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CENTERS FOR DISEASE CONTROL

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OCCUPATIONAL SAFETY AND HEALTH

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ADVISORY BOARD ON RADIATION
AND WORKER HEALTH

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FERNALD WORK GROUP

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MONDAY,

JUNE 17, 2013

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

MEMBERS PRESENT:

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

ALSO PRESENT:

TED KATZ, Designated Federal Official
BOB BARTON, SC&A
KATHY BEHLING, SC&A*
ELIZABETH BRACKETT, ORAU Team*
HARRY CHMELYNSKI, SC&A*
STU HINNEFELD, DCAS
KARIN JESSEN, ORAU Team*
KAREN KENT, ORAU Team*
JOSH KINMAN, DCAS contractor
TOM LaBONE, ORAU Team*
JENNY LIN, HHS*
JOYCE LIPSZTEIN, SC&A*
MARK ROLFES, DCAS
MATT SMITH, ORAU Team*
JOHN STIVER, SC&A

*Participating via telephone

TABLE OF CONTENTS

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

(9:00 a.m.)

MR. KATZ: Good morning, everyone. On the line is the Advisory Board of Radiation and Worker Health Fernald Work Group. For notice for people on the line, the agenda for this meeting and some other materials for this meeting are posted on the NIOSH website under the Board Meetings section under today's date. So you can follow along there.

Let's start with roll call. And since we have a specific site, let's speak to conflict of interest as well. And let's begin with Board Members, with the Chair.

(Roll Call.)

MR. KATZ: Brad, it's your agenda.

CHAIRMAN CLAWSON: Okay. Well, first of all, I kind of wanted to recap where we are at on Fernald right now. And, as we sit right now, there has been an SEC. It was

from '68 to '78, correct?

MR. STIVER: Correct.

CHAIRMAN CLAWSON: And that was under thorium?

MR. STIVER: Correct. That was on the in vivo thorium reported as milligrams thorium.

CHAIRMAN CLAWSON: Okay. And then I just want to make sure that we have that kind of in place right there. And what we're looking at is the earlier years right now of construction workers. And I believe it's into SC&A's court to start out and go from there.

MR. STIVER: Okay. This is John Stiver from SC&A. At the last meeting, in March, I know DCAS had 11 different action items that they were tasked to produce for this meeting. And the first six relate to this issue of whether the uranium bioassay coworker model is indeed applicable to the subcontractors employed at Fernald in the

pre-1986 time frame. This is during NLO's, National Lead of Ohio's, tenure before Westinghouse came and took over the M&O contract.

And Stu and Mark had posted several documents related to this. So it might be best if you guys would just kind of lead out and, you know, give us all an overview of what you did and what you feel the conclusions are from that and we can follow up with them.

MR. HINNEFELD: Okay. Well, we had a couple tasks or several tasks related or items related to the applicability as a construction subcontractor or applicability of the coworker model to construction subcontractors, the reason being that until about 1986, not all of the bioassay or subcontractors got placed into the database, the site-wide database, although some of it has been captured in hard form and we do have a smattering of subcontractor bioassay data

over early years. And then starting in about '83 or '84, there is a more consistent pattern of subcontractor data.

So one of the items we were to look at was a specific set of subcontractors from 1969 who were, in fact, for bioassay and look at the exposures that they received. And that is item number 1. There's a summary presented in item number 1 that shows several people involved in that and how their intake rates would relate to the intake associated that the coworker model would assign. And in each of those, for each solubility class, there were intakes for this monitored population that would be higher than the coworker model.

So there is evidence here that there were some -- this set of contractors was exposed more heavily than the coworker model would dictate. And, of course, on the other side of the coin, these contractors were monitored. So Fernald appropriately

recognized that in this case, these contractors would be exposed or were being exposed. And so they were placed on bioassay programs.

So that is a piece of information that may be relevant to our discussion. Its interpretation is not definitive. You can say, well, and one interpretation is that contractors were more heavily exposed than the site worker population. And, therefore, the site worker population model won't hit. That is one interpretation.

The other interpretation of the same data is that when these contractors came in and were going to be heavily exposed, they were monitored. And so the coworker data wouldn't be used for them anyway. So those are essentially the two ways to interpret that data set. And it is hard to -- you know, the evidence that we have been able to gather doesn't provide a definitive, you know,

description of which interpretation is true.

I think it is a fact to know -- you know, something that has kind of flavored my discussion here is that quite often or maybe not at all will we get an exposure history record for a subcontractor prior to sometime in the '80s, '86 or maybe a little before that, because the site as it appears to me did not generally generate an exposure record for subcontractor personnel, even as far as the film badge. In very many cases, subcontractor personnel wore a visitor badge, as opposed to a defined badge with an exposure record, in their name.

So that is one aspect of this that kind of influenced my thinking. But, on the other hand, this one piece of evidence we have of a subcontractor workforce that said we were exposed, they did appropriately monitor them for bioassay.

So that is sort of one of the tasks

that we did and piece of information I think might be relevant to the discussion. But, like I said, the interpretation is not definitive. You don't really know how to interpret that piece of information.

Does anybody want to offer anything beyond that or should I go on with just in general the discussion of subcontractors in general?

MR. STIVER: I would like to say something about that. I am kind of in the same place you are, Stu. This is John Stiver. Because you have, as you say, you have got this long period of time. You ignore the bookend years, like you say, of '51 to '53, when the construction was going on in a pristine radiological environment when they were actually building a site.

And then, evidently, based on another report that we haven't talked about yet Gene Potter put together, it looks like

there may be enough data on the '84-'85 time frame.

So then you have this period from about '53 to '83 where there is, like you say, a smattering of bioassay data for these people. And as you go back in time, it becomes more and more spotty, really. Prior to 1969, there just isn't any at all.

So there is this question, this nagging question, about what do you do about subcontractors in this early period? They are not well-represented. And, I mean, you can't necessarily make the presumption that you have kind of a random group from the same population, some monitored, some aren't, so that you could build the -- you know, just use this coworker model to bracket these exposures for the unmonitored group. So I guess that is my main problem.

We had quite a few technical issues about how some of the assumptions that went

into the dose reconstructions you guys did for these unmonitored and non-claimants, the nine non-claimants, --

MR. HINNEFELD: Right.

MR. STIVER: -- and also for -- you were able to pick out I think the top four from that group who have the hard-copy records in that pre-1986 environment. Evidently there are about 20 claimants in that group, too. And so you are able to pick out the high four or five of those and do reconstructions for them as well. So we have some questions about kind of, you know, whether, really, the proper type of comparison was done between those.

But I think the bottom line is that this little -- you can't really tell the statistical analysis because there is not enough data. It's really more a proof-of-concept study.

You know, given that we have this group from this period of time and we actually

have bioassay data for them, you know, what would the coworker model have provided were they not bioassayed? And then you can make a comparison between the two.

And, as you say, the evidence is that, no, the coworker model probably wouldn't bound most of these people. So you're stuck in this position. What do you do now? It would appear to me that the coworker model really is not applicable to this group of workers in this period of time. It is not too representative of them and certainly not bounding.

So I guess that is where we are at this point for the 10,000-foot view. I mean, certainly we could get into some of the details if we need to.

CHAIRMAN CLAWSON: Well, I was a little -- I was still trying to understand which approach that NIOSH was really hanging their hat on because a lot of this -- you

know, we have gone back and forth in several different revolutions of what we were going to do. And one of my questions was to NIOSH, what approach with the coworkers were we going to take. And if it was a coworker, you know, I wanted to make sure that we justly went in and reviewed it because at one time, we have kind of gone back and forth.

And we have got kind of a course that is kind of a broad spectrum, let me say, what your approach towards it was. And I wanted to pin down exactly the approach that NIOSH was wanting to proceed forward with.

MR. HINNEFELD: Well, with respect to the coworker uranium intake, I think we have been relatively consistent on that. I mean, we have done some examination of subcontractor populations we could find compared to the in-house workforce. But I think we have been fairly consistent in saying that we have a coworker model built from

bioassay data that, until 1986, is only the in-house employees pretty much and what we felt that their exposure experience would be sufficiently representative or bounding of subcontractor exposures who were not monitored, subcontractors who were not monitored. So, I mean, that has pretty much been our approach.

And then most recently, the alteration was we did look at the last three years of that period, so the '83, '84, and '85, I guess, and said, in those 3 years, we have enough bioassay data from subcontractors. We have over 30 people monitored in each year. And that is kind of our number where we will start to look at the feasibility of a coworker population. And we have said that, if desirable, we could use to build or construct a subcontractor coworker model for '83, '84, and '85, but '85, it would be no different. There is really no difference in

'85 between subcontractors and the in-house.

So that is really I think the only variation we have made, is that we could, we believe, construct a construction coworker model for construction workers who were not monitored in '83 and '84.

And then from '85 forward, the coworker model that we had proposed all along would be the model for everybody because everybody's data is in that data set. So, you know, that is I think the only variation we made on this.

Now, later on, we have had several possible approaches when we get into DWE work or the early thorium work. So there are several possible things that are going to take around there, but I think on this one, we have been fairly consistent.

CHAIRMAN CLAWSON: What about on -- this is Brad -- the Type 50 data? This came in and now --

MR. HINNEFELD: Well, the Type 50, there was some confusion about whether Type 50 should be in the database or not. And I think it was sort of a misunderstanding. Type 50 seemed like it was like incident type and it wouldn't be indicative of routine exposures when, in fact, I think a 50 was assigned to anyone who wasn't on a routine bioassay program. And so very often the subcontractor's code was put in as a 50 because he wasn't considered one of the in-house routine monitored people.

CHAIRMAN CLAWSON: I apologize if I misrepresented, Stu, when I was saying what I was. I guess what I was looking at is the information with just Type 50 data. And it's gone out back and forth. I was just looking at some point of the approach of it and what information is going to be used in it. And so that's --

MR. HINNEFELD: I think most of

that has been settled. And I think, really, the question here for the Work Group -- and, you know, I didn't come down here to fight about anything. I came down here to kind of get all of this information out here and kind of moving forward.

And so the question, then, is, given what we know -- and there are some big holes in what we know, but we may want to look at some more stuff later on in terms of the number of contractors, to scan a list of contractors without data. Given what we know, do we feel like construction subcontractors, like in 1969, when there was a job that warranted monitoring, that Fernald was consistently picking people and monitoring those people? So that there just wasn't that much construction work between '54 and '69 that would have occurred in a controlled area.

And so that is why there is not much data from subcontractor-side data or do

we feel like the 1969 incident, where Fernald appropriately monitored the subcontractor, was that the aberration and that someone just happened? You know, a particular person happened to be particularly conscientious who was following that job and made sure those people were monitored; whereas, other jobs, maybe not quite as bad as that one, but other exposed jobs done by contractors, did not get -- you know, they weren't quite as conscientious. And so you have got subcontractors out there unmonitored who may not have exposures represented by the coworker data set. To me, that is really the question that we need to address.

And before we get too far down that path of addressing it, I think there is one file or two files that we posted under item number 4 which are lists of workers, lists of claimants who do not have bioassay data. Now, we have a much longer list of claimants that

was compiled around 2010, and we didn't update that. It is just an extraordinary amount of work. And I didn't think the nature of the claimant population -- there is no reason to believe it would have changed between 2010 and 2013. It would just be an extraordinary amount of work to update that spreadsheet.

So on there, what I did is out of the entire list, which I can also -- I can put the entire unedited list of claimants on item 4 if anybody wants to look at it, but what I did from that entire list of claimants was clipped out the ones who do not have bioassay and put them on the spreadsheet. So there are like 80 names who do not have bioassay. And this includes the job description for those people. So you can make some judgments about that.

And there are certainly -- there are certainly construction trades in there. There are sheet workers. There are asbestos

installers. There are things like that. And there are security police officers, who only worked for a short period of time who are almost surely hired by the prime. Certain people who only worked for a short period of time were almost certainly hired by the prime, but were there for such a short period of time they didn't get a bioassay sample. There was an AEC employee, pretty clearly an AEC employee, on the list.

So we can kind of take a look at those and see what we think about what that informs of. There are a lot of last employment dates around 1954, which would indicate to me they were involved in the construction of the plant because I think they were still building things up until then.

And then it occurs to me that the health and safety buildings; for instance, the health and safety building, was built later than 1954, but it wasn't a lot later. I think

it was the last half of the '50s when they decided they should build a health and safety building, rather than do whatever the health and safety activities or the lab or administration building, where they had been doing them. So there might be some there. I haven't looked at it very thoroughly. But there are some.

The one that comes to mind is a particular person who was an asbestos installer or something like that who worked for a subcontractor and has a long period of time of verified employment. So that would not be someone who was only there for the early construction.

So, I mean, if you would like, it might be worthwhile for us, if we can look at that spreadsheet, just for each person, to kind of look and see if there is something noteworthy about job titles from that.

MR. STIVER: And to kind of follow

on, Bob Barton had done a similar exercise from our end. And they are about the same number of personnel. And he was also able to compile statistics on the average period of employment, the number of workers on a monthly basis at that time and the same type of information on jobs.

Maybe, Bob, you want to talk a little bit?

MR. BARTON: Yes.

CHAIRMAN CLAWSON: Is that one of the things you sent last night?

MR. STIVER: Yes. It was about 10:00 last night. My apologies for that.

MR. BARTON: This is Bob Barton. As John was just alluding to, we kind of today focus review of some claims on NOCTS, you know, specifically targeting construction job types. And you can go into their DOL initial case file. And it will very often tell you whether they are employed by NLO or Legge -- I

am not sure if that is how you pronounce it or any of these other subcontractors that were at the site. And, not surprisingly, they don't have internal dosimetry. We found about just under 50 claimants that are, in effect, subcontractors that would be under consideration for all the discussions concerning application of the coworker model.

And, actually, see, you had something that I think is an important point in that if we could come around and say, well, listen, these 1969 workers, for instance, they were monitored. So you wouldn't even necessarily need the coworker model for those workers if they were monitored.

I mean, you probably need it for unmonitored dose, the last bioassay sample, you would have to apply the coworker model there. But, as I understand it, the data for the 1969 workers was not something that was sent by DOE. It was as a result of an

independent records search that you guys had performed. And you found a lot of books, and you transcribed them yourself.

So I guess, as a practical matter, you know, maybe they did go in and did take samples from these subcontractors who were the highest exposed. And so, you know, you would have records for them.

But when you go to do a DR, if you are not getting those records from DOE and -- you know, are you going to go out and try to find all of the log books that are possibly out there? Because if you are not actually getting their data to do a dose reconstruction on it, well, then you are stuck applying the coworker model.

MR. HINNEFELD: Yes. I understand what you are saying. What you are saying is that this data that we have from 1969 was captured in a data capture. And Mark can probably speak more knowledgeably about how

confident we are of whether we got everything that could possibly exist because your point was maybe Fernald did everything the right way and they were appropriately conscientious every time a contractor came in and was exposed, but they didn't make an exposure record for that person. So we don't get it from DOE.

CHAIRMAN CLAWSON: Right.

MR. HINNEFELD: And our ability to find that depends upon whether we actually captured every possible data point that we could have captured. Now, Mark, can you say more about how to capture the data or maybe somebody from ORAU?

MR. ROLFES: Yes. I recall this has been a few years back that we requested the data. They were on urine sample request cards. Basically an employee would be asked to report for a sample at a certain time.

The results came on maybe

four-by-six-sized cards. We asked for log books for urine sample request cards, anything and everything that we thought would fit the bill for a bioassay sample. To the best of our knowledge, we requested everything we could think of to request, and we received everything that DOE has been able to find.

I don't think there's really too much more, but what we have done with those results, we have linked them in NOCTS. We have a document in our Site Research Database. And each individual claimant who provided a hard copy or a urine sample that is recorded on a hard copy piece of paper, we have linked that document from our Site Research Database into the claim file.

And so when we originally thought we had an unmonitored claimant, it turns out that if you look in the Site Research Database or in this other exposure record, it turns out that who we thought was unmonitored may have

actually been monitored.

But yes. You know, we have no guarantee that we have every piece of data. So that comes back to what we would do with an unmonitored person, a true unmonitored person.

MR. HINNEFELD: So we attempted -- I should have said this, for instance, we didn't get a chronologically complete, in other words, for essentially every workday, a set of urine request cards. We got a pile of urine request cards, but presumably it doesn't cover almost every day of the --

MR. STIVER: Essentially you have kind of a patchwork for those years, but --

MR. HINNEFELD: Right. We don't know.

MR. ROLFES: We didn't have a list, for example, that says we have urine request cards taken on all of these days and we don't have, you know, something to compare what we have in our possession to a master list, --

CHAIRMAN CLAWSON: Right.

MR. ROLFES: -- for example, of all those samples that were ever collected. But, to the best of our knowledge, we requested and, to the best of our knowledge, we received everything that was recorded.

MR. STIVER: So that 940 hard copy records for about 180 individuals over the 9 different years is basically -- that plus the HIS-20 is what we have got to work with.

MR. ROLFES: Yes.

MR. BARTON: That is where we will expect to find more data.

MR. HINNEFELD: Yes. We would not expect to find any more.

MR. STIVER: So to the extent that you could build a coworker model for those earlier years is contingent on that data. And you're telling me and from what I have seen, too, certainly, '84 and '85 look pretty good, '83 possibly. But before that, just in this

kind of gray area where you really don't you have to make -- in order to make any kind of determination, you have got to make some presumptions about how many people were actually monitored, how complete those records might be, whether in earlier years they are not monitored because they didn't have an exposure potential.

Now, personally I find it hard to believe that, you know, during the whole period of operations, where there is equipment being used, wearing out and replaced, there are some capital projects going on, you've got guys, an asbestos worker there for a number of years, sheet metal workers, all these people who would be expected to be highly exposed.

I find it hard to believe that lack of monitoring data for these folks would be indicative of a lack of exposure potential. And so I guess, you know, the problem we have got now is really to identify -- you know, of

all those construction trades and those claimants, to the extent that the claimant list, those 50 or 80 are really fully representative.

You know, you have indicated some, like security guards that are there for a few days and so forth. They wouldn't really fall within this group. Whereas, sheet metal workers, asbestos workers, carpenters, people of that type probably would be.

