now those types of jobs and positions, you would expect to see a greater chance of higher acute intakes than those people who are out there painting a post or mowing along fencelines, things like that.

Can you identify that difference or are you going to put them together?

MR. ROLFES: Yes, that is a good point. I mean, that is what our discussion is about.

You know, if we don't have the employment information for that claimant or for that person, we would have to have them file a claim to get their employment information. From there, we would be able to identify what their worst-case potential exposure could be.

In a case that we didn't know that a person only entered into a radiological area one time, but they provided a bioassay result, if we had their employment information saying

that they worked six months, we would assume for that entire six months that they were potentially exposed, and we use their bioassay result to assign an intake for that entire six-month period that they were employed.

DR. GLOVER: Oftentimes, these coworker models -- and I apologize, Brad. I saw you were about ready to speak.

CHAIR CLAWSON: No, no.

DR. GLOVER: We actually use -- and, Mark, you look at a lot of these -- but just because a guy works a few days, we take that six-month integrated exposure rate and multiply it. We give him that intake. This is the intake. We don't use a two-day rate, because that two-day rate out here may have been what gave the guy the intake in general, because we don't know the intake rates when we do develop these coworker models.

And so, we don't say, okay, this is a micro-R per day and we are figuring out a very small intake rate, and that is what we

assign. We actually still use these bigger intake rate sets or intakes.

MR. STIVER: This is John Stiver. I have a question for Sam.

A while back, you mentioned Savannah River, that you are looking at these two different distributions. It sounds to me like you have got them pretty well characterized as far as intakes with the construction workers versus the site employees.

Now were you able to locate this kind of employment history data for the Savannah River construction workers, the subcontractors? Do you know?

DR. GLOVER: I don't believe that -- from a policy and from an analysis standpoint, NIOSH doesn't want to try to go in and dig out. I believe that if we can leave this at compare the two distributions without trying to micro -- because that becomes, if we start trying to do this at every site, that is

going to be very difficult.

MR. STIVER: That's true.

DR. GLOVER: And if it is still a claimant-favorable number -- that is why I think it wouldn't be a bad thing to walk away with -- the decisions made here on the spur of what our final path forward is, it is hard to speak for NIOSH as to what the final number needs to be.

MR. STIVER: Yes, I was just trying to seek clarification of whether that type of data might have been available for some of the sites or if you just looked at the distributions and, like John Mauro had proposed, just take the two different distributions and just assume there are chronic intakes.

DR. GLOVER: Yes, depending on the site, they may be able, coupled with external dosimetry programs and what monitoring -- you know, so there is other practices that could perhaps be done.

CHAIR CLAWSON: Well, I want to caution us on one thing, too. I know that Sam kind of touched on this.

Being on the Savannah River Work Group, I realize what quality records that they do have. So, the way they were kind of split up is a little bit different than what Fernald was in the earlier years. I think we would have a much harder time separating these two groups out.

This is why we went to one-size-fits-all. Because my understanding was -- and you can correct me if I'm wrong, Mark -- but up until `85 or so far, the Ohio Lead people were intermixed with the contractors. The issue gets into that is all well and fine; we can separate out who the subcontractors are when we have the hard-copy data. But if we have another 1,000 or 2,000 people that haven't filed a claim, we don't have their employment history.

This puts us right back to what

Sandra was saying earlier on this. This puts us into a situation of how do we go about this.

So, I suggest, looking at the time and the way everything is going on, I would like to take a comfort break right now. And then, we can come back and we can discuss a path forward that we want to be able to do, if this is all right with everybody.

MR. KATZ: Yes, that sounds good.

CHAIR CLAWSON: Okay.

MR. KATZ: Okay. By my clock, it is 10:50. So, 10 minutes you said? Fifteen minutes? How much?

CHAIR CLAWSON: Let's give them 15 minutes.

MR. KATZ: Fifteen minutes. Okay. So, about five after, we will kick back in.

I am just putting the phone on mute.

(Whereupon, the foregoing matter went off the record at 10:51 a.m. and went

back on the record at 11:06 a.m.)

MR. KATZ: So, welcome back.

This is the Fernald Work Group. We just finished a break.

CHAIR CLAWSON: Well, I guess what I am looking at is the other Board Members and SC&A and NIOSH to be able to determine a path forward, which way we want to go, what we have got to be able to go to, to come to a conclusion on this.

Because we have been dealing with this for quite a while. We need to come to a resolution.

Ray, you made a comment to me that still needs to go onto the record, but you spoke to me about it earlier, about the contractor jumping back and forth.

MR. BEATTY: Right. My name is Ray Beatty, a former worker. I served for 14 years.

Having a lot of dealings with the union business and working on the Davis-Bacon

Committee, I had a lot of interaction with the building trades in that respect. It wasn't uncommon to see building trades guys come out of the hall and come and work in-house, whether it be as a Fernald Common Trade Labor Council Union represented or they might become a salaried person. And then, when that campaign was over, or whatever they were assigned to do, they would go back to the building trades. So, there was some back-and-forth movement there.

And keeping that separation would be key on doing this two-times-a-dose thing. So, you would have to take it into consideration.

And another thing, I kind of detect something, too. There seemed to be a little bit of a problem possibly of identifying, but I have since learned that at Savannah River site they are trying to use badge numbers to segment or break apart the building trades or the subcontractors from the

in-house.

That could very easily be done at Fernald, I think. Because of the uniqueness of badge number assignments, that might be an easy question to say, like DOE, as to what low number was assigned to NLO, say all the in-house union-represented employees versus salaried. And then, there was a separation for the building trades.

I know that for a fact, that in-house union employees were four-digit numbers; salaried had five, because I had that separation one time myself for a short period of time.

MR. ROLFES: You are right, Ray. This is Mark Rolfes.

When we had badge numbers available, there was typically a 5000 series. They were usually in a form of F-5000 something or R-5000 and something.

MR. BEATTY: Okay.

CHAIR CLAWSON: So, we have looked

at being able to separate them by badge number?

MR. ROLFES: Yes, but, once again, let me go back to the hard copies. The bioassay data for subcontractors is usually delineated. If there isn't a badge number, they are delineated by the subcontractor name.

MR. STIVER: So, what you are saying, then, is that the bioassay data provides a better identification than, say, the badges?

MR. ROLFES: It tells us which company they worked for and would give a better indicator as to whether they were involved in construction or something else.

CHAIR CLAWSON: Well, I guess I am looking at suggestions to be able to move forward. Because, right now, we have not been able to come to a conclusion of what to be able to do with this.

On the one hand, we feel that it is able to be bounded. But, on the other

hand, I am having a hard time saying that we can justify it, too.

So, I guess I am looking to other Board Members, SC&A, and NIOSH, to be able to say which way we want to be able to proceed with this area. Because we've got to come to a conclusion with it.

So, Paul?

MEMBER ZIEMER: I think I heard, Mark, one of your colleagues say that you had actually done a similar analysis before, similar to what John described.

MR. ROLFES: Yes.

MEMBER ZIEMER: And that perhaps that could be updated using some of the additional claims that have come along. If it is feasible to do that, and recognizing that even though we are talking about a coworker model that will probably only apply to less than 1 percent of the workers who made claims or something like that, a very small number, you still need to have it, right?

MR. ROLFES: Yes, it wouldn't be 10 percent. It would be about 10 claims that we would need to apply this to.

MEMBER ZIEMER: Well, there may be future claims.

MR. ROLFES: And there could be some, yes.

MEMBER ZIEMER: But, in any event, you need to have some kind of a coworker model, apparently.

MR. ROLFES: Right.

MEMBER ZIEMER: One gets a little concerned about we spend 90 percent of our effort trying to deal with those few claims. But, be that as it may, is it feasible to do something along the lines of what John described, to ascertain differences in these distributions, and then to be able to make a decision as to what you do with that information. Or does SC&A need to look at more finer detail than they were able to do from the HIS-20 database, and try to resolve

that five-to-eight factor versus the two factor? I am just asking what is feasible to do to bring that --

MR. STIVER: This is John Stiver.

I think that we came very close, really, to what John Mauro had mentioned earlier in our original analysis here, or at least as the first step in that, in this comparison, this draft comparison.

The difference being that the Type 50 data that we looked at was a mixture of both subcontractors and non-subcontractors. So, to the extent that we could narrow that down to only the subcontractors, I think we would have the basis for this side-by-side comparison, looking to two distributions, acknowledging that, yes, you are somewhat comparing apples and oranges here because you have some of these spot-samples and short-duration samples, predominantly for the subcontractors, and there is not so much for the others. But we could certainly make those

kinds of comparisons if we had that kind of purified, if you will, dataset.

MEMBER ZIEMER: It appears to me for the other side, the large group, that the inclusion of the Type 50s probably has very little impact on the distribution, since it is a very small part of the distribution. So, whether you left it in, blended or not, probably is not going to affect that, but it will definitely affect the other side.

MR. STIVER: Well, I think we have seen, if you leave it in, basically, you are seeing there is an increase to 1.2, 1.5.

MEMBER ZIEMER: A little bit.

MR. STIVER: Right.

MEMBER ZIEMER: Is that what it is?

MR. ROLFES: It is just a small increase in comparison, yes.

MEMBER ZIEMER: Yes, right.

MR. STIVER: Whereas, it is quite a large increase, a factor of four, from what

we have seen.

MEMBER ZIEMER: Right.

MR. STIVER: The other thing that we would like to look into, if possible, if NIOSH could investigate the availability of the employee records for claimants, and to what extent that data is there and usable. I mean, it might be possible to at least get some sort of a handle on what the employment periods were, what the distribution of those periods might be.

That seems to be the real final problem here, that one missing piece of information that we would need to get a robust model put together.

CHAIR CLAWSON: The way I am seeing this is we have actually, in my mind, we have got a path forward, but it is kind of two-pronged. NIOSH needs the raw data, the raw information, from NIOSH, correct.

I guess this is actually NIOSH -- we can give suggestions to NIOSH, but,

actually, NIOSH has the responsibility to give us their path forward of what they want to do. We can evaluate it past then, but I just want to make sure that we are not sending NIOSH off in a direction that is not going to be usable for us.

And you said that you have already got the raw data, that it just may need to be updated?

MR. ROLFES: Yes, correct. We previously went through and looked at how many people were unmonitored and whether or not they were subcontractors. That was done about two years ago. So, we had to go through any additional claims that had been received since then.

CHAIR CLAWSON: Okay. So, that would have to be updated. Do you feel that that is going to change NIOSH's response of the .2 being a bounding coworker model? Or what I am trying to get to here, Mark, is I want to be in unison when we get this product,

to be able to have SC&A actually continue on and look forward.

Because, to tell you the truth, in listening to what John said, I think that we have basically already done this to a point on either side. I am trying to figure out how to be able to tie this together and put this to bed one way or another.

So, I guess, what do you need?

MR. BARTON: Well, if I can make a comment here, one more, too, when we talked about going in and looking at these claimant records, what we really meant was to go in and find a group of contractors or subcontractors who have monitoring records. We can evaluate those claimants, evaluate their actual intakes.

Now we have an intake value that we can reasonably go and compare with the coworker group. Because, right now, we are kind of almost comparing apples and oranges because you might have some acute intakes that

you try to compare the urinalysis values with chronic intakes, and that is kind of the gist of where NIOSH is coming from by saying, well, we are going to have some chronic intakes. So, this is going to bound the acute intakes.

Well, you could find that out if you went in and you found a sample of subcontractor claimants, evaluated their intake, and actually compared intakes to intakes, because that is where the real meat and potatoes is.

CHAIR CLAWSON: Okay. This coworker data, this coworker model is not just going to be for contractors. It is going to be used for everybody, if I am understanding this.

MR. ROLFES: We have developed a uranium intake distribution for all employees who were potentially unmonitored at the site. What we have proposed for subcontractors was to multiply the full distribution of all employees that we have available to us by a

factor rounded up to two for subcontractors who were not monitored.

CHAIR CLAWSON: So, the Fernald employees would be given one coworker model, and the contractors times by two?

MR. ROLFES: Yes.

DR. GLOVER: But that is consistent at Hanford.

MR. STIVER: We will use this pooled dataset, and then use it to apply different values to different subgroups.

DR. GLOVER: Yes.

MR. BEATTY: Could I interject something? I am sorry for interrupting.

CHAIR CLAWSON: Could you state your name?

MR. BEATTY: Ray Beatty, and I am a former worker.

To try to rationalize what is being done here, it is difficult for me because of being a former worker, not in a production era, but in the cleanup. I can

only fathom what the production workers were exposed to versus building trades or subs, with the sub group coming in to maybe do a new facility or construct this thing. And there is no radiological hazards yet there, until the production people come in and put it there. But, yet, they are going to be assigned a higher number to do a dose reconstruction.

Do you see my point? Like if someone tried to file a claim in 1951 or 1952, prior to production even starting up -- and let's face it, construction built the site, but there was no constituents of concern at that time. So, there would really be no basis for a claim there. It would be easy to not even file a claim. Or you can talk to someone and say you can't claim something that wasn't there yet. That is kind of what I am saying with the building trades on doing certain campaigns.

Now, in the cleanup years, just

the opposite. They were exposed to a lot of mixed waste, gross contamination coming together with all this residue. So, it is just the opposite of what it was when they were building the new facilities.

MS. BALDRIDGE: Can I interject? This is Sandra.

Some of the documents in the petition state the dilapidated conditions and the need for repair and going in and changing from one operation to another. In those cases, the workers coming in at that level would be exposed to all the dust and all the contamination that was there in the tearing-down and reconstruction. So, it would definitely have a bearing, whether it was new construction on a clean slate or replacing a facility or repairing a part of a dust collector or, you know, something that was already assessed.

MR. ROLFES: This is Mark Rolfes.

I understand exactly. What we

would do in a case where we had an individual who wasn't monitored, we would give the benefit of the doubt to the claimant and assign an unmonitored intake to that worker, if we had no information to contradict that.

MEMBER ZIEMER: But if they were there before the sources were brought, I assume that you wouldn't do that.

MR. ROLFES: Correct. We would not assign an intake prior to the site being a covered facility with radioactive materials on that site.

CHAIR CLAWSON: Well, it comes back to this: basically, in my feeling, Mark, it comes back to, if what NIOSH's stand is on this coworker model, do they -- ultimately, it is up to you to tell us what you are going to try and SC&A to be able to review that. We can give suggestions, and so forth, but I guess after today's talk I am looking at you and Sam both of where do we want to go? Where do we want to head from this?

We have heard what our issues are and our problems. I am looking for a path forward.

MR. ROLFES: Well, our opinion is that we can develop a correction factor based upon the data that we have available. And ultimately, I guess it is up to the Advisory Board to decide whether they feel that that approach, whether it is claimant-favorable, whether it is appropriate.

I have heard a lot of discussion of sufficient accuracy lately. And what a professional health physicist has as an opinion of sufficient accuracy in the completion of dose reconstructions might be different from the definition of sufficient accuracy for members of the public, for claims, for members of the Advisory Board, coming from different perspectives.

So, ultimately, at the end of the day, we can make scientific recommendations and provide scientific approaches, but it is

ultimately up to the Advisory Board and their contractor to decide what they feel is the appropriate path forward.

DR. GLOVER: Brad? I'm sorry.

MEMBER ZIEMER: Well, I think the only question in my mind for you folks is whether or not that factor of two changes with the newer, the additional data that you have. Your analysis was based on claims up to what, 2009 or something?

MR. ROLFES: The factor of two would not change. That would just be, unless we received -- well, I don't see that factor changing based upon additional claimant data. That would just give us indications of how many additional people might have been unmonitored or monitored.

MEMBER ZIEMER: Yes, I am thinking probably the number of claims or -- the claims aren't going to be that different from what you have already looked at in terms of the distribution. So, I guess I would be

surprised if your two changed based on that. So, that leads me to think that we need to finish up what you guys are thinking about and asking about that five-to-eight or whatever those numbers were, whether or not that changes for you after you look at the hard-copy stuff.

MR. STIVER: Yes, I think we would want to look at the hard-copy stuff and, also, the report that Gene mentioned, where they looked at employment duration and bioassay for two years. We could see that.

MR. ROLFES: To clarify what I said, Dr. Ziemer, the factor of two shouldn't change because we built our coworker model, the adjustment factors for subcontractors, based upon all the data available to us.

MEMBER ZIEMER: Right.

MR. ROLFES: So, we already have data for non-claimants, their bioassay data.

MEMBER ZIEMER: Right.

MR. ROLFES: So, the only thing

that we wouldn't have would be, when we are comparing the total intake between two different populations or two potentially different populations of workers, the total intake could possibly be different.

Once we get the subcontractors' employment duration, we would know their potential total intake, which could be compared to an NLO employee's total intake. So, that would change a little bit in the actual application of the coworker model. But the factor of two, the bottom line wouldn't change, as we have already rounded it up from the actual factor that we calculated. The highest factor for any years was a factor of 1.61, I think, and we rounded that up to two. So, I don't see it jumping up based upon the approach that we --

MEMBER ZIEMER: A few more cases, right?

MR. KATZ: Can I just ask a question? It seems like there is some talking

past each other on this.

What we heard today was that, I mean, John Mauro and SC&A sort of proposed a pure comparison versus the mixed comparison that you have performed. And so, it seems like the question is, does DCAS want to do that pure comparison to sort of verify what the real factors should be versus this mixed comparison, to button up this difference? It was sort of a substantial difference that there may be.

Like John Mauro said, you can just assume, as you did when you pooled them, that you treat them all as chronic, despite the fact that there are these differences, or whatever.

But I think that is what is on the table. Does DCAS want to do that analysis and at least give a chance of reconsidering what that figure is? Or are you standing by what you have, despite the discussion that was had?

DR. GLOVER: I think we would be

willing to do it. I think we want to do it, and I was going to offer, would it help to take a few example DRs and say this is how it would be applied?

MR. ROLFES: I mean, that is ultimately we are getting down -- we could just compare intake to intake.

DR. GLOVER: Take some of these guys who are -- we can't do it for everybody, but maybe we could say that this is some examples of how it would be applied for a guy who has data, but, you know, compare how those intakes would have been used if he didn't, but here's what his real intake was.

MR. STIVER: It would almost be sort of a pilot study comparison where you say here are the dose reconstructions for people we have the data for, and under these two conditions, here's what ultimately --

DR. GLOVER: This is what the thing generated as his intake, and here is what the intake would have been if we had used

his bioassay data to actually do a best estimate.

I don't know what the timeframe is. We don't want this to drag on forever. That is why, if we have our --

MR. STIVER: Go ahead, Brad.

CHAIR CLAWSON: Well, and this is kind of the dilemma I am in, because I really don't want to take this to the Board right now and tell them that we have got a difference of basically 6 percent on either side, because NIOSH is saying two and we are seeing anywhere from five to eight. Because, to me, it doesn't look like we have done due diligence on this. We have got a very large spread there.

I understand what Mark has said is that this is what DCAS's stand is, the .2. This is where I am really having a problem of which way to push forward, because that is a big difference there.

MR. KATZ: I think they haven't

made a stand yet, right?

MR. ROLFES: It is not .2, but a factor of two, a multiplier of two, rather than .2.

But we have done a direct comparison for clarification on an annual basis of the actual uranium urinalysis excretion rates. We got similar results to what SC&A has already gotten.

If you take a look at one particular year, the factor was about 1.6, 1.7, 1.7, and then it went up to 2.2, 3.6, 1.8, up to a factor of five and six, back down to three, 1.8, and less than one, which was .9 factor.

MR. BARTON: Mark, what are you reading off of right now?

MR. ROLFES: This is something that we had previously done and sent out.

MR. BARTON: Okay.

MR. ROLFES: It was a direct comparison of the subcontractor urinalyses

plus all of the Code 50 compared to the coworkers on an annual basis.

CHAIR CLAWSON: So, Mark, excuse me for interrupting.

But this is something that SC&A has not seen?

MR. BARTON: This is essentially the analysis that we presented in our --

MR. ROLFES: They did it as well --

DR. GLOVER: So, are we still talking excretion rates and excretion rates?

MR. ROLFES: No, no, no. This is just urine data. This is comparison of the Type 50 subcontractor urinalyses to the full distribution of urinalyses, comparing it to intake. What we had proposed, or what I had proposed, or we had been discussing, would be to compare the total intake for the two different populations of workers. That would give us the best indication. You know, is the subcontractor population different from the

full distribution of workers?

DR. GLOVER: And that is consistent with what we are going to do at SRS. So, that is what I propose that we come back with.

MR. STIVER: Okay. That is exactly what I --

DR. GLOVER: We can look at the distributions of those and see, do they match up or not?

MR. STIVER: Look at the two distributions --

DR. GLOVER: Of the intakes.

MR. STIVER: -- of intakes. And then, from that, you can derive whatever adjustment to this factor.

DR. GLOVER: Yes, because that is the dosimetric unit of pertinence.

MR. ROLFES: It is the total intake that that individual experienced that needs to be compared.

DR. GLOVER: Does that seem

acceptable?

MEMBER MELIUS: This is Jim Melius.

I am a little confused because, Sam, you talked about this as doing example dose reconstructions. I think we need more than just a few examples. There needs to be some statistical basis to that.

