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CENTERS FOR DISEASE CONTROL
NATIONAL INSTITUTE FOR OCCUPATIONAL
SAFETY AND HEALTH

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ADVISORY BOARD ON RADIATION AND
WORKER HEALTH

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WORK GROUP ON TBD-6000

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FRIDAY
APRIL 26, 2013

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The Work Group convened via teleconference, at 10:30 a.m., Eastern Daylight Time, Paul L. Ziemer, Chairman, presiding.

PRESENT:

PAUL L. ZIEMER, Chairman
JOSIE BEACH, Member
DAVID KOTELCHUCK, Member
JOHN W. POSTON, SR., Member
WANDA I. MUNN, Member

ALSO PRESENT:

TED KATZ, Designated Federal Official
DAVE ALLEN, DCAS
BOB ANIGSTEIN, SC&A
BOB BARTON, SC&A
DAN CHUROVICH
SAM GLOVER, DCAS
JOSH KINMAN, DCAS contractor
JENNY LIN, HHS
JOHN MAURO, SC&A
DAN McKEEL
JIM NETON, DCAS
JOHN RAMSPOTT
JOHN STIVER, SC&A
BILL THURBER, SC&A
TOM TOMES, DCAS

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

10:29 a.m.

MR. KATZ: Okay. I think this is close enough to time, and we probably have everyone online. Good morning, everyone. This is the Advisory Board on Radiation and Worker Health. This is the TBD-6000 Work Group. Let's get started with roll call. We're speaking about specific sites, so all agents and related people please speak also of the conflict of interests. And let's go with Board Members.

(Roll call.)

MR. KATZ: There is an agenda for this meeting that's posted on the NIOSH website under the meetings section under today's date, along with several papers that are going to be discussed for the four different sites.

Before I turn it over to the Chair, just let me, for phone etiquette, there's a lot of background noise already on

this phone line, so, please, everyone, everyone who's not speaking should mute their phones. If you don't have a mute button, press * and then six. That will mute your phone. And then pressing * and then six again will unmute your phone. But, please, mute your phone while you're listening because the background noise is difficult. That sounds much better already.

And, also, for everyone on the phone, remember don't ever put the call on hold. Just hang up and dial back in if you need to, but putting the call on hold will disrupt the call for everyone else.

So thank you. And with that, Paul, it's your meeting.

CHAIRMAN ZIEMER: Okay. Thank you, Ted, and good morning, everyone. I'll officially call the meeting to order. You should all have an agenda, either online. I think, perhaps, one of the public callers from GSI does not have that agenda since he doesn't

have a computer, but the rest of you do. But I'll just take a minute here and review what we'll be covering today.

We have four sites that we're dealing with at the present time: General Steel Industries; Baker Brothers in Toledo, Ohio; Joslyn Manufacturing; and Simonds Saw and Steel. I didn't assign any time intervals to these four facilities, but it's my expectation that the bulk of our time will be focused on General Steel Industries.

And, also, I must apologize. I've developed a cold here, and I'm having some trouble with my own voice. So I apologize if you have a little trouble hearing me or understanding me this morning, but we'll do the best we can to proceed through the agenda.

My plan is that we would expect a lunch break at approximately 1 p.m. Eastern Time. And we will take a comfort break before that. That comfort break will be determined either by the Chairman's comfort or someone

else in more a state of discomfort than I at some particular time. But, in any event, we will take a break, as appropriate.

So I want to begin with General Steel Industries and just identify before we discuss anything that, as a starting point, we have a White Paper from NIOSH prepared by Dave Allen, and that White Paper was distributed. We have two responses from SC&A prepared by Bob Anigstein, the first called "Review of NIOSH Estimates of External Exposure at GSI" and the second called "Review of NIOSH Estimates of Internal Exposures at GSI."

And then I would also like to call attention to a number of documents that were provided by the petitioner. And I do want to make sure that the petitioner, at some point, has the opportunity to amplify any points he wishes to make, as well.

We have a document dated April 5th, a response to Dave Allen and DCAS White Paper, by Dr. McKeel. We have also from Dr.

McKeel a paper dated April 22nd, which deals with the radium era and some information on building 6 and also some information from the petitioner regarding the -- if I can get the paper out here -- the stolen radium plumb-bob. And I think there may have been one other one. No, I think that was it, so those three main documents, as well, that we want to also acknowledge and have an opportunity to have input on.

So we're going to begin with Dave Allen's presentation. And Dave was dealing mainly with the issue of external dose estimates for non-radiographers and the issue of job categories and also how the internal dose estimates would be carried out and used. So, Dave, why don't you highlight for us the issues in your paper, and then we'll proceed to SC&A.

MR. ALLEN: Okay. Thanks, Paul. Like you said, during the last Work Group meeting, February 21st, I was asked to do

those three things you just mentioned. It was the give our details of the external dose for non-radiographers prior to 1963 and describe how we would assign individual cases to the different job categories, which are, essentially, radiographer and non-radiographer, as well as the details on how we would use the data we already agreed to for internal dosimetry, exactly how we would use it for dose estimating.

The first one, the non-radiographer dose estimate, for that one, I started with the August 1962 survey of the radiography room, which was surveyed using our cobalt-60. The new cobalt-60 sources at that time were being exposed. From the hierarchy of data, the actual measurements are usually considered better than any kind of modeling, so I started with the actual measurements from the cobalt-60, but, obviously, it has to be adjusted to account for the differences between cobalt and radium and the source

strengths.

But, also, there's at least some indication that some shielding was added soon before that August 1962 survey. So I adjusted those survey readings up to account for additional shielding prior to that survey.

The indications, essentially, were the, it was a map that indicated 24-inch walls and a notation that said shielding added June and July of 1962. And then the prior drawing of that room was in the AEC initial application that indicated there were 16-inch walls. So from that, I took it as eight additional inches of concrete block shielding and the write-ups in those AEC documents indicated mortar-filled, so I indicated or I took it as eight inches of mortar-filled concrete block additional shielding added in June and July of 1962.

So adjusting those readings up for the lack of, the less shielding in the radium era and slightly higher source strength of the

radium, I came up with, adjusted those survey measurements to what they would be with the radium sources before the shielding was added. And from that, you can see from the rest of this, from that and the work practices source utilization time, et cetera, which I estimated a dose for somebody at the wall, on the outside of that radiography wall, if they were there all their work time, and that is the estimate we intended to use for non-radiographers in the radium era.

The next thing on there was how we would categorize individual claims into radiographer and non-radiographer. Like I said during the full Board meeting, we would start with the telephone interviews.

So what I did was took a search of all the claims we had from GSI so far, and I actually started with the job title that's in our claims database, which is the job title that the claimant puts on the forms when they originally filed the claim. And I put a list

in my White Paper of the types of jobs that we would flag initially as radiographers, or at least potential radiographers. That list is a short list. I took quality control; film reader; radiographer, obviously; inspector; anything that said betatron; magnaflux operator; metallurgy department; or x-ray.

From that, we had 284 claims in our database, and that search resulted in 21 claims matching one of those. But, as I said, we would use the telephone interview, and I did not go through all 284 telephone interviews as part of this exercise. But I parsed it a little by starting with those 21 to see what those telephone interviews said. I also look at telephone interviews for anybody that had a job title as unknown or some variation of that. And, lastly, I checked it against the names we had on the Landauer film badges for the later years, and I included anybody that names matched that, and that gave me a list of claims for which I

actually checked the telephone interviews.

That's not the process we intend to use when we're actually doing claims. You know, the telephone interviews are always reviewed, and they would be reviewed for those job categories or any other indications that they were doing radiography. This was just an attempt to see how this process would work.

Of the 21 claims we flagged from the database job titles, you could confirm 11 of them are definitely radiographers from the telephone interviews and the Landauer records. Ten of them we could not confirm, but that doesn't mean they weren't. We still intend to call them radiographers for the purpose of dose reconstruction. And I put a little bit of information that, you know, they may or may not be and why we would continue to call them that.

And then later on here, besides those 21 that were flagged from the database search, there were 23 in there with a job

title of unknown. As it turns out, when you start looking at the telephone interview, that's where people really tell you what they did and, often, even a job title. And when I say job title, it may be the actual job title at the work or it may just say he was a machinist or a welder or an accountant or something to that effect.

In any case, I went through what I found there. The vast majority of them did have some sort of information in their CATI interview, in their telephone interview about what job they did. We did end up adding, I believe, two after looking at the telephone interview, even though they were unknown job titles listed in our database.

And then, lastly, I checked those whose name matched the Landauer dose records. The primary issue I had there was the Landauer dose records were by last name, and some last names are very common names. For example, just to make up a name, I don't think

it actually shows up on Landauer, but if somebody had a last name of Smith, we would have a dose record for a Smith. And then if you searched 286 claim files, you're going to find more than one Smith in there. And that was the case we had for several of these, so it's not unexpected that most of those would not be considered radiographers. That was just to give me a list of claims to actually check their telephone interview for this exercise.

What we did find is we had 31 that matched the names in the Landauer records, and 11 of those had already been caught with previous steps. Two were added, but, again, it was based on the telephone interview, not just, it was not from the Landauer records but based on a telephone interview. We just checked those telephone interviews because of the Landauer records for this exercise.

And two of them, even though they had some other job title or an unknown, well,

not an unknown but some other job title, we ended up adding them or considering them radiographers because of their telephone interview. And I think, in the end, we ended up with 26 claims that we would have considered radiographers, and we can only confirm about 12 of those actually were. The other 14, there's some information in there indicating at least some of those likely would not, were not radiographers. But we would have included them, one because of just complete lack of, I think it was just one from complete lack of information. There was no information, no job title, no information what he did. There was just no information at all.

In any case, moving on to, lastly, the White Paper discusses the internal dose estimate. And, previously, we had gone through a couple of Work Group meetings and presentation for the Board of the data we intended to use for the air sample data, and the 95th percentile of that came out to be

68.7 dpm per cubic meter.

At the last Work Group, the Work Group wanted to see, okay, that's the number we're going to start with, but how are we going to use it? So in the White Paper, I started with that 68.7. I intended to assign that to anybody considered, well, actually, I intended to assign that for the time period that they would have been handling uranium. We have the hours of uranium work that we previously talked about, we estimated. And from other previous work with the external dose, we had a scenario on how long they shot this uranium and how long it took them to set up the next shot, et cetera.

I did not give them that intake or the time they were taking the shots. They would not have been in the betatron shooting room at that point. They'd be in a control room. I gave it to them for the time in between shots. And after that, that's just for the direct handling type of airborne.

I did also account for any contamination that would be caused from that airborne using the TBD-6000 techniques that we went through some months ago and, using those settling rates and settling time, came up with a contamination value of what it would reach after such a time as to build up to an equilibrium value and re-suspended that to get an airborne that I was intending to use for the times, actually for full time, which we're using 3,250 hours a year. So my intent was to use that re-suspended airborne full time for everybody's employment, the airborne from actually handling the uranium for the time that they would be in the shooting room setting up shots with the uranium, and then using TBD or, I'm sorry, TIB-9 for the ingestion. And we also, I believe, agreed, either during a Board meeting or a Work Group meeting, that we should use TIB-70 reduction of the airborne levels during the residual period after the operational period stopped.

And I just put a footnote in there about that, that we intended to decrease that or using TIB-70 values.

That is, essentially, what the White Paper says. I summarized some of that. I don't know if anybody wanted more detail or not.

CHAIRMAN ZIEMER: Okay. Thanks, Dave. Let me see, just before we go to SC&A, if any of the Work Group Members have questions. I'll ask a couple here, and then we'll see if others do.

We now know that 1952 is also included in the active period. Your chart doesn't include `52. What would you have in the chart for `52 on the year scheme?

MR. ALLEN: I'm sorry. That was just my neglect there. I would, the intent would be to continue the same thing back until, I think it's, if I recall right, it's October 1st, 1952. What I would probably do is prorate that uranium work to where, right

now we have hours per year, so it would be, essentially, a quarter of that for 1952, starting October 1st. That would give them the same intake rate per day starting October 1st, `52 through June 30, `61. And we would be doing the same thing with the external. We'd be getting it at the same daily rate.

CHAIRMAN ZIEMER: Right. Okay. So the inhalation from suspension would be the same value, the 1441, or not? That's per day, right?

MR. ALLEN: Yes, that's per calendar day.

CHAIRMAN ZIEMER: Right. And then the uranium work hours per year you would, that would, you'd have to determine what that is. Is that a quarter of a year?

MR. ALLEN: Yes, so it would be that number there divided by four.

CHAIRMAN ZIEMER: Right.

MR. ALLEN: It would result in the exact same inhalation and ingestion rate per

day.

CHAIRMAN ZIEMER: Right. Got you. So you end up with the 15.45.

MR. ALLEN: Yes, for the ingestion.

CHAIRMAN ZIEMER: Thank you. Other Work Group Members, questions?

MEMBER MUNN: This is Wanda. I don't have any real question, but I do want to call to Dave's attention the fact that on page seven you have a typo on the date when you refer to the Work Group meeting in the very first paragraph.

CHAIRMAN ZIEMER: December 12th, 2012.

MEMBER MUNN: It says December this year.

CHAIRMAN ZIEMER: It should have been last year's date, yes. Right. Josie or John?

MEMBER BEACH: Yes, this is Josie. I don't have anything right now.

CHAIRMAN ZIEMER: Okay. John? Okay. I'm not hearing John, but he may be on mute. Oh.

MEMBER POSTON: I turned my mute on instead of off. I don't have any questions.

CHAIRMAN ZIEMER: Thank you. Let's go on to Bob Anigstein. Start with your external exposure document, and then we'll do the other one separately. So why don't you go through, I know you handled a number of issues with the current NIOSH proposal, so I think you also, are you putting something on our screens for those --

MEMBER MUNN: Yes, he just did. Yes, it's up now. At least it's up on mine.

CHAIRMAN ZIEMER: Okay. Can you reduce the magnification so it fits on the screen? Did you put this up? Yes. Bob, I'm not hearing you. Are you on mute maybe?

DR. ANIGSTEIN: I was on mute. I have the screen on full screen. You should

see my title page.

CHAIRMAN ZIEMER: Yes. Actually, on mine it's way more than full screen.

DR. ANIGSTEIN: Say again.

CHAIRMAN ZIEMER: What's your magnification? Can you reduce it a little bit or --

DR. ANIGSTEIN: I'm using full screen. I'm not sure how to get different magnification at your end.

CHAIRMAN ZIEMER: Oh, I see. Okay. Fine. Go ahead.

DR. ANIGSTEIN: Okay. Well, I'm going to run through, we have a number of, as Paul said, we have a number of issues. So I'm going to start off. Some of it's a little repetitious, but I just want to give a quick framework of the time frame.

Now, I wasn't aware. I heard of the information Dr. McKeel had presented some time ago that the work started in `52. And as a matter of fact, SC&A maintained from the

beginning that it started in `52. But I wasn't aware that this was becoming official; so every time you see `53 here on my slide, include `52.

So, anyway, just a quick run through. It started off with two radium sources and the 24 MeV betatron, what they call the old betatron. Then in May `62, GSI acquired cobalt sources, small cobalt sources, and there had been orders to discontinue the radium used by the State of Illinois.

Somewhere late in `63, the new betatron began operating. We don't have the exact dates. I assumed October. However, NIOSH indicated that they would go with whole year, so you can say all of 1963 the betatron was in operation. That will be a limiting exposure during that time. And then June 30th, `66 is the end of the operation period, beginning a residual period. And I don't indicate here, but the residual period extends to 1993.

And the sources of external exposure, we start just with external, are, of course, the radium sources during the earlier period and then the exposure to direct penetrating radiation photons and neutrons from betatron operations, which is both the stray radiation from the betatron itself while it's on and the delayed radiation from the activated metal when the betatron is turned off. And then you have skin exposure both from handling the uranium, and the natural uranium itself gives you some beta radiation, and then that's much higher for a short period of time after irradiation because you have the short-lived uranium isotopes that are strong beta emitters. And then the second source is the activated steel, also beta emitters.

Here are the differences between SC&A and NIOSH. We've all agreed that radiographer will be represented by a triangular distribution with a minimum of about 6.3 rem; I'm rounding off. A mode, the

peak of the triangle, is 9.7 and then a maximum of either 15 or 12. We did some research. We had mistakenly, and SC&A takes responsibility for that, I should take responsibility, we thought that the new AEC rule lowering the exposures came in `55. No one ever contradicted that, but Dr. McKeel had asked for some documentation on that. So we did some research, and it turns out, no, the rule was adopted, was promulgated or made effective January 1st, 1961. So up until, starting somewhere around 1949 when AEC was actually not in the business of regulating radiation exposures, except in the government complex because they actually were not licensing anyone to use byproduct material outside of the government complex, but, nevertheless, they were abiding by an NCRP recommendation of 300 millirem, mR or millirem, they used the terms interchangeably, per week, which comes out to a maximum of 15 rem in a year. So this was for their own

operations.

And then in February 1957, they issued the first 10 CFR 20, which was a rule that applied to all licensees, again, 300 mR per week, which amounts to 15 rem per year. So this would affect the limit from `53, or `52 if you will, through 1960.

Beginning with 1961, 1961 and 1962, the two years of the radium era, the limit was 12. So the same triangular distribution, except, I mean a similar triangular distribution except with an upper limit of 12 instead of 15.

And then from `63, and NIOSH has agreed to give it for all of, to use it for all of `63. Our analysis is that the layout man should get 9.2 R per year, and NIOSH, I'm just using betatron as a source and the layout man is the same scenario, is about 4.5. And, also, the major distinction is that SC&A believes that the radiographer doses during the radium era should apply to all employees,

whereas NIOSH has two different calculations for radiographers and for non-radiographers.

This is quite different than in the, shall we say, new betatron era where NIOSH had agreed and we understood that that would also apply, and, apparently, it doesn't, to the radium era that whatever dose, whatever was the most claimant-favorable assumption, that everyone was either a layout man or a betatron operator, whichever was most claimant-favorable in a particular instance, in a particular claim, usually it would be the layout man, would get that dose. So we were rather surprised when NIOSH indicated they would treat the radium era differently.

And then reasons for our disagreement. The scenario that Dave Allen just presented, and let me show you a quick picture. I'll go back and forth. This is the drawing, actually part of the license application -- no, this was already after -- I'm not sure when this was. I think it was

part of the license application or after the license application when they were -- I'm contradicting myself now -- when they gave the results of a survey. They show that the scenario that Dave predicted or postulated where you would have someone standing right outside the wall is actually unrealistic because they clearly indicate that these were areas used for storage of drums. There was no access to the building on either the, I think this is north to south, either the north or the south wall are not accessible. So that's not a realistic scenario. And then neither is the east wall that the nearest workstations would be 20 feet away, 15 to 20 feet away to the nearest wall. And that would be at the end wall, so that's actually further from the sources. The sources are postulated to be in the middle. So that scenario simply does not represent any real person.

Also, we questioned the idea that bricks were added, according to information

supplied by a co-petitioner that the building was put up in 1955. So, first of all, that scenario would not apply then for `52 through into `55 if there was no building.

Secondly, there was no additional shielding. The additional shielding that is indicated on the drawing -- remember that drawing that Dave Allen referred to, similar to one, not the same one, was furnished by the nuclear consulting company or corporation. They were consultants who came in. They took information they got from GSI. They did not -- their job was to make radiation measurements, so they were not privy necessarily to the history of this.

My conclusion is that one of the radiographers, the only radiographer that was active during that time who's still available to be interviewed, said that steel was added, this steel shielding. That was added at the time they started using cobalt or just before because it was necessary to shield the -- you

know, during the use of cobalt, here were the steel shields, here were the control cables.

So they had a very safe operation where the operator stood behind a steel shield and manipulated these cables, just, you know, long wires that would turn and crank the sources in and out of the lead shield, whereas before, during the use of the fishpole, that made no sense because you can't stand behind a steel shield. You have to stand right there where the casting is to put in the fishpole because they didn't have those steel shields.

And that, I believe, this armor plate is what was put in during this period of time and not additional brick work which -- I won't go into all the details in my report -- would have made no sense. It just wouldn't have made sense because they had already done a calculation to show that the 16-inch concrete was sufficiently protected and met all the regulations. So it would have made very little sense for them to have submitted

that application or granted the application, purchase the cobalt sources, and then said, oh, by the way, we're going to add more bricks. So I don't think that's a realistic scenario.

We believe that the only scenario that you can hang your hat on is the limitation, and we agreed to the triangular distribution, that no one got more doses than the radiographers. And that is the only plausible bounding number that can apply to all workers.

We don't know where the other workers were. We know there were incidents, for instance, two cases where two individuals who were not radiographers. Therefore, they were unmonitored. Two separate cases. One was inside an army tank in the betatron room while it was being radiographed. Nobody knew he was there, and he didn't realize the betatron was on. And somebody else was also in what's called in the betatron. We don't

know how many other such incidents there could have been.

We also know that the fishpole technique was notorious, was known to be unsafe. The State of Illinois has banned it. I did a search, and every state that mentions it simply says it cannot be used or only under special circumstances with special permission.

So all in all, without giving every -- I mean, I have more detail in the report. We do not believe that the assignment of the calculated dose that they've allocated to non-radiographers is scientifically justified, nor claimant-favorable. It was always our understanding that the same dose, just like with the betatron, that the same dose that is given to the -- everybody gets the worst case. You don't have to worry about what his job was, where he was, where he spent his time. It's unlikely that anyone would have gotten more than these doses.

Going on to, although this was not

in the most recent report, this was discussed at a previous Work Group meeting. And there was a question raised, which I didn't get a chance to answer it. I was under the weather, so I wasn't thinking too clearly. But to summarize the differences between why we have these differences to the layout man between SC&A and NIOSH is NIOSH used 15 betatron scenarios. They started off with modeling 15, and then they selected on the basis of -- perhaps, arbitrary is the wrong word. I understand how they used. Some of the scenarios were simply not realistic. You don't shoot at a 45-degree angle to penetrate the steel. You always shoot at the shortest path through the steel.

Also, for the position orientation, which was something that was made up -- I don't mean to sound disparaging because it was a range of possible things -- the main objection is this normalization that it can't be more than 10 mR per week. Now,

this is the picture of a new betatron building based on an account of one of the radiographers. Here's the betatron shooting room. The betatron, this is our scenario. Here's the picture. Here's the betatron itself. Here is the casting that we hypothesize is like a typical casting, representative casting.