So maybe it might be good to take a look at that, the spreadsheets, and kind of go through and get an idea of the types of workers that we would be concerned with here.

MR. KATZ: So, Paul, are you online as well as listening?

MEMBER ZIEMER: Yes, I am on the line.

MR. KATZ: So are you able to also look at that spreadsheet?

MEMBER ZIEMER: Yes, I have it

here.

MR. KATZ: Okay. Great, great.

MEMBER ZIEMER: I do have a couple of questions if I could interpose at this point.

MR. KATZ: Yes, by all means.

MEMBER ZIEMER: I don't know if Stu would answer this or one of the others, but were there any policies and procedures in the plant, the subcontractors, in terms of determining formally when bioassay would be required or was it just left up to an individual HP as to whether the folks they were monitoring would have bioassay?

MR. HINNEFELD: I don't think we have found anything about the guidance, you know, some set of procedures or policies that the company adopted.

MEMBER ZIEMER: Okay. So we can't hook into anything other than sort of the implication that perhaps since we have some

folks like these 12 who seem to have high values, that perhaps it was required, but that is no reason to hook into to put them away, then, right?

MR. HINNEFELD: I don't believe we have found anything like a policy from the '50s, '60s, or '70s that would say when subcontractors come in or anybody, you know, here is the policy for monitoring people working at the site. I don't know that we have found anything that would fit that.

MEMBER ZIEMER: Okay.

MR. ROLFES: I can take a look and see, Dr. Ziemer, and get back to the Work Group after this meeting.

MEMBER ZIEMER: Well, if there is no policy and it was left sort of to the individual to make a judgment sort of on the fly, then it creates a little bit of a problem for us, I think, in terms of assuming that there was some consistency. It's a little

more ad hoc.

The other question was, were these 12 contractor individuals or any of them included in the coworker model? Did I understand it was just the regular employees in the coworker model?

MR. HINNEFELD: Yes. The coworker model we built was built from the data in the database. And these 12 people's results were not in the database. And so they would not be in the coworker population that was used or the population that was used in the coworker approach.

MEMBER ZIEMER: And what impact -- I don't have a good feel for -- there are just 12 individuals here. Do we have other subcontractor data, bioassay data, or is it just these 12?

MR. HINNEFELD: Well, in 1969, I don't think we have any other subcontractor data. We have a smattering of subcontractor

bioassay data from a handful of years. I forget exactly how many of years between '52 and '82. There's just a handful of years when we have -- and in any given year, we have certainly fewer than 30 and probably fewer than 12 individuals, you know, subcontractors who were monitored. Do you have that data, Mark?

MR. ROLFES: Yes. I have it pulled up here. For 1969, we have 52 results; 1971, there are 85; 1972, there's 17; 1973, there were only 4; 1981, it's 35 results. Then in 1983, it jumps up to 164; `84 is 275; '85 is 307; '86 is 370.

MR. HINNEFELD: And those are the results.

MR. ROLFES: Correct.

MR. HINNEFELD: That's not the number of monitored people.

MR. ROLFES: Correct.

MEMBER ZIEMER: Right. Did you go

to that random or are these just the 12 highest?

MR. HINNEFELD: The 12 people we selected from 1969 were 12 people that we had bioassay data for, and they I think probably were the highest bioassay data. They were pretty high. That probably was the highest bioassay data we found.

MR. STIVER: If I could step in? This is John Stiver. We picked that group in 1969 because mainly it looked as though they had an acute exposure that wasn't experienced by the corresponding primes. So it was really, they had some high results. It looked like there was an event they were involved in that wasn't experienced by the prime workers. We thought that would be a good subset to look at in terms of comparison because it would appear that the exposures would not have been representatives or -- excuse me -- the prime exposure distribution would not

necessarily be representative of the types of exposures these guys had.

And so they were high, but I think it was also the fact that it was just when compared side by side, you could see they were definitely involved in some other work that was unique to their particular operation.

MEMBER ZIEMER: Well, see that sort of casts a different thing, too, because there wasn't an a priori determination that they needed bioassay.

MR. STIVER: That just hits the point. We don't have any procedures or policies in place that would set up a framework, at least that we know of, that would set the criteria for who is going to be monitored under what conditions. And so it could very well have been one HP on the fly who was cognizant and realized these guys can be decontaminating and pulling out scrap material that is pretty heavily contaminated.

We should probably put them on the bioassay.

Did that happen every time there was a dirty job? We don't know.

MEMBER ZIEMER: Yes. Okay. That's helpful. Thank you.

DR. LIPSZTEIN: May I step in? This is Joyce Lipsztein.

MR. KATZ: Of course, Joyce.

DR. LIPSZTEIN: Okay. We have looked at the claimant files of the four people that were analyzed. And if you look at the claimant files, you go into the claimant files, there isn't really an internal monitoring data like we used with regular workers.

For example, one of the workers was employed from September '69 to December '74. The only thing that there is in his internal monitoring data is some sheets of material of union monitoring from '71 from several workers. That is it, nothing more.

And even his employment from '69 through '74 can only be proved through the affidavit from a colleague of him. So it's not like, you know, the definite proof. Well, that is what he says. He said he was employed from this date to that date. And he has an affidavit from a colleague that will tell I worked with him. And then there is this union monitoring for a lot of workers during some months in '71. So that's it.

So, even for the claimants, we don't have too much information.

MR. STIVER: Joyce, if I could step in again? This is John. What Joyce is talking about is we're not looking right now at the 1969 non-claimants. This is the group of claimants that were also among those 180 or so workers or during that early period from '69 to '85 who had the hard copy records. And these represent the ones that had, I believe, the top four of that group of claimants in

terms of the excretion rate data.

And this particular guy Joyce is alluding to also indicated that he was bioassayed on a regular basis every couple of weeks. Yet, during that four-year period of employment, '69 to '73, there are only records for August of 1971. And so that kind of calls into question, you know, the completeness of his bioassay records. And so it is just another kind of unknown that has to be addressed in one way or another.

Either presumptions have to be made or, you know, we have to kind of possibly consider the fact that this guy has an incomplete record and it's not really indicative of what his true exposures were.

Anything else you would like to say about this particular worker, Joyce? Joyce, are you still there?

DR. LIPSZTEIN: You know, urine results were a lot of work. It's not like his

bioassay. You know, it's a sheet with bioassay for a lot of workers in the same date. And many of them have worked during other months and other times. And this particular one says he worked from '69 to '71 and we just have data from '71, for one month in 71. So it looks like it's incomplete.

I don't have anything to add. You said everything. And in his telephone interview, he says that he collected urine once a week, sometimes twice, every two weeks, and sometimes every three weeks. And we don't have anything. And he worked in '69 also. And he is not among the people that were monitored in '69 in our list.

MR. BARTON: This is Bob Barton. I think that kind of goes along with that earlier point that the only reason we know this guy was monitored, because he happened to be in that list of 939 results that was uncovered with the data capture. I mean, if

you didn't have that, you wouldn't have anything.

And so, I mean, I understand that that probably represents all that we can get, all that is available to us as far as data is, but it sure looks to me like it is not complete. So you are going to have subcontractor claimants that may have been monitored because they may have had a dirty job, but we are never going to know about it because DOE didn't put together exposure records. So we're not going to see that information in a dose reconstruction setting. So we have to apply the coworker model that started this whole thing off.

MR. STIVER: And the coworker model, at least for the groups that we have looked at, the individual does not appear to be bounding. So what do we do? Where are we at this point?

CHAIRMAN CLAWSON: In essence, this

is where we've gotten to. And we're looking at one Class of employees, correct? And this is just construction workers or subcontractors, as they called it?

MR. HINNEFELD: Well, that would be something we would want to define carefully, to go through this list of subcontractors.

I mean, there are a couple of physicians on there. There are some people who look like AEC or DOE employees who probably worked in the administration building. One is called an administrative officer.

We'd have to be careful about how we define this. And then we also have -- for instance, I think there is a sprinkler repairman, who would have a job a lot like a construction subcontractor probably in terms of his work, that may not be caught on a Class Definition that said like construction trades. And it may require some conversation with DOL

about the administration. How can we get this Class administered the way we think it should be administered, what language to write into the Class Definition if we're going to go that way?

CHAIRMAN CLAWSON: Well, I'll be right honest with you. This is one of the struggles that I have been having about -- yes, know, I know it's not our place as a Work Group or anything else like that to set these boundaries, but we have also got to be able to give them something that they can actually implement.

MR. HINNEFELD: Yes. I think it is kind of our job to decide --

CHAIRMAN CLAWSON: Yes.

MR. HINNEFELD: -- where are the boundaries of infeasibility if that is where we are going here. I don't want to presume anything. If that is where we go here, what are the boundaries of infeasibility? And can

we describe them in a fashion that the Department of Labor can then administer it the way we think, you know, in accordance with those boundaries? So that may be difficult yet.

MR. STIVER: Yes. Some of these are pretty obvious that they would be included, but, like you say, I mean, you have got other -- and then others would be clearly excluded, but there is kind of a gray area sometimes. Like you say, an instrument mechanic, he could very well be going into a dirty area and refitting in a very dirty environment.

So the implementation in the Class Definition certainly is going to be tricky.

MR. ROLFES: The other thing I wanted to point out also is that some of these individuals don't have more than 250 days of employment.

MR. HINNEFELD: So by the numbers,

you can only have a few months. One has one day.

MR. ROLFES: Quite a number.

MR. STIVER: Yes, there are. Bob did an analysis on that. What was it, about 60 percent of them are a year?

MR. BARTON: Yes, a rank order.

MR. STIVER: Yes. Yes. I said that's what I said last time. There was a graph that I showed with the curves.

MR. BARTON: There are a fair number of -- and, again, I was doing kind of a focused review with any job that I thought could possibly be subcontractors. I mean, we're not saying everyone here was only on site for a few weeks. I don't want that notion to be out there. There are certainly --

MR. HINNEFELD: There are a number that --

MR. BARTON: There are a number

that --

MR. HINNEFELD: -- were out there for a long time.

MR. BARTON: -- that would not meet the 250-day criteria, but most of them I think -- because, you know, maybe they were only on site for a month or two but then, you know, a couple of years later, they were back on site for a month or two. And you add up all of those time periods and maybe --

MR. KATZ: Well, it doesn't really matter because --

MR. HINNEFELD: Yes. It doesn't matter.

MR. KATZ: -- at the end of the day, that is not an issue for whether you add a Class or not. It doesn't really matter. You don't need to factor that in.

CHAIRMAN CLAWSON: That is what the 250 days is.

MR. KATZ: Well, yes. All I'm

saying is you never know. Some of these people may have worked at another site. That might work out for them if they have another site that is an SEC. But, anyway, it's not really a consideration for whether you add a Class or not.

CHAIRMAN CLAWSON: My question -- and I just wanted to clarify while I have got all of you in here because I'll tell you right now I am proposing a Class where the construction appears from, actually --

MR. KATZ: I'm sorry. Someone on the line hasn't muted their phone. They just said O-R- A-L or something like that. But, anyway, whoever you are, just if you would mute your phone? If you don't have a mute button, if you would press *6, that will mute your phone. Thanks.

CHAIRMAN CLAWSON: I am looking for the construction workers because I want to separate the construction builders of Fernald

from when the facility went hot. It went hot in what, '54?

MR. HINNEFELD: It depends on what you are talking about.

CHAIRMAN CLAWSON: Okay. They were probably doing things in '52 in the pilot plant, I think.

MR. STIVER: '51, actually, they started receiving material.

MR. HINNEFELD: Started receiving material in '51?

MR. ROLFES: I think some of the drum K65 materials were sent.

MR. HINNEFELD: Okay. So the K65 materials I think would have just been dumped in the silos, right?

MR. STIVER: Yes. The pilot plant was doing some experimental work. They were handling the uranium in '51, but the other plants, the refinery and so forth, weren't online until, weren't completed until '53. So

'53 on, you've got the process is in place, materials are being pushed through. And so there's potential for exposure at that point.

But I would say prior to '53, the construction was being performed in essentially a pristine environment outside of the pilot plant. So I think that those bookend years would probably not be appropriate to include. So basically '53 to '83 is what I am seeing at this point based on what the data tells me.

MR. BARTON: I think one important point here, though -- and we're kind of talking construction versus non-construction. We're really talking about subcontractors.

MR. STIVER: We are talking subcontractors.

MR. BARTON: Because the delineation seems to be whether you worked for NLO or not, whether you were involved routinely in the bioassay program would have

that information available. Now, it is going to be mostly construction workers, but there are also NLO construction workers that would have records.

MR. STIVER: Yes. We need to make that distinction, that this is -- the subcontractors are the ones who are not on a routine bioassay. So it wasn't the focus on --

CHAIRMAN CLAWSON: So we are looking at '53 to '83.

MR. HINNEFELD: I think we're starting, really, on '53. And then we should next talk about the end date.

MR. STIVER: Yes. The end dates, what I'm seeing, 1983 is kind of in a transitional period. You've got about 38 individuals, 164 results. Certainly '84 and '85 are, for all intents and purposes, indistinguishable from '86 in terms of the number of personnel in the samples per person.

And, having taken a look at that data, it seems to be pretty well represented throughout the year. '83 seems to be more loaded towards the end of the year, November-December time frame. I don't know if there's much prior to June or July. Bob, have you looked at '83 in detail?

MR. BARTON: No. I think it was later in the year, though.

MR. STIVER: It was kind of more weighted towards the tail end of the year. You kind of see a progression of implementation over time here.

CHAIRMAN CLAWSON: When did Westinghouse actually officially take over after '85?

MR. HINNEFELD: December of '85.

CHAIRMAN CLAWSON: But we saw in the earlier years, the '83 to '85, that the bioassay program starts --

MR. STIVER: You can kind of see it

start to ramp up here in this table here. '83 is kind of in a transitional area. So we might need to look at that in a little bit more detail in terms of the representativeness. A first approximation, I think we would be looking at '53 to '83.

MR. HINNEFELD: If I remember my history, '83 was probably the year that NLO made the papers and there was a lot more focus on the site.

MR. STIVER: Yes. So that --

MR. HINNEFELD: I think that was the year of the Plant 9 dust release. Was that '83?

MR. STIVER: Okay.

MR. HINNEFELD: '82?

CHAIRMAN CLAWSON: I think it was '82, Stu. They were starting to see the results.

MR. HINNEFELD: Yes. I mean, up until the Plant 9 dust release, you know,

became big news -- I'm sorry. I'm speaking from history here, speaking from my conflicted knowledge here, but up until the dust collector release in the early '80s, -- I can't remember what year it was -- Fernald was pretty, you know, unknown. It was pretty anonymous around here. And then when that hit the news, it was all of sudden a big deal and there was a lot more focus, a lot more folks on the site from the Department of Energy as well. And so there was a lot more emphasis.

And so it is not surprising that sometime around there, they started to pay more attention to construction workers and getting them in the bioassay program and things like that. That is kind of consistent with my memory of the history of the place. That was very early in my tenure there.

MR. STIVER: Yes, it seems to me reasonable given the timelines of the activity taking place.

MR. HINNEFELD: Yes. That would have accounted for the increased activity then prior to Westinghouse taking over. And then once Westinghouse took over, a really different regime took place and a different mindset about how to deal with, you know, uranium plants.

MR. BARTON: Looking at the 1983 data, it's almost all in the August to October time frame. So you have three months, really.

MR. STIVER: Yes, yes. So no question there. Yes. Kind of my sense is that it was weighted towards the tail end of the year. I would say, you know, until we really look into it in a little more detail, it's kind of a first approximation. '53 through '83 would be the years to be looking at here for subcontractors.

MR. HINNEFELD: And this would be for subcontractors.

CHAIRMAN CLAWSON: Non-NLO.

MR. STIVER: Not NLO. Basically in subcontractors, you remember NLO was a prime contractor.

CHAIRMAN CLAWSON: Right.

MR. STIVER: So everybody who is a non-NLO employee would be those to be considered.

MR. HINNEFELD: Okay. Does that sit with everybody okay? I mean, you can look down the job categories here. I mean, there is an administrative officer who looks like he worked for the Atomic Energy Commission to --

MR. STIVER: There is a draftsman and designer who was in --

MR. HINNEFELD: I think those were probably NLO.

MR. STIVER: Yes. The rest of them, you go down here, millwrights, ironworkers.

MR. HINNEFELD: Yes. I think I have very few issues with the names on the

list.

MR. STIVER: Yes.

MR. HINNEFELD: But there are one or two that strike me as odd.

MR. STIVER: Spectrographer.

MR. HINNEFELD: That's probably an NLO employee.

MR. BARTON: In my experience, when I was trying to find a sample of subcontractor claimants, I found that that information was usually readily available in the Department of Labor files that they --

MR. HINNEFELD: Yes. You know it was a subcontractor.

MR. STIVER: Yes, yes, right.

MR. HINNEFELD: Labor can tell who is a subcontractor. They have already told us that.

MR. STIVER: Yes. So the issue really isn't that big of a deal. I mean, you've got the people.

MR. HINNEFELD: Yes. We know who the --

MR. STIVER: Yes.

MR. HINNEFELD: And DOL has told us they can tell who the subcontractors are. And it's pretty clear in the claim files. The question is if you make it subcontractors, you are going to put in people -- I mean, the physicians were subcontractors.

MR. STIVER: Clerks.

MR. HINNEFELD: The clerk might be an NLO or might be a subcontractor. There is a person who I'm pretty sure is an AEC employee. There might be another I saw on here that might be an AEC employee who probably worked in the administration building.

MR. KATZ: But those wouldn't be subcontractors, right, AEC employees?

MR. HINNEFELD: No. Well, I guess AEC would not be.

MR. KATZ: Right.

MR. HINNEFELD: These would not be.

MR. KATZ: So you could specify not including physicians, for example. You could actually put that in your definition.

MR. STIVER: You have a --

MR. KATZ: Right? Is there any reason you couldn't exclude physicians?

MS. LIN: Typically we don't do --

MR. KATZ: No, we don't, but, I mean, that would be a relatively easy --

MR. HINNEFELD: Well, if it will make any easier, just leave them in.

