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ADVISORY BOARD ON RADIATION AND WORKER HEALTH

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FERNALD WORK GROUP

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THURSDAY
AUGUST 11, 2011

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

PRESENT:

BRAD CLAWSON, Chairman
MARK GRIFFON, Member
PHIL SCHOFIELD, Member*
PAUL ZIEMER, Member*

ALSO PRESENT:

TED KATZ, Designated Federal Official
SANDRA BALDRIDGE
BOB BARTON, SC&A
ELIZABETH BRACKETT, NIOSH*
MEL CHEW, ORAU Team
CHRIS ELLISON, DCAS
SAM GLOVER, NIOSH
KAREN JESSEN, NIOSH*
TOM LaBONE, NIOSH*
JENNY LIN, HHS
JOYCE LIPSZTEIN, SC&A*
JOHN MAURO, SC&A*
ROBERT MORRIS, NIOSH*
GENE POTTER, NIOSH*
BRYCE RICH, ORAU Team
MARK ROLFES, NIOSH
BILLY SMITH, NIOSH*
MATTHEW SMITH, NIOSH*
JOHN STIVER, SC&A

*Participating via telephone

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

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

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

9:02 a.m.

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

This is the Advisory Board on Radiation and Worker Health, the Fernald Work Group. We are just getting started here.

(Roll call.)

MR. KATZ: Okay, we're back.

This is the Advisory Board on Radiation and Worker Health.

Sorry to everyone on the phone. We had a dysfunctional phone, but I think we are all set now. And we have finished roll call.

Let me just remind folks on the phone to mute your phone except when you're addressing the group, *6 if you don't have a mute button, *6 again to unmute.

And it's your agenda, Brad.

CHAIRMAN CLAWSON: Okay. I guess, first of all, can everybody hear us okay, just to check in?

DR. MAURO: I can. This is John.

MR. KATZ: Oh, great.

CHAIRMAN CLAWSON: Thank you, John.

MR. KATZ: One last thing, Dr. Ziemer, you are on the line now? Are you on mute, Paul?

MEMBER ZIEMER: Yes, I was on mute. Sorry.

(Laughter.)

MR. KATZ: Okay. No, just glad to have you. I just wanted to make it official. Thank you.

MEMBER ZIEMER: Thank you. I'm on the line.

MR. KATZ: Okay. Thanks.

CHAIRMAN CLAWSON: Okay. To bring everybody up-to-speed a little bit, we're just finishing up the last parts of the Fernald Work Group. We've got an agenda that has been printed out on August 11th which I hope that everybody has.

The first issue we are going to talk about was a NIOSH response that we need to find out where we're at. This is the coworker model for uranium exposure for Fernald construction workers. Do we have anything on that yet, Mark?

MR. ROLFES: Yes, Brad. Thanks. And, yes, we were trying to work as fast as we could and try to get a response in for this Work Group meeting. I have a draft report, and I can provide the update to you.

I am probably going to rely upon some help from Gene Potter possibly if there are questions. He is on the line, I believe.

Basically, what we have done, because prior to 1986 subcontractor uranium urinalysis data was not in the electronic database HIS-20 for Fernald, we went back and compared hard-copy subcontractor, construction subcontractor uranium urinalysis results. And there was a concern expressed, I think we heard it back in January of 2010, I think was the timeframe that we had first heard this concern initially. So, this was something that we sort of looked into well into several years after the petition was initially received.

We have completed an analysis where we have compared that hard-copy uranium urinalysis data from the subcontractors at Fernald to the hard-copy urine samples for the full coworker population, the OTIB-78 population. Well, I take that back.

The subcontractors were actually sampled, and there was a code of Type 50 which designated it as a special sample. Just about all of the subcontractor urine samples were designated as special samples. So, what we did, we went back and compared special samples from full-time Fernald employees to compare the excretion rates. We did this for years in the 1960s, seventies, and eighties to see if there was any difference in the subcontractor excretion rates versus the full-time Fernald employee excretion rates. And it turns out there is a difference.

Based upon the year, we have gone through and analyzed the difference for each year, and NIOSH is proposing now, based upon our analysis of the comparisons of the data, we are proposing to assign intakes to the construction worker/subcontractor work group population. If they are unmonitored, we will assign an intake equal to two times the intake of the OTIB-78 values.

So, it turns out that there was a valid concern that the uranium urinalyses appeared to have higher excretion rates. There's many reasons behind this. It appears that subcontractors, when they were sampled, were sampled at the end of a short duration of work.

And so, there are some reasons that would explain the higher excretion rate, one of which is possibly contaminated samples.

However, we have no information -- if it is a contaminated sample and we are not sure about it, we would use that to the benefit of the doubt for the claimant in the dose reconstruction process. So, that is one possible explanation for the higher excretion rate.

The other possibility is that some of these samples that we have, the person could have only been on-site for two weeks, for example, and might not ever come back to the site. So, Fernald health and safety people, in order to make sure that they could get a uranium urinalysis sample from the individual, they would get that sample whenever they could before the person left. So, they tried to collect as much data as they could while that person was on-site, so they could get something. And so, that is one of the possible explanations as well for the higher excretion rates as well.

You know, it could be, also, that

short-term subcontractors may have been brought in for a potentially dustier job and could have had a higher short-term duration exposure. So, it could have been a higher airborne concentration that they were working in over a much shorter duration.

So, that is our proposal that we have on the board. We have a draft report that I have had the opportunity to take a look at, and we can get that out to you as soon as we have it finalized.

CHAIRMAN CLAWSON: Where is it at now, Mark?

MR. ROLFES: It is with DCAS in hard copy here. We have yet to finalize the report. I've got the majority of it. It should just be a matter of getting the report out in a matter of the next couple of weeks, I believe.

MEMBER GRIFFON: It is hard. I think we will wait for most of the discussion until we see a report, but I am trying to figure this out. You said you compared Type 50, the special sample, special out of the entire HIS-20 population, right, the non-subcontractor data?

MR. ROLFES: Correct.

MEMBER GRIFFON: Do you have any reason to believe that the specials were the same? I mean, did specials mean the same for subcontractors as they meant for the contractor personnel?

MR. ROLFES: Yes.

MEMBER GRIFFON: In other words, usually "special" indicates there is some kind of incident; you might think an immediate one.

MR. ROLFES: Correct.

MEMBER GRIFFON: As opposed to subcontractor "specials" where they --

MR. ROLFES: The special scenario wouldn't have changed definitions based upon who was being sampled. So, the special sample, Type 50 sample, it was the code number of Type 50. That Type 50 wouldn't have changed between the two populations.

MEMBER GRIFFON: But, I mean, the regular contract workers, a lot of their samples were not Type 50. I mean I assume a small percentage of their samples were Type 50, right?

MR. ROLFES: No, just about all of the subcontractor uranium urinalyses were reported as Type 50.

MEMBER GRIFFON: Let me try that again. The contractor -- I might have said "subcontractor" -- the contractor ones, what is the percentage of --

MR. ROLFES: The full-time employees that were on-site at Fernald, not all of their samples would be reported as Type 50 samples.

MEMBER GRIFFON: But I mean, a small percentage or was it a --

MR. ROLFES: I don't know.

MEMBER GRIFFON: Okay.

MR. ROLFES: Gene, perhaps you

might have an answer? Out of the nearly 400,000 uranium urinalyses in HIS-20, do you have a feeling for how many of those might be Type 50 samples from the full-time employees on-site?

MR. POTTER: Yes, we have got those exact numbers. I don't have them right in front of me. Perhaps I can get back to you in a little while.

MR. ROLFES: Yes, I know I put you on the spot, Gene, but --

MR. POTTER: Yes, the comparison, Mark, was we have the subs that we captured in hard copy that were not in HIS-20 and we had other Code 50s that were in HIS-20, and we did a comparison of that group combined with the results from the coworker study which came out of HIS-20.

And the biggest difference we saw was in one quarter for which almost all of the results were non-sub Code 50s, if that gives you any appreciation for what we did.

MEMBER GRIFFON: Sort of, yes. We'll have to wait and see, I guess.

CHAIRMAN CLAWSON: We are going to have to see it anyway.

So, what you are telling me is that the subcontractors that were there got twice the exposure of the full-time people that were there?

MR. ROLFES: That could be. That's very possible. Over short durations, because of the way they were sampled, that's very possible, yes.

We can't find any documents or reasons why the excretion rates could have been higher. So, if we have an unmonitored individual who is a subcontractor, we would basically double the intake rates that we would assign to them.

MR. STIVER: Mark, this is John Stiver.

Maybe it is a little premature to get into the details on this, but you said that you looked at the sixties, seventies, and eighties. Did you see that kind of an offset or kind of a difference throughout all time periods or just in certain periods?

MR. ROLFES: I think the more significant difference was probably during the seventies. Let me see. Let's see. It appears that when you get up into the eighties that the difference between the two populations has decreased, the early 1980s. I would say, you know, well, late 1973 the difference between the two populations was about 1.2, a factor of 1.2 in difference. In 1980, it was a factor of 2; `83, it was a factor of 2; `84, it was a factor of 1.2, and 1985 was .99. So, the subcontractor population actually decreased below the routinely monitored.

MR. STIVER: It would be interesting to see the paper when it comes out.

MR. POTTER: This is Gene again.

Mark, did you mention that, actually, starting in late `85 and from `86 on, the construction subcontractors are in HIS-20, although if they had a code of Type 50, they would have been excluded from the coworker study.

But what is also going on is that you have many more significant numbers of construction types, and the site exposure in general is going down. So, it doesn't look like including the Code 50s after 1986 would make a big difference.

MR. ROLFES: Thank you, Gene.

CHAIRMAN CLAWSON: Okay. We look forward to seeing that report.

The next issue is recycled uranium, and this is SC&A needs to respond to the second White Paper on RU.

MR. STIVER: Yes, this is John Stiver. I will go ahead and start on this.

The recycled uranium, as you recall, we had a pretty extensive discussion at the April meeting. But this is all in relation at this point to the review of our second White Paper, which we were tasked to produce back at the November meeting.

I did, indeed, present to the Board in February. After those discussions, DCAS got tasked to respond to that second White Paper, which they did just before the April meeting. Because of the short lead time on that, we were not able to provide a formal response. However, we provided our initial impressions.

We, then, were not tasked to pursue this issue until the full Board meeting in May. At that meeting, we presented our preliminary observations. I believe it was shortly thereafter that DCAS provided a response, and they also posted some other references that had been uncovered, some spreadsheets, and so forth.

And, then, they provided a paper just this Friday, which is a position paper on what they believe the appropriate contaminant defaults in recycled uranium should be for a different timeline.

Again, we have done a preliminary review of this second position paper. We haven't looked at -- there were approximately 50 new citations presented there. We looked at some. There seemed to be a lot that we already had. There was some new information. But we certainly were not able to do a comprehensive analysis of that.

However, I think we are still in a position to be able to discuss that. I would like to go ahead and just kind of lay out where we are at this point.

I guess our second White Paper, we could really group our eight findings into about two different areas. One was our concerns about the quality of the program at Fernald prior to, you know, it was National Lead of Ohio's tenure before Westinghouse came in 1986.

There are a lot of indications in the record that the program was really geared to more a heavy metals production capacity, that the radiological side of the shop was not really up to par. Now they do have good urinalysis of uranium or the metals work. In about the mid-1980s, about 1985, that all changed as a result of increasing awareness of the importance of recycled uranium, depleted uranium, and some of the other radionuclides.

So, we have some concerns about the adequacy of the program in that early period. We also questioned how DCAS had used the DOE mass-balance report data to set these defaults. I remember, originally, these were set as the basically arithmetic means of these 19 subprocesses. We looked at the data, and we felt that they really were more well-described by log-normal distributions, and in the interest of claimant-favorability and upper limit, that those distributions should be considered.

And finally, we were concerned with the applicability of that data to different subgroups of workers. Which groups were really the most highly-exposed and over what period of time? And how was that data then applicable to those subgroups?

When DCAS came back with their response on April 17th, we were quite pleased to see that we had actually made some progress towards resolution on some of these issues. DCAS did acknowledge that the log-normal distribution was probably more appropriate in ascertaining these exposures. They acknowledged the concentration mechanism which was really one of the prime drivers of the second RU report.

We had looked at on-site dust data that demonstrated that in Plant 5, the metals production plant, there were significantly higher levels of plutonium and neptunium, and some of the fission products, than the earlier proposed defaults.

And a lot of this had to do with magnesium fluoride, and we investigated this further. There really is kind of an interesting physical process going on where about half of the plutonium and neptunium, and presumably other fission products, a partition into the magnesium fluoride slag during the reduction process in metal production. About half of this material is reused from one cycle to the next, and is then sent back to be remilled in Plant 1.

And so, we are really kind of concentrating on those. We know that the metals production is one of the dirtiest jobs and you certainly have the highest dust concentrations. And it appears to also have the highest concentrations on a uranium mass basis of some of these constituents, too.

And so, we investigated this. It appears that, as far as the timeline is concerned, this mechanism would be in place pretty much from `61 on through. As soon as metal production started, you're going to be getting -- the process didn't change over time. As soon as the recycled material started coming in, you started getting this concentration.

And so, we feel that that dataset for the magnesium fluoride is really applicable for the entire period of production. The reason we make that demarcation is because, in about 1973 to I guess in the mid-eighties, there were some of these tower ash and incinerator ash shipments from the gaseous diffusion plants which were considerably higher than the specifications for receipts in feedstocks.

DCAS's position on this was that, well, we're going to use that as kind of a cutoff date; we know that after that point why we have a lot more of this material coming into the plant.

Our position is really that, well, that may be true, but by the time you get to this most highly-exposed group of workers, which are these metal production workers and millers, what they are seeing is this material that came in, it was more highly contaminated. It was down-blended on the front end of the process for the most part.

And so, the goal being to produce derbies that were within spec that could then be shipped offsite. So, the amount of materials that are experienced by these workers would really not be influenced much by the influx of these highly-contaminated materials and feedstocks on the front end.

And so, we feel that what we are seeing in this dataset -- and, granted, it is from `82 to `86, after the most highly-contaminated materials arrived; we don't have any data before that -- but we are fairly confident, based on the information that we have read and the historic accounts, that this material was down-blended before it was ever reduced to metal.

And so, we feel that the defaults, the new defaults that were proposed by NIOSH, this 400 parts per million of plutonium, which is really the driver here, and also the higher neptunium and technetium, should really apply throughout the entire period of production. And there shouldn't be an arbitrary cutoff in 1973.

And I believe in the paper -- I think it is the second paper -- there was some kind of scoping calculation that was kind of predicated on the notion that there would be a proportionality in concentration relative to the feedstock. That might be true initially, but most of those types of mechanisms kind of follow the sigmoid curve and reach a saturation point at some point.

We don't know the dynamics of the particular physical and chemical processes that are going on. But I think to err on the side of claimant favorability, it would be a wise choice to do that.

Now we have this other. And let me state that that 400 parts per billion is a reasonable bounding value for the most highly-exposed group of production workers, which would be the metal workers and the millers.

However, we do have an issue with the personnel who handled these highly-contaminated residues on the front end, what we call the down-blenders and bystanders. We feel that there has been a lot of discussion back and forth about the quality of the health physics controls that were in place. The problem is, though, that we feel that all these arguments are really subjective judgements.

I can understand from a health physics standpoint. I'm a health physicist. You know that you have got a competent HP in charge of the facility and you have procedures in place. You would assume -- it is kind of a tacit assumption -- that those procedures are going to be followed, the due diligence will be done.

However, if you don't have that kind of culture in the facility, you don't necessarily have management support, and you don't necessarily have the resources to produce and maintain a good program in business-as-usual mode, you may still wind up with a lot of problems. We feel that NIOSH has not demonstrated quantitatively that this 400-parts-per-billion value would be applicable to that subset of workers.

In the references that Mark did provide in the August 5th position paper, we found some additional information which was really quite nice and enlightening. Because, for the first time, we have actually have a historic record, not a complete record, but we can trace the history of at least part of those shipments of the most highly-contaminated material that came in in 1980. This was five hoppers, approximately one-third of this material, that was repackaged in Plant

4 from April through May in approximately a three-week period in 1982.

During this time, there is a memo that describes what was done. There was air sampling going on. The two technicians who were doing the actual transfer from the hoppers to the drums did, indeed, wear air-line respirators. So, we know that at that point, at the transfer point, that at least the workers that were actually involved in the actual transfer were wearing appropriate attire.

However, due to complaints or concerns raised by other workers in the area, some of these millwrights and other people in Plant 4, and the fact that on a couple of occasions materials were dumped out onto a metal platform and broken up by hand, which resulted in pretty high airborne levels, they decided to go ahead and move the operation to Plant 1.

The trail kind of goes cold at

that point. We don't know what really became of that Plant 1 material. We weren't able to find any documentation of how that material was handled and processed.

The Joint Task Force report indicates that management claims that the same type of procedures were in place, but they didn't find any corroborating evidence that did take place.

Now we go on about three years later, 1985. This material, this original five hoppers, has been down-blended and processed into UO3, approximately 168 metric tons of it. We think this is probably the same batch of material based on the dilution factors from the contaminants and the amounts amassed that would have been produced versus -- about a factor of 25 dilution really applies to both.

So, this material, the UO3, is about 40 parts per billion. The original material is about 1,000, give or take. So, about a factor of 25.

And so, we are reasonably sure that this is the same material. So, we have this period of about three years, mid-`82 through `84, where we really don't know. We know that the down-blending and processing and repackaging was going on during this period of time, but we don't know how it was done. We know that downstream of this repackaging that basically the standard respiratory protection requirements applied.

And now, pick up in 1985. There is a planning document that describes how this material was going to be handled. It shows that there is an awareness that, you know, there is going to be swipes taken, there's going to be air samples, breathing zone samples. Basically, a good program is in place to track this.

However, they say that they feel that no respiratory protection beyond what would normally apply in a dusty situation is required. So, it kind of indicates to me that you have got this uncertainty here. You've got a situation where you don't have a quantified demonstration that this 400 parts per billion would apply.

Now you can make a common-sense kind of heuristic argument that, well, there is not that much of this stuff. It is going in in small batches, we believe. So, when you do an integrated, chronic dose reconstruction, 400 parts per billion, it would probably wash out any of the spikes that came along.

We don't know that. We haven't seen a quantitative demonstration of that. So, we still have concerns regarding that. And for that reason, we feel that the issue is still open from the time this material arrived in 1973 until Westinghouse came in and took over and instituted a robust health physics program in 1986.

I don't know, Mark, if you would like to -- that's kind of my essential presentation -- if you would like to say anything about the new paper you put out or something about that?

MR. ROLFES: Okay. Yes, I am probably going to just give a brief introduction and then turn it over to Bryce Rich for any detailed points.

Yes, I know this has been the hot topic lately. We have gone back, I guess as a result of the last Work Group meeting, you know, there were some things that were going on. We had previously been asked if we could find some of the raw data. NIOSH was able to locate a database containing 3800 raw results based upon the analyses conducted at the Fernald site for various transuranic contaminants, basically, over the history of the receipt of the materials.

We have also found I don't even know how many additional reports on just documentation and discussion between the Fernald site and places like NFS, you know, just discussing that Fernald has established a maximum quantity of recycled uranium or a maximum quantity of plutonium at 10 parts per billion in the uranium that they received. They basically operated on that, basically, from the beginning of receipts of recycled uranium.

We have some responses from the Fernald site back to NFS saying that, if the material exceeds 10 parts per billion, do not send it to the Fernald site; we will not accept it.

We did find an interesting memo that Gene Potter had identified as well, which I sent out to the Work Group as well. This was from, it is in the Site Research Database. It was an extract of Reference 94117.

It was basically an evaluation of individual's uranium urinalyses looking at how much uranium would the person have to be exposed to. They basically were looking at excretion rates of uranium in urine and making assumptions about how much plutonium could also be there, and whether or not that would be detectable with current bioassay.

MEMBER GRIFFON: What is that reference number, Mark? I'm sorry.

MR. ROLFES: It was 94117.

So, it appears that the health and safety staff had considered the use of uranium urinalyses to evaluate whether or not somebody could have had a credible exposure to plutonium at the Fernald facility.

So, what we have done in our most recent response, we have taken that information and evaluated some of the uranium urinalyses, and took a look to see how much uranium one would have to inhale to produce this excretion rate for plutonium.

I am sort of jumping around a little, but I don't know if we want to -- Bryce, would you like to discuss some of these things and we can come back to this or --

MR. RICH: Why don't you go ahead?

MR. ROLFES: Okay. I was just going to call on Gene.

This is the last few pages of our August response here.

Gene, to sort of put you on the spot, you have prepared some of the intake and dose estimates here in Table B-1 on page 29 of 32 of our most recent recycled uranium White Paper response.

Could you please go through what I just briefly and quickly summarized a little bit better for me?

MR. POTTER: Give me a minute to get that document open. I have got about 15 things open, and I don't think that is one of them.

(Laughter.)

MR. ROLFES: Okay. If this is a bad time, we can always come back to it as well.

MEMBER GRIFFON: While he is looking for that, Mark, on page 11 of your report, at the very bottom, you mention the data from 1986 and the 10 samples out of nearly 500 were at the MDL level, although expected to be taken between two and sixty days after the intakes. Do you have a document number for that data as well?

MR. ROLFES: The data are in the Site Research Database. There were 500 plutonium urinalyses that were collected.

MR. STIVER: So, they are in the SRDB?

MR. ROLFES: Yes, they are. I don't have the reference right off the top of my head.

MR. RICH: Yes, I can give that.

MEMBER GRIFFON: Okay. That would be good.

Do you remember if that was raw data?

MR. ROLFES: Yes, they --

MR. RICH: Actually, it was summarized in the Bassett report.

MR. STIVER: Yes, I read that report, the 1989 report.

MR. ROLFES: Five hundred plutonium urinalyses samples were collected from --

MR. RICH: From 441 people, workers, something like 600 urinalyses, 671, as a matter of fact.

MR. STIVER: Yes, I read that report.

Now this was actually during the campaign process, that 168 metric tons that we discussed earlier. That was material that was at a level, an unblended level, of about 40 parts per billion. I think it is right around 35. It ranged from 20 to 40.

This was also during the period when Westinghouse had more robust processes in place. And so, there is a little bit of concern there that it is an apples-and-oranges type issue. Our main concern is from 1980, basically pre-1986, during a period.

We can see, we agree with you guys that post-1986 you have a program in place that is adequate to control exposure.

MR. RICH: I may just make a comment here. That is that, during that period of time from the eighties, shortly after the eighties, there was a great deal of concern about how to process the high-level stuff. They knew that it was coming. As a matter of fact, they delayed receiving it until the bulk of it got there in 1980.

You mentioned the T-hoppers that came in that had the bulk of it. But I suspect they were cleaning out the bottoms in the D&D effort at the gaseous diffusion plant at Paducah, and it was unusually high and they knew it. It was a high-sensitivity receipt and process. And so, as a consequence, there is a number of documentation associated with the planning, associated with that activity.

As you indicate, they did repackage it, first of all, to get it in a position where the barrels themselves could be handled semi-remotely instead of dumping the T-hoppers. And they did have some issues. They had a spill, I think, associated with that operation.

But the sensitivity of the program associated with handling that material was very high. And so, there is documentation associated with an indication that, No. 1, they were aware of it and they were sensitive to the issues.

The air samples that were taken associated with that one process -- for example, there is a report about the process in Plant 4, when they were changing, as you mentioned. It indicated that the air sampling results were relatively high. And so, they, of course, had stimulated some additional sampling.

And the down-blending from that, of course, resulted in an increase. Well, it doubled the amount of plutonium in the whole plant in that one shipment in 1980. It simply doubled. Twenty-five grams of plutonium came into the plant at that time. And we can go on. I have some other comments that, essentially, the plant levels raised across the board.

MR. STIVER: Yes, I realize that, but to get back to the model you guys are producing here, this is a one-size-fits-all model. You don't have the granularity to assign -- you know, intakes by job category. So, the integrity of this model is really, in my view, completely dependent on the ability to capture, credibly capture, the highest-exposed group of workers.

I think we have a fairly good handle on most of the process workers in the plant because we have the MgF2, the magnesium fluoride data, to really -- it kind of defines -- luckily, you have got a good dataset, it is fairly robust, and it follows a log-normal fairly well. I think when you take the 95th percentile on that, you have got most of the production workers controlled.