And we put the layout, we tried two positions for the layout, and we found that this was the most claimant-favorable to get the highest dose. So this was the only thing that we modeled at this time.

Now, NIOSH -- so here would be the same thing, the betatron would be here, the casting would be here, and NIOSH calculates the doses to the film badges. Well, there were several things wrong with that.

First of all, they borrowed, we shared the MCNP model. Well, the initial MCNP model that we ran back in 2008, to make it claimant-favorable, we were calculating that.

We weren't thinking of this type of scenario. We were calculating the doses to the operator in the control room and absent knowledge of the walls. The only thing we had to go by then were the FUSRAP reports, so we didn't know really what these walls were like here, the side wall. So we made them thin and lightweight to make it more claimant-favorable to have a higher dose.

Since then, we got the FOIA material from NRC where there was much more detail, and it turned out that these walls actually were heavier. They were filled with mortar. They were not hollow. And, therefore, and assuming -- and then we also tried to match the later survey reports from the large cobalt-60 source. The nominally 80 curie source was more like 50 curie by the time they did those measurements. And we saw no way could we match those if we used thin walls. I think here and there we assumed there were thin walls. Where we used the

thicker walls, the ones that were described, you came closer. You came a lot closer.

So in this instance, the thin walls are not claimant-favorable because they assume a lot of scattered radiation that gets on the film badges and says, well, if the film badges never got more than 10 mR for a whole 168-hour week, then we're limited in which shooting scenarios are possible.

And that's mistaken for two reasons. One is the walls were too thin. They basically modeled this whole area as empty space. Now, this area was filled with furniture, all kinds of equipment which we don't know, of course, what had been the details of. And so, therefore, it's incorrect to say the radiation was coming but no attenuation from here to there and also through the thin wall.

And then the final assumption, and this was an understandable misunderstanding which we clarified by having our consultant,

who was a former Landauer official, and he was in contact with the current vice president of Landauer who does go way back and does know what was, dug up the records.

At that time, they supplied, as is in the film badge records -- every film badge record weekly reports has a control badge numbered zero. And the NIOSH assumption was, well, if that badge always shows M, minimal, which it does, it means that it's under 10 mR, that would mean for other badges it could be under 10 mR; therefore, it could not have been important. That's not correct. It turns out that their practice was to take that reading on that badge and subtract it from all the other badges, including itself. So that badge was, by definition, always zero on the report.

The only time they would report an actual reading for that badge was if the raw reading was more than 50 millirem or if it was higher than one-half of all the badges issued to the workers, if it was a higher reading

than the lowest half of the badges. Then, and only then, would they notify the client, hey, something is wrong there, you're keeping your control badge in a high radiation area, and that questioned the validity of all the readings. But since that never happened, we don't know anything about the control badge. The fact that it said M cannot be used in the model.

Now, there was another badge that was called betatron CTL, badge number one. We have no information on where it was kept. One person that was a former employee that was interviewed by one of the -- well I can't mention his name -- said he distributed the badges and he had no recollection of any control badge. So even on his report there was a beta -- I misspelled it, betatron. Put in the T here. Sorry. There's no spellcheck on this.

That could have just as well been kept in the old betatron building because we

have information from a supervisor, former supervisor, no longer with us, who said his office was in the old betatron building. So even though the film badge rack was here, maybe that second badge was kept there. We just don't know, and, not knowing, you can't use that, that information. And, again, we have problem with the model, even if it worked, to be kept in that betatron building.

More minor problem is NIOSH assumed that the worker, the layout man, was here dead center on the railroad track. First of all, that's unrealistic. He'll be blocking the rail tracks if he had his casting there, so castings couldn't move in and out. But more important, that actually was not the worse position. We modeled this position and also one, a symmetrical one, on the other side of the railroad tracks, and it turned out this is the highest one because it's actually lying outside of the betatron, you had this ribbon door so you could not literally see it, but

that it showed negligible shielding and the beam strongly focused forward. But, nevertheless, it trails off, but not to zero. And at this steep angle, you still get some direct radiation. So that's another reason why we have a higher dose, you know, the 9.2 instead of the 4.6.

And then there was other things. They included a door, heavy door. 0.85 inches, two centimeters, heavy-steel door in their model where the worker described as a sheet metal. And then the reason for the difference in the beta dose is they use, actually, SC&A results, and this was brought up before. I'm just mentioning it for completeness. We used a very early, one of the earliest releases of the MCNPX that did this activated metal. And since then, there have been improvements in the model. They said it was a beta model, nothing to do with beta, the beta particle, you know, alpha, beta, gamma, in terms of testing. It was a preliminary experimental

release. And since then, they have the final release, and that one, which they've improved the code and that one gives much higher beta concentrations of the beta-emitting nuclides activated in the steel.

Okay. And then here's just to round out the picture. For the photons exposures, our greatest concern that we see that even for the neutron we have approximately three times the exposure rate, dose rate, as NIOSH calculated. And the beta dose, depending on what year because of the different mixes of uranium and steel during those times, we go as high as three times on the beta dose and five times through the other skin.

Okay. Perhaps I should stop now and ask for questions because now we're going to a different topic. This has all been about direct external -- Paul, what should I do? Should we just continue?

CHAIRMAN ZIEMER: No, this is

probably a good point to ask for questions. So let's do that. First let's see if the Work Group Members have some questions. Josie, John, Wanda?

MEMBER MUNN: Well, this is Wanda. One of the questions that comes to mind, listening to Bob's presentation, has to do with the use of -- can you go back one slide to the one that you were looking at before? No, no, the one where you were talking about the -- yes.

DR. ANIGSTEIN: This one?

MEMBER MUNN: Yes, right, the MCNPX version that was used.

DR. ANIGSTEIN: Oh, yes. That only affects the beta dose.

MEMBER MUNN: Yes, but the beta dose is important in the --

DR. ANIGSTEIN: For skin, for skin, it's very important.

MEMBER MUNN: Exactly, and what we have going on right here. My question has to

do with whether this difference in the versions that were used that's been discussed, does NIOSH have a rationale for using that preliminary version?

DR. ANIGSTEIN: Well, my understanding is they didn't use that version. They used our results, the results that -- we shared our runs with them back in 2008.

MEMBER MUNN: Right.

DR. ANIGSTEIN: And they used those, those, those runs because that's all that was available then. Two years later, when the final version came out, we re-ran it, and we did a comparison. We showed a much higher activation of the beta-emitting radionuclides.

MEMBER MUNN: Well, I'm probably not formulating my question properly, I guess; and it probably needs to be addressed to NIOSH. I really have some question in my mind as to what sort of discussion and whether any adjustment was made following this use of the

later, of the final MCNPX. I guess I really should be asking NIOSH that, rather than you, Bob. I just --

DR. ANIGSTEIN: Okay.

MEMBER MUNN: -- can't --

MR. ALLEN: Wanda, this is Dave. I think we discussed this one in the Work Group back when we were discussing the issues with the SEC petition.

MEMBER MUNN: I think we did, but I'm trying to remember what was said. It raises another issue, I mean it raises another question in my mind, and I couldn't remember what we said.

MR. ALLEN: Probably because there wasn't a whole large discussion. We agreed with SC&A. As I recall, Version 26E was just in its infancy as far as this technique, and then they found some issues with it, revised it, and the revised version gives a different number and everyone agreed the revised version with the correction should be the one used.

MEMBER MUNN: And the key question for me is always how significant is that? My assumption is that it's not truly very significant.

DR. ANIGSTEIN: Oh, it's a three- to five-fold difference.

MEMBER MUNN: Three- to five-fold difference for how many cases at GSI?

DR. ANIGSTEIN: Oh, I have no idea how many skin cancers there were.

MEMBER MUNN: Okay. Just wanted to get a feel for what impact that had.

DR. ANIGSTEIN: But if Dave said that NIOSH will make that adjustment, then the question is moot.

MEMBER MUNN: Yes, it seems to me that it is.

DR. ANIGSTEIN: Okay.

MEMBER MUNN: All right.

CHAIRMAN ZIEMER: Okay. Other questions? John or Josie?

MEMBER POSTON: No, I'm fine.

MEMBER BEACH: I'm fine, as well.

CHAIRMAN ZIEMER: Okay. Now, it seemed to me, because we only got this material a couple of days ago and I think the petitioners probably only got it yesterday or pretty recently, and I don't know if NIOSH has had a chance to review the SC&A material in any depth. Dave or Jim Neton, do you have any sort of responses on the SC&A paper at this time?

One of my concerns is that there may, you know, if we've gotten this material very late and some of it I ended up reading this morning, but it seems to me that, before we can resolve some of these differences, that there may be a little more time needed. I'm thinking in terms of scheduling of their meetings in a few weeks, unless NIOSH is ready to respond at this point.

MR. ALLEN: I think we're ready to respond, at least, you know, like you said, there wasn't a lot of time, but I think we can

answer most of the issues raised by Bob.

CHAIRMAN ZIEMER: Okay. Are you going to do that, Dave?

MR. ALLEN: Yes, I'd like to start it.

CHAIRMAN ZIEMER: Okay.

MR. ALLEN: Starting back at the beginning there with the building 6 dose rates outside, I think Bob said he didn't feel there was any reason to believe or it wasn't credible that bricks were added, as far as shielding, to the building 6 radiography room. And I think he put "illogical" in his write-up on that.

The first thing I wanted to point out is we started with the cobalt-60 measurements, and then we adjusted them up for a slightly stronger, and we adjusted them up again for shielding that was added. If there's no shielding added, then our estimate is simply too high.

DR. ANIGSTEIN: I agree, but I

just, I'm pointing out it's just unrealistic. I agree it is too high, would be higher. But it's high, not low.

MR. ALLEN: And it seems illogical that you basically said that, because we accounted for shielding that was added that you said we're not sure was added and that workers were not necessarily next to the wall where we placed them, that our estimate is, essentially, too high again; and, therefore, we should use the radiography dose, which is considerably higher. That seems very illogical to me that you would, your resolution would counteract your basis.

DR. ANIGSTEIN: Our opinion is that this scenario is simply unrealistic and not scientifically correct and cannot be used, whether it's -- it cannot be used as a basis for dose reconstruction because that's one of the requirements of the Act that we're supposed to comment on is whether it's scientifically correct. And since it's a

completely, I mean, it's basically something that's made up, and I don't mean to sound pejorative, but the reason for giving the high doses, the radiographer doses is that is the only scenario that we have some reasonable assurance and actual agreement among all the parties that these are bounding doses. Nobody can get higher than that. So if we go with that, we say here is something we can know.

The other is there is a million possibilities of what about, what about some inadvertent exposures, what about the man who took the radium source home, which we now know really did happen and there was a credible account, and I guess perhaps we incorrectly questioned whether that really happened. There's pretty concrete evidence. So we're simply saying that the radiographer dose gives you a broad enough umbrella that will cover all of these unknowns.

John Mauro, do you want to sort of weigh in on this? Because you had some strong

opinions on it, also.

DR. MAURO: Yes. We almost have a philosophical difference. What we're really saying is that, to try to mechanistically model other scenarios, other than the triangular one, puts us in a place that's very difficult to do. I understand that you have done your best to parse people, that you felt these 22 were the number of people. You know, we're pretty confident that it's reasonable to assign the high-end doses to those guys, and even that may be pretty high, you know, because in the triangular distribution, we get up there pretty high.

And so we have no dispute that you picked a good group, you picked a good distribution for the high-end exposures. But then the philosophy goes, okay, but now we've got these other 200 people, and the sense is that we now have another way to assign doses to them and it's something different and substantially lower than the other one.

So we're sort of caught in a difficult spot, the spot being we're not comfortable with the fact that you have this other group that we can say that, well, they're less likely to have experienced the high-end doses because of the job categories, but we're not that sure because of two things. One is we don't really know where they were, how long they were, and what they did. So what you've done is say, well, we're going to hypothesize that they were here for this time period and assign to them that dose. That's a construct to somehow find a way to deal with these other 200 people.

And in my sense, and, again, this is not really science now. What this is, is what I would consider to be where science and policy come together and some prudent judgments have to be made. And the way I see it is we're in a difficult spot, and I respect and understand why you want to make the parse where you made it. But I'm not sure if you

really can, and I come down in a place that says, given that we really don't know what to do with these other 200 workers, what do you do? Do you assign to them this number, this construct, which has certain limitations that we discussed and limitations that may be, where, as we just said, they may be, for that construct, the degree to which it actually exists and we know who they are, might actually be too high. So we're in a funny place.

In my opinion, I like to keep things simple and say that, well, I have a different way of looking at it. I'm saying we have all these workers that were somewhere involved, and I use the term the radiological envelope where people were coming and going and may have been here, may have been there. They may not have been radiographers. They may have been welders. They may have done this job, and they may have done that.

And so we can't really get at this

thing mechanistically, but we can say with confidence that this upper triangle captures it all. And in a way, it probably is extremely claimant-favorable for an awful lot of workers, but I see no other alternative, unless, of course, as we mentioned in our last meeting, there's affirmative evidence that, no, this person, this person was administrative and spent just about all his time, we know that, out of this thing that I'm calling this radiological envelope.

So it's almost like a philosophical difference on how to deal with a difficult circumstance. And SC&A's position is I think that, and it's a judgment call, me, I would go with the upper-end triangular and apply it to everyone and the rare individual that I could say with confidence, no, it just doesn't apply to that person. Then I could see going to some lower number. What that lower number is I don't know.

I know I'm going on a bit. So I

see that the majority of those workers, these two hundred twenty-something workers, the steps should be we're going to give them the upper end unless we have some affirmative reason not to.

DR. NETON: John, this is Jim. I think you're starting to get towards where I think our position really is, and that is if it doesn't, you know -- we normally do not provide this high dose to all workers if there is evidence that, you know, they were clearly administrative in nature for the entirety of their career, and there's precedent set for this. We've done this in TBD-6000 --

DR. MAURO: Yes.

DR. NETON: -- which has been vetted and agreed to that there are different Classes of workers, such as supervisory or plant worker, that sort of thing. And maybe the issue here is, in Dave's example, we tried to be a little too fine-tuned with the analysis. But I think you would agree, and it

sounds like you would, that there are certain categories of workers that they have never really entered the plant much at all, such as secretarial or accounting type folks that may have traversed the plant but not have worked their entire time in there. And to give them this very high-end triangular distribution seems to us to be not reasonable.

DR. ANIGSTEIN: Well, Jim, it's Bob. See, I'm thinking along those lines. See, if that were the case, and we even discussed this at the last meeting when you said, you know, Bob, how about give them zero, and you said, no, no, you can't give them zero. Well, this would have been a judgment that should have been made perhaps by DOL that only a certain category of workers are even considered, even fall under EEOICPA.

DR. NETON: No, no, that's not the way it works, Bob. All dose reconstructions are sent to us. And if a dose reconstruction is zero, it's zero, and we would return it

that way. I mean, we get dose reconstructions for places that had no radiation exposures, and the doses come out with almost zero, maybe medical exposure. So our jurisdiction is to take the Class of employees or the eligible employees at that site and reconstruct their dose, whether it's zero or 15 rem.

DR. ANIGSTEIN: But I'm saying, perhaps I'm misunderstanding, perhaps I'm misunderstanding the policy under the Act. But it would seem to me that the Act, that if these people are covered by the Act, and maybe, you know, again, this is not my place at all, but maybe they shouldn't have been. Maybe there should have been two categories of people, and the only employees that are eligible were those with known contact with radiation --

DR. NETON: No, no, Bob, that's what dose reconstruction is all about: to decide which one had high exposures and medium exposures and low exposures in some

quantitative fashion. That's what we do.

DR. MAURO: Jim, I'd like to go back to where you're coming from because I like where we're going with this conversation. And I think the difference in thinking is I look at it, I have 228 -- I think that's the exact number, 228? Is that the right number of claimants?

DR. ANIGSTEIN: Two hundred eighty-four. Two hundred eighty-four minus twenty-seven.

DR. MAURO: Okay, whatever that number is. I can't do it in my head.

DR. ANIGSTEIN: Two fifty-seven.

DR. MAURO: See, I would look at it, I'm going to give it to all of them unless I have reason to believe I shouldn't, as opposed to the other way around. I, right now, have some evidence that I should give it to these 28 and the rest not and only because, and I would agree with you that it's only because of this site, because of the

circumstances that we've encountered at this site and for reasons we don't have to go into right now because we've discussed it many, many times.

So I would come out and say, listen, I'm going to give it to everyone, except for those ones that I can say, you know, it would really be ridiculous to assign this dose to this person and we have affirmative reasons for believing that. And I don't think that's what happened.

Now, what we would give these other people, let's say we could do that, let's say we went through some exercise and you could go through and you could say, listen, I can say with a high degree of confidence on a case-by-case basis that these are the conditions under which I would not assign the upper end and here they are, here are the people. And then you would say and, because of that, here's the dose I would give them.

Now, whether it's the dose that you folks derived, it sounds like there's some difficulty with that approach because, you see, the other dose that you're applying, as far as I'm concerned, they're still within the radiological envelope. And so I say to myself that model that you've come up with for assigning some other doses, you know, to me, if you're in the radiological envelope and you're wandering around in the facility where there is the fish poles being used and there's these kinds of exposures maybe occurring, the person falls in that box. But if you could say that, no, they're more administrative, secretarial, and there's good evidence to that effect, these are the ones we're going to cherry-pick out and assign something else, which may be what you would call an ambient dose. I'm not even sure what it would be.

So it's just a different way of coming at the problem. I think, fundamentally, we're in agreement, and the

problem is how do we draw that line?

DR. NETON: I think we are in agreement, and I don't think that Dave's calculation is incorrect. I think it's a reasonable approximation for what these folks received. It happens to be about an order of magnitude of 10 percent of the middle dose that's assigned to the radiographers.

MR. ALLEN: Okay, okay.

DR. NETON: And that precedent, and that wasn't designed that way, that's just the way it came out, that is, often we use that in other situations where a person that was not a so-called process or production worker would receive 10 percent of the production dose. So it kind of fits in that envelope. I don't see that it's necessarily an inappropriate dose to assign.

DR. ANIGSTEIN: But what do you do with the guy who was a radiographer and he was sitting outside the tank? There's one case a name was supplied, in another case there was

no name given.

DR. NETON: No, no, I'm suggesting --

DR. ANIGSTEIN: We won't know. When you're doing a dose reconstruction, you won't know who that person was.

DR. NETON: Bob, I'm suggesting that we don't parse it that thinly or that finely. We just say anyone who was a production process type worker would receive X dose, the high dose. And people who clearly fall in this administrative type category would not.

DR. ANIGSTEIN: Okay. That's not the way Dave explained it.

DR. NETON: I understand. I started off by saying --

DR. ANIGSTEIN: Okay. That sounds good. I can go with that. Another thing let me just throw out off the top of my head, there's also, in the AEC regulations two categories, as I'm sure all of us health

physicists know but it was different in those days, the two categories of exposure. There was exposure limitations on people who were inside the restricted area, meaning the posted area, nobody is allowed here without a film badge, and also the unrestricted area. The unrestricted area, at that time, at least in the 1961 rule, I'm not sure about the earlier rule, was 500 mR per year.

So it would seem reasonable to say, well, if they're observing good radiation practices, nobody got more than 500 mR except the people who were directly involved with radiation work. So that would seem to be -- I'm just throwing it out to --

DR. NETON: That doesn't sound like a bad idea, except then you get into this argument, well, did they really exercise good control practices? And you take Dave's calculation, and it puts an upper bound on someone who even was not --

DR. ANIGSTEIN: If you apply that,

if you applied that, that to the administrative workers, I have no problem.

DR. NETON: Well, I think we're in agreement here then. I think that's, I don't see any --

DR. MAURO: I think we've come to the nub of the issue, and that is parsing is fine, but it was sort of surprising to me that only 28 workers out of the two hundred and whatever would fall into the high-end category.

DR. NETON: Yes, and I hear you, and I'm thinking about this even with the ones where the CATIs put them in one bin or the other. It's difficult to say that a person didn't actually do some radiography at some point, you know. Mostly, we have the current job title, and you can go back a few years, but I'm more comfortable with the split with administrative versus what I would call production or process workers.

DR. ANIGSTEIN: Me, too.

MEMBER BEACH: And this is Josie. I have a question. This will be for Dave Allen. If you look on page four of your report, Dave, it talks about the dose limit of the triangle of the higher limit 12 and then 15 in 1953 to 1954. Is that the cutoff date there or are we going -- I was under the impression it would be through `62 for that higher end.

MR. ALLEN: That's not going to be the right cutoff date. From what I looked up, I thought it was 1958. From what Bob put in his paper, he's saying, I believe it was 1960.

DR. ANIGSTEIN: Yes, that was my fault. It was actually `55 that we had said earlier, and that was my error. And I have the actual, if anybody wants it -- as a matter of fact, it's in my report in the references, the actual -- it just so happens that we have a lawyer who's one of our associates who's also knowledgeable in radiation and health physics who used to work with us, and he was

able to dig up these rules.

DR. MAURO: I think we're in agreement that, whatever the rule was that is applicable based on historical records, we're going to go with that. Right now, it's our understanding it was right up to what? 1960?

DR. ANIGSTEIN: January 1st, 1961 when the 12 rem came in.

DR. MAURO: Now, if it turns out you believe it's something different than the 15, it's something other, and you have records for that, I mean, we're making our case that we think it goes to that. So, yes, Josie, we're saying that we think we should change that date from -- what was it? That would be before --

DR. ANIGSTEIN: It was `55 --

DR. MAURO: We're saying we think --

DR. ANIGSTEIN: -- through `60.

DR. MAURO: `61.

DR. ANIGSTEIN: No, through `60.

Through 1960. January 1st, `61 is the lower --

DR. MAURO: Right. Certainly, if NIOSH goes into this issue, because I got to say we didn't look that closely at it. We should have. Now, when they look at it closely, if you see something different, you know, we could always talk about that. But I think we're in agreement --

DR. ANIGSTEIN: I'll be very surprised if they saw a different Federal Register than we did.

DR. MAURO: Listen, I'm ready for anything. But I'm saying that the philosophy, though, is whatever is determined to be the applicable bound and the year in which that occurred and when it changed, I think that we both agree that's what we're going to use as the upper end of our triangular distribution. Now, we believe it's through 1960. If you find that it's something different, then, of course, we need to talk about that.

So, Josie, I think the answer to your question is it is our, SC&A's recommendation that, as best we can tell, it looks like that 15 should be pushed up a little bit from the `55 date up to 1960.

DR. ANIGSTEIN: Through 1960.