I recognize that your available data may be small, but I think it needs to be more than just one or two cases.

DR. GLOVER: I apologize. I apologize, Jim. We would do a full analysis --

MEMBER MELIUS: Okay.

DR. GLOVER: -- and show you what the intakes were. And I thought it might be practical to follow that up with a few examples of how it was used.

MEMBER MELIUS: Okay.

DR. GLOVER: This certainly would not form the basis, right, Mark?

MR. ROLFES: We can compare statistically the distribution differences or the total intake differences between the two populations, if that would give you more meaningful information.

CHAIR CLAWSON: Okay. Then, it is looking like we have got a path forward for Issue No. 1 here. I guess I will take it over to Sam and Mark of what your path forward is because, to tell you the truth, I don't understand it right now. You have explained it, but I just want to make sure that we are heading in the right direction and that everybody is clear on it.

What are you guys, what is your path forward?

MR. ROLFES: What we just proposed is to compare the total intake experienced by the subcontractor to the total intake experienced by our coworker intake model, basically.

MEMBER ZIEMER: These are

distributions, which would each have their own mean and their own variance.

MR. ROLFES: Right.

MEMBER ZIEMER: And then, you can statistically ask whether they are significantly the same or different.

CHAIR CLAWSON: Okay.

DR. MAURO: This is John.

Are you going to do that in the aggregate over time? Or are you going to pick different time segments to see if, in fact, it changes in the sixties as compared to the seventies, or something like that?

DR. GLOVER: It would sound like it is a coworker model. It is how you would do a coworker model for the 50th and 84th percentile, and you find where the breakpoints are.

MR. KATZ: Annually.

DR. GLOVER: Well, annually. It could be lumped differently, depending on how much data. But you would use --

DR. MAURO: Okay. I hear you.

So, you are going to try to break it up into time segments, to the degree to which you have sufficient data. If you can do it annually, great. If you can't do it, if it has to be by decade in order to get enough data, I guess that is something you have to look at.

MR. ROLFES: Yes, and we discussed this earlier. In certain years, there weren't many subcontracts going on at the Fernald site. So, you can't really break it down by year.

What we have previously done for our direct comparison, we had captured three different decades. We had 1969, 1971, 1972, 1973, 1981, 1983, 1984, and 1985. Those were the years that we looked at because those years were not in HIS-20. And also, those were the years that data was available to us.

DR. MAURO: Sure sounds good to me.

CHAIR CLAWSON: Okay. So, we have got a path forward for DCAS.

Now, for SC&A to be able to continue on, you were talking about earlier the data that --

MR. STIVER: Yes. We would like to see, first of all, the spreadsheets and the hard-copy records talked about earlier today that would delineate the subcontractors from the other NLO workers.

And also, to the extent that we could find some data that evidently is available from those, two years out of date, that links -- that shows the comparison of bioassay and employment duration for the claimants that have been processed. We would like to have those pieces of information available to us.

MR. ROLFES: The bioassay and -- could you repeat that last part?

MR. STIVER: A few minutes ago, earlier on, Gene Potter had mentioned that you

guys have some information on employment duration and bioassay for these contract workers.

MR. ROLFES: Yes.

MR. STIVER: But it was two years out of date.

MR. ROLFES: We actually compiled it for everyone.

MR. STIVER: Is it possible, then, to tease out the construction workers from that?

MR. ROLFES: I think we can do something. It might take a little bit more effort because I think we lumped it all together as one initially, when we had completed it. It will take a little bit longer. We can certainly do that.

MR. STIVER: We would like to have that.

MEMBER SCHOFIELD: Refresh me, Mark, on that very point there. When you were putting that together, just roughly -- I am

not asking you specific numbers -- did you see like there was much difference, much of a difference between, when you were putting that together, kind of a general feeling?

MR. ROLFES: Differences in what?

MEMBER SCHOFIELD: The subcontractors versus the regular contract employees.

MR. ROLFES: And differences in?

MEMBER SCHOFIELD: In the exposure rates, the intake rates.

MR. ROLFES: Well, based on our direct comparison that I had discussed before, we did see differences in the excretion rates for uranium, which varied from less than a factor of one. The coworker model was actually a higher intake rate than -- or excuse me -- a higher excretion rate than the subcontractor intake rate. But, then, in other comparisons, it was up to a factor of four, five, six. The highest one that we had was 6.6. This is similar to what SC&A had

identified as a range of five to eight, up to five to eight higher.

But that only considers short-term. It only gets you a picture of the excretion rate. It doesn't necessarily tell you about how long that person had an intake which produced that excretion rate.

So, even though a bioassay result could have been higher, that doesn't always mean that the intake, the total intake rate was higher or the resulting internal dose.

DR. GLOVER: I just want to make sure, because SRS, the thing we are going to keep coming back to is we are going to compare intakes and intakes, and recognize that these things had a GSD of 5. They are big GSDs. They are a big distribution. These things aren't like a point estimate. There is a lot of variability in what the excretion rate, you know, these intake values come out to be.

So, when you lay them on top of each other, do they look the same? I mean,

that is really the bottom line.

MR. STIVER: We are going to have to look at intakes and, yes, we understand that there is a huge amount of uncertainty involved.

DR. GLOVER: I think we are on the same --

MR. STIVER: Even when we have really good data, you still wind up with, based on the biogenetic models themselves, individual variability.

MR. KATZ: So, Brad, can we just get clarification now?

CHAIR CLAWSON: Yes.

MR. KATZ: SC&A is going to obtain these different materials from DCAS. But are you at this point just going to verify sort of what the discussion from today, on the basis of that, or are you going to produce some sort of new analysis?

MR. STIVER: I think what we would like to do is kind of take a look at the

analysis that we have already done and make an adjustment to that, so that we can be comparing two separate populations.

MR. KATZ: It seems to me they are going to be producing a comparison that you are going to look at.

MR. STIVER: This will give us kind of a metric that we could then look back to compare the data.

MR. KATZ: Okay. Okay.

MR. STIVER: We will not be doing the things in parallel.

MR. KATZ: I just didn't want you to be duplicating each other.

CHAIR CLAWSON: The way I understood is, in speaking with Gene, SC&A did not have this data. So, this is basically corroborating what you already put together a little bit.

But DCAS is going to -- and let me put this out to both sides, SC&A and DCAS. When we get done with this, we will have an

email sent to me and Ted to make sure that each side understands which path we are going down.

MR. KATZ: Action memos.

CHAIR CLAWSON: Action memos, yes.

MR. KATZ: Action item memos after the meeting, right. Right.

CHAIR CLAWSON: So, I want to make sure that that is clear, too. So, does everybody understand which way we are going?

MS. BALDRIDGE: I have a question.

CHAIR CLAWSON: Okay.

MS. BALDRIDGE: Is there going to be any differentiation of whether the contractors were working in a uranium area or a thorium area? There were furnaces that were torn down and removed which would have provided thorium exposure back in the sixties that would be different.

MR. STIVER: This is John Stiver. I might be able to answer that.

What we are looking at right now

is strictly the uranium bioassay and uranium analysis, dose assessment. Thorium is another issue altogether, and there are different approaches that are being used to get to thorium doses, quite different.

CHAIR CLAWSON: We haven't even gotten to thorium yet.

So, if we both have a clear line of direction of which way we are going to go, then I want to make sure if there are any more questions of what is being required of DCAS or SC&A.

MR. STIVER: I think we are clear on our side.

CHAIR CLAWSON: And, Mark and Sam, you understand what we are looking at?

MEMBER ZIEMER: Do you have a rough timetable for that? Are we talking about a month or two months, or a couple of days?

(Laughter.)

MR. ROLFES: You have asked for a

lot of additional things.

MEMBER ZIEMER: No, I know.

MR. ROLFES: Try to keep in mind that we are working on many other sites.

MEMBER ZIEMER: Right. I understand that. I am just trying to get a feel for whether this is something that is down the road a ways.

MEMBER SCHOFIELD: We will give you 48 hours.

(Laughter.)

CHAIR CLAWSON: Well, and I understand that DCAS is working on numerous other things, but I just also want to make sure that people understand Fernald has been on this table for over five years now. We are coming to the end.

MEMBER ZIEMER: We have got some other sites that are like that, too.

(Laughter.)

CHAIR CLAWSON: And I realize that. I am on a few of those. So, I also

want to keep that in mind, too.

So, if there's not any further questions on this, the next one we are going to go -- oh, goodness gracious.

MR. KATZ: Goodness gracious?

CHAIR CLAWSON: It is 11:41.

I am going to ask a question of everybody here. Do we want to -- because the next one on there is recycled uranium -- if we want to start into this right before lunch here? I would offer up not to because we are going to get barely started into it, and we will probably be one o'clock getting out of here.

So, if there are no objections, I would suggest that we break for lunch now and start with recycled uranium right after lunch, if that is all right with everyone.

MEMBER ZIEMER: What are our estimated times on each of these?

CHAIR CLAWSON: Well, I thought that Number 1 was not going to be that

difficult. I thought it was going to be about an hour and 15 minutes, but I was wrong.

(Laughter.)

MR. STIVER: I probably shouldn't do this, but I will get out there. I would say it shouldn't take us more than an hour to go through recycled uranium.

MR. KATZ: Okay. And what about the other issues? Are any of them shorter?

MR. STIVER: The other issue is probably a little more involved, the Issue 6b on the thorium chest count. That is going to take a lot longer.

So, if people want to break for lunch now, that's fine.

MR. KATZ: And then there is the recycled thorium.

MR. STIVER: The recycled thorium is kind of tied in with the chest count.

MR. KATZ: So, it sort of relies on the others? Okay.

It sounds like we need to do that,

then. We need to break now.

MR. STIVER: Yes.

CHAIR CLAWSON: Okay. We will break for lunch and we will be back --

MR. KATZ: So, for an hour?

CHAIR CLAWSON: Yes, let's be back here in --

MR. KATZ: So, let's try to get started, because it is a lot left, probably So let's try to get started at 12:45. Yes?

CHAIR CLAWSON: That would be fine.

MR. KATZ: All right.

Thank you, everyone on the line. We will be back online, we hope, at 12:45.

(Whereupon, the above-entitled matter went off the record at 11:43 a.m. and resumed at 12:46 p.m.)

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

12:46 p.m.

MR. KATZ: This is the Fernald Work Group hacking away. We are just reconvening after lunch.

CHAIR CLAWSON: Okay. I would like to welcome everybody back.

We are going to turn this over to John Stiver. We are going to be talking about the recycled uranium.

MR. STIVER: Excuse me. My throat is a little dry here.

This is John Stiver from SC&A.

The next issue we are going to move on to here is that of recycled uranium. This was SEC Issue No. 3. It has been the topic of discussion, basically, since I took over on this particular site in January of 2010. And so, we have been looking at it at just about six different meetings since that time.

Our initial concern, I will kind

of back up a little bit, and we will talk about what the original issue was. Recycled uranium is basically uranium that has already been through an irradiation cycle and it has been chemically purified for reuse. During the chemical purification process, inevitably, some of the contaminants, namely, plutonium, neptunium-237, fission products such as technetium-99, are carried through in the final product.

Because this material can pose a source of exposure to workers who handle it, and NIOSH really didn't have the bioassay or the monitoring data to ascertain intakes of these constituent radionuclides, as they are called, a strategy was set up whereby the uranium bioassay data could be used to derive an intake of uranium. When this is applied to recycled uranium, they set certain default values, default levels, on a parts-per-billion uranium mass basis for plutonium, neptunium, and technetium, the three big players, but

principally plutonium being the most important from a dosimetric standpoint. These values I believe were 100 parts per billion for plutonium, 3500 for neptunium, and 9,000 for technetium-99.

SC&A was tasked to review this model and make our observations. In doing so, we discovered that there were a certain types of workers for which we felt maybe have had higher exposure potential for which these default values might not be applicable.

Basically, it came out of a review of the original model, which you will recall was the NIOSH 2008 coworker model. In that was an appendix, B, which had dusthouse collection samples. We found that, for Plant 5, which was the metal reduction plant, and also for Plant 1, where a lot of the material was milled, there were significantly higher values in these integrated samples than the NIOSH default.

So, we started looking into this,

and we looked at the source documentation which comprised these DOE mass-balance reports that were put out around the turn of the millennium, right around the 2000-2001 timeframe.

We started looking at this mass-balance report and all this data that were collected, and how NIOSH had developed their values. We came back with some criticisms in our second White Paper review, which was produced about this time last year.

Basically, we felt that, because of chemical concentration processes that occurred during the metal reduction process and the magnesium fluoride pot liner, reduction pot liners, which was subsequently reused, and the fact that this material was recycled back through Plant 1 to be remilled, there was an elevated exposure potential for that group of workers. And we were able to see that that process differentiation and the potential actually reflected in the samples

that were collected for the timeframes of interest.

NIOSH came back with a revised coworker model based on our findings in our second report. What they proposed to do at that point was to really look at three different time periods. These time periods are very important in terms of the potential exposure to various workers in the plants.

To the best of our ability to discern it, recycled uranium first was delivered to the plant in 1953. However, it wasn't processed in the process stream until 1961.

During that interim period, I believe there were about 45 metric tons which were received, I think, from 1958 to 1960-1961 timeframe. Before that, there were a couple of drums onsite, but it was really very low amounts. And also, the concentrations of these constituent radionuclides were quite low in this initial amount of material.

From 1961, material was put into the process stream where it was converted to oxide, fluorinated and then reduced to metal, and fabricated into various shapes.

From about 1961 until the early 1970s, about 1972, the materials that came in, principally from Hanford, were fairly low concentrations. I believe the plutonium was typically less than 10 parts per billion, which was kind of an agreed-upon value for production quality control purposes at Hanford.

And so, you had this concentration process using the magnesium fluoride in metal reduction that caused an elevation in this concentration, up to about a factor of four to ten, based on later data which we were looking at, which gave a better picture of what the real concentrations might have been.

In 1973, the Fernald site began receiving shipments of these highly contaminated materials, mainly tower ash and

incinerator ash from the gaseous diffusion plants. These materials were shipped in in several different batches.

It wasn't really until 1980 that you got probably the most pivotal change in the environment for recycled uranium exposures, and plutonium exposures, in particular. This was when, in June of 1980, the plant received 16 hoppers containing about 22.5 metric tons of recycled uranium that was very highly contaminated and consisted of tower ash materials that ranged anywhere from about 100, I think it was 67 parts per billion up to about 7500 parts per billion, with an average value of 1125, I believe.

So, this introduced, basically, about 25 grams of plutonium into the Fernald site, essentially doubling the inventory for the entire lifespan of the site. So, it was really a sea change in contamination control requirements that should have been put in place at that point.

And so, what happened was that, when that material came in, it was stored for a period of about two years. After that time, these hoppers were taken and repackaged material was taken out of the hoppers and repackaged into these 55-gallon drums to facilitate semi-remote handling in the various process operations for which it would be used later on.

During this time, we have a certain amount of data which tells us approximately how many shifts it took to repackage this material, some information on the workers who were involved, number of workers, like I said, the shifts. And so, we have some information that gives you an idea of how long it took to handle this material.

But let me back up just a little bit. I was kind of getting offbase here.

To get back to the actual exposure potential during this timeframe, the mass-balance reports show that this magnesium

fluoride had very high concentrations. It was about the second highest of all the source materials, except for this Type 10A material.

In looking at the source data, we were able to determine that these 400 samples of mag fluoride were actually, indeed, from Fernald from the various process steps that we were concerned with in metals reduction. And NIOSH's approach is to take these datasets that comprise this highly contaminated material, use a log-normal fit to the datasets, and then pick off the 95th percentile of that to get an upper-bound estimate of what these people could have possibly been exposed to.

And it turns out that, for the magnesium fluoride workers, that is a very appropriate dataset to use. It is a good dataset. That 400 parts per billion is fairly close to what we saw in some of the other samples, which, incidentally, are part of that dataset, were represented by the upper end,

the ones that we were concerned with initially.

And so, we think that, to make a long story not quite so long, that that particular set of data and that approach from the 1973 period on, when this most highly contaminated material was handled, is probably adequate to bound the most highly exposed group of workers, that being these Plant 5 metal reduction workers. These were the guys, not only did they have these metal reduction pots, this magnesium fluoride that was concentrated in this material, it was also one of the dustiest operations. So, they had the highest exposures to dust, and that dust also consisted of some of the highest concentrations of these constituents. So, we are pretty confident that that particular subgroup of workers was, indeed, bounded.

Our concern that we voiced in the August 2011, last summer, in the meeting there, it was not about that particular group.

We were concerned about this other group of workers who can't be identified based on the records. These would be these people who might have handled the material on the front end, these guys who repackaged the materials, and then were involved in these down-blending steps. So, anybody who was handling this highly contaminated material before it had been down-blended with uncontaminated materials to achieve a particular goal in terms of contamination level.

And so, at that particular meeting, NIOSH was tasked to attempt to quantify the timeframe that might have been involved in actually handling this material. The way they went about that was to look at that data that I had described earlier for the repackaging operation. This was for five of the most highly contaminated hoppers of material.

There is a table in the reference which we have. Let's see if we can take a

look at this paper. It is called SC&A's Response to NIOSH's Subgroup 10A Impact Analysis, dated November 1, 2011.

Go ahead and turn to Table 2 on page 8 of 13. This is the recycled feed material. This shows the 16 hoppers, the mass of uranium in kilograms for each of the hoppers, the concentration of plutonium on a uranium mass basis and, also, on a sample basis. So, you can see the broad distribution in that set of data.

If you move on to the next page, on page 9, you have the repackaging data. This came from this 1985 report, officially a four-page report, kind of an after-action report on what happened during this repackaging operation.

You can see here they identify the hoppers, the shifts, and the dates during which these operations took place, the plutonium mass -- the kilogram mass I have gone ahead and added in for each of these --

and then, the number of shifts.

You can see there was a total of about almost 12,000 kilograms of material processed over the course of this period from April 19th to May 7th. Nineteen shifts were required.

And so, what NIOSH did was they said, okay, we know that this was probably the most problematic aspect of handling this material, was repackaging it, taking it out of these hoppers. Several problems were encountered during the repackaging operations.

So, they felt that, by looking at this particular set of data, it would provide a bounding time estimate on any subsequent steps. Because there really are no data that indicate what times were involved in, say, taking these barrels of material and down-blending, say, in Plant 8 with other uncontaminated materials. I believe they used calcium uranate on some cake in Plant 8, and there were also other applications of blending

that took place, which were kind of semi-remotely controlled and remotely handled.

And so, this hands-on set of data during the problematic timeframe, then, provided a time bound for the amount of time any given worker might have been exposed to this material.

Based on some, I believe, expert judgment on the part of the health physicists who were interviewed, they came up with an idea of about, or an estimate of about 8 percent of the time over the course of a year where a given worker could have been exposed to this material, if, indeed, they were involved in handling it full-time during that year.

This was based on them handling only the hoppers that were measured at greater than 400 parts per billion and, also, assuming a five-hour shift to provide some worker protection for the respiratory protection and other types of protective gear these guys were

wearing. They figured, instead of an eight-hour shift, we can go ahead and just give them a reduction factor to a five-hour shift.

And so, we looked at that and we said that seems fairly reasonable. We don't necessarily agree with all of the assumptions that were made. So, we went ahead and did our own analysis, just assuming some slightly higher parameter values. We assumed we are just going to look at all this material. Let's look at all 16 hoppers, assume an eight-hour shift without any protective values whatsoever.

And we looked at all the material that was processed through during those five hoppers and came up with about 675 kilograms per shift. Based on that, we figured, to process all the material, it would take about 36 shifts. Eight hours per shift, you get about 288 hours or, roughly, about 14 percent of a year's hours, if a given worker were, indeed, involved in this process during the

entire period of time.

To get a handle on what that person might have been exposed to, what we did was a weighted average concentration. We assumed a baseline of 100 parts per billion because we realized that the people who were doing this were probably not also Plant 5 metal workers or millwrights in Plant 1. And so, they wouldn't be necessarily exposed to 400 parts per billion continuously. In fact, they were probably exposed to less than 100 during these times when they weren't handling this material.

And then, we gave them the full average value, the 1122, for the 288 hours where they were handling material. Then, doing a weighted-average, it came up to a value of about 240 parts per billion during that 14 percent of the year when they were handling the materials.

So, based on what we felt were some pretty conservative claimant-favorable

assumptions, we agreed with NIOSH that the 400 parts per billion is, indeed, likely bounding for this group of workers, in addition to all of the workers for that period of time.

So, in summary, I could say that we feel that we have come to a consensus on this, and we feel that you could probably move this particular issue over to the Site Profile discussions to the extent that these discussions need to continue.

And so, that is really all I have to say about recycled uranium as it stands at this point.

CHAIR CLAWSON: So, let me understand, and maybe this is for you, Mark. What we are looking at is a tiered step to be able to, when we do this dose -- I just want to make sure that I am clear that in the earlier years we are going to do, we will do the 100 parts per billion?

MR. ROLFES: Yes, for the years that particular uranium was processed at

Fernald, we are going to add in 800 parts per billion of plutonium on a uranium mass basis, as well as additional intake of 3500 parts per billion and 9,000 parts per billion of neptunium-237 -- excuse me -- 9,000 was neptunium-237; the 3500 is -- I got that backwards. Thirty-five hundred parts per billion of neptunium-237 and 9,000 parts per billion of technetium-99.

