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

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ADVISORY BOARD ON RADIATION AND
WORKER HEALTH

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TBD-6000 WORK GROUP

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FRIDAY
OCTOBER 11, 2013

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The Work Group met telephonically at 10:30 a.m. Eastern Daylight Time, Paul L. Ziemer, Chairman, presiding.

PRESENT:

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

ALSO PRESENT:

TED KATZ, Designated Federal Official
DAVE ALLEN, DCAS
BOB ANIGSTEIN, SC&A
BOB BARTON, SC&A
ROSE GOGLIOTTI, SC&A
JOHN MAURO, SC&A
DAN McKEEL
JIM NETON, DCAS
JOHN RAMSPOTT
MUTTY SHARFI, ORAU Team
JOHN STIVER, SC&A

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

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

10:28 a.m.

MR. KATZ: Okay. So, thank you, everybody. This is the Advisory Board on Radiation Worker Health. It's the TBD-6000 Work Group. There's an agenda for the meeting posted on the NIOSH website, subject to a little bit of amendment, but under the Board pages, under meeting pages, under today's date. And there are a variety of papers to go with the meeting also at that site. So if you want to follow along with discussion, it might be helpful to see the papers.

Okay, then. Let's do roll call. We're speaking about specific sites, so please speak to conflict of interest, too, for Agency- related personnel. And let's begin with Board Members, with the Chair.

(Roll call.)

MR. KATZ: All right, then. So let's just all mute our phones except when we're speaking: *6 to mute your phone; *6 to take your

phone off of mute.

And, Paul, it's your agenda.

CHAIRMAN ZIEMER: Okay. Thank you very much. Good morning, everybody. I'll officially call the meeting to order. I want to remind you that the focus of the meeting today is on General Steel Industries. The meeting was original scheduled to have two items at our June 20th Work Group meeting. We had a task by NIOSH to summarize in a White Paper their approach to settling velocity. They had explained it there verbally. SC&A expressed a desire to see it in writing so that they could better respond to it.

So we have that document from NIOSH on settling velocity, or determination of settling time and we have a response from SC&A on that document.

The second document was that NIOSH agreed to summarize in a White Paper the various portions of the dose estimates for GSI. They issued a White Paper on August 21st. SC&A reviewed that and we got the comments. I think

the date of the comments was October 6th. The co-petitioner also had comments on that on August 30th and some follow-ups after that.

So we have those documents to focus on today. And just for scheduling purposes, my plan is -- I have to adjourn us at 2 o'clock. That's my own schedule and I can't go beyond 2:00. So I'm going to try to make sure we get through all of this today. And if possible we will be able to take a look at some of the open items on the matrices, on the issues matrices. But the initial focus here is on these two main items.

Also I'm not planning -- we really got started a half hour later than we'd planned. The meeting was originally planned for 10:00, but somehow got posted on the Web site as being at 10:30. So my plan is to go straight through to 2 o'clock. Individually, you can just take breaks as you need them. You're sort of on your own. If you need to get a little food in you, just grab a snack while the meeting goes on. But the

plan is just to proceed on through.

So we're going to begin with the item on the agenda that's entitled "TBD-6000 Rev 1." We have the presentation by DCAS and Dave Allen.

And I don't know, Dave, that we need to actually go through that. We all have had copies of that for over a month and I think the way for us to proceed here would be to go ahead and have SC&A present their comments and then we'll see where we are on this, if that's agreeable. Unless, Dave Allen, if you had any specific comments you wanted to make first.

MR. ALLEN: Yes, that's agreeable to me.

CHAIRMAN ZIEMER: Okay. Then I think we'll proceed. And I think Bill Thurber had the lead on this part of it for SC&A.

Is that correct, Bill?

DR. MAURO; Yeah, Paul, this is John Mauro. Bill was not able to join us. However, I did work closely with Bill on this work product and I'll be able to summarize it and hopefully

answer any questions.

CHAIRMAN ZIEMER: Okay.

DR. MAURO: So I'm ready to go, if you guys are ready to go.

CHAIRMAN ZIEMER: Yes, proceed, John.

DR. MAURO: Okay. In fact, I can set the stage real quickly. What we're dealing with is an aspect of TBD-6000 dealing with settling time. A good way to think about it is you got a big area where people are doing metal working. They're grinding stuff and they're rolling stuff and airborne dust is out there and people working there are exposed to the inhalation from airborne uranium, externally exposed from large pieces of uranium rods, et cetera. And also from any uranium that deposits on the ground. And that's the issue here, the uranium that deposits on the surfaces.

And that's important from two perspectives. It represents a source of external exposure and resuspension as a source

of internal exposure. And it's especially relevant after the operations stop, because then that source really becomes the only source of exposure in what you would call maybe the post-operational time periods.

So, now, when you're dealing with that source, the stuff that's on the ground, first and foremost you've got to estimate how much is there? How many becquerels per square meter of uranium residue or dust oxide is on the ground? And we've had a number of exchanges on that model.