DR. MAURO: Through 1960. There you go.

MR. ALLEN: Yes. And this is Dave, and I agree with them. The 10 CFR 20 is the controlling standard when it's in effect at the time. You know, assuming that's what it says, then, yes, we'll push it up through 1960. I had NBS 59 out, and then I realized where the error between the `54 and `58 came from. There was an amendment that changed NBS 59 to, essentially, 12 rem per year, but they left the `54 cover page on it, which confused --

DR. ANIGSTEIN: No, what happened was AEC took three years from the time they suggested the rule to actually promulgate it.

MR. ALLEN: Right. So, like I said, we would agree --

DR. ANIGSTEIN: The NBS Handbook came out earlier.

DR. MAURO: I think we've got this part solved, and I think the only, the place that's really the -- we'll get the internal in a second. But, I mean, from an external point of view, I think we're there. I think it really becomes a matter that, as Jim said, to maybe take a closer look at where does that split really occur? Is it 28 people, or is it something bigger? In other words, is the tent going to be a little bigger for putting the people into the upper-end distribution? And, of course, this is going to be a judgment call, and I believe that, in the end, you know, we're all going to see it maybe a little differently and we're going to converge, as we always do. We try to, anyway, converge on, okay, I think we've placed in the right place.

Right now, our sense is it's not

in the right place, you know, where that cut is occurring. It just doesn't, you know, the nature of the work and the classification of the workers. If you folks are willing to go back and take another look at that, maybe make it a bigger tent, and be comfortable with it, I think that we're on our way to resolving this.

CHAIRMAN ZIEMER: Okay. Let me pose a question for NIOSH at this point. This is Ziemer again. And either Dave or Jen, I think what I heard that NIOSH would propose is that you would not use the triangular distribution if you had, basically, solid information that confirmed that the person could not have been in the radiological area, such as a secretary. We're talking about before, if that group, aside from everybody else in the plant and under those conditions, are you saying that everyone else in the plant would get the brand new distribution --

DR. NETON: This is Jim. That's

what I'm saying, that we would make the dividing line, what I would call production process workers versus administrative personnel.

CHAIRMAN ZIEMER: Now, can that actually be done in practice is my question? Because it's not unlike the question we had even at GE Cincinnati where we said is there a way that that could actually be identified?

DR. NETON: Well, this is a little different, Paul, in the sense that we're not saying that they didn't enter the radiological area. We're saying that we can bound their dose if they did using Dave's calculation.

CHAIRMAN ZIEMER: Yes, well --

DR. NETON: We don't need to necessarily say they never entered the area. We could say that, if they did, they certainly weren't doing radiography. And if they weren't doing radiography, then this approximately 900 millirem or milliroentgen would bound their exposure per year. And, in

fact, we do this. Again, we have different categories of workers in TBD --

CHAIRMAN ZIEMER: Right. Well, yes, I'm not disputing that. I'm trying to understand how it would actually be administered in terms of doing it. So if there was any doubt that the person was somehow outside basically most of the time, they would be assigned the higher dose.

DR. NETON: Correct. They would be given the benefit of the doubt and claimant-favorable assumption and be assigned the higher dose.

CHAIRMAN ZIEMER: Right, right. So it comes down to that issue, Allen described it as the size of the tip. Yes --

DR. NETON: Well, I suspect that this is going to enlarge the group tremendously, you know. If you were in the process production area, you'll be assigned the high -- I don't know what fraction of the people will show up as administration, but I

suspect it's a fairly small percentage of the total workforce. I haven't looked at it.

CHAIRMAN ZIEMER: So are you proposing that you would go back and kind of do the reverse of what Dave did and say can we find from either the CATIs or the job description people who definitely were not in the radiological area for --

DR. NETON: I had intended to do that as an exercise. I mean, I was trying to describe the approach we would take in dose reconstruction, but if --

CHAIRMAN ZIEMER: Well, I think I'm asking do we know we can even do it? And if we can't, then it defaults to the larger group.

DR. NETON: Well, we just don't know. Well, it would, but, in practice, though, I would still like to maintain that option if we, you know, if it's, if the data are there and we have job categories of people who, for instance, were secretaries their

entire career or during their employment period, we want them to have that option to assign the lower dose. That's all we're saying. I mean, it just doesn't make sense to take someone with a job category of secretary, or maybe accountant, or draftsman, and say that they received up to 15 rem exposure every year of their employment. It just doesn't make sense to us.

CHAIRMAN ZIEMER: Right.

DR. NETON: We can go back and re-look at the job categories, but I think what we would do is very much like we've done in the past at many other sites. This is not unique. This is not something that we're proposing that is unique to GSI.

CHAIRMAN ZIEMER: Right. Well, I want to ask one additional question. I'll ask SC&A this question.

DR. ANIGSTEIN: Tom, I have another --

CHAIRMAN ZIEMER: Hang on, Bob.

Hang on just a minute. Here's the question: so if this approach were used, I assume SC&A still has some issue with that actual calculation in terms of the dimension or the shielding or the actual value --

DR. ANIGSTEIN: Yes, that's what I was just going to get to. We had done in an earlier report a calculation of the dose using the actual radiography room to someone standing just outside the door with the radium sources exposed, and the difference being this would not have been caught by the later survey because the steel shields had been installed. Now, prior to installation of the steel shields, you had a clear path for radiation from the radium source in the middle of the room going right through a very thin door, typically a hollow steel door total of an eighth-inch or quarter-inch of steel, someone outside. And there we calculated, based on the 30-percent occupancy, which was something taken from the NCC assumption, that there

would be 2 rem a year, 2.086, if I remember correctly. And that's based on a direct calculation, and I think it's a little more defensible. I mean, obviously, it sounds like I'm promoting it because we did it, but it seems to be a little more defensible. And they could change the occupancy factor to whatever seems, you know, that's a judgment call. That's not a physics problem.

CHAIRMAN ZIEMER: It would be different for people who were plant workers versus the casual --

DR. ANIGSTEIN: Well, this would be a casual person. I mean, you could hypothesize that a casual person would have walked up to that radiography room, and this would have been, by far, the most highly-exposed location just outside the door, steel door, as opposed to outside the thick sand-filled bricks. We didn't know what fraction of the wall was actual concrete and what fraction was sand. We modeled it as if it

were just sand because sand has a lower density than concrete, so, essentially, we made sand walls. But that doesn't really matter because we're looking at the radiation through the door, so the walls really have nothing to do with it.

That's a suggestion. I'm not saying it's a policy statement. It's a suggestion because before actually NIOSH had adopted that, and I said, well, no, we did not intend for this to be the definitive dose for all non-radiographers. But now that you're limiting the Class to whom it would apply, that would seem to be a good starting point, and we'll be happy to share the MCNP files if you wanted to check them. And this is a very sample calculation. It could be done with any version of MCNP, just the direct photon radiation.

CHAIRMAN ZIEMER: Okay. Thanks, Bob. Let's see if there's other questions.

DR. ANIGSTEIN: Hello?

CHAIRMAN ZIEMER: Yes, still there?

DR. ANIGSTEIN: I heard a beep.

MEMBER BEACH: Paul, this is Josie. I don't have any questions right now.

CHAIRMAN ZIEMER: Okay. Now, I want to do one other -- we hadn't made a final decision on this, but I know that Dr. McKeel had some -- I'm hearing some noises here, but Dr. McKeel had some issues with Dave Allen's presentation and maybe some issues with some of the shielding issues, as well. Dan, I'm going to give you an opportunity, if you want to comment at this point, on this external dose issue.

DR. MCKEEL: Yes, I'm muted. Can you hear me all right?

CHAIRMAN ZIEMER: Yes, go ahead.

DR. MCKEEL: Okay. Well, I do have some comments. I guess the first points that I want to make that I think has been totally overlooked in this morning's entire

discussion, and that is that the scheme that NIOSH seems to be near agreement upon that there will be two classes of workers under this new plan, which I assume we're all in agreement we're working toward a revised Appendix BB.

CHAIRMAN ZIEMER: That is correct.

DR. MCKEEL: Do we agree with that?

CHAIRMAN ZIEMER: Yes.

DR. MCKEEL: Okay. And under the old Appendix BB, there was also a two-level plan there. It didn't include office workers, but it included, primarily, radiographers, betatron and isotope, et cetera, versus non-betatron, isotope radiographers, so all the other people in the plant. In that scheme, that would have included office workers.

And the fourth paper that I sent you all in the wee hours of this day, which I thought was extremely important, was to point out that there was a meeting held by NIOSH, a

town hall meeting, October 9th of 2007. And in that meeting, Dave Allen made an extensive presentation of the facts that -- and this was in the period, this was October 2007. I have information from Laurie Breyer that somewhere around two-thirds of the dose reconstruction done at GSI had already been completed. And Dave Allen made the statement over and over and over in many different ways that almost everybody was assigned the highest dose, that is as it was put in that meeting summary of betatron radiographers.

Now, that's basically the same thing that was said on December the 11th, 2012, just before the full Board voted to deny SEC 105. And I had said in my administrative review that we filed for SEC 105 with HHS that those remarks by Dave Allen but also contributed to by others was a very serious misleading of the Board.

I also think that Dave Allen's statements in October 9th of 2007 were very

serious misleading of the public as to what would be done with the dose reconstructions, and that's my question that I have today. I have two questions. One is Dave Allen's presentation today did not reflect that he had read any of my papers. There's no reference to the 1952 data. There's no reference by Dave Allen to my and John Ramspott's information that we sent about the inner structure in building 6 being built in 1955, that the walls were one row thick of blocks, concrete blocks with holes that were filled with river sand that were six to eight inches wide, not 16 inches wide, not 24 inches wide, that the workers deny that bricks were ever added to the outside of those walls. All of those facts we had just sent. And, you know, I hear no indication that Dave Allen read that material, considered any of those things, and certainly they're not entered into his models, which are based on solid concrete walls that are 16 to 24 inches thick.

We know that the walls in the inner building were not solid concrete because we now have two workers, Mr. Churovich is on the phone and he's one of them, and [identifying information redacted] who was an employee that was there from 1950 through 1977 and was there, actually, for a very long time and directly observed the inner structure being built in 1955. So all the facts that we put in our paper were [identifying information redacted] affidavit, eyewitness, sworn statement, and Mr. Churovich can amplify that showing the same thing.

But the main problem with all of this scenario that the highest dose will be assigned is the fact that, based on the completed dose reconstructions that I've seen and that John Ramspott has seen, it is definitely simply not true what Dave Allen said would be the case in 2007 and it's not true what Dave Allen was referring to in December the 11th. And I am skeptical that,

even though you may say that the highest dose should be assigned to everybody, that the language we're all using is so vague about which jobs are going to be covered under the highest dose scenario that dose reconstructors will be free to do whatever they want to do.

I have a communication, too, actually, from Laurie Ishak Breyer, who is the SEC counselor, where she laid out explicitly that, as we all know, Appendix BB has two levels of dose assignment. And, in general, my observation is that the real betatron isotope operators, which are only a few, as Dave Allen pointed out. A deceased betatron radiographer, John Terry Dutko, sent you all a list that there were 11 radiographers that he was aware of that filed claims. That's all.

And so, you know, so 11 from 284 only leaves you, that's 273 people who are not betatron radiographers. And according to Dave Allen, in 2007, most of those people should have gotten the betatron radiographer doses.

Well, they didn't get those doses. They were assigned the lower dose level, just like Appendix BB Rev 0 in 2007 indicated they would.

So here's what I have to say. Before any of this morning's discussion is credible at all, NIOSH now must produce statistics where it breaks down the statistics that should have been delivered a long time ago to add fact to this broad general discussion, which is entirely qualitative and it's based on supposition and speculation and so forth.

And so what they need to do is they need to say, of all the people who've undergone dose reconstruction, how many of those people are strictly classified as radiographers? That certainly should include the 11 people that Terry Dutko identified. It certainly should include the 12 radiographers that Dave Allen spoke about this morning. But the most important thing he needs to fill in

is how many of the remaining 273 people were assigned that highest betatron radiography dose? And I will be surprised if that number isn't way lower. I think you're going to find that the dose reconstructors gave the non-radiographers the non-radiographer dose and, in general, the radiographers got the radiographer's higher dose.

Now, of course, the problem is for Rev 1 of Appendix BB we have another huge problem, and that is, in the former Rev 0, the betatron operators got higher doses than the other workers did by an order of magnitude tenfold at least, whereas the situation has changed dramatically now that NIOSH has reverted to normalizing the MCNPX betatron model to the film badge reading. And the idea that, as you heard today, Bob Anigstein said the fact that you have to normalize to no more than 10 millirem doses on the film badges, and Bob gave you his reasons this morning why SC&A doesn't even think that normalization process

is valid.

So I think, I think that is a very major thing. I think that for the radium calculations for the non-radiographers, you know, you have to use half-value layers of river sand for an eight-inch or six-inch thick wall. And, also, the quotations that Bob Anigstein gave this morning that no workers came within 20 feet of the inner structure is refuted by abundant testimony from many workers who knew that area.

[Identifying information redacted], who observed the inner building being built, reminded all of us, I didn't know this before, that he parked his locomotive in the winter in building 6 very close to the inner radiography room in building 6. John Ramspott reminds me that it's virtually a straight line from that thin door of the inner building, which, by the way, there's testimony that we believe that that door wasn't even present probably up until 1962.

[Identifying information redacted] testifies that when he saw the building 6 inner building being built that they framed in the door, but they didn't put a door on that structure. So we think that it may be that there was no door on that up until 1962 when the cobalt-60 licenses were built. But in any case, even if there was a thin door, radium could penetrate that quite easily, and it was a straight shot into the foundry building which was right across from that inner building, which was, by the way, for the record, roofless.

So I just think the entire modeling and the entire idea that you can assign everybody this high dose and that it actually will be carried out during actual dose reconstructions is really a house of cards this morning. I think that the Work Group should ask NIOSH to provide the statistics that show that non-radiographers have been assigned radiographer doses and that

this scheme that's being proposed actually comports with the facts of what has been done since dose reconstructions really began at GSI in 2007.

So, anyway, I have -- oh, and there's one other thing I just need to mention about today's discussion. When we're talking about, when Jim Neton is talking about identifying and Paul Ziemer is talking about identifying workers who were secretaries and accountants and, interestingly, Dave Allen brought up accountants in 2007 at the October meeting, but Paul wants to be able to identify those people solidly, and I'm saying that's not possible. And I point out to you that I had written Rachel Leiton and asked her about whether Department of Labor was able to identify radiographers and non-radiographers at GSI, and she wrote back to me and I sent that letter to all of you and the Board. She wrote back that their system, quote, was not sophisticated enough to distinguish

radiographers and non-radiographers at GSI, that most of the job classifications DOL had related to chemical operators and laboratory workers.

So I think that's another issue that has to be reconciled, and I think it would save a lot of heartache and angst and mistakes and time and money and effort if Department of Labor could be consulted on this matter. Can they actually pick out secretaries and accountants from all the other people that they would have to distinguish? Can Department of Labor help you pick out radiographers? I just feel that that's going to be impossible to administer accurately.

So I have other comments about the internal doses, and I have to say on the SC&A papers that, you know, I got my copies yesterday. That's not nearly enough time to review those papers and digest them. I find out this morning that Dr. Anigstein has a PowerPoint. I think all of you all well know

that every time that a PowerPoint is shown I want a copy, and that could have been done proactively. You know, the petitioners ought to have the material that's presented at Work Group meetings, and I've asked for that over and over again.

So while all that discussion was going on and you all were watching everything on your screens, I had no access to that information, and I need to have that. So, you know, I wrote you all four papers, and three of them came in much earlier. The response to Dave Allen took three days, and it's 24 pages long. And I need you to read that, and I need it to be discussed intelligently this morning, along with Dave Allen's paper and along with the two papers that SC&A wrote in response to it.

So I guess the final thing I would say is I noticed that Dave Allen mentioned that radium-226, exposure was the way he put it, was slightly more than you get from

cobalt-60. Well, you know, I'm not a house physicist, but it wasn't hard to read that radium-226 generates five alphas with energies up to 7.7 MeV each, plus the gamma photons that are 2.3 MeV versus, as you all know better than me, Co60 only has gamma photons in the 1 to 1.1 MeV range.

So I think, again, that's a, it's a misleading statement to say it's only slightly greater. It's more than two-fold greater energy wise, radium compared to cobalt-60.

And I'd also mention that you all act as though you know a lot about the GSI radium sources, but, in fact, you all don't know who manufactured them, who was the vendor, when they were purchased, whether there were any radon leak tests which I have in my papers. He brought that up to this Work Group in 2009. And, you know, all of that should have been worked out years ago.

In fact, when the radium plumb-bob

incident was first described, it was described as a stolen plumb-bob. The word radium was not attached to that at all. And now that John Ramspott and I have read about all of this extensively, it's pretty clear to us that plumb-bob, plumb-bob refers to a radium source. It doesn't apply to a cobalt source. It doesn't apply to a radium source. So as soon as that testimony came up in 2006, everybody should have started looking for radium-226 sources at GSI. And the NRC FOIA 2010-0012 that I obtained and NRC supplied to us in full unredacted, that clearly has in there that both St. Louis Testing and NCC performed leak tests on the GSI sources.

Now, presumably, that was on their cobalt sources, but what's really relevant is who or did anyone perform leak tests on the GSI radium-226 sources? And you all know better than I do that those sources build up gasses within the little capsule, and they often rupture, and that's the reason why they

were so dangerous. That's the reason why their use was discontinued. That's the reason why people had to, for safety reasons, perform leak tests. And we don't know anything about that at GSI and, as far as I can tell, the effort has not been expended to find out about that, to get those records from NRC or the Department of Energy.

I've sent three FOIA requests to get those license records for NCC and for St. Louis Testing, and I can't get them. NRC says they have no records of those licenses. Department of Energy, Pat Worthington, Dr. Worthington just wrote me and I sent that to you all. The Department of Energy cannot find those byproduct licenses for either NCC or for St. Louis Testing.

So the way I look at that is that all that information that's in the GSI license application that talks about NCC, St. Louis Testing, some of the facts in there are just plain wrong. We certainly believe the

drawings of the inner building 6 building in that GSI license application are just plain wrong. They weren't 16 inches, they weren't 24 inches thick.

So, anyway, that's basically what I have to say on that. I do have some comments to make about the internal doses, but I'll let it go at that. Thank you.

CHAIRMAN ZIEMER: We'll get to those later, Dan. Thank you. Let me make a couple of comments here just for the record. On the radium sources, the alpha energies are not pertinent since none of the alphas can get out through that capsule. So our --

MR. KATZ: Paul, you --

CHAIRMAN ZIEMER: I'm on mute. Sorry. I was on mute. Just a couple of comments here. I just wanted to mention, on the radium sources, the alpha energies aren't of importance here since the alphas do not get outside of the capsule. So the alpha radium sources are very well known in terms of the

exposure rate, as are cobalt, so those numbers that Dave has used were very well known numbers in terms of the output at various distances per unit activity. They're very well-known, well-established numbers.

One other thing I'll comment on because, on the leak test issues, we had commented way back that one of the reasons radium was removed from use around the country and it was not mandated by AEC, they had no control over radium and this was really done on a consensus basis and enforced by states mainly, but was the leaking issue.

Now, not all radium sources leak, and one of the issues would be for radiographers, if you had a leaking source, you basically couldn't use it for radiography because you would be contaminating your films with the leaking material. So the fact that one was able to continue to use the films in radiography is, at least, an indirect indication that there was leakage because if

you had such leakage you wouldn't be able to really conduct valid radiography pictures.

And there was indication in some of those early records, at least statements that leak testing was done. We don't have those records, but, indirectly, one can confirm from the fact that they were able to use those sources for radiography that they weren't leakers and, hence, the external contamination apparently was not an issue. Now, I --

DR. MCKEEL: This is Dan McKeel. Can I please? I need to break in because you're just overriding my central point. My central point is that when it was time for GSI, when they bought their cobalt-60 sources, there's some early letters from the AEC saying that their leak testing and calibration records for the survey meters were late, that they hadn't done them.

So, certainly, for those meters and those sources, the AEC was very interested

in whether those tests had been performed. And I know from everything I've read that radium, when you say, I believe your term was that you can infer that the GSI radium sources which were used for at least 10 to 11 years, and don't forget the information that you all cite as the basis for saying that AEC limits were not exceeded, that statement in the GSI license says that those measurements went back 20 years. And so that means well before 1952 there was some source that was being used at GSI. The betatron didn't come in until 1952. And so the main source that would be used at GSI for the first part of those 20 years was radium-226. That's all they had.

And so I'm saying that, and I believe you would have to admit this, that at any well-run installation that people did do leak tests for radium-226 sources, and the literature is full of methods to do that using Polaroid land film and so forth. And as I said, those leaks, a lot of them couldn't be

seen, couldn't be seen by visual inspection. And, of course, the marker that made them visible was the radon that was released through, those tiny holes in the capsule would expose the underlying film and that would show you the sources of the leaking.

So I'm going to claim again that it was probable that the GSI radium-226 sources did leak. We had no idea whether they were changed out at all. We just have no records about that. If you say they weren't ever, that leaking was not an issue, that was your phrase, I don't believe we know that. I think that's speculation and conjecture. So, you know --

CHAIRMAN ZIEMER: Well, I don't agree with that because if they were leaking that would show up in at least two ways. One is your survey meters would not be usable because they would be contaminated. Number two, your films would be contaminated and would not be usable.

DR. MCKEEL: Well, here's the problem. You don't have any films, you don't have any reports, you don't have any calibration record on any test instruments. So all of those things could have occurred, and we would never know the difference. We simply do not have records. Everybody is making up things that should have been the case at GSI, but we have no proof that they were actually the case.

So, again, you know, I cannot require anything. I can just say that the petitioners, and speaking for the workers and the advocates at this site, strongly believe that you all should, to be claimant-favorable, presume that those radium sources did leak radon and you have to bound those doses. That's all I'll say about that.

CHAIRMAN ZIEMER: Bob, do you have a comment?

DR. MCKEEL: I do have one other comment. I'm sorry. But, you know, you call

me out of order. I thought that I was going to be on the spot after all the discussions were had from SC&A. But I want to mention that when Bob Anigstein was going through his list of sources at GSI during the radium era, he mentioned radium and the betatron. But, in fact, we have testimony that there was iridium-192 source and there were two 250 kVp x-ray machines, and the same situation told during the, quote, betatron era. It wasn't just the two betatrons and two cobalt sources. There were also the two kVp, 250 kVp machines. There was the iridium source and two overlooked sources that when I was preparing the administrative review we ran across. But there's testimony from three workers at GSI that they were required as part of their radiographer GSI jobs to go over to American Steel and to use their one million kVp x-ray machine and their iridium-192 source to do overflow GSI work. And, more importantly, those men were required to wear

their GSI film badges over to American Steel.