Then, beginning in, I believe it was 1976, I think was the date -- I will have to take a look back.

MEMBER ZIEMER: Seventy-three.

MR. STIVER: Seventy-three, I believe.

MR. ROLFES: Seventy-three, we would default the 400 parts per billion of plutonium on a uranium mass basis. So, we would be adding in the recycled uranium constituents based upon the reconstructed uranium intakes.

MR. STIVER: Assuming it was kind

of a three-tiered, stair-step function really, if we look at the time periods where these different activities took place.

MEMBER ZIEMER: And SC&A's actual value for that 1973-on period was this 242 value, using slightly different starting assumptions.

MR. STIVER: I should probably clarify that. That was basically just to look at this one subgroup of workers who, in 1982, from about 1982 to 1985, could have been involved in down-blending and handling this material on the front end, before it was processed into other materials.

MEMBER ZIEMER: Right.

MR. STIVER: And so, it didn't take this, typically, and just run it right through the process. They tried to blend it down with uncontaminated materials.

MEMBER ZIEMER: But they would still be covered by this?

MR. STIVER: Yes. So, the idea

was that was the only outstanding group that we weren't quite sure that might be covered by 400.

MEMBER ZIEMER: Right.

MR. STIVER: And this analysis demonstrates that they are, indeed, covered.

MEMBER ZIEMER: Right.

CHAIR CLAWSON: Are there any other questions? Do you have any questions, Phil, or are you good with this?

MEMBER SCHOFIELD: I'm good with this.

CHAIR CLAWSON: I will have to admit, I had to have John help me understand the stair steps. So, I have already been through this in detail.

So, if there isn't any other questions, we will accept that and move that to the TBD.

MR. KATZ: So, is the issue actually closed? Is there something to resolve at a TBD level?

MR. STIVER: The only issue at the TBD level might be for this period from 1958 to 1961, and NIOSH proposed zero defaults with that. But, certainly, it is a tractable problem, boundable. We had reservations about zero default for that period, just on the basis of claimant-favorability in the dose reconstruction process, although we realize they are very low levels. There could have been people handling that material that could have gotten some exposure. But we are perfectly fine with 1961 to 1989.

MR. ROLFES: Yes, that is something, I mean, we would be interested in hearing what the Work Group's opinion is.

CHAIR CLAWSON: Right, but what we have proved is that we are able to bound it, and so forth. But the earlier years, we still have to -- so, that will be in the TBD.

So, with that said, and you got a drink of water, now you can go on to thorium.

(Laughter.)

MR. STIVER: I think I need another drink of water before that.

MEMBER ZIEMER: So, we consider this issue closed then.

CHAIR CLAWSON: It is closed and moved to the TBD. There is still one small portion that we have got a discrepancy on, but I think that we will be able to bound that. It is just in the earlier years where NIOSH claimed zero, but there was product there. I think we can come to a resolution on that, though.

It's yours.

MR. STIVER: Okay. The next issue is Issue 6b. This is the use of chest counts to reconstruct thorium-232 exposures in the post-1968 timeframe.

I will just kind of back off a little bit and talk about thorium in general. From 1953, when they first started receiving thorium onsite, until 1967, they really didn't have any bioassay thorium exposure. What they

did have were these daily weighted average air concentrations, which were really essentially based on, for the most part, the breathing zone air samples that were taken for workers at different times throughout their workday, the different operations that would be performed.

And then, weighted this concentration by the time it took to perform any given task. They came up with what they called a daily weighted exposure or daily weighted average. This approach had been remarkably consistent from the early 1940s all the way up through 1967.

A huge amount of data is available for different plants and different years and for different categories of workers. And so, we feel that that dataset is actually pretty -- the first part of our thorium discussions really focused on these DWEs.

We were concerned initially because these measurements were never taken

for dose calculations for determining body burden. They were really more of an industrial hygiene sampling process to improve the working conditions.

And so, as a result of that, they never do any uncertainty analysis on this. So, we have lots of numbers. We realize that these are snapshots in time, and that they may not represent the full range, the full distribution -- exposure that any given worker could have experienced during the course of his day.

So, Dan Strom up at PNNL, back in 2008, and Adam Davis came up with an uncertainty analysis that looked exactly at this particular issue. They looked at about six different plants from the period 1948 to 1955. They did some fairly sophisticated statistical analysis and came up with a robust uncertainty analysis to be applied to this site.

Over the course of our

discussions, we came to a consensus with NIOSH that, yes, the DWE data can be used in conjunction with this uncertainty analysis to bound workers for plants, various plants and various years, throughout that period of time.

In 1968, Fernald went away from doing the DWEs. They went from that to doing chest counts for thorium and also for uranium. But they still maintained uranium bioassay, but they also had these supplemental data, this chest count data.

And for this, they used what they called the mobile in vivo radiation monitoring laboratory. They would bring this in at various times throughout the year, and they'd collect the workers and they would run them through.

I believe they used an array of sodium iodide detectors. They would measure, I believe, thorium-234 to get a handle on uranium concentrations.

They also had the capacity to

measure thorium, thorium-232. Now they couldn't measure it directly, obviously, because a lung burden of thorium-232 is not going to emit any detectable levels of radiation outside the body. But thorium-232 has a very long decay chain associated with it. Several of those species are fairly high-energy gamma emitters.

And so, what they would do is they would measure the regions of interest that corresponded to two of these daughter products, actinium-228 and lead-212. This was really the basis for this system. From that, you could get an idea of the age of the source and back-calculate to the thorium-232 intake.

However, there are some real problems with this technique. This is really where we have some issues with the approach that NIOSH has taken.

I want to say upfront that we certainly are not casting aspersions at the personnel who were conducting these

measurements or the DOE scientists who developed this method. We think they were fully aware of this, and they were fully aware of the limitations of this counting system at the time.

But, be that as it may, we still have considerable issues that we feel need to be redressed before this data can be used to accurately and sufficiently bound intakes during a certain period of time.

But let me back up again. I am kind of getting ahead.

I wanted to say that, from 1968 to 1978, this data from the mobile laboratory was reported in units of milligrams thorium only. There was kind of an overlap period in 1978. I think there might even be some in 1977. We have milligrams thorium, and they also have, beyond that, they had data reported in nanocuries of the two isotopes, actinium-228 and lead-212.

And so, from 1978 on, you have

this hook. You have this ability to get back to what the thorium source term age might have been.

And the reason that is important is because, when thorium was separated from the ore, it is essential broke in the decay chain. What you are left with are two isotopes of thorium. You have 232 and you have 228. Thorium-228 decays away with about a 1.9-year half-life. And so, it is an unsupported progeny, and it starts to drop off fairly quickly.

Lead-212 is one of the daughter products of thorium-228. Well, at the same time, the thorium-232 progeny are building in. They are building in at the half-life rate of radium-228, which is 5.75 years.

So, at the time that the 228 is dropping off, you have got this buildup of the daughter products, and the short-lived daughters -- excuse me. Let me back up.

Radium-228 decays to actinium-228,

then to thorium-228. And so, then, you have the situation where the progeny from thorium-228 are all short-lived and they build into equilibrium fairly quickly.

And so, you have a buildup based on the radium half-life, radium-228 half-life. You have a dropoff of the thorium-228 that was in the sample to begin with. And so, if you are trying to measure these short-lived or these gamma-emitting progeny, you have to find out where on that decay curve you are relative to the initial separation time, in order to back-calculate to what the thorium intake could have been, based on that measurement.

From 1968 to 1978, we don't have that source data available. We don't know which isotope was measured, whether it was actinium or lead. We don't know what effort might have been made in order to calculate the value in milligrams of thorium.

If you are looking at --

MR. KATZ: One second.

Someone on the line is not muted and we are hearing you are moving something around near your microphone or near your phone, and it is really distracting. So, everyone on the phone, would you please mute your phone? Use *6 if you don't have a mute button. Thank you.

Sorry, John.

MR. STIVER: So, actually, we can kind of group our concerns into three levels:

The first is these uncertainties. Let me say, right now, we are just looking at this period from 1968 to 1978. The data are reported in milligrams thorium.

We have concerns related to the inherent uncertainties in trying to get back to thorium-232 based on these progeny measurements. Especially considering that we don't have the source data, you are either forced to accept this value, just accept it at face value, or try to do some kind of analysis to see whether it makes any sense that those

are reasonable intakes to have.

So, there is the problem of which isotope was measured, when it was measured, when the intakes took place, over what period of time prior to the measurement. All these things kind of come together in a very complex way to generate these enormous uncertainties in what this measurement could have been.

In addition to that, we have translocation issues once the material is actually in the lung. Thorium, which is typically Type M, forms complex iron very quickly, and, basically, is retained in the lung; whereas, the progeny are much more mobile and can move out in systemic circulation or away from the source of the intake in the chest. So, we have that problem as well. We may not be measuring all of the daughter products in the location where we presume them to be.

Another issue has to do with the limitations of the counting system itself.

Looking back at some of the historic documentation, one particular reference that comes to mind is this paper by Hap West that was put out in 1965. They used a similar system, used the same basic pathology. It wasn't mobile, but Y-12 had the same type approach.

What they did was they devised what essentially boils down to a triage-type measurement to determine whether a person did or did not have a thorium intake. They caveat this and very distinctly describe that, in order for this type of measurement to be quantitative, multiple measurements have to be taken to ascertain the age of this source. Either that or you have to have the process knowledge on hand. It is a fairly distinct process these people are being exposed to.

You have to talk to the product engineer or the process engineer or the health physics staff to get an idea of what the age of the source was. If you don't have that,

you don't have that hook back to some reasonable measure.

In addition, the system had a very high detection. It is being reported as being 6 milligrams of thorium. This has been kind of a point of contention with SC&A or really a point of discussion -- it is not contention really -- between us and NIOSH as to just what does this really mean if you have such a high MDA.

We only have about 3 percent of data above the MDA, and the rest, basically -- I will point out to NIOSH's paper here. This is called, Response to SC&A Response to NIOSH White Paper on FMPC MIVRML Calibration, by Bob Morris and Bill Smith and Tom LaBone.

Beginning, let's see, on page -- where is it here? -- on page 4, there is a series of normal probability plots here. What they are showing is two lines represent 6-milligram MDA in the 95th percentile. These basically show that the data below the

detection limit are pretty much normally distributed. This is what you would expect from 1978 data.

You are basically looking at some signal, but mixed in with a lot of electronic background noise. So, it can be a null distribution, I mean if you are really looking at zero analyte or it could just be a limitation of the detection system, the detector's ability to actually measure a dosimetrically significant quantity of material, which is what we believe we have here.

When you look above the 95th percentile, you see there's a sampling, in this particular case for 1968, you see there's about 14 or 15 values that clearly are up above the line. In our opinion, this represents real exposures, but in a categorical sense. Either they are or they are not.

Due to all the uncertainties in

the milligrams-of-thorium data, and also the replicate measurements that NIOSH shows in their graph No. 12, you can see that the high values, when there's multiple measurements here, you can see the two. This is on page 15 of 27. You see the high measurements, those two values. You have got values of 17 and 2.3 for multiple measurements over a short period of time. So, we have got tremendous uncertainties associated with measurements on a given individual over a given short period of time.

And so, this kind of dovetails with what we have been able to ascertain from reading the historic documentation. You know, this is a system that was acknowledged to be kind of a triage-type system. It could be used in that regard or it could be made to be quantitative, given the right precaution and the right careful measurements and replicates that were needed in order to do that.

And so, what we did after our last

meeting is we tried to look for evidence of people who had, workers who had lung burdens that were higher than 6 milligrams to see, were there follow-up measurements made, and was there some attempt to really get a better idea of what the intake might have been?

I don't remember the exact numbers offhand. I think we looked at about 70 individuals. Of those --

MR. BARTON: Well, 50 individuals and 70 samples.

MR. STIVER: Yes, 50 individuals, 70 samples. Of those, I believe none of them had a follow-up sample in six months.

Also, we were able to pick up about 15 or 20 claimant files which had the same type of characteristics. These were high-measured lung burden. We looked for evidence if there was any kind of a follow-on measurement or some attempt to determine lung burden.

In every case, what we saw was

there was no attempt -- the thorium values were reported. They weren't significant; there were no calculations to go along with them. However, the uranium values in all those cases were adjusted to try to calculate a percent of body burden for U-235.

This kind of gets us off into this area of, instead of adequacy, the completeness paper where we kind of looked at that sort of thing. And so, we come back to this point where it is getting to be pretty clear that the system was really in place to measure uranium. Thorium was kind of ancillary.

When there was an attempt to look at it, it was the very first year of operation, I believe, in 1968, where they tried to get together a group of thorium workers and measure them. We will talk about that in a little bit. I will let Bob kind of take that discussion since he basically headed it up.

But, getting back to the adequacy

issue, we have this situation where you have very high uncertainties going into these measurements. We have no source data. You have a system that is very insensitive to the levels that are of dosimetric significance.

If you take a look at our report, SC&A's Final Position on Thorium-232 In Vivo Data Quality and Adequacy for FMPC Workers, -- that is a mouthful -- if you take a look at our report, starting on page 4, what we did, what Joyce Lipzstein did, was to take a look at what potential intakes and doses would you get from a 6-milligram lung burden under different exposure scenario positions.

We have, basically, a set of different scenarios, the first one being worker exposed for 30 days to thorium Type M. In scenario two, they are exposed for 90 days, and so forth, up to 180 days -- or excuse me -- up to the full year. There are four different scenarios.

The important thing to take away

from this is, depending on when the intakes take place relative to the monitoring period, you get big, big doses, big organ doses, bone surface doses that range from 1.3 sieverts up to almost 10 sieverts, a sievert being 100 rem. So, we are talking really big doses. And lung doses are also quite high, 10 rem up to about 80 rem.

And so, the fact that you are looking at sub-MDL data doesn't mean that you are looking at actual background levels of lung burden. You have got dosimetrically highly significant data that the system is just incapable of measuring.

I believe Joyce did some research on the background levels of thorium in the lung, and it is on the order of about 3 or 4 micrograms, which is about three orders of magnitude less than the situation here that we are looking at.

The final thing we looked at was, during this period of overlap, 1977 to 1978 --

Bob went ahead and copied off this Table 1 that we have back here. It is under Section 3 of our report on page 10.

This is where --

MR. BARTON: It is in the actual report.

MR. STIVER: This is actually in the report. In case you don't have that available --

MR. BARTON: Does anybody not have the report?

MEMBER ZIEMER: What page is it on?

CHAIR CLAWSON: Page 10.

MR. STIVER: It's on page 10.

MEMBER ZIEMER: Okay.

MR. STIVER: This is on page 10. Take a look at the data, the reported thorium results in milligrams. Look at those top three values. You have got 2.2, 4.3, and 5.10 milligrams of thorium.

The next column, column two, is

the reported lead-212 activity in nanocuries. Those top three values have negative lead-212 activity measurements.

If you look at the range of these activities for lead-212 and actinium-228 relative to the ranked thorium data, you see they are all over the map. And so, this really causes us concern because NIOSH kind of has this implicit assumption in their analysis that it was lead-212 that was measured, and that if lead-212 is the analyte being measured, why, you can certainly bound the disequilibrium ratio to about .42 or so, depending on whether you have a closed system and how many purification cycles the materials has gone through. But it becomes a tractable problem when you have those measurements.

Here this is evidence that we don't see that. We don't see any evidence that that was, indeed, the measurement.

We also have problems with the biokinetic realism of some of these

measurements. There are a couple of instances where we have values that are 10 and above, 10 milligrams or more. And then, a measurement a couple of months later is down around .02, .03, when if you look at the clearance of thorium compounds that were present, you would expect maybe a 30 percent drop, from 10 down to 6 or 7 milligrams. And so, we are not seeing that.

And again, compare that back to the graph 12 in the NIOSH report. You see that you have got incredibly inconsistent, highly variable and highly uncertain data during this period of time. For that reason, we believe that this remains an open SEC issue.

For the period 1978 to 1988, the data are actually reported in nanocuries of lead and actinium. And so, that source data is available. So, we believe the source measurements are available. We have not yet seen that data. But if it is, indeed,

available to review, we believe that during that time period that the intakes can probably be measured with reasonable accuracy. And so, we feel that, as far as an SEC is concerned, the real period of concern now is 1968 to 1978.

Now let me back up just a minute. From 1978 to 1988, we are not saying that it can definitely be a calculated boundary. We just say we kind of put it in the parking lot while we looked at this other time period which we felt was much more significant.

So, while we feel that there is a much better likelihood that that later dataset can be used to do reconstructions, we haven't actually looked at the data in any kind of in-depth manner to determine that.

We also noticed that in the latest files that Mark posted today, there is a slideshow in there that shows, basically, I believe you have three different scenarios, separation times, up to three separations, and

how that would affect the lead-212 disequilibrium ratio.

There are also some calculations I believe looking at intake retention fractions for shared versus independent kinetics. I guess that gets back to some of the concerns we had regarding translocation. But that is, again, something that we would have to look at in greater depth.

That is really what I have to say about that.

Joyce, is there anything you would like to add? Anything I missed or got wrong that you would like to clarify?

MR. KATZ: Before you go on, just can I clarify a date? Because you said earlier 1968 through 1977, and then just now 1968 --

MR. STIVER: Oh, 1968 to 1978. There is an overlap period.

MR. KATZ: Ending at the end of 1977 or ending at the end of 1978?

MR. STIVER: Basically, any data that relies on milligrams of thorium, which there was an overlap period in 1977, mainly in 1978, but there is a little bit in 1977. So, it would be up through the end of 1977.

MR. KATZ: Okay. Thanks.

DR. LIPZSTEIN: You asked me if I have something to add. I think I can't see what the results of milligrams of thorium really means. Depending on the scenario, we made calculations that they can give a very high dose to the organs.

Even, for example, if you take data that is below the detection limit of 6 milligrams that were reported by NIOSH as being non-exposed people, 1 milligram, for example, can give, depending on the scenario, can give a dose to the bone surface higher than 1 sievert, which is a very high dose.

The 1 milligram, you know, it is impossible to be background because background volume is around 2 micrograms. I have many

experiences in measuring people that are exposed to thorium. If you really see some actinium or some lead, that person is highly exposed.

Then, if you look, as John has pointed out, if you look at Table 1 and you look for the people that had results reported in milligrams of thorium and in nanocuries of lead-212 and in nanocuries of actinium-228, you can't see any correlations between the milligram thorium results, the reported lead in nanocuries, or the reported actinium-228 in nanocuries.

You have, as John pointed out, the 4.3-milligram result, which was done in 1971, which is in the period of time we are looking at. It has a minus .04 lead-212 result. And then, the 2.2 has negative results for lead-212 and for actinium-228. And then, you have the same result with positive actinium and lead-212.

So, there is no relation. We

don't know which nuclide they were measuring and what these milligrams really mean. The problem is that, if it has a real significance, it would give them very, very high doses. So, I think those results don't have any significance. I don't know what they mean.

And also, John has already pointed out, also, that we took some results that had follow-up. We had, for example, I think, 25 milligrams of thorium lung burden result that were taken in March, and then in July it dropped to .03 milligrams, when you would expect in July 8 milligrams. So, we don't know.

I think the result in milligrams doesn't have any meaning that we know. We don't know. So, I think we have a bunch of numbers, a big bunch of numbers, that don't mean anything.

What we wanted to say is, also, that I don't know if people at Fernald took

these results as meaningful results in terms of looking at the workers' exposures because, even for the high milligrams result, there was no follow-up. So, we would expect if someone had a very high chest result, that would mean a dose much higher than sieverts, higher than 10 sieverts, might be implied in this. Then, they would have a follow-up to see what this really means, but you don't see it. Instead, it is just see they are calculating what this means in terms of maximum permissible result for uranium, not thorium.

So, that's it. I do think that those numbers, we don't understand them. We don't know what they really mean.

MR. ROLFES: This is Mark Rolfes.

John and Joyce both covered a lot of various different topics about the uncertainties of thorium lung counting. Rather than trying to address each one of those, I would prefer to come back one at a time, so that we can provide our most recent

response to each of these issues that have been presented and each of these concerns.

We just recently received these new concerns. We have responded to many of these previously in earlier White Papers. Some of these we just disagree with SC&A on, and others we share the same findings, I guess, for example, or the same concerns. But most of those are related to the uncertainties associated with measurement.

It is NIOSH's opinion that we can provide a claimant-favorable method to interpret those uncertainties to give the benefit of the doubt in workers' dose reconstructions.

Regarding the high doses for thorium, you know, the dose to a given organ is all going to depend upon the solubility, the amount of exposure to thorium, the distance in time between the exposure and the measurement, the biokinetics of the body, and various biological systems.

If you take a look at bone doses and lung doses, for example, those are going to be two of the higher-exposed organs for thorium, while other systemic organs are going to have doses on the orders of magnitude much lower than the reported bone surfaces.

The bone surfaces are a very, very small, thin layer of active dividing tissue. Because the active dividing tissue is so, so small, there is a lot of energy deposited in that tissue. That is why the doses are so high. This all depends, though, upon the solubilities of the thorium.