I think we agree with just about everything except one issue. And one issue is the accumulation time. You could visualize. You've got this dust in the air. It's settling at some velocity and it just keeps settling and it accumulates. Well, in theory, you know, if it goes on for 10 years, you're going to have 10 years' worth of accumulation and build up an awful lot. But that doesn't happen.

And we actually have a lot of good

data to help us understand how long does it take before the stuff that falls settles and achieves some level of equilibrium? Because, as you can imagine, as it's accumulating it's also leaving because it's re-suspending and it's being moved around and it's being exhausted from the facility. And NIOSH's position, based on a review of various data sources -- Adley, Simonds Saw -- is that a good accumulation time before equilibrium is reached is about 30 days. And we looked at that.

And we have a paper, and I'm going to go through it very quickly, that came out on October 13th. And for those of you who have it handy on their machine in front of you, we could very quickly get to the bottom line. If you would go to table 3 on page 7 -- it's only an eight-page report -- if you can go to that table, that's what we're going to talk about for a few minutes here.

We went in and basically did a very similar calculation that was done by NIOSH,

except we did something a little different. NIOSH used a data set that came from this large complex -- I think it's called the Hanford Metal Melt facility -- where they had lots of data on airborne levels, on deposited levels. We have good information on deposition velocities. And they calculated from the data, well, how long did it take before the material on the surfaces reached equilibrium? And they collected all the data.

But it's a big area. There are some rooms that are large; some rooms that are small. And collected all the data and got rid of some data that was really not appropriate, but in the end came up with their data set of what's in the air and what's on the surface. And calculated, well, how long did it take for it to -- and collecting all the data. And they came up with 30 days before equilibrium is reached.

We did the same thing, except, to keep it simple, we said, well, you know, let's break the building up, because the building

isn't one building. It is really a large area called, I think, the bay area. And then there are a number of smaller rooms where they did the same thing. We have airborne levels and we have deposit levels.

And so what we really have is a richer data set. That way you could say, well, let's look at the large bay area, which is the size of a half of a football field, and then let's look at the smaller rooms where other things were going on, where we have data, air and deposited, and look at them separately and to try to get a distribution of what the duration time for settling is. Because I think we could squeeze more information out of the data sets by doing it that way.

And that's what this table shows, table 3. We do have some differences between our approach and NIOSH's approach in some of the assumptions. And we could get into that if you'd like, but I think the important point and the bottom line is that when we did the analysis,

we broke it up into different segments.

As you can see in table 3, we've got it broken out into different time periods and different rooms. And you will notice that our results effectively come in about the same, if you aggregate everything, to where NIOSH came in. And notice that, if you look into that table 3, on the last two entries, Main Bay Winter, Main Bay Spring, we come up with time to achieve equilibrium. It's the far right- hand column that says days to equilibrium. We come up with 15.7 days and 17.4. So, for the main bay area, our finding is that it reaches equilibrium even more quickly than the numbers that NIOSH reported at 30.

But then we also looked at a number of the other smaller rooms which are on the order of maybe 20-by-20, 30-by-30 feet. And you could see that some of the smaller rooms where we have data, we come up with some numbers that are higher, in some cases substantially higher, than the 30 number. Which all rings true. Because,

remember, NIOSH aggregated all the data, so it sort of like averaged out everything. So coming in at 30 makes sense, more or less, you know, given some small differences in assumptions that we used and they used.

But we'd also like to point out, though, that there are some rooms where clearly the characteristics of the air turnover and the removal rates are obviously different, and as a result equilibrium is achieved in quite a bit longer than 30 days. And that's our finding.

And, you know, we have other assumptions that we could get into that I consider to be the fine structure of the analysis. But I think the real important point here is that perhaps the best -- I guess the story at the end is it looks like a 30-day number is a really good number, especially if you're dealing with relatively large areas, like the bay area, which I think was something like 1,200 square feet. I think that was the number.

But for relatively smaller rooms, on

the order of let's say 500 square feet, we have the numbers here in the report, perhaps that accumulation time is not as claimant-favorable as it should, the 30-day, one-size-fits-all.

And that could be important. Most of the time this is not important because we're dealing with doses that are coming from this residual level. But if the only exposures you're interested in -- if you're doing a dose reconstruction, and let's say at a site, and the exposure to the person you're interested in is for the residual period. You know, you're not really concerned about operations. Let's say they granted an SEC for the operations period; often that's the case. But there's no SEC for the residual period at an AWE facility. Then this does become important in performing a dose reconstruction.

And our recommendation is: take that into consideration. That is, when you're doing the residual period and you're estimating what has accumulated on the surface, for the purpose

of doing external and internal exposure, just automatically applying the 30-day deposition accumulation time may not always be claimant- favorable. And really that's the bottom line of our story.

CHAIRMAN ZIEMER: Okay, John, let me ask you a question, then. It isn't clear to me precisely what you're recommending, whether you're recommending that the upper value be used for everything, or that there be a gradation, that if the size is known, that you take that into consideration. Or if it's unknown, you would assume the higher level. Is that the recommendation?

DR. MAURO: I don't think there's any words here to that effect, but I think that's a good take-away. That seems to be the reasonable thing to do.