However, you still have this other issue of peopling handling stuff on the front end. Now I realize that we have got a lot of qualitative judgments here and assumptions about what was done and how it was done. But we haven't seen, from a dose reconstruction standpoint, as required under Part 82.17, a quantitative demonstration that that 400 parts per billion would be bounding for that group of workers. That is really my main concern.

MR. RICH: John, I would just respond a little bit here, too.

The data per se are not used to deal with construction. The data are used to establish a default.

MR. STIVER: Yes. I know. I know.

MR. RICH: And the default, by the way, in my personal opinion, based on looking at the entire dataset and the circumstances, is enormously conservative.

And let me talk just a little bit about the magnesium fluoride stream, that 8.

MR. STIVER: Subgroup 8?

MR. RICH: Subgroup 8.

You see the magnesium fluoride did, you know, as you indicate, 40 to 50 percent of the plutonium --

MR. STIVER: Is what we are concerned with.

MR. RICH: The interesting isotope, although neptunium plays a role, too, of which we are aware.

In terms of just a simple question of a calculus problem, if you put in a certain amount in the front end, then the process was to take -- well, first of all, the process involved taking uranium tetrafluoride and mixing that with magnesium metal granules.

MR. STIVER: Right.

MR. RICH: And the desire was to mix those as homogeneously as possible. The reaction that took place, as you incrementally raised the temperature -- and it had a thermal shield made out of magnesium fluoride. Initially, it was made out of dolomite, which is an ore, calcium and magnesium ore.

But, after that, once that thermal reaction happened -- and they call it a bomb because it happens rapidly, and the temperature went up to 3,000 degrees. The metal coalesced. The reaction is between magnesium and the fluorine in the uranium fluoride. And so, you have a metal coalesce and drop to the bottom of the pot. Then it was cooled in a way in which it was necessary to accomplish that.

The point I am trying to make is that they were producing magnesium fluoride. So, they wound up with a surplus of magnesium fluoride.

So, No. 1, and particularly in enriched uranium feed streams, then the material is extremely valuable. And the uranium in the magnesium stream ranged from .05 percent up to 4 to 5 percent, and it averaged about 3.6 percent uranium. So, there is uranium in that magnesium stream.

And the reason you mention that, of course, is that all the documentation we have got, it ratios the plutonium to the uranium in parts per billion for uranium. Well, there is still uranium in it.

And the issue is, if you get a really good magnesium fluoride reaction complete, you have less uranium. The less uranium you have, the higher the ratio goes, not that there is so much more of the contaminants there, but the ratio goes up.

MR. STIVER: I think that is one of the problems with using the ratio method. I think that is kind of a shortcoming of it, but in the situation where it is going to default to that --

MR. RICH: And that is recognized. The only problem is, you're right, they didn't take routine urinalysis for transuranics. The reason for that is -- and, by the way, there is lots of documentation that indicates that throughout the history of the plant from 1964 on through they did routine evaluations, including some feed sampling. They have had to send their unit sampling off-site because they didn't have the capability. You know, that is a detailed -- you have to do a plutonium separation.

MR. STIVER: Right.

MR. RICH: Gene hasn't had a chance to talk about his, and I would like him to do that because he developed a spreadsheet that is very instructional from the standpoint of looking at detectability of the uranium -- or the plutonium -- in the feed streams that we are talking about and at the exposures, uranium exposures.

And by the way, we keep referring, and I am probably digressing just a little bit, but we talk about in the uranium facilities it is easier to do a fluorometric mass analysis for uranium in urine than it is to do a chemical analysis or an alpha spec analysis. And it's excellent, good sensitivity.

Indeed, the permissible concentration in people is, from a heavy-metal toxic limit for uranium, is higher than the radiological -- or is lower than the radiological limit up until you get to about 3 to 5 percent enrichment.

And so, what we have decided, of course, is to use the ratio of the plutonium to the uranium. Since you don't have specific plutonium analysis, then one size fits all. Most of the plant processes are way below the magnesium fluoride, that process. And so, you are overestimating by at least an order of magnitude or more the exposure to other plants.

Now, back to 10A, this is a process. We had five T-hoppers that contained the bulk of the material, the bulk of the contamination. They took it in, they repackaged it, and then waited until they had material that they could blend it. So, it took them a while. They didn't do it all overnight.

But when you process a barrel or two of that, that is a short-term process, it does not go on for 2,000 hours a year. As a consequence, even the down-blended material is going to be in the 40 to 80 parts per billion. Some of it went through the plant, and some of it, it got used as UO3 directly.

MR. STIVER: Yes, the data I saw had the UO3 at about 40 parts per billion. Then, they evidently did a 1-to-4 dilution with clean UF4 before they reduced it.

MR. RICH: Yes, but, you see, your point -- and this talks to one of your other points -- says that in the period of time from 1961, we have got good data from the primary suppliers, Savannah River and Hanford primarily, and from the gaseous diffusion plants, too. The material that came in for sweetener from the gaseous diffusion plant was in the parts per trillion. It was decontaminated. You know, the recycled uranium, once it hit the gaseous diffusion plant, that is a big problem for them because the UF6 process, the insertion into the gaseous diffusion plant dropped it out. It came out in the ash, about 94 percent of it, as a matter of fact, and neptunium a little bit less, and technetium went on, all the way through.

But we have got a lot of information and data reports associated with evaluating all of those contaminants in the plant, a big report on technetium, for example, in the Fernald plant. It was not a radiologically-archaic program. They were mindful of their needs. They did routine evaluations based on the levels that they were seeing.

And by the way, in the plant they defaulted to 80 parts per billion in the plant in general. And in the magnesium fluoride stream we have data that indicates that on a 95 percent log-normal distribution probably it would default, but that is enormously conservative on the basis of the people that were actually working at Plant 5. Because once you get through with the magnesium fluoride, you turn around and load uranium tetrafluoride. That is pure uranium.

Then, on the other side you have the breakout, and you are cleaning up. So, the U3O8. So, it is not just an exposure to the uranium fluoride stream itself.

MR. STIVER: No, we know that. I think we are in agreement on every one of these points.

The point being that you still have this group of guys, say in the breakout area or the pod cleaners, and so forth, who are getting, obviously, based on air sampling data, you know these are the dirtiest jobs. You also know that you have got a lot of this material being concentrated with respect to magnesium fluoride. Sure, across the entire plant you say this has got to be bounding for all these workers, but they never were even in here handling this stuff.

But you still have this other group, and it is just a matter of having a quantitative assessment, a demonstration that 400 parts per billion is adequate. All these arguments, you know, they sound very good, but at the end of the day we are looking at qualitative assessments.

MR. ROLFES: That is a good point.

Bryce, excellent job. You couldn't have said it any better. I wasn't going to attempt that, and I am glad to have you here to set the record straight, to make sure that we are aware of the process, because you have brought a lot of valuable information and insight into this process. I do really appreciate it.

CHAIRMAN CLAWSON: Mark, before you go on, you started out with something and I want to make sure. That is, you said that you had found so much data, and so forth, pertaining to this paperwork. Is this new data that we have not seen or is this just data that you have recovered? You are talking about surveys, and so forth. Is there anything new? That is my question.

MR. ROLFES: Yes. Yes, there is a combination of both new data and old data.

We were asked by SC&A to go back and get, as a result of our discussions, I think it was at the last Work Group, back to get some of the raw results. We were able to locate an electronic file that contained 3800 analyses that were conducted at the Fernald site over the operating history.

We spoke with the statistician and one of the people that were responsible for compiling all that information in this database.

We also found some additional results. We have just sampled those boxes of records a while back.

So, yes, there are both new and old records to give you --

CHAIRMAN CLAWSON: When you say you sampled --

MR. RICH: When we talk about raw data, a good share of the data that is in the DOE 2000 report is from the 1980s, and early eighties. As a consequence, that data is high. It is high. That is the maximum levels that were received at the plant.

When we say "raw data," we found the working spreadsheets that were used by one of the team leads. We took the analytical data sheets that are in a file that exists that we had not been able to --

MR. STIVER: So, the actual raw data are available you're saying for some of the --

MR. RICH: The raw data is available.

MR. STIVER: That comprised that spreadsheet?

MR. RICH: That comprised that spreadsheet. But it is in a file that is in the system someplace. We have not been able to retrieve that specific file that they used for the base.

MR. STIVER: I know I looked at the spreadsheet that Mark posted. It is basically the exact same information that is in Appendix C of the DOE report.

MR. RICH: Yes.

MR. STIVER: I know we have a lot more information. See, I was a little concerned about the magnesium fluoride. First of all, were we are dealing with Fernald and we were we dealing with the right types of material? It turns out, yes, we are.

MR. RICH: The other thing, they had magnesium fluoride from that, from other plants, too.

MR. STIVER: Yes, although the data that went into that subgroup was only Fernald. And that is what I wanted to see.

MR. RICH: And the dates are there.

MR. STIVER: Yes, they have the dates. Yes, I charted it up.

MR. RICH: Yes.

MR. STIVER: So, I have got a pretty good handle on that.

CHAIRMAN CLAWSON: So, really, this isn't new data? This is just --

MR. STIVER: It is just more information on what we have already --

CHAIRMAN CLAWSON: Okay.

MR. RICH: It is perspective, Brad.

CHAIRMAN CLAWSON: Yes, I just wanted to make sure that we didn't have something new that had come up.

MR. RICH: Now there is new data

in the system. One example that comes up, there is a 400- or a 500-page document, a compilation of a variety of things. It has a lot of good and new information.

MR. ROLFES: I was going to say, these are what I referred to earlier. From NFS, there is correspondence and analytical results discussing the recycled uranium limits in the Fernald site in the sixties, basically, correspondence of Fernald saying that they would not accept anything that had a concentration of plutonium in excess of 10 parts per billion because that was their established level for control because of the radiological concerns about materials in excess of 10 parts per billion.

Gene, have you been able to open up -- this is, basically, when it comes down to it, you have asked for a quantitative assessment of the dosimetric impact to a Fernald worker.

What this Attachment B that I had

referred to earlier, it is basically four pages, which I would like Gene to just briefly explain. Basically, we have put together -- well, I will let Gene explain it.

Gene, have I allowed you enough time?

MR. POTTER: Yes.

MR. ROLFES: Okay.

MR. POTTER: I have got the information in front of me.

It is hard to talk about a table full of numbers, but maybe I can just sort of summarize what this is attempting to show.

The bottom line would be that the fact that they were not sampling for plutonium in the early days is not a technical problem because even the paper we referred to earlier in the eighties showed that uranium would be much more easily detectable.

For example, if you look at Tables B-3 and B-4, you see that it would take, in order for it to be detectable by the best, for plutonium to be detectable by the best labs in the country, even sampling at day one after an intake, it would require RU with 400 parts per billion. Then, of course, it goes up if the parts per billion -- in the eighties, they actually referred to 80 as kind of their worst-case when the dosimetry folks looked at this issue.

So, as you go out farther from the intake or reduce the parts per billion you are assuming, then plutonium just becomes more and more difficult to detect. That is all those four tables were trying to show.

MR. STIVER: We understand that the uranium was the detectable isotope, and that they didn't necessarily make any attempts to do bioassays for plutonium in the early days. But that is really kind of not the real issue.

Our concern is more that there was kind of a lax standard for enforcement of the procedures that were in place, to where there could have been exposures that would have been missed, whether or not plutonium was actually being sampled. So, it is not whether they had an adequate bioassay program in place early on, which would have been nice if they did, but if they had that, then we wouldn't have to have this model to begin with.

We have got a good set of uranium data, and we are making some assumptions that are supposed to be as claimant-favorable as possible, so that we can ensure that we have captured the most highly-exposed group.

Our concern was in this period during NLO's tenure, that it just didn't have the robust processes and procedures in place that were actually enforced to ensure that these exposures were not incurred. That is really our main concern here.

MR. RICH: John, can I make a comment now?

MR. STIVER: Yes.

MR. RICH: There were control

programs in place. They were placed upon uranium.

If you look at the uranium in urine results over the years, they decrease year by year by year. They were making progress in terms of increasing ventilation controls, which in many instances was based on the uranium urine results.

As a consequence, it is like looking, if you are monitoring in a reactor situation, you don't monitor the urine for every isotope that you could possibly be exposed to.

MR. STIVER: Yes, if you know the ratios, then you can make assumptions.

MR. RICH: Yes, and you take gross analysis or you look at strontium or cesium, some of the longer-lived --

MR. STIVER: Indicator radionuclides.

MR. RICH: Then you say we're controlling because everything else is going to be so much less.

Now, again, through the years, there is documentation where they did hazards analysis and compared it to the maximum permissible -- well, based on the activity that they saw in the process streams. And like I say, they have generated what they were seeing in the plant. The maximum was 80 parts per billion.

So, that is what they used for a lot of the hazard analysis that said, fundamentally, that if you had that level, and compared to the maximum permissible concentration, which gives you CEDE, and a couple of them, where they actually looked at individual organ doses, in plutonium, when you get into that area, starts to control it.

What we are talking about here in terms of a default, quite frankly, my personal opinion is that 100 parts per billion adequately controls, but now we are set at 400 for this later period of time when the maximum contamination in the plant resulted in maximum contamination levels everywhere. Those levels will produce exposures. If you take the defaults and calculate the exposure compared to the kind of transuranic activity that they were seeing when they sampled the people in 1955 -- `85 and `86, the results from the default are going to be higher than what you would get from the default in the transuranics in the air.

MR. STIVER: I can kind of see where there is a divergence here. Part of it is that we have got this idea of what the real exposure might have been, and then we have, for EEOICPA a high-sighted model that we are trying to generate. We have this set of data. We have got 19-some processes. And we are trying to say, okay, if we look at all this data, all these different processes, can we find a set that would definitely provide a plausible bound for everybody?

Now we are not even going to look

at what they really got. We are looking at what we are going to give them as part of the dose reconstruction compensation program.

Okay, we have got a set that is for magnesium fluoride. We know this probably represents for an ongoing, continuing process probably the highest ratios, not necessarily, as you mentioned before. Depending on the amount of uranium that is in the material, it can be all over the place. But we are using ratios as the method. So, is that really the highest plausible intake that somebody could get would be based on a particular dataset?

We also have Group 10A. We know that this is real data that came in during the 1980s predominantly. We know that there were personnel who were potentially exposed to this. We don't know the frequency that they were exposed or what period of time they were exposed.

So, there needs to be some kind of an assessment in that range. Okay, say if you had, I could kind of see this being -- I am not going to tell you how to do your job or anything like that. But, for conjecture, we could have a scenario where you have got Worker A is exposed to X hours per year of a down-blending operation in addition to the 400, or various different combinations. You could do that.

Is there a net impact, significant impact, on the final bottom line? That is kind of what I am thinking of, when I think of the quantitative assessment, that this 400 parts per billion is really bounding.

MR. RICH: Yes, the problem there, of course, is that it is very difficult administratively to count their location there --

MR. STIVER: Yes, well, you can't. So, you just have to make some assumptions.

MR. RICH: You simply can't do it.

MR. STIVER: Yes.

MR. RICH: So, it forces you into

a one-size-fits-all --

MR. STIVER: That would be a one-size-fits-all --

MR. RICH: And in the case, in my personal logic and reasoning, based on probably 10 analyses, it is probably a factor of less than 10, but a factor in that range, higher for a short period of time in a campaign, and blending in 168 metric tons of that material into how many --

MR. STIVER: Yes, 168 is one-third of it. There's still two-thirds unaccounted for.

MR. RICH: Right. We included that for what appears to me the logical and reasonable and justified -- primarily because the people that would be working in Plant 4 in that operation were normally exposed to stuff that was a couple of orders of magnitude less than the 400.

MR. STIVER: Yes, there is also Plant 1, and a lot of this went on in Plant 1.

But there is this period from `82 to about `85, and you have got boundary data that show high levels, spiking from `82 up to `80, come back down at `85.

MR. RICH: When you say high levels, let me just modify that and say high ratios, if you will.

MR. STIVER: Yes, they are the ratios, high ratios.

MR. RICH: The levels were extremely low.

MR. STIVER: Yes, levels were low, but the ratios were high. This whole model is predicated on the ratios.

MR. RICH: And the Titan Mill in Plant 1 was the one that was used fundamentally to blend and to break up the magnesium fluoride, so that they could --

MR. STIVER: Right. That was one of our concerns of our paper.

MR. RICH: Yes. As a consequence, that particular mill showed a higher ratio than normal.

MR. STIVER: Right.

MR. CHEW: We had the bioassays. Whether the process was effective or not isn't an issue here. It is most important that you have bioassays to do those reconstructions.

MR. STIVER: Well, we really are stuck with the uranium bioassay ratios based on --

MR. RICH: That's true, yes. Before 1986 --

MR. STIVER: I would like to ask, John Mauro, are you on? John, are you out there?

DR. MAURO: Yes, I am. I was on mute.

MR. STIVER: Okay. Did you have anything you wanted to add about this?

DR. MAURO: Yes. I am listening, and, of course, we have had our own internal discussions. My sense is we do not have any dispute on the facts, which is an interesting place to be. In other words, I don't think we are disagreeing.

I think it is the interpretation of the facts and what they mean in terms of making judgments on what can and can't be done, you know, what can be done with sufficient accuracy, what can't.

Let me just ask a couple of questions and see if we agree on these facts. Is there general agreement that there might very well have been some workers at some point in time who were inhaling airborne uranium where the ratio of plutonium was 400 parts per billion? That is, these would be the people who were working with the bomb, with the dolomite, that may have gone through a few cycles where you did have an opportunity for the plutonium to be somewhat enriched, you know, richer in the dolomite. Do you believe that there were some people at some time that might have inhaled uranium that contained 400 parts per billion?

Is there general consensus that that is true? Because if there is not, then we do not agree on the facts.

MR. RICH: I don't disagree. For short periods of time, there could have been an operator that was exposed to air that had 400 parts per billion.

DR. MAURO: Okay. Now, so that is a good start. Why would you say for short periods of time? Let's say his job was primarily to be involved in the bomb-reduction process and handling these bombs, breaking out the metal from the bomb, collecting the dolomite, and doing those things with the dolomite that you do to use it again in the next bomb.

And we are trying to find facts. Wouldn't a person who had a job like that possibly be exposed to dolomite for protracted periods of time?

MR. STIVER: I would have to say yes. You are looking at derby breakout personnel and people in the dirtiest jobs. Those would be basically what they did eight hours a day or more.

DR. MAURO: Now is it possible that --

MR. KATZ: Hold on a minute.

DR. MAURO: All right. Keep going.

MR. STIVER: I am sorry, I'm jumping in there.

DR. MAURO: No, no, this is good because I was trying to find where it is that we disagree or agree on the facts of the matter. That is important.

MR. RICH: John, this is Bryce.

I would have to modify that again just slightly to say that any one individual that worked in Plant 5 in the thermite process would also be involved with handling another part of the dusty operation, which is handling and loading and blending the UF4, and, then, again, working with the derbies, which now are uranium and other materials. I would have to say his exposure to the magnesium fluoride on a continuing basis should be modified by the fact that he gets exposed to other materials, which would reduce -- and we are saying average 400 parts per billion as the default. Well, that has probably got to be, well, it is way conservative, even for that --

DR. MAURO: Okay. You know what? I want to say that common sense would dictate that it is unlikely that there would be a person that would be handling dolomite for such a protracted period of time containing the upper-end concentration. We know it is at the highest, but it is sort of at the upper end -- I believe it is the 95th percentile -- for an entire year.

And one could argue that that would be certainly bounding, perhaps unrealistically bounding. It would be hard to find someone --

MR. RICH: That you had

unrealistic bounding, I will agree with you.

DR. MAURO: I said perhaps. I used the word "perhaps" because I don't know.

But, you see, we are zeroing-in on a place, and this is what I like to do, zeroing-in on a place where people can agree that, yes, you know, two things have to happen. One is you have to have the 400 parts per billion of your dolomite, which we know to be at sort of the upper end, and we have to have a person, a real person, that worked with that upper-end dolomite for a protracted period of time, perhaps the course of a year or two years. And put those two things together; one could argue, you know, that doesn't seem to be very plausible. In all likelihood, if you really were making the measurements, you would not expect to find such a person.

If that is your position, you know, it is one of degree. Our position is, well, if you are going to pick a number and you want to be sure that you are not underestimating anyone's exposure that handled the dolomite, you pick 400. And everyone would agree, yes, we could all sit around the table and say, yes, there is no doubt it wasn't greater than that. And more likely than not, the highest guys are probably even less than that.

So, now it becomes a judgment call. This is where really it moves out of science, and it moves out of interpreting sufficient accuracy or degree of conservatism.

But I think we can agree that, yes, I would say 400 would certainly be bounding. I would not dispute that maybe it is overly-conservative, but certainly it is bounding. Where you really put the number, do you put it at 100? Do you put it at 200? In other words, you may find a different place that brings you comfort that, no, I think this is a better bounding number.

But I think we could all agree

that that particular practice is probably a practice that is a baseline, that extended over protracted periods of time throughout the operating history of Fernald, where dolomite was handled, and not withstanding we realize that metal, that UF4 they handled could have had varying levels of plutonium in it from a few to maybe tens of parts per billion. But the very process itself, the dolomite process, results in this enrichment of the dolomite.

And we also know that at the high end there were at least some samples of dolomite which actually reached the 400-parts-per-billion level. Whether or not you could agree that, yes, 400 represents a bounding number that, if we applied it to everyone that might have worked at that facility, whether that is unrealistically-high or not, that becomes one of these judgment calls, and we have really left the realm of science and we have entered the realm of interpreting the intent of the regulation. And where are we comfortable?

I could say right now that SC&A has gotten to the point where we are comfortable with having the 400, as opposed to the 100, as a baseline for exposure. If you know who the workers are, great. But if you don't know who the workers are, you are sort of in a tough spot.

We know that everyone didn't get that, but our sense is that perhaps some people got that. If you don't know who they are, you have got the situation.

And Paul Ziemer has mentioned this on a number of occasions. Certainly, everyone could not have gotten that, but on a person-by-person basis you could ask the question, is it plausible that he might have gotten it? And the answer would be yes. The next person is yes.

What you end up with is a circumstance where you know everyone couldn't have gotten that, but you don't know which ones didn't. So, you have very little choice but to pick that high-end value and assign it to everyone.

So, that is where SC&A is coming out. Now we would be the first to agree that maybe 400 is too high because it is the upper 95th percentile of all that data. That is 400 numbers you took. And to assume that a person is exposed to the upper 95th percentile week after week after week after week into years may be pushing the edge.

Here's where judgements are made. Quite frankly, it almost becomes more a judgment that is made by the Board. Are they comfortable? I mean, given that as the reality of the situation, and that a judgment has to be made, and I am not going to argue with you. I think we are in agreement.

The only one is a judgement call is, at what point are you at your tipping point where you say, "I think that is a little too high?" I think, for us, 400 is just the right sweet spot where we are comfortable as your baseline, not the 100, but the 400.

And this is my understanding of the state of affairs with regard to this particular matter. A separate matter, which you will talk about momentarily, has to do with the down-blending, which is a separate story and a separate issue.

But I think that there is a platform that we are all trying to build that we could say we could stand on and agree on. It sounds like we are not quite there yet.

You folks are uncomfortable with using 400 as your baseline. You would be more comfortable --

MR. STIVER: Actually, John, this is John Stiver.

Actually, they have accepted 400 as the baseline.

DR. MAURO: Well, I mean, if we have got that -- and that would be for all locations at all times?

MR. STIVER: No, they are not accepting it pre-`73. From `61 to `73 -- I probably should have given Mark a chance to describe it. From `61 to `73 -- go ahead, Bryce -- they are going to use 100. Basically, it would be the old default.

DR. MAURO: Okay. Now good. That's good.

Now do we have data or reasons to believe that the `73, somehow before `73, things were different in a way that it is virtually impossible to have generated dolomite at 400 parts per billion?

MR. STIVER: We don't know if it is virtually impossible, but it would depend on the concentration dynamics, the physics involved and the amount of times, how many times the material was reused, and the amounts in the feed. And all these factors would come together. The rate at which it builds up would all come into play.