So I understand that's a legal question as to whether work done by GSI employees at another facility should be covered under their dose reconstructions under the Act, but that is a fact. It is on the record, and it was put on the record during two worker meetings in July and August of 2006.

Okay. Thank you.

CHAIRMAN ZIEMER: Okay. Bob, you had a comment?

DR. ANIGSTEIN: Yes, I have a couple of comments. First of all, about the exclusion, I never said that no worker could approach within 20 feet. I said there were no workstations. That was according to the GSI application. There were no workstations in that area. There's 20 feet on either side to the nearest work area, and there was oil drum storage on one side, which made it difficult to approach the walls. There was one wall, I

think it would be the east, the west wall, which was approachable. Obviously, they had to go in and out the door. But there was no permanent station where somebody would spend a good part of eight hours a day. That was what I meant. I never meant to say that they could not approach the wall.

The other statement was about the, some comments about the leak testing and the radium. The fact that the AEC raised a question about the cobalt leak testing was simply a matter of how the regulation was interpreted. And Dr. Kronecker of the NCC, the Nuclear-Chicago Corporation, simply said he assumed that they had to be tested within six months of being put into use, not within six months of it being first -- they did do leak testing. There was just a quibble over what was the appropriate date, and he said now we understand the right date. It's not that they didn't know about leak testing and they did not do leak testing.

And the same way with the instrument calibration. It was a question of the dates that it had to be done on.

Now, I did some independent research I never got around to reporting on what was, the radium sources were not owned by GSI. They were leased; and, as a matter of fact, that's one of the reasons given in the license application why they wanted to get rid of them, besides the fact that the State of Illinois ordered them to, which I think was the real reason, but also that the leasing was pretty much extremely expensive in those days. So even the leasing was expensive, and they thought it was cheaper to simply buy the cobalt sources outright and be done with it and not have to pay, you know, annual, monthly, whatever it was, lease.

And that being the case, now, we don't know who was the purveyor, but I did find information on some purveyors that leased radium sources. And they also provided at a

very low cost leak testing. There were two ways. They would either sell a leak testing kit, and I'm going by memory but at some very nominal cost, like $19. I know we're talking about years back, but still $19 on the operation side of GSI was not significant. Or they could send the sources, and they would leak test them. And, again, I'm just trying to, I think it was something like $70. Less than a hundred dollars and they would leak test them. But since they were leased sources, it would be very simple to say, okay, send us, we're sending you the source for leak testing, send us another one in the meantime since we could just swap them since they're owned by this company anyway.

So it's not proof. But given everything else, that they went to the expense of having film badges when they weren't required to, it would seem very reasonable to think that, since leak testing was provided at a nominal cost, why would they not take

advantage of it? That's sort of a heuristic argument. It's not conclusive proof.

CHAIRMAN ZIEMER: Okay. We need to take a comfort break here. We've had a lot of discussion, but let's take a ten-minute break and then we'll return, have one more discussion, and then we'll do our lunch break probably about a quarter after one Eastern Time. Okay, ten minute break.

(Whereupon, the above-entitled matter went off the record at 12:32 p.m. and resumed at 12:46 p.m.)

CHAIRMAN ZIEMER: I think we'll go ahead and proceed. I want to start with a question, which I will ask NIOSH. Let me frame it this way: if we were to proceed with a scheme such as they describe with the triangular distribution and the idea that everyone would get that dose unless you could specifically confirm that there's no way they would have been in the operational area,

number one, I assume that that would result not only in a revision of Appendix BB but would initiate a PER that would go back and review all of the previous dose reconstructions, at least those that were below the 50-percent value, and determine whether or not they now are qualified for compensation under the revision. Is that correct?

MR. ALLEN: This is Dave Allen. Yes, that's correct. I mean, that's been the plan all along is to settle all the issues, revise the appendix, and then perform a PER.

CHAIRMAN ZIEMER: And, essentially --

DR. NETON: Paul, this is Jim. I'd like to correct one thing you said. You said that we would confirm there's no way they entered the radiological area.

CHAIRMAN ZIEMER: Okay. I want to --

DR. NETON: I would say that we

would confirm there was no way that they actually performed radiography. That's really what we're saying because these people did not perform radiography --

DR. ANIGSTEIN: Excuse me. This is Bob. That's not what John Mauro and I understood you to say earlier. There will be an exclusion for proven administrative personnel who would not have been in the plant. Obviously, some secretary might have been sent to give a message to a radiographer, the boss wants to see you or something like that. But I mean they would not be only in the plant. Now, we're going back and forth because --

DR. NETON: No, no, no, I misspoke. What I meant was they could have been in the plant by, you know, traversing it, delivering paychecks, you know, whatever. So Paul said that they would not been in the plant, but I'm saying they could have been in the plant but they were administrative

personnel only and --

DR. ANIGSTEIN: Yes, yes, that's what we understood you to say. We hope --

CHAIRMAN ZIEMER: But anyone who worked in the plant --

DR. NETON: Right. They did not work --

CHAIRMAN ZIEMER: -- would be considered a radiographer under this scheme.

DR. NETON: Correct. I misspoke. Thanks for correcting me, too.

CHAIRMAN ZIEMER: Yes, okay. I wanted to make sure I understood where that was going. And then the other part of it is then, if they were in the category where they were what you're calling currently sort of administrative, I'm not using that necessarily as the job title but conceptually, not someone who worked within the plant on a regular basis, you were going to apply the lower dose based on the calculation that Dave had developed.

And what I wanted to ascertain was, and perhaps this would be a next step, a couple of things. One is that there's agreement between SC&A and NIOSH as to how that is actually calculated and, number two, that NIOSH would look at the issues raised by the petitioner in terms of the wall and so on and at least confirm what they did in terms of those issues. We had some debates about the composition and the thicknesses and just to assure that those were taken into consideration. Is that something that -- and I just raise this at this point. I need to get into it from the other Work Group Members, but I'm trying to think about the next step here because, to come to closure, you need agreement on some of these things.

DR. ANIGSTEIN: This is Bob. I've got two problems, not to be, you know, beating a dead horse. One is if you assumed that the wall thickness never changes, as Dave pointed out, it doesn't matter what the wall is

because then he would simply be using the survey measurement and then make a correction for the difference between radium and cobalt and the 500 millicuries and the 260 or 80 millicuries.

But the other question this raises is what do you do, starting in `52 into sometime in `55, when there was no building? I mean, we've accepted, I believe I talked to you privately, Paul, and we've accepted that the email that Dr. McKeel furnished indicating that this was built in `55. We have no real reason to not believe that because the only information, just to round out the picture, that I had was somebody whom I interviewed, I believe it was a radiographer, a part-time radiographer, who said, well, it was there forever, but they really didn't know because he didn't come to work, he had worked earlier at GSI but then he came back in `56, `56 or `57. So he simply said definitely the building was there when he came back, but he

really didn't know about the earlier time.

So, therefore, in the absence of any other information, I'm just sounding, I'm just deliberately sounding skeptical, but even with skepticism that account that it was built in `65 is consistent with most of the other information we have. So given that, how can you possibly use that calculation to assign doses in `52, `53, `54, and at least part of `55?

CHAIRMAN ZIEMER: Well, that's, essentially, the question I'm asking.

DR. ANIGSTEIN: Yes, whereas my suggestion, again, it's not meant to be self-serving, is the MCNP calculation we did, which only took credit for a very thin steel door, kind of eliminates that question. Of course, it does assume that there is some kind of a structure which excludes people from coming any closer to the radium source within that door, so, actually, I have to withdraw that. That would be good for after the building was

put up, but it would not, again, it would not serve when there was no building. And that, again, is a puzzlement. What do you do?

I would suggest simply saying, since we adopted the regulatory limit, or at least this triangular distribution which incorporates a regulatory limit, to radiographers, why not use the limit to members of the public? They were people occupying unrestricted areas, which, from `61, was 500 millirem. I'm not sure if the earlier rule had a limit, had a non -- I would have to look at that.

DR. MAURO: Oh, 500 millirem has been around for a long time and then, of course, was changed --

DR. ANIGSTEIN: That goes back to, that was in the `61 rule. I'm not sure if it was in the `57 rule.

DR. MAURO: This is John --

DR. ANIGSTEIN: But I have it, I have it here. I can find it.

DR. MAURO: But before we go too far down that road, I think we have changed the paradigm sufficiently that says we're now talking about what doses we're going to assign to people who we think spent a very limited time in the operations area, predominantly not in the operations area, which means that it becomes a different kind of problem than the one that David modeled. And I have to say that, you know, there has to be a prudently conservative, we want to assign something to these people that probably had minimal potential for exposure.

What you propose, Bob, is certainly one way to come at it, namely if there was a non-occupational limit. But, you know, I have to say right now I'm not sure how you would come at the problem. You may not want to, you know, go with David's approach because David's approach really is saying that those people were working, the ones that you were talking about were working in this

envelope and they're going to be the people that are going to get the big triangle.

Now we have a different group of people that we really never engaged before, these administrative personnel that only occasionally may have entered the area. So I think this is going to require a little bit of thought.

DR. ANIGSTEIN: By the way, I'm just looking at the `57 rule. The `57 rule goes to, is 2 millirem per hour or 100 millirem in seven days, assuming 100-percent occupancy. It does not have an annual limit, you know, to account for the fact that people are not going to be at the worst location. This is for any unrestricted area. So at 500 millirem in any seven days would be up to, you know, 50 weeks, that would be 5 rem.

DR. MAURO: No, see, what that restricted area --

DR. ANIGSTEIN: This is the unrestricted area.

DR. MAURO: Right. Inside that, though, the reason that was set up is because if you're inside that you do have the potential to have an exposure --

DR. ANIGSTEIN: No, no, excuse me, John, there was one for restricted area and another one for unrestricted area. Unrestricted area is everything outside the sign that says "radiation, keep out." And --

CHAIRMAN ZIEMER: I just want to conceptually get the idea. I want to hear from Jim Neton and Dave Allen, conceptually, what do you think about how to go forward on this?

MR. ALLEN: Well, this is Dave. I'd like to get things settled as much as possible today, and I'm not real comfortable with the idea of basing the 1954 dose on a 1957 limit. But, honestly, we had a model previously for the radiography outside the radiography room. It involved people at a boundary and then walking through the area.

That ends up being, I think, around, if I remember right, around 1.3 rem per year. We could use that for the administrative type of people.

DR. ANIGSTEIN: Now, Jim, that's consistent with the -- because even in `57 there was a 2 mR per hour rule.

CHAIRMAN ZIEMER: Jim Neton, do you have any comments?

DR. NETON: Yes, I agree with the one that we talked about earlier that John alluded to is probably not the right approach. I think Dave hit on the right one. I think the one where people could have been walking through the plant while they're doing radiography and just been incidentally exposed, I like that approach.

DR. MAURO: Me, too.

DR. NETON: It doesn't involve any --

DR. ANIGSTEIN: What number are we talking about now?

DR. NETON: It was the 1.3, Dave, did you say?

DR. ANIGSTEIN: I see. Okay. That was based on what kind of occupancy? Not full-time occupancy, obviously.

MEMBER MUNN: Casual walking through.

DR. ANIGSTEIN: Okay. That sounds good. I would say, I would say that's reasonable.

DR. MAURO: The philosophy, the concept is solid. The actual number you pick is a judgment call, but the idea of coming at the problem that way sounds to me the right way to come at it.

MEMBER MUNN: Any reasonable person would certainly take the position that 1.3 rem a year for casual occupancy is more than generous.

DR. MAURO: I agree with you, Wanda.

DR. ANIGSTEIN: Okay.

MEMBER MUNN: It's probably a tenth of that for purposes of dose reconstruction. That certainly could not be argued by anyone as not being limiting.

MEMBER BEACH: And, Paul, this is Josie. I can agree with that, also.

CHAIRMAN ZIEMER: Yes. And I don't know. John, did you come back on the line?

MEMBER POSTON: Hello? I'm here.

CHAIRMAN ZIEMER: Okay. Did you hear this past discussion, or did you just get aboard?

MEMBER POSTON: No, no, I heard the discussion and everything.

CHAIRMAN ZIEMER: Okay. Any comments or --

MEMBER POSTON: No, I'm okay with it.

CHAIRMAN ZIEMER: Yes, okay. So what's being proposed then is a triangular distribution, which would apply to everybody,

basically, that worked in the plant, with the exception of individuals who you can confirm, either through CATIs or otherwise, were not regular in-plant workers, who we're currently calling administrative, and that they would be assigned an annual dose based on the previous calculations that were based on the radium being used in the open areas and the possibility of people walking, actually walking through the restricted area, which I think at that time was 2 mR per hour. Is that correct?

MR. ALLEN: Yes, I think that calculation was based on the reports that they made a boundary at one and a half times the distance, the --

CHAIRMAN ZIEMER: Whatever that was. Right, right.

DR. ANIGSTEIN: Oh, in that case -- this is Bob. In that case, I have to disagree because that was a third hand account that we got of one and a half times the

distance of one person talking to somebody else, and I don't think we can go by that. I think the fact that it was a 2 mR per hour boundary seems reasonable. One and a half times is not, I don't agree with. I don't think that's -- that has been contradicted by other accounts, and I don't think that can be used as a basis. Now I understand why it's less than full-time occupancy.

CHAIRMAN ZIEMER: I think, Dave, you used the 2 in your calculation, didn't you?

MR. ALLEN: I could be wrong, but I'm pretty sure that was the lower dose that you give one and a half times distance.

CHAIRMAN ZIEMER: Gave you the 2 or --

DR. ANIGSTEIN: He took the 2 mR and simply used the inverse square law, so it was, essentially, 2 mR divided by 2.25, which is the square of 1.5.

MR. ALLEN: Yes, I'm pretty sure

that's the way that was calculated.

CHAIRMAN ZIEMER: Well, you need to go back and double-check.

DR. ANIGSTEIN: No, the math I'm sure is correct because we checked that. The assumption we don't agree with. And the 2 mR per hour continuous occupancy brings us right back -- I would still argue for the 10 CFR 20 rule, which would then give you 5 rem for the period up to January 1st, 1961 and would give you 500 millirem, drops by a factor of ten, after that. Since we're using that for the radiographers, it seems reasonable to use that for those administrative personnel conceptually. And if you want to do a distribution based on the maximum, I wouldn't object to that, like a uniform distribution from zero to --

MR. ALLEN: The 5 rem would conclude 100-percent occupancy, though.

DR. ANIGSTEIN: No, no, the 5 rem, yes, the 5 rem, the 1957 rule is silent on

occupancy. It simply says in an unrestricted area you will not have any exposure rate, if it were occupied full-time, would result in more than 100 mR per week. In the `62 rule, `61 rule, it goes further. It has those same words but, on top of that, that no person should have more than 500 millirem in a year, no real person. First, they talked about a fictitious person, you know, a ghost being there all the time. And then they say for a real person it shouldn't be more than 500 millirem. It's silent on that.

CHAIRMAN ZIEMER: We got to go back to 1952.

DR. ANIGSTEIN: I know. I said the earlier rule, which would have been applicable in -- it was promulgated in `57, but it was observed by the AEC prior to that. They simply did not have the power to regulate. And since all we're going by is the assumption that they follow the AEC limits, even though they were not subject to them

because they were not an AEC licensee, so, by the same philosophy as we're adopting to 15, I would suggest the 5 would probably bring us right back to that triangle.

DR. MAURO: Mind my jumping in here? There's something about that that disturbs me. I can't envision under any circumstances an administrative person who maybe occasionally walks into the operational area ending up getting 5 rem a year.

MEMBER MUNN: No.

DR. MAURO: I don't like that. I think that's, I think we've got to -- if we're going to go to the regulatory limit, you know, that worked well for the upper end triangle, but to apply that to an administrative personnel, I think that's just pushing it too far. I like the idea of coming up with a kind of thing that David was talking about. Whether you draw the line at 2 mR per hour, the --

DR. ANIGSTEIN: But if you do the

2 mR per hour you end up with 5 rem.

DR. MAURO: No, no, I'm not saying he's there all the time. I'm saying the person leaves his office someplace in the administrative building, he comes in and, maybe inappropriately, crosses over, walks through, and --

DR. ANIGSTEIN: But then we have to assume an occupancy --

DR. MAURO: Here we are arguing between SC&A, but you do that, we do that on TBD-6000 where they break up people into supervisory personnel, operators --

DR. ANIGSTEIN: All right.

DR. MAURO: -- and there's a percentage of time that the person is present in a particular setting. And, of course, there's a certain amount of judgment made when they say, well, we're going to assume it's 5 percent, 25 percent, 50 percent, these different categories of workers. I think that philosophy holds. We've always been

comfortable with that as applied to TBD-6000, and I think that the same kind of thing goes here. It's just a matter of making that judgment, you know, what percent of the time would the person be in the vicinity of this operations area if his job is administrative type. So the idea of giving someone 5 rem in a year as an administrator, I have a strong reaction --

DR. ANIGSTEIN: I agree with you.

DR. MAURO: Yes.

DR. ANIGSTEIN: I just couldn't think of anything, any other --

DR. MAURO: And, Board Members, we apologize. Here we've got SC&A jumping in and arguing. But the --

DR. ANIGSTEIN: Well, we obviously didn't discuss this ahead of time.

DR. MAURO: We didn't.

CHAIRMAN ZIEMER: Okay. I'm going to interrupt at this point. We're on a concept here, and part of that concept needs

to be further developed. I think, I don't know if this is going to require a technical call between NIOSH and SC&A to hammer out how this would go, but I think the Work Group needs to know what that's going to look like for those people that we're currently calling administrative. I think we have general agreement that everybody else in the plant, at least during the radium era, would get whatever the triangular distribution delivers to them. And we're talking about a few folks, apparently, that would get a lower dose and what that's going to look like.

So do we need to have a technical call or just do this -- NIOSH, do you have enough information to flesh this out and give us what you think it looks like and have SC&A take a look at what you're proposing?

MR. ALLEN: I have a general idea where people want the number to end up. I'm not quite sure how I'm going to get there, but I can come up with something.

DR. NETON: Yes, I think we can come up with something. I don't think this is the forum, correct forum for a technical call. We have to be working out, you know, technical -- we'd be doing more than working out technical details. We'd be developing an approach. So I think it would be better for us, NIOSH, to put something out there and --

CHAIRMAN ZIEMER: Yes, and I don't want to start with the proposition in saying where do you want to end up. I think you've got to make some reasonable assumptions and see what that brings you to. It's got to be plausible and still be claimant-favorable and still, you know, take into consideration the situation there.

MEMBER MUNN: Well, we discussed the possibility earlier of where a line might be drawn, and that seems credible, although extremely favorable, for non-radiographer personnel. And it would appear to me to be a logical starting point, even though it is

extremely generous. And anyone who is familiar with how plants operated in the 1950s knows that that would be a very firm basis on which to make the ground rules for how you're going to approach it.

CHAIRMAN ZIEMER: Okay. And so do we have agreement that we're not going to make a final decision on this, but we'll ask NIOSH to flesh that part out, give SC&A a chance to look at this. When are we talking about time-wise here, Jim or Dave?

DR. NETON: I'll leave that up to Dave since he's the one doing it.

MR. ALLEN: If we're just talking about this -- I'm thinking me and Jim can put our heads together and come up with whatever concept we want to. I can get it written up and out late next week possibly.

CHAIRMAN ZIEMER: Okay.

MR. ALLEN: An email, a White Paper, or what are we looking for?

CHAIRMAN ZIEMER: Well, I think a

White Paper would do it, and then SC&A will have an opportunity to comment on that. So I want to push this toward getting resolution on this and having another, it's going to have to be a telephone meeting, but, you know, if we can do that within a month or so, that would be great. I want to try to tie this up.

MR. ALLEN: I think, for this particular issue, I think we can shoot for a White Paper towards the end of next week, but I don't want to guarantee it. I can guarantee you two weeks.

CHAIRMAN ZIEMER: Okay.

MR. ALLEN: But I'll shoot for next week.

CHAIRMAN ZIEMER: Okay.

DR. ANIGSTEIN: All right. And I would like to have two weeks to respond to it.

CHAIRMAN ZIEMER: Right, right, right. Okay. So we'll push ahead on that part of it and try to get it resolved. We want to take a look at the internal dose issue

next. I'm looking at the clock here. It's 1:15 Eastern Time. We need to take a lunch break. Let's take, would 45 minutes be enough for everybody? Reconvene at two?

MEMBER MUNN: Fine with me.

CHAIRMAN ZIEMER: Yes, let's do that. We'll take a 45-minute break and reconvene. We want to discuss the internal dose issues for GSI, and then I think the other three facilities, what we have left to do -- I'm sorry? I'm not hearing what somebody is saying.

DR. NETON: It sounded like a Cincinnati Bell recording of some kind.

MEMBER MUNN: It sounded like it.

CHAIRMAN ZIEMER: Okay. And we'll proceed from there. So we'll take a 45-minute lunch break. Thank you.

(Whereupon, the above-entitled matter went off the record at 1:12 p.m. and resumed at 2:00 p.m.)

CHAIRMAN ZIEMER: We're back in

session, still dealing with GSI. We'd like to now address the estimates of internal exposure. Dave covered his part for us for NIOSH this morning, and we want to hear from Bob Anigstein and John Mauro, and they have also distributed their comments called Review of NIOSH Estimates of Internal Exposures at GSI. Who's got the lead on this? Bob, do you still? Or John?

DR. MAURO: Yes, Paul. Bob does have the lead. He's probably on --

DR. ANIGSTEIN: I'm on mute. I was on mute. I have my briefing on the screen now, if everybody can see it, the 11th page of the presentation. Is that visible to everyone?

MR. KATZ: Yes, Bob.

DR. ANIGSTEIN: Okay. All right. So I start off with, this is the log-normal distribution that we all agreed to. It was NIOSH and SC&A making comments, and we went back and forth in the Work Group, educated.

So we all agree to use a distribution with a geometric mean of 17.54 dpm per cubic meter. Oh, John, you just did something.

DR. MAURO: Yes, my mistake. I'll --

DR. ANIGSTEIN: Yes, okay. And then a 95th percentile of 68.7 dpm per cubic meter. And using that distribution, I calculated the arithmetic mean. There's a formula for that of 24.72. We'll get to the reason for that in a moment.