CHAIRMAN CLAWSON: I would rather make sure that we capture the people. I understand what you are saying, Ted. But I really don't -- and I apologize. I've got a migraine that's killing me.

MR. KATZ: Do you want some medicine?

CHAIRMAN CLAWSON: I took some.

That's why I apologize if I'm a little bit distant. My head is just throbbing.

But, anyway, my thing was looking at this -- and I dove into a lot of this and, Stu, I'm at the same thing with yours. Looking at some of the people, but I am more worried about the questionable ones, the instrument and this and that because we had a lot of instrument people going into the site. I would much rather push it towards a subcontractor.

MR. STIVER: To be inclusive.

CHAIRMAN CLAWSON: Just to be conclusive. You know, it is going to come down to I think there is still the judgment of it. But myself, I would push for '53 to '83 on non-NLO subcontractors is what I would -- I looked at the non-subcontractor construction workers. And still some of the people that I think you would worry about would still fall into it. So I'd rather be more conclusive

than anything. I just --

MR. HINNEFELD: Yes. I think if you try to get too -- you might leave out people you don't want to leave out.

CHAIRMAN CLAWSON: Right. And that's my biggest worry.

MR. HINNEFELD: You wouldn't want to say, contractors in the construction trade or with construction trade job titles. You wouldn't want to say that.

MR. STIVER: You wouldn't want to exclude anyone unless you were absolutely certain that they --

MR. HINNEFELD: And, to be honest, maybe the physicians took tours in the facility --

MR. STIVER: It could very well be.

MR. HINNEFELD: -- spent time out there.

MR. STIVER: Or secretaries, for example. They could --

MR. HINNEFELD: They could --

MR. STIVER: Yes.

MR. HINNEFELD: They could have been set up out back if they were subcontractors. I don't know.

CHAIRMAN CLAWSON: So I guess my suggestion -- and this comes -- Paul, you are on the line, correct?

MEMBER ZIEMER: I am on the line.

CHAIRMAN CLAWSON: My suggestion is that we push for '53 to '83 for non-NLO subcontractors for an SEC. Any questions that you have on that?

MEMBER ZIEMER: No, not really. I think, you know, we had this problem on many sites. I think of the GE site in Cincinnati. There's just not a way of excluding the people that you know intuitively shouldn't be excluded, but you don't have any way of identifying.

CHAIRMAN CLAWSON: Right.

MEMBER ZIEMER: I mean, we know that there are going to be people -- in a sense, it's sort of unfair -- in the other direction that really shouldn't be there but you don't know how to identify them. So in order to take care of the ones that should be included, you have to err on that side. And you are going to throw in some that are sort of getting a free ride is how I would explain it.

But I think unless somebody can come up with a creative way of filtering these, you will have the same old problem that we had in many sites. We just don't have a way of excluding those that should be.

CHAIRMAN CLAWSON: Paul, I can personally testify to you I have personally left it. This is the only way that I can see to be able to get the people that really are deserving. Yes, sometimes maybe in what you said it could be a free ride, but I would like

to be able to bring this before the Board in the July meeting if there is no problem with that. So I am looking for you and Phil on this.

MEMBER ZIEMER: I would support it.

CHAIRMAN CLAWSON: Okay. I just wanted to make sure that we --

MEMBER ZIEMER: Well, we still have to define that time frame. Is somebody going to go back and do that and clarify? In other words, is it at the time of the contract change or --

CHAIRMAN CLAWSON: Well, it's actually two years before the contract change, but on the paperwork, if you look into it -- and Stu kind of -- there was a release on Fernald which started getting them a lot of credit. I believe it was mid '82. And after that started to happen, we see the bioassays start to ramp up.

The beginning of '84 time period,

we started to see a lot more of these people having bioassays, speaking of contracting personnel. So what I have --

MEMBER ZIEMER: You have to go to the top end of the ramp, I think, when it is fully ramped is where you have to do the cutoff probably.

CHAIRMAN CLAWSON: Well, SC&A -- and I guess I have tasked them. You have already done the workup on this.

MR. STIVER: This is John. As I was saying, we are looking at '83 as kind of a transitional year. And I think Brad and Stu indicated that Plant 9 dust release got a lot of press and probably caused some soul searching or certainly at least an impetus to improve the worker protection and safety program there.

And so I think what you're seeing is in '83 a response to those events. You are seeing an increase in the number of workers

who were monitored and the number of samples in total, but you don't really see it reach a steady state until about 1984.

And also when you look at the data in '83, almost all of it is backloaded to August-December time frame in '83. So you don't really have a good representation for the whole year there. So as kind of a first approximation, what I had recommended to Brad would be '53 to '83 inclusive.

CHAIRMAN CLAWSON: Because in '84, we see a broader spectrum of bioassay and it gives it a better result. So my personal feeling, Paul, was this was when we started to see the better bioassay program. So that is what I am proposing.

But we can check into it deeper if you'd like, but, for all intents and purposes, I was looking inclusive from '53 to '83.

MEMBER ZIEMER: It's through '83.

MR. KATZ: Right. Through '83,

yes.

MEMBER ZIEMER: Yes. It's through '83. And I guess my only question was I think, John Stiver, you said it was sorted or -- there's never a clear-cut cutoff point, I guess. It could be December. It could be November. But, just to be assured, you're saying, okay, let's use January 1st as the -- or December 31st as the cut point. Is that correct?

MR. STIVER: I would say it's probably -- without trying to get too detailed and unwarranted levels of detail, I would say that would be a good choice.

MEMBER ZIEMER: Yes. We are pretty confident that we have reached the top of the ramp at that point. It may have been somewhat sooner, but that would certainly assure that we are at the point. Is that what you are saying?

MR. STIVER: Yes.

MEMBER ZIEMER: I guess can we get some feedback at NIOSH? Are you guys comfortable with that to the extent that NIOSH can be comfortable?

MR. HINNEFELD: Yes. Dr. Ziemer, it has been a long time. I haven't been comfortable since I took this job, but --

MEMBER ZIEMER: Yes. That's why I added that in.

MR. HINNEFELD: I believe that given the information, we want to make sure that we're clear. I think we have just a little bit more work to do, which is to state pretty clearly the bases for concluding this. And I believe it is a basis that would not cause too much heartache in some areas of NIOSH. All decisions for this program have caused some heartache somewhere.

MEMBER ZIEMER: I understand.

MR. HINNEFELD: I think this would not cause too much heartache. And I think if

I can start here, and then people can supplement because I kind of want to talk a little bit about what I believe is being concluded by the Work Group here. And that is that the information that we can obtain on subcontractors -- and we're talking about their internal exposures here -- prior to 1984 is not -- a) it is not complete. We are not confident it is complete.

Were we confident that we could have every piece of data and all of these subcontractors who came in have been monitored -- well, that's not true.

You know, one of the main arguments -- I'm going to start back over my recap here. A big argument is that we have a population of subcontractors who were exposed to work that would not be described by the coworker approach. Their exposures were higher than the coworker approach. Those particular workers, at least the ones in 1969, were

monitored.

It doesn't seem to us that we captured all of the data from subcontractors. We had to rely on data capture to get bioassay information from subcontractors. And there seemed to be a pretty decent likelihood that we didn't capture it all.

If I understood Joyce correctly a while ago, one of our other populations of workers where we had the high five people, where we found the hard copy, we really only found a little bit of data from a guy who worked there for five years. And so that would lend credence to the fact that our data capture did not capture everything that maybe it should have captured.

Given the fact, then, that, at least in some instances, construction workers could have been exposed to having exposure situations that are not described by the coworker and that we don't feel that we can

consistently find the monitoring information necessarily for those people, we just don't feel we have a technique for reconstructing that we can feel confident we can reconstruct in construction worker subcontractors, subcontractors' exposures, internal exposures based on information at hand.

We don't think the coworker model can be counted on because of what we have observed in terms of some construction or subcontractor exposures. And we don't think their own records can be counted on because we don't know that they are complete for people who were exposed. Is that kind of --

MR. STIVER: Yes.

MR. HINNEFELD: There might be more to it than that.

MR. STIVER: I think that is a pretty good recap.

MEMBER SCHOFIELD: I agree. That is a good recap there.

MEMBER ZIEMER: This is Ziemer. Can I ask one more question? Stu, did Fernald use work permits in those days for the subcontractors?

MR. HINNEFELD: Well, I wasn't there in those days, but --

MEMBER ZIEMER: Yes. But do you know whether they did?

MR. HINNEFELD: We don't know a work permit program at that time.

MEMBER ZIEMER: Okay.

MR. HINNEFELD: Later on, there was a work permit program, but I don't know that there was one. I don't think we have seen any evidence of it during the time period we are talking about.

MR. STIVER: Yes. I have seen evidence of it in the late '80s.

MR. HINNEFELD: Okay.

MEMBER ZIEMER: Okay. But if there had been a work permit program, then you would

have a basis for someone making an evaluation; for example, that bioassay was not needed on this job, number one.

Number two, if he completes the job, there would be a verification that, in fact, either based on air sampling or other monitoring that, in fact, it was a good decision, but in reacting to that, you don't have anything to back up the absence of this information.

MR. HINNEFELD: Yes, I don't think we have found any evidence of a work permit program during the period we're talking about.

MEMBER ZIEMER: Okay. Good.

MR. STIVER: Okay. So from here on out, then, Brad, how do you see us proceeding as far as defining any more work than is involved in the time period? Do you want us to take a look at the earlier years a little bit more in depth or are you comfortable with this definition of the --

CHAIRMAN CLAWSON: Myself, I wouldn't go into a great depth to it, but when we present this to the Board, I want to be able to explain to them why we have the start date and the end date and be able to give them a good feeling of why and why we picked these because I know that we have had several other things thrown out.

And one of the things I want to make sure is because, as you guys said in the earlier years, the pilot plants were going. And we did have uranium there, but I don't think that we had the construction workers like we did after the process was up and running. So I think that we are pretty good from that standpoint.

I just would like to be able to make sure that we have done due diligence on it, that we're not starting too early, not start -- myself, I feel good about that '83. I'll be personally honest with that. The '53,

that's where I've got my questions.

MR. STIVER: Yes. That is what I was thinking, too. Is it '53 or '54? Maybe we could do a little bit more research on that aspect.

CHAIRMAN CLAWSON: Let me explain to you my feelings of why because, as I have seen in all of these plants, when they first started getting these pilot plants set up and running, invariably they would find parts in systems that aren't working right and they have to reconstruct them.

I just want to make sure that we get the earlier years. The later years, it shows on paper here where we start to see the ramp-ups. My focus, Phil and Paul, is that we just make sure that the start date is good. And I guess I would ask SC&A to look into that a little bit, not a --

MR. STIVER: We can do a little more in a search. It doesn't need to be a

White Paper or anything but certainly it is kind of a --

CHAIRMAN CLAWSON: Right, to be able to make sure --

MR. STIVER: We have done the due diligence.

CHAIRMAN CLAWSON: -- that that year is there so that when we bring it before the Board, that we can bring a level of comfort to them that we have checked it out and this is why we have gone with these dates.

MR. KATZ: Brad, are you going to have SC&A do a presentation for this?

CHAIRMAN CLAWSON: A short presentation.

MR. KATZ: Yes. And then just, Phil, you need to speak on the record. You have nodded your head but just --

MEMBER SCHOFIELD: No. I am in total agreement --

MR. KATZ: Fine.

MEMBER SCHOFIELD: -- just because the early years, in particular, like Brad says, I have never heard of one of these facilities come online without problems.

MR. KATZ: Yes.

MEMBER SCHOFIELD: So where were these subcontractors? Were they in there helping make those adjustments? You can't find that data.

MR. KATZ: Right. And I only meant, really, speak on the record your support for this motion --

MEMBER SCHOFIELD: Yes.

MR. KATZ: -- from the Work Group. That's all, in general.

MEMBER SCHOFIELD: Yes, I am supportive.

MR. KATZ: Thank you. That's all.

MR. STIVER: Okay.

CHAIRMAN CLAWSON: Okay.

MR. HINNEFELD: Perhaps before we

get to the next topic, can we take a comfort break?

MR. KATZ: Yes.

MR. STIVER: That was my next motion.

(Laughter.)

MR. KATZ: But staff can't make motions.

MR. STIVER: Strongly suggest it.

CHAIRMAN CLAWSON: Let's take a comfort break. What? Ten minutes?

MR. STIVER: Yes. Recharge the caffeine.

MR. KATZ: Okay. All right. So ten minutes. It's about 10:10 right now. So about 10:20, we'll get going again. I'm just going to put the phone on mute.

(Whereupon, the above-entitled matter went off the record at 10:09 a.m. and went back on the record at 10:23 a.m.)

MR. KATZ: We are back on line,

Fernald Work Group.

(Roll call.)

MR. KATZ: Okay.

CHAIRMAN CLAWSON: Okay. Paul, you heard our path forward on this about the earlier years when we left off. Did you hear what we were doing?

MEMBER ZIEMER: Yes, I did.

CHAIRMAN CLAWSON: Okay. And SC&A will get with this before the Board meeting, but tentatively this is what we are pushing unless we see some more information.

MEMBER ZIEMER: Understood.

CHAIRMAN CLAWSON: Okay. John, I'll turn it back over to you for the next issue or NIOSH.

MR. HINNEFELD: Well, I was just going to ask. At the March meeting, I mean, we had essentially three things we were talking about. We were talking about the subcontractor, the one we just did --

CHAIRMAN CLAWSON: Right.

MR. HINNEFELD: -- talking about the early thorium DWE. And we were also talking about the interpretation of the in vivo from '89 or something --

MR. STIVER: '79 to '88.

MR. HINNEFELD: '79 to '88. And we provide a lot of information on '79 to '88. Is there more discussion there? That seemed to me to be kind of solid.

MR. STIVER: Yes. This is a discussion mainly more from a mechanistic standpoint. I think it's a Site Profile-type thing.

MR. HINNEFELD: Okay. So we can leave that until the end.

MR. STIVER: Yes. That is what I was planning to do.

MR. HINNEFELD: All right.

MR. STIVER: We will talk about that towards the end.

But why don't we go ahead now and talk about the thorium coworker model in the early years? This is the 1954 or 1953 to 1967, when the thorium coworker model is dependent on these daily weighted exposures that were taken throughout the plant by the health and safety laboratory. And this has been the ongoing topic, gosh, for about five years.

There are now five different revisions to the coworker model. The latest revision you guys produced in response to some of our concerns at the March meeting resembles very much the previous revision, which was produced in October of 2010. That particular model is the one size fits all-type bounding model.

Let me just kind of back-step to revision 3 for a minute. That particular revision basically assigns the highest DWE, daily weighted exposure, for any given

building for that particular year to all workers, that building and that year combination, with a geometric standard deviation, the GSD, of five.

And the crucial aspects of our acceptance or one of the aspects of it was the fact that the uncertainty in the measurements had been taken into account based on the model relied fairly heavily on an uncertainty analysis produced by Dan Strom at PNL back in 2008. And they looked at this very issue, about six different plants, in the earlier years, '48 to '53, I believe, if I'm not mistaken.

And they factored in just about every type of uncertainty. And there are five different types of uncertainty they looked at. One thing they didn't look at obviously was the representativeness of a particular DWE to other workers. There is no way you can possibly do that. So they looked at the

uncertainty and the variables that actually went into developing the DWE.

And they came out of that with this recommendation that, you know, if you don't actually do your own uncertainty analysis for a set of data, certainly I think the GSDs range from about 4 to 8, 5 being -- I think the 95th percentile was a little over 4. So they recommended a GSD of 5, and that's what NIOSH did.

And so we were okay with that. We said with one caveat that you need to be able to demonstrate that you can indeed place workers in a given plant in a given year. And Bob Barton did a White Paper last fall where he looked at that very issue through all the data. And the long and the short of it, it turns out that no, you can't place workers in a time period in a particular building. There just isn't the data available to do that.

And so NIOSH went back to the

drawing board and came back with another revision. And we have had some problems with it, mainly related to an unwarranted level of granularity that was implied. So it came back out with revision 5.

And revision 5 basically follows revision 3, only with a couple of differences, probably the biggest being that, rather than trying to place workers in a given building, we basically are going to take -- look at all of the DWEs for that year for all the buildings and assign the highest value to all the workers.

So this is a one-size-fits-all model with one fewer degree of freedom than in the revision 3 and again with the same caveat that the GSD of 5 would apply.

Now, there are two aspects to this that we were kind of concerned with. The first one has to do with data completeness. And this is for the period of 1964 to 1967.

Now, if you look at the -- Bob Morris from ORAU had put out a thorium timeline back in like 2008, which was incorporated into revision 3 of the coworker model. I don't know if you have revision 3 available. It was posted along with all the other papers regarding action item 10, which is the DWE model.

But on page 2 of that, you have figure 1, which basically shows which plants were involved in thorium production during the years '54 through '67. And then down on page 12, you have a list of the available DWEs for those particular years and those particular plants. And for '54 all the way up to '63, DWEs are indeed available for all of the plants in which thorium was being processed.

However, for '64 through '67, we have DWE data for Plant 1, which is the sampling plant. They basically prepared samples, ground them up to a uniform particle

size, drumming operations, so forth, things like that, to send on to the refinery for subsequent steps.

And then Plant 8 in 1966, it was one year Plant 8 was involved in thorium production. And there is DWE data for that plant as well. However, for the pilot plant, where a lot of thorium-related activities were taking place, for '64 through '67, there are no data, no DWE data.

And so our concern was, well, you know, you have got an incomplete data set. So how do you know that the exposures in the pilot plant weren't necessarily higher in Plant 1 than Plant 8? And without the data, there is just no way to ascertain that. I mean, you might be able to build a case of, well, you know, based on the process knowledge of what was going on in those plants, we feel that Plant 1 would be bounding.

But when you go back to the thorium

timeline -- I don't know if you have that available. It's figure 1 on page 2. It's a nice little chart here for each of the -- it's a timeline for all the different plants and all the different activities that were taking place. And plant --

MEMBER ZIEMER: Excuse me, John. Hang on. What document are you looking at right now? I want to make sure I'm on the same page.