You know, if you know the size of the area, yes, the 30 certainly -- and it's big -- and it often is big -- 30 certainly is a good number. But if you don't, or if you know it's a small

area, relatively, you know, maybe you want to go -- and they'll have about a factor of two or three effect. So, yeah, I think the latter statement that you made I would agree with completely.

CHAIRMAN ZIEMER: Let me ask NIOSH to give their response to this.

MR. ALLEN: Yes, this is Dave Allen. One point I tried to make in the White Paper that we wrote was the number of days versus other parameters is -- what the individual parameters is not so important as what the purpose of these numbers and the final result is. And the whole purpose of these numbers is to come up with a surface contamination value.

In this recent document from SC&A, they adjusted the air sample values. They're still using the settling rate lower than what we're using, and they're coming up with times associated with these different parameters.

DR. MAURO: Yes.

MR. ALLEN: What I did was looked at

their unadjusted airborne concentrations and our assumptions, which is the settling rate of 0.00075 meters per second and a 30-day, 24- hour-a-day settling time. And it ends up giving you a higher surface contamination for six out of the seven values, or lines, that they have in table 3.

The one exception is the furnace room in the spring where the SC&A come up with a 166-day settling time. That would produce, I guess, a higher concentration. Actually it wouldn't because of the airborne. It gets confusing.

DR. MAURO: You know, David, I agree. I think it's good that we're going here, because you're right, we used that 0.00052 settling velocity based on the slip.

I have to say that, in retrospect, you know, we try to -- because we discussed this in the past, the settling velocity. And I think our analysis, you know, is I think the 0.00052 is probably a good number because it tries to

bring a little bit more sophistication to the way in which these particles actually settle. You saw our report. But at the same time, I would hate to sort of pick away at something like that.

I like the 0.00075 number and I don't think we should go down the road -- and so I'm agreeing with you -- of trying to get to a level of resolution at a site where we really can't. I mean, you know, the slip velocity depends on the size of the particle, its shape and issues that aren't always easy to address. So I would have to agree with what you just said. That is, let's stay with the 0.00075.

But where I would tend to disagree with you is that the other part of the calculation has to be how long is the activity airborne? In other words, one of the assumptions I believe you made is that the airborne levels that were at the Adley data, for example, were at that airborne concentration, the measured values in those different rooms, 24 hours a day.

But we believe that, well, that may be a 30- percent overestimate of what the average airborne concentration was. Because they were not working on, you know, triple shifts. I think they only had one shift or so, or something like that, per day. So the time period when you had the dust loadings that were measured was probably only during the operations. So I think that that is an adjustment that I would say we need to make.

So I agree with you, don't let's gild the lily with regard to the 0.00075. But at the same time, I do think that we do have to factor in, when you do your calculation, coming up with what we believe the average, 24-hour average, dust loading is in the room, take into consideration when there is -- you know, when there's only, let's say, one shift eight hours a day is when you have that dust loading and the rest of the time you don't. So there's where I think we still need to talk a little bit.

MR. ALLEN: Well, I don't disagree

with you on that one, John, but the point here is that when you adjust -- you adjusted the airborne levels, which is one way to do it. It's mathematically identical to adjusting the time that it's settling per --

DR. MAURO: Yeah, exactly.

MR. ALLEN: And with the weekends considered in there, et cetera, the 0.32 factor you came up with is similar to settling for 7.68 hours per day instead of 24.

DR. MAURO: Yes, yes.

MR. ALLEN: But right there is a factor of three difference if our default is to use 24 hours per day.

DR. MAURO: Yes, but we also acknowledge that there is a build-up and a decline period. I agree. So it gets a little complicated.

MR. ALLEN: Right, it does. But I'm just saying, for your values to work out, you have to essentially assume the airborne value for a little less than eight hours per day times

the number of days you have in the table, whereas we would assume 24 hour a day times 30 days.

DR. MAURO: Well, you're losing me. Why would 24 hours a day work, I mean, if you don't have that dust loading 24 hours a day? I mean, the actual empirical data that shows you the accumulation on the plates, in Adley, you know, they were sitting there for 24 hours a day, but the airborne dust loading was not at that level that you used for 24 hours a day. So you have to take that into consideration, and I don't think you did.

MR. ALLEN: I understand that, but you guys did and in the end the surface concentration you would calculate out is smaller than what we would have calculated out.

DR. MAURO: Is that right? Okay. I'm not going to dispute that. So I don't think we have any disagreement here. What I'm saying is that we looked at this the way we looked at it. All our assumptions are there. And you're pointing a couple things out that I'm not

disagreeing with.

You know, we certainly could go back and see what would happen if we left the 0.00075. And do you agree that the concept that you have to take into consideration, the fact that you only have a dust loading for part of the time; in other words, as opposed to assume the dust loading is there for 24 hours a day?

MR. ALLEN: Well, I think your analysis showed that the values we've chosen for this even account for that.

