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

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ADVISORY BOARD ON RADIATION AND
WORKER HEALTH

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WORK GROUP ON FERNALD

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THURSDAY
APRIL 19, 2012

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The Work Group convened telephonically at 11:00 a.m., Bradley P. Clawson, Chairman, presiding.

PRESENT:

BRADLEY P. CLAWSON, Chairman
PAUL L. ZIEMER, Member

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ALSO PRESENT:

TED KATZ, Designated Federal Official
ISAF AL-NABULSI, DOE
SANDRA BALDRIDGE
BOB BARTON, SC&A
EVERETT "RAY" BEATTY, SR.
ELIZABETH BRACKETT, ORAU Team
MEL CHEW, ORAU
SAM GLOVER, DCAS
STU HINNEFELD, DCAS
KARIN JESSEN, ORAU Team
JENNY LIN, HHS
JOYCE LIPSZTEIN, SC&A
JOHN MAURO, SC&A
ROBERT MORRIS, ORAU Team
MARK ROLFES, DCAS
BILLY SMITH, ORAU Team
JOHN STIVER, SC&A

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T-A-B-L-E O-F C-O-N-T-E-N-T-S

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

(11:00 a.m.)

MR. KATZ: Let me get this started. This is the Advisory Board on Radiation and Worker Health, Fernald Work Group and we will begin roll call.

(Roll call.)

MR. KATZ: Let me just mention this for everyone involved. Some of the papers, I think, are already posted on the website for this meeting.

There are a couple papers from DCAS, they're not yet posted, they may be posted now, but they were sent, Sandra, to you directly by email by the program since they hadn't been posted yet, so I hope you have those, Sandra. One of those was a presentation, a PowerPoint presentation that I think DCAS will probably be using during this meeting, as well as, again, the agenda for this meeting is also on the website. It's a

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pretty simple agenda.

Now, I drafted this agenda without input just by seeing the materials that were flowing back and forth between the parties, but the Chair and DCAS didn't have an opportunity, or didn't get around to commenting on the agenda, so I guess it's your agenda.

Brad, if you wish to admit revisions or solicit them from the program, that'd be the place to start, I think. And, everybody, please, other than the people who are speaking, mute your phones. If you don't have a mute button, use *6.

There's quite a lot of hiss in the background, which makes me think there are a lot of people that are not on mute. And then to take your phone off of mute, just press *6 again. Okay, Brad, it's your meeting.

CHAIRMAN CLAWSON: Well, I appreciate that and I'm trying to find my

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agenda that I just dialed in on. It's terrible when you do that, but I guess, basically, I'll turn it over to John Stiver. He's the SC&A person on this and we'll start out from there.

MR. STIVER: Okay. Thank you, Brad. My name is John Stiver from SC&A. I know most of you all. Today's topic is fairly focused. This is our issue 6b, which is the in vivo chest count data issue for the period of 1968 to 1978 when the data were reported in units of -- milligrams thorium.

So a bit of a recap. At our last meeting we had, kind of, come to a position where, this was on February 9th, SC&A had presented our kind of final position on this.

And that was that we felt that, because of uncertainties in the data set, which could, we felt, depending on how the milligram data were calculated, could give rise to underestimates up to possibly a factor

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of 100 and also overestimates for the unexposed contingent, which could approach a factor of three orders of magnitude.

We felt that this data set was just not adequate for reconstructing doses with sufficient accuracy in an SEC context.

And shortly thereafter, NIOSH, right before the Board meeting in Oakland, they had posted some additional documents which were related to this issue, some of which, many of which we had already seen and I believe there were a couple that we hadn't seen.

And then, at the meeting, I presented kind of a summary of our position. I looked at two issues, really. One was the uncertainties inherent in the data themselves and we also looked at this, kind of, an overarching issue.

This idea of whether the system was adequate for the intended purpose under

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EEOICPA, which was the issue of the stated detection limit being so high as to result in almost a de facto SEC-type situation where almost, virtually all metabolic cancers would likely be compensated.

And that's, really kind of an SEC Subcommittee issue. I tuned in to the meeting yesterday and the Board was taking a very deliberate approach on this issue of sufficient accuracy and what it really means and how it should be defined.

And that's probably, you know, the best way to approach this. And DCAS is in the process, evidently, of putting together a matrix of all SEC decisions and their bases, and then that's going to kind of serve as a starting point, kind of a gold standard.

And so that second aspect of sufficient accuracy regarding the system itself, I think, will be tabled today. I really don't intend to discuss that because it

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really is more of a program-wide issue.

So what we're going to talk about, really, is this issue of the particular data set itself and the problems we have with it.

Since the Board meeting, Joyce Lipsztein put together kind of a summary of all the new NIOSH references as well as the previous White Papers going back until, I believe, August of 2011 and kind of laid out what we feel are the SEC issues and the technical bases for those.

And after that, I believe it was April 9th, DCAS submitted a presentation outlining their position in a nice spreadsheet with links to the different documents that kind of support the positions on the various slides.

And one thing that really jumped out on that list was an interview with L. Max Scott. He was the principal architect of the mobile system at Y-12 and evidently he's still

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active and a professor at Louisiana State University, I believe.

And so they interviewed him and clarified some of the issues for us, but unfortunately, also crystallized some of the problems that we saw in the data set.

But Mark Rolfes also put together a presentation outlining their position, and I guess the best way to approach this would be to have Mark go ahead and give his presentation and then we can talk about the issues that we have with the derivations for the milligrams thorium as they stand now.

MR. ROLFES: Okay, this is Mark Rolfes. Thank you, John. Yes, I just put together a brief slide show on bounding thorium-232 intakes using the mobile in vivo radiation monitoring laboratory data.

NIOSH can bound thorium-232 intakes -- I'm just going to go ahead and read through these slides for people who don't have

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it in front of them.

NIOSH can bound thorium-232 intakes. Sufficient data are available. A coworker model has been developed using more than 5000 mobile in vivo radiation monitoring laboratory thorium chest count results.

Chronic thorium-232 intake retention fractions were derived, which account for the differential biokinetics of decay products. And sample intake and dose calculations have been completed to demonstrate the methodology and feasibility of the methodology.

NIOSH has investigated where, when, and why the mobile in vivo radiation monitoring laboratory technology was developed, how it was calibrated, and the operating procedures.

We did learn from Max Scott that the techniques in calibrations of the mobile in vivo lab were identical to the Y-12

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stationary whole body counter.

We also learned who was selected for chest counting and why, and information regarding the reporting and interpretation of the mobile in vivo results.

The calibration of the mobile in vivo lab used a REMAB phantom. We learned information about the thorium calibration standard, which had a thorium-232 to thorium-228 ratio of 1.27 and a thorium-232 to radium-228 activity ratio of 1.67.

It was reported that for that material the limit of detection was 6 milligrams of thorium. The thorium chest burdens were reported in milligrams rather than in nanocuries, and this was based solely upon an established protocol for reporting uranium at Y-12.

Rather than reporting activities of uranium at the Y-12 counter, they also did a similar reporting methodology, reporting

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uranium mass. So to be consistent, they reported thorium in mass as well.

Thorium-232 chest burdens derived from measurements of deposited progeny are basically determined by the 240 keV gamma ray from lead-212, plus the 330 and 900 keV gamma rays from actinium-228.

These photopeaks were measured and allowed the mass of thorium to be determined using an established Y-12 technique of using ratios of the count rates from adjacent regions with the interests in the spectra.

Some of the adjustments that need to be applied to the measurements, for example, when NIOSH has a measurement from the mobile in vivo, we assume that the age of the thorium exposure material in this historical exposure scenario, we assume that that thorium material age is unknown.

The chemical separation and the purification of thorium disturbed the

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equilibrium of the thorium-232 and progeny. And so in order to bound intakes under EEOICPA, we've assumed that three chemical separations of the thorium material occurred in order to produce the worst-case disequilibrium ratio of thorium-232 to thorium-228.

The ratio becomes, after three chemical separations of the thorium over 8.8 years at specific times over those 8.8 years, you get the largest disequilibrium ratio for triple-separated thorium, which becomes a ratio of thorium-232 to 228 of about 1 to 0.19.

So if you divide 1 by 0.19, that gives you a correction factor of 5.25, which we would apply to the measured results, but you also have to correct -- I'll get back to that in just a second with a specific example.

The next slide shows the algorithm that was used by Y-12 to estimate the quantity

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of thorium in someone's chest. And we've got an equation here which basically shows the count rates in a particular region of interest.

And, for example, the region of interest from 0.208 to 0.248 is the count rate from the portion of the spectrum between 208 keV and 248 keV, which was one of the photopeaks from lead-212.

There's a factor representing background data. It's basically from measurements of 1100 unexposed workers. This factor was determined to be 3.23. And then we have a thorium coefficient of 8.84 to convert to units of thorium mass.

In summary, the NIOSH and ORAU whole body count experts have reviewed and approved the approach to bound thorium intakes and doses to workers. We believe that the thorium mass reporting methodology is not an SEC issue and that the thorium intakes

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estimated from the mobile in vivo radiation monitoring laboratory are plausible, claimant-favorable and bounding.

And to get back to a brief example of our proposed approach, if we have 1 milligram thorium chest burden of material identical to the calibration source, that would contain 0.086 nanocuries of lead-212.

So if this was actually triple-separated thorium, we would then multiply this value by the triple-purified thorium correction factor of 5.25 to give us a value of 0.45 of correction factor.