MR. STIVER: I am now looking at a document called "FMPC Thorium Timeline" by Robert Morris in February of 2008.

MR. HINNEFELD: Yes. The file title, Paul, is "White Paper on FMPC DWE Reports, Rev. 03" from October --

MR. STIVER: Actually, that has summary data, though. I'm trying to --

MR. HINNEFELD: You're looking at a different one.

MR. STIVER: I'm looking at the

source data. Really, you don't have to pull this up right now, but the point being the pilot plant -- there are a lot of activities going on during this time period. There was thorium melting and casting. There was purification, solvent extraction, purification of feed, thorium oxide.

Let's see what else was going on here. Gel production. The thoria-dense production was going on. So there are quite a few things happening in this time frame in the plant, the pilot plant.

And whereas Plant 1, there is a not a lot going on. And basically also based on the interview with a couple of site former employees who were experts in the process that was taking place, it says right here in the annotations that interviews provide a basis for the assumption that relatively small exposure to thorium may have occurred in Plant 1 at any time thorium was produced elsewhere

and had to be safe. And that's borne out by the levels of the DWEs provided in revision 3.

So our concern here is that you have got a lot of activities going on in the pilot plant, one of which, this casting and melting, is known to be a very high, real dirty, dust-intensive process based on what we know about Plant 9 in 1955, which is going to be a topic here in a minute.

So I guess our concern here is that you've got certainly a potential for much larger exposures in the pilot plant, but we've got no data for it. So what do we do at this point? So I throw that out there for discussion.

MR. HINNEFELD: This is Stu. And I have a little trouble keeping straight the various information I have received on this topic, that there is actually some additional data. I was looking at the summary, the Morris, the White Paper, on it. That gives

DWEs by plant. And down toward the bottom of it, it has the DWE by plant from '54 through '67.

And for the pilot plant from '64 through '67, it describes using the 95th percentile of 18 air samples collected apparently in 19--

MR. STIVER: '77.

MR. HINNEFELD: -- 1967.

MR. STIVER: Yes, that's right. '67.

MR. HINNEFELD: Nineteen sixty-seven.

MR. STIVER: Yes. '77 --

MR. HINNEFELD: Yes, '77 being the DWE or the 97th percentile.

MR. STIVER: Yes.

MR. HINNEFELD: Now, there is apparently other thorium information air-sampling data that we have found. There is some thorium air-sampling data from 1964

where there was air sample data taken and there were DWE studies done on two specific jobs which were described in 1964 as being the two jobs that are chronically high-dust jobs.

And so there is a DWE study. And I am hoping someone can point us toward that.

MR. STIVER: Nineteen sixty-four in the pilot plant?

MR. HINNEFELD: Yes. And there were two jobs identified as having chronically high air dust. And there are DWE studies on those two jobs.

MR. ROLFES: Yes. Let's see. Stokes furnace operator I think.

MR. HINNEFELD: That was one of them.

MR. ROLFES: That has been proposed. I'm trying to find the other one. I don't know. Maybe Karin. Karin Jessen, are you on the line and able to help?

MS. JESSEN: Yes. There are air

samples for 1964, '65, and '67 in the pilot plant but no DWE reports.

MR. HINNEFELD: Well, what was described about '64? There were two, like the Stokes furnace operator and something else. There was some sort of study about their exposure, right, from '64?

MS. JESSEN: I believe there was -- I've got like three documents here. So I'm trying to find which one it was. Give me a few minutes if you don't mind.

MR. HINNEFELD: Okay.

MR. ROLFES: Okay. I found an email here. This is out of the Fernald thorium worker location issue, consolidated final revision, from February of 2013. There is a little excerpt that I will read, "The second item to note also concerns the pilot plant. The maximum unweighted air concentration from 1967 was used in Morris 2010. Since it wasn't feasible to combine

weighted and unweighted data, the search of the Site Research Database for additional DWE data was conducted. Ross 1964 is not a full DWE study, but it contains 1964 thorium DWE data for the only two jobs in areas that exceeded the National Lead of Ohio concentration guide at the time of 100 dpm per cubic meter."

And these two jobs, Stokes furnace operator and briquetting operator, were noted as the only ones that consistently generate high air dust levels and that respiratory protection should be worn during these operations. It was also noted that additional ventilation would be required if these operations became routine.

And the Ross 1964 reference is in the Site Research Database. It's Ref ID 42862. And then the value for the Stokes furnace operator was 410 dpm per cubic meter, or about 6 max using the 70 dpm per cubic

meter concentration guidelines. And the briquetting operator was lower value.

Our proposal was to use those values, those two high values for the pilot plant for the years of operation that we don't have DWE data specifically.

MR. BARTON: So it's the maximum, not the 95th percentile?

MR. ROLFES: Well, let's see. These were the two jobs that were sampled that consistently generated high air dust levels. Let's see. It's 1964 thorium DWE data. It doesn't specify whether or not it's the maximum or 95th percentile, but --

MR. STIVER: Is it a DWE or are they just air concentrations, unweighted air concentrations?

MR. BARTON: It sounds like it is just air concentrations.

MR. ROLFES: From what it says here, it says it is not a full DWE study, but

it contains 1964 thorium DWE data for 2 jobs that exceeded the concentration guide at the time.

MR. STIVER: Okay. I guess my question is, is it an unweighted air concentration -- that would be like raw data -- or is it the actual --

MR. ROLFES: Yes. I'll take a look and see if I can pull up the Site Research Database document.

MR. BARTON: And it looks like they have breathing zone and general air for a number of casts, but it's not delineated by the time spent. So they are unweighted air samples.

MR. STIVER: So they are unweighted air samples.

MR. BARTON: Yes.

CHAIRMAN CLAWSON: So is this also an average or is this --

MR. BARTON: They provide a high,

low, and average air concentration, then convert that into the control guide at the time, which I guess was 100 dpm per meter cubed.

I'm looking at the highest activity here was unloading the Stokes furnace. And that had an average value of 41,000 dpm per meter cubed.

MR. STIVER: Did they give you the time allocation or anything?

MR. BARTON: No, there's no time.

MR. STIVER: Sort of like an average, unweighted air concentration data.

MR. BARTON: Right.

MR. STIVER: And then the -- hold on just a second here. And what is the value they came up with, then? It was like the --

MR. ROLFES: For unloading the Stokes furnace BZ, we had 410 times the NLO concentration guideline. It looks like loading the Stokes furnace was 130 times the

national concentration guideline. We've got some general areas that are lower, some more BZs that are lower. The GA was 55 times.

MR. STIVER: But you have breathing zone data for this particular task.

MR. HINNEFELD: Attached at the end of this, there is a DWE worksheet.

MR. STIVER: Let me see. What's the name of the SRDB again?

MR. HINNEFELD: The SRDB reference is 42862.

MR. STIVER: 42862. Let me pull that up.

MR. BARTON: You're right. There is some. They do break it out by time at the end.

And, just looking at the breakout on some of these, the higher concentration jobs, it looks like they were doing it for a few minutes a day. You have 4.1 times the national concentration guide as the final

daily weighted exposure for that worker.

MR. STIVER: And the concentration guide, is this an equivalent to a MAC or is a different level?

MR. BARTON: It's 100 instead of 70. So it looks like you do have --

MR. STIVER: Okay.

MR. HINNEFELD: So if you use the old MAC number --

MR. STIVER: Yes. That's 6. Yes. And this is from 1964.

CHAIRMAN CLAWSON: Stu, did you say NAC?

MR. HINNEFELD: MAC, maximum allowable concentration.

CHAIRMAN CLAWSON: Okay. I just thought I heard you say NAC. And I was sitting there.

MR. HINNEFELD: No. That's something else.

MR. STIVER: Okay. So it looks

like you do have some useable data here, '64, '67, I'm a little more concerned with because you only have those 18 samples. And this is basically the 95th percentile with the unweighted average, a little more concerned with that. It looks like '64, you have got something that you could make a good case for it being a reasonable high-end exposure, certainly higher than what you would have gotten from --

MR. BARTON: Based on that paper that was prepared in February, it looks like what the proposed approach is is to use that '64 data for all the way through '67.

MR. STIVER: Yes.

MR. BARTON: I'm looking at table 1 in Fernald thorium worker location issue consolidated draft. And they indicate which years for which plants they would be using. And it's for the pilot plant. It would be that 1964 data. Of course, I mean, now the

approach is whatever the highest plant was in that year. So, I mean, that might not be what ends up being used.

MR. STIVER: Well, actually, when you look at the only year that it wouldn't be would be '66, where you have a 7.1 MAC from Plant 8.

MR. BARTON: Okay.

MR. STIVER: So let come back to this one. So basically you would be looking at 4.1 MAC, whatever the -- accounting for the difference in the 170. So you would be a little over 5, close to 5 for the Stokes furnace job.

MR. BARTON: I guess one of the things maybe you have some evidence to establish is, you know, you have those high-risk jobs in 1964 and you want some assurance that it can apply that to the years after that, that the versions hadn't noticed to be changed.

MR. STIVER: Yes. I guess that is true. I was not really comfortable buying off on a plant after all of those years until we had a little bit better understanding of whether that would have applied because we certainly have, you know, the melting and casting. I was thinking the Stokes furnace might be related to that during those years.

MR. HINNEFELD: I don't remember what the Stokes furnace was on the plant.

MR. STIVER: In looking at the other activities that are going on, I wouldn't be involved in the solvent extraction, obviously, or purifications, all the chemical extraction processes.

MR. BARTON: I mean, you always want to use a daily weighted exposure just to --

MR. STIVER: Oh, certainly, yes.

MR. BARTON: -- sample measurements.

MR. STIVER: Yes.

MR. BARTON: But at the same time, if you look at the older drafts, what was assumed before was that 1967, based on the 18 samples, --

MR. STIVER: Yes, but I've got a problem with that.

MR. BARTON: -- was 77 MAC. And now we're talking about like six MAC.

MR. STIVER: Well, 77 I always thought was kind of a --

MR. BARTON: Yes.

MR. STIVER: -- tenuous number to begin with. It's based on such sparse data. That was one of the problems I had with it.

I would tend to believe that for '64, you are okay. I am not quite so sure about '65 through '67, though, just extrapolating that across the board without real data.

Like I say, you know, for every

other year, you've got an individual map that would apply. That is the only time, that three-year period where you are hanging it all on one job from 1964. And I guess if you make the assumption that the work environment was essentially unchanged for the subsequent three years, then that would hold, but --

MR. ROLFES: I just wanted to point out also it is the highest job that we --

MR. STIVER: Right. It is highest, which is inconsistent with your approach.

CHAIRMAN CLAWSON: Mark, could you explain to me what the Stokes furnace was in comparison to this encapsulating? I guess this is where I want to understand the process, but what was it? How did it play into these other parts of it?

MR. ROLFES: If you're asking what a Stokes furnace is, I don't know what it is.

CHAIRMAN CLAWSON: Well, yes. I guess the part that got to me was the

encapsulation process, the casting, and so forth like that. And what I was wondering is where they just call it the Stokes furnace, I was wondering if that is what that went into previously before the castings or --

MR. STIVER: This isn't related to the dirtiest job. This isn't what that was concerned with.

MR. HINNEFELD: I don't know if anybody on the phone knows the answer to that, the Stokes furnace, what part of the operation the Stokes furnace was used in. I am not hearing anything. I guess not.

MS. JESSEN: I don't know what the Stokes furnace is.

MR. STIVER: I think we can infer it's related to the melting and casting operation. Apparently what aspect, is there another --

MR. HINNEFELD: Yes, furnaces were used for production, the reduction process.

They are used for melting and recasting. And they are used for oxidation. Usually when you are reclaiming something to oxide it to get in good material, good --

MR. STIVER: Right.

MR. HINNEFELD: So those are generally the areas where you're going to use a furnace. And I don't know what the Stokes furnace, which -- that's probably a manufacturer. So I don't know what it was.

MR. STIVER: We can kind of infer from the job description separating the ingot from the mold, that would certainly involve the melting and recasting.

MR. HINNEFELD: Yes.

MR. STIVER: I don't believe there was any oxidation going on, at least according -- let's see.

MR. ROLFES: Yes. Some of the subsequent operations that were sampled in this paper --

MR. STIVER: Yes.

MR. ROLFES: -- refer to removing the mold from the furnace, placing it in a cooling booth, loading crucibles with thorium powder, metal, and brick head.

MR. HINNEFELD: That's melt. Yes. That's the melt part.

MR. STIVER: Yes. That would be the melting aspect of it.

MR. ROLFES: Crucible loading.

MR. STIVER: Where are you finding that, Mark?

MR. ROLFES: If you look within that Site Research Database document, this is on page 4 of 8. I was just looking at some of the subsequent operations and locations that were sampled following those first two, the high values. It's just further down.

MR. STIVER: Okay. I see.

MR. BARTON: That's the job there.

MEMBER SCHOFIELD: Do we know what

the capacity of these furnaces was? Any idea?

MR. HINNEFELD: Capacity in what sense?

MR. BARTON: When they are loading or unloading. Are we talking about 1 or 2 kilograms or are we talking about 10 or 20 kilograms loads?

MR. HINNEFELD: I don't know how they cast the thorium, if it was uranium casting was, you know, what, several hundred kilograms, I believe, maybe more. I don't know about the thorium.

MR. STIVER: According to this thorium timeline, we're looking at about 30 tons being processed.

MR. KATZ: By the way, I just Googled Stokes furnace and went around that and found Rufus Stokes. It's an air purification system he invented for furnaces. So I don't know if that tells you much about what kind of furnace it was, but --

MR. STIVER: Yes. It looks like they're definitely involved in what I was concerned with, which is the dirtiest job, casting. Now, when we look at the Plant 9, that what we will talk about with Ted, but we had the really, really high MACs. And almost every one of the highest five MACs from Plant 9 in 1955, the highest values involve some aspect of cleaning crucibles or some aspect regarding this recasting and melting operation.

That's why it just kind of jumped off at me for the pilot plant when I saw that. It's like, wait a second. Here is your dirtiest job that you've got. And it looks like the Stokes furnace DWE captures that. Certainly that is my first impression looking over this for 1964.

Now, this begs the question, of course, given the fact that the Plant 9 was so much higher for similar operations. Could we

be missing something for '65 through '67? The operation was essentially going on that entire time.

So is the '64 DWE really representative of the subsequent years or could it have been higher? I guess that's the thing that is kind of nagging at the back of my mind now.

MR. ROLFES: For Plant 9, are you referring to like 1954 and '55?

MR. STIVER: Nineteen fifty-five is where you had the highest MACs recorded.

MR. ROLFES: That was also when they produced like 33 percent of the total thorium --

MR. STIVER: Oh, yes.

MR. ROLFES: -- produced, too.

MR. STIVER: Right. But, you know, the fact that at any given rung, it was going to be comparable, I would think, you know, the potential for high-dose loads in some aspect

of that job for a particular crucible wouldn't be any different. You have a total throughput for that facility unless you were to scale everything down, which I doubt was the case. So it becomes the issue of, can we really hang our hats on 1964 data for the subsequent there years or not? I don't know.

MR. HINNEFELD: Well, I can find the '67. I think the '67 data is in SRDB-2280.

MR. STIVER: Yes, yes.

MR. HINNEFELD: And it is intermixed with uranium --

MR. STIVER: Yes.

MR. HINNEFELD: -- data set. You've got to be careful if you're looking at thorium when you look through those.

MR. STIVER: Yes.

MR. HINNEFELD: There is a lot of information on some of these. It almost looks like you can build, do a DWE for some jobs

that were monitored in '67. I mean, they haven't drawn out, they haven't done the DWE calculation themselves, but it looks like they have taken the samples that would allow a DWE calculation.

They talk about certain high-dose concentrations in their duration numbers, which I believe it was the sample duration because they took the sample for the duration of that activity.

MR. STIVER: I remember looking at that back in 2009 and trying to go back and recall 4 or 5 years ago. But yes. I looked at some of that data when I first started here at SC&A.

You know, at this point, I am not comfortable buying off on it. I certainly want to look at that '67 data in a little bit more detail. I realize this would have to be something we could do for a couple of days, you know, in a reasonably short time

turnaround time.

MR. HINNEFELD: What we should probably do on our side, Mark, you read an email or a report. You read from a report that said there were data from '64, '65, and '67. Is that right?

MR. STIVER: Yes.

MR. HINNEFELD: The only DWE study being the sort of informal one that was done in '64 or not really informal. They did the DWE calculation time. So it would seem incumbent that we collect all of that data, if possible the data sheets, as well but collect the data on like a spreadsheet of some sort so we can convey what information we have concisely.

You know, the SRDB references are handy to go back and look at, but you would really like to have the information from the data sheet transferred onto the spreadsheet. You can see it all handily in one place, that

here was the data taken, this was the description of the activity, this is what they collected in terms of sample time, particularly on these DWEs because they tended to sample the operations. So the sample time was also the time of the operation.

MR. STIVER: Right.

MR. HINNEFELD: What you may not get is how many times was the operation done in a day. If you're just looking at air sample sheets, you may not get that.

MR. STIVER: Right. You don't get the weighting.

MR. HINNEFELD: And you may not know how much time. They have got some GA samples. You would have to deduce what time to apply the GA sample, depending on how many times they did that, the BZ monitored activity. So there might be some things you can put together. And I would think that that can be done in a relatively short period --

MR. STIVER: We would certainly like to be able to make some definitive judgments in advance of the Board meeting.

MR. HINNEFELD: Yes, yes. We'd need to do something in --

MR. STIVER: In the teleconference call or whatever.

MR. HINNEFELD: -- or would we need to thrash it out at the Board meeting?

MR. KATZ: We can have another. We can have a teleconference meeting of this Work Group. There is time for that. So that is not a problem.

MR. BARTON: You said that reference number was 2280?

MR. HINNEFELD: I probably said it wrong. 2280. It's an analytical data sheet.

MR. BARTON: Yes. Those are actually dated 1977.

MR. HINNEFELD: '77? Oh, my god, you're right. We're talking about '67, aren't

we?