It is basically a first-order

linear differential equation. You have got a rate in, and you have got a rate out, and you have got a concentrating mechanism.

MR. ROLFES: This is Mark Rolfes. I just wanted to address what you had said, John.

Basically, you have come to agreement with us that we are able to bound doses to the workers who were potentially exposed.

MR. STIVER: No.

DR. MAURO: Could I qualify that? I think if you folks have picked 400, let's say for the time being, from `73 to what, `86, as being your baseline default for the workers, I would agree, with the proviso we still have to have a little conversation regarding the down-blenders because they sort of fall outside that envelope.

So, I would like to put that in the parking lot for a minute. Maybe if we could solve the 400/100 number and agree on a baseline, and then superimpose on this baseline, that really is where I believe the action is, once we resolve this, is going to be the down-blending perturbations.

But I am more than open-minded regarding arguments that could be made regarding why it is that 1973 is an elbow when it comes to the 400. I haven't really heard the arguments, but I believe you. I believe that, for some reason, you feel that something changed going from `72 to `73, where the 400 is now just implausible if you are pre-1973.

Is there a 30-second sound bite that could explain why that change occurred?

MR. RICH: Yes. This is Bryce now.

In 1973, the AEC made a decision that they were going to assign the recovery of uranium from the scrap materials, ash, et cetera, fundamentally, to Fernald. They sent some to Y-12, and they never touched it. They did the right thing, disposed of it or sent it back.

In `73, we have data to indicate that the levels in the waste coming in 1973 were lower, but it was higher. Enough POOS material was chosen, that time period, when they started to receive the waste from the gaseous diffusion plants.

In 1980, it was another story. They got to the bottom of the barrel, and they got a big charge in on that year. As a consequence, we are saying we have applied 400 parts per billion to the time when we started to receive the material from the gaseous diffusion plant waste and CIP/CUP material and everything else.

Before that, I think we have enough evidence to indicate that the levels were a couple of orders of magnitude less parts per billion --

DR. MAURO: So, Bryce, help me a little bit with this. You are producing UF4, so that you could go put that in your bomb.

What you are saying is the UF4 contains, you know, it could be material that does not have any recycled uranium; probably most of it didn't. And it could contain some material that does have recycled uranium.

And there was a spec that says, "We're going to control the amount of RU that is going to go into our UF4." Whatever the material coming, we are going to blend it down, get it to a place where we are comfortable, and then that is the material, the UF4 that is going to go into my bomb, along with my magnesium.

Then, of course, the bomb, the reduction process takes place. Whatever plutonium that is in the UF4 that was fed into the bomb, it finds its way into the dolomite. Then, of course, that is redone over and over and over again, and you enrich, enrich, enrich your dolomite. And here we are at some time later on; you run into the 400 parts per billion.

Now are you saying that in the earlier years it was such that the down-blended -- we know that recycled uranium was showing up there, I guess, for quite some time. I am not sure when it started, maybe even in the fifties.

MR. RICH: 1961.

DR. MAURO: `61. So, really, what you are saying is, from 1961 to 1973, the amount of plutonium that was in the recycled uranium or the amount of recycled uranium that was arriving was such that it really did not create a circumstance where it was plausible for you to produce dolomite of 400 parts per billion, but it was possible after `72?

MR. RICH: After what?

DR. MAURO: After 1972. Before 1972, in your mind, it was just not possible to produce high-end concentrations of plutonium in the dolomite that was 400 parts per billion, but it is possible after 1972?

MR. RICH: Yes.

DR. MAURO: That is effectively what you are saying? And I presume it has something to do with the fact that the uranium that was showing up before 1972 generally had lower levels of plutonium in it than the material that showed up after 1972?

MR. STIVER: Can I say one thing, John?

I think one of the issues here, John, is that post-`73 we have got a down-blending process that essentially results in material that is getting ready, that is put in the bombs, that is close to the spec, from 10 up to about 30 parts per billion.

DR. MAURO: Okay.

MR. STIVER: Before that, the feed material that is coming in isn't down-blended, and it is pretty much below 10. So, there is a bit of a differential there.

MR. RICH: It must have been 05.

MR. STIVER: Yes, and so, there is going to be concentrating. We are saying even though we know the 400 may account for some material that is not completely blended-down below 10 parts per billion, still, the fact that you go from that level up to 400, as the 95th percentile, and even up into the thousands, indicates that the concentration mechanism combined with the variability in the uranium content is driving this ratio up.

And so, we are saying, is there sufficient evidence to indicate that in the earlier periods that same type of process would not have resulted in a 95th percentile that was near 400?

I believe Bryce's position is that that would not have happened, that 100 would be probably bounding.

DR. MAURO: John, I know you have been looking at this closer than anyone. Do you feel that is a reasonable place to be?

MR. STIVER: Well, unfortunately, we only have data for magnesium fluoride from 1982 to 1987.

DR. MAURO: I got you.

MR. STIVER: We don't have earlier data.

DR. MAURO: So, right now, you are using intuitive process knowledge that says, based on looking at the data, their position is not unreasonable. It very well could have been a sea change that started, more or less, in the seventies.

MR. STIVER: I wouldn't know if you would call it a sea change from the perspective of what is actually being reduced would be more like maybe a factor of two on average, two or three.

DR. MAURO: Okay, but a concept, the idea that for some reason pre-`73 the dolomite probably was even -- you know, we are already being conservative when we are going to 400. I would be the first to admit that, even in the eighties.

MR. STIVER: Pre-`73, it is an open issue. I think that it could certainly be bounded.

MR. RICH: John, we do have a little information, but it is not in the level of numbers, pure numbers, that we did have in the eighties. In the 1980s, they did a massive process analysis. And in the seventies, some of it came from other plants, some of the magnesium fluoride. Fernald is not the only one that uses that process.

MR. STIVER: Right now, I am only talking about what is in the Subgroup 8 dataset. There is a little bit of --

MR. RICH: Right. In the before seventies time period.

MR. STIVER: There is some data in there with no --

MR. RICH: There is some data.

CHAIRMAN CLAWSON: Well, this is a good discussion going on, but I think we do need to take a comfort break at this time.

(Laughter.)

So, I want everybody to keep in

mind where we were at. We are going to mute the phone, and we will come back in at a little after 11:00.

MR. KATZ: At 11:00.

CHAIRMAN CLAWSON: Yes.

MR. KATZ: Sound good?

(Whereupon, the foregoing matter went off the record at 10:47 a.m. and went back on the record at 11:03 a.m.)

MR. KATZ: Okay, we are re-collected after a short break, the Fernald Work Group.

Let me just check to see. We have -- Dr. Ziemer, are you on the line still with us?

MEMBER ZIEMER: I am on the line, Ted.

MR. KATZ: Great. Thanks.

CHAIRMAN CLAWSON: Okay. I guess we will pick where we did. But, you know, this discussion, as the Work Group Chair, part of the thing is we have been going back and forth about this for numerous years.

John, I appreciate your trying to put forth the effort to be able to figure out where we are at.

But there were some statements that somewhat kind of bothered me, and that was that earlier, as Bryce said, this was going beyond science, and so forth, like that.

But, also, John, you made some comments.

I want everybody to realize that, basically, SC&A and NIOSH are presenting to the Work Group. It basically comes down to the Work Group to be able to express or send something to the Secretary to be able to do it. So, yes, it does come down to us, to the Board. As a Work Group, we take it to the Board, and it is the full Board that makes this decision based on the information that we have.

But one of the things was that policy plays into this, too. And these are the cards that we have been dealt, and this is what we have to be able to deal with.

You know, part of the problem with Fernald is, on one hand, it has got some good urinalysis data, in my personal opinion, but, also, too, it lacks an awful lot of information. It is true that this plant was run like a heavy metals plant. It was National Lead of Ohio. They had dealt with this kind of stuff. Uranium was a little bit new.

But the bottom line is we can't separate people out from one area to the other. We don't have that kind of data. So, we are trying to find a point that is going to be able to cover everybody. And we do have options in this. This is where the SEC comes into play. We do have different options than going so high, and so on.

But we are trying to reach a point that we have been trying to go to for years. And so, as we go into this discussion more, I just want you to remember, yes, sometimes science, it goes against our grain sometimes, but what we have got to remember is this is for the petitioners. This isn't for proving that we can do reconstruction, or whatever. This is set up for the petitioners to be able to give them compensation.

Go ahead, Mark.

MEMBER GRIFFON: Yes, let me just pick up on one thing that Brad said, and this came up in the earlier discussion, the idea that -- I forget who said it; I think Bryce said it -- we were kind of forced into a one-size-fits-all model. My sense is that -- not really. I mean NIOSH did have a choice. NIOSH could have said, well, we have to give the SEC for this particular subclass of workers.

I think the problem from NIOSH's standpoint that you run into is that, if you would even consider that in the situation, it would probably be for this blending operation, which probably is a small subset of workers, but you can't identify who they are. So, then, if you establish a Class, it is the broader, you know, it is all workers, essentially. I am sympathetic to that.

On the other hand, when we look at SEC petitions, our charge is to look at whether we can bound under plausible circumstances for all workers on all time periods. That plausible is another thing that came up in our earlier discussion, which was that I think we had agreement with Mauro on the phone and Bryce that, at least for the mag fluoride side of things, that this number was unrealistically-bounding -- sort of a new term that Bryce fashioned, I think.

(Laughter.)

But that begs to question that policy side of things, again, the plausibility. So, are we just increasing the number until everybody kind of says, "Oh, there's no way any of the workers could be higher than that." That raises some questions about how we interpret it from the policy side on the SEC decisions.

So, I just wanted to give some context as to where we are going.

DR. MAURO: Mark, this is John.

MEMBER GRIFFON: Aren't you on mute? This is usually where I need to mute you.

(Laughter.)

DR. MAURO: If I have had enough, I will stop.

CHAIRMAN CLAWSON: No, no, no, John. We're just kidding with you.

DR. MAURO: I think right now, Mark, you are absolutely right. We are only talking about this baseline. We haven't talked about the down-blending. I think that is going to be where the action is.

I could say that, right now, you have the hardest job. The Board, the Work Group has the hardest job.

I think the facts of the matter have taken form, and I don't think we have any disagreement with NIOSH, at least from `73 to -- what is it? -- `84. I think, to try to put it, again, into a 30-second sound bite, 400 parts per billion is certainly a high-end number to assign to everyone, perhaps plausibly unrealistic, but using what I call the Paul Ziemer rule -- and I have learned that well -- you know, if you pick one person at a time and you really can't say whether he got that or not, but you could say it is very, very unlikely that he could have gotten that exposure for a protracted period of time, that places you in a place that says, well, it might be a little bit unrealistic, but it is where we are in terms of the definition of bounding, in my mind.

And I would say SC&A's position -- because we have had a chance to talk about it, so I am not just speaking for myself -- SC&A's position is that, at least from `73 to, I believe it's `86, a baseline for all workers or most buildings -- and John could clarify that -- but I think it is just about all workers, 400 parts per billion, as opposed to the original 100 parts per billion, seems to be in the right place. Whether or not NIOSH agrees with that, whether the Board agrees with that certainly, but, I mean, you could understand our sensibility about why we would come to that place.

I think we are at a point now, and there is really nothing more SC&A can say or there are any more facts of the matter that need to be aired. We are at that judgment point. Whether or not it meets your threshold of sufficient accuracy, plausibility, that sort of thing, that is where the judgment comes in. That is why you guys have the hard job.

The other half that we didn't talk about is the pre-1973. Right now, I can't speak to it, but it sounds like there is good reason why the 400 may be a little bit too high for pre-1973, and maybe a good case could be made for some lower number, perhaps 100. But I can't speak to that.

I can speak to the 400. And maybe, John, we certainly would say, well, if you were to extend the 400 to pre-`73, that is certainly bounding. But, at the same time, I don't want to be unreasonable because Bryce just pointed out, well, you know, there was enough of an elbow in `73 where, boy, you are really pushing it if you want to hold it at 400 to pre-`73.

MR. STIVER: John, I think the point there for pre-`73 is that we feel that it is boundable.

DR. MAURO: Yes.

MR. STIVER: Now whether you choose 100, 150, 400, it is a judgement call there. But I believe that that is boundable.

DR. MAURO: Okay.

MR. STIVER: What we are concerned

with is the `73-to-`85 period.

DR. MAURO: Right. Okay. Well, that is a good way to say it.

MR. STIVER: I think the 400 is boundable for all but this Class of workers, these down-blenders as well.

DR. MAURO: Right.

MR. STIVER: I think we have got agreement on that part. So, it is really the down-blender issue now.

DR. MAURO: But, in effect, what we are doing is we are saying we believe a plausible upper bound could be placed on pre-`73 also. We are not going to say what that number is. It might be 400. It might be something lower. And I know how much Mark hates this word, but, in theory, it's tractable.

I think that is as far as SC&A can go with this. Really, for this aspect of the discussion we are having, the ball is now really in the hands of the Work Group. That is, do they have a degree of comfort with these numbers?

SC&A is comfortable with the 400. For pre-`73, we think you could bound it. What that number would be, it is hard to say right now.

MR. STIVER: John, could I step in here? Something you said there kind of worried me just a little bit.

DR. MAURO: Okay. Sure.

MR. STIVER: The 400 isn't for all workers.

DR. MAURO: Oh, okay.

MR. STIVER: Remember, we are concerned with this subset, the problem being that they can't be identified.

DR. MAURO: Well, that's what I'm saying --

MR. STIVER: So, that is why you wind up in a situation where you have got to have the one-size-fits-all.

DR. MAURO: I mean, we've been

there before.

MR. STIVER: You need the right terminology there.

DR. MAURO: If you don't know who they are, you are left with no choice.

MR. STIVER: Yes.

DR. MAURO: And that, again, becomes not a scientific question anymore; it's a policy question. What do you do when you don't know who they are? You are sort of left in this uncomfortable position, well, if you don't know who they are, you have got to give it to everybody. Now does that meet the letter and intent of the statute?

MR. KATZ: That is something that we deal with all the time. It has been applied in many, many places. It is not really breaking new ground.

But it seems like this discussion needs to move on and put to bed what you have been trying to wrestle with, which is the down-blenders question.

DR. MAURO: Yes, I would like to move on to that, yes.

MR. ROLFES: I would also like to make one point. Basically, I just wanted to reiterate what John Mauro is saying on the phone.

This is Mark Rolfes.

What I am hearing is that the science demonstrates that we have the ability to bound dose. So, this effectively removes this issue from the Special Exposure Cohort.

CHAIRMAN CLAWSON: No, no.

MR. ROLFES: It turns it into a Site Profile realm because we are just trying to decide which level of contaminated material that we are going to assign in the dose reconstruction process, whether it is 100 parts per billion or 400 parts per billion.

CHAIRMAN CLAWSON: And the time.

DR. MAURO: And this is John speaking for SC&A.

I would agree that that is the

question, but that doesn't mean that the Board or the Work Group could say, listen, that choice or that judgement does bring us into the world of SEC and sufficient accuracy. And therein lies the job of the Work Group and the Board.

You know, understanding the facts, as we understand them, and I think they have been communicated very well, and the judgement needs to be made whether this, in fact, leads you to the degree of comfort that, yes, we could place a plausible upper bound and a place that everyone could agree upon. If the uncertainties, the extrapolations, et cetera, are to such a degree that you are uncomfortable with it, that is your call. But, I mean, I think that we don't make that call.

MR. STIVER: Hey, John, this is John Stiver again. Can I say something?

DR. MAURO: Sure.

MR. STIVER: I think we are

getting a little off-track here. Remember, this 400, we are only considering the workers who are not within this cohort of down-blenders.

DR. MAURO: Oh, yes, I am trying to move off this.

MR. STIVER: Okay.

MEMBER GRIFFON: But when you say all workers --

MR. STIVER: When you say all workers, it kind of implies that they are all lumped together here.

So, there is this issue of, is the 400 parts per billion bounding for the down-blenders? Or could it be higher?

DR. MAURO: Yes, I wanted to get this behind us before we talked about it because the down-blenders is the next tier.

MR. STIVER: Yes, because you can't separate out the down-blenders, it does become --

DR. MAURO: Exactly. Exactly.

MR. STIVER: -- an issue of all workers from `73 on.

DR. MAURO: Right. But, I mean, if we can't resolve this aspect of the conversation, we are almost done; we are really done. But if everyone feels, okay, I think we have aired this out adequately, we understand the issues, we have to make our judgements, now we get to the tough problem.

The tough problem is the down-blending because the down-blending represents, if you accept that, let's say just for the sake of this conversation, you accept the `73-to-`86 400 as the baseline. Then, you say, but we've got a problem; there are spikes that come in from time to time where there are going to be some workers -- we don't know who they are -- that have worked with material, down-blending it, that could have been on the order of thousands of parts per billion for short periods of time and for a handful of people. We don't know how long it is, and we don't know who those people are. What do you do with that? To me, there's your showstopper.

Now NIOSH could make a case -- and I am not going to say that they would be wrong -- they could make a case, say, listen, there's enough conservatism built into the 400 that it covers all ills. That is, those little spikes that occur from time to time, they are not there for long periods of time. So, therefore, the 400 has enough fluff in it to cover it.

I have to say, until I see that done quantitatively, I am not comfortable buying off on that technically. I understand intuitively why one would make that argument, but I think we all have an obligation to the Work Group to be a little bit more quantitative on why we feel those spikes are not of such an extent that they upset the apple cart for at least some workers. And so, right now, I think therein lies the SEC issue.

CHAIRMAN CLAWSON: I understand what you are saying, yes. That is also why this is coming before the Board, too. It is because we have come to this point and, basically, we are at a point right now that neither side really agrees. It basically comes down to the Board's decision now.

DR. MAURO: Yes. Now the only thing I will ask of NIOSH is, do they feel -- I know your position, and I respect your position -- that those spikes are something that should not upset the apple cart. But I think you haven't really made your case why they don't upset the apple cart. You don't leave us with enough information, analysis, quantitative or semi-quantitative analysis, that shows why the 400 will cover that problem. And without that, you know, me at least, I think the SC&A team is at a place where we can't say, yes, that does the trick.

MR. ROLFES: This is Mark Rolfes, and I was just speaking off to the side with

Bryce Rich.

Speaking with him, these down-blending operations truly only occurred probably for about maybe two weeks per year, if that. So, essentially, we are talking about a very short-duration exposure potential during the down-blending operation, say up to two weeks. I don't know the exact.

But these were something that occurred over a very short amount of time in any given year. It was a campaign-based type of operation to down-blend the materials.

Keep in mind, I think the recycled uranium materials being processed at the Fernald site were a small fraction of the total quantity of uranium being processed over the history of the site. I don't know the exact percentage right off the top of my head, but maybe Bryce might or might have some additional insights into the potential.

There could be an exposure potential during the down-blending. However, this is going to be a very short duration, possibly one or two days in any given year, possibly two weeks.

MEMBER GRIFFON: Bryce, it was more than just the ash that we are talking about, right? It was the decom from the CIP/CUP?

MR. RICH: CIP/CUP was really relatively low in ratio. The CIP/CUP came primarily from the cascade internal. So, this is stuff had already been decontaminated, but was in all the uranium that came out on the front end. And so, when you run it through the cascades, it is lesser than certainly in the ash.

MEMBER GRIFFON: There's some really high ratios in the CIP/CUP central, but --

MR. RICH: On some isotopes.

MEMBER GRIFFON: Neptunium especially.

MR. RICH: Yes, neptunium.

MEMBER GRIFFON: Not as much plutonium, I agree, right.

MR. STIVER: That is captured in the group.

MEMBER GRIFFON: So, I guess what I am getting at is that it was more than just like seven or eight drums of ash. It was other stuff, too.

MR. STIVER: Yes, there were hundreds of metric tons. And you know, Mark brings up from the dose reconstructor's standpoint, this is a small amount of material, but in this program you have got to understand the issue here isn't how big the dose is necessarily or how small one subgroup is. It is, can the doses be reconstructed with sufficient accuracy?

MEMBER GRIFFON: That is really where we are at.

DR. MAURO: You know how I think about it, too? Let's say material comes in from time to time, and there is a two-week period where you are down-blending. So, it really isn't -- in terms of the broad sweep of decades. Two weeks it comes in, I don't know, once a year. I don't know how often this happens and when it happened.

But I put myself in the position, well, there may be a guy or a group of people that, when that stuff comes in, that is what they do. Okay? So, I asked myself, well, if that is possible, and I say, okay, well, this guy has got his baseline of 400, which clearly we all agree is very bounding in itself. Then, we are saying, of course, along comes this tower ash one week in 1982, and he works with that. And he gets his inhalation.

Does the 400 cover him, if he is the guy or that group of people who from time to time do that job? You know, the answer is I don't know. What do you do in a situation like that?

One side of me says, you know, that 400 has got to do it because there is so much fluff built into it already, but the other side of me says, well, listen, we are working with a very unusual regulatory framework that imposes a threshold that is pretty tough. I could see someone arguing, "No, you really haven't placed a plausible upper bound on everyone here."

That particular guy, if he did exist, you may not be giving him the benefit of the doubt. And I will be the first to say I don't know what you do at this point. But I think we are reflecting the facts on the ground as best we see them.

MR. ROLFES: So, I guess what it comes down to, would the exposures that were incurred, you know, those other 51 weeks out of the year or 50 weeks out of the year, while that specific operator was not involved in down-blending operations, would the application of the 400 parts per billion to the uranium intakes that we'd assign for the entire year --

DR. MAURO: Yes.

MR. ROLFES: Would those intakes account for any potential exposures that occurred during the one-week period of down-blending?

DR. MAURO: Exactly. That is the perfect question.

MR. RICH: This is Bryce.

Process Subgroup 10A, which is the materials that are coming in directly from the gaseous diffusion plants, even though the number of data is not large -- it is only 39 data pieces -- the 95 percentile default on that one would be 1732 as compared to 400. And so, it is a factor of four and a half higher than the 400. Whereas, the normal exposure of the people that did the down-blending would not be 400. It would be in the eighties probably maximum. And so, if you take time of exposure versus the normal 1700 to 80, I just feel that this is adequately bounding for even those down-blenders.

MR. MORRIS: Ted, this is Bob Morris.

MR. KATZ: Bob, go ahead.

MR. MORRIS: I just wanted to point out that we have an interview with a Process Engineer, a former Fernald employee, dated September 11th, 2007. In that interview, the controls and the approach for the blending operations is described in some detail.

I think it is important to note that he says the material was not just dumped in; it was actually -- there was a careful process that it was done with because of the value of this material.

And so, I think that this down-blending question needs to be considered in light of our documented interview about how that process was done.

DR. MAURO: This is John.

I think the two statements that were made by Bryce and Bob just now are exactly where this conversation should be. What I mean by that is a weight of evidence is being put forth now that takes us the next step. Whether it is too little too late, that is your call on the Board.

But, basically, what the argument I am hearing is, if we accept the 400 as a baseline, and then you listen to the argument Bryce just made and the information that Bob just brought to the table, in effect, they are trying to put factual information on the table that says, you know, I think one could argue convincingly that the 400 that you apply for the entire year is going to account for this blip that might come in every two weeks for this particular worker.

So, if you could bring it there and make that case quantitatively, along with the other information of Bob, you are addressing the problem at least. What I am getting at is, rather than arguing, what we are doing is we are almost like taking our hats off and looking at it, and trying to think about, does their position give enough weight of evidence that says, yes, I think it meets my test. But, in the end, it has got to meet the Board's test. But I think the kinds of things that are being said really are of great value to inform the decisionmaking process.

MEMBER GRIFFON: Are there any other documents in your references, Mark, that speak to the campaigns, the timing, how frequently? Because I have heard two weeks, one week, one day per year.

MR. RICH: There are a couple of documents that talk about the fact that the material that came in, they sometimes had to wait until they had material with which they could blend it. And so, it was not done all at once. It went over a year or so.

MEMBER GRIFFON: Okay. Because that is the most --

MR. RICH: Yes.

MEMBER GRIFFON: I mean I think Bryce's argument with the 1700 and the timing is the most convincing to me, if we can piece together that timing. Because I don't know enough about how many campaigns.

MR. STIVER: This is John Stiver.

I found one reference related to the Plant 4 repackaging. It is not really exactly the down-blending, but it does give an idea of how long it took to reprocess these five or to repackage these five hoppers. It was about a three-week period. And so, you extrapolate that to the full batch of about 10 weeks just to do the repackaging.