DR. MAURO: I know I was looking at your numbers and I checked them, you know, in getting ready for the meeting. Unfortunately, Bill can't be here to carry this one at a higher level of detail than I can. But I did your check your numbers and they're actually in table 1 right in the beginning of our report, if you go up to the beginning o page 5. And I think built into those numbers is 24 hour a day.

So, in other words, the air dust loading that we have in the table for the

geometric mean -- if you have it in front of you, for 534 and the derivation of the settling days, et cetera, all that information -- I believe that reflects the assumption that the air concentration of micrograms per cubic meter is present for 24 hours day. Am I right? I mean, did I get that wrong?

MR. ALLEN: I believe you're right, but I'd have to review these numbers, honestly.

DR. MAURO: Yeah. Well, Paul, what I think we have here -- I mean, I know it's important that we get to GSI and you want to -- I don't think we have anything -- I think that we fundamentally agree that there is a strategy that should be used here.

We carried it a level of granularity that was a little higher, a little finer in order to explore the value and the merits of the 30. And our take-away is that 30 is a good number for large areas, like areas the size of half a football field. But when you get to smaller rooms, there might be a problem.

And now these other matters that we're talking about, such as what is the air dust loading you should use, I think we're in agreement that when you're calculating what's accumulating on a surface the right thing to do is what is the -- for, you know, the accumulation you would use the concentration that represents the 24-hour average, because that's what accumulates. Accumulation is going on all the time, 24 hours a day. But the airborne dust loading is not always at the high level that you might measure during operation. It might be high during operation. And then if they are not working 24 hours a day, it's lower. So we're in agreement. And where the numbers come out, they come out.

We also agree, SC&A, that I don't think we should gild the lily on the 0.00075. We did do that here. It carried into our analysis. We used 0.00053 because it had the slip factor. But I would also agree that maybe that's taking it a little bit too far in terms of trying to,

you know, sharpen the point to a point that we can't go.

So, I mean, I think, in principle, the only thing that we're arguing here is that take into consideration that maybe the smaller rooms behave different than these large open areas. And also take into consideration the daily average concentration of the airborne dust loading. And that's really what we're saying.**

Now, the numbers we have, that we've calculated, you know, the assumptions we use are what they are. And I would say that, you know, if, David, you feel that some of those numbers need to be adjusted because maybe we didn't do it the way you felt it should be done, I'm not going to disagree with that. I mean, I can't say you're right or you're wrong, but, you know, if there is some aspect to how we derived these numbers, some assumptions we've made that you feel need to be corrected, I'm fine with that. It's really the concept that I'm interested in.

So, Paul, you know, I think it would be unfair to automatically conclude that, yes, you know, SC&A's numbers here are what should be adopted. I would say, no. You know, maybe a little more polishing of the apple is needed in what should be the numbers that represent large rooms versus smaller rooms. And where we come out on that, you know, may be a little different than our table after both NIOSH and SC&A maybe have a chance to look at some of the points that David is making.

David, are we in agreement fundamentally that that approach that I just described is how we should go?

MR. ALLEN: Well, I'm not positive I followed the whole approach you described.

DR. MAURO: Well, all it means is that, when you look at the Adley data, don't just aggregate all the numbers from all the rooms. Let's parse it out where we can and see if the settling times are substantively different for different rooms.

DR. NETON: Hey, John, this is Jim. I'd just like to interject something here. I think what we really need to look at is the ultimate goal here, which is to estimate the surface concentration as a result of airborne activity. What does it accumulate to? What's the value? And what Dave has said, and it's true, we use values that are more conservative than what you've used in your calculation. We use this 0.00075. We've assumed that it settled over 24 hours. But in using those conservative assumptions we end up with a higher surface contamination than you have generated or predicted using your more realistic assumptions.

DR. MAURO: Is that right? I mean, I can't --

DR. NETON: That's the bottom line here. So the reality is what you've done with your more sophisticated model is to demonstrate that our conservative model appears to be exactly that: it's conservative.

DR. MAURO: All right. Let me think about that for a second. Jim, but if you have a higher -- you went with a higher -- I mean, let's just talk about the settling velocity.

DR. NETON: Settles faster.

DR. MAURO: If it's settling faster --

DR. NETON: Right.

DR. MAURO: So then if you're settling faster, the time it takes --

DR. NETON: And 24/7.

DR. MAURO: It's going to accumulate. It's going to reach equilibrium sooner.

DR. NETON: Yes.

DR. MAURO: Granted. And then the average concentration, if it's lower -- you know, in other words, if we go with the average for the 24 hours as opposed to the eight-hour. Now, if the concentration is lower, though, that's going to make it longer. So I don't think both assumptions --

DR. NETON: No. No, if you're settling a high concentration for 24 hours --

DR. MAURO: In other words, you want it to --

DR. NETON: All I can tell you is if you do the calculation your way using your -- you know, the surface concentration is a product of the settling rate, the air concentration and the time that it settles. Right?

DR. MAURO: Right, but there's also the average concentration in the air. So, I mean, you know what it is, we got three parameters: deposition velocity; the average concentration in the air, which I think, you know, you folks used what I consider to be a higher concentration.