MR. BARTON: Yes.

MR. HINNEFELD: Oh, my bad. My bad.

MR. STIVER: It's only a factor of ten.

(Laughter.)

MR. STIVER: It's a small fraction.

MR. HINNEFELD: Well --

MR. STIVER: I know that --

MR. HINNEFELD: -- it's 70. That's all that was important. I was going to say ten years is not that important. Between '50 and '60, it is. Between '60 and '70, it is. I'm sorry. You are right.

CHAIRMAN CLAWSON: Looking at this --

MR. HINNEFELD: Either way, we need to compile the data from '67 and --

MR. STIVER: Yes, see what you can pull up. It sounds like there may be

something for '65. '66 is questionable. And then there is some for '67. So if we can see that data.

MR. HINNEFELD: It would be handy to be able to refer to the SRDB figures so people can look back.

MR. STIVER: Right.

MR. HINNEFELD: It feels better to see the SRDB reference. You know, it may not tell you any more information, but it feels better.

MR. STIVER: Yes.

MR. HINNEFELD: And then -- okay.

CHAIRMAN CLAWSON: Because the dates that I was actually worried about, Stu, are like from the '64 to the '67 era, right through there. It wasn't clear. And it looked like, to me, that there were some outstanding questions on how we would do that, especially with that higher data.

MR. HINNEFELD: And Morris Rev 3

proposed -- you know, they put in that cable, the 95th percentile. That would be air data, the 18 air samples data in 1967, quite a high number.

MR. STIVER: Yes. Where we left that, I have some concerns about that and also about that 686 MAC in '55 from Morris Rev 3. But we kind of tabled that so we could determine whether it was even implementable, you know. So I guess we are kind of picking that up again now.

MR. HINNEFELD: Yes. Okay.

CHAIRMAN CLAWSON: Help me understand. Because in my short knowledge of this, on this DWE data that we're going through, they're going to use that for everyone.

MR. STIVER: If it's a high DWE for that year, everybody gets it because there's no way you can parse people out by where they might have been in the situation. So it's a

one-size-fits-all model. It's kind of analogous to what we wound up with for the recycled uranium where everybody got 100 parts per billion plutonium, couldn't determine who was doing the jobs.

CHAIRMAN CLAWSON: Those heavier -- I can't remember what year it is. My mind is not working too well right now. But, anyway, the heavier data, we had some very high set-points that to me seemed very, very high. That was '67, I believe.

MR. STIVER: '67 data isn't a DWE. Basically it's a fit to 18 unweighted air samples at the 95th percentile. And this is kind of similar to what is being proposed in Revision 3 for 1955. It's Davis and Strom in their report. And there's a passage in there. I don't remember the exact words. It's to the effect that if you don't have DWE data but you do have air-sampling data, the 95th percentile and the unweighted air-sampling

data, while probably higher than what anybody would have gotten, would be certainly a bounding number. And so that's kind of the basis that underlies that approach here.

Back in 2010, I thought that was still kind of a weak number to be applying, you know, past a four-year period because it was based on some pretty sparse data. But now it looks like you certainly have a DWE that's a representative for 1964, the Stokes furnace DWE. '65 and '66 we don't know. There may be something in '65. '67, we need to look at the data, see if there indeed are only 18 samples and what those samples represent.

CHAIRMAN CLAWSON: My question on that, too, are they an average or --

MR. STIVER: The 1967 data, this is basically a 95th percent of a bunch of unweighted air samples. There's no time-weighting associated with those values. So they're very high. They are very high. So

it's a big number.

CHAIRMAN CLAWSON: And I realize -- because to me, basically, we're getting into the aspect of could somebody plausibly --

MR. STIVER: Well, that's a nice segue to the 1955 issue. Before we get there, though, I guess what I would like to do, I guess, so, you guys, NIOSH's side, you're going to get everything together that you can on the '64 to '67 --

MR. HINNEFELD: Right. The data that's described in that, we will get that and --

MR. STIVER: Right. If you get that posted, we can look at it.

MR. HINNEFELD: -- and also the SRDB references.

MR. STIVER: Right, and the references that go with it. And then we can look at that and then have a teleconference call before the Board meeting and decide how

we're going to go on those four years, whether we're in agreement or not. And then we'll bring the issue up, discuss it at the Board meeting, but we have all got to be on the same page, or at least have our position staked out.

CHAIRMAN CLAWSON: Because '64 to '67 is my issue. I want to discuss this with you in detail before I go on that. So the path forward, we're going to have NIOSH deliver a spreadsheet and everything they've got on that because some of this kind of -- I haven't seen. So I apologize. I didn't know it was out there. That's my fault for not reviewing that. We're going to have to have a Work Group call.

MR. KATZ: Teleconference.

MR. STIVER: Another teleconference call, I don't know, in a week or so.

MR. HINNEFELD: You might want to go more than a week.

MR. STIVER: So what have we got? We have got one month.

MR. HINNEFELD: We've got a month before the Board meeting.

MR. KATZ: Yes, and the best is two weeks. We'll just have to find a date that works, too. So it may be two weeks. It may be longer than two weeks depending on when people are available.

MR. HINNEFELD: Okay.

MR. KATZ: Right now, the only time period I'm quite certain about is the week right before the Board meeting, I know there is still quite a bit of availability for a Work Group. In general, it's nicer to do it earlier because then we can get presentations ready and so on earlier. So we'll shoot for, if you want to shoot for, two weeks from now.

MR. STIVER: Yes, that would give us plenty of time. In a day or two, we'll have a -- or once we see the data, we'll have

a better handle on where we are.

MR. KATZ: Right.

MR. HINNEFELD: It would be the week of July 4th. We're only off on the 4th.

MR. KATZ: And my availability is good that week. It's just --

MR. HINNEFELD: The week after there is a civic society meeting, that might affect John and some people. It might affect some ORAU folks.

MR. STIVER: Yes, yes.

MR. HINNEFELD: It won't affect Mark or me.

CHAIRMAN CLAWSON: And July 8th through the 16th or 15th, I'm not available at all.

MR. STIVER: That is HPS, though.

MR. KATZ: Right.

MR. HINNEFELD: Yes. That is the HPS meeting.

MR. KATZ: So why don't people look

at the calendars now while we're on the topic? The week of July 4th, is that enough time, Mark and Stu, to get a spreadsheet together?

MR. ROLFES: I would think so.

MR. HINNEFELD: I would think so.

MR. KATZ: Okay. And giving SC&A a couple of days to be able to look at it and make sense. So the July 4th is off, obviously, but I have good availability then. It's up to all of you and all on the phone, too.

MR. STIVER: I could be there any day. It doesn't much matter to me.

CHAIRMAN CLAWSON: With the holiday falling on the weekend, I don't want to get into that. I'd prefer to do it the first of the week.

MR. STIVER: How about Tuesday, the 2nd?

MR. KATZ: So Paul, how is July 2nd for you for a teleconference? It wouldn't be

a very long teleconference. This is just one issue, I think, unless we end up having --

MR. HINNEFELD: Well, unless we have other --

MR. KATZ: Right.

MR. HINNEFELD: We're not done today. We may be able to wrap up the rest of them.

MEMBER ZIEMER: July 2nd, did you say?

MR. KATZ: Yes. Paul, how is that for you?

MEMBER ZIEMER: I think that will be all right. I don't know for sure because -- well, let's just say at the moment it looks okay.

MR. KATZ: Okay. Well, I guess let me ask you this.

MEMBER ZIEMER: It's a little crazy right now.

MR. KATZ: Yes. I know. Is July

3rd better or same difference?

MEMBER ZIEMER: No, no. The 2nd will be better, I think.

MR. KATZ: Okay. And p.m., a.m.? Do people have a preference.

CHAIRMAN CLAWSON: Earlier in the morning. Actually, what is July 1st? That's a Monday.

MR. KATZ: That's a Monday.

CHAIRMAN CLAWSON: Yes. I'm trying to schedule this around my days off.

MR. KATZ: Right.

CHAIRMAN CLAWSON: And that would -- July 1st would actually work better for me, because July 2nd, I'm actually supposed to be back for work. But --

MR. STIVER: I was going to say it doesn't matter to me. Whatever you guys --

MR. KATZ: Does that still work for you guys?

MR. HINNEFELD: Yes.

MR. KATZ: July 1st? Okay. So what about July 1st, Paul? Is the better or worse than the 2nd?

MEMBER ZIEMER: July 1st works for me.

MR. KATZ: July 1?

CHAIRMAN CLAWSON: That's a Monday, Paul.

MEMBER ZIEMER: That's okay for me.

MR. KATZ: Okay. So let me just see what I have on my schedule. Yes. I have something I can move. I can do away with things in the way. So July 1st a.m., you're saying is better?

CHAIRMAN CLAWSON: A.m., in the morning would be better.

MR. KATZ: So you're out West. What time your time is --

CHAIRMAN CLAWSON: Whatever time you guys -- I'm used to getting up at 5:00 to go to work. So --

MR. KATZ: Okay.

CHAIRMAN CLAWSON: -- we're good on that.

MR. KATZ: Nine a.m.? Does that work for everybody? Nine a.m. Eastern time on July 1st?

CHAIRMAN CLAWSON: Let's do it.

MEMBER ZIEMER: Okay.

MR. KATZ: Okay. Nine a.m. July 1 teleconference. And it probably won't last that long unless we have a lot of other issues. Okay.

CHAIRMAN CLAWSON: I want to be clear on this. Are we looking at the '64 to '67 data or actually more --

MR. STIVER: This is the '64 to '67 data for the pilot plant. This is pretty focused.

CHAIRMAN CLAWSON: Okay. Good.

MR. KATZ: Paul?

MEMBER ZIEMER: The question raised

about what a Stokes furnace was, did somebody answer that?

MR. KATZ: Well, I just looked up Stokes and saw that he had invented an air pollution device that works pretty well for furnaces. That was one of the things.

MEMBER ZIEMER: The Stokes Corporation came up with a furnace for plutonium and uranium melting. It was a vacuum furnace.

MR. STIVER: Induction furnace.

MEMBER ZIEMER: Yes. And that was used, I think, for casting and vacuum melting and those kinds of things.

MR. STIVER: That's exactly what we --

MR. KATZ: So that is really helpful.

MR. STIVER: Yes.

MR. KATZ: That answers the question, then.

MR. HINNEFELD: That's consistent with the job description this morning.

MR. KATZ: Yes.

MEMBER ZIEMER: Yes, I think it was initially proposed for plutonium work.

MS. JESSEN: Just so you know, there is a used vacuum furnace for sale on the internet that uses Stokes roughing pumps.

MEMBER ZIEMER: Okay.

(Laughter.)

(Simultaneous speaking.)

CHAIRMAN CLAWSON: Okay. So we will look more into that in detail, then.

MR. STIVER: Okay. So, again, we'll kind of keep this in abeyance until we can sort out the data issue.

The next aspect of the DWEs was this 1955 Plant 9 issue. And I had sent around a spreadsheet last night that I hope everybody got.

Let me see if I can pull this up

here. Okay. I am not used to working with the 2010 version of Excel here.

MR. ROLFES: John, is this on the K: drive by chance?

MR. STIVER: No. This is something I had done years ago when I was working on the DWE problem called "Plant 9: 1955-1306-12A." I sent it to Stu last night.

Basically, this is my re-creation of the DWEs, which I did for basically all of the plants that we were tasked to look at based on the raw data that Mark had provided back in 2009. And the important thing to see, we talked about this last week in the technical call, that Mark had indicated that Bob Morris, who was the author of the model, thought that maybe this, which is the secondary welder's helper that had the highest DWE, 686 MAC, but there might have been some transcription errors. It just seemed like too high of a value for it.

So I went through all of the data for all of these workers in Plant 9. And it turns out, like I said, the highest DWEs, there are five of them. And the top four ranged from 215, 233, 473, and 685 a pack.

So there's quite a few jobs here that all have very high values. And in every case, they tend to be driven by one or two very high breathing zone samples. Now, these are probably obviously transients that are captured, you know.

Of course, with the measurements, you wouldn't necessarily expect to sustain a cloud of thorium at 900,000 dpm per cubic meter for any length of time. But it looks to me that the pattern here -- I mean, once again, every one of these tasks that has the real high DWE are the types of tasks you would expect. And for all those four positions, the highest are over 500,000 dpm per cubic meter.

So I don't think what we're dealing

with here is a situation where we have transcription errors. It's just that you've got a very high concentration, a transient concentration, during a particular task.

Now, you ask yourself, is it reasonable to think that this worker is going to be breathing this stuff while he is in there? And the answer is no. You go to the HASL reports. For this particular one, it's Stefanec in 1955. And they actually say that for the high-dust operations, the respiratory potential is one.

Now, of course, the question is, what is high-dust operations? Is it ten percent? How do they define it? But the problem being is that we're kind of in a unique situation in using air data to provide intakes.

Here we have got a situation where we have real exposures. The dirtiest job in the entire Fernald plant is, in 1955, thorium

metal production. And these are the real DWEs that were gathered there.

The wrinkle is that they didn't account for respiratory protection in doing these studies. So you have a few high samples in each one of these that you know the guy is probably wearing a respirator during that time. And if you don't consider that fact, you end up with a value. You take 686 MAC. And then you consider the specific activity of natural thorium is 2.2 times 10 to the minus seven curies per gram, I believe, which is very low specific activity. And that translates to about 100 milligrams per cubic meter for that value.

Now, that's just about the physiological tolerance limit that anybody can stand for any length of time. So do you then give everybody 100 milligrams per cubic meter, 8 hours a day, for the entire year or does that just seem unreasonable?

So, you know, NIOSH became -- the thing you guys posted was an alternative way of looking at the intake for that particular year. And correct me if I'm wrong. Some of the people on the phone are involved in this. But it looks like what you did is you took all of the air-sampling data for 1955. You fit it to a log-normal and did repeated sampling to generate some theoretical distribution of air concentration data, and then took off the 95th percentile of that, which was, I believe -- well, that was one aspect. It was one way of doing it. You got a value that's about 100 MAC, give or take.

The other aspect was to look at -- I think it was like 785 actual samples. It wasn't a complete set. And you just did a normal, you know, a log-normal fit to that and did the same. That came out to about 75 or 80 MAC.

And so it's an alternate way. It

kind of concerns me because here we're throwing out real -- if you go that route, you're throwing out real data because it seems too high.

Now, this is real data for real workers and real jobs. And it's got some limitations because it doesn't consider respiratory protection. We don't really worry about that when we're dealing with one or two MAC, but when you start getting up to the physiological tolerance limit, it starts making a bigger difference. Is it plausible that somebody could breathe that much?

And so I guess the question, the place I am at right now is that, you know, you've got an alternate approach where you can go through and model an intake based on a bootstrap approach, or you can take the real data and then possibly account for respiratory protections.

We did a couple of

back-of-the-envelope calculations using protection factors of 10 and 100. And, as you expect, you apply it to the highest MAC. It's that one task that's 900,000 dpm per cubic meter for 70 minutes that's driving the train. And you apply a protection factor to that. You can knock it down to about 10 to 70 depending on whether you use a protection factor of 10 or 100. I mean, you can do that. You can figure out what is a reasonable value for the respirators that were used at the time.

And so you end up with a number that is pretty close to what the bootstrap analysis gave, but in our opinion, it seems to be more reasonable because you're using the real data. You're not throwing it out.

And so that is kind of where we are on that particular number. You know, I think I am speaking for the SC&A team. And I think 686 MAC is not a reasonable value to give

somebody on a continuous basis.

MR. BARTON: And, John, when you talk about converting 686 MAC to the equivalent in milligrams of dust, that is the daily weight of exposure for that entire day. If you actually look at that, just that 75-minute activity where he's up around around 600,000 dpm.

MR. STIVER: Oh, yes.

MR. BARTON: I mean, it's like even out of the realm of unreasonable. It's unrealistic.

MR. STIVER: Oh, yes. Yes.

MR. BARTON: You wouldn't be able to breathe. You would be choking on it.

MR. STIVER: And not only that, if you take 686 MAC and put a GSD of 5 on it, you are looking at one and a half grams per cubic meter. You know, he can't even sustain a cloud --

MR. BARTON: I think you put it

very succinctly, John. I think our main concern here is not the number that is being proposed. I mean, it is right in the realm of the number if you apply respiratory protection. I think our main concern is how we are getting there and that if we are going to start throwing out these numbers because they are very high, to me, that is a very dangerous precedent to set and could be a Pandora's box. Whereas -- and I know it is policy not to ever really account for respiratory protection because you are not sure if they're wearing it, but I think in cases where it is physically impossible that he wasn't wearing it, then maybe that is a reasonable consideration to take when you are trying to arrive at a reasonable number to apply at a coworker model.

Like I said, our problem is not the number you came up with. Our problem is really just sort of the philosophy behind the

process of -- we have these daily weighted exposure reports, which are great tools to try to get an idea of the exposure potential these workers faced. And to kind of deconstruct them and start using the raw data I think sets a rather dangerous precedent.

MEMBER SCHOFIELD: I have a question. You're talking about you're going to assume that they're using some type of face mask. Do we know what kind it is?

MR. BARTON: No. Airline respirator is what they --

MEMBER SCHOFIELD: Just airline respirator?

MR. BARTON: -- actually talk about in the daily weighted exposure report. They specifically say these don't take into account the fact that workers wear respirators in high-dust environments. Again, we don't know what that high-dust environment is.

MEMBER SCHOFIELD: It's just one of

the little cardboard-type ones, one of the --

(Simultaneous speaking.)

MEMBER SCHOFIELD: Yes, but I'm simply saying, really, when you start interjecting that, you need to really know what they're using because otherwise there are huge factor differences of personal protection equipment they could have had available to them.

MR. STIVER: I think you'd have to do some forensic research. You know, back in 1955, for an airline respirator in this type of operation, what was the protection factor, what type of cartridges and so forth if they use cartridges -- they didn't use cartridges then -- but what kind of value would be reasonable?

So, you know, the question is, it would have to be implemented in a TIB of some kind, but it would take some work. It's something that, in theory, could be done. You

know, in practice, you know, first of all, what do you decide a high-dust value is? And what is the appropriate protection factor you would apply to it?