We have also worked on developing some new intake-retention fractions. I am jumping around, but I am trying to give you some updates as to what we have done, and then I would like to go back to discussing one issue at a time, to hopefully state where we share the same opinion or where we have our differences.

Before we get to that, though, we

prepared a small presentation just to provide a written summarization of what we have tried to do in the past couple of weeks since we have received SC&A's reports. This is just a draft update presentation. It is not our formal response yet. We tried to prepare something, so that we had something to discuss at this meeting.

This is something that I sent out. It is a PowerPoint presentation. I will just briefly go through some of these points in here.

This was the NIOSH position on FMPC local in vivo radiation monitoring laboratory thorium chest counts. We went back and had NIOSH conduct a review of all the White Papers and exchanges. We had Don Beal go back to review the NIOSH White Papers. And then, he endorsed the current positions on the issues. Don Beal has experience in lung counting at Pacific Northwest National Laboratory.

We also had Tom LaBone go back and revisit worst-case assumptions regarding the chemical separations and the intake retention fractions for thorium progeny. He has produced a new White Paper. I don't have the final White Paper yet. It is still in review in DOE as well as in DCAS. As soon as that is developed and our comments have been incorporated, that will be sent out to the Work Group.

It is our opinion, NIOSH's opinion right now, that plausible bounding dose calculations are feasible, and we have demonstrated them.

I have a couple of graphs in here of the thorium decay chain and, also, the activity of natural thorium following chemical removal of the impurities or the daughter progeny.

Don Beal's review confirmed previous positions that there is a wealth of good information and papers on the subject --

MR. KATZ: Excuse me. Someone on the line, would you please mute your phone? Someone on the line, we can hear people talking in the background. Thank you.

No, we still hear it.

Joyce, is your phone muted? Is that coming from you?

DR. LIPZSTEIN: No, it is not coming from mine, no. There is nobody here.

(Laughter.)

MR. KATZ: Okay. Thank you. Thank you. You were the only one who was talking, so you were the only one I would -- but someone has joined the call, perhaps just joined the call and has not muted your phone.

Please mute your phone. Press *6 to mute your phone.

CHAIR CLAWSON: There we go.

MR. KATZ: No, I still hear it. I heard someone say sorry in the background, for example. That phone is not muted.

Someone is on the call. They have

not muted their phone. Would you please mute your phone or hang up, either way. But we can't proceed with all this noise.

I can hear shuffling of paper. Please, everyone on the phone, please mute your phone. Press *6.

Okay. Thank you.

Proceed.

MR. ROLFES: So, the most recent review that was conducted by Don Beal, he felt that we have appropriate bioassay and that we can make bounding assumptions to reconstruct thorium intakes.

NIOSH has produced a demonstration of the dose calculation method. A new White Paper by Tom LaBone on the calculation of chronic intake retention fractions for thorium-232, assuming shared biokinetics.

Tom has produced some calculations which now show the worst-case scenario disequilibrium, which is a result of three sequential time separations. The timing of

which -- the separations were conducted -- was done to generate the worst-case disequilibrium of thorium and progeny. So, this we feel will address the thorium mass-to-activity conversion issue.

MEMBER ZIEMER: Excuse me. Is that something that has occurred since the SC&A critique?

MR. ROLFES: Yes, it is.

MEMBER ZIEMER: They have not seen that?

MR. ROLFES: That's correct.

MEMBER ZIEMER: Got you.

MR. ROLFES: This was just recently drafted. It is dated February -- well, I have seen bits and pieces of it from February 2nd, and I think I have, hopefully, a close to final --

MEMBER ZIEMER: We have not seen that?

MR. ROLFES: No.

MEMBER ZIEMER: Thank you.

MR. ROLFES: No, it has not yet been shared with the Advisory Board because it is still --

MEMBER ZIEMER: I have so many things, I am not sure what I have seen.

(Laughter.)

MR. ROLFES: We have done our best to respond.

MEMBER ZIEMER: No, I appreciate that. Thanks.

MR. ROLFES: But, then, I mean, we have tried to have a week or two turnaround from the time we received some of this information.

MEMBER ZIEMER: Right.

MR. ROLFES: So, we have scrambled to put as much as we could together in a short amount of time, so that we could basically have some responses to the most recent review.

So, NIOSH's opinion is that we can bound thorium intakes based upon the mobile in vivo radiation monitoring laboratory data. We

have ample measurement data, and we can make bounding assumptions which incorporate all the various uncertainties.

Our coworker model pools data, and it makes the worst-case scenario intakes unlikely. Correction factors can be applied to each worker from this coworker model. The in vivo coworker model will be modified to incorporate these worst-case correction factors when the TBD is revised.

The items that remain unresolved with Item 6b on thorium-232 lung counts by SC&A: we have six bullets, I think, that were presented to the Advisory Board at the December 2011 Board meeting in Tampa.

NIOSH and SC&A agree that appropriate bioassay samples were taken. The SC&A issue is with the ORAU team interpretation or the NIOSH team interpretation of those data.

NIOSH has completed a series of calculations which allow conservative bounding

estimates of the lung burdens to be made from the lead-212 in vivo measurements. These calculations account for the disequilibrium created by multiple chemical separations up to three, and we have developed intake retention fractions for each of these various scenarios.

These calculations adjust the thorium mass results to account for new, independent biokinetics of thorium and its progeny, and that the disequilibrium factors caused by the chemical separations that could have occurred during the processing of thorium at Fernald. This will change the original TBD disequilibrium factor of .42.

In summary, these worst-case bounding scenarios for thorium exposures, worker exposures to thorium-232 are extremely conservative and unlikely. In a worst-case scenario, a single lung burden measurement would be no more than 5.25 times the value determined by the protocols set forth in our Technical Basis Document. We feel that

bounding estimates can be made for potential thorium intakes, and that this is primarily a TBD issue related to the interpretation of the data we have available.

We have included an intake retention fraction summary chart showing that, in order to get the worst-case disequilibrium of thorium-232 and progeny, one would have to complete chemical separations at a time interval of 4.5 years, a second chemical separation at 7.1 years, and then another chemical separation at 8.8 years in order to come up with a worst-case scenario factor of five.

Now whether this was actually done at Fernald, whether they had a schedule that separated thorium three times at these particular intervals, I highly doubt that it occurred, but this is the worst-case hypothetical, basically, a bounding correction factor as to what the worst-case disequilibrium could be.

This would sort of be applied for one point in time, for one thorium lung burden, one measurement. But if you have more data, more thorium lung counts, the likelihood of encountering that worst-case scenario each and every time that person has lung count is impossible, essentially. So, this is a hypothetical upper-bound, worst-case correction factor of five, and, in reality, it is likely much lower than that.

It is very improbable that all workers could have chronically been exposed to the worst-case scenario thorium progeny distribution. Intakes that possibly occurred in reality would have been comprised of thorium progeny distributions with a correction factor much less than the 5.25 bounding factor, due to the more realistic assumptions regarding thorium processing timelines.

The more measurements that exist that were given an individual or a group, the

greater the precision and confidence of the total set of lung burden measurements and the smaller the chance of underestimating a worker's thorium intake. And for that reason, we previously committed to a worker who has some thorium lung counts, rather than using their individual thorium lung counts off the bat, we would default to the 50th percentile intake for thorium, unless that individual's own data resulted in a higher internal dose. So, right off the bat, anyone and everyone with thorium lung count data would receive the 50th percentile intake.

That is our summary, and we can go through the specific issues, if you would like.

MR. STIVER: This is John Stiver.

Speaking for SC&A, we have kind of laid out what we feel are the big issues here. Without having a chance to really read this paper, I don't think we would really be in a position to comment on it at this point.

DR. LIPZSTEIN: John, may I put something?

MR. STIVER: Certainly, of course.

DR. LIPZSTEIN: Okay. If you go to slide 9 of the presentation, it says on the first bullet that NIOSH has completed a series of calculations which are low, conservative, bounding estimates of the lung burdens to be made from the lead-212 measurements.

We, SC&A, agree with it. I think that we didn't see it in the LaBone paper on the IRF for biokinetics from the daughters of thorium in the lung. But, anyway, all of these are very good for the data that we have on lead-212, which is after 1978. For the period of time between 1968 and 1978, when we have the data on milligrams of thorium, this doesn't help at all because we don't know what this data in milligrams of thorium means.

Again, if you look at our Table 1, it doesn't say anything. You can look at the data on the -- let me go to Table 1 again,

where Table 1 states everything.

Because we can't say what was -- look at Table 1. You have, again, 4.3 milligrams of thorium, and that is a negative lead-212 activity. So, certainly, it was not calculated to lead-212, the 4.3.

Then, you look at a lot of results that were equal to 2.10. And I put on the table the results were taken from, the first one, two, three, four results were taken in the same month from people that were in the same pilot plant. And then, the other results were taken at Plant 4. The five results that had 2.10 milligrams of thorium results were taken from Plant 4 at the same time, in October 1979. And you look at the different results that they have for actinium and the lead, and you can't make a relationship between having the same 2.10-milligram results. And then, you have the three other results with negative lead-212 that we have pointed out. So, what I mean is that we don't

know what the result in milligrams is.

If you look also -- again, I am repeating myself again. It is just to say that those results don't have a meaning in terms of intake. We can't relate it to intake so we can't relate it to dose.

And this is not something that you can just say, oh, these are background numbers. They are not background numbers. They are thousands of times higher than background. Background is on the order of 3 micrograms, and here we are looking at 2 milligrams, at 1 milligram, at 4.3 milligrams, 5.1 milligrams, and we are saying, oh, they are less than the detection limit of 6 milligrams. But I don't know what is this detection limit. I don't know what it is because we have thousands of times higher than the natural background.

And they can imply in very high doses, okay, it is for Type M thorium. But we had exposure to thorium nitrate. We know we

had exposure to thorium nitrate, and it gives very high doses to the bone.

And we didn't make implausible scenarios, no, when we made it. Because the thorium workers, they didn't work for the whole year. We know they have worked for a certain amount of time, and then they were measured sometimes after their exposures.

If you look at them, all the scenarios give very high doses. So, I don't know if those doses are real or not. We just know we don't understand what those numbers mean.

And if they were measured through actinium and not through lead-212, then we would have an uncertainty of more than 100. We can see this by your slide here. In that presentation, you have the slides, one of your first slides, slide 4, and we can see that, if those measurements were done through actinium and not lead-212, we could have 100 times uncertainty here.

So, I don't know. I think these results in milligrams of thorium, it doesn't have any significance in terms of intake or dose, or at least we don't know how to relate them to intake and dose, whatever calculations we do.

MR. ROLFES: Joyce, this is Mark Rolfes.

The magnitude of the dose that you are reporting, to say that these are high doses is sort of subjective because you are identifying, essentially, one of the highest-exposed organs, the bone surfaces. And it is not true for all organs that these doses are so high.

And also, it is reported, you are reporting 50-year committed effective dose equivalence. The dose to the bone --

DR. LIPZSTEIN: For 30 days' exposure. You know, just for 30 days' exposure. Imagine someone that was exposed, you know, many times.

Even if you go to the 1 millisievert, which you see on your graphs on the response to SC&A and the response which replaced this document, you will see that there were many people that were exposed, you know, that have data between 1 milligram and 6 milligrams, and these are sieverts also.

So, I mean, I don't know. And then, you look at the people that had results of lead measurements, actinium measurements, and have milligrams of thorium, and you can't make any sense of how they calculated it.

MR. ROLFES: Joyce, I am trying to address the magnitude of the doses.

DR. LIPZSTEIN: Maybe they didn't calculate it to actinium or lead.

MR. ROLFES: I can't really --

DR. LIPZSTEIN: That is what our Table 1 shows. They didn't calculate it to actinium or lead.

MR. KATZ: Okay, Joyce, let's let Mark respond.

MR. ROLFES: To finish what I had started saying before Joyce -- you are presenting these doses as very high doses. And from a regulatory standpoint nowadays, that may be true. We are not trying to get the best estimate of a person's dose necessarily in this compensation program. We are trying to calculate a claimant-favorable dose. We make a lot of assumptions in doing that, but, ultimately, the dose could be very high.

But, still, to get back to what you are saying, the high doses is a subjective thing. If you are talking a bone surface dose of 10 sieverts over 50 years, that really doesn't amount to much per year. I mean, you are talking, if you have, for example, 100 rem over 50 years, when you divide that 100 by 50, you get down to much more representative doses.

In this program, we are using annual doses. Doses of 10 rem to an organ per

year are not unreasonable. And in fact, you would need doses of that magnitude in order to receive of Probability of Causation greater than 50 percent.

DR. LIPZSTEIN: Can I? Well, this would be doses for someone, for example, that was exposed for 30 days. Imagine that this worker came back and was exposed the other year.

But I am not talking about that. What I wanted to point out is not that. What I wanted to point out is that we don't know what the milligrams results indicate. Because in the paper that was presented to us, the last paper that NIOSH presented to us, it was said that most of the workers had very low intakes, very low doses of thorium, and that they had exposures near the natural exposures. And I am saying that this is not true.

What is reported as very low background, it is not background. It is a thousand times background. A thousand times

background means really high doses.

So, what I mean is that I don't know if this milligrams volume results, what they mean in terms of intake and in terms of dose, because we cannot relate them with the real measurement that was taken.

So, if you look at Table 1 again, I am sorry to be repeating myself, but if you look at Table 1 again, there is no --

MR. STIVER: Correlation.

DR. LIPZSTEIN: -- correlation between lead-212 or actinium-228 and the reported thorium results in milligrams. So, if you used shared semantics, if you used non-shared semantics, if you use daughter, if you use a -- I don't know what conservative assumption about it could lead them to use this. It is impossible to correlate what was seen in the measurements of lead-212 and actinium-212 and correlate it with milligrams of thorium.

So, if they were not measured

through lead-12 or actinium-228, it doesn't mean there weren't assumptions you make. Understand? You don't know what those measurements mean. That is my point.

MR. STIVER: Joyce, this is John Stiver. Could I just step in for just a second?

DR. LIPZSTEIN: Yes, please.

MR. KATZ: Sam?

DR. GLOVER: Just as a matter of perspective, historically, as a chemist, thorium is talked about as 100 percent natural thorium-232. And so, when they talk about milligrams of thorium, that was a natural consequence. I realize we are talking progeny and how to relate that back.

I did want to relate one thing, though. Dirt is about, you know, it is on a micrograms-per-gram basis. Thorium does accumulate, as an hypothesis in autopsy data. And so, it is fairly measurable, even by alpha spectrometry.

And so, it is not a microgram in the lung; it is more on the order of 100. I think, Joyce, probably it is maybe 100 micrograms, several hundred micrograms of thorium are probably present as an adult ages with insoluble thorium.

And so, it is not just a single microgram, a couple of dpm or tenths of a dpm. It does accumulate in your thoracic lymph nodes.

So, just as a point of perspective.

DR. LIPZSTEIN: The 3 micrograms is for an adult. It is the background for people that live in the United States. That was the measurement that was done by Shawki Ibraham and Wrenn, Singh and Wrenn. They made measurements in Washington, D.C., and they made measurements in Denver, Colorado. The range for the adults was between 3 and 6 micrograms of thorium total.

MR. KATZ: Before anyone speaks

anymore again, we have someone on the line who hasn't muted their phone or has come back on the line without muting their phone. Please press *6 to mute your phone.

I'm still hearing, there's conversation. Someone has an open phone line. Please mute your phone. Press *6 to mute your phone.

We are still listening to you. Hello?

Excuse me. There is someone on the line who has an open line. Would you please mute your phone? You are disrupting the call.

I'm sorry. I can call the number and get them disconnected, which I will do.

Try to go ahead.

MR. STIVER: Okay. This is John Stiver.

I just wanted to kind of step back and look at this in the broader perspective rather than debate whether the natural

background is 5 milligrams or 100 milligrams.

The reason we did this comparison was really because, in the NIOSH paper, there was a statement that a high preponderance of sub-MDL measurements really indicated that we were looking at background-level exposures. We kind of believe that that was not quite the entire story because in this situation that we are dealing with, it is a counting system that is just not sensitive enough to measure the intakes that would still result in significant doses.

And so, whether it is sievert level or rem level, the point being these are significant intakes from a dosimetric standpoint. And that is really the point we wanted to make here.

We picked those numbers of bone dose because, you know, thorium is known to be a bone-seeker. That is one of the most significant organs from a dosimetric standpoint.

We could very easily have added a table of other organs, but I think the point being that these are not background-level exposures. These are real exposures.

MR. ROLFES: We agree with that, and that is an important point. We are not saying that the Fernald workers had no exposure. What this translates into, since we have an MDA, a minimum detectable amount of thorium, since this is 6 milligrams, anything that is below that, if we don't have a good feel for what exactly the reported value, if we have a number below 6 milligrams, if it is a non-detectable amount of thorium, we still give credit to the claimant in the dose reconstruction process. We assign half of that minimum detectable amount and use that to assign an intake.

Now, in addition to that, the way coworker models are developed, the less than minimum detectable amount intake -- or excuse me -- lung burdens are also used in the

coworker distribution to calculate a thorium intake.

MR. STIVER: Yes, I understand how you guys do your modeling process. Our real concern is that here we have a situation where what appears to be categorical data, you have some group of exposed personnel who we have shown in most cases can be identified as thorium workers. But there is such large uncertainties, there is such a lack of sensitivity in the measurements, that we can't see much more than you have got. You have got an exposure or you don't.

And once you start getting down into what is left of the detection limit, as table 1 shows, I mean, all these values are sub-MDL. So, it doesn't surprise me that you have got actinium and lead measurements that are all over the map.

Our concern really is that we don't have sufficient accuracy in this dataset for it to be adequate in terms of dose

reconstruction. I guess that is really the bottom line, when you kind of step back and look it from a conceptual standpoint.

I mean, we can certainly argue the details of certain parameter values and whether certain personnel might have been measured during a particular period of time or not. But when you get back to the bottom line, that is really it. We just don't have a sufficient accuracy in this dataset.

I mean, I think it is not because it was the fault of anybody or any technical staff. I just think this particular approach was never really intended to be quantitative analysis.

MR. ROLFES: I disagree with that last statement. I agree with you about what you had said up until that point.

This measurement technique was actually developed to become a quantitative approach to estimate thorium lung burden. SC&A stated in their report that it was more

of a qualitative report, but that is not true. The excerpt actually from the Hap West document that is cited by SC&A, where it said it was to be a qualitative approach, states the exact opposite.

On page 24 of the Hap West report, it says, in-vivo gamma spectrometry is a suitable method for detecting quantitatively certain thorium daughters, on page 24 of that reference from 1965.

And then, on page 27, it says, summary of personnel monitoring considerations. It says, in summary, for personnel, there is presently no developed technique for quantitatively estimating thorium lung burden by analysis. A body counter can be used to make this estimation, but has certain interpretational limitations, as we just described.

It is NIOSH's opinion that we have developed methods to address these uncertainties and these limitations.

Definitely, the way that this program interprets the data that we have available to us, we feel that it is very claimant-favorable to apply these missed doses or doses below the minimum detectable amount of the system that was used. We feel that is claimant-favorable and appropriate for a compensation program.

It is my opinion that the sufficient accuracy definition is more important as to what type of cancer one has, rather than how high the dose values are sometimes. Those are equally important things, the high-dose values and which type of cancer. You can have a 100 rem in some scenarios and have brain cancer, and that 100 rem isn't going to be enough to cause a brain cancer. But, on the other hand, if you have 100 rem and have leukemia, that is typically going to be significant for a sufficient amount of radiation to cause leukemia.

So, I mean, sufficient accuracy is one of those things that is subjective. We

might have one opinion of what might be sufficiently accurate, and someone else might have a different opinion.

MR. STIVER: Sure.

DR. LIPZSTEIN: May I comment again to Table 1? I'm sorry.

MEMBER ZIEMER: Hold off, Joyce. You have had more time than anybody. Let's get some other comments in.

These are minimum detectable activities in the sense that we use it in counting. So, it is not surprising that they don't correlate with other things.

It is somewhat like in a film badge, let's say your minimum dose that you can measure is -- pick a number -- 10 millirem, for example. NIOSH typically says, okay, if it is either a zero or an M or something under the 10, we don't know what it is. It is zero or it is not, and that looks really inaccurate, but it is only inaccurate right down here near your detection limit.

So, you have been typically assigning half of that value because over a period of measurements they are going to probably cluster around that point.

This to me looks similar, if you did that with thorium results that are below 6.

DR. LIPZSTEIN: May I?

MEMBER ZIEMER: The mistake, I think, is trying to correlate that with these other things, which also are hovering around zero. To take any individual ones and say, well, here's 2.10 and .25, that doesn't make sense if you compare it with 4.3 and 0.4. Well, of course not. You are way down here in the noise of the system.

It doesn't matter that it is way above the background level that people have in their lungs. It is the noise of the system that you are looking at.

DR. LIPZSTEIN: No, it's not.

MEMBER ZIEMER: Yes, it is. It is

the noise of the system.

DR. LIPZSTEIN: May I just one second?