DR. NETON: Exactly. So we're settling a high concentration over with a higher settling velocity which maximizes the surface, the contamination on the surface.

DR. MAURO: And then the outcome, you're saying, ends up with activity on the

surface that's higher than ours.

DR. NETON: That's what we've been saying all along.

DR. MAURO: My goodness.

DR. NETON: Except for this one case of the furnace room in the spring.

DR. MAURO: Yes. Well, listen, Jim, very good. I mean, I'm not going to dispute you. I'd sure like to --

DR. NETON: But you need a chance to run the calculations. But that's what David's been saying since the last meeting.

DR. MAURO: Oh, okay. Listen, I accept that, Jim. I'd like to pass this on to Bill. I said, I'm filling in for him. But what you're saying makes sense to me.

Paul, I wish I could be conclusionary here at this time, but, you know, I would like to have a chance just to talk about this question of -- and you're right, ultimately the issue is the build-up -- not the settling duration, but are we sure we're being

claimant-favorable in the amount, becquerels per square meter on surfaces?

And what I'm hearing is arguments that that's really the point. And I agree with that. But I would like to give Bill a chance to look at this before, you know, we close the door. I'm sorry I can't answer the question definitively and agree right now, but what you're saying certainly seems reasonable.

CHAIRMAN ZIEMER: This is Ziemer. Yes, we can certainly do that. We would want that to occur fairly soon so that we can close this out. I think this is the only remaining open issue on the matrix for TBD-6000.

DR. MAURO: I think you're right. A real quick aside: I have a 2010 matrix for TBD- 6000. Is that the latest matrix?

CHAIRMAN ZIEMER: Yeah, October 12th, 2010. And then we have this follow-up on Revision 1 that Bill Thurber worked on, and that's where this question has arisen.

DR. MAURO: Okay.

CHAIRMAN ZIEMER: So it came up in that context. Actually, most of the items were in abeyance or resolved on the original one. But, in the meantime, this revision came out and we had that sort of open item.

DR. MAURO: Okay.

CHAIRMAN ZIEMER: So we do need to get this closed so that we can take formal action on it. But I think what you are suggesting -- and then we'll get some comment from others here on this -- was for SC&A to double-check the calculations that give the surface contamination level, right?

DR. MAURO: Right. Exactly. Exactly. And that won't take long. Bill will be available Monday and I'll sit down with him. We'll talk it over. I mean, I understand exactly what Jim and David are saying. I'll just talk to Bill about it. He and I will put our heads together, you know, with John Stiver, and we'll get something out quickly.

I know next week we'll be in Denver,

but perhaps Bill could put together something that week while we're in Denver. In fact, we could even, perhaps, you know, let you know so that you have it for the Denver meeting. And it would be nice to be able to say, yes, we've resolved this.

I mean, I have to say, my instincts tell me that Jim is right and David is right in terms of the build-up, but I don't want to do that until I give Bill a chance to --

CHAIRMAN ZIEMER: No, understood.

DR. MAURO: Yes.

CHAIRMAN ZIEMER: Understood. Let me ask if other Work Group Members have questions or comments on either the paper or for Dave or for John.

MEMBER MUNN: Well, this is Wanda. I had several questions coming into this, but I think that what I've heard in the discussion here has cleared up most of them. As a matter of fact, I think it's probably cleared up all of them. I had some questions about

concentrations and the amount of work hours that were being assumed and that sort of thing, but it sounds to me as though that's pretty well been covered.

One of the requests that I have with respect to where we're going with the overall TBD-6000 issues is that it would be very helpful for me, I don't know if others would like to have an updated copy of the matrix or not, but it would be very helpful for me if I could receive an updated version of the matrix so that I had a better feel of exactly where we were.

CHAIRMAN ZIEMER: Yes, I think we can certainly ask SC&A to distribute that. I believe the updated version of the original matrix is dated October 7th, 2010, but this material grew out of supplementary comments on Rev 1 that were prepared by Bill Thurber, and that's dated May 13th of this year, 2013.

MEMBER MUNN: Yes, we have that in our files for reference.

CHAIRMAN ZIEMER: And if you look at

that one, there's a number of sort of bottom line bullet points in that. Of those bullet points, there was agreement on everything except this one issue where we asked for NIOSH's response, which is what generated the White Paper. And there was one other thing, one other bullet point where there were some actual errors in the tables of section 7 of TBD-6000 which NIOSH already agreed to correct.

So those were the two open items. But if we can bring this to conclusion fairly rapidly, we could close out the TBD-6000 issues so that they're all in place.

MEMBER MUNN: It would be helpful.

DR. MAURO: Yeah, Paul, I think that I'm optimistic that we could do that within a matter of a couple days. That is, we'll get on it on -- well, Monday's a holiday. Well, anyway, we'll get it on this week. And it's not going to take very long with the help of Rose and Bill to put all this to bed, get a final updated matrix that reflects the May 13th material, and

of course today and the other White Paper material that is here. And also, as I said, my instincts tell me that we probably have just resolved this matter, but we'll give Bill a chance. And that will all be reflected in the matrix.