CHAIRMAN CLAWSON: So, John, if I am following you right on this -- and, Mark, correct me if I'm wrong on yours. What NIOSH did was took these high doses and it didn't use them and did a log-normal distribution. And what your issue is, is that you don't want to throw these out but to put the respiratory protection limit, use it. Is that what --

MR. STIVER: Yes. Both approaches, you get a number that is within the same range given the uncertainties we are dealing with. The difference is our number -- not our number; we don't necessarily own it -- but using the DWE as a starting point, you've got a more solid basis. You've got real measurements that are actually documented. We know that the highest, dirtiest tasks were

done with respiratory protections. You know, they even tell you the type of respirator they used. Without that, it would be physically impossible to do the job, situation where it had to have happened.

So we are that point where, really, if you're going to consider the fact that these people were exposed, that this is the dirtiest job you could possibly do, and if you make all the claimant-favorable assumptions you normally would when you are dealing with doses and intakes that are down in the lower range, you wind up with a number that is just implausible to how you could possibly survive that.

So our approach is to say, okay, how are we going to take this data that we have and generate a reasonable intake knowing what we know? And applying respiratory factors to the real data we believe is probably preferable to throwing that data out

and generating a theoretical statistical construct to replace it with.

CHAIRMAN CLAWSON: Mark, did I represent NIOSH's side of it? You guys are not using this data correctly -- or, correct?

MR. HINNEFELD: Well, I think why our most recent proposal is called the bootstrap analysis is that we don't have the entirety of the air samples because when you get a DWE report, it will say there were six samples taken, you have the min, max, and average. And so you don't have the entirety of it. And the bootstrap program is intended to -- well, let's assume that they're log-normally distributed. Knowing the min, max, and average, we can build -- we know what a log-normal distribution would look like. And we will populate that, essentially randomly generate numbers in there. That gives us then a complete data set and allows us to use, I think, the 95th percentile of the

complete data set, which was what Strom also kind of endorsed. And so that's the point.

Now, SC&A has expressed discomfort with that approach over an actual measured, collected set of data that were taken for the purposes of measuring exposure. I mean, that's what these were. These were exposure studies. And the technique was developed at HASL. I think, actually, Fernald did them themselves because I think these people that ran health and safety early on at Fernald came from HASL.

And so their position is you've got all of this good data, you've just got this problematic 1955 year. And there is other evidence that '55 was the worst exposure year for thorium. We've got a memo that I can put on -- I probably should have done it before today -- it was a memo between two people at HASL, one relating to his boss the conversation that he had had with his former

colleague who was the medical director at Fernald. And he said he essentially called him up and said he wanted to deal with the thorium exposures you had last year, last year being 1955. And he describes numbers as high as -- exposures as high as, 50,000 micrograms per cubic meter, with individual samples as high as half a gram per cubic meter.

And the description that he pretends that was given to him by the medical director was, "well, they were very high. We got this rush order from DOE. They were trying to make it. Since the numbers were so high, we told them they had to slow down the production rate. And we got the exposures down, the maximum exposures down to 15,000 micrograms per cubic meter." And so there is some other kind of information.

Now, I think 50,000 micrograms relates to, what, 170-some MAC or something. So, you know, all of these things indicate

that somewhere in that neighborhood of 100 to 150 or 100, somewhere close to 100, is probably a decent number for the exposure for that year. You know, the DWE with respiratory protection, you can only count 10, a protection factor of 10, which is pretty low for an airline, but you can only do that.

All of these things kind of indicate that there is a number. It seems like there is a number that can be worked out. The DWE is probably a sufficient method for doing this with some modification.

So I think on the face of it here, we have some discomfort from SC&A. I would guess maybe the Work Group would share that discomfort with the bootstrap program and would share the preference for the DWE, or the DWE with some consideration, because of that one implausible, that 686 number, which just doesn't seem realistic.

CHAIRMAN CLAWSON: Well, in my

looking at it, because I never want to put NIOSH into a situation where -- a lot of times you don't take into consideration the respiratory part of this, and I don't want to put NIOSH into a situation where it creates problems for them in other areas. But, on the other hand, I'd like to be able to say that we used all of the data that we had and we used it.

MR. HINNEFELD: Well, if the preference is for actual measured data --

CHAIRMAN CLAWSON: Right.

MR. HINNEFELD: -- then I would say you share SC&A's discomfort with the bootstrap program, which essentially generates distributions with essentially a random number generator in the distribution. And you generate the results.

So then you would share SC&A's discomfort in that and prefer some utilization of measured data.

Now, the letter between the two HASL employees is a sort of a -- he says 50,000 micrograms per cubic meter. It's a throw-away. You know, it's sort of like it was as high as this, and they used a nice, round number. So I don't know that you want to attach a lot of precision to that for that number. But he was giving the ballpark of the kind of thing it would be.

So, to me, it sounds like it's something that can be worked out in conversation to arrive at a number. It is going to be really high.

And, in fact, the thorium exposures in general, if you go through these DWE numbers, I mean, I don't know that there is a year where the DWE is less than the MAC. Is there?

MR. STIVER: There are some. The Plant 1 numbers are down.

MR. HINNEFELD: Yes. But when

you're going to choose the highest one --

MR. STIVER: Yes. Well, the highest one is --

MR. HINNEFELD: -- in any given year and then you're going to apply a GSD of 5, I mean, these are going to be some high --

MR. STIVER: Some high intakes. You know, we understand that, but, like I said, you put it exactly as I would state it, Stu. That is really our concern, that we don't want to start getting away from the actual exposure measurements if we don't have to.

I've used the bootstrap technique before to verify or to kind of, you know, do a validation of distributions. You know, if we were to go through and get a good sample, you know, exactly the same thing that Tom LaBone and your guys did, it is a useful tool. I would feel discomfort -- that's a good way to put it -- in replacing the actual data with

that kind of bootstrap --

MR. KATZ: Just another alternative way to think about it, is you could, though, flip that around, what you just said. Normally you used a bootstrap to validate whatever. You could turn it around and use that to validate. Since you are saying the numbers come out about the same, you could use that to provide reassurance in terms that the bootstrap's coming out at the right place, as opposed to replacing the bootstraps --

MR. STIVER: You could take the inverse. I guess the problem there is you are losing the pedigree of the data by doing that.

MR. KATZ: Well, yes.

MS. LIN: It's validating.

MR. KATZ: It's validating your model, basically, and you're using in its place.

MR. STIVER: Yes.

MR. KATZ: That is still relying on

data. It is not like it's being pulled out of the blue.

MR. STIVER: Yes, but instead of using the actual data, you're using the model, using the data to validate the model, which is kind of backwards.

MR. KATZ: So the issue there is just whether there are advantages to using the model over going the approach that you are talking about. I don't know whether there are or aren't, but whether developing your approach is --

MR. STIVER: The other aspect of it is if you're using the model, you're using the time weighting aspect, because essentially what they're generating is a whole series of unweighted air concentrations.

This is sort of the fallback position in Davis and Strom. If don't have DWEs, the high percentile of the unweighted air concentration distribution would be the

next step.

CHAIRMAN CLAWSON: And I guess, from my standpoint, my issue is if we have the data, we should be using the data. But as you have already pointed out to me on some other occasions, you are going to have to use the respiratory or it's --

MR. STIVER: You would have to apply some respiratory factor to those high exposures. Otherwise, you would wind up with an air concentration that would not be physiologically --

MS. LIN: So there is actually information for NIOSH to develop a protection factor?

MR. STIVER: I think that kind of information is available in health physics.

MS. LIN: But you would basically be developing a model.

MR. STIVER: It wouldn't necessarily be a model. It would just be

looking at the airline respirators and the protection factors that they normally have. There is a lot of data out there for that kind of thing.

MS. LIN: Okay.

MR. STIVER: It wouldn't be a theoretical construct.

CHAIRMAN CLAWSON: Bob, when you guys used this data and used a protection factor, what did you use, a protection factor of ten?

MR. BARTON: We did two runs two runs. And, again, the 686 MAC job had one 75-minute task that was really just -- basically what we did is we said, all right, what if we take the data that went into the DWE and say for that one specific task, we're going to assume that he had some sort of respiratory protection, and we calculated for a factor of 10 and 100 and we came up with numbers that are in the same ballpark as a

bootstrap. And, actually, they were a little bit lower.

And I'm perfectly fine with that because I honestly feel like making adjustments to daily weighted exposures like that, where it is just physiologically impossible that anybody could inhale that and not completely choke on it, I think that is one instance where it is realistic and okay to apply a protection factor because, I mean, we were just dealing with situations that you have to have it. I mean, you just can't have a worker in that environment breathing that in because she wouldn't be able to breathe.

MS. LIN: So these respiratory protection equipment that was used during this time period at Fernald is also used at other sites. Whether a worker was actually choking on the actual environment is --

MR. STIVER: Well, it would be used in any kind of high-dust environment --

MS. LIN: Right.

MR. STIVER: -- whether it be mining, manufacturing, wherever you are --

MS. LIN: Okay.

MR. STIVER: -- generating large quantities of dust.

MS. LIN: So for any other site, if something like this happens and we have evidence showing that there is actually respiratory protection equipment used, would SC&A be proposing the protection factors if the value isn't high but they just --

MR. STIVER: Well, I would say that it is a matter to be considered. I mean, up until now, this has never come up because we have never had real measurements that are that high.

MS. LIN: But you were --

MR. HINNEFELD: So, if I could offer something, Jenny. As a general rule, we don't provide credit for respiratory

protection because if you go back, really, even as recently as early in my career, sites would not have quantitative fit-test programs and the kind of training and proving proof of fit that you have today in order to claim the protection factors NIOSH recommends on various kinds of equipment today.

And so because those things, you know, those programs just -- you know, not only were they not documented. They probably didn't exist. We have not claimed that.

The special circumstance here, though, is that the measured data is essentially not breathable.

MS. LIN: Right. So then we still don't have validation and we need it to say that the respiratory equipment actually passes the test of what we were talking about.

MR. HINNEFELD: Yes. You are right. There is no --

MS. LIN: That would be --

MR. HINNEFELD: The only reason that this is different from our other position where people didn't have, you know, programs, is that in this case, you just can't breathe that concentration that was measured in this DWE.

MS. LIN: Right. But then we're using the measurement to drive the respiratory protection test and whether that will be applicable. So I am not entirely sure that will be --

MR. STIVER: I see where you are coming from. You are saying, yes, if you are going to apply it here, you should probably --

MS. LIN: Right.

MR. STIVER: -- apply it in all of the others as well.

MS. LIN: Yes.

MR. STIVER: And then you don't have a real --

MR. KATZ: You have to have --

MR. STIVER: -- valid basis for using those values at that time.

MR. KATZ: What you have here that is distinct is the certainty that it was used; whereas, you don't in a lot of other circumstances.

MS. LIN: But how effective was it implemented?

MR. KATZ: So to get to that question, the one thing I am just wondering about is -- so it's airline, it's air-supplied, basically, respirator. And generally with air-supplied, you have less of a fit factor issue than you do with respirators where you were actually drawing the air through a filter, because the air is being, in effect, blown into your mouth.

So you have less of a fit issue with air-supplied respirators. The only thing I am just wondering about is we're talking about 1955, which is a long way back. It

predates NIOSH being involved --

(Laughter.)

MR. KATZ: For example, NIOSH has done respirator research since the '70s, '71. And there is a lot known about the performance effectiveness of different types of respirators now. I just have no idea what that literature is like when you go back to '55. But the one thing you have, again, in favor is that this is air-supplied. It's not a filter, it's a respirator.

So there may be good enough evidence that you could be certain of a certain fit factor. I don't know. I just don't know. That is my question.

CHAIRMAN CLAWSON: You know, Jenny hit on exactly what I was trying to get to a little earlier when I was talking about NIOSH, because I hate to -- this is one situation where I am trying to use the actual data, but the actual data drives us to such a high point

that it is physically impossible.

You know, do we use the bootstrap method and do we use the actual data to verify it using a respiratory factor versus the other? Because we are going to be in other situations in other sites where the possibility is that NIOSH has to -- you know, they have held pretty firm on we can't take credit for this.

And this is what was creating somewhat of a conundrum for me, because I didn't want NIOSH to -- Jenny, I'm glad you brought that up. So I guess we've got two ways that we can look at this. We can use the actual data to verify NIOSH's model. Doing that, we have actually used it, but we are still in the situation where we are not using the respiratory protection --

MR. STIVER: You have a consistent application of policy.

CHAIRMAN CLAWSON: Right. That is

my issue. And that is what I was trying to bring up to you in a way, Stu, without coming out and questioning it, because we are going to get into situations in other sites and stuff like that. We have through the whole process. And we can't take credit for some of the respiratory issues.

I guess this comes down to a judgment call on us of how to proceed forward with this. My question is, between the two, from the bootstrap to the SC&A's approach, how much of a difference are we looking at? I guess, Bob, that --

MR. BARTON: I don't have NIOSH's number in front of me, but I believe it is somewhere around 100 MAC or something like that.

MR. STIVER: Eighty-five and 130 or something.

MR. BARTON: Eighty-five? Yes.

MR. HINNEFELD: The bootstrap

document has two techniques. One is using a 95th percentile, unweighted 95th percentile, of the sampling data. And the 95th percentile of that in 1955 was 135 MAC.

MR. STIVER: Yes. That's the number that --

MR. HINNEFELD: The bootstrap area result, which was done only for 1955 in that paper, the 95th percentile is 81 MAC.

MR. BARTON: And the back-of-the-envelope calculation for that one, 686 MAC, brought us in a little bit lower than that. To get a protection factor of 10, it was somewhere in the 70s. A protection factor of 100 would bring it down to the teens.

And I think this idea is that -- it's kind of like we could be setting a bad precedent both ways. In one way, we could open the door to applying respirator protection. In the other way, we open the door to throwing out data because we feel it

is too high.

I think the one facet that is most important about this particular situation is that we -- the data shows us that there is going to be too much dust to breathe in. I mean, it's not an issue of, well, they weren't wearing the respirator or anything like this. They had to be because otherwise they wouldn't be able to breathe in that environment. And I think that is the important point.

So if we are going to talk about policy and how this might apply to other sites, I think that if you encountered situations where, again, we're seeing, you know, 600,000 dpm. And it's just an intolerable dust loading. Then maybe it would be reasonable to take a similar approach and adjust those daily weighted exposures, because I think, honestly, I think it is a more realistic and scientifically defensible way to

reconstruct doses because the daily weighted exposure reports are individual workers and defining their exposure potential; whereas, you know, the bootstraps were kind of just reconstructing and taking all of the raw measurements and doing some sampling and then note the 95th percentile.

And while they come up with reasonably similar numbers in the same ballpark, I am more comfortable with the respirator approach, even though it actually will give you a lower number than the bootstrap did, because I think it has a better base in the actual science.

MR. STIVER: And the question, of course, was how is that going to be applied in the --

MR. BARTON: It will be a very tricky implementation. I agree.

MR. STIVER: Okay. We know the respirators were being used, but given the

fact that we still don't feel comfortable that we can really get a handle on what the protection factors might have been, we, nonetheless, need to invoke that with more of a claimant-favorable allowance for potential for higher exposure. So where do you draw the line on it? I guess that's maybe the policy aspect of it.

MR. KATZ: Can I just check in with -- I know Paul has to go before noon. Paul, are you still with us?

MEMBER ZIEMER: I am still on the line. I am en route to another location. But, anyway, yes, I think NIOSH has to tell us what they would do specifically in this case. Obviously, we use the real numbers, but if the result is implausible, which it would be in this case, then you have to do something about that. So I guess we need sort of a specific proposal. I think the point that was raised by John is a good one. And you need to

specify how to handle these kinds of situations.

MS. LIN: Except but, to clarify, that bootstrap approach that is used specifically for Fernald in 1955, it is still based on the site-specific information.

MR. ROLFES: Yes, correct.

MS. LIN: And that comes off bootstrap.

MR. ROLFES: Yes. We basically just filled in some missing samples, essentially what we did to re-create a distribution of the air samples if it is still the real data that --

MS. LIN: Right.

MR. STIVER: But the thing is you have high, low, and average, though.

MS. LIN: Yes. So both the bootstrap and the proposal that SC&A is suggesting, those are used in the industry. They're not like just something you pulled out

of thin air. So Strom used the bootstrap as a fall-back, right? And the DWE approach used by you guys is also scientifically valid. And both approaches come out with a sufficient, accurate dose reconstruction value.

MR. STIVER: Well, that is a judgment call as to what is sufficiently accurate. The problem I have with the bootstrap is you are taking -- you don't have the real source data. You are inferring what it would have been given the assumption that it's a tight distribution. And so it is one step removed from the actual data that was generated presumably for a worker in a particular job on a particular day.

MS. LIN: Right. And validated by the real data.

MR. KATZ: So is this something that needs to be settled before the -- I mean, is this a TBD issue ultimately or does this need to be --

MR. STIVER: It is almost like we're kind of getting into an over-arching issue in some ways. You know, the whole idea of respiratory protection --

CHAIRMAN CLAWSON: When we first got into this, Ted, it was looking somewhat as an SEC issue, but the more that I have looked into it, we have been able to be able to put it together. So my personal opinion is this is coming closer to a TBD issue.

MR. KATZ: Yes, so my only question is whether if this needs to be an agenda item for the teleconference or, really, this is just something that has more time to be worked out. Does it need more time to be worked out than when we have the teleconference? Because it seems like you have talked it out already as far as it can be talked out here at this point.

MR. HINNEFELD: Are we looking for what Paul suggested, that in light of the

discussion today, for us to come back and propose what we believe would be --

MR. KATZ: That's a --

MR. HINNEFELD: -- a good approach? Okay. It will take us some time because we have several people on the phone listening who are smarter than I am. So we will need to have some conversations on our side about why do we think -- you know, what is our approach and why do we think it is the best approach, having the benefit of the discussion today.

And so it will take us a little while to develop. It may take us more than one discussion.