If you divide this by the specific activity of thorium-232, 0.11 nanocuries, it gives you a value of 4.1 milligrams of thorium.

So by applying this triple-separated thorium correction factor, the 1 milligram of thorium measured in the chest we would interpret to be 4.1 milligrams. So

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we're applying essentially a correction factor of 4.1.

Let's see. That basically sums up the approach that we have proposed to use the mobile in vivo radiation monitoring laboratory results to reconstruct thorium intakes.

The intake rates are actually going to be recalculated based upon this correction factor and we are working on a draft of that right now at this time. And with that, that's our summary of our proposed approach. If there are questions --

CHAIRMAN CLAWSON: Hey, John, before you jump in, this is Brad speaking. Mark, I was kind of taken a little bit back by not even knowing about this interview until all of a sudden it pops up in a report that you were putting out.

I was kind of under the understanding of when we did interviews like this that, you know, the other party that

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would be involved was also to be notified and then to be able to find this out coming out into a report, it just kind of caught me off guard with this.

So, you know, this person's a valuable asset to be able to bring a lot of this to light and I'd just want to know that -- I didn't see this -- just to tell you the truth, I'd probably would have wanted to be involved with it if they were performing interviews like this. There's a few questions that I had too.

MR. ROLFES: Well, we did hear back from Max Scott. This was, basically, a last-minute schedule, based upon our tight work schedule, to get results back to the Advisory Board Work Group.

And Mr. Scott is willing to speak with us again if you'd like to ask him some additional questions. I'm sure we probably have some clarification questions for him as

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well.

CHAIRMAN CLAWSON: Well, you know, we've tried as a Board, and we've also tried as NIOSH, to be able to limit, especially to these sites and these individuals, six or seven different times.

Having to come back into this, I would just appreciate that if anything like this does come up, at least notify us. I realize that sometimes these individuals come up and it's a spur of the moment thing, but at least notify us of the interview and that a report is following, because I was somewhat blind-sided by this report.

All of a sudden I had to start asking, where did, you know, the interviews come from and so forth like that. So I just caution us to try to cooperate with each other's side just a little bit better on this and communicate it.

I know that these things come up.

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It's just I personally would like to have been notified and know of it so that I knew what was coming. With that, I'll turn it over to John to have his response and we'll go from there.

MR. STIVER: Okay. Thank you, Brad. This is John. And thanks, Mark, for the presentation. I'm going to go back to our previous concerns before the Scott interview, clarify some of this, and we were concerned as to exactly how the milligram data were derived.

And we know that post-1978, the actual activities of lead-212 and actinium-228 were calculated, which then allows the derivation of the age of the material, and based on the lead-212 then, using Tom LaBone's intake retention fraction White Paper and the triple-purification claimant-favorable-type assumptions, we feel that those techniques can yield a plausible upper bound for that period

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during which the activities of the progeny are actually provided.

We never had a problem with that. What we have is that we also were not quite sure which nuclide was measured in order to get the milligram data for the '68 to '78. And the interview with Max Scott has clarified that, no, they did not actually measure the activity.

They had this empirical formula here which looks at ratios. And I'd like to draw everybody's attention to the paper, or actually, just a technical note that we sent out just a couple of days ago, April 17th. I hoper everybody has a copy of this.

It's entitled: SC&A Comments on Slide 7 of the NIOSH Presentation. And before I get started on that, I'd just like to take a look at, in Slide 6, where Mark had discussed the triple separation.

I think it's very important that

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we don't conflate these two methodologies. We believe this is a perfectly adequate approach to take to the lead-212 measurements when they're reported actually in activity units.

However, this particular -- that would not apply to this empirical formula on Slide 7. So I'd just like to kind of discuss this equation a little bit and make sure everybody on the Work Group, and all the participants, really understand what this means.

As Mark said, you have three regions of interest. The first is for a 240 keV emission from lead-212. The second is for the 330 keV emission from actinium-228. And the third, 900 keV emission from actinium-228.

And what they did, these ratios, really, they took the region of interest where the photopeak is, they divide that by the adjacent higher region, where there is no counting from the particular emissions, and

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then this ration then gives you an idea in comparison to background, whether there is an elevation in those photopeaks relative to an unexposed population.

And I guess there's two empirical values here that I'd like to discuss and this really clarifies a lot to me. The first is this 3.23 and the other being the conversion factor to get from this dimensionless ratio to milligrams of thorium, this 8.84, which is contingent upon the calibration source used for the calculations. That's where this 8.8 comes from.

If we take a look at the 3.23 first, now, this represents the ratios in these three regions of interest. The summed ratios for these 1100 unexposed workers from the Y-12 plant.

And, really, this is a 3.23, basically, they're just a little over 1 and when you look at the spectra in the paper, I

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believe it was the Scott paper for 1969, you see there's a little bit of elevation in there, but not a lot.

The uncertainty in that distribution of summed ratios for the 1100 was rather tight, as we'd expect for such a large sample size, and that is 0.7. And now, I understand where the 6 milligram stated detection limit for the system came from.

It's basically 0.7 times 8.84. It gives you 6 milligrams. So basically, it's the uncertainty in that background distribution. So if you had, essentially, no differentiation between your measured individual in this background distribution of summed ratios, the uncertainty in that empirical approach would correspond to 6 milligrams.

So it's not a detection limit in a classical sense based on the accounting statistics of your system using the approach

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of Currie's paper in 1968. It's really an empirical construct.

And so we note that there are also, if you take a look, I believe, on our little paper on Page 3, we had a discussion about this.

And so I guess the problem being is that this kind of solidifies our concerns regarding the detection limit and also its applicability to Fernald because I note that the mobile system did not have the same level of shielding.

The iron was not quite as thick and it wasn't quite as low background as the thick system at Y-12. So you'd expect a slightly different -- we really can't determine what the difference would be in this background distribution of summed ratios and we would also vary depending on the number of individuals who were measured.

I mean, obviously, if you only had

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20 or 30, they'd have a much larger spread as you get more -- you know, it's just a known quantity of the central limit there and how that works.

So anyway, that really calls into question the whole issue of the 6 milligrams and I know Dr. Ziemer, Paul Ziemer, had a lot of questions regarding that in the last meeting.

And that was in relation to Table 1, which, remember, this was this period of overlap where we had two groups of workers in 1979, one from one plant for a short period of time, another from a separate plant for a short period of time.

And we had lead-212 measurements, all but one of which were above the detection limit based on that approach that we're taking post-1978. And they also had another column which gave, you know, milligrams of thorium.

And because you're looking, almost

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certainly, at a single source for those two groups, and you have a range of lead-212 measurements, all of which are above the detection limit, you would then have expected that the milligram thorium data would be proportional to those lead-212 measurements, if indeed, the milligram data were calculated off of the lead measurements.

And what we found instead was that every single one of those was listed as 2.1 milligrams. Well, the real reason for that is because if you have a detection limit for lead-212 of 0.23 nanocuries and you assume secular equilibrium, that's going to correlate just based on the specific activity for thorium, it's going to be 2.09 milligrams. So that's where the 2.1 came from.

What they did is, they just assigned the MDL value of milligrams that would have corresponded to the detection limit for lead-212. And, you know, we brought that

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up and we keep bringing it up at every meeting, practically.

Just illustrate that, here's the situation where you have a set of data, but you've got a comparison of the two. And it looks like the approach that had been declined by NIOSH really wasn't used. And so now, when we can step back, we can also look at this from another view is that, well, now we know what the 6 milligrams really means.

And it's a highly variable quantity that depends on the characteristics of the summed ratios for these unexposed workers.

The other aspect of this is this value 8.84, this conversion factor to get from this dimensionless ratio to milligrams of thorium.

And looking at the papers by Mr. Scott and Mr. West back in the mid-'60s where they were doing this system development, it

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appears that what this really does is this is another empirical value which basically takes that calibration source they had at Y-12, which was at about 80 percent equilibrium for lead-212 and about 60 percent for radium.

And they used that, placed in their REMAB phantom in the lung tissue, I believe they used sponges to simulate lungs in this phantom, and put these little vials of material in there, and then they knew the mass that they were inserting, and they were able to determine that, you know, when you get a ratio increase of 1 compared to the unexposed group, this corresponds to about 8.84 milligrams of thorium.

And so they indicate that they selected that source to be representative of the thorium that was being handled there at Y-12.

Well, now you take this system, you got a mobile system now in a tractor

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trailer rig, and you're moving it around to the different labs, but the real reason for using this was to measure U-235 and get maximum permissible body burdens.

The thorium measurements were kind of ancillary, but they had a technique and they could apply it. So, you know, they're here, we can measure them, let's go ahead and measure thorium too, but the real reason was to measure uranium and get these, you know, quantitative measures of the MPBD for that.

But when you're taking it to place like Fernald where, during a period of thorium production, you have all different types of sources.

You've got type M, you got type S, you've got freshly separated material from the refinery, you've got that feeding them and being converted to an oxide; very similar, almost exactly similar to the uranium processing approach.

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Then it's fluorinated to produce a tetrafluoride, which is then reduced to metal, and so forth, and then, I believe in thorium, they sent it offsite for extrusion into rods, but there was some machining done there as well.

And so you have this whole range of source terms; ages of the source. And so when you try to apply this value, we have a set of ratios to some of them.

And that's all you have, really, that summed ratios, and you've got this calibration factor which is dependent, entirely, on the characteristics of the calibration source and you hope that it's representative of the material you're actually trying to measure.