Here is the comparison. We differ very much on the model of how to use these parameters to calculate the intakes. So NIOSH presented, I was reproducing from Dave Allen's report, a range of intakes, at least during the operational period, from 15 to about 34 something dpm per calendar day. Our model is for all workers and through all periods right up until the beginning of the residual period. We get 264 dpm per calendar day, and I'll get to how we do that. And then after June 30th,

`66, we use the same number, but we decrease it exponentially by the amount in OTIB-70.

The basis for disagreement. Okay. The NIOSH model postulates exposure to this 95th percentile concentration, but during only 20 percent of the uranium handling hours each year. So the maximum uranium handling hours would be something like 437 2 hours, so, roughly, 80 hours a year will be the maximum amount of time where the workers, the betatron operators -- we're then giving the same thing to all workers, would be exposed to that dust because that's the time that they spent handling the uranium setting up a shot. In between, the shots were assumed to be 60 minutes each and took 15 minutes to set up, so you take a total of 75 minutes and 15 minutes is 20 percent of that.

Then they say, however, how do you account for the time in between the shots. Well, we simply say, well, you have this deposition for 30 days. It only happens once,

at the beginning of the operational period. The dust settles for 30 days. They derive taking the 30 days, multiplying it by this 68.7 dpm per cubic meter and multiplying by the deposition velocity of 7.5 times ten to the minus four per meter. They calculate a surficial contamination of 1.34 times ten to the fifth dpm per square meter. And then they apply a resuspension factor of 10 to the minus 5 to then get the resultant dust from the surface contamination. Now, this is very important to follow.

Our objection is, first of all, this 30-day deposition, that's a period it takes to equilibrate if you have a constant concentration. It does not apply to these intermittent concentrations. We don't know, even though we know how many total hours there were per year, we don't know whether all the uranium worked with at the ending of the year, which is probably unlikely, or whether they did it once a week for a few hours, which is

arithmetically possible, or they -- we just don't know what the intervals were.

But, nevertheless, the 30-day deposition would mean to have the dust generated during a short period of time, and then it takes 30 days for it to settle. That means a column of air will be about 2 kilometers, which is not plausible. The building is only 35 feet high, approximately, in size. And, more important, we go back to the basic definition of resuspension factors. Even though it's called resuspension, what it really is, is a ratio of the concentration in the air and the concentration on the surface. So if the concentration in the air, we accept the 68.7 dpm per cubic meter and the concentration on the air is 1.34 times ten to the fifth, so 134,000 dpm per square meter, we end up with an effective resuspension factor of five times ten to the minus four. So they're depositing it, assuming -- they calculate the surface contamination assuming

five times ten to the minus four, and then they calculate, they re-calculate the air concentration using ten to the minus five, a 50-fold difference.

And, yet, while this dust is settling, and there's no reason it would only happen once. If it happened once, it would happen every time they handled the uranium. Nobody is being exposed to it, except for the 15 minutes it's settling and, yet, the building is vacant, essentially. No one is breathing this 68.7 dpm per cubic meter that is gradually settling over a period of 30 days.

So the model is not consistent. It's not consistent. It's not scientifically correct. And then a more minor point is the control room is not airtight, so, even if you're in the control room, there would be some uranium drifting in. They would be tracking it on their feet and so forth, so they would not be -- now, our model is a

bounding assumption, but it simply assumes that, because we don't know how long the dust takes to settle and we don't know how often. We do know the total time, but we don't know whether this happens once a week, once a month, once a year. Our assumption is assume that we have this concentration all the time, but then if it's all the time then it's unrealistic to assume it's a 95th percentile because that's only five percent of the thing. So we say either use the arithmetic mean or use the entire distribution and put that into IREP, which would be comparable to the arithmetic mean. The way statistics work, it's hard to predict exactly what it would be.

So we're saying that, assuming the arithmetic mean, we get this 264 calculated over a period of a year and then divide it up into 365 days, so we get 264 dpm per calendar day, which is, roughly, depending on what year you pick, it's roughly ten times the NIOSH. And just as a matter of point of reference,

the dpm per calendar day, we're talking, it's probably not very meaningful. It's comparable. What you get out of here by looking up the dose conversion factors, it's equivalent to one rem per year effective dose, one and a fraction. Much closer to one than to two. So it's not an unreasonable, it's not like they're being -- especially when we consider the doses now that are from external and then the lung dose is a few rem, I think maybe six, so it's not a huge addition to the external dose that they're already getting, but it is certainly claimant-favorable. And, again, if it's given all the error, it would be unreasonable to assume it's a 95th percentile and it's defensible. It has a simple but outgoing and all the mechanism of the 30 days is what is the settling rate and where does the dust come from and how come it's not being inhaled? This avoids all of these questions, so we think it's a preferable model and it's bounding and, yet, plausible.

So that's really where we stand on this.

CHAIRMAN ZIEMER: What about the residual period, Bob?

DR. ANIGSTEIN: Say again.

CHAIRMAN ZIEMER: Residual period.

DR. ANIGSTEIN: Oh, the residual period, we simply assume that this is the, we have this concentration -- sorry about the phone in the background. I can't turn it off. This is the concentration at the end of residual period, at the very start of the residual period, we had the same concentration. And we also do not believe that you can jump from ten to the minus five to ten to the minus six because, as NIOSH, Dave Allen points out, agrees, the ten to the minus six is based on an age activity in a quiescent region.

Now, gradually, this uranium will age, but it's certainly not quiescent. I mean, you have the floor of the betatron room

is being constantly traversed by foot traffic, by vehicles. So, if anything, ten to the minus fifth is on the low side, but we won't question that.

But you really don't need to know that because, by using the -- our floor concentration will be much higher, and then we would re-suspend that amount, so we will get back, basically we will get back this 24.7, and that's what we would go by, 24.7 decreasing exponentially month by month, year by year, however it's calculated, using the OTIB-70 approach, which we agree with.

CHAIRMAN ZIEMER: Well, that's what I was trying to determine. So you're suggesting that ten to the minus six is not applicable. What are you proposing in its place? Just an exponential --

DR. ANIGSTEIN: Ten to the minus -- well, if you want to do it this way, you can always propose ten to the minus fifth, the same as in your operational period. But our

model doesn't actually involve a resuspension factor because we simply say, whatever the mechanism is, this is what you're going to get in the air is 24.7, and you can use that and the ten to the minus fifth to calculate the floor concentration and then multiply it again by ten to the minus fifth to get the air concentration. But, you know, that doesn't, it's just a closed loop.

So during the residual period, we would say it's the same floor concentration decreasing by whatever that is, 0.4 or 4067 per day I think it is, I'm just going by memory now, and year by year, of course, that decreases. So at the end of 30 years, you have a substantially, much, much lower activity.

But, again, the advantage of this model is the simplicity and the bounding nature, and it requires really no assumptions other than the initial distribution, which we all agreed to.

CHAIRMAN ZIEMER: Okay. Thank you, Bob. I'm wondering if NIOSH has had a chance to take a look at this and have any initial responses, Dave or Jim.

MR. ALLEN: Yes, this is Dave. I've had a chance to look at it, and I may not have digested it all completely, but, yes, I take some issues with it. First of all, Bob was saying that the contamination levels somehow were not realistic because it would have to be settling from some 2-kilometer column or something.

DR. ANIGSTEIN: Yes.

MR. ALLEN: No. I mean, this is, this is, this is bounding. It is true that, once you start handling the uranium, you get some airborne and it would take some time to settle enough to reach that balance between removal mechanisms and production and the settling rate. But, I mean, this is something we hashed out long ago in TBD-6000 and came to the conclusion that the 30 days with that

deposition rate was appropriate for what that balance would eventually reach. We didn't do a bounding estimate by assuming that we reached that equilibrium concentration right off the bat, yes. But the rest of that was I'm not sure what.

As far as he mentioned, oh, the 20-percent factor or whatever. You've got to realize that that is for, the airborne that we agreed on is actually for handling the uranium metal by whatever means, by hands, by fork truck, by chain falls, et cetera, where you get some airborne from rubbing oxidation off of that. That's not happening when it's being x-rayed. That's what the purpose of that 20 percent was. That's when you start getting -- about the only mechanism at that point is resuspension of contamination, and that's at its maximum level from the start.

The intent is to give that -- actually, in the calculations, we gave that resuspension for 3,250 hours per year. So

we're trying to account for that resuspension, including in the control room while the shot is going on. That is being accounted for, so the argument about the room not being airtight or tracking some contamination in there, that's accounted for.

And, lastly, I'd like to point out that the uranium handling, you know, we've done this estimate long ago based on purchase orders. It was nowhere near 100 percent of their job. It was a part of their job, and we have an estimate, based on the purchase orders, of 100 hours per year. I think the maximum might hit 400 something hours per year, but it's nowhere near 3,250.

I don't think it's more scientifically valid to take the average of the air concentrations you get from handling uranium and apply it to 3,000 hours when they only actually handled it for something less than 400. And to call that more scientifically valid makes no sense to me at

all.

DR. ANIGSTEIN: Well, the main point is you cannot have the dust settling, if you want to have a 30-day settling and there are frequent handlings, then it would be in there all the time because if you assume if it's at least once a month that they're handling it and it takes 30 days for it to settle, then it will always be there. You can't have it settling and not have anyone breathing it. You can't say the dust is in the air, it's settling on the floor, but that doesn't count. The only thing that counts is it's being re-suspended from the floor after it settled. That's where the problem comes in.

MR. ALLEN: That's a separate argument, and we've talked about that one before several times, too. And that is, basically, it also doesn't instantaneously reach this equilibrium level. It takes some time to build up this, you know, when you

start working with uranium, it doesn't instantly become --

DR. ANIGSTEIN: Yes, but that's not in the model. That's not accounted for. And before you had, the problem we had before was instantaneous settling, that it only settled, that it stopped settling the moment the activity, the handling stopped. Now we're pulling in 30 days, but if you throw in the 30 days, it's always there. If it's at least once a month, it's always going to be there.

MR. ALLEN: Bob, we're accounting for resuspension.

DR. ANIGSTEIN: No, but you're saying it's settling. You're saying there is 68.7 dpm per cubic meter in the air for 30 days while it's settling to the floor, and this same phenomenon should take place for each batch of uranium that comes in to be handled and radiographed. So, therefore, it's continuous. You can't say, you cannot say the dust is settling but no one is breathing it

until it hits the floor and it gets re-suspended. That's illogical.

MR. ALLEN: No. What we're saying is the level of contamination that reaches the balance between removal and production can be estimated by the settling rate that we've been using and settling it for 30 days. Whether that happens in 30 days, 7 days, or one hour, it means the same.

DR. ANIGSTEIN: But if you're sticking with the 7.5 and ten to the minus four deposition velocity, which has been agreed to, then you can't have it both ways. You can't say it settles immediately and have it as settling at that slow rate.

MR. ALLEN: No, I'm saying the combination of that settling rate with that time gives you the equilibrium value that we saw for the Adley paper and --

DR. ANIGSTEIN: I know, but all of those are not, none of that is this intermittent handling that we have here.

MR. ALLEN: That is true. If your removal mechanism continues and your production mechanism does not, then the actual equilibrium value would be somewhat lower.

DR. ANIGSTEIN: Yes.

MR. ALLEN: Okay. So we're overestimating the equilibrium --

DR. ANIGSTEIN: No, but you're not. You're using two different ratios. You're using a ratio of what's on the ground to what's in the air of ten to the minus fifth and the ratio of what's in the air to the ground is five to the minus four. Those two are just completely inconsistent.

MR. ALLEN: I think bottom line is we're overestimating the surface contamination using these numbers --

DR. ANIGSTEIN: I don't think so.

MR. ALLEN: -- and we are estimating the inhalation from direct handling for the time period that they're direct handling based on the 95th percentile of the

distribution we agreed to.

DR. ANIGSTEIN: The 95th percentile for those few minutes, those 15 minutes per 75 minutes, is claimant-favorable. But the assumption that you can have something in the air -- I mean, you just cannot, if you go with these parameters, then you cannot have the result that you get and not have it, and have it plausible and consistent. It's just not consistent. You cannot have this stuff settling to the ground and no one is breathing it while it's settling.

MR. ALLEN: Well, yes, you can. People leave the shooting area, Bob.

DR. ANIGSTEIN: But you're assuming that this contamination is all over. Yes, they leave the shooting area, but they don't leave it for 30 days.

MR. ALLEN: Assuming the 68 is entirely from resuspension.

DR. ANIGSTEIN: No, but it's

falling. You have it falling continuously for 30 days. That's the only way you can build this up is to, the only way you can get this number, this 1.34 times ten to the fifth square meter, is to have it falling for 30 days, and it cannot be falling for 30 days at 68.7 dpm per cubic meter and then say but nobody is breathing it.

MR. ALLEN: No, we're assuming it's building up over some time to an equilibrium value, and we're bounding this estimate by assuming it's there from the start. We know that's an overestimate.

DR. ANIGSTEIN: But you can't have -- whatever it is, if 30 days, and I think 30 days was the agreed-on number. I don't think it's an excessive number. I think there was even talk about having it higher in our critique. So if 30 days is what's agreed to, you can't say it's an overestimate and we won't use it. If 30 days is agreed to, then you have to say it's settling for 30 days. If

it's settling for 30 days, someone is there breathing it for 30 days. And then next month they come in with another shipment of uranium, and it starts all over again.

We just can't accept this calculation. It's not valid.

CHAIRMAN ZIEMER: Okay. I'm going to jump in at this point because I'm assuming the rest of the Work Group is in the same boat I am. I just got this paper yesterday, and I haven't had a chance to observe it. I've heard both of the arguments here now, and it seems to me this is one where we're going to have to consider it further. I don't know. Other Work Group Members, are you in the same boat that I am that we need to look these papers over in more detail and, having heard these arguments, try to sift through this?

MEMBER BEACH: Paul, this is Josie. I definitely agree with that. I think SC&A and NIOSH really need to come together a little closer because they're far away from

each other on this.

CHAIRMAN ZIEMER: These arguments are also very technical, as opposed to philosophical, at this point. Who else is trying to comment here?

MR. KATZ: Oh, it's Ted, Paul. I'm just, I just wanted to suggest this is exactly the kind of thing that we do have technical calls for because there's a communication issue, which is why they're apart, too. And until they can hash that out, nobody gets a very clear picture of what the bottom line is on either side. So this would be a good one for them to actually have a technical call and just straighten out where each of them is coming from here and why there's this different understanding of what's being said.

CHAIRMAN ZIEMER: I agree with that. And I think what we'll do is ask NIOSH and SC&A to arrange such a technical call, let us know when you're going to do it. The Work

Group Members may wish to listen in and be informed as to what that discussion is. And then we can go from there.

I also, before we leave that, I do, in fairness, want to give the petitioner an opportunity for, he had some comments on the residual period, as well. And, Dan McKeel, if you want to input some comments here, this would be appropriate.

DR. MCKEEL: Thank you, Dr. Ziemer. Yes, I do have some comments. The most salient one is that I have been stating for a long time and in my recent papers for this meeting have in there that I don't think OTIB-70, the model in there, at all recapitulates what happened at GSI during the residual period. So I'm talking now of the residual period. And that is that TIB-70, as I understand it, and I think everybody has been saying, assumes that you know the airborne concentration of uranium, say at the beginning of the residual period, and then you

calculate a smooth exponential function. And I think that's the way it was just described: a smooth exponential function. It decays down over the rest of the residual period, which, in the case of GSI, goes from 1967 through 1992.

And we have provided, that is the petitioners, site experts, workers, have provided really enormous affidavit, eyewitness testimony, and written records that many different companies occupied the General Steel Industries building complex, Buildings 6 through 10, all during the residual period. And, you know, they had various steel production activities going on there, pickling the steel in acid, et cetera, and some of those activities were in Building 6, some of them were in Buildings 9 and 10.

And we have also long pointed out that, unlike what's being talked about right now, the uranium deposition did not only occur in the two betatron buildings. The two

betatron buildings were only reachable for uranium by rail. And railroad cars driven by engines, two different types, would take the cars and the uranium into the betatron building. But along the way, they traversed from the dock, the loading docks. The ingots were weighed. They were handled there. There's old 2006 testimony and some new testimony that the uranium was stored temporarily before and after it was returned to Mallinckrodt in a Building 6 locked metal cage. Then it was put on the railcars and transported -- oh, and those cars were cleaned of dust about twice a year, and they were then transported alongside the factory and through Buildings 6, 7, 8, 9, and 10 into the new betatron building, and then the tracks ran outside and into the old betatron building.

So not only were there, you know -- so that was the situation in the betatron building. But inside General Steel there was all along that transport pathway, we've called

it the uranium transport pathway, there were repeated disturbances. And so what you would have to have is a cyclical model where there were alternating cycles of new resuspension of the dust with the uranium in it and then settling. And the exact periods that those companies occupied the General Steel complex and exactly what they did and how much dust was disturbed are all completely unknown. But a simplistic model, like TIB-70, that assumes a constant level at the beginning or a known level at the beginning and then decaying down in an exponential smooth function, it's scientifically not an applicable model. It can't be used. You've got to come up with something else.

And I don't know. I think that's as much as I can say. I have mentioned this many times, but I need today, I need to hear both NIOSH and SC&A tell me why the reasoning that I've just outlined is not correct. Why do they think that TIB-70 should apply? And I

would also go back and remind this Work Group that applying TIB-70 for the residual period at GSI is a relatively new idea.

Before, there was a model based on the amount of residual uranium that was in an industrial vacuum cleaner found in the old betatron building when DOE came to clean up the site at the end of the residual period, and there was then going to be back-extrapolation to get to the mid-point and the beginning of the residual period. And I think we convinced everybody that the old and new betatron buildings certainly were disturbed mightily with power washings, renovations, reconstruction, and so forth. But right after that and in several papers, in great detail, we outlined that the whole rest of the uranium transport pathway was similarly disturbed all during the residual period.

So I would like to hear NIOSH and SC&A defend the use of TIB-70 for the residual period at all. And to save time, I do want to

get in just a couple of comments that I did not get a chance to make this morning. And that is when you all were talking about assigning ability based on jobs. Then, as I understand it, the two classifications now are people who worked in the production areas, which haven't been defined, by the way, and administrative personnel.

And so my specific questions are, would Dr. Neton and Dave Allen, would they think that clerk and timekeeper would be people that they would place in the administrative personnel. And I'm worried that that might be the case, but we certainly know of both clerks and timekeepers, for example a timekeeper whose job was to track down specific castings that had been inspected by the betatron. Soon or immediately after they were inspected, his job was to go all throughout the plant and look at those castings and certify what was done and where they were and was the problem being taken care

of. And then we know of several clerks who also later became and inherited the job of handling all the film badges, and so they were in contact with the operators, they were in and out of the betatron buildings, and so forth.

I think even for the administrative personnel you're going to have a very, very difficult time saying with any degree of certainty that administrative personnel, including secretaries, that they always worked in the administrative building, which was away from the rest of the plant. So that's another comment.

And the final two comments quick, and that is that overexposure instance, like the radium-226 stolen plumb-bob that was later returned after about a week to GSI, there's a section of 42 CFR, which is Section 83.9, and there are two subsections in there that say that on such overexposure incidents, it is not sufficient for only worker affidavits to be

assessed, that NIOSH, and it says that specifically, has to do further investigation into those incidents.

And this is a situation where the one we've documented in our paper that happened in October of 1953 where it's very possible and likely that more than just the person who took the plumb-bob was exposed to that radium source. The newspaper stories, some of them say that the radium source was kept at the plant for a while. We know that it was recovered offsite, but we don't know whether one individual or many individuals came into contact with that. So I believe that's an incident that has to be where the dose has to be calculated not for an individual but for the group of people that may be exposed.

And I guess, in that regard, we're now talking about Appendix BB and dose reconstruction and not about the SEC. And I may be incorrect, but it seems to me that we

need to be talking about assigning doses based on sufficient accuracy, rather than just bounding limits, which, in my view, some of the Members discussed. And, certainly, to some of the Work Group Members, it sounds like some of the bounding limits are, quote, too claimant-favorable or may, in fact, be implausible.

So, you know, speaking for the workers, I think they do need to be claimant-favorable, but I just worry that we're not really addressing the issue of, with all our assumptions and all the, well, I just say guesses that we're making at various things, conditions of the workplace, that we're really operating within the bounds of sufficient accuracy.

And the final thing I would say is that Dave Allen was asked earlier, I think by Dr. Ziemer, how he would handle the 1952 operational period. And I believe Mr. Allen said that he thought that the purchase orders

defined the number of hours that were allowed at GSI for the uranium, and that's the way the source term was defined. Well, I just remind everybody there are no purchase orders from 1952 up through, I think it's March of 1958. So really what you're doing is taking 1958 and later data and back-extrapolating to those early years, but I will remind you that the 1952 documents that I contributed through a FOIA request and that NIOSH contributed a couple of days later and that have led to the official extension of the GSI operational AEC contract period to start October 1, 1952, that those documents actually described different types of betatron NDT research and development work that was ongoing in 1952 and, specifically, they were working with thin billets and they were also working with what is described as a new uranium shield, you know, S-H-I-E-L-D, that was constructed by Mallinckrodt and was being tested and refined at GSI for the betatron work. So this is

really very different kind of work than the NDT work that went on from 1953 through 1966.

I guess that's my comment. I think that that, in other words, the 1952 dose assignments need to be made bearing those other facts in mind. And I thank you for letting me have the time.

CHAIRMAN ZIEMER: Okay. Thanks, Dan. I'm not sure that SC&A and NIOSH would be prepared to address the TBD-70 issues today, but I would suggest that, in the technical call, that they at least try to address the impact those changing conditions that Dr. McKeel mentioned and that we're also aware of in terms of whether or not the proposed approach for the residual period would, in fact, bound those kinds of situations where we have those changes going on.

DR. MCKEEL: Dr. Ziemer, this is Dan McKeel again. If I just may make one comment about the technical call.

CHAIRMAN ZIEMER: Yes.

DR. MCKEEL: Several months ago, I wrote Ted Katz and Josh Kinman to try to find out if there had been -- this was while I was preparing my administrative review on SEC 105 -- I tried to find out if there had been any past technical calls or technical meetings for GSI between SC&A and NIOSH and was told that that information was not really available, they really didn't think there had been, but nobody could be very definite about that issue.