MR. KATZ: So we have a July 1 teleconference. Do you think that is something that you are likely to get settled before --

MR. HINNEFELD: You know, it is hard for me to predict.

MR. KATZ: Okay.

MR. HINNEFELD: We need to get the other item done for July 1st. From this, it is a little hard for me to predict because, frankly, our contractors' availability is different than it used to be because of the money situation.

MR. STIVER: Yes. My personal deal on this is that this is not something we necessarily have to resolve before the Board meeting.

MR. KATZ: Okay.

MR. STIVER: It is an implementation issue.

MR. KATZ: Okay. So then at the Board meeting, you can update them on the situation and let them know that this is something that the Work Group will continue on?

MR. STIVER: Right.

CHAIRMAN CLAWSON: Correct.

MR. KATZ: Right?

CHAIRMAN CLAWSON: Yes.

MR. KATZ: Does that sound like a good resolution there?

CHAIRMAN CLAWSON: Yes, because I think after we have gotten into this a little bit deeper, I don't see it as an SEC issue because both demonstrated that, yes, we can do it. It's just what is the best process to be able to do it, because I say this in all sincerity, Stu, when I say that I don't want to push NIOSH into a situation that, well, you did it here, so you need to be able to do it here," but we were in a situation. Throw out the data or whatever.

So I don't think that we need to be able to do that. They could give us an update if they had something come up at the teleconference, but myself I think this falls more into the TBD issue.

MR. KATZ: Okay.

MR. HINNEFELD: Just to make sure,

686 MAC is a little shy of what, 50,000 dpm per cubic meter?

MR. STIVER: It basically translates --

MR. HINNEFELD: Seventy? It would be 70 times 686, right? So 70 times 700 is 49,000, right, or did I slip a decimal?

MR. STIVER: I was looking at it in terms of dose loading, about 100 milligrams per cubic meter.

MR. HINNEFELD: So that's about 100 milligrams per cubic meter. And how does that fit in to what is tolerable?

MR. STIVER: There are a couple of papers we looked at. Actually, when we did Chapman Valve, this guy, Wes Van Pelt, who is an expert in this area, indicated that -- well, he actually did a couple of different studies. One was what's respiratory -- whether it was respirable in terms of tolerance and also what kind of air

concentration could be sustained just from the physics of cloud formation, of particle respiratory size. And that aspect, I think it was a paper by a fellow by the name of Craig in the '70s, who does tell you that he indicated that the highest concentration that could be sustained was about 500 milligrams per cubic meter. And so we are about a factor of five lower than that.

But there are a couple of other studies. I know there is a paper by Stewart that John Mauro found in reviewing some of the work for TBD-6000 that indicated about 100 milligrams per cubic meter is about the upper limit of physiologic tolerance. We have a couple of different references converging on that number. It felt pretty solid. That number is probably about where we would be drawing the lines to what you couldn't really expect anybody to be able to tolerate it for any length of time.

MR. HINNEFELD: Okay. And 100,000 micrograms per cubic meter?

MR. STIVER: Depending on the specific activity.

MR. HINNEFELD: And then the 686 translates into what?

MR. STIVER: Six eighty-six translates into, I think it was, 98 milligrams per cubic meter. So roughly --

MR. HINNEFELD: Right around the same --

MR. STIVER: Roughly around 100.

MR. HINNEFELD: Well, what about -- just before we break for lunch, something else to think about.

MR. STIVER: This is without respiratory protection.

MR. HINNEFELD: Yes. What about if we use the DWE value as constant?

MR. STIVER: That was the other thing I was thinking was a possibility, would

be --

MR. HINNEFELD: As opposed to planning a GSD of 5.

MR. STIVER: Yes. Well, it could be used as a constant, but then you still have the issue, is it really feasible for somebody to be breathing at that tolerance limit on a daily basis for a period of --

MR. HINNEFELD: Yes. They would be doing that all year long --

MR. STIVER: Every day and all year long.

MR. HINNEFELD: -- the same. Yes. I see.

CHAIRMAN CLAWSON: Well, let me see.

MS. LIN: One last question, though. Bob?

MR. BARTON: Yes.

MS. LIN: Okay. So you were talking about respiratory protection factors

that could be applied to the value. So you said it was a factor of 5 and 10 and 100?

MR. BARTON: I did 10 and 100 as sort of a sample conduit.

MR. STIVER: This was kind of a scoping calculation. Those aren't real values.

MS. LIN: Oh, okay.

MR. BARTON: It wouldn't actually effect --

MS. LIN: But how would you then take a factor?

MR. STIVER: There are studies NIOSH has done in recent times --

MS. LIN: Okay.

MR. STIVER: -- that actually look at concentrations, you know, outside air versus, you know, the inside of a respirator and picking different types of configurations.

MR. KATZ: There is lots of research in that area since the '70s, but I just --

MR. STIVER: You are trying to back-extrapolate, saying, what we have now has always been what they were --

MR. KATZ: But the technology probably in the early '70s versus the mid-'50s is probably not that different. I don't know.

MR. STIVER: Eighteen years is probably not all that --

MR. KATZ: For that because that industry doesn't evolve that quickly, I noticed.

(Laughter.)

MR. STIVER: There have been a lot of redevelopments in respirator technology.

CHAIRMAN CLAWSON: Well, but I have to fall back on my personal thing. Jenny, when we go into a certain area, depending on what the DAC is in there, they tell us what type of respiratory that we use. All of our stuff was qualified. And part of what I have heard from these earlier years, they used to

just leave the airline respirators hanging on the wall. And this is why I am in such an issue of not using it or using it, because the processes that we use now are much different.

I mean, I have heard people talking about blowing the dust out of the mask before they can put them back on. And I am sitting there, "holy cow."

MR. HINNEFELD: Early in my career, respirators were reused. Absolutely.

CHAIRMAN CLAWSON: And reused.

MR. HINNEFELD: They were reused early in my career.

CHAIRMAN CLAWSON: Yes. And I'll be honest. This is where we're into a situation here. But my number one concern is, number one, that we give the claimant the benefit of the doubt, but then also, if we have the data, that we actually use the data when the data is actually telling us it is almost physically impossible.

MEMBER ZIEMER: Well, we are not going to solve that today, are we? So --

MR. STIVER: I think NIOSH can come back with some proposals on this.

MEMBER ZIEMER: Yes. Brad?

CHAIRMAN CLAWSON: Yes?

MEMBER ZIEMER: Brad? This is Ziemer. I have to sign off. So hopefully I'll be back later in the day.

CHAIRMAN CLAWSON: Okay. I appreciate your input, Paul.

MEMBER ZIEMER: Thank you.

CHAIRMAN CLAWSON: Good luck.

MR. KATZ: Thanks, Paul.

MR. ROLFES: This is Mark. I was just going to offer that individual dose reconstruction, sometimes for the -- you know, not for any particular site, but when we interpret bioassay data; for example, plutonium bioassay data, the further away from intake date that a bioassay sample is

collected you are going to start getting a higher and higher intake if you have an exposure. And then the further away the bioassay sample is collected, the larger the intake is going to be.

And then when we make assumptions about the type of plutonium, for example, that a person is exposed to, and basically knowing about the biokinetics of plutonium, if you assume that it is Type S material, these are some of the assumptions that we make in a dose reconstruction that if you would look at the actual air concentration of plutonium that the person had to have been exposed to, you can get some very high numbers in a similar situation. The only difference is what we're talking about is a low specific activity material.

So the mass of the material in the air is what sort of sets this apart from other approaches that we use in dose reconstruction.

It's something that we handle in dose reconstructions. And it's almost a routine-type thing. You can get some pretty high air concentrations when you interpret and make claimant-favorable assumptions about a bioassay sample.

MR. HINNEFELD: You wouldn't encounter this kind of airborne limit. You know, the air just can't hold that much to where people can't tolerate with a lower specific activity, short of half-life material.

MR. STIVER: Mark's point is well-taken. I mean, you make a lot of claimant-favorable assumptions that are probably not realistic. But, yet, it doesn't result in a situation where it's clearly, you know, it's not possible.

MR. HINNEFELD: Physically impossible, yes.

CHAIRMAN CLAWSON: Okay.

MR. KATZ: Should we take a break for lunch?

CHAIRMAN CLAWSON: Yes.

MR. KATZ: And rejoin about 1:00 o'clock? It's almost noon right now.

CHAIRMAN CLAWSON: Sounds good.

MR. KATZ: Okay. Thank you, everyone on the line. And we'll start back up again at 1:00 p.m.

(Whereupon, the above-entitled matter went off the record at 11:55 a.m. and resumed at 1:03 p.m.)

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

(1:03 p.m.)

MR. KATZ: This is the Fernald Work Group Advisory Board on Radiation and Worker Health. We're just returning after lunch break.

(Roll call.)

MR. KATZ: So we're good to go.

CHAIRMAN CLAWSON: Okay. With that, I will turn it back over to you, John, and proceed on down.

4. THORIUM-232 COWORKER MODEL BASED ON CHEST COUNT DATA FOR 1978-1988 PERIOD; IMPLEMENTATION

MR. STIVER: Okay. Yes. This is John Stiver. The next issue that was on the agenda is the in vivo thorium model for the 1979 to 1988 time frame. And I guess the issue that came out of the last Work Group meeting and kind of a culmination of a series of White Paper exchanges is that we're in

agreement with DCAS that it is possible to bound the doses for the intakes of thorium based on the in vivo accounts that are reported in units of nanocuries, actinium-228 and lead-212. And there is also the claimant-favorable assumption of triple separation for the thorium, which then results in a disequilibrium factor of about five for lead-212 in relation to thorium-232. We agree that that is a claimant-favorable approach.

One thing that was kind of outstanding, though, is that a lot of the results, the positive results, kind of indicate the higher levels of actinium-228 than would be expected. And oftentimes or maybe not oftentimes because there are not that many positive results to begin with, but there are several instances where there is a positive actinium measurement and there's a sub-MDA lead-212 measurement.

Then the question becomes, okay,

what do you do in a situation where you have a positive actinium and no lead-212? And how do you ever get back to thorium from a situation like this?

And I have put together a response in action item 7, where they looked at the data. If I did this wrong, you guys be sure to correct me. It looked at all the different data and used a report 44 technique to characterize the background distribution as a normal distribution centered around zero and then a log-normal fit to the values greater than the MDA, which would then allow you to separate out the sub-MDA data, then reboot the noise and really look at the positive data. And in a situation where there was an offset of a mean from the zero -- they were pretty small offsets, as I recall, like about a tenth of the MDA value in most cases. And there would just be a correction that currently is the bias in the data. There might be bias one

way or the other because of the -- you know, in theory at least, the background distribution should be centered around zero that's a true noise.

And so they went ahead and they adjusted the actinium and lead data and, based on the 95th percentile, I believe, of the background distribution, came up with kind of an average detection limit, about .12 nanocuries. And using a kind of a rule of thumb of twice that for the MDA of 1.96, you are looking at about .24 nanocuries for detection limit. And this comports well with the actual measurements that were generated for the in vivo system.

So we found that at least it seems to be -- as far as looking at the actual data generated from the system, you should be able to re-create. The detection limit and the background and all seem to be correct using two different approaches, the actual

calibrations. You are using the actual data to get back to that.

However, there are a few situations where there is a high actinium value. I think the highest is like 18 times the lead value. And, rather than try to use that actinium and assume some level of disequilibrium to get back to thorium, you guys have kind of invoked the possibility of unsupported radium-226 as the cause for these high values.

And so I guess we were kind of curious about that because it sort of opens up an awful realm of radium exposure and raffinate exposure for the thorium. Granted, there are very few of these values, but I haven't really looked at the source data in detail. But it seems to me if you have a ratio of 18, that would be indicative of maybe a contaminated sample or a bad sample that maybe really isn't indicative of workplace exposure.

You know, you also use the triple separation to get a kind of upper bound on what the ratio of actinium to lead would be. I think it was about 1.5 or so. So you sort of use that as a cutoff. And above that would be presumed to be a radium exposure.

I know Joyce had had some questions about that. She is probably closer to this than I am. I am kind of giving the broad-brush stroke overview. But, Joyce, are there some particular issues that you would like --

DR. LIPSZTEIN: Yes. I say it is okay. Maybe there is a radium source, but there are other scenarios that are also possible. I would say that things like that are complicated. And maybe there are other scenarios that are bounding and that could explain the actinium being higher than the lab activity.

We know all NIOSH papers and the

TBD on internal doses assumes that the production years went up to 1979. And the reason for the positive results after '79 was that workers were assigned to some maintenance duty for thorium or repackaging of thorium for shipping, et cetera.

What happens is that lead-212 is very sensitive to the number of separations that is assumed. So one bounding approach, the lead-212 result is to assume actually full separation. But that is just a --

MR. KATZ: Joyce, we just lost you.

DR. LIPSZTEIN: I'm sorry?

MR. KATZ: I'm sorry, Joyce. Just a moment ago, we lost you, whatever you were saying. You went quiet there.

DR. LIPSZTEIN: I'm saying, did you get up to '79 with the production years of thorium?

MR. HINNEFELD: Yes. We did get that.

MR. STIVER: We got that.

DR. LIPSZTEIN: Okay. So after the production years, then we have measurements of lead and actinium. While lead-212 is very sensitive to the number of separations that the source had, immediately after the exposure, after the source is separated while actinium, it's not a fact that by the number of separations because it comes just after thorium-232 and radium, but it is very sensitive to the lag of time between measurement and separations.

So one other plausible scenario for actinium-228 results being higher than the lead-212 results is that the time between separation and thorium exposures or thorium measurements is long. So if you have more than a year after the separation, you find that actinium-228 might be higher than lead-212 depending, of course, on the number of separation. Let's assume the three

separation like NIOSH assumed.

So I think it's not -- you know, I would not be surprised to have actinium-228 measurements higher than lead-212 if the separations stopped in '79 because it will come -- like measurements would be one, two, three years after the separation. So the actinium-228 will rise. And you will end up having high activity of actinium-228 in the lungs.

So I think this discussion is not an SEC issue but is a TBD issue, while we have to take into account the value scenarios that actinium-228 would be higher than lead-212 and see which scenario is more bounding to interpret the data.

I think it's -- you know, I'm not saying that exposure to additional radium-228 is not possible. Of course, it is. But then we would have to go into what source of radium-228, how much. And you can get the

same results if you just know that the separations ended in '79, measurements were done after '79. So we would expect actinium to build.

MR. HINNEFELD: Okay. This is Stu. So, Joyce, what you are proposing, then, is, rather than just make the blanket statement that if the actinium-228 is more than 1.5 times the lead-212, then we consider this radium intake. That is what we are proposing. What you are saying is, as an alternative, look at the date of the measurement compared to 1979, which would have been the last separation.

DR. LIPSZTEIN: Yes.

MR. HINNEFELD: And then, based on that, perhaps draw some -- you know, see what your expected actinium ratio would be. Okay. I can --

DR. LIPSZTEIN: Exactly.

MR. HINNEFELD: I think I would

like to see sort of something in writing to complete it because I am struggling with how we are not bounding by doing what we propose and it --

DR. LIPSZTEIN: No, no. You aren't bounding what you propose when you have lead-212, but then sometimes you don't have the lead-212 results. You just have actinium.

MR. HINNEFELD: Right.

DR. LIPSZTEIN: So if you consider the time after separation, you can use the actinium results also. So you have more data.

MR. HINNEFELD: Okay. So you would say use the actinium monitoring result to determine your thorium-232?

DR. LIPSZTEIN: Yes, yes, yes.

MR. HINNEFELD: Okay.

DR. LIPSZTEIN: Knowing that the separation ended in '79 and before '79.

MR. ROLFES: So are you proposing, then, instead of using like the MDA value for

lead-212 like we would sign the waste intake based upon the minimum detectable amount of lead-212, we should use the actinium-228 or whichever is more favorable? Is that --

DR. LIPSZTEIN: Yes, yes, whichever has positive results.

MR. HINNEFELD: So you feel like it's more desirable to use a detectable actinium-228 result with an estimate of the time since separation --

DR. LIPSZTEIN: Yes.

MR. HINNEFELD: -- to predict --

DR. LIPSZTEIN: Like, for example, with my monitoring work in Brazil, we in general use actinium-228 because lead-212 has a problem with what rate. So sometimes lead-212 is high because of radium.

MR. HINNEFELD: Yes.

DR. LIPSZTEIN: But we don't have the same with actinium. The problem with actinium is that it is very sensitive to the

time after separation. But once you know when separation ended, then the actinium result is okay. And you have a lot of positive actinium results. So you have more data that are useable that you have positive results.

MR. HINNEFELD: Okay. I am going to ask the ORAU people on the phone if they see any particular issue with that approach.

MS. JESSEN: Tom, do you want to answer that?

MR. LaBONE: This is Tom. The one statement I would make is that in a universe where you can have triple separated thorium, you have to have radium-228 by itself. And so I understand. I don't know all the ins and outs about how this material is handled, but if you have these separations going on, somewhere in that facility, there has got to be radium-228 by itself because it has a long enough half-life.

The practical problem I see with

what Joyce is saying is that I don't know if separation ends in '78 or '79, and I have a chest count in '81. Do I assume there have been two years of in-growth from the thorium-232? Is that what you --

DR. LIPSZTEIN: Yes, Tom, because after a certain time, it is very sensitive until the first of the year. And then the actinium-228 becomes almost stable after this bypass, for example. So you can rely on the actinium-228 measurements. And then, you know, you don't have to make any hypothesis about some radium that you don't know how much radium it is. And then the actinium doesn't have -- you know, because you have to first see where actinium is very sensitive to the time after separation.

Then after one year, it becomes a little bit more stable. And what you see is really with the workers that have a lot of actinium-positive results. If you plot the

actinium with the years, it is relatively constant. So you will have positive results, and you will know how to interpret it. And you don't have to go into how much radium was there and how much was formed and how much the person was really exposed when he was in the packaging or when he was doing some maintenance duty.

MR. LaBONE: Okay. I think it would be good to look at what you are proposing and then see how that compares with this default separated thorium. I think a lot of it comes down to and how was this material being handled during the time frame of the late '70s and up to the '80s.