And in our little ditty here, we did some calculations, Joyce actually did these calculations, where she took an example, okay, we've got a type M thorium, the guy is

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exposed for 60 days, and then he's monitored in the mobile laboratory.

We looked at several different contingencies. First, we looked at the middle of his exposure, 30 days from the onset of the intake, we looked at the last day of exposures, and then at 90, 120, 180, and 360 days after the first day of exposure.

And what we did, we kind of finagled just a little bit, just for illustrative purposes, we used the detection limits that were in place post-1978 in units of nanocuries.

And when we took a look at this and we said, okay, let's look at three different contingencies. We've got a material that's essentially in equilibrium. And they looked at, okay, given this 10 -- oh, and, basically, what you wind up with is a 10 milligram value at these different time periods.

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And so, you know, based on just the range of, we presented something similar to this at the meeting, but just bear with me, intakes over this period of time, corresponding it to 10 milligrams, it ranges over about from 17 to 140 becquerels, so it spans about an order of magnitude.

If the worker was exposed to material in equilibrium, it would be detectable. Then we took a look at, okay, let's take this triple-distilled thorium. Let's look at that.

And so we took a look at that, give it the same amount of material that would be in the lungs at the end of these exposures, and in those situations, the range of activity of lead-212, and of actinium, would not have been detected.

So here's a situation where you would have a 10 milligram burden which would be virtually undetected given this technique,

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even if you had the conversions to get back to activity, which, in this case, we don't. All we have are milligram values and we don't even have the ratios or the count.

We finally looked at, okay, here's another situation. Let's look at a situation where you, instead of just having a triple purification, we just have one purification. And in that situation, you would have detectable counts in the lead-212 peak by virtue of the unsupported progeny of the 228 thorium, which would be decaying away.

And the progeny build-in is governed by iridium-224, which builds in at, I believe, a 3.6 day half-life, so within about three weeks the progeny are in equilibrium with unsupported 228, but then the actinium ROIs, the two actinium ROIs, would detect nothing. And so this is illustrative of how you could have very large intakes that would be missed all together.

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And so I guess, at that point, really, I can say that our concerns aren't really alleviated. If anything, I think they're more crystallized here regarding the applicability of this data set without any way to get back to what an activity of lead-212 might have been.

The only way I can see where you could get that is if you had the count data and the efficiency. And from that, you could apply the LaBone method to get back to a worst-case situation, you know, for triple distillation.

But as it is now, SC&A feels that this is just a really unstable foundation to build a coworker model on what is going to be the fundamental basis for making compensation decisions for hundreds, if not over a thousand, workers.

And that's, kind of, where we stand at this point. So if the Work Group or

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DCAS want to resolve that. And I'd also like to, because we have Stu onboard, and he was, you know, working at Fernald during this period of time.

Maybe, Stu, could you tell us a little bit more about this 8.84, and what do these values really represent, and do you have any knowledge of the system and how it was used at Fernald?

MR. HINNEFELD: Well, this is Stu. I didn't start until 1981, so I wasn't there during the '68 to '78 time period, but the mobile in vivo counter was being used for several years after I started.

At the time that I was there, it was, really, a uranium counter. Thorium work, by the '80s, I don't recall any actual thorium processing or production. There was thorium in storage here and there, and there may have been some over-packing of containers that were corroding.

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MR. STIVER: Right.

MR. HINNEFELD: But thorium was largely just kind of left alone. It was in warehouses. It tended to be, for the most part sort of remote from the rest of the plant. That's not a 100 percent true. There was some thorium stored in relatively close proximity to some of the process areas.

So my recollection from my experience at having been there was: we didn't pay much attention to the thorium numbers because no one was really being exposed to thorium anyway.

I have spent quite a lot of time, since I've been authorized, and also in anticipation of being authorized to speak to the workers about this, and trying to follow the discussion here.

And I think it's pretty clear from everybody's work that, when the mobile counter was recording thorium milligram numbers, it

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did not actually arrive at activities of lead-212 and actinium-228 as an intermediate step and go from there.

John described really well that there was just a sum of the ratios approach. And so my question has always been, well, how is there a method to deconvolute a milligram of thorium number into lead-212 and actinium-228 numbers that would give you a reliable bounding estimate, because I'm pretty clear you can't really deconvolute it and an amount of it gives you a precise estimate and then interpreting that milligram number, but can you do a bounding interpretation?

And so from that standpoint -- and some of my thoughts here just crystallized during John's presentation a minute ago. There are, from the calibration source that's described in either the West or the Scott paper, I forget which paper describes it, there is some knowledge that we have about how

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the mobile in vivo counter interpreted a thorium source with those specific activity ratios; how it interpreted that into milligrams of thorium.

And so the question then is, well, if you don't have a mixture of thorium that is the same as your calibration source, what are the possible combinations of lead-212 and actinium-228 that would be interpreted the same way as, say, 1 milligram of the calibration source?

And so having, you know, thought about that, it's not a simple issue to explain and the discussion that John talked about of the years when we have reports that were listed both ways, both with the activity of lead-212 and the actinium-228 and the same subject reported in milligrams of thorium, that, to me, kind of becomes more troubling as I think about it.

The issue there being that, you

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know, if, in fact, the mobile counter interpreted -- if you have 1 milligram of thorium resulted to a source that had 0.086 nanocuries of lead-212 and a somewhat less activity of actinium-228.

And if, in fact, the subject had freshly separated thorium in his lungs rather than the calibration source mixture, then the counter would see only lead-212 and it would require slightly more lead-212 than 0.086 milligrams because the lead-212 ratio has to account for the entire sum of ratios.

He doesn't get any additional sum from the actinium piece. And so 1 milligram then, if it were strictly lead-212, should translate to something, you know, not a whole lot more than 0.086 nanocuries of lead-212.

The reason being that, the paper that describes the ratios and the one calibration we have that shows how much each region of interest contributes, shows that, at

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least for that example, the lead-212 contributed more to the excess ratio than the actinium-228 at, what I believe to be, the calibration source ratios.

So if we're counting only lead-212, and we got 1 milligram, then we should see some value slightly above 0.086. And then when you look at the measurements from the year when you have both milligrams and activity, and John cited some of these, you have some measurements where the lead-212, I think it was in the 0.4 region, maybe higher than that, maybe higher then 0.4 in some cases, and the milligrams were at 2, then it seems to me that, just on the face of it, the 2 milligram in vivo result shouldn't have been much higher than about 0.2 nanocuries because, you know, 0.086 and a little bit bigger than that, say, 1 nanocurie for 1 milligram, so then 2 milligrams would be about 2 nanocuries.

And so if you have 0.4 or higher

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nanocuries of lead-212, why isn't that providing a sufficient boost to that sum of ratios that get a higher number than 2 milligrams?

So there is a lot of stuff here. A lot of complications that SC&A has kind of pointed out, and there's some really sketchy available data on the actual calibration, at least that I've seen, calibration data.

So, to me, I'm really struggling with making a firm conclusion here that you can deconvolute that thorium number. So that's what I'm struggling with

MR. STIVER: Yes, I guess we've kind of come to the same place in that regard.

MR. ROLFES: This is Mark, and I know we had previously discussed a potential negative bias in some of the lead-212 results. I'd like to check, maybe, if Bob Morris might be able to possibly comment on that if that would play into this discussion here when they

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switched over from reporting mass results to the activities of both actinium-228 and lead-212.

I believe SC&A previously had a comment that the lead-212 activities appeared to have a negative bias to them. And so we had proposed adjusting the reported lead-212 values upwards by this negative bias amount.

Bob, I don't know if you have any comments on this.

MR. MORRIS: No, you said it correctly.

MR. ROLFES: Okay.

MR. MORRIS: We did take note of the SC&A comment, which was made, well, probably a year and a half ago, pointing out that there was, obviously, too many negative values, too many values below, hovering around 0 to be statistically appropriate, and we agreed with that.

Went through and made a process of

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a bias adjustment to bring dose up as an additive value. So I think that that is in the plan for when we revise the coworker model for '78 to '88. It has no bearing on the coworker model for '68 to '78.

MR. HINNEFELD: This is Stu. I mean, you know, that's an adjustment that's made when the data are reported in activity and if you make that adjustment to the lead-212 activity for the year when you have both, well, instead of having 0.4, then you've got 0.6 nanocuries of lead-212, and you still have the same issue.

Why doesn't that much lead-212 give a higher boost to the ratio than what turns into 2 milligrams? Am I right? Am I on somebody's speakerphone? I hear myself echoing.

MR. STIVER: I'm hearing the echo too, Stu. This is John.

MR. HINNEFELD: I mean, I can deal

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with it. It's not very loud.

CHAIRMAN CLAWSON: This is Brad. It happens to me too. Actually, I was having a little bit of a hard time hearing the one that just spoke before you, I believe it was Bob Morris or Robert Morris, because he was cutting out, but I think it's just a bad phone connection.

MR. KATZ: This is Ted. I think some people have not muted their phones and that's probably contributing to this background noise. Somehow it's feedback. So everyone who's not speaking, please mute your phones, and press *6 if you don't have a mute button.

MR. STIVER: Okay. This is John Stiver. I'd kind of like to add a little bit to this. Another thing that we had commented on in our previous papers is the milligram thorium data also don't appear to comport with biokinetic processes.

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And what this means, really, is that you've got very high values reported followed, not immediately, but within in a period of time where you should be able to predict, you know, within reason, a follow-on measurement would have been done.