MR. RICH: And any one hopper did not take --

MR. STIVER: Yes, they have it. There is a table in the reference that shows which days which hoppers were done. It is about three or four days per hopper.

MR. RICH: Right.

MR. STIVER: And so, that at least

gives you kind of an idea of what it would take to --

MR. RICH: That was not a continuous operation.

MR. STIVER: Yes.

MR. RICH: A hopper contains about -- I forget what it is --

MR. STIVER: A couple of tons.

MR. RICH: -- but major large barrels. So, it is a matter of simply repackaging; the barrels could be handled remotely.

MR. STIVER: Evidently, there is quite a bit of hand work involved in breaking up bigger chunks in order to get it into the barrels and that kind of thing. So, that at least gives you a little bit of a baseline on what it would take to do one part, one portion of the job.

As far as the actual down-blending in --

MR. RICH: And by the way, those

hoppers accounted for a major share of what came in in the 1980s.

MR. STIVER: Yes.

MEMBER GRIFFON: Since I absolutely have no time to go through all those references, can you narrow that down for me, that reference or the Site Research Database number for those?

MR. RICH: If John's got it, that's fine.

MEMBER GRIFFON: Okay.

MR. RICH: Otherwise, I can get it to you.

MEMBER GRIFFON: All right. Thanks.

And that earlier one I talked about, Bryce, if you have it, which gives the data for the 600 samples or whatever.

MR. RICH: I think you have got that also. John said that he --

MR. STIVER: This is SRDB 33730, sampling of Plant 4 packaging. That is the one that talks about, has the table that shows it.

MEMBER GRIFFON: 3373?

MR. STIVER: 33730. I could just send it to you. I've got it here.

MR. RICH: John, you said you had the Bassett report for him also?

MR. STIVER: Yes, I have got the Bassett report, yes.

MEMBER GRIFFON: That is the one that has some samples and --

MR. RICH: Right.

MR. STIVER: I only have the report. I don't have the actual raw data to go with it, though.

MR. RICH: No, no, the Bassett report did not have the raw data.

MR. STIVER: Yes, it didn't have the raw data.

MR. RICH: It has a summary. Gene has it, though.

MEMBER GRIFFON: Oh, you have the

raw data?

MR. RICH: Yes.

MR. STIVER: The data is available.

MR. RICH: Yes, we have the raw data.

MEMBER GRIFFON: And is that in the Site Research Database anywhere?

MR. RICH: No. Well, no, it's -- ask Gene.

MR. ROLFES: Are you referring to the spreadsheet?

MR. RICH: No, the urine and fecal sampling data is in the Site Research Database, and Gene knows where it is.

MR. ROLFES: Yes, correct. The plutonium urinalyses that were taken from Fernald employees, and, then, I believe also, you know, of those 500 samples that were taken, there were 10 which were approaching the minimum detectable amount. So, those 10 people actually were lung-counted to look for any kind of long-lived transuranic materials in their lungs.

MEMBER GRIFFON: Can you just give us that number?

MR. ROLFES: I don't have the Site Research Database number.

MEMBER GRIFFON: Gene has that number?

MR. ROLFES: I can definitely identify it for you. If Gene has it on the phone, definitely please go ahead and provide the number. I just don't want to put you on the spot if you don't have it, though.

MEMBER GRIFFON: I mean I think we have got the arguments, right? I think we have just got to kind of consider this further. I am not ready to --

CHAIRMAN CLAWSON: I just have --

MR. KATZ: I'm sorry. I just was going to check. Gene, are you on the line?

MR. POTTER: Yes, I am.

MR. KATZ: Did you hear that

question about whether you have the Site Database reference?

MR. POTTER: This is for the 10 whatever they were, higher employees?

MEMBER GRIFFON: Yes, the 10 or that whole set, yes.

MR. ROLFES: The entire 500 bioassay samples for plutonium conducted on Fernald workers in the 1980s is what we are asking about.

MR. POTTER: Yes, I think I can come up with that in a minute or two here.

MR. KATZ: Okay. Great.

MR. ROLFES: If you don't have it, I can come back to it after lunch as well.

CHAIRMAN CLAWSON: That's fine.

But one person just left.

Let me ask Mark then, so you are saying that this down-blending and stuff only happened two weeks out of the year?

MR. ROLFES: Yes, up to right now, basically, everything that we have seen could have been a one-day-type operation. It was a small campaign blending materials that we can say we can conservatively put an upper bound on the quantity of time that it took to down-blend the materials. But based on everything that we have seen, it is a pretty short duration.

I don't know if Bryce has anything to add about the duration, you know, how many weeks, up to how many weeks per year could this have been done on any given year, the down-blending operation?

MR. RICH: We haven't looked in that much detail. But we do know it took a couple of years to finish it all up.

MR. ROLFES: So, it was over a couple-of-year process, but it was campaigns that lasted, you know, it is a matter of short campaigns.

CHAIRMAN CLAWSON: So do we have something --

MEMBER GRIFFON: Yes, I would like

to know whether it is one day a year or a week a year.

MR. ROLFES: Sure.

MEMBER GRIFFON: Because then we can put that in perspective. If it is half a year, it makes a difference.

MR. ROLFES: Sure.

MR. STIVER: This is John Stiver.

The way I envision this is we have a couple of different scenarios that are more claimant-favorable-based, but you have to have some baseline to at least get some reasonable value of what that time period was.

CHAIRMAN CLAWSON: I want to see some kind of data that says, oh, yes, it was only this --

MR. STIVER: The only definitive data that I have seen is for the repackaging. I think you actually have the exact point in time it took, and that is in this reference I am sending you.

MEMBER GRIFFON: Can I ask one

more question? I think we are kind of winding down as much as we can say on this topic today.

MR. RICH: And by the way, this is Bryce again.

As John I think alluded to, that is not the blending operation. That is just the repackaging operation.

MEMBER GRIFFON: Right, right.

MR. ROLFES: You said it was three weeks, correct?

MR. STIVER: It was three weeks for five hoppers.

MR. ROLFES: And that was the worst-case material, right? That was the Paducah tower ash --

MR. STIVER: I don't know whether it was the worst case. It was what was processed, the 168 metric tons that were done.

MR. ROLFES: Okay. So, that was the Paducah tower ash that came --

MR. STIVER: It was one-third of

the tower ash. We don't know which of those hoppers were --

MR. ROLFES: Okay.

MR. STIVER: Actually, we do know.

MEMBER GRIFFON: Actually, it is one of the worst cases.

MR. ROLFES: So, it was in the 1980s, is what I was getting to.

MR. STIVER: From 1982.

MR. ROLFES: Was it that shipment, those T-hoppers?

MR. STIVER: It was in the T-hoppers, yes.

MEMBER GRIFFON: And this is a little tangential, but if we are winding down this topic, on the top of page 12 this is back to that same Bassett study. And I understand your argument that you are making relative to the uranium measures that were found in this for these 10 individuals' bioassays, that they ranged from 2 to 5 micrograms. So, therefore, how could you possibly have -- you know, it doesn't support having that much plutonium of an intake.

My question is, at that time period was the MDL for uranium that low? In other words, you are below 7 micrograms now. You are reporting a range of 2 to 5. It wasn't fluorometric? Or this may have been a later period.

MR. RICH: I think it was fluorometric.

MEMBER GRIFFON: It was fluorometric? And the MDA, I mean these are below the MDAs that I have seen. I don't know in the later time period. I mean 7 micrograms is normally the value I have seen.

MR. ROLFES: Yes, it changed over time. That time period was right around the same time period where they had dropped from about a less than 10, is the way they would report things. Right around 10 over the operating history was the detection sensitivity, the fluorometry.

And, then, right in that time period, it dropped down. I would have to look specifically at the year because in some years it was less than 5; in other years it was less than 3. And so, yes, this is basically less than detectable.

MEMBER GRIFFON: Near MDL levels. Okay. All right.

And I will see this when I get this part that Gene is looking for, but I mean the data that you have, the raw data we are calling it, it is not something that you took out or extracted from a database or anything?

MR. ROLFES: This is the raw data, correct, yes.

MEMBER GRIFFON: It is actually urine cards or whatever?

MR. RICH: No, no. No, no. That is in the database.

MR. ROLFES: These data do also appear in HIS-20 as well, in the electronic --

MEMBER GRIFFON: Okay. Well,

then, okay, I think we need to see that because, I mean, something I ran across was some of this stuff at Paducah. We actually found a couple -- it wasn't a lot -- but a couple of cases where, because the database at Paducah was built many years later, that this 2 to 5 micrograms could actually be 2 to 5 milligram.

The people that were entering the data in later years, when they saw these values, they just assumed micrograms. When you went back to the cards, you said, oh, my God, this is the earlier year stuff and they really had milligrams of uranium exposure, you know. So, that is why I asked about the raw data.

The 2 to 5, if you actually look on your Table B-1, it would actually predict plutonium levels that were near MDL.

MR. ROLFES: I understand what you are asking, yes. I wasn't sure if you were asking about the plutonium raw data or the uranium raw data.

I just wanted to reiterate, we did do an analysis, a comparison of the accuracy and completeness of the HIS-20 database. We did compare hard-copy results.

MEMBER GRIFFON: Well, you did a statistical analysis.

MR. ROLFES: Yes.

MEMBER GRIFFON: That doesn't necessarily speak to these 10 samples.

MR. ROLFES: The plutonium samples, no, but the 2-to-5-microgram range would be the uranium values, not the plutonium.

(Simultaneous speaking.)

MR. STIVER: Yes, I think it was safe to say --

MEMBER GRIFFON: People that were exposed during the mid-eighties, right, because you are talking about that campaign.

MR. RICH: They took these samples in 1986. That was the 440 people that they selected from most-likely-exposed, and that is the data we are talking about.

MEMBER GRIFFON: Okay.

MR. RICH: So it's later.

MEMBER GRIFFON: Yes.

CHAIRMAN CLAWSON: Okay. Do we need to discuss down-blend duration more?

MEMBER GRIFFON: The only actions, I think in terms of actions, it's just the supporting documents that can help us to understand the campaigns. How long? You know, what was involved?

MR. STIVER: Really what we need to do is --

MEMBER GRIFFON: If you have some documentation to that effect that we can, then, tie the year for that subset, you know, the 1750, or whatever it was, 1732 parts per billion, I think that would give us some idea. That is sort of the strongest argument that I heard in terms of being --

MR. STIVER: That is really what I

would like to see. That would give us an idea of whether the 400, really, you know, those spikes are washed out.

MEMBER GRIFFON: Right, right.

CHAIRMAN CLAWSON: I just want to check with Paul and Phil, make sure that if you have any questions -- understand, this is coming before the Board in basically two weeks. So, this is kind of at the end of things to be able to catch up all of our loose ends that we had hanging out there because we had some papers that hadn't been reviewed, and so forth, like that.

And I just want to make sure that Paul and Phil, or, Bob, even if you are on there, do you have any questions or comments?

MEMBER ZIEMER: This is Ziemer. I would be glad to make a couple of comments.

CHAIRMAN CLAWSON: A 30-second sound bite?

(Laughter.)

MEMBER ZIEMER: Okay. Or more.

Well, first of all, I really felt that the NIOSH analysis, the August 5th analysis, was very well-reasoned. And I agree with John Mauro's comments in how he was evaluating it in terms of the idea that there is every reason to think that the 400 parts per billion default would certainly be bounding for the normal operations.

To me, on this down-blending issue, the only thing that would convince me that the 400 was still not bounding would be if someone were able to show that the down-blending operations extended beyond the couple of weeks that it seems to be what they were. There would have to be clear evidence that these were extensive and more regular.

And keep in mind that the concept of bounding, as NIOSH has used it in the past -- and I know John Mauro and I have had a lot of discussions on this, as to whether it is reasonable. Sometimes we bound too high. I think one could argue that perhaps 400 is almost close to too high in terms of being someone actually getting that, but it is not perhaps unreasonable. But in any event, I don't see any way that the 400 would not be bounding unless one were able to show that these down-blending operations were very extensive.

And, then, the final comment is that, as bounding is used, I don't believe it always guarantees that there might not be one person on a site that would exceed it. You know, we are trying to sort of hit it at the high probability that we have covered everybody, but you can never show that there might not be one person that exceeded some bound conceptually.

But the 400, to me, is not only reasonable, but extremely generous, unless, as I have said, one could document that there was extensive down-blending in terms of the time and the operations. Otherwise, I agree with John that this 400 would make a lot of sense.

And I think, again, to me, the 100 is also reasonable for the earlier time period, when you look at the data.

CHAIRMAN CLAWSON: Okay. Appreciate that.

Phil?

MEMBER SCHOFIELD: The only question I have is, in the use of this magnesium fluoride, how much of the concentrations, what levels actually increased in there? So, the exposure potentials for those people working with it is -- I don't know if that is a real serious problem or not. I really don't know, but that is something I was wondering about.

As they recycled this material, are they getting these lighter elements in there where they might have more of a neutron factor in there?

CHAIRMAN CLAWSON: This was with the bombs, reusing the --

MR. STIVER: I am not quite sure

what you are getting at.

CHAIRMAN CLAWSON: Phil, could you clarify at what point you were --

MEMBER SCHOFIELD: Yes. On those bombs, when they recycled the recycled materials back through the process, when they were doing these reductions, I was wondering if there are contaminants picked up in this that start getting concentrated. And are those a real concern from the standpoint of exposure?

CHAIRMAN CLAWSON: Is that the dolomite?

MR. STIVER: That is the issue that we are discussing here, was the concentration of plutonium and neptunium and fission products in the dolomite through reuse.

MEMBER SCHOFIELD: How high do those concentrations get, ultimately?

MR. STIVER: The dataset that we have, of course, remember, it is on a part-per-billion uranium basis. I think the highest was about 8,000, but the 95th percentile was around 400 parts per billion.

MEMBER SCHOFIELD: One last question. Was there administrative guidelines that said, when these concentrations got to certain levels, the dolomite was not recycled through the process?

MR. STIVER: I haven't seen anything to that effect.

Bryce?

MR. RICH: No.

MEMBER SCHOFIELD: Okay. That's the only questions I had.

(Off record comments.)

CHAIRMAN CLAWSON: Sandra, I apologize, I didn't mean to interrupt you. I was just trying to stay on track.

MS. BALDRIDGE: That's fine.

You know, as I am listening to all this, and even what was discussed at the previous meetings, it is almost like there is an attitude that, if we can get the bounding levels high enough so that it is all-inclusive, then we don't have to deal with the sufficient accuracy requirement of the SEC because you don't know who was exposed, what they were exposed to, where they were exposed to it, or when they were exposed to it.

And if you get the level high enough and you get them under there, then it kind of is giving the impression that it is an attempt to blanket and cover the fact that they don't know who, what, when, and where these people were.

MR. KATZ: Sandra, the rules, actually, that is, having a single bound in certain circumstances is fine under the rule. And that is actually an appropriate approach for an SEC question, provided that the bounding is based in material fact and is reasonable, and so on.

MS. BALDRIDGE: That's the point. Is it fact or is it logic?

MR. KATZ: Right.

MS. BALDRIDGE: Because the plausibility issue gets into the subjectivity of the individual who is evaluating the information. And everybody's thought processes are different.

One of the problems with the worst-case scenario is one dose reconstructor was looking at a file and, based on their interpretation of the plausibility that they had sufficient exposure, determined whether the dose reconstruction was done using the OTIBs available or they actually looked into the data and the possible situations concerning exposure.

MR. KATZ: But individual dose reconstruction, what gets done there is really a separate matter. And here, the judgement is being applied, there are judgements being applied to facts. But, I mean, you don't have it -- you are not relying on one person. You are relying on two organizations providing input to then a Work Group, which is providing its consideration of those judgements, and then, ultimately, the entire Board providing its considerations to judgements.

So, there are judgements involved as well as data.

MS. BALDRIDGE: Right.

MR. KATZ: There always would be. But those are sort of highly-deliberated on and, then, will reflect the perspectives of all these different parties.

MS. BALDRIDGE: Right. I appreciate the process, and I know there has to be consistency across the board for all the SECs, that each one is dealt with in the same way, because that is the only way it is fair.

MR. KATZ: Thanks.

CHAIRMAN CLAWSON: And, Sandra, I will be honest, this has been, what you just said is something that the Board deals with continuously. It is very difficult for many of us to -- it goes against us sometimes. I will be very honest.

But these are what we have been given and how we have to be able to deal with it. We earnestly -- I can say that all sides try to do the best that we can.

MS. BALDRIDGE: I realize that.

CHAIRMAN CLAWSON: And we have further claimants in mind. Sometimes it is very difficult, and Fernald is a difficult site. From the outlook of it looking in, I can tell you truthfully it looked like a very easy site until we got into it. And the information, and the lack of information, and the ability -- it is very hard, and we try to deal with the facts that we do have and how we can do it.

But I understand your point and I appreciate that.

MR. STIVER: How are we doing? It is right around noon.

CHAIRMAN CLAWSON: Right around noon.

Before we go into something else, I think I would say we could go to lunch because I don't want to open up something and leave in the middle of it, if that would be all right with everybody.

MR. ROLFES: Brad, if it is okay, if I could just identify the files for Mark?

CHAIRMAN CLAWSON: Yes.

MR. ROLFES: There are multiple ways of querying our Site Research Database, and that is what I have done here just while we are sitting here.

MEMBER GRIFFON: Oh, okay.

MR. ROLFES: I searched for "plutonium urine" for the first set, these first three references. I don't know if I need to read it for the record. And, then, I searched for "plutonium bioassay" for the second set.

So, the first search resulted in three files. The second search resulted in one, two, three, four, five, six, seven, eight, nine, ten, eleven files.

Let's see.

MEMBER GRIFFON: But if Gene knows the specific one that goes to this, that would be great.

MR. ROLFES: Okay. The specific urinalysis data I believe are in the first one, 4158.

And if Gene has something different, he can clarify it now or --

MR. POTTER: It's 94117.

MEMBER GRIFFON: 94117?

MR. POTTER: Page 69 to 76.

MR. KATZ: Sixty-nine to 76, pages.

Okay. Thank you, Gene. Right.

CHAIRMAN CLAWSON: Okay. With that, I would suggest that we break for lunch, come back about one o'clock.

MR. KATZ: Thank you, Brad.

Thank you, everyone on the line. We will be back with you at 1:00.

(Whereupon, the foregoing matter went off the record for lunch at 11:53 a.m. and went back on the record at 1:03 p.m.)

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

1:03 p.m.

MR. KATZ: So, good afternoon.

We are back from lunch break, and we are about to get started on thorium issues, I think.

Let me just check on the line and see if we have our Board Members.

Dr. Ziemer and Mr. Schofield?

MEMBER ZIEMER: Ziemer here.

MR. KATZ: Thanks, Paul.

How about you, Phil?

(No response.)

CHAIRMAN CLAWSON: Mute?

MR. KATZ: Phil, are you on the line?

(No response.)

Okay, not at this moment.

CHAIRMAN CLAWSON: Okay. This is Brad.

We are going to continue on. I don't think we have really come to a resolution on the uranium. I think we have all voiced our concerns. We have got a little bit of information that we need to review, and so forth.

But we are going to continue on to the thorium-232 post-`68 era. And, SC&A, I believe the ball is in your court to respond.

MR. STIVER: Yes. This is John Stiver.

We produced two responses to NIOSH regarding the thorium chest count data which was used to assess thorium-232 intakes from 1968 to when they stopped using the other data, up until the closure of the facility in 1989. And we really looked at it from two perspectives.

One was the adequacy of the data. Basically, is the pedigree of the data good enough to use in dose reconstruction?

And, then, kind of separately, we looked at, assuming that the pedigree was acceptable, were the data sufficient in numbers and types to be useful in creating a distribution which would capture all subgroups of concern?

Joyce, are you on the line?

DR. LIPSZTEIN: Yes, I am.

MR. STIVER: Okay. Joyce produced a -- let me back up just a minute. At the April meeting we were discussing this issue. I believe it was Bob Morris had mentioned and Mark had mentioned there was some information on calibration for the Y-12 mobile in vivo system from about 1965, which is very similar, if not identical, to what was used at Fernald.

And we had had questions regarding the calibration methods, particularly for the data that was reported 1968 to 1978 in units of milligram for thorium. From 1978 on, it was reported in nanocuries and there were actual measurements available of lead-212 and actinium-228, which allowed then a reconstructor or other interested party to assess or at least estimate the age of the thorium source term.

So, Joyce, if you would like to present your paper and give some highlights there?

DR. LIPSZTEIN: Okay. Can I just make a very small introduction to the problems of measuring thorium? I think we have been through this a lot of times, but it is always good to repeat.

It is very difficult to interpret the data from thorium monitoring in lungs because you don't measure thorium itself. Thorium decays, thorium-232 decays to radium-228, which decays to actinium-228, which in turn decays to thorium-228 and radium-224, then radon-220 and polonium-216, and then lead-212.

So, in order to calculate the lung burden due to thorium-232, you either have to rely on measurements of actinium-228 or lead-212. And what's the problem with that?

The problem is that you have to

make a lot of assumptions to interpret the data that you measure in the lung. So, the first thing that you have to make is an assumption about the source. You have a thorium-232 source that was separated from the daughters. So, you only had thorium-232 and thorium-228.

Depending on the age of the source, the daughters would start to build, and thorium-228 decays in a different rate than thorium-232 because it has a much shorter half-life. But, on the other hand, it starts to build from radium-228 also. So, when you measure lead-212 and actinium-228, you have to relate to the age of the source

The second thing is in the lung itself. You have the lung, and you have to know how much time. Once you have established what was the equilibrium of the source, what was the age of the source, then you have to know how much did daughters and the thorium have decayed in the lung itself. So, you have to know the time after exposures that a worker was measured, and you have to know if it was a chronic exposure, if it was an intermittent exposure. Like you measure, and then after some time, the worker is exposed again and he is measured again. So that the daughters that you are measuring in the lung, they come from two different equations, from how much was in the source at the various times of exposure and how much it decayed on the lung, because you didn't measure just after exposure.

And there is a third thing that complicates this thing. It is that the test location rates for the daughters is not the same as for the parent. For example, there are many studies showing that radium-228 goes out from the lung, translocates from the lung at a faster rate than thorium.

So, if you imagine you had a thorium source that was Type M and you had radium Type M, but even radium and thorium Type M, the radium will translocate faster.

But if you had a thorium source that was Type S, then radium can only be Type M. So, depending on where the radium was produced in the lung, it would come in a faster or not rate from the thorium-232. But if the person had inhaled also radium-228 from the source, then this radium certainly would have a faster translocation than the thorium-232. So, when you measure the lead-212 that comes after or the actinium that comes after the radium, you have to know this. You have to interpret what was the ratio of the thorium-232 to the radium.

So, I want to make sure that everybody knows that it is very complicated. It is not just a question of this problem here, but it is very complicated to interpret in vivo measurements of thorium in lungs through the daughters. Okay?

So, let's come to what was done. Is that okay?

DR. GLOVER: Joyce, this is Sam

Glover. I would take a little exception to the radium-228. I completely agree that they were separate source terms; they will dissolve at their own rate.

And I have made the oxalates from aged thorium and watched the solubility characteristics of radium-228 compared to thorium-232. Thorium-232 crystalline structure prevents radium from leaving except at the surface. It is a surface-area-dominated effect. It is just like plutonium and americium. It can't dissolve faster than the thorium dioxide crystal.

So if it is an aged material that was born in that oxide, it is not going to change that. It is going to dissolve like the thorium does.

It is a complicated measurement. It is hard to do, especially --

DR. LIPSZTEIN: Yes, that is what I was telling.

Now there are also some studies

where, even if you have the thorium in the lung which is insoluble, radium can dissolve, translocate faster sometimes. There are some studies in the literature about that.

But the other thing is that you have to know how much radium came from the source itself, which depends on the age of the source. So, this radium certainly has a translocation rate differently.

And you are measuring the actinium. So you have three things at the same time that you have to interpret with just one measurement. So that is what makes it very complicated. It is not only here. Right?

MR. ROLFES: Joyce, this is Mark Rolfes.