If for any reason our take-away later is that, no, we're not really in agreement here, I will certainly immediately let you know. But, as I said, it sounds like we are.

MEMBER MUNN: That would be very helpful, John, and much appreciated. Thank you.

DR. MAURO: Sure.

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

MEMBER POSTON: I don't have any questions. I agree with what Wanda said.

MEMBER BEACH: And this is Josie. I agree, also, with the discussion. I just have a quick question. John, what you're going to discuss with Bill, is that going to bring in the

30-day settling time versus the --

DR. MAURO: Yeah, it really means the settling time is really not relevant.

MEMBER BEACH: Okay.

DR. MAURO: What's relevant is -- and I agree with this -- is what's the concentration in becquerels per square meter that you're going to assume is the starting point for your residual period? And is it claimant- favorable? And, you know, what we're hearing from Jim and David is that, when you go to first principles, that their levels that they calculate are higher than what we would calculate. Now, intuitively, I mean, if that's true, it's true. But we would like to check that.

MEMBER BEACH: Okay. So I just wasn't clear on that.

DR. MAURO: Yeah, and that would be great if it turns out it comes out with a higher value. Now, I don't know why then we would have different settling times. That seems to be

non-conservative, but I could see how those things could play themselves out. And the most important thing is what is the build-up on the surface that's going to be used? And that doesn't take long. And I'm sure that Bill and Rose would be able to take care of it, you know, next week while we're doing our thing in Denver and to get this finished up for everyone's consideration sometime next week.

CHAIRMAN ZIEMER: Okay. Thank you very much. Any other comments?

(No response.)

CHAIRMAN ZIEMER: So we'll look forward to hearing from you guys fairly soon. We can talk at the meeting next week about the potential -- I'm hopeful. I'm not optimistic that we're going to be able to get far into the matrix today of Appendix BB, but I would like us to be able to move into that fairly rapidly as well. So we may be able to schedule a meeting in the fairly near future to address both of these, what really will end up being the matrices

issues. So make sure that we're in a position to address open items that may not have been closed or dealt with fully.

Okay. Let's move on specifically now to GSI.

MEMBER MUNN: Paul, I have one question before we go.

CHAIRMAN ZIEMER: Oh, yes.

MEMBER MUNN: Are we going to be using our Live Meeting capability with visuals at all?

CHAIRMAN ZIEMER: I don't know the answer to that. Do we need anything --

MEMBER MUNN: I just was thinking I'd get off that screen if we're not going to have material.

CHAIRMAN ZIEMER: Live Meeting is available ad the Work Group Members. I don't know if SC&A has something they wanted to present.

DR. ANIGSTEIN: This is Bob Anigstein. I have a briefing that I was going

to present.

MEMBER MUNN: Oh, good.

DR. ANIGSTEIN: And Ted and Zaida both have sent me invitations. I don't know about everyone else.

MEMBER MUNN: That's fine. Thanks much.

CHAIRMAN ZIEMER: Well, I'm looking here and I think the Work Group Members are on there. I assume that what you are going to present is just a summary of what's in your document itself?

DR. ANIGSTEIN: Well, it's slightly expanded because of Dave Allen's latest communication. But you are correct, it's not new material.

CHAIRMAN ZIEMER: Well, I want to make sure that whatever you present here we will be able to make it available fairly quickly. I know it has to be reviewed through -- fairly quickly to petitioners and members of the public so that there's not a big time delay before they

get to see the --

DR. ANIGSTEIN: Sure. I will send it to Ted as soon as we're finished.

CHAIRMAN ZIEMER: Okay.

DR. McKEEL: Chairman Ziemer, this is Dan McKeel. May I make a comment, please?

CHAIRMAN ZIEMER: Sure.

DR. McKEEL: I requested that if anyone -- Dave Allen or Bob Anigstein, SC&A -- were going to make a presentation at this meeting that I be sent a copy. That must have been at least a month ago. It may have been two months ago. So, you know, this is very, very disturbing. It happens over and over. You all know that the petitioners need this information. There's no reason that couldn't have been sent to me ahead of time. So that's my comment.

CHAIRMAN ZIEMER: Yes, thank you, Dan. I don't think this has been made available to any of us in advance. I assume it probably got prepared last night or something.

DR. ANIGSTEIN: You're right.

CHAIRMAN ZIEMER: But, in any event, it does have to go through that process, so we'll try to get it available as soon as we can. You will hear verbally what is being presented. But I know you want the written material as well, so we'll do our best to get that out to you.

So we have the White Paper that Dave Allen prepared in August. And then we have the response from SC&A, which was dated October 6th. And then shortly after that on -- the date that I show here is October 10th, we got some feedback from Dave Allen reacting to the SC&A comments. So we have all of that. And then we also -- I assume everybody has had a chance to see Dr. McKeel's comments as well, and we'll give him an opportunity to comment as well.

And I think, Dan, on our agenda where it says -- you're showing as item D, if you're agreed, I'll move you up so that you're -- we're not going to dispose of Appendix BB issues until you have a chance to comment on the documents

here that are before us. So I'll move you up to comment right after the SC&A review here.