If you look at Table 2 again, you have dose numbers, 2.10, right? That would supposedly be below detection limit. But if you look, for example, there is one that has lead-212 of .40 and actinium of .7. That is above the detection limit. The detection limit is .2, .23 I think. So, it is above detection limits, and the result is 2.10, which we are not considering below detection limit. So, we have many results here that are above .23, which would be the detection limit for lead-212 and actinium-228. And although that, we have a result that is 2.10.

So, it is not the question of -- you know, I think we don't understand what they did, how they calculated these results in milligrams.

MR. STIVER: I think, Joyce, you have a good point there. When you look at the

data, there is greater than 6 milligrams that are reported in milligrams of thorium. You also see huge amounts of variability in numbers. And we really don't have any way to get back from those measurements what the real thorium intake may have been.

If you accept this, you are accepting those numbers at face value, the very, very tiny number of them that are actually indicative of any kind of exposure. And even within that, there is huge amounts of variability.

So, you know, this idea of sufficient accuracy, it is subjective. The Board has to balance sufficient accuracy against claimant favorability.

We are in a position where I think we have done what we can do here. We have laid out our understanding and our concern. I haven't heard anything at this point that really causes me to change my mind on this.

MR. ROLFES: When you have a

measurement around the MDA, there is going to be a lot of variability.

MR. STIVER: Even above the MDAs, you will have variability.

(Laughter.)

MR. ROLFES: Sure. I mean, well, keep in mind that the MDA is going to change based on the individual's own body type, the location of counter. So, there is a lot of uncertainties there that you can't just discount and say it is always 6 milligrams.

MR. STIVER: Oh, sure. But, Mark, when you see a value of 10 or 15 milligrams in one reading that six months later is .02, how can you possibly imply from that --

MEMBER ZIEMER: Yes, that is a separate issue, I think.

MR. STIVER: That is a separate issue in a way, but it shows that there is just this huge amount of uncertainty, even at values that are supposedly above the stated MDA.

MR. ROLFES: Let's talk about this, though. We have previously been -- this has been referenced by SC&A previously, the 10 milligram which dropped down to a less-than-detectable value in a matter of, I think, 40 days, was what was previously cited.

DR. LIPZSTEIN: Yes.

MR. ROLFES: We had requested this information from SC&A. We wanted to take a look at this, so that we could investigate this on our own. We haven't received any scenario from SC&A where they have been able to reproduce what they have quoted as the 10 milligrams dropping down to .2 milligrams.

Now keep in mind, though, if we had a 10-milligram measurement followed by a .2-milligram measurement 40 days down the road, we wouldn't treat that .2-milligram value at face value. We would treat it as one-half of the limit of detection of 6. So, we would actually assume that .3 was an order of magnitude of higher, at a value equal to 3

milligrams. So, we would use that value to reconstruct the person's intake.

DR. LIPZSTEIN: Yes, but the 10.2 milligrams, if you make the calculation, you expect something around 6 milligrams. And then, we have another example where we had 25 milligrams thorium lung burden in March 1976, and then in July 1976 it dropped to .03 milligrams, when you would expect 8 milligrams. So, you had from 8 milligrams to .03 milligrams. There is no uncertainty that would say, oh, this is correct.

MR. ROLFES: Joyce, this is Mark.

Once again, if you have a value of 25 milligrams which dropped down below the limit of detection to .02 milligrams, we would use, for that less-than-MDA value of .02, we would actually bump that value up to a 3-milligram value because --

DR. LIPZSTEIN: Yes, but you would expect 8 milligrams.

MR. ROLFES: That would also

depend upon the -- well, excuse me. That would depend upon the solubility of the thorium and several other factors involved in that count.

DR. LIPZSTEIN: Yes, but I am talking about Type M, which is the most soluble you can expect from thorium. If you think it is Type S, it would be a higher value.

DR. GLOVER: Except for contamination.

MR. ROLFES: Or large particle ingestion, are two other scenarios that could play into a more rapid decrease in a measurement.

DR. LIPZSTEIN: No, the .03, it would be below the detection limit. I think we really don't know what those results mean. Probably they are a mixture of measuring radium, thorium instead of measuring something, you know. That is why there is this big drop.

MR. ROLFES: If you have an individual who was just walking out of the job and had just been exposed to thorium, if you took a lung count measurement from him, it is likely going to be higher for that immediate count. You would want to have a little bit of a separation in between the exposure and the measurement.

DR. LIPZSTEIN: Yes, because of the thorium, yes.

MR. ROLFES: In order to get the idea of how much thorium is remaining in the body and delivering the dose. That is the key. If it is cleared fast, it is not delivering dose. If it is remaining in the body, it is delivering dose. And you want to have that separation I time between the exposure and the measurement. That will give you a better idea of how much material resides within the lungs or in the body and how much dose the lungs and other organs of the body are receiving.

MR. STIVER: Now that may be true. This is kind of a theoretical discussion here, if it was one day or it was ten days. We don't really know what the time period was. We could probably make a pretty reasonable assumption that it wouldn't be an immediate -- that particular intake might have happened, I don't know, a month ago.

DR. MAURO: This is John.

Let me ask a sufficient accuracy question. I am listening intently to this discussion.

What I am hearing is that, at least in one case, the low limit of detection was 6 milligrams of thorium-232, and Joyce explained that that must be associated with about 600 rem to the bone surface, and whatever the other values are to other organs.

So, what that really means is here we have a person. We don't know what his dose is. It could be zero, but it could be 600 rem, someplace between there. We don't know

where it is.

MEMBER ZIEMER: Committed dose, John.

MR. STIVER: Yes, it is committed over 50 --

DR. MAURO: Oh, okay. Then, per year, we could divide that. But that is just one measurement. Of course, then, what I am getting at is, for that one -- let's work with that number, just because I am going to pose a very simple question.

The committed dose from that person could be, what I am hearing, if you accept that lower limit of detection, it could be anywhere from zero to 600 rem to the bone surface, 30-year committed dose. That kind of uncertainty, zero to 600 rem, committed dose, does that meet your criteria of sufficient accuracy?

That is what is happening here. We have got a person and we can say -- and I don't know if Joyce would agree. What I heard

is she may not believe that is 6 milligrams, either. I understand it could have been higher. But let's just assume that is the low limit of detection and we take that on face value.

I am going to make it really simple. You take that on face value that, yes, we believe that, in fact, the lower limit of detection was, in fact, 6 milligrams. That means all you can say is that the real, but unknown, dose commitment to this person's bone surface is anywhere between zero and 600 rem. Does that meet the test of sufficient accuracy?

MR. ROLFES: John, this is Mark Rolfes.

That is essentially asking if the way the body handles a particular material is sufficiently accurate.

DR. MAURO: Well, no, my problem is that we are not talking about zero to 10 millirems, as we are with less than the lower

limit of detection for a TLD. We are talking about zero to 600 rem.

This implies that we cannot reconstruct the dose, in my opinion, to meet sufficient accuracy because the methodology does not allow for you to predict the dose with sufficient accuracy.

MR. MORRIS: Ted, this is Bob Morris. Please?

MR. KATZ: Yes, Bob.

MR. MORRIS: I have an opinion about sufficient accuracy, too.

(Laughter.)

It is one that I actually developed from listening to Dr. Ziemer say this at some point in the past. The question about sufficient accuracy really focuses on, can we make an appropriate decision regarding compensation of the individual? Not can we get the dose right, but can we make the right decision in terms of the claimant-favorable compensation or not?

And I would submit that we have shown you every reason to believe that we will be making these compensations in favor of the claimant.

MR. ROLFES: Good point, Bob. Thank you.

CHAIR CLAWSON: Very good point, Bob. So, we are going to give them all 600, anybody that is in there. I beg to differ with you on that being sufficient accuracy.

MR. ROLFES: That is exactly what we do. We give the benefit of the doubt. The way this program is designed, you give the benefit of the doubt to the claimant. It is within your purview. If you don't believe that our approach is accurate, so be it.

MEMBER SCHOFIELD: Well, I have a question, going back to the calibration. I mean, obviously, in your instrumentation you are going to have to do some kind of calibration. Do we know what kind of procedures they used for their calibration,

which would give you at least a good feeling about the accuracy of their measurements? Do we have that data?

MR. ROLFES: Yes, there is information. Since this is light bulb noble in-vivo counter, the calibration scenarios and background on the machine, there is a reference developed by Hap West in 1965. And then, there is also some calibration data that we have got on the K: drive as well, showing different calibration using different types of phantoms and different amounts of thorium of different ages. That has been out there for years, I think.

MR. STIVER: Yes, inherent in all those discussions of calibration is this caveat that the measurements have to be very careful. They have to be measurable, both the different isotopes, actinium and lead, in order to gauge the age of the source. There is a very inherently uncertain practice to do this.

And so, I mean, John has a very good point. What are you going to reflect this to find what is sufficiently accurate? In this particular case, we are taking an MDL dose, which could range from zero to 600. Or if you look at a later time period, depending on the scenario, it could be higher than that. Is that really reasonable to use that as the basis for accuracy?

For a TLD, when you are looking at a dose of 10 to 20 millirem, perhaps. But this is the kind of decision that has got to be put before the Board. Is this really reasonable to make that kind of determination?

MR. ROLFES: It is a subjective call, I mean ultimately.

DR. GLOVER: I would just submit that it is for all plutonium, Super S. This is a multi-sitewide discussion that you are entering because it is saying, at what point does an MDA become non-sufficiently accurate?

DR. MAURO: I think that you have

just nailed it. This is a very fundamental discussion we are having here.

DR. GLOVER: It is.

DR. MAURO: We haven't had this conversation before. It is very important.

DR. GLOVER: In a multi-sitewide discussion, a generic analysis, I mean, I would definitely like to have senior NIOSH management, Neton and folks, weighing-in and providing discourse.

This isn't related to Fernald. I just want to make sure. We have some limitations on this. And so, I think if we want to get into this perspective, I would really appreciate that we put it into an arena where it is elevated at the right level.

CHAIR CLAWSON: Well, I am going to be right honest with you. We have been beating this around for long enough as a Work Group and stuff. And it appears to me that we are going to differ on our opinions. To me, this can go before the full Board. I am sure

that the full Board is going to weigh in on this issue.

Paul?

MEMBER ZIEMER: Well, in one sense, it is not any different than what we do on all of our doses. You know, they all show up as a distribution in a sense. And so, a given worker whose most probable dose is here at the peak, who may have had no dose, we go up to the tail. If you had, let's say if all the exposures were below the 6-milligram value, yes, you would have a distribution of some sort around that. And you say, yes, but we are going to select -- we are not going to assign zero. We are going to say it is possible that that person -- we don't know for an individual where they are in that, but we are going to give them the upper end of that tail in order to make it claimant-favorable.

And if they have more than 6, always you go up in your detection system. You can become more accurate in terms of your

measurement. It is true of any system, like film badges or whatever, or any counting system. As you get higher, you get more accurate. As you get more accurate, the uncertainties -- and you can assign that dose more accurately.

So, the people for whom you don't know the dose very well get a much, much bigger break. I often tell people, if you want to press for accuracy of measurement, then the tail is going to come down, and your Probability of Causation is going to be much closer to your real value. It is sort of like, the less we know, the better off you are.

But, for those down in this tail, John, I don't think we are saying zero to 600. We are saying we can bound it at 600, or something like that. Do you see what I am saying?

DR. MAURO: Yes. No, remember, I can --

MEMBER ZIEMER: And I don't care what the number is. I don't care if it is 600 rem or if it is 60 millirem, because you still have that distribution. And conceptually, you are saying, can I bound it? Can I put a limit on it?

DR. MAURO: With sufficient accuracy. No, see, I just raised a very narrow question, and it really goes to, at what point is your lower limit of detection so poor that you cannot assign a dose with sufficient accuracy?

MEMBER ZIEMER: Well, if all your readings are below --

DR. MAURO: Now you could place an upper bound --

MEMBER ZIEMER: -- that, yes.

DR. MAURO: I could take it a step further. If the person did not die of acute radiation syndrome, you could argue that you placed -- I mean, I am being a little facetious now; I realize that -- but you could

say, well, he didn't get more than 600 rem whole-body dose, because that is of the metric.

I have got to say, when I am hearing 600 rem as being a low level --

MEMBER ZIEMER: John, this is 600 rem on 50 years. It is about 10-12 rem a year.

DR. MAURO: Twelve rem a year, yes.

MR. ROLFES: And this is for one of the highest-exposed organs. That is the most important point in there.

DR. MAURO: Right. I agree. But I think that the concept, this concept has not been discussed before. When we have a low limit of detection that is up in a range that is very high doses --

MR. ROLFES: These aren't that high of doses, John.

DR. MAURO: Let me just ask Joyce one question. Joyce, do you believe the 6-

milligram number is a number that is a reliable number. That is, that is, in fact, we can say with a degree of confidence that, when they say my lower limit of detection is 6 milligram, do you feel that that is a number that you trust?

DR. LIPZSTEIN: No.

DR. MAURO: Okay. That is the second --

DR. LIPZSTEIN: It doesn't reflect Fernald. We have many other detection limits in other papers saying about 10. We don't know.

And I have another question, John. Suppose someone had a result -- you know, it is not a coworker model; I mean a worker. A worker had a result of 2.10 milligrams of thorium. It was positive, higher than the detection limit of lead, .40. It is higher than the detection limit of .65, actinium-228, which is also higher than the detection limit. So, you have a result of 2.10 that could be

measured through lead and actinium. And you calculated those on this 2.1 milligrams.

And then, you have another worker who had the double, 4.3 milligrams, and you calculated those for 4.3 milligrams. He will have the double of the dose of the guy that had 2.1. But, yet, he had lower than detection limits lead and actinium. So, what does this mean?

DR. MAURO: So, what I am hearing is there are two questions that I guess SC&A is putting on the table.

One is, the data itself, as it speaks to us, does not make sense. So, we don't believe the 6 number that we are looking at as being the low limit of detection.

And second, even if it is, we are raising a policy question, is it appropriate to move forward and make judgments on sufficient accuracy when the real, but unknown, dose could vary over such an incredible range?

I guess those are two. That is what I am hearing this all boils down to.

In one case it is a technical issue where SC&A does not trust the numbers that we are looking at as being reliable, and that certainly is a very -- in other words, our position is we do not trust those numbers as measured between 1968 and 1978 and reported, for the reasons discussed.

And second, the other half of it is, even if we did, we are raising a policy question of whether or not that low limit of detection is compatible with the concept of sufficient accuracy.

MR. STIVER: John, you are kind of inheriting that. Really, it gets into the point, a particular detection system, when is it deemed not suitable for a particular application? That is really, I think, the question we are asking here.

MEMBER MELIUS: This is Jim Melius. If I can ask a more mundane,

practical question?

It is my understanding NIOSH has two reports that are about to be -- I can't tell if they are in review or they are completed or not, but I am trying to get a sense of when they will be available.

MR. ROLFES: Dr. Melius, this is Mark.

We just received SC&A's two or three reports on thorium within the past week or two weeks. We just prepared some draft responses to those for discussion today. We should be able to get our more formal response, we hope to have it in time for the upcoming Advisory Board meeting, but it is going to be a very tight schedule at this point.

MEMBER MELIUS: I would strongly urge you to get it done by the Advisory Board meeting.

MR. ROLFES: We will definitely work to do that, sir. We have got many irons

in the fire. So, we will work to achieve that.

MEMBER MELIUS: I will email Stu to that effect also. You know, I understand what you are saying, and I don't expect you to commit to that. So, that's fine.

DR. GLOVER: There was a question I think Mark was asking earlier. I want to make sure. I know that Joyce has said that there are certain workers where we saw this large dropoff. Are we clear on who those workers are, Mark? Do we have that data?

MR. ROLFES: That information hasn't been provided to us.

DR. GLOVER: Can we get that? That's okay. Could we just get it? I just want to make sure we have it. Because being a whole-body counter, there's lots of reasons why things happen, why you can count people multiple times, and that is certainly appreciated. But I think it is good if we have that data, so we can address it.

I would like to remind everybody that DOE orders, as they stand now, allow 50-rem committed dose effective to an organ. That is, it is not 5 rem a year. It is actually organ dose is 50 rem over your lifetime, committed effective dose.

And so, the 500, while being high, sounds a lot higher when you talked about 5, but it is really 10 times the limit of what you have today. And so, you are talking about technology shortfalls. At what time does this technology shortfall become incompatible with this, I think is what you are meaning.

MR. STIVER: Yes, that is really what we are getting at here.

MR. ROLFES: Yes, that is a good point, Sam. That is what I wanted to say.

I'm sorry.

DR. GLOVER: No, that is okay.

MEMBER ZIEMER: One other point on minimum detectable activity, and I don't know whether this 6, or whatever, number is right

or wrong, but the fact that some other counter or unit or facility gets 10 I think is immaterial.

You can improve your minimum detectable activity by counting longer. So, we can't intercompare the facilities like that. If 6 is what Hap West got with counting with a certain size crystal for a certain length of time, I am not concerned about that number per se. That is what they get.

They are saying, "We can't detect lower than that with any confidence." And some other counter or some other group may say, "Well, we can't do any better than 10." They probably have a different counting system and maybe they have a different period of time.

But, whatever it is, I think the issue is more the philosophical one that you are talking about. It is not the fact that it is 6, and that that results in a lifetime dose that is high when people get more than 6. If

it is 6, so be it. What do you with the values that are below that, because they are in the noise of the system, which was what I was trying to emphasize.

MR. STIVER: Yes, that was really our point in trying to illustrate this. We are kind of getting to that whole issue of, was this counting system really adequate for the task at hand? Again, it kind of transcends Fernald at this point.

MEMBER ZIEMER: Right. And I suppose at that time, where they are working under -- what years were these?

CHAIR CLAWSON: 1968 to 1978. That is under Ohio --

MEMBER ZIEMER: Well, 1968, they are already in the 5 rem per year. They were. But the lifetime limit, then, for a person --

MR. STIVER: So, you probably preferred something on one year which --

MEMBER ZIEMER: Yes. I mean, a person for a 50-year working life could

technically get -- well, that is whole body. That is whole body. Now, if you use the correction factors, like if you are talking lung dose, for example, it is not 5 rems per year.

MR. STIVER: If you are looking at stochastic effects, yes, for that particular -- you know, you are going to have the weighting factor that goes along with that.

MEMBER ZIEMER: Yes. Well, I am just trying to say the number sounds big until you put it on an annual basis and put the organ weighting factors in, and it is not much different from other exposures then.

CHAIR CLAWSON: But, Paul, wouldn't you also be able to take, if it is over the detectable limit, be able to back-extrapolate it and come up fairly close to what they came up with? I have a hard time with --

MEMBER ZIEMER: Well, when you get over -- I don't know. Are they detecting the

actinium separately?

MR. STIVER: Part of the problem is this table shows during the period of overlap, when you actually have the actinium and lead measurements in milligram form. You only have during this short period of time here.

CHAIR CLAWSON: 1968 to 1978.

MR. STIVER: We are trying to determine whether these measurements of lead-212 really match up to the milligrams-of-thorium data. It is really inconclusive when you look at this, their comparison here.

MEMBER ZIEMER: Yes, and if they are down in the detection limit on both of those, it is going to be hard to correlate that kind of data.

MR. STIVER: In this particular data, you have got one that is close to the detection limit, but you can see that these other values are way off.

CHAIR CLAWSON: So, did we look at

anything over the detection limit?

MR. STIVER: Yes, we did. In the previous paragraphs, Joyce brings up under the issue of biokinetic realism these situations. There are two kind of anecdotal discussions here about, if you had measurement A at time period T, what would you expect subsequent to that? And the data that we see don't really seem to comport with known biokinetic properties. Granted, there are individual variations and the type of the particular characteristics of a given intake are going to have a big impact on that as well.

DR. GLOVER: Again, I would just, from a standpoint of does something make sense, often faced with contamination issues as a whole-body counter -- and I did actually misspeak. Fifty rem per year every year you have a plutonium intake, we do the committed effective dose, you know, that you are allowed to get 50 rem to the lung. The next year you have got to get the next 50.

MEMBER ZIEMER: But that is a dose-weighted factor.

DR. GLOVER: No, so every year the lung gets 50 rems.

MEMBER ZIEMER: Yes, sure.

DR. GLOVER: And the next year, you get another. So, you can actually receive, even today under DOE rules, limits that the 12 rem is not outside of from an organ-specific dose. Those are allowable limits. Those are within the allowable limits today.

MR. STIVER: With your actinium and in your .12 factor for --

DR. GLOVER: But those are for whole body. This is for the lung.

MR. STIVER: Yes.

DR. GLOVER: Remember, that 12 rem was for the organ.

MR. STIVER: Oh, sure. Right.

DR. GLOVER: Remember, we talked about some organ-specific dose.

MR. STIVER: Yes.

DR. GLOVER: And so, that is within the legal limit today. You could deliver 12 rem to the bone surfaces every year.

MR. STIVER: I guess the question is, do you want to have a counting system that is accurate enough to where you can get down to a much lower --

MEMBER ZIEMER: Oh, yes, you always want to --

DR. GLOVER: I am just letting you know that today's list -- anyway, it is your all's --

MR. STIVER: So, these are high doses, and, granted, you are allowed high organ doses which would appear to be outrageous in some senses, but they are good, once you start factoring in the actual weighting factor for a given cancer.