CHAIRMAN ZIEMER: I can tell you that I'm not aware of any. If there were, I'm not aware of them.

DR. MCKEEL: But one thing that --

CHAIRMAN ZIEMER: Because I assume that, if there had been, I certainly would --

DR. MCKEEL: Okay. Well, I appreciate that, and that's what I kind of turned up with. But what I also learned during that is that the petitioner, I'm the

one asking the questions about TIB-70, but there are two things that that arrangement that you're outlining is really not at all satisfactory to me because I have, I cannot listen to those technical calls and there are no minutes and there is no transcript and there are no notes kept. So nobody in the Work Group, unless they choose to listen in, will ever know the results of that technical meeting, except as reported. And all I can say is, having sat through now 16 meetings of this Work Group, I know that we need to see the numbers. I personally won't be satisfied until I see each and every number that's discussed in there, and I need to hear or see in writing the specific reasons why TIB-70 is or is not judged to be a satisfactory model for the GSI residual period.

So I'm not trying to interfere with your process. But I do decry the fact that it is not an open process for the petitioners, and I need to be aware. So I

guess I would ask do you ever let petitioners listen in?

MR. KATZ: This is Ted. The technical calls, we're not going to deal with the TIB-70 issue in the technical call because really those are restricted to clarifying when we have just the kind of communication in technical sort of understanding issues that we have in this case. I mean that's what they're limited to, but we don't do any kind of discussion in terms of agreement or what have you between SC&A and NIOSH on how to deal with an approach. We don't do those in the technical calls.

So, I mean, that will happen, that discussion, if we can't, for example, the TIB-70 response to Dan, I mean, that can happen today if they're ready to address that sort of philosophical question or that general question today or it will happen at the next Work Group meeting. But that won't get addressed in a technical call because that

just is not what we use technical calls for.

DR. MCKEEL: All right. Well, I appreciate that. And I would also say that what I'm really asking for is, I have made my arguments in writing several times and today on why I don't, I don't think the TIB-70 is a satisfactory model. And I would like NIOSH and SC&A to come back to me and say, Dan, we agree with you, or, Dan, we do not agree with you for the following reasons and lay it out, one, two, three, four.

And so I don't think a technical meeting would be satisfactory to answer my questions. And, obviously, I understand that maybe the question can't be answered today. On the other hand, the model is being proposed today, and everybody is talking about the model, so I don't really see why SC&A and NIOSH couldn't answer my question today.

DR. MAURO: I'd be happy to answer it, this is John, unless Jim wants to.

DR. MCKEEL: No, I want to hear

first Dave Allen because Dave Allen is proposing -- NIOSH is the one that's supposed to bound and determine doses with sufficient accuracy. And, Dr. Mauro, I would enjoy hearing your idea, but I want to hear Dave Allen defend his use of TIB-70, please, first.

MR. ALLEN: My defense is pretty simple. TIB-70 was based on more than one site and there's more than one type of site. I think a steel mill, a chemical, at least one chemical place, and a few other sites, and the numbers all came out to be somewhat consistent as far as how fast the available contamination was reduced over time. It essentially comes down to an industrial type of atmosphere. It may not be applicable to an office; but, to a steel mill and a chemical plant, it seems to be --

DR. MCKEEL: Dave, here's my response to that. I have said, just like, you know, you first proposed using TBD-6000 surrogate data at GSI, and then that was

challenged for various specific reasons. And, eventually, you wound up using surrogate data that was really not in TBD-6000, and it did have to be rather stringently, more stringently justified in order to have it acceptable for use.

Now, I've said I'm not -- I think Dr. Ziemer referred to my question as a general question. It's not a general question. It's a highly specific question.

I'm saying that there was not, the model says that you take an initial high level, and by high I mean a level of uranium in the air, and then you model how that decays, that concentration decays, diminishes, over time. And the curve fit is a smooth exponential curve.

And I'm saying that if you just think about what happened at GSI with multiple companies moving in, each time massively disturbing the dust on the floor, along the railroad tracks, in the buildings, and also

inside the betatron building, that that was not what happened during the residual period at GSI. Even at GSI, in the betatron buildings, the old betatron building was constantly used for storing transformers, and we went into all of that information. And things were done in that building basically from the end of the operational period in `66 all the way through at least the late 1988s, the betatrons were stored in there, transformers, PCB-containing oil, et cetera.

So I'm just saying that TIB-70 is not a model for what happened at GSI. And, personally, I don't see how it could be fit as a model for that, regardless of things that you said, that it applies to a few other sites. And I understand that you've widely applied that as a model for AWE sites in general, but I just think it's a poor model for GSI.

DR. MAURO: Can I take a shot at this?

CHAIR ZIEMER: Yes, go ahead, John.

DR. MAURO: Please. It's so easy to get lost in the woods when you talk about this stuff, and let's keep it real simple. Let's, for a moment, make believe we know what the concentration in becquerels per meter squared is on the ground, on the surfaces, in the vicinity where the uranium was handled, and we know it in units of becquerels per meter squared. Let's stipulate that. Let's make believe we know that. All right. Now --

DR. MCKEEL: John, when you're doing this model, when you say where uranium was handled, are you talking about all throughout all the buildings along the transport --

DR. MAURO: Yes. I'll say yes to that.

DR. MCKEEL: All right.

DR. MAURO: I'll say yes to that. All right. So let's assume that we all agree

that, yes, we could place a plausible upper bound on what we believe to be on the surfaces, on the floor in your house right now, okay? And it's there. Now, all OTIB-70 says is that, once you have some good idea of what you think is a plausible upper bound on what the accumulation was on surfaces at the time of the end of operations, what happens then is that, okay, you're no longer adding anything to it. The only thing that's going to happen to the stuff that's on the ground now is it's going to be re-suspended, come back down, and leave through various natural attenuation processes.

So the question you're really asking, Dr. McKeel, is, all right, you have to agree that, if we're stipulating we know what's on the surface in becquerels per meter squared. And then --

DR. MCKEEL: No, no, Dr. Mauro, I'm sorry. This is where my --

DR. MAURO: We'll get --

DR. MCKEEL: -- comment gets distorted. You don't have any measurements --

DR. MAURO: No, no, we'll get there. See, I'm trying to parse it in a way so that we can get our heads wrapped around --

DR. MCKEEL: Well, but don't say if you know the amount that was --

DR. MAURO: I'm going to get --

DR. MCKEEL: -- on surfaces at the end of the operational period. You don't know that.

DR. MAURO: I'm going to show you how we're going to get there.

DR. MCKEEL: Okay.

DR. MAURO: Because that's a tougher problem.

DR. MCKEEL: It's an impossible problem, in my view.

DR. MAURO: I could stop at this point, and then we could leave it to the technical call, but I think I've got the answer to this thing.

DR. MCKEEL: I don't think you better leave it to the technical call because, like Mr. Katz said, that's not an appropriate topic for the technical call.

DR. MAURO: Well, I'll leave it up to the Work Group. If you'd like me to tell my story, I'll be happy to, or we can save it for another time --

DR. MCKEEL: I will be quiet. Go ahead.

CHAIRMAN ZIEMER: Well, right now, right now the model is a separate question. We know the starting point. We're stipulating that, say you know the starting point because, in fact, we have a value that we're using for the starting point. It may be that that's not accepted by all, but the question was how can the TBD-70 be used if we have all these disturbances, and I think that's what you're trying to address.

DR. MAURO: And that's all I'm trying to do.

DR. MCKEEL: Okay. That would be fine. I would appreciate it if you would finish your --

DR. MAURO: Yes, it becomes -- and I understand your question because I struggled with the question for quite some time. So we know, let's say we know becquerels per meter squared on the surface anywhere. We know it. Now, the question is what happens?

DR. ANIGSTEIN: John, use dpm per meter squared to be consistent.

DR. MAURO: dpm per meter squared. Okay. Now, what's going to happen here, what's going to happen is that material is going to be re-suspended. Okay. Starting at day one of the residual period, it's going to be re-suspended. And what you're really saying is, whatever the activity is on day one during the residual period, you're concerned about that resuspension factor. So am I.

And one of the things that I've been arguing for the longest time is that if

you got a place that's very dirty and it's got loose contamination on it, the resuspension factor could be pretty high, okay? Stay with me. NIOSH typically used to use ten to the minus six. I'm sure that, in the circumstances that we're talking about where there's a lot of activity going on, ten to the minus five is probably a pretty good number. But you know what? I'd be the first to say there actually may be certain circumstances over certain short time periods where the resuspension factor could be even ten to the minus four. So I'm not disagreeing with you on that.

But, in principle, on that day one of the first day of the residual period, if you know what's on the surface, you can very readily determine what might be airborne for inhalation by applying an appropriate resuspension factor. And in my mind, the resuspension factor of ten to the minus five, I would say, I would argue strongly, unless

there was clean-up right after they finished operations, if there was no clean-up and the material was allowed to accumulate and it was loose, I would go with ten to the minus five.

And if you're right that there was a lot of really aggressive activity going on in a given room, I would go with ten to the minus four. So you're actually arguing now the judgment of when do you use ten to the minus six, when do you use ten to the minus five, when do you use ten to the minus four. Right now, I mean, what you just described, I could see someone saying, you know, because I do know circumstances where it goes up to the ten to the minus four.

So now let's say a reasonable disturbance is on the order of ten to the minus five, and I'm very familiar with the literature and that's not a bad number when there's loose contamination. But now what happens, though, is that number doesn't -- so you get a concentration in the air on day one

by simply multiplying the activity that's on the surface in dpm per square meter times the resuspension factor, whatever number you decide to pick, and you get what's in the air.

But what happens is that's going to go down. Now, what NIOSH has done in OTIB-70 is they selected a rate at which it goes down, which is 0.00067 per day. That's a very, very slow rate of decline. In fact, in other words, they're being very claimant-favorable, and I know where they got that data. We don't have to go into the details of it, but they picked data in a way that it probably goes down faster than that, but they're going to assume that that concentration in the air is going down very, very gradually.

So in my opinion, if you have found a pretty good number for what's on the surface and we could agree on a fairly reasonable resuspension factor based on the amount of aggression to which the stuff might

have been disturbed, and then there's no doubt in my mind the 0.00067 per day number, the rate at which it smoothly goes down exponentially, is extremely claimant-favorable. You're done. You've got the problem.

So all we're really talking about right now, at least for the residual period, is we need to come to agreement on what we believe to be a reasonable dpm per meter squared that was present on the surface, on average, because when you're dealing with resuspension you're not interested in the high spot, the low spot. You're interested in what the average is because it's an integrative process.

So a good reasonable, plausible upper bound is, for the average concentration of the uranium in dpm per square meter that was on the surfaces on day one of the residual period, we need to agree on what resuspension factor seems to make sense for the kind of

activities that took place in those various rooms during the residual period. And we certainly, in my mind, the 0.00067 rate of decline is a great number, and it's done. You're done.

Now, we have a little work to do. Clearly, we haven't decided what is that activity that's on the surface at the end. And there may be some disagreement regarding what's the best resuspension factor, but this is a very manageable problem. It's just a matter of sitting together, taking our hats off, and put our science together and saying what's the sensible thing to do.

So as far as I'm concerned, the residual period problem, we will solve. The biggest problem we have-- and that's my story on the residual period. The biggest problem we're going to have, and I think, conceptually, I know the solution to this, too. I have a conceptual approach that is fundamentally what Bob described.

You see, what happens is, and shut me off if you think I'm going too far, but what happens is surrogate data was found that says, listen, there's a number of sites out there that were handling uranium and they measured the airborne activity, the breathing zone, the activity for people that were handling uranium, and they said that's a pretty good surrogate data because we're doing it because the same kind of thing was being done at GSI. And --

CHAIRMAN ZIEMER: John, I'm going to cut you off here.

DR. MAURO: Okay. I'll stop here because I think I've got my --

CHAIRMAN ZIEMER: All right. Just one other comment. I think one of the questions, and Dr. McKeel can correct me if I'm wrong, was the issue of the TBD-70 approach looks like a smooth curve when, in reality, there may be what I'll call disturbances along the way, so you get these

spikes. I think you're saying, though, that, overall, the area under the curve, if you get a spike, you still only have so much material that you're dealing with, you're not adding any source term. So in the end, the area under the curve, if you've got spikes along the way, you're spiking something that has already been depleted to some extent.

DR. MAURO: Yes, absolutely.

CHAIRMAN ZIEMER: And so to get the spike, the total or the integrated amount under the curve ends up, over the long term, as being the same. Is that what you're saying?

DR. MAURO: Yes, yes. And the real question is that resuspension factor is a thing that picks up the spike. You see, when I hear the word spike, it means, oh, all of a sudden someone came along and did something to generate, to re-suspend a lot more. And there could be short periods of time where that occurs, but we have a lot of data on that. So

we can pick the right resuspension factor.

And you're right. There could be times when you have more, times when you have less. But it's continually going down because natural attenuation by air turnover is going to cause this thing to drop and the rate of decline overall -- think of it like this: there's a certain number of curies in the building, you know, in the building. And those curies are going down, and they're going down because of natural attenuation. And the approach that NIOSH has picked, the rate at which it's going down is very, very slow. In other words, the 0.00067 per day, so it's going down.

Now, during that time period, yes, you've got periods when you have a little bit more resuspension, periods when you have less. But if you pick the right resuspension factor that you say effectively represents the airborne dust-loading that's due to these processes, someone could very well argue ten

to the minus four for maybe some short periods of time --

DR. ANIGSTEIN: John, remember, we don't need a suspension factor if we simply go with the declining constant air concentration.

DR. MAURO: You could do that.

DR. ANIGSTEIN: So then, as long as you say you agree that it's the same resuspension, the resuspension factor doesn't change during the residual period, then all you need is a declining air concentration.

DR. MAURO: That's another shortcut. But --

DR. ANIGSTEIN: Because, you know, it's back and forth. You end up with the same number.

DR. MAURO: Yes. Well, what you've just done is a shortcut to OTIB-70.

DR. ANIGSTEIN: Well, no, no. I still use the OTIB-70 as a decrement, and that is also shown in other comments -- I know we can't talk about this forever --

DR. MCKEEL: So this is Dan McKeel. I need to get a word in edgewise here.

DR. ANIGSTEIN: Yes. Well, just a second, Dan. Let me answer, you asked SC&A to answer the question. I would like to answer one of your points that you already made and you requested an answer to.

DR. MCKEEL: Okay.

DR. ANIGSTEIN: So please give me a chance. I mean, John has said some of this, and that is of course the actual disturbances are episodic and they're not a smooth curve. The smooth curve is simply an averaging because no matter what happens, on average, there's air coming into the building and air going out. It's not a sealed, it's not a hermetically-sealed system. And every time there's air movement, some of the uranium dust is removed permanently from the building, so there's always going to be some decrease even if, on a given day -- as a matter of fact, the

more you stir it up, the more it decreases because then you have more in the air and it will go out with the ventilation system.

So all of that, when you average it over, if you look at it for any one moment, you're right, it's all over the place. But if you average it out over a course of a year, and doses are almost always assigned on the basis of a year, the smooth curve is not a bad approximation. And the more it gets cleaned up, the less there is. So when you're saying there were aggressive clean-ups, this is actually claimant-favorable because the OTIB-70 approach does not assume any aggressive clean-ups. So if there's a clean-up, it means you washed it down, it went out into the sewers, and it's gone.

CHAIRMAN ZIEMER: Dan, do you have an additional comment? Let Dan make his comment now.

DR. MCKEEL: All right. Here's my comment. Dr. Mauro made this comment. He

said that his model that he was talking about would be a problem if there had been clean-ups, he said unless there was clean-up after operations. Okay. Well, what I --

DR. MAURO: Well, that's, you know, you're misusing -- that was only doing the backwards calculation. I hear where you're going, and I think, again --

DR. MCKEEL: Well, no, you need to let me finish because you don't know where I'm going.

DR. MAURO: My apologies.

DR. MCKEEL: All right. What I'm saying is that each of these companies that came in did different activities and, therefore, if you had to model, truly model what was going on in there, there would be, I understand that there are different daily resuspension and settling rates, velocities, and so forth. On the other hand, if you thought about the residual period as a series of events and each event was a new company

moving in there and setting up operations, doing various steel operations, and then moving out, then, in fact, it would be clear that those companies did various types of clean-up operations once they were leaving, getting ready for the next owner to come in. They were leasing the space. They didn't own the space. They were leasing the space.

And so I think that the proper way to model that mathematically is, first, you would have to calculate each one of those events as a separate -- you'd have to know the amount of uranium in the building, in the buildings as a whole, and then you'd have to know the resuspension factor for that company, on average, and for however long they were there, one month or two years. And we do think that they were there from those limits. Some were there for months, some were there for years. And then you would have to know what the uranium level was in the air and on the surfaces after they left. That would be

your new start point for the next company.

So, you know, you would have a series of curves with peaks, and there's no a priori reason to think that the settling would be exactly the same. If they did different things, the composition of the dust particles, their size and so forth, their mass, what they contain, that could all change.

So these places were constantly being disturbed and made up. And then, eventually, I understand that if you had that series of curves, you may be able to fit an exponential curve. It may take some other kind of curve. That's not the only kind of curve that will fit data, as you all know. But we don't have that data. That's a guess. It's an educated guess, but, basically, what you're saying is, Dan, we have TIB-70 and we've put in certain surrogate data in there, and, by golly, I'm saying, I'm declaring that those are good numbers. Well, if there's no measured data at GSI, which there is, there is

zero measured data, no breathing zone data, no process zone data, no ambient air data at GSI ever at the entire plant in all of operations, then, basically, you're making an educated guess. And I'm saying that if that's the best you can do and so forth, but I can tell you I would never, never buy the explanation that's been given out that that exponential curve actually has a good relationship with real data, except, except for those sites that are defined in OTIB-70, and that's it.

You know, I understand this is a deep philosophical argument. I'm not going to pursue it any longer, but I appreciate the explanations. But I certainly am not convinced, so I think I'll leave it at that.

CHAIRMAN ZIEMER: Okay. Thanks, Dan. Now, I'm going to bring us to a close on this facility for the day. We have two tasks that have to be done. The first one, NIOSH is going to prepare their final model for what I'm calling today the administrators. And

then SC&A is going to review that, and we're hoping to have another meeting then in, roughly, a month or four to six weeks, something like that. And, also, we're going to have SC&A and NIOSH conduct a technical call, which they'll arrange, to deal with the residual period.

MR. CHUROVICH: Dr. Anigstein, this is Dan Churovich. Can I interrupt just a second?

CHAIRMAN ZIEMER: Who's speaking?

MR. CHUROVICH: Dan Churovich. I was there, and let me tell you, you're talking about people handling something, handling radioactive material. What if they don't know what they're handling?

CHAIRMAN ZIEMER: Yes. Well, the models don't depend on whether or not they knew it. We're assuming that they're going to get exposed, so the models will cover that. Anyway, we have those two tasks to complete, and then we will schedule another face, or not

face to face but a phone call meeting. It's got to be a phone call for my purposes at this time.

And so that's where we'll leave it on GSI for today, and I want to move ahead quickly to Baker Brothers. And we had some DCAS responses to the SC&A review. And, Tom, if you're still on the line, you can address the Baker Brothers issues there for us.

MR. TOMES: Okay. This is Tom. Just to summarize, we have received a brief paper from today on talking points for the Baker Brothers ER, and they listed several issues to discuss, the most significant of which was possible contamination levels from fires at the facility. And during the previous meeting, John Mauro discussed or he thought that that could have a bearing on the modeling.

And so, in the interim, I put together some information and sent it out showing that it's likely that Baker Brothers

was possibly not decontaminated, but was at least cleaned up based on information that DuPont required those contractors to sweep and remove all visible residues from the surfaces and machines and ship it back to the government. And that was required for the various contractors that we have records of in 1943 and `44. We don't have specific information on the dates and what specific activities were done at Baker Brothers, but we do have records of shipping and sweepings in 1943, which would have occurred after the fires were under control.

So, based on that, SC&A responded and sent us some information saying that it is likely that they had some clean-up and that they feel that we can likely bound these, but they say there may be some issues with the numbers that we use. The ER assumes a contamination level, an airborne level of 5480 dpm per cubic meter. That's based on a bounding operator concentration for machining

operations out of TBD-6000.

And that was used as the conventional settling and resuspension model that has been just discussed for GSI and came up with an air concentration for the beginning of residual period. And I believe the question that SC&A proposed in their memo was the clean-up, was the supposed clean-up at Baker Brothers sufficient such that the resuspension factor was valid that we used? We used ten to the minus six, presuming if there was some clean-up that there was not a lot of loose contamination.

And just to summarize what they were doing, the operations at Baker Brothers ended approximately, I don't know the exact date, but it ended in August 1944. And they had containers of scrap and residues that were sitting around the facility, and they were there for some weeks later. And there was some records of shipments being, the last shipment that I saw a record of was October

1944 being shipped out of there.

So the ER, basically, uses this information to assume that by the end of 1944 that the contamination levels in there, in the ER would bound those doses and that the resuspension factor of ten to the minus six would also be a valid number to use.

And there are some other issues. I don't know if you want to get into more discussion of that, but there are some other minor issues that SC&A identified in the talking points that we can --

DR. NETON: Yes, Tom. This is Jim. I think that, you know, we have to keep in mind that this was an analysis that was done to determine if there was any SEC issues in the residual period that would keep it from not being an SEC. So, you know, I'd like to, personally -- this is Jim -- I'd like to just focus on those issues for this call.

DR. MAURO: Jim, I agree with you completely because this is where we've really

been paying attention. Certainly, we'll get to the others. These are, basically, issues seven and eight in our original list, and, as you recall in our last meeting, you know, what we really did was we were asked to take a quick look, take a look at it, and we came up with this list of concerns. And the big ones that we felt are the ones you just described.

Bill Thurber and I, and especially Bill did all the heavy lifting and has a good story to tell regarding it, and it's a story that, you know, to go to the end of the story, I think we're okay. That's the take-away. So I take the end of the story away, but it's important to know where we're headed.

I think your arguments, we looked very carefully at your arguments, and I think Bill has a rich story to explain that will help, that will close this thing out. Bill, you there?

MR. THURBER: Yes, I'm here.

DR. MAURO: You got it.

MR. THURBER: Okay. Very, very quickly. In part, speaking to Jim Neton's point about whether these are SEC issues or not, based on our review, we don't think they are.