And so we've got some pretty oddball situations where you've got, like, over 10 milligrams followed, about two months later, by 0.2 or it was 0.02. I know Bob Barton could probably give you the exact numbers on that.

So I guess the issue there is that, you know, we have kind of multiple snapshots of evidence here, all of which kind of tend to result in us questioning the veracity of this data set in terms of how it was actually developed, whether it was really done in a way that can be used to create a coworker model.

It would be nice if we had the

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actual count data for these regions and you could kind of pick it apart and say, okay, yes, well, we got more lead counts here and so it's going to mean -- and once you get that, then you can kind of get back to activity and then you can put some bounding assumptions on it, but we're kind of adrift here.

We don't have that hook to get back to a reasonable milligram thorium value and so that's kind of where we are. We're just still struggling with this and I just don't see a reasonable way to put a bound on numbers.

John, we talked about this yesterday. Is there anything you'd like to add? John Mauro.

DR. MAURO: Yes. I'm here listening and yes, we did. My takeaway, and to really down to the bottom, I think, not withstanding some of the problems that were described post-`78, you may very well have a

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tractable situation, because you do have the counts from lead.

But everything I'm hearing regarding '68 to '78 is impossible. And what really brought it home to me was the fact that the calibration source that was used to come up with this equation, I guess we'll call that Max's or Hap West's equation, it's only applicable to Y-12 for that particular calibration source and that particular background.

And it works as long as that, in fact, is what you were dealing with. And what I walked away from with is, but wait a minute, that only worked for that system at Y-12 and that calibration source.

And now you bring this on the road, which is a different shielding, different background, and who knows what the mix is of what people are actually experiencing. It certainly is not going to be

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the mix of radionuclides that were used for the calibration source.

And I walk away from this saying, you cannot reconstruct those doses. You know, I just can't see a way of getting out of this. Now, there may be some folks on this who really have a very deep and rich understanding of these problems, and there may be ways of teasing things apart, and maybe making bounding assumptions, but I haven't heard it, or if I did hear it, I certainly did not understand it.

So right now, you know, in trying to watch this and stay close to it, I can't see a way out of having a tractable dose reconstruction coworker model from '68 to '78.

MEMBER ZIEMER: This is Ziemer, could I ask a question here? Hello.

MR. STIVER: Certainly.

MEMBER ZIEMER: Oh, okay. This is for Mark. Mark, when you talked to Dr. Scott,

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did he indicate, when he said that they used the same procedures, anything about the counting times used for both the Fernald and the Oak Ridge cases?

I mean, there's been a lot of focus on this 3.23 factor which is associated with the background data for the Y-12 group, but, you know, if I were doing this, and I've done a lot of whole body counting work in the past, if I had different shielding and different background, I would just be adjusting my counting times.

I could get the same basic number there by adjusting counting times to get it. Basically, what you're after on these kind of counters is looking at a figure of merit, which, basically, turns out to be sample squared to background ratios.

And if you go from one background to another, you can make that adjustment through counting time, but is there any

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indication that something like that was done?

MR. ROLFES: Dr. Ziemer, this is Mark, and as far as, from my recollection, I believe it was a 1000-second count. This is just from recollection. I'd like for someone from ORAU to --

MEMBER ZIEMER: I mean, is that what was used at both places?

MR. ROLFES: I'll get to that. I apologize. What we do know from Max Scott, we do know that, I can use another example, he had actually run what was the precursor to the mobile in vivo at the Weldon Spring plant to determine thorium exposures for workers in 1966.

And we know that, at that time, he had adjusted due to the higher background of that system that he built. It wasn't the mobile in vivo unit yet per se, it was basically a room that he had constructed which had a much higher background than the Y-12

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mobile counter.

And so in that case, at the Weldon Spring plant, instead of using the actinium-228 and lead-212 photopeaks, he used a thallium-208 photopeak to quantify thorium exposures.

We do know that Max Scott was actually one of the first people to come to Fernald with the mobile in vivo counter that we're discussing today. And we know that background is somehow adjusted for in the counts.

We do know that the background was much lower at Fernald with the mobile in vivo than its precursor at the Weldon Spring plant because they were able to use the actinium-228 and lead-212 photopeaks there.

So we do know that he was involved, and Y-12 was involved, in concerns about elevated background. We know that Max Scott trained the personnel at Fernald to

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operate the mobile in vivo and was also called several times, as he indicated in his interview, to basically troubleshoot and come up to the site, to Fernald, when there were issues with elevated background.

He gave an example of one of the times in his interview that he had come up to the Fernald site due to elevated background.

MEMBER ZIEMER: Okay.

MR. ROLFES: As far as the counting times, I don't know the answer off the top of my head if the count times were the exact same or if they were adjusted based upon the background count rates.

That's something maybe ORAU might be able to answer, but I don't know the exact answer at this point.

MEMBER ZIEMER: Well, if you're going to have a follow-up interview with Max, maybe you could address that. I have more concerns about the other issues that John

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Stiver raised than that 3.23 issue because I think, in principle, you can adjust the system to achieve that same level of -- it sort of looks like a detection limit, but it's, I think as you mentioned, 3.23 times 8.84, I guess; whatever that comes out.

MR. STIVER: This is Stiver, if I could jump in again. That 3.23, you got to remember, that's not based on the counting statistics of the system and it's based on the sum of ratios using this same approach, just taking the ratios of those three photopeaks to the adjacent area, the higher energy, and adding those up.

And also, the count time that I've seen is 1200 seconds, so it's a 20-minute count. I haven't seen any information on what the count times were at Fernald, but, you know, all the calibration information is for Y-12.

So it's not like you're actually,

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you know, taking the background distribution of your counting system so you can count it longer and get more precision in this situation. It's really a --

MEMBER ZIEMER: Well, using the same calibration source in both cases, I assume.

MR. STIVER: Yes, it's a similar construct for that particular set. So if you've got the same counts, unexposed, and the same ROIs, you get 1100 of these people, they sum those up, they get an average value. That's what that 3.23 represents.

MEMBER ZIEMER: Right.

MR. STIVER: It's the average counts for 1100 unexposed people. And the spread in that is 0.7, and that's, then, where the 6 milligram MDA came from.

MEMBER ZIEMER: Yes. So you're talking about the three regions of interest in the background and suggesting then that if the

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background is different, the regions of interest are probably different then.

I understand your point, yes.

DR. LIPSZTEIN: May I clarify something? When you look at those, supposedly, 3.23 is correct. And that dose is the same at Fernald and at Y-12. When we measured the people, the persons, it's 3.23, plus or minus 0.7.

Now, you have those three regions. In order to have the milligrams of thorium, you multiply it by 8.84. This is something that is only related to -- that's special for us that they used at Y-12. And I made the simulation of someone that really had 10 milligrams of thorium in his lung.

Suppose he had 10 milligrams in his lung, and suppose that the force that they were dealing at Fernald had the three separation steps, so what happened is that, all of these three ratios that we see in the

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equation, they will all be around the 3.23.

So the result we would have, minus 3.23, would be between minus 0.7 and plus 0.7, or minus 6/plus 6, if you multiply it by 8.84. I don't know if I'm clear, but the three ratios that we see in the equation would not be different from the normal people, even if this person had 10 milligrams in his lung.

And we would get any number between minus 6 and 6 if we apply the equation. And in reality, he had 10 milligrams, so we cannot determine which error was --

So depending on the mixture you had in the lung, it's impossible to determine what error, was the maximum error on this. Did I make myself clear? I don't know.

MEMBER ZIEMER: Yes. I agree with that. I think that would also happen if you had the same thing at Oak Ridge, though, would it not?

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DR. LIPSZTEIN: Yes. If you had another source, yes, exactly.

MEMBER ZIEMER: If the calibration's different from what you're counting, you're going to have that same issue.

No, I was just raising the issue of whether or not they wouldn't have adjusted this knowing what the background regions of interest were at Fernald versus those at Oak Ridge, and using the standard calibration source, if they wouldn't have been able to adjust for the background part of this.

No, I agree with your other supposition that, in actual situations, if you have very different ratios in an actual person, your calibration is going to not be so useful.

So I'm a little bit like Stu in raising the question, can we bound from this or not? I think that's sort of the question,

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realizing that you can get very different answers depending on what those ratios are. Well, I'm more concerned about those issues than I am the background issue.

I think that part, even as a starting point, could -- but we would need to talk to Max Scott to see how they did that I guess.

DR. MAURO: Paul, this is John. One of the things that crystallized my thinking was reading the minutes of the conversation with Max Scott and it certainly -- oh, Scott Max, it certainly appeared that that equation that we're looking at right now, was what was used with that 8.84 and it really was the 8.84 that tipped me over.

That is, that 8.84, embedded in that is a certain ratio of, I believe it's radium-228 to thorium-232, and also a certain ratio, I guess, of thorium-228 to thorium-232.

There are certain ratios embedded

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in that which reflect a mix of radionuclides in the calibration source that, in effect, was not even necessarily a certain age of thorium, but a thorium source that even had some other contaminations, if that's the right word, of, I want to say, radium-228.

So that was a very unique source. And in my mind, that was not only problematic as applied here in Fernald, but it may very well have been problematic as applied at Y-12 if they did not take into consideration that the actual people that they were measuring had that mix.

And, quite frankly, how far you could be wrong. You're certainly going to get a bad answer if you're, you know, counting everybody on the assumption that that's the mix that these people have.