MR. ROLFES: The high doses, what I had tried to say before, it is more how the

body acts. I mean, what type of cancer you are reconstructing a dose for, it is the biokinetic models and the dose delivered to different organs depend more upon those organs and the processes going on within those organs.

The dose value or the intake or exposure amount is equally important, but so is the type of cancer, the organ. To say 600 rem over 50 years, which is roughly, on average, 12 rem per year, you get more dose in the first few years and less towards the end of the 50, that same 600 rem to the bone surfaces is going to be less than 100 millirem to another organ over 50 years. So, it all depends on what organ it is. The bone surface is just that organ that is such a small mass where thorium progeny concentrate. That delivers all the dose.

The prostate, for example, or the bladder or your eyeball or your skin, thorium does not concentrate there. So, the doses

over 50 years or even annually, the doses are going to be orders of magnitude less.

MR. STIVER: Mark, I just want you to realize that we didn't put those in -- it was basically illustrative of the magnitude of doses which could accrue that would be relatively important for thorium exposure. That is why we included that.

MR. ROLFES: Sure.

MR. STIVER: Now the magnitude in a particular year, relative to certain limits, that is another issue altogether. We wanted to show that here we have a situation where we have data that are less than the detection limit that are still, nevertheless, able to result in a very high dose.

MR. ROLFES: Right.

DR. MAURO: This is John.

One more issue perspective related to what I call this policy question is, there was a time when very high doses would be assigned, and everyone agreed it really

couldn't be higher than this. I remember the OTIB-4 where you would assign 100 MAC as being a default high-end number, only to be used for the purpose of denial. That was the philosophy.

That is, yes, you may be at a place where, for the sake of expediency, you could assign a very, very high dose. But if you still deny, that is an acceptable method to go.

In a funny way, we are in that kind of situation here. What you are saying is we have a technology that will allow us -- by the way, I am not really saying we agree with this. But if we, in fact, said -- and we are not saying this -- but if we in fact said, yes, we believe that 6 milligrams is a reasonable upper bound and it can't really be higher than that, now, as we heard Joyce say, we don't believe that. But if we did, the question becomes, shouldn't a number like that be used only for the sake of denial, because

it represents an upper bound that is extremely high that may not be real?

So, that is another perspective to offer because we did encounter a similar situation with OTIB-4.

MR. ROLFES: John, I think you are referring to OTIB-2 on the 28 radionuclide worst-case --

DR. MAURO: No, no, this was an OTIB that was used as a default value for AWE facilities to place an upper bound on uranium exposures. It was being used solely for the purpose of denial, because that was the highest numbers ever experienced, 100 MAC. It was on that order.

And right in the beginning of the OTIB -- now it is no longer being used. You have since replaced that with TBD-6000. But the philosophy was, yes, there are times when we can default to some upper-end number. It is a number that we will assign for the sole purpose of denial, to get through the process

quickly.

I only bring it up because we might be in that territory right now, notwithstanding the fact that we don't even trust the 6-milligram number. But I am saying, even if we did, I think we are in an area where a conversation is needed when you have that much uncertainty in your ability to reconstruct a dose because of the methodology. Are we in the realm of we don't have sufficient accuracy? And now we are at a place where this is no longer a question for SC&A to address.

CHAIR CLAWSON: Thanks, John.

I have one question.

MR. KATZ: That's all right.

DR. GLOVER: I was going to defer to him.

CHAIR CLAWSON: 1968 to 1978, the reason we are looking at this era is because this is when it was done in milli --

MR. STIVER: Milligrams, the

source data.

CHAIR CLAWSON: Right. What happened prior to this?

MR. STIVER: Prior to this was the DWE work.

CHAIR CLAWSON: Okay.

MR. STIVER: From 1953 to 1967.

CHAIR CLAWSON: And then, after 1978, we --

MR. STIVER: They were still using the same counting system, but they were actually reporting the results for the nanocuries of lead-212 and actinium-228. Before that, you don't have that.

CHAIR CLAWSON: So, have we even looked at the information from 1978 on?

MR. STIVER: We kind of tabled that while we were looking at 1968 to 1978.

CHAIR CLAWSON: Okay.

MR. STIVER: We acknowledge that there are advantages to using that data which would minimize the uncertainty. Namely, you

have the lead-212 values.

CHAIR CLAWSON: Well, I guess that is part of my question as the Work Group Chair, because I know that we have got big differences on this. I think it is going to go -- to tell you the truth, I would like to be able to bring this before the Board, the bottom line. We have been beating around this 10 years for I don't know how long. I don't know how to bring it before them to get them involved in this. Because, personally, I don't think that, as a Work Group, we are going to come to a resolution on this. So, the bottom line is I think it falls onto the Board.

And so, I am looking at Phil and Paul. How do you feel that we should proceed with this, because we have been going at this one for a very long time? We are not anywhere closer, in my opinion, than we were before.

So, Paul, any suggestions?

MEMBER ZIEMER: Well, I guess we

need to have SC&A look at what their responses are. Or do you think that is going to change anything?

MR. STIVER: Well, it is kind of hard to say without seeing the details of it. From what I see from the presentation, I can't say that there is anything in there that really seems to be a game-changer to me. You know, the devil is in the details on most of these things.

CHAIR CLAWSON: Well, and DCAS has said that they need the raw data or the individual files?

MR. BARTON: That is actually in the database that gets compiled. So, you have it.

MR. ROLFES: Yes, that was just one small portion of it.

MR. BARTON: Yes, that was an anecdotal --

MR. ROLFES: We can search for 10.2 and look where it is in the hard copy and

find it.

DR. GLOVER: I just wanted to make sure that what deliverables we walk away with. One of those, we have a higher-order thing about this technology shortfall in this dose. Is that something that you are asking us to address specifically? We could generically assume this -- because we have thrown a bunch of numbers around about whether it would be 500 or 600 rem. We just took that at face value. We can do a couple of scenarios or how does that look compared to various things, and describe what that is.

But I certainly think it is a broader-term thing. I wouldn't want to resolve it at this level, at least us. We would want to talk to other folks. I want to make sure we get the right action items.

MEMBER ZIEMER: Well, let me ask this question: from SC&A's point of view, a priori, were you thinking that the lead-212 and the actinium-228 and the thorium

activities would be relatable on a proportional basis?

MR. STIVER: At levels greater than the detection limit, we would expect to see some correlation in those values.

MEMBER ZIEMER: And is that based on an assumption of the age of the thorium? I mean, if you don't know when the intakes occurred, there is obviously separations in the body metabolically between the three. You have some possibilities on the ages of the thorium.

So, I am wondering how useful that is. What if you simply said, look, here's the range of values that we get for these bioassays on each of these three nuclides? Let's take the group that is above minimum activity level, and here's the ranges.

We take all that data and you assign top of the range in the coworker model for all these three nuclides. That would be one way to do it, regardless of what the end

dose is.

MR. STIVER: Well, what you are proposing would be just like a bounding dose based on the highest values that were --

MEMBER ZIEMER: That is what I am saying, yes. Conceptually, is that an approach or are you saying that, unless these three correlate, we can't depend on --

MR. STIVER: Getting back to the milligrams of thorium without knowing what those numbers are, I mean, you could in theory be off by a factor of 100 on the final result if you are looking at actinium-228, for example.

DR. MAURO: Paul, this is John.

Post-1978, that is the reason we believe we do trust and do like those numbers. You do have the actinium and lead information that allows you to reconstruct the intake of thorium-232.

MEMBER ZIEMER: Right. I am talking about pre-1978. I am trying to find

out what --

MR. STIVER: Pre-1978, we just don't have that type of data.

MEMBER ZIEMER: If you don't have that, what do you do?

MR. STIVER: If you were to say we have got to use this data that we have available, you would have to look at that small subsample for each year of maybe 2 or 3 percent. You would be essentially in a position of just taking, I don't know if we would call it an extreme upper bound, but a very high upper-bound value. And then, you would have a certain amount of, you would have a large amount of uncertainty that would have to be factored into that.

Even then, I mean, regardless of --

MEMBER ZIEMER: But I am pointing out, when you do that, that actually helps you because it pushes the tail way out.

MR. STIVER: It does, but then it

comes down to a matter of plausibility and sufficient accuracy again.

MEMBER ZIEMER: Well, if you are using real numbers and you accept --

MR. STIVER: Well, you've got to accept them at face value.

MEMBER ZIEMER: All right, yes, okay.

MR. STIVER: That is kind of the conundrum we are facing here.

MEMBER ZIEMER: Okay. So, is the concern, then, that even the real numbers are off so much that you don't --

MR. STIVER: When you do look at the few values, there aren't very many of them, yes. The ones that are available that are above the protection limit are highly variable as well. There is some aspect of individual variability that goes into that, but there is also just the innate difficulty of measuring thorium using this particular technique.

So, without having those numbers -- you have post-1978 where you can actually go back. But, also, post-1978 is a period of thorium storage. So, you have more of a homogeneous source term than you would have had during the processing period. So, there is a lot less uncertainty there.

MEMBER ZIEMER: Well, let me ask Mark this question then. If you were doing a coworker model for those early years, let's just take the thorium part. I mean, we have all these ones that are below detection limit, but you have some others, too.

MR. ROLFES: Yes.

MEMBER ZIEMER: In a coworker model, what would you do? Would you take everything or do you take these in the coworker model and assign them the midpoint before you put them into the mix?

MR. ROLFES: We would generate a 50th and 84th percentile. We would basically use the data we have reported to us and do a

log-normal distribution.

MEMBER ZIEMER: Of everything?

MR. STIVER: Uncensored.

MR. ROLFES: We would not censor the data, correct. So, we would include a reported value of 1 milligram, which was less than the minimum detectable amount at face value.

MEMBER ZIEMER: Got you.

MR. ROLFES: That is the way we complete our coworker intakes.

MEMBER ZIEMER: Okay.

MR. ROLFES: To get back, when SC&A had said that it was their opinion that the doses could be -- or excuse me -- that we could be off by a factor of 100 or more, NIOSH, we feel that the upper-bound worst-case correction factor would be a correction factor of five.

MEMBER ZIEMER: Based on?

MR. ROLFES: This is based upon on our draft new demonstration regarding the

worst-case separations, chemical separations, of thorium from progeny.

MR. STIVER: Can I go ahead and make a statement? Oh, go ahead and finish, Mark, and then I will say something.

MR. ROLFES: This is something that we developed since we received SC&A's new report. This is new information that SC&A has not yet seen.

MR. STIVER: One thing about the separation, this is based on an assumption that lead-212 is the radionuclide being measured. And so, that is where this thrice-purified material would have a ratio that would be -- you would have that factor of five.

Our concern is they may not have even used lead-212. They may have been using actinium-228, in which case, you know, because of the buildup of the radium, you could be off by huge amounts, depending on when actual separation and intake occurred.

MR. ROLFES: We have addressed that in this paper, different scenarios which show -- maybe, Tom, I am not sure if you are on the phone. Maybe you might be able to speak to the different scenarios we have laid out on how we would handle calculating thorium-232 intakes chronically, and maybe give us a brief summary of what you have done in your chronic intake retention factors White Paper, if you out there?

MR. LaBONE: Yes. Yes, I can do that.

MR. KATZ: Thank you, Tom.

MR. LaBONE: There were two issues that I addressed in that White Paper. The first one was we have been discussing all of the problems of just trying to figure out, if you say you have thorium, just exactly what is it; what is the mixture?

And so, what we said was, well, let's just take a look at like how bad a mixture could it be. And so, this is where we

looked at it and said, okay, how many times did it go through a chemical separation? Because every time you go through the chemical separation, it will disrupt the equilibrium of it and the amount of thorium-228, which is really the parent of what we are looking for, which is the lead-212, will go down.

So, anyway, we said we will just run it through three separations at the worst time. And so, those times in Mark's slides are the times where there is a minimum -- let me go back to his slide No. 12. The time in years, those are the minima. So, for example, right at time zero, you do a separation, and then the minimum, which we have used in the past, is at about four-and-a-half years. Then, at that time, we did another separation, and the next minimum occurs at 7.1 years after time zero, and so forth.

So, anyway, we went through and said, after three separations, this is probably about as bad as it can get in

reality. And so, if you use basically the mixture about a month after 8.8 years, give the radium-224 time to grow in, and so forth, and then the lead-212 will grow in, that that would be the worst-case scenario mixture to use.

And so, it would be hard to come up with a mixture that would give a higher dose base for a unit lead-212 chest count. So, given nothing other than the fact that you have some thorium and it has gone through separations, that would be the worst case that I could think of, anyway.

And so, in the absence of any information about the source term, that is what I think we are proposing to use.

DR. LIPZSTEIN: But how do you know that it was measured to lead-212?

MR. LaBONE: I am not addressing the issue of the milligrams of thorium that you are talking about. The calculation was, given a lead-212 chest count -- I understand

your issues with how exactly did they come up with the milligrams of thorium, okay, but this is not related to that.

This was the question of, what mixture should we use, given a lead-212 chest count, and we don't know anything about the source term?

MR. STIVER: Okay. So, this is really applicable to the 1978-to-1988 time period.

MR. LaBONE: Or, if you could somehow relate the milligrams of thorium back to what is the lead-212 that would have been present from that, but to do that, as Joyce is pointing out, you need to know something about how did they calculate the milligrams of thorium.

So, you could, theoretically, if you came up with enough information about how they did the calibration, you could use that same ratio.

MR. ROLFES: Thank you, Tom.

Now, along the lines of a reference regarding whether or not lead-212 was used as the photopeak to determine how much thorium-232 was ultimately present, Don or Bob, do we have a reference that says this is the photopeak that was used? If one of you two could possibly speak to that, please?

CHAIR CLAWSON: *6.

MR. ROLFES: If either of you are speaking, Don Buhler or Bob Morris, you might be on mute because we are not hearing anything.

MR. MORRIS: I am not ready to answer that question off the top of my head.

MR. ROLFES: Okay.

COURT REPORTER: Who was that?

MR. KATZ: That was Bob Morris.

MEMBER ZIEMER: That was Morris.

MR. ROLFES: Okay.

MR. STIVER: This is John Stiver.

I can say that our investigations have not yielded that type of information.

MR. ROLFES: Okay. We will definitely get back to you on that. We will take a look once again for any information.

DR. MAURO: This is John.

Tom LaBone, are you still on the line? Tom spoke before?

MR. LaBONE: Yes, I am.

DR. MAURO: Tom?

MR. LaBONE: Yes?

DR. MAURO: Yes, you heard Joyce's concerns, basically, and John Stiver with regard to the 1968-to-1978 data where we have these thorium-232 numbers that are reported for the chest count. When all is said and done -- and, Joyce, certainly correct me if I am misquoting you -- but she doesn't trust those numbers as being numbers that she believes are correct and can be relied upon for the purpose of dose reconstruction.

What do you feel?

MR. LaBONE: I have not looked at detail, at the thorium milligram numbers. I

have read Joyce's paper and I have listened to the conversations. I can make a couple of comments.

The one is the issue of basically the variability in the thorium chest counts. It was suspicious, the large amount of variability. To me, one of the best ways to look at that is to go back and look at the replicate counts that were done. So, somebody who was counted multiple times on one day. How did those results vary?

And I went back and looked at that. So, basically, it was kind of like a control chart. You should be able to get in one day some degree of reproducibility.

I thought, when I looked at that data, that it was fairly reasonable. The problem of comparing a count of, say, 10 milligrams on one day with another count a month and a half later is some strange things happen with chest counting. You can get a positive result.

Normally, what you would do if you had 10 milligrams is you would take the person, run through the shower, change clothes, and so forth, looking for external contamination. And then, you would count them again.

And so, if there was no duplicate count on that day, then it is hard to say that, well, according to such-and-such a model, this is implausible, but that happens. If you have done chest counting, I mean, I see results all the time that it is high one day, and then you bring the person back a week later and there is nothing there. I think that most people who have done chest counting would say, "I have seen things like that."

And I don't know if that is inherently evidence that the system is flawed. So, again, I don't know if the analysis of the duplicate, replicate counts, I should say, was ever sent out, but I think that is something to look at, is that.

The other issue is the --

MR. STIVER: Tom, can I interrupt you for just one second?

MR. LaBONE: Absolutely.

MR. STIVER: Yes, this is John Stiver again.

You are talking about this replicate count. Now this is in data that are available during what timeframe? We certainly haven't seen that, any information to that effect for the milligrams-of-thorium data. Is this the post-1978 timeframe then, where you actually have the --

MR. LaBONE: Hold on just a second. I will tell you exactly when the timeframe was. I have like eight -- there are so many papers.

(Laughter.)

Okay. I have, from 1968, it is during the timeframe we are interested in. I mean, I can actually send this to Mark and he could show it or something. Or I don't know

if it needs to be cleared or what the protocol is.

But this was during the timeframe. It was from, I think, basically, the same Excel spreadsheet that all of us are working off of. I don't think I had access to anything special.

But, anyway, I mean, you haven't seen it. I guess you really can't comment on it. But the thing is that I would look at that to see and the reproducibility of that for multiple counts.

I mean, there was one person that had like five counts in one day, and they were fairly tight as far what the result was.

DR. LIPZSTEIN: I was looking at -- I am sorry to interrupt -- I looked at all the results that were above 6 milligrams. We had one result that was measured on the same day, and the first result was 17 milligrams and the second result was 2.3 milligrams. But they think the problem with the results is

because, when you measure thorium just after the worker has come out of the work, you have a lot of inference from the thorium, from the radium-220. So, if you count some hours later, you will get an amount that is smaller than the first one. So, that is probably why he had 17 and then 2.3 milligrams on the same day.

So, the problem with the same-day counting is the influence of radium-220 if lead-212 was measured. So, I don't know. The problem is that we don't know what they did. That is our biggest problem.

MR. LaBONE: I was looking at this basically -- the question was, do I have faith in the system? The thing I was looking at was, was the system reproducibility on any given day? And so, if they did five counts and it was plus or minus 30 percent for those five counts, that gives me some faith that they could at least reproduce that count that day.

DR. LIPZSTEIN: Yes.

MR. LaBONE: Again, when you start comparing counts that are months apart, there are so many things that can be going on there, that it is difficult for me to decide, is this thing out of control or not?

DR. LIPZSTEIN: Tom, it is right, but what I am saying is that I saw -- I have discounted all of them that I looked above 6 milligrams. I had one that was measured twice on the same day, and the first result was 17, the second was 2.3. This was in 1971. Oh, I'm sorry, 1976.

MR. LaBONE: 1976, yes. See, I had it -- most of these ones that had multiple counts, for some reason, were, it looks like, less than 6.

DR. LIPZSTEIN: Yes.

MR. LaBONE: There was another one similar to what you are talking about in 1969, where it looks like they counted, it is a large spread, and it looks like they counted

them and then recounted them, and it was nothing there.

MR. BARTON: Tom, this is Bob Barton.

On the spreadsheet you are looking at, all the way to the left it has Column A is file and Column B is page number. Do you happen to have those in front of you for the bio counts in one day?

MR. LaBONE: I don't. I don't have the spreadsheet open. This was a summary plot that I did.

I think the important thing is -- I don't know if we are going to resolve this thumbing through the Excel spreadsheet, but it was, again, I was asked, what did I think of the data? And I hadn't looked at it in detail, but these were things that I did look at.

I can point out, I can send the analysis to you, and you can look at it and see what you think. But, again, it was the

concept of looking at the reproducibility on a given day. Could they reproduce counts? Or were they all over the board counting the same person within a day, because that is a bad indication that it is completely out of control? Statistical control is what I am talking about.

So, anyway, that was the one thing that I looked at. And then, the other is the issue of this is primarily analytical noise. This is a common problem. And I have pointed out before that today in 2012 we have the same issue, even with all of our technology, with things like weapons-grade plutonium. The noise in the system correlates to fairly high doses, to the point where it makes it of limited utility for occupational settings.

Because in an occupational setting, if it is below detection limit, we don't assign anything. But in a compensation program, we do. And so, that is a big difference I think we need to keep in mind of

occupational versus compensation, that a high detection limit is not a good thing for occupational and it is not good for compensation. But, again, you can work around it if you just want to assign a conservative estimate of dose.

Again, the issue, I think it is a valid point about how was this, when you say milligrams of thorium, what exactly was there? How was it calibrated? And how do we interpret that in terms of the lead-212 I think is a valid point. I haven't looked at that data to see as much as you have.

Those were kind of my thoughts on the thorium chest counting.

MR. ROLFES: Thank you, Tom. Very good points. I appreciate your input.

CHAIR CLAWSON: Okay. So, I guess my question is -- Sam, did you want to say something?

DR. GLOVER: No, go ahead.

CHAIR CLAWSON: Where do we go

from here? Because, basically, we are at an impasse here. We have been on this for I can't remember how many Work Groups; well, at least the last couple of years.

MR. STIVER: Tom, could I ask you a question here about the replicate samples here?

MR. LaBONE: Yes.

MR. STIVER: When you are counting, when you are doing, basically, replicates of background noise, wouldn't you expect those to be more tightly centered than, say, if you were measuring an analyte? I mean, that is basically telling you that the electronics are stable.