Let's talk first about the question of the evidence that Tom presented on whether there was clean-up or not. And, certainly, it's a judgment call as to whether you can irrefutably say that clean-up was done or not. Rather than making that judgment, what we said in our memo is, look, you don't have to make a judgment as to whether clean-up was done or not, but you do then have to make a decision as to what resuspension factor you will use.

So looking at the problem that way, you don't have to say, well, the evidence is irrefutable that clean-up occurred. You just say: we're not convinced, so we're going to adjust the resuspension factor.

But that leaves open the question

of whether any, whether the chip fires were sufficient to cause initial levels of contamination at the beginning of the residual period that would be greater than those you would obtain by using TBD-6000. And we provided information in our memo, well, TBD-6000 says that the limiting air concentration for machining operations was 5,480 Dpm per cubic meter. And that happened to be a worst-case number from all of the machining operations that they looked at in TBD-6000, which were originally derived from the paper by Harris and Kingsley. And that number of 5,480 dpm per cubic meter was for centerless grinding. They picked that as representative of any operator doing machining.

In fact, typical machining operations such as running lathes, the number was one to two orders of magnitude lower, but that's the level of conservatism that was built into the generic operator category for machining in TBD-6000.

Well, we looked at some data that was in the Harris and Kingsley paper that was not woven into TBD-6000, and they said, for the case where a fire actually occurred, it wasn't with machining but it was with briquetting uranium turnings, which would be quite similar, that the average exposure was only 600 dpm per cubic meter. Again, about an order of magnitude lower than the generic TBD-6000 number.

We also looked at data from Adley, a paper that we've talked about on a number of occasions in the past, and they had machining data. And in many instances, their machining data noted that there was heavy fume or burning during the machining operation. And in none of these instances that Adley quoted, and I think there was seven or eight of them, was the airborne concentration close to the generic TBD-6000 limit.

So we concluded that the TBD-6000 value of 5,480 dpm per cubic meter was an

appropriate bounding number, even if there were chip fires. And so those were basically the two conclusions that we arrived at as, A, if you don't believe that clean-up occurred, you can deal with that by adjusting the resuspension level; and, B, the TBD-6000 generic number for machining adequately covers the air concentration from chip fires. And we feel that both of those are not SEC issues.

CHAIRMAN ZIEMER: Okay. Thank you very much. Any questions, Board Members, Work Group Members?

MEMBER MUNN: This is Wanda. I don't have any question, just a comment. Earlier, it was indicated that the two items that were being addressed were items seven and eight of the original report. I didn't go back and look at the original report, but Bill's report covers items eight and nine, I believe.

DR. MAURO: I may have had the numbers wrong, Wanda.

MEMBER MUNN: Oh, just a nit, for the record.

CHAIRMAN ZIEMER: Okay, thank you.

MEMBER MUNN: It was an excellent report, easily understood, and it looks clear to me.

DR. MAURO: Just to add a little bit. What the real question is, when you look at the story that's being told, NIOSH makes a very good argument that it probably was cleaned up, you know, because that was the practice that was being used widely at that time and there's good reason to believe that they probably did clean up after the fires because of the practice that was involved. But we don't actually have direct statements, you know, that this happened at this facility.

So we asked ourselves, okay, so someone may not accept that. I mean, someone may say, well, listen, unless you have affirmative proof that, yes, it was cleaned up, but we're saying that, even if you don't,

you know, if it was cleaned up, that's the end of the story, and you go, you know, and then you use ten to the minus six resuspension factor. But if it wasn't cleaned up, let's say someone says, well, you know, we think it -- but even then, that 5,000 number is so large that it envelopes even if there was some fires. But, of course, then, if you make that assumption, then you don't use the ten to the minus six. Then you use the ten to the minus five.

And I think that's where we are right now. And this is a judgment that, I guess, needs to be made because we're not saying that the answer, everything is done. What we're really saying is, depending on which path you want to go down, whether you want to say, yes, it was cleaned up because of the evidence as laid out in the report. Well, if you go with that, then go with that ten to the minus six resuspension factor and the 0.00067 per day depletion rate.

If you think that, well, we like to be a little bit more claimant-favorable since we don't have all that direct evidence, okay, don't do that. The airborne concentration is fine. Go with the standard approach, but don't use ten to the minus six, use ten to the minus five because it wasn't cleaned up.

So it's really, that's the choice that needs to be made by, I guess, NIOSH and the Work Group, which approach. But, certainly, it's a solvable problem.

CHAIRMAN ZIEMER: Okay. Thank you very much. I did want to check with Ted. Ted, were there petitioners on this one that wanted to comment?

MR. KATZ: No. Paul, I think the message we got is that the petitioner here is already fine with what happened with the SEC action and was not planning to participate.

CHAIRMAN ZIEMER: Okay. So I think we can -- what do we need to do action-

wise? Remind me. We need to recommend to the Board on the residual period, or where do we start?

DR. MAURO: This is John. Yes. We're dealing with whether or not the residual period was -- you know, the SEC is covered, but the residual period is a question. And there's a strategy that was adopted for dealing with the residual period, and the only thing that we brought up was this fire thing. Well, that may mess you up a little bit because does the approach that NIOSH has adopted, is it adequate to envelope and deal with the fact that there were indoor fires?

CHAIRMAN ZIEMER: Well, you're basically saying, either way, it still is appropriate for bounding this.

DR. MAURO: Yes, yes, yes.

CHAIRMAN ZIEMER: And I'm not sure it's going to give a very different answer. It may be a slightly different one, but the recommendation we have to make to the full

Board is whether or not an SEC should be provided for the residual period; isn't that correct?

MS. LIN: Dr. Ziemer, this is Jenny Lin with OGC. And Ted can correct me if I'm not right on this, but my recollection is that the Advisory Board, when they voted on recommending adding the SEC Class for the operational period, they specifically leave open the questions about residual contamination and then also task the Work Group to evaluate the dose reconstruction methods for the residual contamination period. So do you think that the Advisory Board will require a recommendation from the Work Group?

CHAIRMAN ZIEMER: Right. So what we would need would be a motion to make a recommendation to the full Board on the residual period.

MS. LIN: That's my understanding.

MEMBER BEACH: So, Paul, this is Josie. I want to be clear because I heard

John say that we could go, we could decide either ten to the minus five or ten to the minus six. John, is your recommendation ten to the minus five or minus six?

DR. MAURO: Where I come down on this, okay, and understand that this is a judgment call, there is a lot of evidence that the standard practice at the time for this type of facility at that time and the process that was used is that they did clean up. The fact that we --

MEMBER BEACH: Well, let me stop for just a sec. So I understand that --

DR. NETON: So, Josie, this is a Site Profile issue, I think, that we're dealing with at this site.

DR. MAURO: Oh, now, we're dealing with a Site Profile. Right.

MEMBER BEACH: You're absolutely correct on that, yes.

DR. NETON: So I don't know if that needs to be decided before the Work Group

recommends to the Board whether an SEC should be added during the residual period, I guess.

MEMBER BEACH: You're absolutely correct. Sorry about that. So I'm good.

CHAIRMAN ZIEMER: It could be either one, but it still is a tractable problem. And we don't have to decide that at this point, yes.

MEMBER BEACH: Okay. So we're just looking at the 1945 to `96 time period, whether it's an SEC or not. I'm clear. Thank you.

CHAIRMAN ZIEMER: Yes. So who wishes to make a motion?

MEMBER MUNN: I'll be glad to make that motion. Based on the information that we have today, it appears that my motion would be that we recommend to the Board that an SEC not be granted because it is possible for dose reconstructions to be done for Baker Brothers for the time in question.

CHAIRMAN ZIEMER: For the residual

period.

MEMBER BEACH: And this is Josie. I'll second that.

MEMBER MUNN: For the residual period.

CHAIRMAN ZIEMER: And Josie seconded it. Further discussion? Okay. Let's just get a quick individual vote. All in favor -- well, John, is John back? Wanda?

MEMBER MUNN: Yes.

CHAIRMAN ZIEMER: Josie?

MEMBER BEACH: Yes.

CHAIRMAN ZIEMER: I'll vote yes. Two yeses. It's not required that John vote. I'll simply report to the Work Group or to the full Board that we're recommending that an SEC not be granted for the residual period for Baker Brothers.

Ted, can we do this on the phone call, do you think, or do we need to go for the full Board meeting?

MR. KATZ: Well, my only question

-- well, I think we do because, one, it's not on the agenda.

CHAIRMAN ZIEMER: Okay. Right, right.

MR. KATZ: And, usually, I think that's important for petitioners.

CHAIRMAN ZIEMER: Yes, we'll do it in the full Board meeting then.

MR. KATZ: Yes.

CHAIRMAN ZIEMER: Right, okay. Let's move on to Joslyn, and I think all we have is a brief report of status from DCAS, right? On Joslyn?

DR. NETON: Actually, I think that, well, I didn't know whether SC&A was going to present what they provided. We haven't had this very long. It's only been a few weeks, and we're still working on it. So we really don't have anything to report as far as our reaction to the report at this time.

CHAIRMAN ZIEMER: I think I would prefer that we simply leave it at that and not

discuss the review at this point, if that's all right, in terms of time and personal issues here with me. So if there's no objection, we'll carry Joslyn forward to our next meeting, and that will give NIOSH a chance to complete their responses. Is that agreeable?

MEMBER MUNN: Reasonable, yes.

MEMBER BEACH: I'm agreeable with that.

CHAIRMAN ZIEMER: Okay. Let's move on then to Simonds Saw. I've got to pull up my file here. Let's see. On Simonds Saw, let's see, we have some fairly recent NIOSH responses on that. Is that where we are on this?

MR. TOMES: This is Tom Tomes. Yes, we just forwarded updated responses from NIOSH to the matrix a few days ago. We were looking at a few of the issues that we had left open on responses previously.

CHAIRMAN ZIEMER: Right. You want

to go through those with us quickly?

MR. TOMES: Yes.

CHAIRMAN ZIEMER: Does anybody have the matrix?

MEMBER BEACH: This is Josie. I do.

CHAIRMAN ZIEMER: I think we have a copy dated -- I don't see a date on my copy here.

MEMBER BEACH: April 23rd is what I have.

CHAIRMAN ZIEMER: Yes, April 23rd. Right. Go ahead, Tom. You want to cover to that?

MR. TOMES: Okay. I'll just go through the findings one by one. Finding one concerns discussion of the external dose models, and our response to that was that NIOSH believes that our model was sufficient. SC&A had questioned why we did not use the extrapolated film badges that were available. That issue concerned, I think there were 20

film badge results from a seven-day period, and NIOSH did not use those. We did not consider them to be a sufficient representative of the doses, so we went through some other methods to reconstruct dose.

And SC&A pointed out that if you extrapolate some of those film badges that some of the workers would have had a higher dose than what the NIOSH model is. And I went through and reviewed those and concurred with SC&A's numbers, but some of those badges, in particular the highest badge results, was suspect. Based on all the survey data we have, we just didn't feel that that was a valid result and we feel that the methods we chose is representative of the external doses. And that was our response previously, and we didn't add any additional response to that.

MR. BARTON: This is Bob Barton with SC&A. Just to kind of clarify a little bit on that finding, I think this wasn't

necessarily that we thought the film badge data should be used as opposed to the approach that NIOSH adopted, which depends on some surrogate data from Aliquippa Forge. They had one general area measurement that was used in some MCNP runs to develop their external dose values.

What we're basically saying was we do have these film badges, so let's take a look. Let's just extrapolate them to the full year, like what was just described, and what do the numbers say? And we found that, if you extrapolate them, we found that some of the workers did have higher external doses.

So the intent of that finding was not to say, well, now you should replace the methods that NIOSH used with these film badge data. Really what we're saying is, given that we have these data and a way to compare them, NIOSH should consider modifying their approach to ensure that the external doses you're going to be assigning are going to be favorable to

all the claimants, the highest-exposed people, because, as the film badge data show, it sounds like at least one of those workers may have suspect results. But there were others, too, that still had a higher extrapolated external doses then what they would get from the TBD methods.

I'd also like to note that, in extrapolating those film badge, we didn't include any sort of, you know, ambient dose from between rolling periods. So, you know, when they're not rolling uranium, there's still contamination present at the site. And NIOSH took that into account in their number. We didn't take it into account in our number. So, basically, what that would do, if you did add that in, it would add about another rem and a half. So you can kind of add that to those numbers.

And, also, I'd like to note that those film badges were taken in 1949, I believe, which, if you look at Simonds' plant

history, you know, they started off and they didn't really know a whole lot and, you know, things were very contaminated. They started instituting some industrial controls. Things got a lot better, and that's kind of in that 1949 period where things get a lot better. So those film badges themselves might represent sort of a more ideal condition at the plant than would have been experienced throughout the plant history.

So, basically, what we're saying is, we're not saying, you know, pull out your method, which probably is a very good estimate what the actual external exposure potential was. But given that we do have these data and they show that some workers likely experienced higher external doses, then maybe you want to go back and sort of modify your approach so that we can be assured that, when we do assign external doses, that it's going to be bounding to all the workers there.

And, also, I think it's important

that in the TBD you kind of discuss those film badge results similar to the way of if you extrapolated it and, you know, explain that the rationale for why that one worker who had the rather high external results that maybe his was suspect, but then we have these other workers that may have had higher doses, but we looked at this, and, you know, because of our proposed model, we are, in fact, bounding.

So, again, we weren't saying that, you know, you should use this film badge data only and throw away everything else. We're just saying, in light of it, you should consider modifying your approach.

MR. TOMES: Bob, this is Tom. I did look at that data quite a bit, and I agree with you that the TBD, the TBD mentions these results, but it doesn't really go into an analysis of those results. But I agree that those should not be used as a sole basis for assigning the dose.

What I find surprising, if you

discard the questionable results, I found that the numbers in the TBD agreed with the extrapolated film badges better than I would have expected. Some of those values in the TBD were derived with, some of them had relatively large GSDs. And you take all the uncertainty into consideration, the numbers are in fairly good agreement.

MR. BARTON: Well, no, I agree, it serves both to validate that your approach is accurate but also that these film badge data results are accurate. All I'm saying, I guess, is that, you know, given the fact that we have these film badges and if you extrapolate them out and you consider the ambient dose, which we hadn't done in the original review, and the fact that the film badges themselves were taken during a period when they had their industrial controls in place so, again, radiation levels were a little bit smaller, you know, all these things combined, you know, maybe you should take a

look at those methods and consider increasing the assigned external dose just so you can ensure that you're going to be bounding. That's where I come out on it.

MR. TOMES: I disagree with that, Bob. I agree that looking at these badges, Jim, is a good thing, and maybe to explain a little better why we're not using them as the basis for assigning doses. But taking one measurement and extrapolating it for the entire year just strikes me as being -- and especially ones with the highest, using the highest value that is suspect anyway, just doesn't strike me as being a good practice.

MR. BARTON: No, no, that's not, I did not say they use the highest value. I'm just saying --

MR. TOMES: No, but none of the other ones exceed. And the fact is that we have a large --

MR. BARTON: No, no, no, there are others that exceed it. I mean, I'm looking at

-- they review at least 6 of the 20 workers.

MR. TOMES: And do you assign those as a constant value or what do you do? I mean, we already have a large GSD to account for the uncertainty in the model. I forget what it is, Tom, but --

MR. BARTON: There are several different ones because of the components. I believe the large one is 4-point something.

MR. TOMES: Yes, 4-point something GSD on the central estimate, which is quite generous. So we're acknowledging, by doing that, that we're not 100 percent certain that the central estimate is exactly right, but we acknowledge that there's another level of values. To increase the central estimate just based on one film badge measurement, to me, doesn't make sense. That's our opinion, and I think we'd be happy to explain maybe that a little better in the TBD, but I can't see increasing the dose based on those badge measurements.

MR. BARTON: Well, that's certainly a judgment call for the Board. And I do agree that it would be definitely beneficial to the TBD to put out that rationale that, listen, we do have these results and, even though if we extrapolate them out, and take some of these things into consideration, like the fact that contamination levels were a little bit lower here and --

MR. TOMES: Well, I don't think the definition of lower --

(Simultaneous speakers.)

MR. BARTON: -- our work is going to be favorable to the claimant for, you know, reasons A, B, and C. So, I mean, if that argument is sound and everybody agrees with that, then I wholeheartedly agree with let's put that rationale and that text into the TBD so that, you know, as people read it and they say, well, there are film badges, what happened to them, you know, what are they

like, you can say, well, we looked into that and for these reasons we feel our model is not only more accurate but also more claimant-favorable.

MR. TOMES: This is Tom. I believe the model is more claimant-favorable. I've compared these, and I just pulled up the TBD and some of the dose has a GSD of 5.7. And the model allows for, I guess you could say the model allows for more uncertainty than the film badge does in some regard.

DR. MAURO: This is John. I think I can help out a little bit, too. I understand where we are on this. Jim, you recently made a very nice demonstration where you pointed out that when you put in a GSD of five or four, whatever you're putting it on a number, and with a geometric mean, you could say to yourself -- this is a very important point, and I think it's worth just spending a minute or two on it.

When we look at some numbers, very

often we will look at the arithmetic mean and we say here's the number, here's the dose rate. Let's say it turns out to be, we come up with a number that's lower than the geometric mean that NIOSH might come up with by a factor of two, three, or four. Someone might say, oh, my goodness, you folks, SC&A, are coming in with an arithmetic mean with no uncertainty that's four times higher than the geometric mean that NIOSH is coming up with. And my reaction was: we can't have this, you know. We're coming in four times higher.

But then Jim went through a calculation, and we just went through this, and it's the same situation we have here. If you come with a number that has a geometric mean, a value, that's, let's say, lower than my arithmetic mean by a factor of four, one would say that's a lot.

But then when one realizes that Jim is also assigning -- it sounds like I'm promoting it, but I agree with the argument.

When they assign a geometric standard deviation of five on top of that geometric mean, and then they run IREP and they pluck off the upper 99th percentile dose, what happens is you end up getting a Probability of Causation that's higher using the method that Jim just described.

So what I'm hearing now, we have a similar -- Jim, tell me if we have a similar situation here. If you were to use the film badge data that might be somewhat higher, as pointed out by Bob Barton, and your standard uncertainty, I believe, of 30 percent on the spread on a film badge reading, as compared to saying, well, using some other method that comes up with whatever the model is that has a geometric mean of a value and a geometric standard deviation of about four, is that what we're really comparing here?

MR. TOMES: Well, if you're going to say that 30 percent would be the uncertainty, I'd say yes. I mean, it's hard

to predict in general, but when you're comparing 30 percent, normal distribution of 30 percent to a GSD of four, even five, hands down, the distribution --

DR. MAURO: Your PoC is going to come in higher. Well, I guess then my question becomes, if you were to use the film badge numbers, you extrapolate the values that were referred to by Bob, you would have to pick a number and assign some uncertainty to it when you inserted that into your IREP calculations.

MR. TOMES: Right. And I would have no idea what uncertainty was assigned --

DR. MAURO: Yes. Well, you see --

MR. TOMES: -- to one measurement based on one campaign. Was that the highest campaign? Was that the high value? Who knows? There's no pedigree on this film badge data at all. It's just one measurement at one point in time.

DR. MAURO: I only bring this up

because, even though I've been working on this, this project for quite some time, the light went on when I realized that that when you assign a GSD of five to a number with a geometric mean, I usually just look at the geometric mean and then do my own calculation. Very often, it's an average. I'll add up some numbers and say, how close do I come? And if I come pretty close, I'll say, okay, everything looks okay. But if I come in four times higher, I say, oh, something is wrong, and that happened recently.

But then Jim pointed out but, no, we're not using a fixed value of what the dose is. We're using the geometric mean with a standard deviation of five. And then he ran IREP, and he came in with a Probability of Causation that was much higher than mine.

So all I can say is, to help out here a little bit, this is something that I learned only in the last month, that I probably should have known for quite some

time. If we have a similar situation here where you're assigning a fairly large geometric standard deviation to your number, that probably will envelope what might be relatively small. How big are the differences between the number you came up with, Bob, and the number that NIOSH is using for this particular person?

MR. BARTON: In the highest case, it's about 20 roentgen. And like NIOSH pointed out, they have reason to believe that that measurement is suspect, and, you know, there are some that are ten and some that are a little smaller than that. Honestly, in listening to this discussion, it sounds like there's a very compelling argument to say, no, no, no, what's in the TBD right now is, in fact, the most claimant-favorable method, even in light of these limited film badge results. And I think maybe the solution here is, well, let's put it in there. Let's lay all the cards on the table. Yes, we have these film

badge results, and if you were to do this sort of exercise where you extrapolate to a year, yes, you will see some of the results on an annual basis are higher, yet the method that we've chosen is, in fact, more claimant-favorable for the reasons that are kind of being laid out here.

DR. MAURO: I know that may be a little bit of extra work for NIOSH, but I think telling that story is important.

CHAIRMAN ZIEMER: Well, I'm wondering if it wouldn't be helpful, Jim, if NIOSH went ahead and expanded here on this response or to the SC&A preliminary response. And that will, I think, help the Work Group, as well.

DR. NETON: Well, I thought Tom -- Tom, didn't you do that? I thought you provided the --

MR. TOMES: I didn't provide any additional response on these external dose issues.

DR. NETON: Yes, and I'm happy to revise the TBD. As you're going to see in the next discussion, we're going to revise the TBD anyway for various reasons. And we're happy to go in there and add this logic into the TBD. If you want more explanation other than what we just talked about, we can do that, as well, prior to modifying the TBD. It was in our discussion that occurred just here, I think.

CHAIRMAN ZIEMER: I was looking at the finding itself, and I think on this finding the last response we have is the SC&A preliminary response.

DR. NETON: No, no, I thought we provided a response on top of that.

MR. TOMES: Not on finding one, Jim.

DR. NETON: Oh, we didn't?

MR. TOMES: No.

MEMBER BEACH: Just on some of them.

DR. NETON: Oh, okay. I'm sorry. I thought we had done something on finding one and presented --

CHAIRMAN ZIEMER: Yes, this one didn't have it in, and I think that would be helpful for us --

DR. NETON: Okay. We will do that then. We will provide that.

CHAIRMAN ZIEMER: And then I don't know the extent to -- well, what we all really want to do is close these findings. And I'm hesitant to close this just based on this discussion without really, I mean --

DR. NETON: Well, I think we would hold this finding in abeyance until we modified the Site Profile.

CHAIRMAN ZIEMER: Yes, right.

DR. NETON: But since we haven't really responded in writing yet, I think we should.

CHAIRMAN ZIEMER: Yes.

DR. NETON: I thought we already

had. Sorry.