Yes, we do understand it is complicated. However, we don't believe it is an issue that cannot be resolved by making claimant-favorable assumptions of the age of the material --

DR. LIPSZTEIN: Okay. Okay. I don't agree with you, but let's go on. I just wanted everybody to understand that it is complicated, that you have three things to know at the same time.

And I wanted to make sure, also, that we will see in the other paper that is probably going to be presented by Bob and Harry that you also have to know the time after exposure that workers were measured and how they were measured. And you will see that probably the thorium measurements were, you know, the in vivo -- I'm sorry, the in vivo measurements were not geared to thorium workers. So you don't know exactly how much after exposure they were -- how long after exposure they were measured.

And you will see, also, that some people that had positive measurements of thorium were not working in the areas where, for example, the Technical Basis Document on internal dosimetry defined as a thorium working area. So, because of that, you also don't know how much time after exposure these workers were measured. You know, you have a mixture of everything and, also, the problems of measurement.

But, okay, the second thing is there was a period of time from `68 to `78 where the thorium results were given in milligrams. And from `78 to `88, the thorium results were not given in milligrams of thorium, but they were given in nanocuries of actinium-228 and nanocuries of lead-212.

So at least during this period after `79 you have the raw data with which to work. But for the period of `68 to `78, you don't even have the raw data; you just have milligrams of thorium.

MR. ROLFES: However, Joyce, though, we do have information on how much material was in the lung. We know how much thorium-232 was in the lung. And once again, you can make assumptions about the age of the material that will allow you to get the activity results.

DR. LIPSZTEIN: No, you can't because you don't know how they were done, and there are too many uncertainties.

I will start going now one problem over the other, so that I can explain myself because I don't think you can make such assumptions and you cannot make what you call the favorable assumptions for the client because you don't know many things. You don't know three different things: the age of the source, how long had passed when people were measured and what material was measured, and the translocation rate.

So, okay, let's start from the beginning. The first thing is we want to know, do we know the sensitivity of thorium measurements in milligrams? I don't think we even know that.

In the paper, in the White Paper, it was stated that the lower limit of detection was 6 milligrams. Okay, the lower limit of detection can't be milligrams, the first thing, it can't be milligrams of thorium because it will depend on how much lead was measured or how much actinium was measured. So you have a minimum detection level that would be in nanocuries of lead-212 and in nanocuries of actinium-228. Okay, that is the minimum detection level.

So when you transform it, you have to know the two things. You have to know the age of the source and you have to know the time after exposure that workers were measured. So, without knowing -- I am just ignoring for now the different translocation rate. I am just talking about the physical decay now.

So even if the translocation rate was the same, you would have different sensitivity limits, depending on the age of the source and on the time after exposure that the workers were measured.

The second thing is that we have a paper, NIOSH cites a paper by Scott from 1966, from where this reference comes of a lower detection limit of 6 milligrams. But on this paper, the source that they were measuring probably was not a pure thorium source that was separated from all the daughters. It probably had some radium because it says that the calibration standard for this 6 milligrams was a thorium source that had a ratio of thorium-232 to thorium-228 of 1.27 and of thorium-232 to radium-228 of 1.67. And this cannot be; this is not possible.

So it probably had an excess of radium-228 on the thorium source. And even Scott in this paper, he indicates that this rate of exposure would be distinguished from thorium by repeating measurements over a long period of time and observing the decay and the growth pattern.

So that this minimum sensitivity doesn't refer to the same source that was in

Fernald, and this is 6 milligrams in the Y-12, for which you made the calibration for the -- whole body counter.

And also, there was a document from NLO from 1966 which contains a letter from 1966 commenting on differences between measurements of a worker who did in vivo monitoring at Y-12 and at the Wright-Patterson Air Force. And in this document, it states that the minimum detection at Y-12 at that Y-12 model in vivo laboratory that was used at Fernald, the detection level was 9.8 milligrams of thorium-232.

So I think that, in summary, we don't have too much. We don't have precise information on the sensitivity of the lung counting results reported in milligrams of thorium-232 material that was handled at Fernald.

The other thing is that we wanted to know how was the calibration of the countings done. And, then, NIOSH has posted to us a description of three phantoms that were used for calibration of activities. And it cites a paper by King and Barkley from 1983 describing the derivation of the conversion factors and three phantoms that were used.

So there was a phantom that was -- they describe a REMAB phantom which is a torso-shaped plastic shell that contains a human skeleton and was filled with tissue-equivalent organic fluids, and sponge material was used in the lung cavity to simulate lung tissue. And the small sources were inserted into holes in the sponge material. Okay. This REMAB phantom was used from the early `70s until `83.

This paper says that past studies have shown that monitoring results can vary by a factor of three or more with source positioning inside the lung cavity.

So, before the REMAB phantom, we don't know anything about the uncertainties on calibration, but we know from this paper that was used to describe the calibration better. Before `83, you could be erroneous by a factor of three or more due to calibration problems.

Okay. Then NIOSH describes how the thorium mass from chest count was calculated. It comes from a paper that was proposed by West in 1965 where they compute how much counting there is in the actinium region and how much counting there is in the lung -- 212 region.

But West's paper does not advocate to dismantle the monitoring for quantitative assessment of thorium burden in the lung, but there is a screening method to distinguish exposed from non-exposed workers. It even says that there are problems associated with monitoring of personnel exposed to thorium, such as the knowledge of the ratio of thorium-232 to thorium-228, since this ratio changes with time after separation of thorium from its daughters, and the fact that the interpretation of monitoring results depends on the metabolic or physical translocation of daughters away from the location of the parent stored in the body. Such translocation can affect the reliability and the sensitivity of in vivo interpretation if the gamma is measured at dose from daughters subject to translocation.

So, this paper, it says that this method that was used should only be used qualitatively.

And there is also posted on the O: drive by NIOSH a paper called FMPC Mobile In Vivo Radiation Monitoring Laboratory Calibration and Data Interpretation, Draft 01, by Robert Morris from May 6, 2011. So, we ran through the examples that they give.

They show, for example, that there was a measurement of lead-212 that had two results, 2.85 nanocuries and 3.15 nanocuries, with an average of 3 nanocuries. And, then, they had actinium-228 results of 2.75 and 2.8 nanocuries, with an average of 2.78

nanocuries.

So, they make the ratio from lead-212 to actinium-228 to decide what was the age of the thorium source. So, they showed that, for these results, this was shown in the paper. It is not an invention of results from myself. This is specifically shown in that paper from `76 that we had lead-212 to actinium-228 ratio equal to 1.08, which leads to the conclusion that the age of the source was 4.3 years.

And they note that the exposure source could also be in equilibrium, which would better agree with knowledge of the case history. So, they show that if they assumed equilibrium, they would calculate 27.6 milligrams of thorium-232. But if they assumed 43 years after prolification, they would have 56 milligrams.

If you were reading the paper that we wrote, I made an error here. I say, based on this example, a factor of 19 -- it is 1.9.

It is an error of two.

But, anyway, you see, the difference between a ratio of 1.08 to 1.0 to determine the age of the thorium, you know, it is very uncertain, and you can make a difference of two on what you are reporting in milligrams.

MR. STIVER: Joyce, this is John Stiver.

The last uncertainty, that last little example, is really just the impact of measurement uncertainty, isn't it? It is not even related to the actual age. This is just an uncertainty --

DR. LIPSZTEIN: No, no, because if you had 1.08, the ratio of the lead-212 --

MR. STIVER: I understand that.

DR. LIPSZTEIN: -- to actinium-228, and you put in the equation, you would think that the age of the source was 4.3 years.

MR. STIVER: I know --

DR. LIPSZTEIN: But if you had a ratio of 1, you would assume equilibrium. Now, if you assume equilibrium, you have a 27-milligram source. If you assume 4.3 years prolification, you have 56 milligrams.

MR. STIVER: I believe, though, that the problem here is the uncertainty in that 1.08 ratio, based on the measurement uncertainty of the lead and actinium. So you could have an uncertainty just in that measurement of a factor of two --

DR. LIPSZTEIN: Yes.

MR. STIVER: -- regardless of what the real age is.

DR. LIPSZTEIN: Yes. Yes, exactly.

MR. STIVER: And you also have uncertainty in the age of the material based on the actual age which range by a factor of two as well.

DR. LIPSZTEIN: Yes.

MR. STIVER: So you have two kind

of independent sources of uncertainty there.

DR. LIPSZTEIN: Yes, yes.

MR. STIVER: In addition to the factor of three -- up to `83 at least, when they were using the REMAB phantom.

DR. LIPSZTEIN: Yes, yes, exactly.

MR. STIVER: So, anyway, go ahead. Continue.

DR. LIPSZTEIN: And, also, in this same paper, there is a note stating that there were new calibration coefficients that cause a 4 percent difference in the lead-212 to actinium-228 ratio, which translates into a 16 percent difference in the equilibrium assumption.

In this paper, there is no precise information on when the new calibration precision started to be used, but the dates of the notes in this document indicate that it was near the end of `77. But we don't have any information from NIOSH on the changing of calibration coefficient so we don't know.

So, in summary, the method used to estimate thorium burdens in milligrams has too many uncertainties and probably should only be used for qualitative assumptions about thorium burden, as indicated in West, `65, in the paper of West from 1965, which is the paper that was cited as the basis document for the calculation of thorium mass from chest count data.

DR. MAURO: Joyce, I see where you are rolling up nicely our concerns regarding the milligram data.

I would just let everyone know on the phone -- again, you know, we did have a dress rehearsal the other day to talk about all of this.

DR. LIPSZTEIN: Yes.

DR. MAURO: And I played devil's advocate. I put on my NIOSH hat. Okay? I said, "Okay, Joyce, all right, we understand that this is strewn with many, many complex, interrelated factors that will lead you to say, you know, you really can't trust these numbers because they could really be off by a lot."

And I asked the question I always ask. Could they have underestimated by a factor of two? Absolutely. How about a factor of 10? How about a factor of 100?

And Joyce gave me an answer that left me in a place where I said she's uncomfortable even saying what that upper end could be. In other words, how wrong could they have been? Am I correct in summarizing that conversation we had the other day?

DR. LIPSZTEIN: Yes.

DR. MAURO: That is, if someone were to say, "Okay, Joyce, in light of all this, and looking at those numbers, how much higher could they have been," and I got the sense that you were not in a position to say, "I don't think I can answer that question." Is that a fair statement?

DR. LIPSZTEIN: That is a fair

statement. That is very true.

We don't know exactly what parameters they used to give those results. There are many complicated parameters. And we don't have the raw data. We just have the transformed data, what they had in actinium-228 and lead-212, and they transformed it into milligrams. So, we don't know. We don't know what these numbers mean.

DR. MAURO: And the information that Joyce has summarized leads you to the impression, you say, well, listen, why do we distrust the people who did this work? In other words, they reported those milligrams. Didn't they know about all this stuff that Joyce just summarized?

And clearly, the concerns that Joyce mentioned regarding the minimum detectable concentration, the calibration issues, these things are a window that perhaps they did make some of these misrepresentations, and not deliberately. So, it is not that we could just say, well, everything about the analysis, why do we distrust the numbers? They were good radiochemists.

But the reality is there are these other matters that Joyce summarized that says but there is too much information here that says these numbers are very soft. So it leaves us in a very uncomfortable place.

And the irony of all of this is that, when they started the chest count work -- I believe it was `69 -- they thought that was going to be an improvement over basing their analysis of thorium body burdens when previously they used the air sampling, the breathing zone data, and the DWE.

Interestingly enough, they were much better off with the DWE data, as we have covered previously. So it is ironic that here we have a later time period, starting, I believe, in `69, when in theory an improvement was made by bringing in the chest count technique. But, lo and behold, we find that they would have been, in terms of dose reconstruction, they would have probably been better off sticking with the breathing zone approach.

DR. LIPSZTEIN: But I don't know what they wanted with those in vivo monitoring, but if you read the papers, they would say that those were for qualitative assessment of who was exposed and who was not exposed.

DR. MAURO: As opposed to an attempt to be quantitative in terms of what the exposures were.

DR. LIPSZTEIN: Yes. Yes, exactly, yes.

So, qualitatively, if you measure something in the lung, it is because someone was exposed. Otherwise, it wouldn't be there. But what, quantitatively, this means, that is the problem.

DR. MAURO: And as I understand,

Joyce, from yesterday, I think it was yesterday or the day before yesterday when we talked, superimposed on all this is that the measurements that were made for the people that we have data for were not necessarily the limiting group. Is that correct?

DR. LIPSZTEIN: Yes, exactly.

DR. MAURO: I know Bob Barton did some work on that.

DR. LIPSZTEIN: Yes.

DR. MAURO: And so, I mean, on top of it all, we have this concern that the people that were measured may not have been the people that represent the people with the potential for the highest exposures.

DR. LIPSZTEIN: For example, I have one interesting example that is in Bob's paper, not on this White Paper that I wrote, but the one that Harry and Bob wrote. He has an interesting table showing that most of the positive measurements were from `69 to `71.

So if you look at `69 to `71, if

you look at the Technical Basis Document, you will see that from `69 to `71 what is cited there is that most of the thorium that was possessed was Type S material, which is logical to have more positive samples when you have insoluble material in your lung.

But, then, when you look at the workers that had those positive samples, they were from a building, they were working in a building where there was no thorium process that went on in that building. So we don't know from where those numbers come. But, of course, if they had positive measurements in the lung, that is because they were exposed. But how, why, why these workers were listed in a plant that was not listed as processing thorium? If they took those workers to other plants, nobody knows.

MR. STIVER: But, Joyce, before you get into the data completeness issue, would you talk a little bit about the data from `78 to `89?

DR. LIPSZTEIN: Yes, yes. Okay.

MR. STIVER: Nanocurie data?

DR. LIPSZTEIN: Yes, yes.

DR. MAURO: I am sorry to interrupt, but I think what we have just done is look at a time period from, I believe, `68 to `78, which says that we think you have a very serious problem in reconstructing doses from thorium during that time period when they are reported in milligrams, very serious.

And now, Joyce, I think you are going to go on to talk about the post-`78 time period, when the measurements were -- there is an improvement, but they also have some problems.

I just want to make a very clean break between what we just talked about, which I think is one of the single-most problems, greatest problems that we have encountered.

But I will let you go and continue with the post-`78.

DR. LIPSZTEIN: Okay. The first

thing about the post-`78 that we already know, that until `83, when they -- the phantom from the Lawrence Livermore phantom, we had a factor of three or more on calibration problems.

Then we also -- let me go here to `78. And then, we have the same, well, not the same problem, because we have the raw data. Okay? But we have the problems of having actinium-228 and having two measurements, and we have lead-212 measurements.

Now we have to establish three things. We have to establish, number one, what was the source age? Second, we have to know how long after exposure workers were measured. And we have to know if they were exposed more than once and when. And we have to know if the workers were like exposed, measured every year. It doesn't seem like it. And who were those thorium workers?

Because if you have actinium-228

and lead-212 and you need to go from those data, go back to the thorium, how much thorium was in the lungs, with those data, you know, you can't have -- if you have two, X and Y, okay, so you have to have more -- you can't have more equations than you have data. Otherwise, you can't go back.

So you have a calibration problem that you don't know how to solve until `83. Then you have to determine the age of the source, how long after exposure people were measured, and how frequently they were exposed to the source before they were measured. And if it was more than one source, thorium source, that they were exposed before they were measured.

So all this -- will end up in just one measurement of actinium and of lead. And you know, it is just very hard to go back.

MR. STIVER: Joyce, for this later period -- John had kind of asked you if you felt that the earlier data could be resolved, and you were kind of hesitant about the possibility of that. For this later data set, do you feel that these types of issues are tractable, that it is possible to get at least a reasonable estimate of the age of the source term based on --

DR. LIPSZTEIN: Unless you identify who were the thorium workers and how long after exposure they were measured, and how they were exposed. I mean how means the time pattern of exposure. Would they be exposed for two weeks, then be out, then be exposed three months later? Or would they just be exposed for one day and not exposed anymore? Or would they be exposed for a whole year and then be measured after that year? Without those considerations, it is very hard to go back to the thorium.

MR. STIVER: It would probably be hard to get a real accurate quantitative estimate. But if you knew what the impact of those uncertainties might be on the result, would it be, then, possible to factor in those uncertainties and at least get a bounding value?

DR. LIPSZTEIN: I think it is very difficult. Nobody has demonstrated to me that it can, but --

MR. STIVER: It seems like here you have at least got an anchor point. You have got two different measurements. You have got lead-212 and you have got actinium-228. From that, you can at least get an estimate of the age of the source.

And, then, from that --

DR. LIPSZTEIN: Of the age of the source? No, you don't, because you don't know how much time has passed between the exposure to the source and the measurement on the lung. Don't forget that you have the age of the source, but the age of the source after it was inhaled in the lung also.

MR. STIVER: What I am trying to do is just kind of take each of these variables separately and kind of look at them.

DR. LIPSZTEIN: Okay.

MR. STIVER: For the moment, let's say you knew the time since exposure or say you had enough bioassay samples that you can kind of figure that out. Let's just take a look at just the age.

What I am trying to see, is there a way you can get an anchor point? So, you can at least have something you could hang your hat on here as a starting point. And, then, do an uncertainty analysis of these other variables to where you could at least come up with some kind of a bounding value you would have some confidence in, that wouldn't be some physiological limit of intake which would leave you with essentially no model.

And so, that is kind of where I am going with it. Do you feel that, given that you have more, the raw data, and it seems to be a little bit more robust, do you think that it is possible to derive a bounding intake from that dataset?

MEMBER ZIEMER: Comment, Brad? Brad?

MR. KATZ: Yes, Paul?

MEMBER ZIEMER: Yes, just a question and a comment. If we are talking about bounding rather than individual dose reconstructions, the issue of not knowing when the exposure occurred is very common on internal dose reconstruction. NIOSH has a methodology where, if you don't know when exposure occurred, you can develop the worst case. When would the exposure have to have occurred to give you the worst dose from the data that you have measured?

So if we are trying to find the bounding issue, you can look at that for all the data points. You can take, in principle at least, when would the exposure have had to have occurred, either chronically or acutely. And, also, you could take various ratios and get a worst case. What would the maximum dose have to be if you had this final measurement? And you could do that with all the data points, in principle, and find the bounding value.

So if we are talking about sort of like a coworker bounding value, it seems to me you could do that. In fact, if you were doing individual dose reconstructions and you had just the value, you could do the same thing. Say, okay, when would the exposure or the intake have to have occurred to give you the worst, the highest dose, given these results, both in terms of when it occurred, whether it was chronic or acute, and what the isotopic ratios would be?

So I don't see how this is different from other cases where we have dealt with internal exposures for bounding.

DR. LIPSZTEIN: I think the problem is that we are relating on the ratio of actinium to lead-212. As the material was separated, the actinium doesn't give a good measurement of the thorium that is in the lung. For a small variation of the ratio of actinium to lead-212, you have a very high difference of how much was the activity of thorium in the lung.

MEMBER ZIEMER: But couldn't you do a sensitivity analysis and see what those extremes were?

DR. LIPSZTEIN: Maybe. Maybe. I didn't see, look, I didn't see --

MEMBER ZIEMER: I don't know whether it has been done, but I am asking in principle. I think we have done this before in other cases. I don't know if Bob Morris or Mark, or one of you, can comment on that, or even John Mauro for other situations.

MR. ROLFES: Dr. Ziemer, this is Mark Rolfes.

DR. MAURO: Even John Mauro?

(Laughter.)

MEMBER ZIEMER: Even John Mauro.

DR. MAURO: Even John Mauro.

MEMBER ZIEMER: Well, I was hoping NIOSH would.

DR. MAURO: I want to sit out a little longer and listen to this.

CHAIRMAN CLAWSON: Everybody, we need to be very careful, especially on the phone, not to talk over one another because the court reporter is having a real hard time.

Mark had wanted to make a comment here.

MR. ROLFES: Thank you, Brad.

Thank you, Dr. Ziemer.

Yes, I think we have been discussing these issues. We put this White Paper out, just so everyone is aware, we put out this proposed model, I believe it was three years ago. We put out this model in 2008. I believe it was in May.

This is something that I think probably didn't really get discussed until about a year ago, it was first taken up and discussed in detail. We have had previous back-and-forths.

And based on what I am hearing, we are talking about a factor of two, a factor of three. There are many uncertainties involved. We acknowledge those uncertainties.

We have the data available to us, and we can make reasonable assumptions about those uncertainties, so that we can use those uncertainties to the benefit of the doubt of the claimant for whose dose we are reconstructing, based upon the facts, based upon, you know, everything that we know about that particular claim.

And I have some individuals on the phone, including Bob Morris, Liz Brackett, and Tom LaBone, who I don't know if we want to listen to the remaining issues from SC&A and then go back to the beginning and respond on those on a point-by-point basis. It is whatever the Work Group would prefer.

But, yes, we can make assumptions about the time of exposure to interpret the data that we have available to us. One can make claimant-favorable assumptions about the time of exposure, about the time in between the exposure and the lung count. One can also use, instead of using just one lung count for that individual, you can use two lung counts. So, that will give you additional data to allow you to understand the lung burden of thorium and the quantity in the body.

We acknowledge there are lots of uncertainties, and those uncertainties are built into the biokinetic models that we use in dose reconstruction. They are built into the integrated modules for bioassay analysis.

We rely upon the most up-to-date scientific information available concerning the biokinetics of thorium and thorium progeny. So, we are using respiratory tract models and biokinetic models from the ICRP 66 and 68.

Yes, we do agree there are uncertainties. However, we disagree that one cannot bound the potential intakes. We believe that the data are available to us that will allow us to bound in a claimant-favorable manner the potential exposures incurred by workers using the lung counting data.

I don't know if Bob Morris is out there possibly or Liz or Tom, if any of you might have anything to add or to go back and elaborate on what I have just briefly summarized here.

MR. MORRIS: Mark, I think you have said it pretty well in summary.

CHAIRMAN CLAWSON: Who is that?

MR. ROLFES: Bob Morris.

MR. MORRIS: This is Bob Morris.

MEMBER GRIFFON: Come on, Tom.

MR. MORRIS: It's Robert.

MEMBER GRIFFON: No, I am waiting for Tom. I want some meat. Come on.

(Laughter.)

DR. MAURO: This is John.

A quick perspective. When I think

about the decay scheme for thorium, one of the things I would like to say -- and I want to make sure I am thinking right, and this goes to everyone involved in the internal dosimetry -- if I have just inhaled some separated thorium, that would mean, if it has been chemically-separated, it is fresh. All I am going to inhale is thorium-232 and one of its daughters, thorium-228.

And if I did a chest count at that moment, I will not see anything because there are no daughters there yet. Nothing has had a chance to grow in yet.

But, as time goes on, you will start to see the actinium and you will start to see the lead-212. Now I believe if you wait long enough, eventually, the ratio of the actinium to the lead-212, if you wait long enough, will be one.

So, what happens is, what helps me to think about it is, if it is really, really fresh, you won't see anything and you will report zero when, in fact, there is something there. If it is really, really old, you are going to see a ratio of lead-212 to actinium which should be one, unless I am wrong.

And so, in the end, what the question is, if you see anything at some detection level and then you start looking at it, how high, given that spread, because I would like to put a bag around this thing, you know, how wrong could you be if you don't know the age of the material that was inhaled and you don't know the time between when it was inhaled and when the chest count was taken?

And you go ahead with that. You have your measurement. Let's say it is just one measurement. How wrong could you be in predicting the amount of thorium that was inhaled? I mean that is the essence of this question.

If you could be wrong by orders of magnitude, I think we're finished. But if it turns out there is a way to say, well, listen, we could be wrong, but only by a factor of two or three, then, I think we are tractable.

This is how I think about it. I think that when we go forward now, I would like to hear the answer in those terms. That would help me a lot.

MR. MORRIS: This is Bob Morris.

You mentioned the possibility that for one measurement we could be infinitely wrong.

DR. MAURO: Yes.

MR. MORRIS: Okay, I am not going to argue with that. But we produced a coworker model which was based on many, many different measurements that were collected over a period of 20 years and produced into a coworker model. We don't have the extreme case that dominates your scenario as you just described it.