How far wrong you could be and whether you could somehow find an upper bound, that was my next question, and then I realize

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that, if this was --

MEMBER ZIEMER: Now, John, I basically was asking the same question because my concern was with the 8.84, not what's inside the --

DR. MAURO: Me too. That was my first concern. I agree with you.

MR. STIVER: Yes. The 8.84, is really, that's what allows you to get to a milligram value. That's really your calibration factor and it is completely dependent on the characteristics of that source term that was being employed.

DR. LIPSZTEIN: And the other problem, if I may, is that if someone had inhaled type S thorium in the lung, the radium would leave the lung faster than thorium-232 and would behave more like a type M than a type S.

So if you had a type S material, you wouldn't have even a worse problem. If

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you don't have, you know, the real number, you just know what the sums of the parcels minus 3.23. So it's very difficult to go back.

MEMBER ZIEMER: Well, in fact, if you had type S, and some of that might account for what were described earlier as kind of strange biokinetics, at least if you were looking at these as being insolubles.

MR. STIVER: Well, actually, if it was type S, I think, you would probably just see a continuous amount. It wouldn't be any decrease to speak of in the lung.

DR. LIPSZTEIN: But you were seeing lead and actinium on both sums, and so if the radium leaves, so you see the daughters leaving too, so when you are measuring, this will also, depending on the time after inhaling that you are measuring the person.

MR. HINNEFELD: This is Stu Hinnefeld. I don't really want to get into this issue very much. I don't know that it

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matters that much, but with respect to the differential dissolution in the lung, there has to be, for type S thorium, there has to be some sort of particle size consideration here, because only the radium that is available to the lung fluid would have any different ability to depart.

In other words, some portion of that radium would be within the particle and not available to be treated differently than the thorium particle itself. I mean, I don't know if we need to get into that very far because I don't know that that's a very big issue.

MR. STIVER: That is true, Stu. You're right.

CHAIRMAN CLAWSON: This is Brad speaking. You know, we get into a lot of this stuff all the time. Basically, there is so many uncertainties out there. I'm of the opinion, we've discussed this for how many

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months now, that, you know, it'd really be hard to be able to come to a concluding point on this.

And, Paul, this is where it basically comes down to; as the Work Group, we need to make the decision on this. We can debate this, go back and forth, and we can make a real nice science project out of it, but the bottom line is, that isn't what I feel that we're here to try to do.

I think we've given it a good faith effort and, myself, I think that it just ought to come before the Board at the next Board Meeting and be presented this way.

I don't know about your feelings, Paul. I guess this is what I'm asking you.

MEMBER ZIEMER: What I haven't heard -- you know, we just saw this stuff in the last day, but what we haven't heard, I guess, is whether or not, given the wide range of potential doses, I mean, it has been

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illustrated, depending on those ratios, does that mean you can't bound?

That's N/A, are you saying that you can't plausibly bound? I mean, you've indicated that --

MR. STIVER: That's our position, that you just can't put a plausible bound on these numbers because of the fact that there could be such a huge variability that you can't possibly quantify.

DR. MAURO: And Joyce made a very good point just now that, I was listening to it and she said that, you could actually have a person that you would say is below -- you'd go through this process, you would say he's below the lower limits of detection, and thereby assign one half the 6 milligram, when in fact, he could have had 10 milligrams.

MR. STIVER: Or typically a lot more than 10.

DR. MAURO: Or more.

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MEMBER ZIEMER: How much more?

DR. MAURO: Yes. I can't say.

MEMBER ZIEMER: Well, that's sort of what I'm asking. What could it be and you could still miss it? I mean, is that a way of bounding? See what I'm saying?

MR. HINNEFELD: This is Stu Hinnefeld. I just want to comment on that. Recall, though, that our bounding approach calls for essentially a factor of five multiplier, on the indicated activity, based on lead-212 if you know what the lead-212.

So, in fact, if the person had, I haven't done all the math on this, but 10 milligrams of thorium and being missed. In fact, if we were going to do that factor of five multiplier, it would seem to me, then, that the missed dose would not rely upon the 6 milligram number or half of that, it would rely on five times that factor. If that was really during the day. So taking into

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account, I mean, the fact that we said, well, we'll interpret these data as a triple separation of this worst case. Joyce has started from 10 milligrams and shown where you can't see that, but I don't think we would claim we would. If you say that's 6 milligrams, then it would become 30.

We're talking about issues here that I don't think are the key issues, because the key issue is that lead-212 number.

MR. STIVER: Yes. If you don't have a lead-212 number, you're adrift. You have no way to get back to the true value.

DR. MAURO: Stu, I think our position is that post-'78, when you have the lead-212, and I think that this adjustment factor that you're referring to, and certainly correct me if I'm wrong, that all applies to the post-'78 lead-212 data that's available to us.

But when you go to the '68 to '78,

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correct me if I'm wrong but that's not at play; these adjustment factors, et cetera, or what we have in this equation, and the inherent flaws in the equation, especially regarding the calibration factor that gives you that 8.8, whatever the number is.

See, that's the rock that equation stands on and I don't think that rock is necessarily very good and the effects of that being wrong are not applicable to a given worker at Fernald. I guess the question becomes: how wrong could you be?

And I don't think we've addressed that.

MR. HINNEFELD: Well, actually, and that's the part of your guy's argument that I really can't refute. That's what I tried to say a while ago in different --

DR. MAURO: And, Joyce, you looked at this pretty carefully. I mean, is there a way, you know, that someone could say, well,

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let's assume this, this, and this, it could never be, under any circumstances, higher than some number?

Of course, when you start going that road, you're inventing a set of conditions that may or may not have ever existed, but, you know, everything I've heard is that, you know, it could be a hundred times higher.

Let's go to this triple-separation process that you folks make reference to. My understanding, under those circumstances, if you are counting a person relatively shortly after that process and he had a very large intake, you would not see anything, but he could have an extremely high body burden of thorium.

Now, I haven't heard what those numbers are, but that's a scenario that is not out of the question. Do we have a feel for how high? I mean, that would be a worst

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circumstance, I presume, that is triple-separated, which means you have minimal amount --

DR. LIPSZTEIN: Hello?

DR. MAURO: Yes, Joyce?

DR. LIPSZTEIN: Yes. Okay. Let me --

DR. MAURO: How bad could it get?

DR. LIPSZTEIN: Yes. That's the example that I did, was to take the separation. We accepted that this factor of five is valid when you have the lead-212 measurement result. But when you have the milligrams, I did exactly this example because then you -- even if the person has 10 milligrams in his lung, the results of applying this equation would be any place between minus 6 and 6, or even, I don't know, even this 0.7 that makes it go from minus 6 to 6.

I don't even know if this is 95

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percent of the mean, 99 percent of the mean, 68 percent of the mean. I don't know. There is nothing about this 0.7 error, but it's 3.23, plus or minus 0.7.

So when you have your result and take out 3.23, minus or plus 0.7, and multiply by 8.8 something, you get between minus 6 and 6 with a mean of 0. That's what you would have for a person that would have 10 milligrams.

But maybe it can even go below that, because I don't even know if this 0.7 is 95 percent of the distribution, if it is 68 percent of the distribution, if it is one standard deviation, two standard deviations, three standard deviations, we don't even have this information.

So, you know, you can get from negative numbers to positive numbers because you are subtracting, you know, something that you don't see. It's similar to someone that

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is not exposed. And in reality, he had 10 milligrams.

So you can't apply this five times correction factor when you know the lead-212 result. It doesn't apply here.

MR. HINNEFELD: Yes, Joyce. This is Stu Hinnefeld. I'm not arguing your point at all. Just a clarification, in the, I guess, West paper from '65. It's the paper from 1965. I think it's the West paper. Oh, I'm sorry. It's by Scott in 1965, sorry, 1966, where he gives the tables of the ratios plus or minus 0.67 in the paper, it's described as 95 percent confidence interval. So that'd be the two-sided 95 percent confidence interval on that.

DR. LIPSZTEIN: Okay.

MR. STIVER: Yes. It's the Table 1 on Page 102. That's right.

MR. HINNEFELD: I'm not arguing your point. I just wanted to point that out

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to you.

DR. LIPSZTEIN: Okay.

DR. MAURO: I think that's a question that has been bothering me. Like, in that equation, with the 8.84, I think that was the number, the calibration factor. So there was a source that they put in a phantom that had a certain mix of radionuclides that basically said, you know, that was to have this source.

Now, let's say the source that was put in the calibration was freshly separated thorium without, basically, effectively, any progeny there, except the thorium was 232 and the thorium-228, that would be it. There'd be nothing else there for all intents and purposes.

And you put that in your phantom and it was a large source, you know, some large source, would you see anything in your regions of interest? Would it come back as

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if, no, this guy's at background, and let's say it was a very large source that was put in there.

In other words, it'd be silly, of course, to do that. I'm not saying anyone has done that or should do that, but what I'm saying is: would you see anything in your regions of interest above controls, background?

MR. HINNEFELD: John, this is Stu. It depends on how quickly you count it after you separate it. What will go back in first is, it will look like the lead-212 number. It's actually one of the precursors to lead-212, which is below thorium-228, and has about a three-day half-life.

And so that three-day half-life in-growth of the rest of the chain, then through the lead-212. So that's what you would count. If you count it the day after you got the separation, you probably wouldn't

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see anything, and then the lead-212 would appear to grow in with, I think it's about a three-day half-life.

DR. MAURO: Okay. So within a relatively short period of time, you start to see a delta between your regions of interest because you start to get this in-growth that would be detectable.