DR. McKEEL: Thank you.

CHAIRMAN ZIEMER: So I think again we've had the chance to see the original paper by Dave.

Dave, unless you have comments on it, we'll save your responses until after SC&A. Do you have any comments, general comments to kick that off, or shall we right into the SC&A review?

MR. ALLEN: No, that would be fine.

CHAIRMAN ZIEMER: Okay. So, Bob?

DR. ANIGSTEIN: Okay. I didn't expect to be on immediately. One second. Let me get into the Live Meeting.

DR. MAURO: I'm sorry to interrupt, but this is John. I went to Live Meeting, it came in on my email, and I'm looking at something, and there's a blue screen that says nothing is currently shared. Is everyone else looking at the same thing?

DR. ANIGSTEIN: Yes, nothing is shared.

DR. MAURO: Oh, okay. I just --

DR. ANIGSTEIN: It'll change in the next 60 seconds.

DR. MAURO: Ah, that's why I asked. Okay. I'm where I should be. Thank you.

CHAIRMAN ZIEMER: So we'll stand by for a minute while that material is pulled up.

DR. MAURO: Okay.

MEMBER MUNN: I'm beginning to regret having asked the question.

DR. ANIGSTEIN: Oh, dear. Let's see. Can anyone help me with this? Because I tried this out yesterday, We seem to have a problem, and I did find a place where it says "share." And now I don't see a screen which allows me to share. Ted, can you -- oh, Dave Allen is currently sharing, but --

MR. ALLEN: Sorry, Bob, that was me. I was trying to figure it out myself.

CHAIRMAN ZIEMER: Well, okay.

Click on the thing called "content" at the far upper left.

DR. ANIGSTEIN: Upper left I just see attendees, voice and video meeting.

CHAIRMAN ZIEMER: Further left. Before "attendees" there's another thing called "content."

DR. ANIGSTEIN: Nope, not on my screen.

CHAIRMAN ZIEMER: Really? You see something called "attendees?"

DR. ANIGSTEIN: Yes. I see "attendees" and I see my name.

CHAIRMAN ZIEMER: "Attendees" on mine is the second box from the left.

DR. ANIGSTEIN: No.

CHAIRMAN ZIEMER: The first item is called "content." And then if you click on that --

DR. ANIGSTEIN: No, no, I don't have anything to the left of "attendees."

MEMBER MUNN: That's interesting,

because mine shows you as desktop under "content."

DR. ANIGSTEIN: My desktop has Paul Ziemer has started sharing.

CHAIRMAN ZIEMER: I clicked on your name, which --

DR. ANIGSTEIN: Link to Live Meeting.

MR. KATZ: Paul?

DR. ANIGSTEIN: Yes.

MR. KATZ: Paul, let's try something. Paul or Dave, why don't you forward your link to Bob?

CHAIRMAN ZIEMER: How do I do that?

MR. KATZ: You just copy and paste your calendar invite into an email and send it to Bob. Or send it to me and I can send it to Bob if you don't --

DR. ANIGSTEIN: Excuse me. Send it to my regular, my [identifying information redacted].

MR. KATZ: Yes, so if you don't have that, I can send it to him. You can send your link to me and I'll send it to him.

DR. ANIGSTEIN: The funny thing is I tested it last night and it worked.

MR. KATZ: Oh, then, yeah, I don't understand. Well, which way did you go in this morning? Did you go in through my forward or through your original thing that you used yesterday?

DR. ANIGSTEIN: Yeah, I went into your forward.

MR. KATZ: Okay. So, drop it. Drop your Live Meeting and go in from the invite you had before that you tested last night.

DR. ANIGSTEIN: Okay. All right. Just a second.

MR. KATZ: Yes. Yeah, go ahead and go back in that way. Then, Paul, you don't need to do anything because he has it.

CHAIRMAN ZIEMER: Okay.

MR. KATZ: That will work.

DR. ANIGSTEIN: Okay.

MR. KATZ: Okay. So go back to what you used last night, that link.

DR. ANIGSTEIN: I will. Give me a second. Give me a second.

MR. KATZ: Yes. No, I know. I'm just saying that should work.

DR. ANIGSTEIN: I hear you. Just give me a second.

MR. KATZ: Because probably your problem is that I think Zaida doesn't have me as a presenter, and that's probably why you're showing a different screen. And I forwarded you my link.

DR. ANIGSTEIN: I see invitation to Live Meeting. Okay. Join the meeting. Continue. Content. Yes, it is different.

MR. KATZ: Yeah, good. Thank you.

DR. ANIGSTEIN: Share desktop. Okay. Nothing is currently shared. Okay. Does anyone see it?

MR. KATZ: Yeah, that works, Bob.

MEMBER MUNN: Yes, we see what's going on on your desktop.

DR. ANIGSTEIN: Okay. Very good. All right. So, sorry for the delay and confusion.

MEMBER MUNN: No problem.

DR. ANIGSTEIN: Okay. I'll try to go through this quickly. One second. Sorry.