DR. MAURO: Good. I mean that is the first step. So, what you are saying is you --

DR. LIPSZTEIN: May I intervene? Maybe there is the scenario that John Mauro is talking about. We just don't know. Because if it was freshly-separated and the person was measured immediately, you probably would be below the detection limit of the daughters, and you even wouldn't know that there was the same thorium there if you measured one year after or 180 years after. So sometimes you would have below detection limits, but that is only because it was freshly-separated.

MR. MORRIS: I agree you can invent a scenario where it happens occasionally, but not on every --

DR. LIPSZTEIN: I don't know how occasionally it is. I never saw a study done like that. I didn't see anything saying, oh, look, let's see how much we would have below detection limit, how much do we miss because they were below the detection limit but probably there was thorium inhalation. What happened after the person inhaled thorium, about the time that it is in the lung, I didn't see anything until now.

MR. MORRIS: Well, the fact is that the mobile in vivo laboratory showed up onsite one time a year. So it cannot possibly be that everything was instantly freshly inhaled because --

DR. LIPSZTEIN: Yes, you can't, but you don't know. You don't know. There is no data about it. I didn't see any data about it.

DR. MAURO: You know what? Joyce, what I like is this.

DR. GLOVER: Ted, could we -- we have a diatribe right now on the phone.

DR. MAURO: Let's say you have a population of workers.

I'll be quiet right after this little conceptual think piece.

You have got a population of workers, and once a year you measure, look for the actinium and the lead for a whole bunch of workers. Some workers the data is going to come back for that measurement, and at that year you don't see anything. But, then, you are going to measure them a year later, and you will get something. Then, you are going to measure them a year after that, and you are going to get something.

So, in principle, what I am hearing from -- I think it was Bob; I'm not sure who mentioned it -- that what you are really saying is, well, listen, we have all these workers, but we have multiple measurements made year after year after year. And if you have enough of those, a picture emerges of the distributions that the intakes might have been for all of those workers, granted.

But if you don't have these multiple years following a person, a real person, for two-three years, you are going to have a problem. And I am anxious to hear that maybe it does become tractable if you have these annual measurements of both the actinium and the lead for all the workers of interest, assuming that you have got the right population of workers that you are looking at.

So, I think, in principle, I hear what you are saying. I would like to hear a little bit more about how wrong you could be.

MR. MORRIS: This is Bob Morris.

MR. ROLFES: This is Mark Rolfes.

CHAIRMAN CLAWSON: Let Mark speak, Bob.

MR. ROLFES: Yes, this is Mark Rolfes. I just wanted to point out that this would be an issue if you had one exposure and one measurement. That is not the case. The exposures at the Fernald site were pretty routine, pretty chronic exposures, and so were the measurements.

So it is much more difficult to understand how much uncertainty is involved when you only have one exposure and one measurement result. The more measurements and the more routine the exposures, the more you can wrap your hands around this and come up with a good idea of what the overall potential exposure was and what the lung burden, to develop a coworker intake model.

There's a lot of uncertainties, as we said, but it is not an unusual -- we have methods to develop a final uncertainty factor in a coworker model. We can combine uncertainty from the age of the materials. We can combine uncertainty from the time of exposure to the time that the person is counted. There are ways of combining all these uncertainties to come up with a reasonable and worst-case-type scenario that will allow us to bound the doses from thorium to workers.

And that is what we have done in our coworker intake model. We have put together all of these factors, all of these uncertainties, to generate what the worst-case correction factor for thorium exposure could be.

DR. LIPSZTEIN: I am sorry, but I didn't see this on the coworker model. I saw you using lead-212 after `78, and I saw you only using a correction factor, a mean correction factor, or even one for the source, not for the lung. So, I didn't see how you did this. I didn't see you doing this. And I didn't see any proposal to do this until now.

I am certain that with the data in milligrams you cannot do it. I didn't see how you can do it with the raw data from lead-212 to actinium-228, but I would wait to see what you can do with it.

But with the thorium in milligrams, it is impossible. You cannot go back. There is no way to go back with all those uncertainties on what they did to calculate those thorium in milligrams.

But if you are telling me you can do it with the later data, I didn't see it. I don't envision in principle how it can be done. But if you say you can do it, I would like to see it, how you can do it.

MR. ROLFES: Joyce, this is Mark once again.

DR. LIPSZTEIN: I don't see it.

MR. ROLFES: Joyce, this is Mark once again. And I don't have the report here in front of me. However, in our previous report I believe we identified that there was an overlapping year before we changed from solely activity reporting, or excuse me --

DR. LIPSZTEIN: Oh, that data is completely out of any basis. It doesn't match anything. And then, on your White Paper, this last one, you modified all the assumptions again, and it doesn't overlap. And if you have calculated, we have shown this, that those data don't make any sense.

And it didn't take into consideration all those things. So, I didn't see anything that was satisfying.

DR. GLOVER: Ted, I would like to

officially just request that -- Ted?

DR. LIPSZTEIN: -- see this next White Paper modifies all the assumptions.

DR. GLOVER: We certainly want to respond when we are given a chance. We let Joyce finish --

DR. LIPSZTEIN: No, I am finished. I'm sorry.

DR. GLOVER: We all have a level of excitement, but it makes it very hard for him to understand and it is not good communication if we are going to keep talking over the top of each other.

DR. LIPSZTEIN: Okay. Okay. Sorry.

MR. ROLFES: Thanks, Sam.

So to finish what I was saying, if you go back and look at the data, the 1968 through -- I don't recall the specific year; was it `78, Bob, I believe? Anyway, at the time of the early years they were reporting thorium-232 mass in the lung. If you look at the year that they switched over from thorium mass to the activity reporting, they were reporting in the more recent era activity of lead-212 and actinium-228 in nanocuries. The year that they changed over, they reported both mass values and the activity values. That can be used as a calibration factor to understand what the types of materials the person is exposed to.

So there is overlapping data for that one year that we can use to make assumptions, once again, in a claimant-favorable manner, to understand how old the material could have been and what the potential exposures were.

MR. STIVER: This is John Stiver. Could I step in for just a second here?

Bob, you took a look at the data in 1978, and I think you have got what is it, like 100-and-some milligram thorium measurements, and there is just a handful of the -- or was that the other way around?

MR. BARTON: Let me see if I can pull that up.

I think in 1978 there is still mostly milligram thorium. I want to say, just shooting from the hip, maybe 30 percent --

MR. STIVER: About 30 percent --

MR. BARTON: -- yes, had the lead and --

MR. STIVER: Wasn't that also the time where there was a transfer -- actually, the thorium processing was essentially over by `79, and then, at a later date, it really became more of a -- it is during the period of thorium stewardship. And so, you don't have so many different disparate types of sources that could cause these uncertainties in age and that sort of thing.

So I guess the question, then, becomes, in `78, what are we really looking at? Is there still enough processing going on that it is kind of representative of what would be before, so you could make some kind of a reasonable calibration factor that you could back-extrapolate with? Kind of in a constant transition time where you couldn't really --

MR. BARTON: There is a timeline of thorium production that Bob Morris put together. It is very nice. It actually maps out by plant and by year for really the period we are looking at.

And it looks like operations had started to tail off, at least qualitatively, based on the number of plants that were still operating in that later period. So, I don't know if you can really --

MR. STIVER: Is there still something going on? I think it was the Pilot Plant mainly.

MR. BARTON: Yes, you still have operations in Plant 1 and also in the Pilot Plant.

MR. STIVER: Joyce, you said that there are some real problems with that period of overlap.

DR. LIPSZTEIN: Yes.

MR. STIVER: Could you kind of quantify what they are and kind of describe --

DR. LIPSZTEIN: Yes. We have shown this in the previous paper, and we have shown graphs that they don't match.

MEMBER GRIFFON: When you say they don't match, what does that mean?

MR. STIVER: When you make the calibration change, was it .11 nanocuries per milligram?

DR. LIPSZTEIN: Yes.

MR. STIVER: That there is an offset in the data?

DR. LIPSZTEIN: Yes. Yes. You can't really match them.

MR. STIVER: So it is not something you could actually -- there is not a bias. It is just all over the place?

DR. LIPSZTEIN: Yes.

MR. STIVER: It seems like if

there was some kind of a bias, you could make an adjustment for it.

DR. LIPSZTEIN: Yes. I am looking for -- there was a table also showing this. There are some graphs that I already found on page 7 and 8 of our previous paper.

MR. STIVER: This is in the June 2010 paper.

DR. LIPSZTEIN: Yes. And there was a table also showing that they are not compatible. So I don't think we can use those data to --

MR. STIVER: I think the other problem we have with the milligram data, which we haven't really gotten into, you know, you have indicated that you have all these uncertainties. There is an uncertainty with relation to calibration, uncertainty in the age of the source term, uncertainty in the time after exposure, and so forth. And there is also a lot of uncertainty in the 6-milligram value for the MDA.

When you factor these uncertainties into that particular value, you could be off -- I don't know. You assume they are all independent, and you will wind up with something about a factor of four or five maybe. So six times five; you are looking at 30. The highest value we have got is 32.

So now you have a situation where, just based on the uncertainties alone, and applied to the MDA, you have got a situation where conceivably none of your data are even measurable. So you have that.

I think what we were getting at when we were having our earlier discussion is that there is really no way to bound that because there is no anchor point. So, really, the only bound is what could you conceivably physiologically tolerate in a dusty work environment, which kind of draws all the relevance of a coworker model into question during that period of time.

MR. MORRIS: This is Robert

Morris.

MR. KATZ: Go ahead, Robert.

MR. MORRIS: I just wanted to point out that there is a lot of inconsistency here in the SC&A position. If you look at the August 4th paper that we received last week, SC&A staff actually produced a quantitative assessment of the differences between thorium workers and other chemical operators in general, showing that there was a difference in the statistics of the population.

MR. STIVER: Bob, this is John Stiver. We haven't gotten into that yet. But this might be --

MR. MORRIS: -- to light. May I continue to talk, Ted?

MR. KATZ: Yes, go ahead.

MR. MORRIS: Now I don't understand how you can say that the data are not usable when you, yourself, used them.

So, I am done, John. You can talk.

MR. STIVER: Can I step in?

MR. KATZ: Yes.

MR. STIVER: Yes, Bob, this is John.

That analysis was really kind of, we are kind of looking at this issue from two perspectives. And that analysis was really predicated on, okay, let's just set this adequacy issue on the back burner for now, and let's just assume the 6 milligrams is for real and that the data are of a good pedigree.

Now let's take a look at the completeness of the data set. And that is what Bob Barton and Harry did with their statistical analysis. And maybe this would be a good time to talk about what he found.

MR. BARTON: Sure. We can get into that.

The report, as Bob mentioned, was sent out on August 4th. There's a couple of charts in there that might be useful to be able to look at. They kind of describe this.

So, if you all have the report that you can pull it out, that would probably help this thing along. In the meantime, it is probably instructive just to kind of give a quick summary of kind of the history and genesis of this whole issue.

As Mark said, the coworker model came out in 2008. SC&A was finally tasked with reviewing it in 2010. Some of the, I guess, findings from that report that are really going to be germane to the discussion today:

Except for 1968, which was the very first year of in vivo monitoring, evidence suggests -- and these are the findings from the previous report; we expand on them in our newest analysis -- the evidence suggests that the program itself was not focused on the thorium workers. And that's for mainly two reasons.

Whenever you look at these data points, and we are focused on the milligrams for thorium because that is what they were measuring during the production period, which was really when you want to look at the story on production workers to see if they are adequately represented, but when you look at these thorium data points, they are always accompanied by uranium measurements. Even if they were focusing on this thorium operation to see what these workers are doing, you see at least some measurements of just thorium; you see some measurements of just uranium, but you never see a thorium measurement not coupled with uranium.

Also, when you look at the actual buildings and years where these people worked, you see no increase in the number of samples in buildings where thorium was processed. And, you know, part of that might be because the in vivo laboratory was not there all year. So, perhaps these workers had moved to another building when they got measured. That may be one explanation. But it does raise some flags.

In fact, there are some years where we have evidence there was thorium processing going on in a plant and there is not even a single in vivo data point for that. So this begs the question, are thorium workers adequately represented by this milligram thorium data, which pretty much covers the entire production period?

So let's talk about what we actually know about thorium workers, how can we identify them, what chance do we really have here. The first and, in my mind, the most useful piece of evidence is a memo by a gentleman named Bob Starkey. This actually listed out 51 thorium workers at the very end of 1967. We know from interviews that the purpose of this memo was specifically they wanted, when this mobile in vivo laboratory showed up, they wanted those workers to be counted. It turns out a little over half of those workers were actually counted in 1968.

The second resource, because the Starkey memo was useful; it actually names workers and associates them with a specific year. So, in that year, we can be fairly confident we know who it was that was working with thorium.

Now, if we want to look at the rest of the production period, that is when we have to go to our only other resource, the in vivo logbooks themselves. The way these logbooks identify thorium workers is you have a list of all that worker's in vivo measurements. In the top right corner there would be a handwritten note that would say either "thorium worker" or "former thorium worker." Now we really don't have any idea when those labels were applied, what an actual former thorium worker entails. Furthermore, this label isn't exactly identified with any of the actual in vivo measurements. So we don't even know which measurements on that list actually are reflective of thorium work.

It is unlikely, and I know it has been discussed in other Work Groups, that you probably wouldn't have a situation where you had a thorium worker and that is all they did for their entire career. So they probably moved among job categories. So it is probably unlikely that, even with this label attached -- and again, it either said "thorium worker" or "former thorium worker" -- even if it says "thorium worker," we don't really have good evidence to link that worker with a specific operation.

From the logbooks themselves, Starkey identified 51 thorium workers in 1968. The logbooks identified 26 workers, and there is some overlap there. It is actually pretty significant. If you pool both groups together, you end up with about 60 names.

But if you look at these 26 workers who have this label on their in vivo log sheet, almost half of them, none of the samples that were taken were ever associated with a production plant and year. So, again, there is that kind of uncertainty as to what were they actually focusing on with this in vivo program.

Furthermore, of those 26 workers, only about 20 percent or a fifth of the samples were actually associated with plants that handled thorium in specific years. So, again, all these things start to add up, and we are like, all right, we are trying to build a coworker model for thorium.

And eventually, bottom line, you are going to be using this coworker model on thorium workers. So we just want to make sure that whatever you are going to apply as a coworker surrogate goes, that you are not going to seriously underestimate this group.

So what we did is kind of made the overarching assumption with these 26 logbook workers and said, all right, we know it is not realistic, but let's just assume that all of their records during this production period are associated with thorium work. That way, we can try to do some kind of comparison and say, all right, out of these 26 workers, how do their numbers compare with other groups?

And again, this is completely a different approach. We are not making any statements here about the milligrams thorium. In fact, the values that were listed in the 6-milligram thorium we still assumed were a positive result for the purposes of this analysis.

So I just do the very -- I am a simple boy, do a simple rank-order chart, and I compared those 26 thorium workers with the all worker doses during that period. And you see a difference. And, again, this is all in our previous report.

We did the same thing with the workers who were monitored from the Starkey list. This time we only compared them with the 1968 data for all workers. Again, when you rank-ordered them, you see a difference in the two distributions.

So that was kind of a summary of what our concerns were at the start. Now let's fast-forward to this past February. NIOSH released a series of responses in regards to this completeness issue.

We had a rather brief discussion during that February Work Group meeting and, in my mind, a much more productive discussion during April. And the two big things that came out of that:

All right, maybe thorium workers were not targeted specifically for this monitoring. But we know from looking at the job titles -- chemical operators were. So there could likely be a very good chance that you could use the chemical operators as a surrogate for thorium workers if they are, in fact, a bounding from a dose standpoint. So that was kind of the first major point.

And the second one, which kind of adds onto this notion, is that the workers who were selected were selected based on their exposure potential. So, inherently, if you are going after the people with higher lung burdens or higher potential to have a thorium lung burden, you are already going to bias the distribution high, so to speak, very well for the coworker model.

So these two points are essentially the premises that SC&A went to investigate further in our most recent report. So let's take a look at that first one, and here is where it would be a good idea to have that report open so that we can look at some of these figures and such. But, if not, I will do my best to try to describe what is going on.

The first issue was whether chemical operators could be used as a sufficient surrogate for thorium workers. The first thing we did is, again, we looked at those Starkey workers from 1968 because, again, that is our only piece of evidence that actually links names with thorium with the year.

So we take those and now we are going to pull up the chemical operators in 1968 who weren't in the Starkey memo. Then we would have all sorts of double-counting. Again, simple rank-order. Again, you see the differences. Okay. All right, so that's 1968. Kind of treated it differently there from the other years because, again, we have that piece of information from the Starkey memo.

All right, let's see what we can do about the rest of the production period. What we did is we took those Starkey workers, we combined them with the workers who were identified on their logbook sheets to come up with our expanded group of thorium workers. Again, there is about 60 of them. And we are going to assume that they handled thorium during the entire production period.

Again, simple rank-order. And,

again, in this case you are going to have some overlap between the thorium and the chemical operators because some of the chemical operators were also designated as a thorium worker or they are in the Starkey memo, or what have you.

So, again, you do a simple rank-order. Again, you see the differences in the curves. These are what I have just discussed with the 1968 Starkey data and then the expanded groups, look at the rest of the years. These are Figures 1 and 2 in the report. Figure 1 is on page 11, for anyone who has it open, and Figure 2 appears on page 12.

The next thing we did to try to get a grip on this issue whether we can use chemical operators is, all right, let's just compare chemical operators to the rest of the workers during that period. This is Figure 3. It is on page 13. When you look at that figure, I mean the curves overlap quite nicely. What that would suggest to us is that chemical operators maybe aren't a bounding worker subgroup, but are actually more reflective of the overall worker exposure potential.

All right, so we do these simple rank-orders. It is barebones stuff. We still have some of these questions and things popping up which is suspect.

So we brought in our statistician, Harry Chmelynski, to say, all right, let's do a little more robust analytical approach and see what that tells us about it.

Can I ask, Harry, are you on the line?

(No response.)

Okay. That's okay. I can summarize basically what we did.

Again, we were looking at this group of 60. Basically, Harry went through and he performed a pretty robust statistical analysis on an annual basis where we can actually look at each year.

And to summarize, when you have a calculated mean by year in 8 of the 11 years we looked at, again, the thorium worker subgroup was higher than the chemical operators. And when you look at the calculated 95th percentile, it is higher for thorium workers in 6 of the 11 years. One of those 11 years they are actually dead-on tied.

To look at it another way, at the mean level chemical operators were only bounding in 3 of the 11 years, and in only 4 of the 11 when looking at the 95th percentile.

But one other pretty interesting facet about this is Harry went in and calculated the 95th percent confidence interval for the 95th percentile value, if you can swallow all that.

(Laughter.)

And basically, what we found out there is, when you look at the confidence intervals, maybe not the specific calculated numbers but the confidence intervals, there is almost no statistical difference by year at that 95th percentile confidence interval, except for, I believe, it was one year, 1972 --

MR. STIVER: `71, I think.

MR. BARTON: -- `71 or `72, right around there.

So when you look at those top-tier numbers, you really can't see a difference between not only thorium and chemical operators, but even the all worker group as a whole. So at the highest numbers that we see, there doesn't appear to be a group that is far away above the other. So that was an important thing.

The last thing we did for this whole chemical operator versus thorium issue is let's just take a look at the records that are specifically above -- at or above that 60-milligram number. So, in other words, based on the assumption that 60 milligrams is our lower limit of detection, what does it look like when we only consider the positive doses?

Just to give you some idea, from 1968 to 1978, there were over 2600 measurements reported in milligrams thorium. Less than 3 percent, so about 76 measurements, covering 57 workers, were at or above 60 milligrams. So, only 3 percent, less than 3 percent of our data is actually positive, if we accept the 60 milligrams as the correct lower limit.

Now, when you look at this group of positive measurements, thorium workers actually become a lot more significantly-represented. For example, we have 76 measurements; about a third of them were associated with that group of people that we call thorium workers because there is indication they worked with thorium at some point. So 33 percent of them are for thorium workers.

Now, if you look at all the data,

including all the ones below, you know, negative, thorium workers only comprise about 13 percent of the total measurements. So when you look at that positive data, again, this is another piece of evidence that says you have a group here who probably had a higher exposure potential, but evidence suggests they weren't concentrated on. And so, I guess, what do you do with that?

I mean, eventually, like I said, the bottom line, you are going to be using this coworker model to assign thorium intakes to thorium workers who weren't monitored, and evidence suggests that they had a higher exposure potential, which is fairly intuitive, I would think.

So that was that first issue of using chemical operators as a surrogate. The analysis that we did suggests that the actual numbers for chemical operators are closer more to the all worker average than this subgroup of thorium workers.

The other issue -- and this one will go significantly more quickly -- was the workers with higher exposures were counted more frequently. We tried to get a handle around this. We did, again, another very simple thing. We are just going to go ahead, plot the number of times a worker was sampled against the relative magnitude of those numbers. And we did this for the average, the median, and the maximum values that we saw, and let's see if there is a positive linear correlation. That is, those workers who had more sampling done for them, did they have higher numbers than the people who only had a few?

Just a quick note on that one. Again, all positive values were assumed to represent a real counting result. We didn't adjust for any sort of MDA or anything. And we also took all the negative values and made them zero because, you know, if you have an extreme negative number, you don't want that throwing off or negatively biasing anything.

Okay. So, basically, we did that. We compared the number of times a worker was monitored versus the relative magnitude for the milligrams thorium and, also, for the two types of uranium monitoring that was done during that period. One of them is labeled just as U. We are not sure what that actually represents. And the other one is labeled as U-235.

For those of you following along in the report, the summary of this second analysis is found on page 26 and kind of summarized in Table 15. And you can also look at the actual plotted median values in Figures 6 through 8, just past that.

It turns out the monitoring for thorium in milligrams thorium actually showed a slight negative bias when you compared the frequency of monitoring to the workers' median and average results. So that actually says that, you know, not only does it not look like the thorium or the workers who had higher thorium results were counted more frequently, and the correlations are not great, but it almost looks like there's a negative bias.

Now we move on to the uranium results and do the same thing, and the uranium all showed a positive correlation. The highest ones were actually for U-235.

So this type of thing may suggest that, while the in vivo program might have been focused on workers with higher exposure potential, it was that exposure potential to uranium, and not thorium, that drove the actual monitoring practices.

The fact that it's U-235, maybe they wanted to take an extra look at enriched uranium. I really don't know, but just from that we didn't find evidence that those workers with high thorium results were targeted more frequently.

So that pretty much wraps up the new material that we had. Are there any questions?

MR. ROLFES: Thank you, Bob. This is Mark with NIOSH.

We received this. I know we tried to prepare everything so that we had it available for discussion at this meeting today. We did our best to get a response out for recycled uranium. We haven't prepared a written response to this -- two new White Papers that we just received on August 5th here.

We do have some tentative analyses. I don't know if Tom LaBone has been able to join us. I know that he had prepared an analysis comparing basically the lung count results between thorium workers and non-thorium workers.

Before we get into any response, though, is it okay if we might take a break?

CHAIRMAN CLAWSON: No.

(Laughter.)

That will be fine.

MR. KATZ: So we will start back up at 2:45? Is that enough of a break? Fifteen minutes?

Thanks, folks on the line.

(Whereupon, the above-entitled matter went off the record at 2:28 p.m. and went back on the record at 2:45 p.m.)

MR. KATZ: Okay. So, everybody is comfortable now. We are done with our comfort break.

And let me just check to see on the line. Do we have you, Dr. Ziemer? Phil?

MEMBER SCHOFIELD: Yes, I am on the line, Ted.

MR. KATZ: Thanks.

CHAIRMAN CLAWSON: Okay. We have had some very rousing discussions here. But Sam brought up a very good issue, and that is I want everybody to keep in mind that this is coming before the Board in a couple of weeks here. I think that both sides need to sit down and really discuss the issues, where we have got a problem and where NIOSH or SC&A, where we feel that we have got the issue that we can or can't do dose reconstruction on.

So, I am going to turn it over to Mark.

He -- we wanted to kind of go over this thorium issue a little bit more in detail and then have SC&A respond.

One thing I would like to say, and especially to the people on the phones, allow the person to be able to finish their conversation. I know that we want to keep track of it and everything else like that, but just so the court reporter can document this.

Just have respect for the other people the way you would want them to respect you on the phone and here, too. Because we have a tendency to speak over one another that makes it very difficult for each side to be able to understand it.