So even if you did calibrate with some fresh source, you know, certainly not the minute it came out, but within a week or so, you know, there would be some in-growth.

See, what I'm trying to do is really help see if we could find a way to come out an upper bound, because we do know that people at Fernald could very well, some of them anyway, been exposed to fairly freshly separated material with a minimal in-growth of progeny. That's probably the exception.

Most of it may, especially, you know, have been of some age and I guess I

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really haven't heard an explanation, to me, that I understood that said, you know, reality is, you could have a guy go in and be counted under this technique, and it's between '68 and '78, come back and say: we don't see anything.

Okay. We don't see anything. And, in fact, he could, in fact, have had as much as this. And I don't think it's 6 milligrams. That's my problem. In other words, you know, the 6 milligrams seems to me the one that works for that calibration source.

But, you know, in other words, when I say the calibration, if the person had that mix in him, then you would say, my MDL is 6. But if, in fact, what the guy actually has in him is something that's relatively fresh and a lot different than the calibration source, how bad can it get?

And I think that's what we're all looking for. Is there a way to say, well --

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MEMBER ZIEMER: Well, that's the same question, John. That's the same question I'm asking. What's the worst case that you could miss?

DR. MAURO: Yes, right.

MEMBER ZIEMER: Or is there a plausible bounding based on this sort of great uncertainty? I guess NIOSH is still saying there is? Is that right, Mark or Stu? And SC&A is saying there isn't?

DR. MAURO: That's where it comes down to. I agree with you, Paul. We're fishing away to find that number.

MR. STIVER: Well, I guess, you know, depending on how early you're willing to go. I mean, if you were within a day or two, you would see nothing. If you waited three or four days you'd get one half-life of radium-226 and the short-lived progeny build-in.

You'd start to see a little bit of a bump on the lead-212 peak.

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MEMBER ZIEMER: I guess you're saying, if it's nothing, then you can't bound it.

MR. STIVER: That's just it. So you have, in the extreme case, you have a guy working with freshly separated thorium in the refinery area, gets a snoot full of it, they happen to count him a couple of days later, very unlikely situation, but it could happen, and he might have gotten 50 milligrams and you're going to see nothing.

And then you've got a whole continuum up to the equilibrium situation that --

MEMBER ZIEMER: Well, a couple days later, I think there is, theoretically, a number in the region of interest and it may be lost in noise, but that's sort of the question: how big would that have to be before you could miss it?

MR. ROLFES: This is Mark Rolfes,

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and this is something that we've considered. And we've agreed because if this exposure condition occurred, basically, someone working with thorium and then getting a chest count following that immediately, if they would only have one chest count, this would be an issue.

However, if they had a second one following that, it would be less of an issue. Anyway, to address this concern, we've proposed defaulting to the 50th percentile coworker intake rate for all employees; essentially.

So rather than use the individual's own exposure data as his own data, we've proposed that, collectively, the entire collection of data would be used for any individual potentially close to thorium.

MR. STIVER: But that's your basic, you know, missed dose model, but that really doesn't answer the question of what would be a plausible upper bound.

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DR. MAURO: And, John, let me also weigh in, but that presumes the database upon which you generate your 50 percent is sound. We're saying it isn't. You see the circular argument? You can't use --

MR. HINNEFELD: John, this is Stu and I think I would differ with that.

DR. MAURO: Go ahead.

MR. HINNEFELD: In order for this to be a critical problem with the entire population of results, then all the results would have to be taken within a day or two of the thorium separation. Seems kind of incredible.

If you have, on occasion, that happens to a person, on occasion, then you would have an issue with that person's in vivo count, and so don't give less than this amount to a monitored person.

But in order for the database to be really sullied, so to speak, by that issue

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of accounting immediately after separation, you would have to have a pretty good chunk of the data counted in that fashion.

And, you know, it just seems to be too much coincidence, because remember, the mobile counter was there for campaigns, maybe once or twice a year, and they'd run as many people through as they could.

So to think that you were going to consistently count everybody within a couple days of a thorium separation just doesn't seem credible to me.

DR. MAURO: Yes. No, I understand what you're saying.

MR. ROLFES: Stu, this is Mark once again. And this fact was documented, actually, the concern about the in-growth of progeny. This is documented prior to the arrival of the mobile in vivo radiation measurement laboratory at Fernald.

It was documented in either the

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Hap West article or in the Scott paper, either in 1965 or 1966, so they did consider this before they began counting Fernald workers.

MR. STIVER: I think they understood the problem at the time, yes. It could be a problem.

DR. LIPSZTEIN: May I go back to the equation again, because even if the lead-212 was spilling and you could see something above that bound on the lead-212, you still have to two actinium fix that would be similar to the two actinium fix in the normal population, the non-exposed population, which is embed on this 3.23 here that you are subtracting.

So I don't know if, when you don't have the two parts of the two actinium fix being summed, and subtracting by 3.23, and multiplying by 8.84, if you really get what it was in milligrams in the lung of the person, you know, because they did this when they had

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this equation, this value, when they have the three picks, the two from actinium and the one from lead.

MR. STIVER: I guess the question becomes, how distorted can this be and the worst-case situation, does that still yield a credible, or plausible, upper bound? That's kind of what we're struggling with at this point.

DR. MAURO: This is certainly the toughest brainteaser we've ever had. It's got to be. I have to tell you guys, to try to tease this apart and come to something that is understandable, with a clear path for setting an upper bound, you know, I have to say, I am struggling to try to find a way.

And I try hard to find a way, believe me, and this one has got me, anyways. It's beyond my ability to really tease out and say, well, if you do this, this, and this, and I'm always looking for that, you know, you

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could find an upper bound, whether it's plausible, of course, that becomes your next question.

Well, I haven't even gotten to that place where I could say, I think you could place an upper bound because this equation doesn't apply to Fernald, or necessarily apply to Fernald. It's sort of like this construct that only applies to a very specific circumstance.

How do you take something like that and say, well, we could play with it and find a way to apply it to the worst-case condition that might have occurred at Fernald? I just can't imagine what you can do.

MR. HINNEFELD: I think, in my mind, it's talking about what's the worst-case condition, probably the question has to be that, you know, what (Phone interference) tell us? What milligram number would it spit out? (Phone interference) worst case, or most

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favorable to the claimant.

(Phone interference) mixture --

MR. STIVER: Hey, Stu, you're kind of breaking up. I can't hear you. This is Stiver.

MR. HINNEFELD: I'm sorry. I'll try and get --

MR. STIVER: I don't know if anybody else is having that problem.

MR. HINNEFELD: If we say, okay, here is a worst-case mixture and maybe it's triple freshly separated and then all you have is the lead-212, you know, we're going to give it a few weeks or so to grow in. All you have is the lead-212 and maybe that's the highest dose intake, I don't know that, of the possible mixtures of stuff.

And then, you know, we kind of know what the intake would be in that situation, but do we know what the in vivo monitor would tell us? If we had X amount of

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lead-212, do we know what the in vivo counter would tell us in terms of milligrams of thorium? To me, that's the question.

MR. STIVER: And that's where I get back to the proportionality issue that we were talking about earlier.

MR. HINNEFELD: Yes. I mean, what we've got now is, we've got a data point where if there's no thorium it'll tell you it's 0 if it's right on the -- or some distribution around 0. And if the ratio value for the specific mixture in the calibration source, if it's 1, then it's a little less than 9 milligrams.

But if we only have lead-212, if that is the highest dose intake, and we don't have the additional contribution of the actinium, do we know what the in vivo monitor would tell us?

And then, we have a data set to compare whether, in fact, it seems to tell us

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that, at least we could -- and this is where I get back to the couple years where we had the paired measurements, both actinium and the math.

If, in fact, we believe that it takes so much, like, lead-212 to give us 1 milligram of thorium readout in the mobile counter, and that would be in the absence of any actinium.

And then we have these measurements that have actinium and lead-212 both, and an associated milligram number, then the milligram number in every case should be higher than what we believe the counter would tell us if it only had the amount of lead-212 reported with that milligram number.

It's very hard to say in a comprehensible fashion, but there's a way to test whether the mobile counter gives results that we think it should give in a way. You can't test it completely, but you can prove

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yourself wrong, you can't prove yourself right.

DR. GLOVER: So, Stu, this is Sam and I have done live in vivo. This is, perhaps, a modelable situation. The difficulty would be in determining background, efficiency, and how the counts in these different scenarios can be bottled in the human.

I mean, folks like John Hunt down at Brazil and Kramer, there's lots of human models you can put in the detectors. It has not been done to date, though, and it is not without some complexity.

MR. HINNEFELD: Yes. I don't know, Sam. You know, you're making a science project out of it. I'm not sure the answer --

DR. GLOVER: And I'm saying, the background may be an object that I couldn't overcome.

MR. HINNEFELD: And I'm not sure

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that answers the question. You know, to me, I'm more concerned with what we know about what the counter actually tells us than doing a Monte Carlo calibration; something like that.

DR. GLOVER: By doing that, you're kind of implicitly accepting that the data are really not adequate.

MR. HINNEFELD: Well, my position here is that it's not really straightforward how you deconvolute that thorium-232, or it's that thorium mass number.

How you interpret that into a lead-212 activity and have that interpretation being consistent with the performance of the counter that we observe when we have both the activity measurement and the mass measurement, and how do you interpret that to a lead-212 in order to even start to apply all the bounding factors that we apply?

That's what I've struggled with

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from the start, since I've been getting into this is, is how do I do that? So that's kind of where I'm at. And I think anything going forward, if there is more work to be done going forward, and I think it should be on that issue.