MR. LaBONE: Oh, yes, I would expect it to be very good, and I think it is. But when you get a higher one, the points that Joyce is bringing up are that, again, if you bring a person in and you get a high count -- let's say you get 17 milligrams -- again, the standard protocol today, and as far as I

have been involved with this, is, again, you would take the person, you would shower them, change clothes, and so forth, and then recount them.

And so, typically, that second count comes down. And so, I wouldn't be surprised on a given day if you get a high count and then followed by -- you know, look at the time. If we have the times of them, look and see, is the high one first? I am assuming the high one would always be first, or else they wouldn't do the second one kind of thing.

So, no, I mean, that doesn't bother me. I mean, if you had somebody with an established long-term thorium burden, I would expect that could be pretty tight, too.

MR. STIVER: Yes, I was just wondering if you would expect an actual thorium burden measurement that is stable doesn't see the same kind of counter-precision that you would see based on just background

noise, I guess. There would be some difference that might --

MR. LaBONE: If it was really there, and it was reproducibility --

MR. STIVER: Yes, I guess the question would be, would it be as reproducible as just a null result? Just replicate backgrounds as opposed to --

MR. LaBONE: If it was like, I mean, what you are asking is, if I were to take a known standard of thorium and count it multiple times, would I expect the scatter of it, would it be tight?

MR. STIVER: Would you expect it to be on par with the null distribution scatter?

MR. LaBONE: The relative uncertainty should be lower, if it is higher. If it is really there, then the relative uncertainty, basically, looking at the scatter relative to the actual results, it should be tighter if it is a higher number.

MR. STIVER: Okay. Thanks.

MR. LaBONE: Absolute-wise, I mean it will be bigger.

MR. STIVER: Yes, relative to the --

MR. LaBONE: Yes, to the amount you are actually measuring there. But, again, I think we would have to look. I don't know if there are records available as far as these actual counts, these ones that there is a lot of scatter on.

But I was not upset by a lot of these that I saw that were in the noise region, which, again, I think still has some useful information as far as the system, if you have questions about the reliability of the system.

DR. GLOVER: So, one thing I do hear, though, is that you guys continue to express concern that we don't understand how to get from, whether it is lead or actinium, or whatever -- that is still a major concern

about how to do that?

MR. STIVER: Yes.

DR. GLOVER: And should be addressed in whatever report we deliver?

MR. STIVER: Yes. When you are faced with just a milligram thorium datapoint without any background information, raw data, none of the analysis that went to that, then, how do you account for all the uncertainties that could go into that particular value, when you could be looking at not only lead-212, but also actinium? So, that is a major concern of ours.

The other issue, of course, being the technical shortfall, which is really kind of a more --

DR. GLOVER: I just wanted to make sure, if we wanted to address the technological shortfall, that is a generic thing that I need to make sure management starts dealing with in a not generic way, but it is a sidebar.

MR. STIVER: It is going to be an overarching issue.

DR. GLOVER: Yes.

MR. STIVER: And, yes, I would certainly want to start investigating that. It is something I think is going to -- we have only seen it now in this particular example, but it may come into play again at some point.

DR. GLOVER: So, Brad, would it help you if our papers talked, I mean from a thorium perspective and why these things drop? We will have the values. There's lots of reasons when you count somebody, like Tom said, why they would drop. They did showers and there's different things. Is that the kind of perspective that you want to see, why we may see these kind of differences? Do you want to get into the noise like that? Is that helpful?

MEMBER ZIEMER: I am not concerned about that so much as I think it is two things, in my mind. One is the correlation

between these. I think that was part of your concern in the early years, isn't that correct, John?

MR. STIVER: It is the correlation, and I think you have the problem in the early years. You just don't have the data. So, you don't really know what was done.

In later years, you have that data, and we feel that it can be used in some way. Now I don't know if you can take that later data and then try to extrapolate to earlier years. You certainly can do it in terms of intakes.

CHAIR CLAWSON: And looking at Fernald, I don't think there is any way because so much stuff changed with so much thorium coming onto the plant and a whole different process. This is part of the issue that we are getting into.

Paul, we have been going at this for two, two-and-a-half years, and we are

right back to the same spot.

One of the things that I am afraid of is that we will go through all this and a process that we think is the best, and then it is going to come before the Board and, basically, we are going to go back one way or the other.

I really feel, and I understand, if you were to do multiple counts, usually the second one, you're right, would go down because they have actually run you in, scrubbed you down. You come back in papers and you get counted. I would expect to be able to see that. That is just normal things.

Now, in the earlier years, were they doing that? Who knows? And this is part of the uncertainty that I am getting into. We really don't know, nor can we really reconstruct what we have got there, in my opinion.

And this is where I am coming to you guys. Myself, I feel it needs to go

before the Board. We have exhausted about as much as what we can.

As far as the hierarchy of NIOSH, you know, they are going to have to weigh in on that, too. I think this comes down to the Board as a whole, of how are we going to handle this. Because I don't think this will be the last time that we will see this.

MEMBER ZIEMER: I guess my question on the early years is this: regardless of what the dose implications are, can they detect 6 milligrams? Is that a reliable value and can they do that?

MR. STIVER: We don't really know that value is reliable. I mean, it is reported for that system. Again, when you look at similar systems, they are going to be higher or lower. Is that a valid number? I guess you are asking, can that data that is above that be used to get some kind of a bounding?

MEMBER ZIEMER: Well, usually,

that is based on a calibration of some sort with a phantom.

MR. STIVER: Yes.

MEMBER ZIEMER: They must have something that tells you whether -- even if it is high, if you say, no, I wish it was 1 milligram, or whatever, it is a separate question. If it is 6, then can we use the data? Can we use the data that is below the 6 in a valid way? I mean, we have data. Can we use it? That is sort of my question.

And then, what about the other ones? How are each of these being determined?

MR. STIVER: Our position is really the values that are less than 6 are really meaningless from a dosimetric standpoint. I mean, you are basically dealing with noise. And so, you can look at, this might be real and it may not. You are going to take a midpoint of some distribution, and you fit to that and say, yes, we can assign that. Does that really have any meaning?

MEMBER ZIEMER: Well, it is noise in the sense of a counting system, but you are still assigning dose for it.

MR. ROLFES: That's exactly it. It is not meaningless.

MEMBER ZIEMER: It is just like a minimum on a film badge; you are assigning dose. In this case, it may be fairly sizable.

MR. STIVER: Yes, and that brings us into the technological shortfall argument, so these are really kind of intertwined.

MEMBER ZIEMER: Well, it is sizable on committed dose. It is not unreasonable for annual doses.

MR. ROLFES: I mean, it may be that our missed doses that we assigned are of significant magnitude alone to generate the Probability of Causation greater than 50 percent. That is the intent of this compensation program.

MR. STIVER: We don't even know that the 6 milligrams is a valid number. You

look at, even when you have data where you have replicate samples of both the actinium and lead, and you have got such a small differential in that equilibrium ratio, apparently so flat in certain points, that just the statistical variability within those numbers for a good measurement can put you off by a factor of two. Could it be 6? Could it be 12? Could it be something higher than that?

MEMBER ZIEMER: Well, that is what I am sort of asking.

MR. STIVER: I don't have a lot of faith in that number. I know Joyce has a fairly strong opinion of that as well.

DR. LIPZSTEIN: I don't have faith in any of the numbers because, first, you have higher than detection limits lead and higher than detection limits actinium, and you have lower than the 6 milligrams in the reported result. And then, you have the opposite also. You have negative numbers, negative

measurements of lead, and have results in milligrams also.

MEMBER ZIEMER: But, see, that makes the assumption that these have to correlate.

DR. LIPZSTEIN: Yes, that is my main point. I don't know how this result --

MEMBER ZIEMER: I would like somebody to show me that they have to correlate. They have to.

DR. LIPZSTEIN: I looked at the results of the order higher than 6 milligrams. For example, I have one person here. He had 20.4 milligrams of thorium in 1969. So, what is reasonable? If it was 6 milligrams, the lower limit of detection, and in 1969 they had some feeling of what this represented. There was no follow-up for this 20.4 milligrams.

And many like those results, I like that also. I just took this now. Another one was 18 milligrams and no follow-up. So, you have those high numbers that

didn't have any follow-up. So, probably this didn't mean anything for them.

MR. ROLFES: Joyce, this is Mark.

DR. LIPZSTEIN: So, it is hard to believe that very high results didn't have any follow-up, if they believed these were real exposures.

DR. MAURO: Joyce, you bring up -- what I am hearing, though, is that you don't believe they were that high? Or do you believe they could have been higher?

DR. LIPZSTEIN: I don't know.

DR. MAURO: You don't know?

DR. LIPZSTEIN: I don't know. I just know that someone had a result of 20 milligrams of thorium in 1969. There was no follow-up at all. So, nobody thought, oh, this is strange; someone was exposed, and maybe I should measure him again and see what is happening. No, no worry about it.

MR. BARTON: In addition to that, Joyce, the very highest example that we found

was 32.5, and that worker was there for another year and a half and he was never measured again.

MR. ROLFES: And in those cases -- I am glad you brought those up -- if we have a single point in time, and we have a 25-milligram measurement, we would use that in dose reconstruction. We wouldn't say, "Oh, that's no good. That's too high." We don't do that. We give the benefit of the doubt to the claimant. If there wasn't a recount, we would assume that that was, in fact, a reliable and good measurement. We would use that to assign internal dose from thorium to that worker.

DR. LIPZSTEIN: Is that a reliable measurement? That's my question.

DR. MAURO: Yes, I think we have just nailed down the question. That is what you always have to do.

Let's talk a 35-milligram person. What I heard is that it is possible that that

35-milligram number, which is very high, no follow-up. If there was follow-up, it may have come down because it was contamination or it could have come down because, I heard also that one of the confounding variables is the presence of radium progeny. I guess it somehow contributes in the follow-up degrees of interest. It could also give you a false-positive.

So, if the issue is that we don't believe the numbers could have been that high, for a variety of reasons -- and NIOSH's position is, well, that's okay, we're going to give them that. It probably is too high or it might be too high; we don't know.

So, that puts us right back into that same arena. Now do you know this number with sufficient accuracy in order to -- granted, that you might be giving the person the benefit of the doubt, but is the uncertainty so great that, you know, can you make a compensation decision on that?

For him, it certainly would be, one would argue that that is, in fact, an overestimate. It is certainly giving the person every benefit of the doubt. But can you make that decision for an SEC? In other words, say that we can calculate doses with sufficient accuracy?

I think that that is, as we heard from Sam, very much a policy and interpretation of the Part 83 that really goes to a bigger arena.

MEMBER SCHOFIELD: I have got a question. At what point do we decide we are going to give a person a dose from plutonium, thorium, whatever, because we know the stuff existed in a particular area? But what measurements were done on them were below MDA. So, are we going to give them all partial dose from these other things or not? I mean, you know, a person could be exposed to thorium and uranium and plutonium, all there.

MR. ROLFES: Right. In this

program, if a person is monitored and has bioassay data for any and all of the above radionuclides, if they are monitored for uranium, thorium, et cetera, unless we have information that the person definitively was not exposed, we would definitely assume that that person was exposed. If they were routinely monitored for those radionuclides, we would assign dose from all the radionuclides that they were monitored for.

Now, to clarify a little bit, you know, for fission products, we might not assign internal dose from all fission products at a reactor site, for example. We would make a judgment as to what fission products would give the highest dose, and we would assign dose from that particular fission product or mixed fission products.

But if an individual has monitoring, if they are positive results, we would definitely calculate an intake that explains those positive bioassay results. If

those bioassay results were less than the minimum detectable amount, we would assign missed dose.

What's the highest missed dose that we can assign that would result in a value, an excretion value, that was half of the minimum detectable amount, is essentially what we would do.

So, this is no different than assigning for external exposures for people who were monitored using film badges. If they had a zero on their badge and the limit of detection was 20, we would assume that they routinely received a median value of 10 millirem per badge exchange. And that 10 millirem could have been as low as zero millirem or up to 20 millirem for every badge cycle. So, we are doing the exact same thing with internal dose here.

MEMBER SCHOFIELD: Are we going to assume only bone-seekers or a missed dose?

MR. ROLFES: No. No, the bone

cancer example, the bone surface example is the sample, it is one of the worst-case organs where thorium progeny concentrates.

MEMBER SCHOFIELD: Yes, but, I mean, if you have got plutonium, you know, some of that is bone-seeker, too.

MR. ROLFES: True. True, it is. The organ of concern is the organ where the cancer originates, that we are reconstructing the dose for.

If it is a prostate cancer, for example, we would calculate the dose to the prostate. There are very few radionuclides that significantly concentrate or impact the prostate tissue.

So, the sufficient accuracy, although dose is important, it is really the biological mechanisms and type of cancer that you have that also play a major contributor into a compensation decision.

DR. GLOVER: I have one real quick. The factor of five or six that you

calculated regarding the disequilibrium factor, would that also affect the MDA calculation? Will we use that? Will we actually have to increase the MDA to deal with that?

MR. ROLFES: I'm trying to think here.

MR. BARTON: That is only for lead-212.

MR. STIVER: It is all based on lead-212 measurements.

DR. GLOVER: It wouldn't apply to the --

MR. STIVER: I would like to make a follow-up statement to what John was saying about this fictitious 35-milligram intake. And Mark has made some points about kind of whether you have to look at the individual and the cancer and the effect on the compensation decision.

But I think the sufficient accuracy becomes more and more important at

these situations where you have highly-uncertain values that could take on very high doses, because it becomes a matter of fairness in compensation decisions, too.

This guy may end up with a whopping-big dose for his particular cancer where he would get compensated, and somebody else who has another measurement that came out may have the same intake, but because of the uncertainty in these values, is going to get a slightly lower one, is not going to be compensated.

And so, you get closer and closer to that POC that there is a payoff point. I think this sufficient accuracy becomes more and more important, just in terms from a policy standpoint; whereas, maybe down in the 10-20 milligram range or some other lower value, it really doesn't impact, it doesn't get you close to that level. It doesn't seem to have as much importance. I just wanted to make that --

DR. MAURO: And this goes to the coworker model because I could envision a coworker model for that time period where you collect all these milligram numbers. And let's say the highest one is that 35, or whatever. And you are using all of these numbers, most of which might be, many of which might be fictitious; I'm not sure. And you build a coworker model. Let's say the full distribution starts at 6 and goes to 35, whatever, or the 95th percentile is up around 25 or 30, whatever.

Along comes a person that wasn't bioassayed or chest-counted for thorium, but you believe he might have been exposed to thorium in this time period, and you are going to assign to him this number, which is quite a high number that in itself is almost like a coworker model that is really based on -- it doesn't have a very good foundation. It is almost like you are building a house on a very poor foundation.

And I think everyone agrees that these numbers are kind of soft that we are hearing. You know, the uncertainties in these numbers are very questionable and they are very large.

And then, to build a coworker model on top of that, and build your whole decisionmaking process on compensation on such a weak foundation troubles me.

CHAIR CLAWSON: Thanks, John.

Sandra wanted to make a comment.

MS. BALDRIDGE: Mark was talking about information that is used to determine that they had no exposure. What kind of information?

MR. ROLFES: For example, in a hypothetical scenario, if an individual had some lung counts, but they wrote down that this employee worked offsite and was brought onsite to represent a control count, for example.

MS. BALDRIDGE: And that is the

only --

MR. ROLFES: That is just a hypothetical thing. In just about all cases, we would assume that the person had exposure.

MS. BALDRIDGE: Okay.

CHAIR CLAWSON: Okay.

MR. KATZ: I was just going to suggest, I don't know -- do you know where you are going with this?

CHAIR CLAWSON: To tell you the truth, I am pretty well done with it because I haven't seen anything to be able to express to me that these are good. Myself, I would push right now to push it to the Board in the way of an SEC, is what I am going to do. So, the Board can then start to deal with it.

But we have been dealing with this for over two years, and I can honestly say that we are not any closer than when we started out two years ago.

So, I have got two other Board Members in here that need to weigh in on their

thoughts, but I would like to bring this before the Board because I am sure that they are going to have something else to come out of it. They are going to have their questions into it.

We are not getting any closer. And when we start getting into this sufficient accuracy, and so forth, that is above us.

So, Paul, I guess, and, Phil, I am wanting to know what you want to do?

MEMBER SCHOFIELD: It seems like to me we have come to a point where we have almost agreed to disagree.

CHAIR CLAWSON: All right. And in that case, this is what I am saying: that I would bring it before the Board under an SEC. If they want to change it or they need more information, or the significant accuracy comes into this, this is for the whole Board to be able to decide. It is not for us to be able to decide this.

And, Paul?

MEMBER ZIEMER: Well, I don't, in a sense, disagree with the fact that we haven't shown that we can reconstruct dose with sufficient accuracy on this case, but I don't think we have shown the opposite, either.

CHAIR CLAWSON: Well, Paul, that is where I'm at.

MEMBER ZIEMER: Because some issues have been raised here in the last couple of days that there are some strings left hanging that haven't really been pulled.

I am still struggling, as I study table 1, for example. Maybe I missed the point, but if the thorium is actually calculated from the lead-212 measurement, is that how it is done procedurally?

MR. ROLFES: Yes, from my recollection, we need to get a reference for that, but I do believe they had considered the actinium-228 photopeak to make an understanding of how old the thorium to which

the individual was exposed. I will have to go back out and pull --

MEMBER ZIEMER: Yes. And if that is the case, then, John, I am saying, yes, then, I ought to be able to see some correlation because the one value is based on the other one, with some kind of a scaling factor.

MR. STIVER: Well, I think Joyce brought up a good point. When you have actinium and lead values that are greater than detection limit, you should be seeing a correlation.

MEMBER ZIEMER: Well, that is what I am saying.

DR. LIPZSTEIN: They could have measured the thorium-232, too. We don't know. That is another way to measure it.

MEMBER ZIEMER: Well, that was what wasn't clear to me, whether these are all done the same way that are in the table. Or is this a mixed --

MR. STIVER: This is to show that the one year where you actually have two types of measurements combined, that is the only time where you actually have any way that you could possibly relate one to the other.

MEMBER ZIEMER: Right. Right.

MR. STIVER: And even when you have data for the two progeny radionuclides, you still don't see a correlation to the thorium milligram value.

MEMBER ZIEMER: Yes. Yes.

MR. STIVER: And so, to us, that calls into question whether lead-212 was, indeed, used to make that measurement in that earlier time period.

MEMBER ZIEMER: Or how it was actually done.

MR. STIVER: Yes. I mean, we can't make much more of an inference beyond that, which is to show that this casts doubt on that assumption.

MEMBER ZIEMER: Yes. Do we know,

Mark, in the earlier years how the thorium value was actually obtained?

MR. ROLFES: That is what I am going to have to get back to you on. Once again, we have previously looked at the Hap West document, which shows information on how to interpret thorium lung burden based upon the -- I can show you a little picture. It has information on the in-vivo screening techniques, and it shows both actinium-228 and lead-212 photopeaks.

But I believe the thorium, I think this was more towards quantifying how old the thorium to which the individual was exposed, and not necessarily --

MEMBER ZIEMER: Based on the size of the peaks?

MR. ROLFES: Correct, the ratios between the area under the peak.

MR. STIVER: See, that is how you would get back to the actual thorium intake.

Sorry.

MR. ROLFES: But we have developed an alternate approach. If we are only using the lead-212 peak, we have developed an alternate approach to show the value of 5.25 would be the worst-case scenario.

So, I have promised to get a reference. Let me see if I can look while we move on and see if there is anything.

MR. BARTON: While you are looking, that five measurements in one day, I tracked it down and that is transcription error in the original database. They are all different dates, different years even. So, there is no person with five measurements in one day.

CHAIR CLAWSON: This whole thing comes back to, you know what? We are dealing with so much data out there that we really can't represent. We can't go back and really pull up. Because my question would be right now, then, I want to know exactly did they use the 2.10 or what they did? And I don't think

we can really come to an answer with that.

MR. STIVER: We have certainly found no evidence to indicate that in our research, that the lead-212 was used in the earlier period. This is an assumption, in our minds, that NIOSH has used.

Well, when you have those data in later time periods, then, yes, you can use that to bound the uncertainty. But before that period where we just don't know what was done, we don't know which nuclide was entered -- in fact, we do have one example that was an actual calculation for a calibration that was using the actinium and not the lead. It doesn't provide any kind of definitive proof one way or the other, but it does cast doubt, additional doubt, in addition to what we have in table 1, that maybe that might be culpable.

Our real problem here is that are big, big uncertainties that just cannot be quantified.

MEMBER ZIEMER: Well, I am not

prepared to sort of vote today on this, but I am hopeful, by the time of the meeting, maybe we will have at least some answer to how this was done, Mark. That would certainly help me to kind of critique --

CHAIR CLAWSON: Well, let me ask you, then --

MEMBER ZIEMER: Because it is going to come down to whether or not you can -- well, two things. One is, how are you handling the individual cases? And then, No. 2, can you use this for coworker data or not?

I had it in my mind that you could just take three distributions of each one separately. But if they are not correlating, then it makes me a little nervous about how reliable those data are, if one is used to calculate the other.

MR. STIVER: Yes, for the method to work, they would have to be correlated instead of an a priori.

MEMBER ZIEMER: Well, correlated

within the understanding that you have different aged thorium, that you have different --

MR. STIVER: Yes, that is another aspect.