CHAIRMAN ZIEMER: Yes, I think that the same is true on the second finding. We have the preliminary response from SC&A, but we don't have a NIOSH response on that one.

DR. NETON: Well, Tom has gone and done some selective responses that are going to -- well, we address what we thought were the big ticket items. The first one we were pretty positive that we didn't need to modify it, and we will respond to that more fully. But the second thing that we're going to talk about, I'll let Tom deal -- unless we want to talk about this a little more.

CHAIRMAN ZIEMER: No, I think we can move ahead.

DR. NETON: And the second issue has to do with the reconstruction of internal dose and --

MEMBER MUNN: Are we going to address these in order?

DR. NETON: No, no, we're going to address finding one and then, Tom, what's the next finding that we're going to --

MR. TOMES: Well, it covers more than one finding, the intake model. Well, these findings, finding two concerns the exposure studies.

CHAIRMAN ZIEMER: Finding three, and that gets pretty much repeated. I think your finding three is the one you want to focus on probably, right?

MEMBER MUNN: Yes, I think so. The urinalysis --

DR. NETON: Well, actually, well, here's the bottom line is anything to do with internal dose is going to change at Simonds Saw and Steel, and that's because this Site Profile was written, it was one of the very first Site Profiles that was written. And SC&A correctly, in their review, identified that, you know, there are some things that just are different than what we normally do.

For instance, you know, taking the data and multiplying it by a factor of two to make it claimant-favorable and, even in the residual period, how we handle, you know, this was written before TIB-70 was done.

So we are going to go back and revise the Site Profile to be more in line with our current way of doing business in the internal dosimetry world, and that would be to, you know, take the log normal distribution, the data, pick the 95th percentile, and use that in the reconstruction of internal dose, as well as using the TIB-70 approach for the residual period, which would take the last measured air sample during the operational period, use that as a starting point for modeling the residual air concentrations over time.

So those cover a number of these findings. I think there's three or four that are wrapped up in this internal dose issue. And we're going to do that. I mean, that's

something that we just have to do. It's probably something we should have done earlier, but so be it. It's time.

CHAIRMAN ZIEMER: Yes, I think your finding three and finding four, those two, and let me see, maybe finding five, as well --

DR. NETON: Tom, I think you had a handle on which ones were affected.

MR. TOMES: I can go ahead through. I just refer to the matrix to keep it straight. I think it started with, the internal discussion started with finding two, and that was where one of the findings was, additional review of the air monitoring data, and I believe that we concluded that the analysis was the way to go on assessing intakes for that one.

CHAIRMAN ZIEMER: Right. But you didn't provide us any wording on finding two, but you did on finding three.

MR. TOMES: Well, my initial

response, I do have an initial response on finding --

CHAIRMAN ZIEMER: Right, right, right, right. We had the initial response.

MR. TOMES: And I think SC&A agreed --

CHAIRMAN ZIEMER: SC&A agreed on that one, right.

MR. TOMES: And then finding three we just discussed, that we're going to revise the TBD. And finding four concerns different exposure categories for the workers in the mill. And I think my response to that is listed in the overall response to finding three. We've looked at that pretty closely, and SC&A correctly points out that we have information on exposures at different shifts and different workers and I spent hours going through the urine data and trying to correlate that information with the urine results, and the information is inconclusive. Why I say that is because one category of worker may

have the highest exposures on a certain day, another category on a different day, and same for the shift work. It just does not seem that we can parse the data sufficiently to be confident that we could separate the workers out.

Additionally, there's workers whose particular function is not known. So to try to assign a lower dose for a certain worker would be very difficult, and that's why we're proposing to revise the TBD and specify the 95th percentile. And that's, basically, the crux of finding number four there.

CHAIRMAN ZIEMER: Well, let me interrupt here a minute, just so we can sort of be consistent on this. On finding two, it's basically SC&A said they agreed to, but they did have a caveat there. And I need to ask SC&A, are we in only partial agreement on finding two?

MR. BARTON: Dr. Ziemer, this is Bob Barton. Yes, there is that caveat there

at the end of finding two. Findings two through five really all concerned the internal dose model during the occupational period. And as NIOSH just explained, they're going in and they're going to revise that. And the fact that they're going to be applying the 95th percentile, and we've had some internal discussions at SC&A with John Mauro and John Stiver, and we concur with that position. Because there's so much variability in the exposure potential that is seen in the daily weighted exposure reports, I mean, you could have two workers on the roughing roll, and the worker on the east side has a magnitude, an order of magnitude higher exposure than the one on the west side.

So there's a whole lot of variability, and that was really the crux of our concern with these four findings on the internal dose model. And for my mind, at least as it stands right now, the proposed approach of going through and, if you're a

mill worker, since you don't really know at any given time which job type was going to have the highest exposure potential, you can assign the 95th percentile and assume a chronic intake rate. And we believe that really kind of puts our major concern there with the internal dose model to rest.

CHAIRMAN ZIEMER: So I'm merely asking whether we can go ahead and close, for example, finding two at this point?

MR. BARTON: I think probably. We are in agreement, but it kind of does necessitate those changes --

CHAIRMAN ZIEMER: Yes, it's more to put it in abeyance maybe.

MR. BARTON: Right. That's what I would recommend.

CHAIRMAN ZIEMER: So on finding one, we're just going to leave that in process because we don't have the words on it. Finding two, are we okay to do it in abeyance? Let me ask the Work Group.

MEMBER BEACH: Paul, this is Josie. I'm okay with that.

MEMBER MUNN: Sure.

CHAIRMAN ZIEMER: Okay. Which means, basically, there's agreement that we have to yet see it in the final document. Finding three, are we going to be in the same category there, I guess. SC&A agrees with NIOSH's plan to further evaluate bioassay data, et cetera. And then NIOSH gave us some additional information what they're going to do. Are we all, everybody okay on that one? Can we put that in abeyance, as well?

MEMBER MUNN: Looks good to me.

MEMBER BEACH: It's fine with me.

CHAIRMAN ZIEMER: NIOSH and SC&A, are we okay on that?

DR. NETON: Yes, okay by me.

CHAIRMAN ZIEMER: I just want to make sure we're in agreement.

DR. MAURO: Yes, this is John. I'm in agreement. I do have a question, a

suggestion. When you do the rewrite and explain that you're using the 95th percentile, which, you know, philosophically, for the reasons discussed, it's the right thing. One of the things I never really understood that, perhaps, I should have understood is: when you have a whole bunch of bioassay data and it sounds like that's for workers, are you saying, let's say you've got 100 measurements or 1,000 measurements, whatever, bioassay data taken over a certain period of time covering a large number of workers, do you pool all those numbers and just say I'm going to rank order them or put them on a log normal and say I'm picking off the upper 95th percentile concentration in becquerels per liter? That means this is your upper 95th percentile concentration of uranium you've observed in urine. Once you have that number, how do you convert that into what the annual intake is for a person?

In other words, if you say that,

okay, this is the concentration that we're going to assume this person experienced at the end of 1959 and then you ask yourself the question: what would his chronic intake be so that --

DR. NETON: Right. The chronic intake scenario. What could he have been breathing and been excreting that level of uranium in his urine on a chronic basis? Starting from first employment, obviously.

DR. MAURO: Oh, so it's like on an annual. So for any given worker, you have a particular year, you're saying we're going to assume that this is the concentration that would have been in his urine, you know, because this is a co-worker model, in effect, would have been in his urine, and then you back-calculate what would his chronic intake have been for that year to give him that at the end of 365 days?

DR. NETON: Right.

DR. MAURO: Okay, good. You know,

explaining that, because I was never quite sure how you used the 95th percentile in a co-worker model. As I said, maybe I should have been, but I wasn't sure whether you did it by pooling the data or you actually went ahead and took the real people that have real data that you can actually recreate what each, out of the large population --

DR. NETON: That would be pretty difficult.

DR. MAURO: Okay, good. Well, the approach you're using, just for the Board, is very claimant-favorable because, by doing that, you're really, you're in effect saying that everyone has urine concentration that's at the upper 95th percentile year after year after year and calculating what the chronic intake would be that would give him that urine concentration. So to assume that everyone is always at that level, or at least within the category -- I can only say you make groupings. Another group you may say is at some other

level. But, I mean, you place an upper bound on the people you believe might have gotten some exposure. In my opinion, and I think SC&A's opinion, that is a very claimant-favorable approach to doing co-worker modeling.

MR. BARTON: If I could make another comment along those lines, too. The 95th percentile is going to be used for mill workers, and also the median value is going to be used for more administrative people. And I guess my only comment on that one, I mean, certainly, it seems like a reasonable approach, but the TBD never really discusses, you know, what these administrative people, like where were they working. I mean, one would expect that, you know, that they'd be a significant distance away from the plant where they wouldn't be exposed to these types of things and, you know, they'd only have periodic exposures of short duration walking through the plant. So it's, you know,

bounding to use that value, but I would suggest, since the TBD is going to be modified, to give a specific intake model for these administrative positions, and we should probably discuss it in the TBD a little bit as to, you know, where were they actually located and, you know, they wouldn't have been in the highly contaminated areas very much at all, just to sort of flesh that out and justify that rationale.

DR. NETON: Yes, that's a good suggestion. And we're actually, internally, wrestling with documenting that in a single document now because, as you know, most often, for a co-worker model at other facilities, we use the 50th percentile for people who weren't monitored. And then we will occasionally use the 95th percentile if we believe that the person falls into the upper range of exposures. And we have documentation as to who gets that in various other places, but it's not really been consolidated in one

central location, and we're going to work towards defining that a little better.

CHAIRMAN ZIEMER: That will be helpful. Thank you. Let me ask on finding four, can we go and do in abeyance on that one? I think we agreed on there.

MEMBER MUNN: I think so.

DR. NETON: Yes, okay by NIOSH.

MR. BARTON: Yes, Dr. Ziemer. Findings two through five, as I said, are all related to that internal co-worker model. And, really, the solution is pretty much constant for all of these findings and the addition of maybe adding a little more explanation in the TBD to really justify and buttress the approach.

CHAIRMAN ZIEMER: Right. And I think finding five is in the same boat. I just want to have all of these on the record. And if there's any exception, let me know, but it looks like we have agreement on these as well. Is that correct?

MR. BARTON: Yes, I would agree with that.

CHAIRMAN ZIEMER: Everybody okay if we go in abeyance on that one?

MEMBER MUNN: Yes.

MEMBER BEACH: Yes, I'm fine.

CHAIRMAN ZIEMER: Finding six appears to be in the same boat.

MR. BARTON: No, in finding six and seven, we're talking about the residual period now. A little different.

CHAIRMAN ZIEMER: We have agreement here, though.

MR. BARTON: Well, on here, finding six was saying that the responses wrap into --

CHAIRMAN ZIEMER: Oh --

MR. BARTON: -- finding seven.

CHAIRMAN ZIEMER: Okay. Gotcha, gotcha. Okay. Let's look at that for finding seven.

MR. BARTON: So, essentially,

it's, again, the external approach to assigning external doses. We're really just looking for a little more documentation as to how much data was available to define the selected values that were used to model the external dose, just a little more discussion of what's out there and flesh out why the values that were chosen are clearly going to be claimant-favorable and bounding.

One other issue associated with this was that, in the residual period, the workday was decreased from ten hours, which was assumed during the operational period, to eight hours. And we didn't really see a rationale for that. We don't know if shifts actually did decrease to that point, and I believe part of NIOSH's response was that, well, we have this very large GSD associated with it, so that covers the fact that we're shortening the workday. But I'm not sure if the two are really related. I mean, I guess I would ask: is there a rationale for shortening

the workday other than we still were being overly claimant-favorable already, or what is DCAS' position there? Are they still on the line?

DR. MAURO: Yes, this is John. I have to say I don't like that. In other words, there are times when the large standard deviation serves us well. In a case like this, it's just too easy to just throw a big standard deviation and say, oh, that accounts for the work hour duration.

DR. NETON: I don't disagree with you, John. Tom, are you still there?

MR. TOMES: I am, yes.

DR. NETON: I'm not sure. We probably need to go back and look at this a little closer. I can't offer a -- I would leave this finding open because six is really, seven is a different beast, I think. Seven covers residual period, but it has to do with internal. Six is an external issue. I don't know that I can, I personally can't describe

what we're doing right now here to any great extent, unless Tom can add to it.

MR. TOMES: Well, I'll have to go back and look at that more.

DR. NETON: Yes. So I think, right now, we just leave finding six open, from our perspective, and we'll sharpen our thinking on this, if that's okay.

MEMBER MUNN: That seems like a smart thing to do.

MR. KATZ: Yes, this is Ted. Just to keep our nomenclature consistent, you'd call this in progress, too. The topic is engaged.

MR. TOMES: Well, finding seven was the issue of the residual internal dose, and this is where we've looked at this and indicated that we need to revise the TBD and specifically consider the 1954 data, which was not included, and look at the number we're using for the start of the intake --

DR. NETON: Right. With the

exception of TIB-70, you know, and this has been a standard practice, lacking any other information, if we have an air concentration that was taken at the end of the operational period, we will use that as a starting point for resuspension at the beginning of the residual period, recognizing that it will certainly be bounding because it would include both operations and resuspension. But, nonetheless, it will be bounding, and then we'll decrement that using standard TIB-70 depletion factor, and that's what we intend to do. We'll go back and make this consistent with how we do business at other AWEs.

MR. BARTON: Well, I had a comment here or, actually, I have a question first. When you say you're going to, did you say you're going to include the 1954 data or are you going to pool that with the current data set? Because what you're doing now is, I guess, an average of several measurements --

DR. NETON: No, the current data

set -- and, again, this was just done I think in 2006 or something -- used a bunch of operational data. And, of course, the earlier years are not necessarily representative of the resuspension that might have been occurring at the end of operations. So the best value to use is a general area air sample measurement as close to the end of the operational period as possible, and that would be used as a starting point of air concentrations in the residual period.

MR. BARTON: I noticed in the response it basically said that the 1948, which was the first year of operational data, wasn't appropriate for these later periods or wasn't representative of the type of contamination you find at the end of an operational period. Unfortunately, we don't have any measurements, you know, after 1954, so that's kind of problematic because --

DR. NETON: Maybe I misunderstood the data we have. Whatever we have at the

very end of the operational period, as close to the end of operational period as possible is the data that we would use.

MR. BARTON: Right. It's about three years away from the end of operations. But I'd also point out, from a consistency standpoint, I mean, you say that he 1948 conditions aren't representative of that later period in the operations, but, actually, your internal dose model assumes it is. Basically, what you said was, because we don't have any bioassay measurements during the later period and there's evidence that several of these industrial controls they had put into lower exposures were either removed or rendered ineffective, that we're going to assume that after 1952 the intake rates on a per-day basis are going to be the same as they were in 1948.

DR. NETON: No, that's not consistent with our current thinking. We're going to have to modify that.

MR. BARTON: Okay.

DR. NETON: Think about what we're trying to do. We're trying to reconstruct the internal dose in the residual period when there's no AEC activity at all.

MR. BARTON: Right.

MR. TOMES: This is Tom. The statement 1948 in that response concerns a question of whether the 1948 data was used in the current estimates for general area air. And SC&A read it and thought, because the 1948 reference was listed, that it was actually used. And the point was that the data was compiled together, but the 1948 data was not used. And what that data shows is that general area air in 1948 was significantly higher than it was in latter years, and that general area air from 1949 through `53 was relatively consistent. It varied somewhat but not a great deal.

CHAIRMAN ZIEMER: It looked like you used the `49 to `53 for your starting value. Is that the 94 micrograms per cubic

meter value? Does that come from the 1949 through `53 exposure studies?

MR. TOMES: Yes, that's an average of 50 results, and they were, those values were four specific areas taken out of the AEC daily weighted exposure studies that were used in worker estimates. So the numbers are not going to agree with, if you just throw all the numbers that SC&A compiled, they're not going to agree because the math was done a little bit different on those.

DR. NETON: Yes, and we're not going to use those values anyway so --

MR. BARTON: Well, I guess my point was, you know, we're saying that the conditions in 1948 are not going to be representative of the conditions at the end of the period in 1957 for the purposes of reconstructing residual doses, and then you flip a few pages back in the TBD and we're saying we're going to use the 1948 bioassay results to represent the internal exposures

from 1953 through 1957. So there's a little bit of an inconsistency there, I guess.

DR. NETON: I'm saying that we're not going to do that. I don't know how many times I have to say this, but that's not going to be used. We're going to take all that out, and we're going to say the residual period will be reconstructed based on air sample data as close to the end of the operational period as possible. We're trying to figure out how much airborne there was to inhale when there's no AEC work going on. That's all we're trying to do. And a general area air sample taken at the end of the operational period will be a bounding value for that intake value, intake estimate.

We've done this at a number of sites. This is not a new thing. It's something that didn't exist when we wrote this TBD the first time, although I do see we revised it since then. We should have incorporated that when we did the revision.

CHAIRMAN ZIEMER: So, Jim, will that be different from what we're reading here right now even?

DR. NETON: We're reading where?

CHAIRMAN ZIEMER: In the NIOSH response of 4/23.

DR. NETON: Yes, I don't have that in front of me, so I don't know what --

CHAIRMAN ZIEMER: Well, it says that you're going to use the 94 micrograms per cubic meter based on `49 through `53. I think that's --

DR. NETON: Yes. That was a description of how that number came about in response to SC&A's comments. But this says that we're going to revise it and consider the 1954 data. There's no number in there because we haven't done that yet.

MEMBER MUNN: Yes, it will undoubtedly be a small one.

CHAIRMAN ZIEMER: So this one needs to stay in progress then, I think.

DR. NETON: Yes. Oh, yes, yes. We're going to revise the entire section to --

CHAIRMAN ZIEMER: Okay.

DR. NETON: -- comport to the TIB-70 approach.

CHAIRMAN ZIEMER: Okay.

MR. BARTON: And just to kind of get it on the record, the other facet of finding seven involves a depletion factor from 1983 to the measurements that were taken in 2007, somewhere around there, and NIOSH agreed to take a look at that, so that would kind of fit under the umbrella of this finding being in progress.

MEMBER MUNN: Yes.

MR. TOMES: Just as some background, the facility was isolated in 1982 and roped off and isolated, and it's been isolated ever since. And the contamination levels were, if you look at the surveys, it was almost all fixed contamination, and there's been, it was isolated to keep people

out and there's been no work done on it. And that was our basis for assuming that as the depletion period.

MEMBER MUNN: Not much has changed.

DR. NETON: Well, the section will be revised and the language added, so in progress or in abeyance, I guess.

MEMBER MUNN: Are we filling in those actions to be taken --

CHAIRMAN ZIEMER: Put in progress. This will be in progress.

MEMBER MUNN: Yes.

CHAIRMAN ZIEMER: Okay. I think that's far as we can go on Simonds Saw today.

DR. NETON: Could I just say one thing about the Simonds Saw? Even though there's going to be wholesale changes in the internal dose models for the early years, we have to remember that this was already an SEC. And so it's my impression that not much is going to change. We're going to do a PER,

obviously, when this comes out, but it won't change much because the SEC cancers tend to be the ones that have concentrated the uranium in the first place. So I don't expect there to be very much in the way of compensation decisions. It won't be zero, but it's not going to be huge.

CHAIRMAN ZIEMER: Got you. Okay. I think we're finished for the day, except a date for the next meeting, which will be focused simply on the GSI stuff. Ted, do you have some dates that we can look at or --

MR. KATZ: Yes, let me just sort that out because I think we need to give Dave Allen, the full two weeks, even though he said he might get it done sooner, and then Bob also wanted two weeks, and I think that's fine. And then I think we need to be sure to have time to get Privacy Act material Privacy Act cleared after that, so that's really at least another week to be certain, so let me look at the calendar. So I think we're into not

sooner, I wouldn't meet sooner than the first week in June for this teleconference, just to be safe, because we do want to get the petitioners, for example, materials in advance because I know, I'm sure Dan is very tired of making his point.

CHAIRMAN ZIEMER: Yes. I won't be able to do anything the first week of June.

MR. KATZ: Okay. The second week of June, unfortunately, I have to block off. It may become open but it's not open for me right now. Now, the next week is the week of the 17th, and the 17th and 18th are going to be consumed, I'm fairly certain, but the 19th, 20th, 21st are all okay on my calendar. I don't know how those work for any of you. And this is, again, a teleconference, so we're not traveling.

CHAIRMAN ZIEMER: The 19th or 20th is fine.

MEMBER BEACH: And this is Josie. Those are both fine for me, too.

MEMBER MUNN: Yes. Likewise. Wanda.

MR. KATZ: You, too, Wanda?

MEMBER MUNN: Yes.

MR. KATZ: And, NIOSH and SC&A, that seems okay to you guys?

DR. NETON: Yes. This is Jim Neton. It works for me. I don't think Dave Allen is on the phone right now, but if it doesn't we can let you know but --

MR. ALLEN: I'm on.

DR. NETON: Oh, you're on? Okay.

MR. KATZ: So Dave Allen, is the 19th and 20th, do they seem okay to you, of June?

MR. ALLEN: Yes, they can work.

MR. KATZ: Okay. And same for John and Bob?

DR. MAURO: Bob's not on the line, but I will say yes.

MR. KATZ: Okay. Okay. So then let's just, let's write in, let's plan on, I

guess, we have some uncertainty about the 19th in terms of people traveling, so the 20th, let's just plan on the 20th.

CHAIRMAN ZIEMER: The 20th? Okay. Let's do the 20th.

MR. KATZ: Okay. And since this will be a teleconference and Wanda is out west and Josie, I think this was a good time to start? Is that right for you two, 10:30 a.m. Eastern Time?

MEMBER MUNN: Yes, that's decent.

MEMBER BEACH: That's fine.

CHAIRMAN ZIEMER: That's good. Okay.

MR. KATZ: Okay.

DR. MAURO: I'm sorry to interrupt. This is John. Were we going to have a technical conference call --

MR. KATZ: Yes, right. And, John, we can arrange that outside of this.

DR. MAURO: Okay. Very good.

MR. KATZ: But, yes, we will need

to schedule that, and we should do that, you know, in the near term so that you guys can sort out your understanding.

DR. MAURO: Yes, the sooner the better. We have some ideas, and so when you folks are ready we're ready.

MR. KATZ: Okay. So folks from DCAS, just let me know, and I'll set that up.

CHAIRMAN ZIEMER: Okay. Thank you, everybody. We're adjourned.

(Whereupon, the above-entitled matter was concluded at 4:20 p.m.)

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