I can have a meeting with the team here, the ORAU and DCAS team working on this. I may refer to some tables and stuff, and explain how this is going to happen, but that's what I'm struggling with.

I believe that, yes, we have a bounding approach when we have the lead-212 number. What I'm having trouble with is, how do I know what the lead-212 number is?

DR. GLOVER: Yes. How do you get from the milligram number based on the ratio method back to a plausible lead-212 number.

MR. HINNEFELD: Right. A bounding lead-212 number would be, you know, theoretically sufficient.

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DR. GLOVER: Yes. What's the worst it could have been given this counting system?

MR. HINNEFELD: And then you check and see, does the counter behave in accordance with that.

MEMBER ZIEMER: This is Ziemer, could you remind me again, is it being proposed that this be used as part of a coworker model or for individual dose reconstructions?

MR. HINNEFELD: Well, for people who have data and their data is above what the -- there will be a coworker model, but for people who have, actually, in vivo data and if their data indicates they would get an intake above what the coworker model would indicate, then they would get an intake based on their own data.

MEMBER ZIEMER: All right, but --

MR. HINNEFELD: People who have

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data that's below what the coworker model is based on will get the coworker model.

MEMBER ZIEMER: In this case, for individuals who have data, this algorithm is what's being proposed and what's being pointed out is that it could, because of the ratios, miss a lot; possibly.

MR. HINNEFELD: Yes, it could miss a lot, but it could -- it's not clear to me there's even a way to get there, you know, knowing what we would miss or not.

MEMBER ZIEMER: Well, I guess that's sort of the question, isn't it?

MR. HINNEFELD: Yes. We could even get from the milligrams to the lead-212. That's easy.

MEMBER ZIEMER: Yes, right.

DR. GLOVER: This is the question we've been debating, now, for a year.

MEMBER ZIEMER: Right. And then if you can't do that, then it means you can't

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bound the dose for every worker.

MR. HINNEFELD: That would seem that way to me.

DR. MAURO: And, Stu, let's say you have a worker where it's reported as 10, 12, whatever, 18, milligrams. It's reported, but it's reported based on this equation.

And if this equation didn't really apply to his mix, you know, his background, and the mix that he had in his body was not the one for which this equation was developed, you know, with the calibration, there's even some question whether or not you can use that reported milligram number for that worker.

I'm not sure, you know, whether or not that worker could very well have been higher or lower than what was reported for him because of the problems inherent with using this equation to derive that number.

MR. HINNEFELD: That's kind of what I'm coming at here, is there a way that,

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you know -- I think that's part of what I'm saying here too is that --

MR. STIVER: Well, you need to get out of the context of the coworker model all together, it'd be just for individual dose reconstruction? And is that number even --

MR. HINNEFELD: I mean, that thorium milligram number is true only for a mixture that is the same as the calibration source.

So the question is, do we know enough about how the calibrational source contributes to the ratio in the three regions of interest so that how much contribution do you get from lead-212? How much contribution to the excess ratio do you get from actinium first and actinium second?

What are the relatives and then, do we know enough to know that? And if we know enough to know that, then we might be able to surmise, you know, what different

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combinations of actinium and lead-212 would contribute to the ratios.

And so at that point, then you would be able to come up with some alternative to that 8.8 whatever number in terms of interpreting, and maybe you wouldn't -- you know, we're kind of going backwards. You know, that 8.84 doesn't mean, really, 8.84 milligrams of thorium, because we don't have the same mixture, but because with this other bounding mixture, the in vivo counter would tell you it was 8.84 when it really was this other mixture of lead-212 and actinium.

And based on that, and some conservative assumptions, then a high end -- I don't know if we can do it or not. I'm telling you that.

MR. STIVER: How many confirmations of those three ROIs could give you a ratio of 1; essentially? Yes. So there's so many unknowns in this and I'm

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trying to grapple with --

MR. HINNEFELD: Yes. There may be too many unknowns for the number of equations. And so I'm not sure that it can be done, but to me, that's the thought is that, if there were a way to take the thorium milligram number, you know, and not automatically interpret it, you know, the fact that the 8.84 only applies for the calibration source is part of what we know.

And so based on that knowledge, is there a way to figure out, if it were the bounding intake, what would 12 milligrams really mean, or 10 milligrams, you know, of thorium? What would that really mean in terms of this bounding intake ratio?

I don't know if we can do it or not. And that's just, you know, at least to this one issue. To me, that is fundamental. If we can't really get to this bounding ratio and what would the mobile counter tell us, you

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know, how would it react to that, and what number do we get out, I don't know that we can do this or not.

But if there's anything else to do, it would be along those lines; I think.

MEMBER ZIEMER: Could I ask one other clarification? The denominators of the region of interest, are those all the valleys, which would be taken as background numbers?

MR. STIVER: No. Those are the adjacent --

MEMBER ZIEMER: Adjacent valleys, right?

MR. HINNEFELD: It's the higher energy adjacent region on the interest and it's --

MR. STIVER: Yes, so it's the adjacent higher energy region.

MR. HINNEFELD: Yes.

(Simultaneous speaking.)

MEMBER ZIEMER: If you put a

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source in, it's the valley, right?

MR. HINNEFELD: Yes. It would be the valley to the high energy side.

MR. STIVER: Yes. It works out that way, yes.

MEMBER ZIEMER: Right. So the enumerator numbers out of the peak energy ranges and does anybody look at the ratios of those, because that tells you something about the mix?

MR. HINNEFELD: Well, if we have them.

MEMBER ZIEMER: Yes. That's what I'm asking.

MR. STIVER: All we have is the milligram number. If you have the counts, you could work your way back.

MEMBER ZIEMER: Yes. You've got the end result of what was plugged into this, in principle, the regions of interest tell you something about the mix.

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MR. STIVER: Yes. That's all you've got. And I think that's kind of where Sam was going if I can be so bold as to --

DR. MAURO: Absolutely, yes. If you knew that.

MR. STIVER: You would try to follow the model of the detectors.

MEMBER ZIEMER: No. I'm asking if that's part of the raw data set.

MR. STIVER: Unfortunately, there is no raw data set for this.

MEMBER ZIEMER: All we have is the output.

MR. STIVER: Milligram number.

MEMBER ZIEMER: Yes. Got you. Okay.

MR. ROLFES: This is Mark. There is some limited raw data, but for the majority of the cases, we do not have the mobile in vivo printout showing the number of counts under each region.

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MEMBER ZIEMER: Each region, because that would, sort of, answer the question pretty fast.

DR. MAURO: Yes.

MR. STIVER: Yes. If we had that, that would be our --

DR. MAURO: We'd be done.

MEMBER ZIEMER: So what we're really talking about is, what's the worst-case ratios? If there's something to be bounded, it would have to be, sort of, what Stu described there.

But I'm wondering, I know that we'd like to come to closure on this, if the NIOSH team would want to take a look at whether they think something like Stu described is feasible or not?

Can you actually get plausible bounding for these individual cases where you're going to reconstruct dose from a person's count?

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MR. STIVER: Given just the milligram value.

MEMBER ZIEMER: Yes. What's the worst it could be or can you not do it?

MR. HINNEFELD: Yes. I think we would owe that. I think if there's going to be further discussion, that's what we would have to provide not just to the Work Group, but if the Work Group wants to, the whole Board for further discussion when it's available.

That's not going to be something that can be done in a day or two, I don't think.

CHAIRMAN CLAWSON: This is Brad. I just wanted to throw out something too, and this is called timeliness. We've been playing with Fernald for how many years now? I think that we need to also put in something to prospective new -- but we owe the petitioners something.

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Gentlemen, I think that, if given enough time, we could battle through all of this stuff, but I want us to all remember that this is a compensation program and we owe it to the petitioners.

If we haven't got it by now, I really think that we have an obligation to be able to bring it before the Board and make this decision. We're talking this ten-year time frame here. We've been battling this for a very, very long time.

And my personal opinion is, in my opinion, this is just mine, time's up. You know, we can battle this and we can go on for years, but that isn't what we're here for. And we're all looking at this and I understand we all want the best, but the bottom-line is what it comes down to is, NIOSH feels that they can, SC&A feels that they can't.

I guess, in my opinion, that this ought to be brought before the Board. I've

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been trying to bring this before the Board for the last two to three meetings. To me, personally, I'll just put it blunt; time's up. Let's proceed forward.

So with that, Paul, I understand you have something to say.

MEMBER ZIEMER: I'm certainly in favor of timeliness. I don't think that the Board doesn't actually have specific guidelines on that. We are charged with looking at whether there's a scientifically appropriate way to do these things.

I guess I'm certainly, Brad, willing that we propose to the Board. If NIOSH is not able to resolve this by the time of the Board Meeting, or, you know -- I think we've got several weeks here. Let's see, what do you we got time-wise?

MR. HINNEFELD: Well, if you're talking about the in-person Board Meeting, that's in --

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MR. STIVER: June 19th, isn't it?

CHAIRMAN CLAWSON: This is Brad. We have scheduled, on the 26th, to discuss Fernald for this time period. And this came from the full Board meeting last time. Basically, the following before that. Go ahead.

MR. KATZ: This is Ted. Yes. So it's on the Board Meeting schedule for next week and, yes, I think the sentiment at the last Board Meeting was that it would be possible to take action on the teleconference, not that it's a forgone conclusion.