So, Mark, did you want to start out then?

MR. ROLFES: Yes. Thank you, Brad.

I think we have a couple of key players that had developed the original thorium coworker intake model back in 2008. I believe we have those individuals on the phone.

I would like to possibly have them go back to that original model and tell us some of the assumptions specific to the Fernald coworker intake model that we have built. How have we addressed the uncertainties associated with the age of the material, and any other uncertainties and interpretations of the data?

And I don't know who would like to take the lead on that. I know we have both Bob, Liz, and Tom on the phone.

I guess, Bob, if you would like to start off or decide if it is appropriate for you to respond or Tom. I will let you take the reins, please.

MR. MORRIS: Okay. Robert Morris here.

The coworker model inherently has uncertainties built in when it applies a GSD value, which was calculated for this effort, and the minimum GSD that was assumed is 3. That is a pretty wide spread of data, actually, when you put it into there.

MR. KATZ: I'm sorry, Bob. Bob, you're sort of fuzzy, your voice. Are you on a speaker phone?

MR. MORRIS: Is that better?

MR. KATZ: No. Actually, your phone has a lot of fuzz to it or static or something.

MR. MORRIS: Okay. Then, maybe I should just let Tom pick it up.

MR. KATZ: Well, no one else was that unclear. I mean, I had the volume cranked because of the fuzziness.

Tom LaBone, do you want to try to do this instead of Bob?

MR. LaBONE: Well, I was more prepared to talk about the SC&A paper, the one that we just discussed. So, I don't know how we want to work that.

MR. KATZ: Okay. Well, I mean, Bob, we will just deal with the static. If you could just try to speak loudly and clearly, it will be okay. Bob?

MR. MORRIS: Okay. Is this any better at all?

MR. KATZ: Yes. Right now, we can hear you. Yes, thanks.

MR. MORRIS: Okay. The coworker modeling has inherent in it uncertainties built in to accommodate the biokinetic modeling and the other common factors. And it is constrained to have a GSD of 3 or higher.

We assumed a midpoint of the theoretical equilibrium curve between -- disequilibrium curve between the daughters of thorium, and put that at .71 in our original model.

In the most recent paper that we did in May of 2011, we revisited that, at the urging of SC&A. We have reset that, so that it is at the theoretical minimum for a chemical separation. That is .42, if I recall the number correctly.

Now that has not been put into the modeling yet, but it is one of those factors that we agree could be more conservative. So, we have agreed to put it to that theoretical minimum for a single chemical extraction.

I think that we will also put in the factors in the coworker model, if it is ever revised, that will address the MDA issues. Because we had to go through and make an adjustment at SC&A's correct observation that there were too many negative values. And so, we have made a single-point adjustment, a bias adjustment. That will be recorded in the next iteration of the model.

And I think that is it.

Calibrations certainly will take

into account the factor of three that Joyce has identified with the phantom. But other than that, I think those will be the changes that will be reflected in the next iteration of the coworker model.

CHAIRMAN CLAWSON: Bob, this is Brad Clawson speaking.

I am kind of at a loss here a little bit. So, you first made the comment that you had not put this factor of 11, or something like that, into it?

MR. MORRIS: I didn't say a factor of 11, Brad.

CHAIRMAN CLAWSON: What was it?

MR. MORRIS: It used to be .4 -- it used to be .71, and we are going to move it to .42 as the disequilibrium value between thorium daughters.

MEMBER GRIFFON: Yes, that was a result of SC&A's comment.

CHAIRMAN CLAWSON: Right.

MR. STIVER: Can I say something?

CHAIRMAN CLAWSON: Yes, go ahead.

MR. STIVER: Bob, this is John Stiver.

You mentioned you are going to apply a geometric standard deviation of 3. Is this, then, considered to roll up all of the uncertainties associated with age of the source, uncertainties in the calculated lead and actinium values and that, and so forth? Basically, everything except the factor of three for the phantom that we brought up today?

MR. MORRIS: The GSDs of three or higher are based on the actual modeling from the lung --

MR. STIVER: Oh, it is based on the actual? Okay.

MR. MORRIS: And so, we will be going back in and making the adjustments to the data that will then get remodeled into a coworker model. I don't think the magnitude of the GSDs will be a bias factor from where it is today.

MR. STIVER: Right. That bias is applied to that lead-210 data that was low, I believe?

MR. MORRIS: Yes.

MR. STIVER: And you guys have acknowledged that in the previous paper.

MR. MORRIS: Right.

MR. STIVER: I didn't see anything in your paper about an acknowledgment about what to do about the high MDA, the fact that there is only 3 percent of the data above, given that that MDA is even correct to begin with.

MR. MORRIS: The MDA really doesn't play a part in coworker modeling, since we don't censor the data.

MR. ROLFES: Correct. That is correct, Bob.

MR. MORRIS: And so, whether or not the MDA is right, we still take the dataset at face value, and the only ones we adjust for are the ones that are zeroes or below zero.

MR. ROLFES: Right.

MR. STIVER: I guess that was one of the problems we had. That was one of our findings. I guess it is still not resolved then. And so, I think we still have something to talk about regarding the adequacy of the milligram data, as Joyce had described.

MR. MORRIS: As I see it, as long as we have got real-number values to put into the data-fitting algorithm, the MDA doesn't matter. We don't have a different treatment for the number, whether it is above or below the MDA.

MR. STIVER: I think it would matter from a practical standpoint, whether that was a believable number or not or it was just a noise term that is inherent in the counting system.

MR. MORRIS: Well, it is; yes, I agree at some point we are going to be modeling noise, as you get close to zero. But it doesn't matter where that point is as long as we have some adjustment available for it, and it defines the shape of the curve.

MR. STIVER: I know in previous discussions Mark had mentioned that, if you are below the MDA, you would just use a missed dose calculation. But that wouldn't seem to apply in a coworker-model-type situation, though.

MR. MORRIS: I agree. For modeling the coworker population, you only make that correction if you are at zeroes or below zeroes. Otherwise, you use data at face value without regard to that. For an individual dose calculation, then you have to essentially that missed dose concept and apply it.

MR. ROLFES: Yes, I guess I wanted to clarify. This is Mark Rolfes.

We would only be assigning this coworker intake model to the individual who was not monitored --

MR. MORRIS: Right.

MR. ROLFES: -- using in vivo technology. So, if we had an individual that had mobile in vivo data in their file, we would use their data. So, we are only talking about a small fraction of the population, possibly, who never had a thorium lung count done.

DR. MAURO: This is John Mauro.

Another question along those lines. So, when we are talking about the milligram or microgram, I guess it was milligram, of data from, I guess it was `68 to `78, you have a population of numbers. If you have a measurement for a real person or a number of measurements in units of milligram, you go ahead and use those numbers. If you don't, you use the coworker model if a person wasn't monitored or you don't have data for him.

You take the population of numbers. Let's say you have got 100 positive readings that are clearly and unambiguously above the MDA, if I understood it. What do you use? So, you have those numbers which are basically chest count data, representing a body burden in milligrams. And now you want to assign that, some number, to a person. Do you take the geometric mean and the standard deviation of 3 or do you take the upper 95th percentile? How do you apply that coworker model?

MR. ROLFES: John, this is Mark Rolfes.

I would have to take a look back. It has been three years since we produced the original model. I don't know if Bob maybe -- you know, the ability to do one or the other exists. It is a matter of choice, based upon, essentially, exposure potential. I think we have previously sort of discussed exposure potentials.

You know, if it is a chemical operator, I would say the 95th percentile would be applied to make sure that we are bounding the individual's unmonitored or potentially unmonitored thorium intakes. If it is someone who could have had occasional exposures but didn't appear to be one of the highest-exposed, you know, the application of the 50th percentile would be more likely.

However, it appears to us, based upon our review, that people with the highest potential for exposure were the ones that were counted most frequently.

DR. MAURO: Okay. Now I understand.

Now Joyce's commentary on the validity, whether or not you could actually use those milligram numbers, as you know from the presentation that Joyce made, whatever the number is, let's say it is 12 milligrams is recorded for a person, or a number of measurements are there for a person. But what I understood from Joyce's discussion is: you really can't trust that number, and you don't really know what it is. It could be substantially higher. From my discussions with Joyce, and Joyce is still on the line, we really couldn't say how much higher because we really don't know how that number was obtained. I think this goes to the heart of, you know, can you reconstruct doses with sufficient accuracy?

Do you agree that these problems that Joyce has raised are real and do represent an obstacle that is going to be difficult to deal with?

MR. ROLFES: John, this is Mark Rolfes.

I do agree that these uncertainties are real. However, they are just uncertainties, and we have methods to deal with the uncertainties to ensure that our dose that we are assigning to unmonitored thorium workers or people who were potentially exposed to thorium, we believe that we can bound those doses. We believe that we can address those uncertainties to ensure that we are using a reasonable -- that based upon a reasonable exposure scenario, we believe that we can bound those doses using this coworker model, if we account for all the uncertainties appropriately.

MEMBER GRIFFON: Perhaps this is the time to have Tom LaBone weigh in on --

DR. LIPSZTEIN: May I step in?

MEMBER GRIFFON: -- addressing some of SC&A's comments. But after Joyce, I guess.

Go ahead. Go ahead, Joyce.

DR. LIPSZTEIN: Yes, I think that the heart of the question is: can we test these numbers in milligrams? And I put out in our paper two different examples that were not made by me, but was in a paper.

One is from NLO, 1966, where it compares monitoring results for a worker that was monitored at Y-12 in this same mobile in vivo counter, and at the Wright-Patterson Air

Force.

And using different techniques and different calibration, they came to three different values for the same individual: 7 milligrams using Y-12 calibration and Wright-Patterson data; 3 milligrams using Y-12 routine technique and Wright-Patterson data, and 1 milligram using Y-12 routine technique and Y-12 data. But we have a difference from 1 to 7, just the same person that was monitored in two different places and using different calibration techniques.

Then, we have again the example that was put on the O: drive also. And we have the example of running, you know, showing that the same numbers could either mean 56 milligrams or 27 milligrams of thorium.

So, we have a huge difference, and I am very uncertain of the meaning of the milligrams monitoring results for thorium. And I am not saying this for use in the coworker model. I mean for use on our worker that has a result in milligrams of thorium.

MR. ROLFES: Okay, thank you, Joyce. This is Mark Rolfes.

A couple of things I want to ask about that. Are the detectors -- the types of lung counters that they are using, are they the same or different types of lung counters? Are they sodium iodide? And also, the dates of the analyses, was there a significant space in between the measurements? Were all the measurements done on the same day, within the same week, within the same year, or different years?

DR. LIPSZTEIN: You mean the Wright-Patterson data?

MR. ROLFES: Correct.

DR. LIPSZTEIN: You know, the person was measured at the Wright-Patterson Air Force using their counter, and the same person was monitored at Y-12. And so, they were discussing why there was a difference. This paper relates -- it is actually a conversation, trying to find out why there was a different between the results from the Y-12 to the Wright-Patterson.

And they come to the conclusion that it's everything. It is the technique that is used and the calibration also and the detector also. But it is a huge difference.

So, I think that, when you analyze, I am thinking more about the milligrams.

MR. ROLFES: Joyce, this is Mark --

DR. LIPSZTEIN: You don't know how they were measured and what they measured and what do they mean. And, then, there is this paper, these counts, running counts, that was put on the O: drive also, where the person that was doing the counts, he said -- he is saying, look, how difficult it is because if you assume that 1.08 is equal to 1, which really doesn't matter too much, you come out with a difference from 56 milligrams to 27 milligrams.

So, you know, the data in milligrams has too many uncertainties. You don't know they really mean. That is the problem.

MR. ROLFES: This is Mark.

Before I ask Tom to respond, I would like to have Tom LaBone maybe give us some additional insight.

But some of the things that we have to consider in having two different locations, we have a military facility doing a measurement versus a DOE facility doing a measurement. You have to know some information about the material, what you are looking for. You do need to know some information about the ages.

In order to compare two different facilities' detectors, we would have to take a look at the types of detectors. They could be two different types of lung-counting systems. They could be two different types of crystals that are used, for example. There also could be different geometries in those counts. You know, they could have the detectors closer to the individual's lungs in one case, then farther away in the other.

And chest thickness calibration data, you can't just say one count at one site is higher and one site has a lower value without knowing all the details of how the counts were performed and the information that went into the final value.

So, if I could have Tom maybe provide any kind of additional insight on this issue possibly, if he has some information to provide?

MR. LaBONE: The question is about the calibration of the chest counters for thorium? The validity of that?

MR. ROLFES: Well, I think I addressed some possible explanations for the differences in two measurements, in addition to possibly a separation of the time that the measurements were made.

MR. LaBONE: Okay. I'm sorry, I have not seen the calibration records for the chest counter. But what I am assuming they would have done, which is how you calibrate a chest counter, is that you have some thorium, and they would have had it analyzed. They would know how much thorium and how much of the daughters.

They would put it inside their phantom. I don't think the phantom is absolutely critical here because of the energies you are looking at; they are fairly high. They are not going to have a lot of self-absorption.

Then, they say, okay, we see this number of photons in an hour registered in the detector in this peak area. And so, they get milligrams per count in the detector.

So, I don't question the validity of the calibration. Now the issue that is being brought up is, how does that relate to what the people were exposed to in the workplace? And that is a valid issue that you have to know something about what was the material in order to make this translation of you are counting the person with workplace material in him versus the phantom with a known mixture of thorium in him.

And so, I don't have an issue with the calibration in those days. But, again, the problem is, how do you apply that to the workers?

I don't know if that answers that question, but that is my opinion on it.

MR. ROLFES: Thank you, Tom.

Okay. Then, the one other thing I did want to call on you, Tom, for was in response to Bob Barton's -- I know we heard Bob Barton present his analysis of the possible differences between the thorium worker dataset and the remainder of the chemical operators.

I know you produced a quick plot.

Could you possibly describe what you have done and explain the analysis that you completed?

MR. LaBONE: Okay. What I would like to do is in the report there is a nice summary of findings, Section 2, that goes through one, two, and three of the findings. So, what you have asked me to do is part of Finding One.

And so, when I read the report -- this is the August 4th report that was discussed earlier -- you come away with the impression after reading the summary that the curves in Figure 1 between the thorium workers and the chemical operators are different. So, that is basically the conclusion. Well, sometimes it is not explicitly stated, but that assumption is carried on through the rest of the paper.

But the thing that is missing here is that there is no test of these two curves. All there is is just basically you are looking at it and saying, hey, they look kind of different.

And what I did was -- I don't have the data. So, I digitized the plots to get the data off as best I could. And I ran the test on it.

The conclusion I came up with is what Mark has, that little piece of paper. It is that these curves are not significantly different. That is, the thorium workers and the chemical operators.

Now this is a test that I think should be done and included in the paper because there can be errors in me trying to digitize the data, and so forth. But, again, that is the type of thing that I think needs to be here.

If you take away the fact, if you go ahead and say, okay, these things are not significantly different, then it changes basically the tone of the whole rest of the paper.

The next assumption that is made is basically the thorium workers who were identified in 1968 existed as a group for about 10 years. And so, I mean, the question I throw out to everybody is -- because I don't really know the answer to this -- is it plausible that that group stayed together as thorium workers throughout that time period? Because what I was hearing was that these were chemical workers and they were assigned various jobs, depending upon what was in production at the time.

And so, those are two issues I have with Finding One. It is basically, did this group stay together through 10 years? And are they different in 1968, which was the one year that there was definitive data to identify a thorium worker versus a non-thorium worker?

So, I think that is probably a good place to stop for one. Does anybody have any comments on that?

MR. BARTON: Tom, this is Bob Barton.

MR. LaBONE: Yes?

MR. BARTON: Just so I understand exactly what you are asking, are you asking if that group of thorium workers that we identified in these plots worked with thorium during the entire period in question?

MR. LaBONE: Yes.

MR. BARTON: Okay. I don't have an exact reference. But my impression, based on Work Group discussions on the site, is that, no, that was not the case.

So, you had overlap when looking at these different lung values that would probably reflect uranium work, even though they were only still labeled as a thorium worker. Aside from 1968, again, the evidence is very flimsy as to who we can identify as a thorium worker and to actually tie their specific in vivo results to thorium work.

If that helps clarify a little

bit.

MR. LaBONE: Yes. It is just that that assumption, I think, from what I read, is made throughout the paper, when the comparisons are made in subsequent years. And so, like everything depends upon that. If that is not the case, or if you cannot assume that as a first-order approximation, then a lot of the comparisons are tough to interpret.

MR. BARTON: Again, this is Bob Barton.

I clearly understand what you are saying. For us to do anything like the analysis that we present here, we sort of have to make that assumption because, otherwise, we have no way to tie any of these workers as a thorium worker aside, again, from 1968.

So, in my mind, when you make that assumption that they always worked with thorium, and then you compare them to chemical operators, you are really kind of almost muddying the water because a lot of those lung counts aren't going to actually be representative of work being done with thorium.

So, if anything, that I think would bias the curves to be closer together than they might have been, had we known who actually worked with thorium on a yearly basis.

MR. LaBONE: I don't know. The problem that I see is you are starting off with an assumption and it is almost circular. You could have just gotten lucky and gotten a stratum that happened to be higher. Again, if you go through and you test these things, and they all turn out to be higher, you can pick another stratum of workers that are completely at random and they may be higher all those years, too.

You need to nail down that these are indeed thorium workers, I can identify these, and these other workers are not thorium workers. I mean, you have to do that first, and then the rest of this flows from that.

MR. BARTON: I really wish I could, Tom. We have not found any references or evidence aside from 1968 that allows us to identify who worked with thorium in those later years.

MR. LaBONE: Okay. I mean, I talked to Bob about this. What I was told -- and again, he can chime in here, if I am misquoting him -- is that there was reasonable evidence to conclude that the workers were basically not assigned as thorium workers per se, but they were chemical operators and then they took assignments that varied, depending upon, I guess, who was up for overtime that week and what needed to be done.

And again, he can chime in here that there was supposedly some sort of documentation from the site that supported that sort of scenario.

MR. MORRIS: This is Robert Morris.

In fact, that is true. If you look into the interviews that we did in the 2007 timeframe with the plant managers and the process engineers, you will see some evidence of that.

CHAIRMAN CLAWSON: This is Brad.

What evidence? Because my understanding is we can't tell who is thorium workers and really who the chemical --

MR. MORRIS: Well, that's right. The plant managers said the assignments were made based on who was available and the campaigns that were scheduled, and it was not a purposeful -- and those assignments came out of the chemical operator population. So, the people were assigned into these jobs based on their availability and based on the campaigns that were going on.

CHAIRMAN CLAWSON: Okay. So, I just want to be clear. So, we have no idea who was and who wasn't, period?

MR. MORRIS: Well, I agree, we

don't know who was and who was not, but we know that they were chemical operators.

CHAIRMAN CLAWSON: Okay.

MR. LaBONE: For the purpose of the coworker model, I think the important thing is that if you counted, if you chest-counted chemical workers, you captured the people who worked at the thorium processes. So, if we can make that assumption, then we can move forward to do something, ignoring everything else that went on in the discussion today.

But there would be people in there who didn't handle thorium, but, again, that can be handled as part of the process. But did you catch everybody? Did you capture everybody, count them, the ones that handled thorium? And so, again, that is just something that we need to decide and we support that.

Okay. Any other comments on One?

MR. BARTON: Just as a note, being

a thorium worker does not automatically mean you were a chemical operator. There were also machine tool operators. In Starkey's list, there's -- I mean, I will agree that they are probably mostly chemical operators, but you also have some other job titles in there. And even just looking at the Starkey memo, which is the only definitive piece that ties it to a year, you are going to see some other job titles in there as well.

MR. LaBONE: Okay. So, I think what we need to do is, again, how many of those and, again, do we capture the preponderance of these? And is it adequate for the thorium? I can't answer that right now, but just looking at the analysis here, that is what went through my head.

MR. BARTON: And, Tom, this is Bob Barton.

I completely agree. If we could have tied down who the actual thorium workers were, this analysis would be a whole lot stronger than it is. But, as I said, with the difficulty we had identifying those people, we felt that this, the assumption that we made was essentially the best that we could do to try to get a handle on what we have here.

MR. LaBONE: Yes. What it comes down to is, it is very hard to look at just count data and decide, did you count the right people? You need ancillary information such as: how was the bioassay program designed? What were the procedures, and so forth?

It is real hard to just look at the data and say, did we count the people we should have counted? Those are the difficulties. I understand it is hard to do this. And sometimes I wish we could do this because it would help us a lot.

CHAIRMAN CLAWSON: It would help us all. But this is part of our issue that we are into, is the data, just a lot of it isn't there. That what creates a lot of our problems.

MR. LaBONE: Okay, the --

MR. MORRIS: But in this case --

MR. LaBONE: I'm sorry, go ahead.

MR. MORRIS: But in this case, I think we do have data. We have got a program, we have got bioassay programmatic description documents, and it said we get chemical operators and these sorts of people. It had a list of them on an annualized basis, when the whole body count or when the mobile in vivo lab shows up. Other people are on a less frequent basis, more or less, every two years, from what we can see.

But the assumption going farther, I think Tom is going to cover this in a moment, that there would be a correlation between the exposure and the number of times the person was counted, it doesn't make that much sense when the counter is only showing up one time a year for a routine, programmatic assessment.

If you identified somebody who was

involved in an incident and then counted them more than once a year, that would be understandable. But, for the routine program, why one would even think to test that exposure potential and number of counts is correlated, except to say that it happened one time a year when the machine was available? It sort of made me wonder why the test was going on at all.

MR. BARTON: Well, this is Bob Barton.

As you just said, Bob, everybody wasn't counted. It was a tiered approach, the chemical operators listed as the highest priority. But even they weren't all monitored year by year.

So, if you had somebody in one year with a high thorium exposure, and you were going after the people with the high values, chances are you probably would see that person counted again at the very least next year. And there are instances where you see someone counted more than once in a year, and not just the next day, sometimes four months apart.

So, while some of this analysis -- I will absolutely concede the limitations need to be brought out, because it is a very important issue, but I think there is still some value to this as a weight of evidence argument.

MR. LaBONE: Okay. Should we move on?

MEMBER GRIFFON: Sure.

MR. LaBONE: I would like to talk about No. 3. Again, this is on page 9 and starts at the bottom.

Basically, if I were to summarize this, the conclusions were: there is a poor correlation between the number of thorium chest counts and the measured -- between, yes, the number of thorium chest counts and -- I am trying to read this off the paper, so I get it straight -- and measured thorium chest burdens.

It was referred to as a frequency in the paper, but I think it was over the 10-year time. It was the total number of counts in the time period, the 10 years. Is that correct?

MR. BARTON: That's correct.

MR. LaBONE: Okay. So, I just called it a number instead of a frequency. Well, I guess you could say it is the number for 10 years.

Now, that was a conclusion. Then, the next one was there is a better correlation between the number of uranium chest counts in that 10-year time period and the measured uranium chest burdens. And so, okay, we will talk about that in a second.

But just assuming that those are true, the conclusion was that, from those two statements, is that the monitoring programs, basically, who you decided to monitor, were based on the uranium work you were going to do and not the thorium work. I guess that is one conclusion you could draw.

Another, I think perhaps more plausible conclusion, based upon the health physics of this, is that, basically, there were more positive uranium chest counts than there were positive thorium chest counts. And that, typically, a program will recount people when they are positive.

For example, there is a table, Table 1 in an SC&A document here. It was June 2010. It was a review of thorium in vivo, coworker study, proposed attachment. Basically, SC&A reviewed the addendum that was going to go on the Technical Basis Document. And I don't expect everybody to have that there, but I just wanted to state the source.

There is Table 1 in here, and there is one, two, three, four, five, six, seven, there is eight of the highest thorium results are listed in here. It was to demonstrate the problems of trying to interpret these thorium results. But all eight of those thorium chest counts were from the same person and split equally in June and October of 1979.

So, basically, in a health physics program, when you see something, a chest count that is positive, you typically will recount it if it looks unusual or whatever. And so, if you are going to try to do something like this, you have to try to account for how many of these were recounts that were triggered by an event where they determined it to be a positive result. And I think, if you look at the uranium chest counts and the thorium chest counts, there were a lot more positive uranium chest counts, which would be consistent with them recounting these people.