DR. LIPSZTEIN: Yes.

MR. LaBONE: You know, was there a possibility of free radium-228 and things like that? We can compare it and see which one looks more appropriate.

DR. LIPSZTEIN: Okay. Sounds good.

MR. HINNEFELD: This is Stu. Is that something we can do from this conversation or do we need some sort of product from Joyce or SC&A to proceed or do we know enough from this conversation to go ahead and do that comparison?

MR. LaBONE: Probably the fastest way is for me to go ahead and do it and then send it to Joyce and say, "Hey, is this what you're talking about?" because I think I know what she is talking about. And I can just go ahead and work it up.

MR. HINNEFELD: Excellent. Excellent.

MR. KATZ: Thank you, Joyce.

DR. LIPSZTEIN: Okay.

MR. STIVER: Thanks, Joyce.

CHAIRMAN CLAWSON: But I want to make sure that all of us understand that this is basically coming down to a TBD and --

MR. STIVER: Yes. It's a TBD.

CHAIRMAN CLAWSON: -- it's not, you know, both sides being able to prove we can bound this. So this stuff that we don't need before the --

MR. STIVER: Right.

CHAIRMAN CLAWSON: Okay. Well, that was --

MR. KATZ: Yes, wonderful.

MR. STIVER: That was really all we had on the table, were those three big issues.

MR. HINNEFELD: Nineteen fifty-three.

MR. STIVER: Yes.

MR. HINNEFELD: Nineteen fifty-three, thorium.

MR. STIVER: That's right. Yes. Thanks for reminding me. For the DWE model, there was a placeholder for '53. And I would note that it was to be determined sometime in June.

MR. HINNEFELD: Yes. Our thorium

timeline starts in 1954 because that's when the bulk of the thorium work happened, but we did have some thorium air samples from 1953. And so you guys said, "Well, what do you do in about '53?"

So in going back and looking at the origin of those 1953 air samples, the ones that I have seen are from what was called the Experimental Machine Shop. And they were machining thorium, which had been -- thorium out of the lead had been made elsewhere. I think it was Simonds Saw and Steel, but one of the AWEs.

And so they received this thorium metal. And they were in the "Experimental Machine Shop" apparently figuring out how to machine this stuff that they were going to have to machine, kind of a pilot plant-type activity.

In fact, the Experimental Machine Shop was just kind of right there by the pilot

plant. That's where it existed, is building 3045. I knew it was building 3045. I didn't know it was Experimental Machine Shop when I was there.

So it appears that we do have some air-sampling data. It looks like since it was an Experimental Machine Shop, you know, they would do thorium sometimes and they would do uranium sometimes. And so there is going to be a limited amount of probably work that was done. And we do have some air-sampling data that we have compiled. We don't have a compilation that shows things like duration that would give you the amount of information you need to build a DWE kind of information.

But since we have to do something about '64 through '67 anyway, right, we have to do something about that anyway, I think the same kind of information about getting the total amount of air data we have down in some sort of spreadsheet or something where you can

see all of the data we have with those data points and coming up with a proposal for this is what we think might bound that work, again, this looks like it is probably sort of intermittent, like they would machine with thorium and for a while and then --

MR. STIVER: They're not involved in a production operation here. It's just a matter of you've got some air concentrations during machining activities. And so if we could go ahead and compile that data along with the '64 to '67 and we can all look at it at one time, it would --

MR. HINNEFELD: Yes. I think that is what we will have to do. We started a compilation. I want to make sure we get that. I think I would like a little more expanse because the compilation we have doesn't include like all of the information you would see on an air-sampling data sheet, which to me sometimes that is really informative.

MR. STIVER: Right.

MR. HINNEFELD: It tells you how long an operation ran and --

MR. STIVER: It sounds like you've got some SRDBs to go along with that.

MR. HINNEFELD: Yes. We found some SRDB air sample data, air sample data sheets from '53, in building 3045 while they were machining thorium and we found some while they were machining uranium as well.

MR. STIVER: We'll just roll that in as one task, I guess, go ahead and look at all of the thorium put together. So I guess at this point.

MR. BARTON: Actually, John, I have one more. And it kind of relates to both the DWE and this --

MR. STIVER: Okay.

MR. BARTON: -- in vivo thorium. And it kind of has to do with the implementation of the model. Basically what

we have said is, you know, aside from the triple separation, such that, you know, if you are a thorium worker or it is suspected that you could have been handling thorium in the in vivo period, that you would be assigned in the 95th percentile. And obviously with a DWE, if you are suspected of handling thorium, then you are going to get the highest DWE value with maybe some different ones in there for the pilot plant in the late '60s and whatnot.

I guess what I would feel to be beneficial to both of these is if we give a little bit more specific information as to who these are being applied to. Based on the write-ups for this meeting, it kind of appears that they will leave it up to the dose reconstructor.

And if the dose reconstructor feels they could have handled thorium and will be assigned the 95th percentile or, you know, the maximum MAC value -- I will use Simonds as

sort of a precedent example. In that case, the coworker model was actually delineated. And we're still kind of fleshing this out, but, I mean, basically where DCAS and SC&A agreed was that if you were a plant worker at Simonds, then you are going to get the 95th percentile where if you were just an office worker and you really had very limited exposure potential, then you would get the 50.

And it is my personal opinion that I think both coworker models in this case for thorium would benefit from that type of classification.

And I know we can't micromanage everything. We can't say every single job type will be fit into whichever bin, but I think some guidance should be put there if, for nothing else, transparency in what the policy of how you are going to assign sort of these different -- in the case of the in vivo thorium, in different strata, you know, 95th

for some; 50th for others, but some guidance as to how that is going to work, whether that is the operators or the plant-wide workers or just some sort of further specific guidance as to how we're going to implement these models, which we basically mostly in principle agreed upon, but it's really kind of ambiguous as to how you are going to assign it and to who.

MR. HINNEFELD: And so this would then be Site Profile kinds of questions.

MR. BARTON: Yes, absolutely.

MR. HINNEFELD: Okay. For a little more specificity about how we'll apply -- okay.

MR. BARTON: At some point you need some professional judgment by the dose reconstructor as to what to assign who, but, I mean, as of now, the entire decision is kind of left up in the air without any specific guidance. And whether that's -- like I said, you know, all plant workers are going to be in

one bin and office workers in another or some other strata.

MR. HINNEFELD: Until we have the two models, the DWE, for lack of a better term, the thorium air data model, those years up through '67, and then from --

MR. BARTON: '79.

MR. HINNEFELD: -- '79 through '88, roughly, '89 for --

MR. BARTON: '89. '89 uses the '88 data.

MR. HINNEFELD: Okay. For that period, then, the in vivo model.

MR. BARTON: Right, right.

MR. HINNEFELD: So we've got the two models.

MR. BARTON: Right, two models.

MR. HINNEFELD: What specific direction in terms of full distribution, 95th percentiles, you know, who gets what?

MR. BARTON: Right.

MR. HINNEFELD: Okay. So that would be a task for us for Site Profile. We haven't worked that out anyway to give you those reconstructions. So it's a task we've got to do anyway.

MR. STIVER: Yes. It seems to me that some kind of a guidance as to these jobs would fall into this bin for the in vivo, 95th percentile and dose reconstruction, we would have to do the due diligence to determine what this person was doing and if they were that particular job, to have to go into that depth, but to have them make all the decisions as to whether they feel that this guy was exposed to -- in a certain level, I think is a little too much to put on the dose reconstruction. You might wind up with some big inconsistencies.

MR. HINNEFELD: Yes. Well, we generally try to provide a lot more instruction to the dose reconstructors so that we get -- you know, we want to do these things

consistently. And then there's a dose reconstructor. There's a peer reviewer and then an HQ reviewer on our side. There are a couple of layers of review. But the key is to get some guidance out there that can be interpreted consistently by various people --

MR. STIVER: Right.

MR. HINNEFELD: -- because that is what you are talking about.

MR. STIVER: The DWE model the way I understand it, they are buying off on basically a one-size-fits-all model. You get the number.

MR. HINNEFELD: Yes. You get the DWE --

MR. STIVER: Everybody in the --

MR. HINNEFELD: Everyone who is potentially exposed. I mean, there could be some -- yes. I don't know whether it was -- there could be administrative people where you've got strong evidence to believe they

were never out in the process area. They may get some sort of environmental thing, but if you're going to be --

MR. STIVER: Right.

MR. HINNEFELD: -- someone who is potentially exposed, you get that thorium model.

MR. STIVER: Right. Yes. So that type of guidance is --

MR. BARTON: Yes. Some discussion along those lines to kind of buttress up these coworker models I think would be beneficial, but like it is a --

CHAIRMAN CLAWSON: Well, and also, Stu, especially being -- maybe this is my standpoint being on the Dose Reconstruction Work Group. We see these sometimes inadequacies. So we're just kind of figuring if we culled this out at the beginning so we had a better idea of how it was going to be out. We wouldn't be seeing these up here in

any kind of dose reconstruction issues.

MR. HINNEFELD: Yes, sure.

MR. STIVER: Okay. So we are looking at a teleconference on the 1st.

MR. HINNEFELD: Right.

MR. STIVER: And for that, if DCAS could provide us with the spreadsheet data for the DWE years for which there is still some uncertainty?

MR. HINNEFELD: Right.

MR. STIVER: I guess we can postpone the Plant 9, 1955. We don't have to resolve that before the meeting. So I think we should be in pretty good shape for that. Tom LaBone is going to provide us with a discussion, kind of a comparison about the radium versus Joyce's approach for the in vivo. And so I think overall we are in pretty good shape, then, going into the next meeting.

MR. HINNEFELD: Now, are you going to do something about the start year for --

MR. STIVER: Yes. We are going to take a look at the --

MR. HINNEFELD: -- sometime?

MR. STIVER: Yes, the bookend years on the early side, '53 to '54 --

MR. HINNEFELD: Okay.

MR. STIVER: -- to get an idea of where there really is exposure potential.

MR. HINNEFELD: Okay.

MR. KATZ: So we'll catch up everyone on that at the Work Group teleconference.

MR. STIVER: Right, right.

CHAIRMAN CLAWSON: So we are only addressing one issue that is an SEC issue. And that is, what, '63 to '67 time period or '64?

MR. STIVER: That is still up in the air regarding what they came up with as far as DWE data. Then we have the other SEC definition for the period of time.

CHAIRMAN CLAWSON: Okay.

MR. STIVER: So we're going to be at the low end on that.

CHAIRMAN CLAWSON: And we're in agreement that the other ones are TBD issues and can be addressed --

MR. STIVER: Correct.

CHAIRMAN CLAWSON: -- at this time.

MS. LIN: I think it might be helpful to have like some or maybe even just one slide showing exactly what the Work Group is recommending to the Board, not just the SEC Class but what dose and what year could be constructed and that --

MR. STIVER: For what aspect are you talking about now?

MS. LIN: So your confirmation is not just focusing on the SEC --

MR. STIVER: Well, there are still outstanding TBD issues.

MS. LIN: Okay.

MR. STIVER: Actually, there are quite a few of them.

MS. LIN: But I think it would be really good to list out the years and the radionuclide that happened to determine. It could be found what are some of the remaining --

MR. STIVER: Okay. Yes. Sure.

MR. HINNEFELD: The most recent discussions.

MR. STIVER: Yes, the most recent discussions. Yes. I have some slides that I can modify pretty quickly.

MR. KATZ: Yes. Just so that the Board is up to date on what SEC issues are closed out as no longer --

MR. STIVER: Okay.

MR. KATZ: -- being SEC issues as well as --

MR. STIVER: We follow onto the last presentation.

MR. KATZ: Right.

MR. STIVER: I did give an update. Okay.

MR. KATZ: That would be great. Right.

MR. HINNEFELD: Hang on a minute. I'm trying to take notes here. Our spreadsheets on the air data, '64 to '67 and '53 and what we conclude from that as an approach for that.

MR. STIVER: Okay.

MR. HINNEFELD: A comparison of the in vivo unsupported thorium that we propose versus what Joyce proposed. We owe you one other thing, don't we?

MR. STIVER: Yes. What else have we got?

MR. HINNEFELD: They're not.

MR. KATZ: I think there is one other.

MR. ROLFES: We were talking about

whether -- well, if we wanted to apply a protection factor for air --

MR. STIVER: Oh, yes. The protection factor, yes.

MR. HINNEFELD: Well, okay. Yes.

MR. ROLFES: Yes. There was an approach, the bootstrap versus protection factor.

MR. KATZ: Right, right. That was the third item.

MR. HINNEFELD: And we were going to give some specificity about how the models would be applied.

MR. STIVER: Yes.

MR. HINNEFELD: That's what I was thinking.

MR. ROLFES: Yes.

MR. KATZ: Oh, implementation, right. So, then, for the Fernald session, I think it's a question, not an assertion, but I think we are probably okay if John presents

the update for all the Work Group's done. We don't really need a NIOSH presentation per se, just preparation --

MR. HINNEFELD: I endorse that wholeheartedly.

MR. KATZ: Yes.

(Laughter.)

MR. KATZ: I just wanted to make sure that --

MR. STIVER: I could do it together as a team, Stu.

MR. HINNEFELD: No. I suffer from overexposure to these meetings already.

MR. STIVER: I know. I told --

MR. HINNEFELD: If you can --

MR. STIVER: The last time your voice was gone about halfway through.

MR. HINNEFELD: Okay. Don't make me. I really badly planned one of them. I had like three presentations at one of them. And I'm the boss. I shouldn't have to do

that.

(Laughter.)

MR. KATZ: Brad, do you want to do an introduction or just --

CHAIRMAN CLAWSON: Well, actually, what I was going to do was let John give a brief overview on that. And then I was going to just give a short presentation as to what the Work Group is presenting to the Board.

MR. KATZ: So I'll have John first. And then you will --

CHAIRMAN CLAWSON: Right.

MR. KATZ: -- be the clean-up batter.

CHAIRMAN CLAWSON: Right. So it will give them a background of where we are at and this is what the Work Group is bringing before them.

MR. KATZ: Great. So it sounds good.

MR. STIVER: That's good. Set the

stage. How much time do we have allocated for Fernald?

MR. KATZ: Quite a bit. I think I allocated an hour and a half.

MR. STIVER: We don't need quite that much.

MR. KATZ: You may not need all of that.

MR. STIVER: Yes. We can rejigger things.

MR. KATZ: But I know I did. So put it on the side of --

CHAIRMAN CLAWSON: But as far as this goes -- and I took a little of it for presumption from Paul when he said that he was endorsing on this construction.

MR. KATZ: Yes.

CHAIRMAN CLAWSON: Except for the earlier years, that I was going to have a vote with him gone. But he had already said he had supported that, that I was going to present it

to the Board that this recommendation --

MR. KATZ: It is, right. No. That's why I asked Bill to speak on the record.

CHAIRMAN CLAWSON: Right, yes. And I just wanted to clarify that. And that's why I pushed Paul a little bit on that, was so that this can be -- because I knew he was going to be gone.

MR. KATZ: Right.

CHAIRMAN CLAWSON: So this will be a recommendation, the Board and --

MR. KATZ: Okay. The other thing, just to give some thought to John and Stu, is what sort of background materials would be useful for the Board in hearing these presentations for those Board Members that like to know a little deeper than what gets presented.

MR. HINNEFELD: So background information we would provide on the O: drive

for them?

MR. KATZ: Yes, yes, meaning just White Papers, whatever, but I know there is way too much on Fernald in general. So just trying to limit it to a few that would inform them a little more on what they're going to hear.

MR. HINNEFELD: Yes. We've got plenty for Fernald. It was recently. I think Mark is still compliant on that.

MR. KATZ: I heard a little bit about that.

CHAIRMAN CLAWSON: Well, and we have tried for the last year and a half, if I'm not mistaken, Mark, to make sure that Board Members are up to date on the papers that we have processed and what they have got in there. So they should have most of them. It's just --

MR. KATZ: Yes. I mean, a lot of these messages just go to the Work Group on

Fernald, these White Papers.

CHAIRMAN CLAWSON: Right.

MR. KATZ: They generally just go. And I will make those available, but if there are some in particular that are useful, that's what I want to know so I can point the Board Members to them.

MR. STIVER: Yes. I have a listing that I put out there in 2010. So I'll update that. It's kind of an overview.

MR. KATZ: Yes. That is going to get too extensive because that -- I don't want to throw 40 documents --

MR. STIVER: No. I mean, sort of like a guide, "These are the issues. These are the pertinent documents that relate to it."

MR. KATZ: Okay.

CHAIRMAN CLAWSON: But I would like to separate off the previous and what we have addressed.

MR. STIVER: Yes. We're trying to keep the focus directed on the most recent developments. I mean, otherwise we run the risk of --

MR. KATZ: Right.

MR. STIVER: You have seen in the past.

MR. HINNEFELD: Do you guys write to the O: drive? Do you save things on the O: drive?

MR. BARTON: Yes.

MR. STIVER: Oh, yes.

MR. HINNEFELD: Okay. So I think we could compile a folder on the O: drive for Fernald, just say "July 2013 Board meeting." And so it is easy for them to find.

MR. STIVER: Right.

MR. HINNEFELD: And they'll have to look in that --

MR. KATZ: The only issue is I need to send documents to folks because they don't

all access the O: drive. So I need the PA-cleared versions of documents. So keep that in mind, but --

CHAIRMAN CLAWSON: And I also want to make sure that -- because Fernald -- let's be honest. There are a lot of papers out there. And this has been a long process to get to where we are at now. And I just wanted to make sure I didn't overwhelm especially some of the new Board Members with -- they have already had access to previous ones. I want to focus on why we're at where we're at.

MR. KATZ: Right. Good.

CHAIRMAN CLAWSON: Okay.

MR. KATZ: So it sounds like we're --

CHAIRMAN CLAWSON: It sounds like we can adjourn, without any other questions. There are no more questions out there.

MR. KATZ: So thank you, everybody. This was very productive.

CHAIRMAN CLAWSON: Okay.

MR. KATZ: Have a good rest of your day.

(Whereupon, the foregoing matter was concluded at 1:41 p.m.)