But the full Board wanted the Work Group to flesh out its last materials. The full Board also wanted the Work Group to have an opportunity to engage DCAS on the question of, if there were to be a determination of non-feasibility for this period, whether there were any Class Definition issues that need to get sorted out in advance.

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You know, not prejudging the Board's final decision on that matter, but to prepare for that possibility.

MR. HINNEFELD: Yes, Ted, with respect to that issue, we have thought about what's available and we don't believe there is a Class Definition that's suitable, other than all workers.

MR. KATZ: Okay. So that settles that question. That's great.

MEMBER ZIEMER: Could you close my door.

MR. KATZ: What?

CHAIRMAN CLAWSON: Somebody's door is needing closed. So basically, to me, we're at a point where we're going to bring this before the Board next week. NIOSH and SC&A can present to the Board. My feeling, Paul, is that we bring this before the Board on the teleconference.

MEMBER ZIEMER: Oh, yes, okay.

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The Board already decided they wanted to do that and that's what we do, yes.

CHAIRMAN CLAWSON: Right. I understand that. So not wanting to cut anybody off on this rousing discussion that we have here, I think that we need to move on. Is there anything else that we need?

We've got the in vivo bioassay White Paper, the SC&A response, have you discussed that thoroughly, John?

MR. STIVER: Excuse me?

CHAIRMAN CLAWSON: I see on Ted's thing we've got an SC&A response to the in vivo thorium bioassay method. I think that's what we've been discussing.

MR. STIVER: That's what we've been talking about today, yes.

CHAIRMAN CLAWSON: Right. That's two of them right there and DCAS has performed theirs. You know, this bottom-line, to me, this is what we've been talking all day and

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the only thing I see on the agenda is this part of it right now.

Paul, you know, you can weigh in, I don't know why we don't have all the Board Members here, and so forth, but I thought we've already discussed this and we'll just bring this before the Board on the 26th.

MEMBER ZIEMER: And that's fine, you know? See, I detect some uncertainty with NIOSH at the moment in terms of their position, but it may be that they'll have some additional information. It's not very far off, but if they have additional comments, that would be helpful.

I mean, I'm not confident at this point that we have a plausible upper bound, so if we were to vote today, I would have to favor going with a Class, but, you know, it seems that, in principle, one should be able to discover, in terms of some number that, once you pass that number, you could detect

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it, number one, in the in vivo counter.

And that number would then be your upper bound, but we haven't reached that point. So here we are.

MR. KATZ: So this is Ted. Brad, I think I could use a little clarification just for preparing for next week. We have these various materials that have been exchanged, so I would suggest that if this seems right to you, that I'd circulate those materials to the full Board.

And then the other thing, if I could get clarification on is, it seems like it would be helpful for someone to give the Board -- we won't have any form of transcript soon enough.

So it would be helpful if someone could just give a summary presentation of this discussion today to bring the Board up to date beyond the papers that they would receive, because I think the papers, by themselves,

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don't tell enough of the story of today.

And that would be a good way to start the Board, I think, discussion next week.

CHAIRMAN CLAWSON: Ted, I agree wholeheartedly with you on that. I appreciate you offering to send this out to the full Board. Basically, to me, it looks like Mark's already got his preparation to present to the Board put together in his slide show there.

And, to me, John Stiver will just have to bring us up to speed, but what I would offer out to him is that we kind of condense it a little bit and that we have not been able to come to a conclusion at this Work Group meeting, that there's too many uncertainties.

Basically, I'd suggest we allow both sides to air their side of it, and put it before the Board, and be able to proceed on from there.

MR. KATZ: So again, I just want

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to get a little more clarification about this though. I think John's always done a good job at summarizing where we stand, but again --

MR. STIVER: Yes, I'll do it. I can tweak my slides from the last time and, kind of, update them to where we are now and --

MR. KATZ: Right. I think, you know, John, given that, you know, we have these other materials I'll distribute, I think your summary really is to bring them up to date with the discussion today.

And then, you know, certainly Paul and Brad can chime in then with what they've concluded from today at that point, but somewhere in there I guess DCAS needs to have its last words because it's going to go back and think about some of the matters that it tangled with today.

CHAIRMAN CLAWSON: Now, I want to make a clarification on this because we're

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only talking the time frame of '68 to '78, correct?

MR. KATZ: That's correct.

CHAIRMAN CLAWSON: Okay. And my suggestion would be to throw out, you know, we're just looking at this area right now. Fernald is a very, very big site. We've got 27, 28 slides we can go through, but I guess, John, what I'm trying to get to the point of is, we just looked at the '68 to '78, the --

MR. STIVER: Yes. I'll keep it to that; focus to that time frame.

CHAIRMAN CLAWSON: Right.

MR. STIVER: I won't try to get into anything else at this point.

MEMBER ZIEMER: And this is going to really hinge on the thorium bioassay, right?

MR. STIVER: This is all related to the thorium bioassay.

MEMBER ZIEMER: Yes. So I think

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that's the focus. If you get too much beyond that, it's going to be very difficult for the Board Members.

MR. STIVER: I won't try to do that.

CHAIRMAN CLAWSON: And, John, I can easily tell you that I cannot put anything before the Board at this time because, to tell you the truth, I don't got a clue where we're at, other than I don't think we can really do it.

So I appreciate you standing up and taking that. And I believe that we owe it to DCAS to be able to present their side of it and proceed on, but we do need to get this new material, Ted, out to the Board Members, especially Mark's presentation, and so forth.

MR. KATZ: Right. This is Ted. So I'll do that. And, John, if you want, I think it would be good for you to send out something in writing for the full Board. Send

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it to me and I'll distribute it to everyone, also, please, to the petitioner.

And, you know, DCAS, Stu, whether you want to send out some conclusory memo in writing or just speak, but since you're dealing with, you know, very little time, if you wanted to speak to the Board, you know, in real time, I think either would work.

The last thing I just want to remind, we do have Sandra on the line, Brad, and I think it would be good to give Sandra an opportunity in case she has anything she wants to say now to the Work Group.

CHAIRMAN CLAWSON: I agree wholeheartedly. I appreciate that tip.

MS. BALDRIDGE: This is Sandra. It's been interesting. I certainly appreciate everybody's efforts, but, you know, some questions can't be answered and I think we've gotten to that point.

So I appreciate everybody's effort

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on this on behalf of all those included in the petition. And I'd like to thank you.

MR. KATZ: Very good. Thank you, Sandra.

DR. MAURO: This is John. Just one thing because it's plaguing me a bit. Stu, it sounds like when we break, you're going to get together with your crew and say, is there a way we could wrestle this thing to the ground and, you know, you're going to give some thought to that.

I don't know whether you will be able to or whether you will be able to between now and next week, but is it possible that if you folks come up with an ah-ha moment and say, I think we got it. Is there any way, you know, we could hear about it?

A concept, you don't have to solve the whole thing. See, right now, I mean, I haven't heard a strategy that could wrestle this thing to the ground, but if you guys come

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up with something, boy, we'd love to hear it.

MR. HINNEFELD: Yes, John. If we do have an ah-ha moment, we'll let you know.

MR. STIVER: Yes. Maybe we could have a technical call, or something, before the teleconference.

MR. HINNEFELD: Yes. That'll be a little problematic for me, but we'll see what we can do. I know next week is shot. I'm at Lead Team retreat, NIOSH Lead Team retreat next week, so I'll have to be working on this either this weekend or nights if I'm going to do anything on it.

But if we have something by then --

MR. STIVER: Okay. We'll be on the lookout for it.

MR. HINNEFELD: -- we'll clue you guys in. I can step out and get on a phone call while I'm there.

CHAIRMAN CLAWSON: This is Brad

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speaking again. Also, too, I'd kind of like to preview both sides' response before the Work Group meeting so that we're not doing it -- or the Board Meeting.

I'd just like to see where we're at because, hey, I have a very hard time following some of this, but I'd still like to be able to read through it and try to get an understanding of where we're at on this.

So as the Work Group Chair, I would like to be able to see the two responses, at least a day or two before the Board, if possible.

MR. KATZ: Yes, Brad, you'll certainly have it from John Stiver, his summary, I think, which will help you a lot.

CHAIRMAN CLAWSON: Well, I'd also like to see DCAS' too because I'm just trying to get the feel. If anything does change to it, Mark. If it doesn't change, you know, that's fine too. So that I can try to digest

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it.

It's probably not uncommon, but I talked to people that can help me understand it in a more basic form. It's been said, you know, I'm not the most scientific. I'd appreciate that. If something does change, that we're kept notified before the Board Meeting.

MR. HINNEFELD: Yes, we'll give it a shot.

CHAIRMAN CLAWSON: Okay. Thank you, Stu. I don't see anything else on the agenda so is there anything else that needs more to be said or that we need to discuss as a Work Group? I guess, Dr. Ziemer?

MEMBER ZIEMER: No. I think it's time to adjourn.

CHAIRMAN CLAWSON: Thank you. I appreciate that. Is there anybody else on here that has something that they need to say? I appreciate Sandra making her comments. I

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know Ray's on here too, but if there's anybody else that wants to have something clarified or say, I guess, at this time, I'd give that opportunity.

If not hearing anything, then, Ted, I guess I give a motion to adjourn this Work Group meeting.

MR. KATZ: And I think Paul seconds you, so we are adjourned.

CHAIRMAN CLAWSON: I'd like to thank everybody for the time they've spent on this. I know that it's been difficult. Thank you.

(Whereupon, the meeting in the above-entitled matter was concluded at 1:07 p.m.)

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