MEMBER ZIEMER: -- biological systems for the people that are handling the stuff because it redistributes in the body.

MR. STIVER: Right.

MEMBER ZIEMER: But, nonetheless --

MR. STIVER: Yes, I think we did bring out that we thought, based on our analysis of one of the earlier reports -- it may have been the July 2010 report -- that we felt that the biological translocation aspects, there are enough studies that that could be quantified. But it is still going to be a large number. It is going to vary from one person to the next.

But the real kicker here, the real problem is you just don't have any anchor

point to get from what previous measurements were used to get to that milligrams-of-thorium value in an early time period.

MEMBER ZIEMER: Yes.

CHAIR CLAWSON: Where do we go from here?

(Laughter.)

MEMBER ZIEMER: Well, I think you are going to make your recommendation to the Board and tell them what the dilemma is.

I think Mark is maybe going to give us any more recent information that will --

MR. ROLFES: Yes, we have committed to preparing a formal response to SC&A's paper that was just recently delivered to us. I mean, this is all draft information that has been developed within the past two weeks, but we are trying to respond.

CHAIR CLAWSON: And then, SC&A is going to respond to what NIOSH has, that we just got today?

MR. STIVER: It depends on if Mark can give us --

MR. ROLFES: It is going to be a tight schedule. I mean, we only have three weeks until --

MEMBER ZIEMER: Well, the other reality is that Board Members will end up getting a lot of this stuff at the last minute, and they are going to object to voting on anything on short notice. Because this whole site is hard enough for us.

CHAIR CLAWSON: Well, I am throwing out this: what do we want to do? Do we want to just bring an update to the Board? But, you know, we have been at this for a long time.

MR. KATZ: Well, I mean, it is sort of before the Board already, I mean in general. Fernald has been before the Board for six months now. So, they have it.

I think you can bring, I think you can work together and bring them an update of

where you stand, where everyone stands on this issue, including the Board Members. And so, they will know exactly where SC&A stands, based on the information, and you will have at least seen everything that you have gotten from Mark, that you will have gotten from Mark in the next week, or whatever.

And vice versa, DCAS can lay out their current point of view on all of that and put it before the Board.

The Board can decide to move ahead without having more Work Group discussion, or what have you.

CHAIR CLAWSON: Well, part of my thing is, Paul, what I want to be able to do is, to me, we are really at an impasse here. But it is a much broader question than just this. It starts to get into -- and we always beat up on this, one side of plausible, and so forth. And I want them to understand what kind of an issue we are dealing with on this one.

MEMBER MELIUS: This is Jim Melius.

I mean, somehow I am failing to see how this is such a huge issue because we have certainly dealt with it before. What I would suggest is that you lead the update on what the work that we are doing, it should lead with this issue. And let's focus on that. We can include, if we have time, an update on the other issues, but lead with this issue.

Get the SC&A report and whatever background with that out to the Board Members now with some note to the effect that this is going to be discussed at the meeting, and be ready.

When NIOSH has its response prepared, we can send that out. I sent an email to Stu and Jim Neton asking them to expedite the NIOSH response.

I think it is important that we get out something in writing because I think

it is helpful, rather than just a verbal report on what is happening. And then, they can elaborate on it in terms of presentation.

But I think it is manageable, and I don't see anything that would be gained by putting this off any further in terms of direction. Now maybe that is what the Board will decide, that they need more information. But let's identify that at a Board meeting rather than you --

CHAIR CLAWSON: Okay.

MEMBER MELIUS: -- wrestling with trying to guess what that might be, and so forth.

CHAIR CLAWSON: And that was what my issue was, Jim. How do I bring it before the Board and make sure that they are getting the information that they want now?

So, we will just plan on bringing this before the Board at the end of this month then.

MEMBER MELIUS: Yes. And you can

decide -- I don't think you have the information to make a recommendation at this point. If you want to hold another Work Group meeting before the Board meeting, I guess that is possible, but I am not sure that it is going to be possible to estimate with sufficient accuracy when NIOSH's report will be available to you.

(Laughter)

So, I am not sure you gain from that. But that is something that you, as a Work Group, need to consider.

MEMBER ZIEMER: Don't expand the use of "sufficient accuracy".

(Laughter.)

MEMBER MELIUS: But I couldn't resist, Paul, the discussion.

CHAIR CLAWSON: Because I was under the impression that I have to bring, the only way I could bring it before the Board and have the full Board discussion was in the context of an SEC. So, we can bring it up

there. And if they want to make a decision from there, then that comes down to the Board.

MEMBER MELIUS: Again, I could be wrong. I have been listening to 90 percent of what has been going on today. I am not sure I see -- I guess there are two options, and I think it is sort of what Mark was proposing in terms of some -- I don't want to exaggerate -- but some sort of arbitrary value that would deal with this thorium measurement issue for the non-detects. Or it is an SEC, because you can't measure with sufficient accuracy now.

I guess there may be other options. I may be missing something. But it seems to me it is one or the other.

But I think we should give NIOSH a chance to -- they already are in the process of responding certainly on the technical level. I think we need to look at that.

CHAIR CLAWSON: Okay. Thank you, Jim.

MEMBER MELIUS: Yes.

CHAIR CLAWSON: Go ahead.

MR. STIVER: This is John.

I just wanted to say that, from SC&A's standpoint, our position really can't change until we see Tom LaBone's White Paper. I think it is probably the next thing we need to look at. So, if we could get that before the meeting, it would help us to prepare.

MR. ROLFES: That one has been drafted and should be available in the near future. I mean, that should definitely be out. That should probably be the first thing that we have available.

MR. BARTON: Was there some confusion? I thought that we established that was about the lead-212 measurements and not the milligrams of thorium.

MR. STIVER: There are some aspects of that, but there is also some other components in that. I am just not 100 percent sure as to what it -- I think most of it is related to lead-212, but I would like to see

that before we can any pronouncement.

MR. ROLFES: He also tempered from that with the reference regarding the use of the --

MR. STIVER: Yes, if the are references that would indicate lead-212 was used in the early days, that would certainly be helpful.

MR. ROLFES: I think, basically, we had previously responded to this. I will have to dig back through my notes. But the limit of detection really didn't change from 1968 up through 1987. It was still 6 milligrams. I think the calibration methodologies essentially remain the same, too.

So, I will have to get back to that to pull out where we got that information from.

MR. KATZ: Okay. Brad, will we have John present at the Board?

CHAIR CLAWSON: Yes.

MR. KATZ: This is awfully complex.

CHAIR CLAWSON: Yes, it certainly is.

MR. STIVER: We will take the responsibility --

CHAIR CLAWSON: Both he and Mark are going to explain that because, then, they can address it.

But, with that, I think we need to take a comfort break. We could go for 10 minutes.

MR. KATZ: That would place it around 4:00 getting back together.

CHAIR CLAWSON: Okay.

(Whereupon, the foregoing matter went off the record at 3:50 p.m. and went back on the record at 4:01 p.m.)

MR. KATZ: Okay. We are back after a short break, the Fernald Work Group.

CHAIR CLAWSON: Okay. We have decided that SC&A is going to bring this

before the Board, and NIOSH is going to bring their part before the Board.

But I have one question that I need to get on the record. We have been focused on 1968 through 1978.

MR. KATZ: Through 1977.

CHAIR CLAWSON: Through 1977. My issue is, what have we done for 1978 on? Have we reviewed that?

MR. STIVER: We have not reviewed the data that are available as backup to the measurements of that particular point, no.

CHAIR CLAWSON: Okay. So, my question is, as the Work Group Chair, because I know that we got focused into this and we have been focusing just at that, I guess I would want to see --

MR. STIVER: If you would like us to do that, we certainly can.

CHAIR CLAWSON: Okay. I just want to make sure. And do we have to task or is that under their --

MR. KATZ: We just did.

(Laughter.)

CHAIR CLAWSON: Okay. Okay, we've got that.

MR. STIVER: And that is going to dovetail well with Tom LaBone's information on lead-212.

CHAIR CLAWSON: Okay. That sounds good.

Then, we are going to move on to the next item, which is the recycled thorium.

MR. STIVER: Okay. This is John Stiver, and this is our last issue for the day.

This is a new issue that emerged. It is related to recycled thorium. It is kind of similar in a way to recycled uranium in that you have material, thorium, that was irradiated at Savannah River and at Hanford, chemically purified, and then sent back for reuse.

We weren't aware of this

particular issue until the Savannah River site teleconference last August. We discovered that a lot of this material had been shipped up to Fernald. And so, we thought, well, you know, we should probably take a look at this, although you are dealing with contaminants that are essentially isotopes of uranium as opposed to plutonium or some others. And you also have fission products there.

But the big players in recycled thorium are uranium-232 and uranium-233. And so, we wanted to investigate the extent to which the presence of these materials might require some changes -- first of all, whether it would be possible to reconstruct and, also, if so, what changes might be needed.

So, this is really kind of an interim report, as you will see. There are some recommendations that come out of this.

But what we did is we went through the SRDB and we pulled out, oh, gosh, upwards of 40 or 50 references related to this

particular topic, and sorted through all those and came to the conclusion that, during almost the same period of time where we are dealing with the milligrams-of-thorium data, from about 1968 on up to the late 1970s, we had these shipments of recycled thorium come in, which is the period of thorium processing. That is understandable.

So, any pronouncements on the usability or the ability to reconstruct, or even the need to reconstruct, recycled thorium is predicated on the ability to have a reliable, credible thorium intake estimate. So, this pretty much hinges on the 1968-to-1978 issue with the chest counts.

Having said that, we determined that literally hundreds of metric tons of this material had been received at Fernald, like I said, from Savannah River and, also, from Hanford during this time period.

We were able to find a study. I will just direct your attention back to our

paper. It is called "The Evaluation of the Impact of Recycled Thorium on Potential Worker Exposure at Fernald, an Interim Report".

CHAIR CLAWSON: And this report is, as we speak, it came to us, right?

MR. STIVER: Yes, this is the report that you have, as of last night, and my apologies for the tidy arrival on this. We need to get better at time management.

As far as the source term goes, we found a very good reference. It was Quigley, 1967. This is an ANL report where they were basically trying to determine whether they could process this material at Fernald without any changes to their system.

What they did is they had six tank cars of this recycled thorium that were brought in from Savannah River. They sampled all the different tank cars, because they were looking at whether this was a feasibility, it was kind of a feasibility study. And so, they wanted to get the highest values possible and

say, hey, in the worst-case scenario, are we going to be able to do this without having to change-up our processes?

What they came up with is, they were looking at primarily thorium nitrate tetrahydrate. That is basically an aqueous solution of thorium. This is how this material was received. And then, from that point, it would be processed through, as any other thorium shipment would be.

That is all laid out very well in several other documents. We won't go into that here.

So, this table 1 you see on page 5 of 20 gives the constituent concentrations in the Savannah River site, thorium nitrate tetrahydrate, which we call TNt. That is our acronym for it.

And you see you have got thorium-232, -234. No tactiniums are coming in. Ruthenium, it should really be ruthenium-106, not ruthenium-108. I think

that was a typographical error. And then, U-233.

MEMBER ZIEMER: In the original report.

MR. STIVER: In the original report, yes. It is in the original report.

And so, we have activity ratios you see over in column 3 relative to the thorium-232. You do the ratios; you have activity ratio.

You can see that U-233 comes out to, on an activity basis, about 21 percent of the thorium concentration or the thorium activity, I should say.

This is probably a fairly recent sample because the short-lived progeny, the thorium-234 and tactinium decay away very quickly. We have a little excerpt that describes that.

What we didn't have here is measurements of the U-232. U-232 is important because it is a very high specific activity.

Basically, it starts with thorium-228. And so from that point down, you have basically the same decay chaining as you have with the thorium-232 after it reaches equilibrium.

So, if you have a high concentration of this material, you can have very high external doses due to primarily thalium-208, .6 MEvs gamma. You have the same thing with 232, but because the parent radionuclide is such a low specific activity, the radiation hazard from the health protection standpoint would be lower for an equal amount.

But, anyway, we looked at the external dose potential and internal dose potential of this material. The external potential, given that -- well, let me back up a second.

We were actually able to find another document, several documents really, that discussed the production, how to control these undesirable side reactions that gave

rise to the uranium-232. What they did was, by a placement in the reactor lattice controlling the irradiation types, they were able to control the amount of this material that was produced.

Basically, they controlled it, depending on the AEC specifications, we found information that indicated anywhere from about 7 up to 500 parts per million on a U-233 basis.

So, based on that, we were able to go back here to table 2 on page 7, under the internal exposure potential, you see you have got three different concentrations, 500, 50, and 7 parts per million, and what the activity concentration ratios would be relative to thorium.

And you see the worst case at 500 parts per million, we are looking at actually about a .25 activity ratio. So, when you look at that and you consider the fact that thorium-232 basically is present in four times

the activity concentration, we went through and demonstrated here in the external exposure section that the external potential from thorium is going to vastly outweigh any hazard from U-232 and -233 that might be present in the material. And we reached the conclusion that really the presence of the recycled thorium at Fernald really didn't contribute appreciably to external dose potential.

We also looked at the external potential as well, using these activity concentrations, kind of a worst-case scenario. You look at table 3 here on page 8; you can see that we have activity-weighted ratios for Type M and Type S. The material received was predominantly Type M, the nitrate solution, but it was also processed in further steps to oxides, fluorides, and eventually to metal, in a similar process to what was done with uranium.

So, we include these values here just to show the ratio of U-232 and -233 to

thorium-232 for the range of organs of concern in the ICRP 68 dose factors. And you can see these very low values. The highest, again, is for lungs for about .42 and .48. But, for most of the organs, we are looking at about 3 to 5 percent of the dose ratio. I mean, these are 50-year. We certainly kind of looked at it that way. Being an interim report, we thought we would just take a little broad-brush-stroke here.

Table 3, that was just a carryover.

Table 4, what we did here is we took -- to get an idea of what the doses would be for a particular worker. Joyce located a particular worker who had two chest counts.

I will back up to say we did use the chest counts for this example without regard to the adequacy. I have made a statement, kind of a caveat, in here, that this doesn't imply any kind of acceptance of the values for use in DR. We just thought it

would be illustrative as a demonstration.

And so, we know that between April 1968 and July 1969 this particular worker got an intake of thorium-232. His measure was zero. His second was 9.1 milligrams.

And so, in kind of a similar vein to what we did in looking at the in-vivo thorium, based on the 6-milligram intake or lung burden, we looked at what would be some exposure scenarios. What kind of doses would you expect from an intake that would give you a 9-milligram lung burden over that course of time?

And on page 10, you can see we have three different tables here that look at three different scenarios of when the intake may have occurred and when measurements were made. You can see, once again, we looked at high-dose organs, the bone surfaces, and we looked at lung as well.

You can see the thorium just far outstrips uranium, both isotopes of uranium,

in terms of dose. Here we are looking at sieverts.

Take a look at table 1a, where you have a July 1968 intake. Measurement was done 235 days after last day of exposure. You have got about, to the bone surfaces, you are looking at 14.5 sieverts and you are looking at 9.8 rem from U-232, 5.7 rem to the lung. And you can kind of see you have got the same type of proportionality here.

So, our conclusion is really that thorium-232 internal doses far outstrip dose from 232 and 233. However, you do still find rem-level doses, possibly rem-level doses, to certain organs from the uranium contaminants.

So, our recommendation at this point is that NIOSH may want to further investigate this issue in assigning internal doses from thorium. If the in-vivo thorium issue becomes resolved, then this would be kind of a follow-on to that.

That is really kind of the

thumbnail sketch of this interim report.

MR. ROLFES: Thanks, John.

This was the first time I was able to look at the document, today.

(Laughter.)

So, I am looking at it, and the only thing, the one question that I had, in your dose calculations here, 1a, 1b, and 1c, the bone surface committed equivalent doses in sieverts for thorium, and then U-233 and U-232, were those doses calculated in IMBA?

MR. STIVER: They were done using aids.

MR. ROLFES: Okay.

MR. STIVER: Joyce could give you the details on that --

MR. ROLFES: Okay.

MR. STIVER: -- if you are interested.

MR. ROLFES: Thanks. No, that's all I need.

MR. STIVER: Okay.

MR. ROLFES: Thank you.

MR. STIVER: So, I have to say, on this particular issue, if anybody has some comments, why, I will entertain them at this point.

CHAIR CLAWSON: This is all hinging on the data we have already been looking at?

MR. STIVER: Yes, this is based on the 1968-to-1978 timeframe and process.

CHAIR CLAWSON: So, for you to do this, you actually used some of the information that was existing there or --

MR. STIVER: This is an illustrative example. This is not any kind of a calculation we would use in a dose reconstruction. This is just to get an idea of what the relative magnitudes --

MEMBER ZIEMER: Yes, but you are saying that, if you can quantitate the thorium to start with, then you should add this?

MR. STIVER: If you can quantitate

the thorium, then you might want to look at this because you conceivably have rem-level doses.

MEMBER ZIEMER: Right.

MR. STIVER: Remember, those are 50-year increments.

MEMBER ZIEMER: Right.

MR. STIVER: It might be worth looking into as a TBD change.

CHAIR CLAWSON: So, this will go to DCAS?

I would say the other one was a priority over this, though, because this is all hinged on that data. So, I guess the Work Group will expect a response back from DCAS on that. But I want to put the emphasis on the other information, because this is what it all hinged on.

MEMBER ZIEMER: Yes, this would become a moot point if --

MR. STIVER: Yes, exactly, it would become a moot point if the fact is that

the other data can be used.

MEMBER ZIEMER: Right.

CHAIR CLAWSON: Are there any questions on it or anything that needs to be brought up?

MEMBER ZIEMER: So, you have confirmed, though, that recycled thorium was present on the site?

MR. STIVER: Yes.

MEMBER ZIEMER: That has been confirmed.

MR. STIVER: It was processed through Fernald, based on that study that they used to generate that table.

CHAIR CLAWSON: How many metric tons?

MR. STIVER: This is another issue that, if it becomes a TBD revision, then we know that hundreds of metric tons were processed. We don't have an exact value, but that would be incumbent on NIOSH to go back and get a better, more quantitative timeframe

and amount. I don't know if the timeframe or the amount would be so important. Once you have the thorium, then you have the proportionality. Basically, you have the default. So, you would use like you did in recycled uranium.

CHAIR CLAWSON: Because part of this came up when we were at Hanford and going through that paperwork. I saw railroad cars of thorium going out.

And so, I guess we really don't have a good gist on that, but that falls back to NIOSH or DCAS to go from there.

And that is all we have today.

If there are any questions on the phone of any clarification that we need or a path, I want to make sure that everybody is clear with the path forward, though. You have got your path forward.

I would like to be able to review, when you get back to your offices, to be able to send it to us, so that all of us know that

we are on the same field of which way we are going.

MR. ROLFES: If there is one issue I guess that you would like to have before the Work Group meeting, then my thoughts, from what I have heard today, it would be the thorium lung counting from the 1968-to-1977 period?

CHAIR CLAWSON: Yes, that is first and foremost.

MR. ROLFES: We will focus our efforts on that, to get something put together before the Work Group, the full Board meeting. We will do our best to do that.

And then, second to that would be the subcontractor --

CHAIR CLAWSON: That is correct.

Both SC&A and DCAS have delivered papers fairly late that neither side has been able to really review. So, as usual, we still need to have a formal response on both of those, all the papers that have been put out

there.

But, as Mark stated, we want to go for, we are shooting for thorium and the coworker data first for the Board.

Is there anybody on the phone who has any questions or any clarification that we need?

(No response.)

MR. ROLFES: One other thing that I forgot to mention is we did do an analysis. We were asked, as a Site-Profile-type issue, to analyze the net effect of the blunders or the daily weighted-exposure results for thorium. We have completed an analysis. If you correct the blunders, the change in intake, I believe, is less than 1 percent.

MR. STIVER: So, it is considerably less than it was for Weldon Spring, then?

MR. ROLFES: Correct. Correct. Just because of the additional --

MR. STIVER: I haven't had a

chance to look at it yet.

MR. ROLFES: I sent it this morning. So, I didn't expect that anyone would, but that was completed. So, I just mention it.

MEMBER ZIEMER: Did we all get that?

MR. ROLFES: Yes.

MR. KATZ: Just today.

MR. ROLFES: It was just this morning.

MEMBER ZIEMER: Oh.

MR. ROLFES: I sent it probably a few minutes before our meeting started.

MEMBER MELIUS: Yes, this is Jim Melius.

Just one, I guess it is sort of an announcement, but at least the draft schedule for the Board meeting as this Fernald being discussed on Wednesday morning, February 29th, roughly at around 10:45. Now that could change as we sort of finalize the schedule.

But if it doesn't, at least people who are interested should know that.

MR. KATZ: Right. So, that is the second day.

MEMBER MELIUS: The second day of the meeting, midmorning.

CHAIR CLAWSON: Okay. Thank you, Jim. Appreciate that.

With nothing else, the Fernald Work Group will sign off then.

MR. KATZ: Very good. Thank you, everyone, for your hard work again leading up to this and through this, and for all of that to come to the Board meeting. It is much appreciated.

Take care, everyone on the phone.

(Whereupon, the above-entitled matter went off the record at 4:22 p.m.)