So, again, I think you can argue which one of these is the proper interpretation of it, but I don't think that it proves the monitoring program was based upon uranium work as opposed to thorium work.

MR. BARTON: Yes, Tom, Bob Barton again.

I completely follow what you are saying. Again, it is important to point out the limitations here with what we are able to do and how strong a conclusion we can actually pull from something like this.

You know, maybe a more in-depth analysis goes to look at more of the people that you just pointed out, like this person in 1979. An even better way, if we could do it, would be to take a look at -- I mean, one thing that could be messing this up is if the person wasn't even employed there anymore. Then, you wouldn't see repeated measurements at Fernald. So, I mean, these are all factors that certainly have to be taken into account.

What we tried to accomplish here was some statements were made that things are relatively okay because you are going to concentrate on the people with the highest exposure potential. And even just a scoping calculation like the one we are referring to here, where we are doing the linear correlations, while limited, it gives you some weight of evidence to try to figure out if that statement is actually correct.

As you said, you could draw different conclusions from it. The one that we saw is like, you know, as a scoping calculation, we see that there is actually, based on the calculation, a negative correlation for thorium; whereas you have much better correlations for uranium. And that could be explained by more positive results or it could be because the program really wasn't focused on capturing the highest thorium results, but was more focused on uranium.

Two pieces of evidence that, again, might go to support that is: one, the samples we looked at are for buildings in years where thorium was processed. And two, you always have a uranium measurement with thorium. It could be argued that that is incidental.

So, while I absolutely appreciate your point, and it is important to point out exactly how much of a conclusion we can draw from this, again, I still think it has some value for us to try to get a handle on it because it is a very complex and very difficult problem.

MR. LaBONE: Right. I agree with you. Data, you know, basically, exploration is good. It is just, again, it is important to point out to the people who will read this report that this is a correlation, which means these things you are seeing them trend together, it is not causation. So, causation, I mean, basically, you have to get some additional information. Again, it doesn't prove what is causing this. You can say, hey, that they are trending together, and this is one possible conclusion.

But, again, I think that needs to be clear because of basically who is going to read it, that they don't walk away that, hey, this proves that this was true.

MEMBER GRIFFON: Hey, Tom?

MR. LaBONE: Yes?

MEMBER GRIFFON: This is Mark Griffon.

I was curious if you -- I don't know if this fell under your task -- but did you have any comments on Finding Two?

MR. LaBONE: Finding Two, what I saw it say is that, hey, the high thorium results, it kind of agreed. Is that what the conclusion was? If we just took the thorium results that we thought were truly above any sort of sensing or detection level, and we looked at it, that they tended to agree between the chemical workers and the thorium workers. Basically, there was no difference.

MR. STIVER: Yes, that is kind of one aspect. This is John Stiver.

That was one aspect of it. That as you got to the higher end of the distribution, there seems to be less of a differentiation among the different subgroups.

But the other issue is the fact that there were only about 75 data points that were above the presumed MDA of 6 milligrams, and whether that calls the utility of this coworker model into any question.

MR. LaBONE: Yes, again, I think this is valuable information, but it has to be combined with something outside of the count data, again, interviews from people who were running the program. How did you select people? What processes were being done, and things that I don't have to access to right this second.

But, again, I think it is valuable to do. It is just it needs to be combined with some outside information in order to draw stronger conclusions.

MR. STIVER: Okay.

MR. MORRIS: Ted, Robert Morris.

MR. KATZ: Yes, Robert?

MR. MORRIS: Okay, I would like to ask a question about in this August 4th, 2011 paper to Bob.

Could you refer to Figure 8, please? Ready?

MR. BARTON: Yes, Figure 8.

MR. MORRIS: Okay. This is, I think, the figure where you have defined that there is even a negative correlation between the number of samples per worker and the median thorium results.

MR. BARTON: Yes.

MR. MORRIS: If you would look at the fit of your line for that data, do you think that you got it right?

(Laughter.)

MR. BARTON: Again, I mean this is a scoping calculation. Are there more exact ways to do this? I'm sure there are. Again, we are just trying to get ahold of this thing and figure out just what we have here as far as thorium monitoring coverage. And this was just one step in the process.

MR. STIVER: This is John Stiver.

You can see the correlation coefficient is very low. It shows, if there is any correlation at all, it may be negative, but it doesn't really appear to be anything. It is hard to tell from that. But this wasn't designed to be a quantitative assessment. It is really just sort of a scope of, did there appear to be any kind of a trend that would indicate an increase in frequency with magnitude of result?

MR. MORRIS: I agree with you; it doesn't indicate anything, but it is one of the things that you highlighted as a negative correlation.

And I don't think your line supports the data, really. I don't think it accurately represents it and proves that there is a negative correlation.

MR. BARTON: Bob, I think that really -- and perhaps this was not stated clearly enough in here -- we did not find any evidence to suggest that the thorium monitoring was directed at the higher-exposed individuals. I think that is really what we are trying to get across here.

We did some scoping calculations to see, all right, again, we are going to look at the trend. What does the trend tell us? Does the trend tend to support this or does it not support this?

And, based on what we saw from thorium, I mean, again, the correlations are low. So, the actual values of the numbers certainly have to be taken into account. But, again, we do not see evidence that the program was geared towards the higher-exposed thorium workers.

Now, I mean, further analysis, maybe a different approach, certainly might come to a different conclusion, and I would, obviously, always welcome the more information we have.

MR. MORRIS: Okay. Then, one last question. I posed it earlier today. How is it that you can use this data to make the analysis and write the paper you just wrote, if the data are not usable?

MR. BARTON: Well, Bob, I think whenever you approach the review of a coworker model like this, it is important to look at it from different lights. I am certainly not going to sit here and pretend to have a real grasp like some of the other folks on the phone, and certainly Joyce Lipsztein, about the way these monitoring things work.

But, you know, if we come in with the problem that we don't like the MDA, we don't trust the numbers, that is one thing. If we can get over that hump, an analysis like this can then be very helpful in interpreting that data and deciding what you do going forward.

So, that is what I would say. I think it is important to look at something like this from multiple angles. You know, we might have sat down today and having the issues over the milligrams thorium, you know, we came up with a satisfactory answer. And now everyone agrees that we can trust this data.

Now you want to take a look at something like this to see, again, are you capturing the correct workers? So, I guess that is the way I would put it.

MR. STIVER: Yes, Bob, this is John Stiver.

What you need to understand is that these two issues were kind of explored in parallel, and it wasn't really a sequential thing at all. It turns out that this is just the way things kinds of fell out.

And so, there is not some cause and effect. It would make no sense that we had previously determined that the data were no good, but we are going to go ahead and analyze the distributions anyway. It just sort of was done in parallel.

And if I could, Tom LaBone, if we could kind of get back, when you first started, when you first came online, we had been talking to Bob Morris a little bit about the overall distribution for the coworker model. And Bob indicated that there is a GSD of three that is presumably based on the several thousand data points and just the statistical parameters that would apply to that distribution.

And we briefly touched on the issue of the uncertainties that would cause a particular result to be suspect based on the age of the source term and some of these other factors that might affect it, and how those uncertainties were taken into account.

And this GSD of three is really just applied to the distribution. It is not looking at the within-measurement uncertainty.

I would just kind of like to get your take on how you might consider approaching that aspect of it.

MR. LaBONE: The uncertainties in internal dose are a tough topic. I believe -- and Liz can correct me if I say this wrong about the coworker -- we can do an analysis and we fit basically the median bioassay data as if it came from one person. So, this is a group of people for each year, and we get their data and we find the median, the 50th percentile. And we will get that for each year. Then, we will fit it with a model for thorium, for example, as if it came from a single person.

And the GSD is going to be basically, I think it is determined from, you said the --

MS. BRACKETT: The intakes.

MR. LaBONE: Yes, the intakes, but for the GSD it is the 84th percentile. I'm sorry. That's it, right?

MS. BRACKETT: Yes.

MR. LaBONE: We fit the 84th

percentile intakes, and then the ratio of those two to the median will give us the GSD.

And if you have tight data, it will come and you can get a small GSD. I believe the minimum that is used is three. So, it can't go below that.

And a GSD, so say a 95 percent confidence interval for that is like times and divide a factor of nine, is what goes into IREP.

MR. STIVER: Okay. I understand how the GSD is determined from the distribution. I was just kind of curious about how you also factor in the within-measurement uncertainty. We may say it might be a factor of four uncertainty within any given measurement, and that is not really captured in the overall GSD for the distribution.

MR. LaBONE: No, because it is being calculated from the scatter of the data points themselves.

MR. STIVER: Then, you would make the assumption that that would be from an individual person. So, it would, therefore, account for these factors. So, I can't see where you are going with that.

MR. LaBONE: Well, again, I think the design, the original design of the factor, the GSD of three, a minimum, was to try to take into account all these things that are, I think, are very difficult to account for analytically.

You know, even today, if you had modern data from this year, it would be very difficult to go through and do a complete uncertainty analysis on that data. And that is if you had something that was easy to monitor, and we are not talking about thorium from 1971.

And so, I think that is what this factor, this GSD of three was for, was to try to accommodate those things that we are going to have a lot of difficulties trying to do basically rigorously.

MR. STIVER: Okay.

MR. ROLFES: This is Mark Rolfes.

I wanted to point out maybe one of the uncertainties -- now correct me if I am wrong, Tom or Bob -- one of the uncertainties earlier on that we had with our coworker modeling was the disequilibrium of the progeny of the thorium-232. We originally had an disequilibrium factor of .71. We have now gone to a theoretical minimum value for disequilibrium of .42. So, we have essentially eliminated the uncertainty of that particular piece of the puzzle, I guess.

MR. STIVER: Yes, Mark, this is John.

We would consider that .42 to be a claimant-favorable assumption, which I believe we put in our response. Still kind of a nagging issue for me is the .42 is a theoretical value for a closed system; whereas, we have this issue of potentially translocation of radium. And even the radon-220, even though it is only a one-minute half-life, it could diffuse outside to another area. It may cause a translocation and the deposition of lead, and so forth, in a different part of the body.

I guess the uncertainty that goes along with that, I know Sam had brought up the fact that whatever is going to be in a particular type, it is going to stay within that matrix. It is probably not going to go very far.

But some of the studies we read, especially for more subtle forms of thorium, have indicated that you have some pretty significant deviations below the .42.

MR. ROLFES: There's really not that many types of soluble thorium out there, though.

MR. STIVER: Yes. Well, this particular study, they used hydroxides and nitrates.

MR. ROLFES: Yes.

MR. STIVER: They were used in the chemical process.

MR. ROLFES: Those types of materials are still Type M materials. I am not aware of any Type F, fast soluble --

MR. STIVER: Yes, there was no Type F as far as I know. So, that is something that just, you know, I realize that -- I tend to agree that the .42 is claimant-favorable, but I think there is still some uncertainty in whether the -- they kind of wrapped all that in.

MR. ROLFES: Sure, sure. I agree. I know. This is just our tentative response. This is what we have been able to do in the past couple of days.

MR. STIVER: Yes.

MR. ROLFES: Just seeing the paper. So, we owe you, we owe SC&A and the Work Group written responses on these papers. I don't know if there are any additional things that we need to discuss really with this until we can get a written response back to the Work Group for your review.

But, like I said, we have only had -- I mean, there are some people, I'm sure, out there that have been working around the clock to respond, to prepare responses for this Work Group. So, I don't want that to go unnoticed. And I know SC&A has been looking at things as well. I don't want to shortchange the work that SC&A has done. We want to make sure that we take our time and look at it closely, and prepare a written response for the Work Group.

DR. MAURO: This is John.

I have a question that I am stuck on, and you have to help me with this. So, if I have a worker, and I do a chest count, and I come back and I say he has got 10 milligrams, that's the number, your numbers basically say it is 10, but there is a possibility, not an insignificant possibility, that the real but unknown number could be 10 times higher. Is that true?

In other words, with a GSD of three, it means that one standard deviation would be a factor of three higher; two standard deviations would be a factor of nine higher.

So, what you are saying, if I have just a single number, measurement of a person, and it tells me it is 10 milligrams -- what I am hearing is now, if I had a number of measurements for this person, and each one of them, you said, had a GSD of three, then I would start to get a little more comfortable because they start to offset each other, if you see where I am headed.

In other words, I have got a series of numbers. Each one has a GSD of three. Then, what happens is, you know, you really are asking yourself, what is the 95 percent confidence of the mean for this guy's body burden? And that narrows down greatly.

If you just have a single measurement, and you run IREP with that -- and you back-calculate the dose, and then you run IREP, the dose that you are going to be putting in could have a spread that is 10 times higher or 10 times lower.

And I am sorry I bring this up, but something about -- and it is only a single measurement -- something about that is disturbing to me. You know, you are giving this number, but you know and I know that there is a possibility, a 5 percent possibility, that that number, his true body burden, could very well be three, four, five, maybe ten times higher than that. But you are going to go with the number you are using as being the best estimate within that distribution.

What I just said, is that a fair representation of what will be done for a real person that only had one value in a chest count?

MR. ROLFES: John, this is Mark Rolfes.

I think you are twisting some things around in there about where the uncertainty lies. And let me explain, basically, how we would interpret the data that we have available to us.

Obviously, there is going to be a lot more uncertainty involved with one measure versus two or ten measurements. The more measurements you have, the better you are able to refine your --

DR. MAURO: Yes, I agree with that. And so, I am not so much troubled when you have a number of measurements because they sort of work themselves out in the average. But if you have one or two, there is where I am troubled by the approach you are using.

I know this is a little offline of what we were talking about. If I am just off-base on this, fine, we will just move on.

DR. GLOVER: Could I -- I'm sorry,

John.

DR. MAURO: Go ahead.

DR. GLOVER: One thing, you are saying that there is only one number in the guys chest that you are measuring as the send lever. And if you make a series of measurements on that, we may have a very precise series of measurements.

I think the GSD of three comes when we then extrapolate back to an intake. It is the biokinetics and all that fun stuff that goes in with what is the intake that gets a GSD of three. I mean, we can maybe infinitely know, when I dissect, if the guy was an autopsy case, which they had a lot of autopsy cases, we know exactly what is in the lung within a couple percent.

DR. MAURO: Right. Right.

DR. GLOVER: But that doesn't mean I can go to intake with a couple percent precision. So what are the biokinetics -- anyway, I will leave it alone at that.

MR. ROLFES: And this is Mark.

Basically, we would use that data to estimate the intake and basically make assumptions about the type of material and look at the case specifics for that case. We would assume a chronic intake assumption, basically, for that person, calculate the internal dose to the target organ or the cancer that was diagnosed, and then that dose value, if it is a best estimate of intake or a claimant-favorable estimate of intake, the uncertainty, the GSD of three would be applied on that dose estimate.

So, it is not on a measurement, the chest measurement. The uncertainty is later on in the dose reconstruction process. So, the GSD of three would be applied into IREP. We would use the -- if you look at the two parameters that are entered into IREP for internal dose for a best-estimate-type thing, the first parameter would be the dose value, and the second parameter would be the uncertainty associated with that dose value.

DR. MAURO: Right, right. And it struck me that you will be putting into IREP a dose, a geometric mean, and a geometric standard deviation which is a factor of three higher than that, and then, of course, let IREP run.

In theory, what you are saying is, yes, there is a possibility this guy's dose is a factor of 10 higher, his real but unknown dose is a factor of 10 higher. And you let IREP run, and it picks off the one percentile or the 99th percentile.

Okay, I am sorry for the diversion. I just got sort of stuck in the mud on that one.

MR. ROLFES: That is an important point.

This is Mark Rolfes again.

Keep in mind that the calculated probability of causation at the 99th percentile essentially represents that there is only a 1 percent chance that that individual's dose could have been higher or that individual's probability of causation.

DR. MAURO: Well, you see, I always thought the reason you operate at the 99th percentile is to account for individual variability in risk per rem. It seems that the uncertainty in the dose is also blurred into that. Do you see where I am headed with that?

I was always comforted by the idea that we all know that we don't really know what the risk coefficient is for a real person. We know what it is for a population as adjusted to the United States, et cetera, et cetera. But for a real single person, you really never know what their risk coefficient, risk per rem is. That is the reason you operate at the 99th percentile.

And I like that, and I think that is very claimant-favorable. It makes sure that you are giving the benefit of the doubt to the person with the cancer.

But blurring in this big uncertainty in dose with that in your sampling seems to dilute out that benefit that you grant when you are operating at the 99th percentile level for the purpose of IREP. Do you see where I am headed with that?

I have definitely moved into a different realm here. I'm sorry. And this is something that I think I would like to pursue, I guess, in another venue.

MR. ROLFES: Yes, John, this is Mark, and I am not sure. We have sort of changed tracks, I think.

DR. MAURO: We certainly did, and I apologize for that.

MR. STIVER: John, we will have to talk about this sometime offline.

DR. MAURO: Yes.

MR. STIVER: It is an interesting topic.

CHAIRMAN CLAWSON: So, where were

we?

MR. KATZ: Look what you did, John.

(Laughter.)

DR. MAURO: I'm sorry. My mind wanders at around four o'clock.

MR. LaBONE: Can I make just a quick summary then?

MR. KATZ: Please do.

MR. LaBONE: Just very briefly, I think it is valuable to do this sort of data exploration that was done here.

Two pure statistical comments are to perform a test on Figure 1 of your choice to see if those things, those two curves are different or the same.

And again, a statistical comment that Bob made is these figures, like Figure 8, you know, there are robust regression techniques that would, I think, better capture the trend and not be as distracted by that outlier out at like 33 milligrams on Figure 8.

Again, I think if you look at that, that will fix that right up.

The issue of the thorium workers, again, do they go through time as a group or do they rotate positions?

And, then, the question of causality has to have additional information. You know, what is the cause of what we are seeing versus just the pure correlation, to make that distinction clear in the conclusions of the report.

So, that pretty much wraps it up.

MR. STIVER: Thank you.

MR. ROLFES: Thank you, Tom.

MEMBER GRIFFON: Those sound like SC&A actions.

MR. STIVER: Yes.

MEMBER GRIFFON: And I think I heard Mark volunteer that NIOSH has the action of putting these comments into writing.

MR. ROLFES: Yes. Yes, absolutely. We will definitely prepare a response to these two White Papers.

MR. STIVER: As kind of a prelude to what we --

DR. LIPSZTEIN: May I make one comment? This is Joyce.

CHAIRMAN CLAWSON: Sure.

DR. LIPSZTEIN: On page 10 of the White Paper that we did on data quality, please be aware I put an example of some lung-counter calibration rounds. I concluded late. So, in this example, a factor of 19 error. This is not 19; this is two. Okay? This is a typing mistake, please.

CHAIRMAN CLAWSON: I understand that. Thank you, Joyce.

DR. LIPSZTEIN: Thank you.

MR. STIVER: Mark, if I could just say one thing, it is important to address these issues in the completeness paper that Tom discussed. But I think it is more important to address the issues we have in the adequacy paper, too. So, we really look forward to what you can provide us on that.

MR. ROLFES: Right. Right. We owe responses on both of these. We have not responded to either of these.

MR. STIVER: And I guess Bryce or you guys are going to try to get a handle on what the downblending time periods might be.

MR. ROLFES: Yes. Correct.

CHAIRMAN CLAWSON: Yes, the two weeks out of the year.

Okay. And these are all action items. Like Mark said, he is going to review the two papers and give a response back. I don't think that we will have them before the Board meeting, though. But what we can get would be greatly appreciated.

Also, we are going to have to give a response at this Board meeting of where our issues are and where we are going. We are trying to get this to be able to be brought before the Board because, I am going to be honest, especially after today, I don't know that the Working Group is really going to come to a total conclusion on this. We are giving it our best effort, but I think, also, too, we need to get the Board involved. They may have some aspects of where they want us to look.

MEMBER GRIFFON: I may regret this, but other than recycled uranium and the thorium, are there other -- I am trying to remember where we stand on --

CHAIRMAN CLAWSON: No.

MEMBER GRIFFON: These seem to be the main ones, right? Okay.

CHAIRMAN CLAWSON: Yes. We could talk about K-65 silos, but --

(Simultaneous speakers.)

MEMBER GRIFFON: I know we didn't discuss it today, but I know SC&A has weighed-in more favorably than I would on the daily weighted average model for the earlier period for the thorium work, right? Is it the 53 to --

MR. STIVER: Yes, that would be

the 53 to 60 days.

MEMBER GRIFFON: Fifty-three, whatever, yes. I don't know. I know I missed the last Work Group meeting.

MR. STIVER: We talked about that at the full Board meeting.

MEMBER GRIFFON: Anyway, I will try to get my thoughts together before the Board meeting coming up.

CHAIRMAN CLAWSON: Well, and I have got to give somewhat of a presentation, too. So, it will come after the full Work Group and to DCAS, and so forth.

But as far as SC&A, we have not -- well, we have got to get the construction coworker model to be able to review it. But that whole thing is going to have to be reviewed.

MR. STIVER: Yes, that is on the way here.

CHAIRMAN CLAWSON: Right.

MR. ROLFES: Yes, that is in our

shop, and we need to finalize it. We will get that out as soon as we can as well.

CHAIRMAN CLAWSON: Right.

The response, and this was probably DCAS to SC&A on the thorium papers, and so forth --

MR. STIVER: Yes, I have got that one captured.

CHAIRMAN CLAWSON: Okay. Now did we have other ones that SC&A owed DCAS?

MR. STIVER: I don't recall anything that came up. I mean, we certainly want to see this downblending time information to determine the feasibility of bounding that, the downblender class, for RU.

CHAIRMAN CLAWSON: Right.

MR. STIVER: And that is really the big issue.

CHAIRMAN CLAWSON: Did we miss anything owed to DCAS? Do you know any --

MR. ROLFES: I don't think so. I think we mentioned the statistical tests at the end of our conversation there.

MR. STIVER: Yes. That will be after we get the formal response from you guys.

MR. ROLFES: Correct. Yes. I don't believe there is anything else that we have missed, not that I can think of.

Sam, did you catch anything else that --

MR. KATZ: So, John, will you work with Brad and Mark to pull together a presentation? We have a pretty big time slot for this. We don't need to use it all, though, but we will make do. I mean, you can use it all, but --

(Laughter.)

CHAIRMAN CLAWSON: No, I am starting to have people asking several questions. I think a lot of this is going to come up, because we are trying to bring the Board up to speed of where we are at.

MR. KATZ: Right.

MR. STIVER: Yes, last time we had a pretty quiet audience. I think there may be a lot more interplay this time.

MR. KATZ: Yes. No, my main concern is just I want to keep, since you have started sort of bringing the Board along, I want to keep them along. So, you maybe even want to be a little bit repetitive of what you have covered before. I want to keep them engaged, so that when they are ready to bite on this, they have all that background.

CHAIRMAN CLAWSON: Paul and Phil, do either one of you have anything that needs to be brought before the Work Group at this time?

MEMBER ZIEMER: This is Ziemer.

Yes. Well, I have appreciated the discussion this afternoon and the issues raised.

I think, John Mauro, your issue, your last one, we can send that to the Scientific Issues Committee, the Work Group.

(Laughter.)

But, in any event, yes, I think the path forward here that has been outlined makes sense. We need to get the responses. So, I am comfortable with that.

CHAIRMAN CLAWSON: I understand. It was good to hear your voice here.

I am not going to see you on YouTube or Funniest Videos, am I?

MEMBER ZIEMER: Well, I hope not.

CHAIRMAN CLAWSON: Okay. I just wanted to make sure. I hope you are feeling better. I am sorry to hear about your little incident there.

Phil, is there anything that you wanted to bring?

MEMBER SCHOFIELD: No. I think it pretty well got covered today. And John totally confused everything before he was done.

DR. MAURO: That's my job.

(Laughter.)

CHAIRMAN CLAWSON: Just a 30-second soundbite, but we appreciate it.

With that, DCAS, is there anything that needs to be brought up before we adjourn? Or, SC&A?

MR. ROLFES: I don't think so, Brad.

MR. STIVER: I think we have covered pretty much all of it.

CHAIRMAN CLAWSON: Okay. With that, we will adjourn. We appreciate everybody. We will see you in a couple of weeks.

MR. KATZ: Thank you, everyone on the line.

(Whereupon, at 3:57 p.m., the meeting was adjourned.)