CHAIRMAN ZIEMER: Bob, let me interrupt you a minute. On our screens your slides are very large, at least on mine.

MR. KATZ: That's true for everybody, I think.

DR. ANIGSTEIN: They're too large?

MR. KATZ: Yes.

DR. ANIGSTEIN: Oh, I don't know. I'm using full screen.

CHAIRMAN ZIEMER: Well, they're more than full screen.

DR. ANIGSTEIN: Okay.

CHAIRMAN ZIEMER: They're about double screen size.

DR. ANIGSTEIN: Ah, wait a second. Right. If you give me a little bit --

MR. KATZ: Right, just go to 75 percent or --

DR. ANIGSTEIN: Is this better?

CHAIRMAN ZIEMER: Make them smaller. You're at about 100 percent. Make them about 75 and see what that does.

DR. ANIGSTEIN: Is this too large still?

MEMBER MUNN: Yes, it is.

DR. ANIGSTEIN: Okay. Our screens are different. How is this?

CHAIRMAN ZIEMER: Much better. Still a little large, but --

DR. ANIGSTEIN: All right. I'll make it 50 percent. Okay?

CHAIRMAN ZIEMER: That works.

DR. ANIGSTEIN: Very good. Okay. I guess I'm used to doing this for the meeting, you know, when we really are a live meeting with a projector.

MEMBER MUNN: Yes.

DR. ANIGSTEIN: Okay. So just for purposes of reference, the time periods we're talking about, October 1st through the newly revised start of operations. And the main source of exposure, the two sources of exposure were the two radium sources and what was then the 24 MeV betatron. Then on May 21st, GSI acquired the cobalt-60 sources and they presumed to have stopped using the radium because they were under orders from State of Illinois to do so.

Somewhere late 1963 -- I just arbitrarily said the October 1st, because it's not likely to have been any earlier than that -- the new betatron went into operation. And the main difference between the new betatron and the old betatron is that the new betatron building was physically connected to the production buildings. So it was right off the No. 10 building.

And so there was a potential for people working -- workers in the, I think they

were called cleaning and finishing buildings, to be exposed to the new betatron depending on the certain exposure circumstances. Where it was not possible with the old betatron. So there is a new change there.

And it happens to be the year after the radium sources went out of use. And, consequently, since NIOSH for convenience tends to work with calendar years -- so '62 would still be the radium era, and '63, I propose, should be -- and we have proposed in the past -- let's call it the new betatron era. And then June 30th, '66, is the last purchase order, so it was the end of the operation period, beginning of the residual period.

Okay. The bounding sources during these periods -- so the radium -- actually I just covered it. Radium would be the bounding source during the radium era, which was essentially the first 10 years of operation. Then at all times you have some potential for stray radiation from the betatron, but particularly during the

betatron era, because earlier any doses from that operation would be overshadowed by the doses from the radium. And at all times you would have delayed radiation from activated metals. So it could be either steel or uranium.

And then the third source of radiation would be the exposure to the skin to beta radiation, which would be either from handling even the natural uranium before it is irradiated, and much more so with the irradiated uranium, which has photo-activated uranium isotopes, and the activated steel.

The bounding scenarios. There are areas of agreement. I'm not even mentioning the administrative personnel where NIOSH has proposed an exposure scenario, and SC&A is in agreement with it.

During the radium era, the Work Group at the meeting on February 21st agreed, it was mutually agreed that we would have a triangular distribution. The lower end would be a calculation, I believe, that NIOSH had made

of 6.279 rem. I think it's a bit precise, but anyway. The mode would be 9.69 rem based on an SC&A calculation. And then the upper would be the limit, the AEC exposure limit, because in this AEC application -- in this application of AEC, GSI stated that, "when we were operating with the radium," even though they weren't controlled by AEC, "we always abided by the then-applicable AEC limits."

So it seems that they were aware that the AEC limits changed over time. And NIOSH has the change over from 15 rem to 12 rem in 1957 because that was the publication date of an NBC Handbook. However, that was not adopted by AEC. There were just, I guess, bureaucratic or administrative delays. And through the end of 1960, there was a 10 CFR 20, which I'm sure most of us are familiar with. And earlier than that they were following, I believe, NBS Handbook 44, both of which in effect allowed doses up to 15 rem a year. It would be on a weekly basis, a monthly basis, but it translated to a possible

dose of 15 rem a year.

There was a Federal Register notice in late 1960 saying that, effective January 1st, 1961, the new 10 CFR 20 dose limits went into effect, and those permitted 3 rem a quarter. And depending on the prior exposure history of the worker it could be as much as 12 rem a year.

So SC&A's position is that we're in agreement with the numbers, but the changeover should be January 1961, not 1958.

And also, NIOSH had it with the 12-rem limit going through 1963. I don't know if that was an error, because there was no radium in 1963. The radium sources were retired in 1962.

So we propose ending in 1962 -- I mean, through 1962, just for making the convenience of the entire calendar year. And then the new betatron, since it went into operation sometime late in 1963, if that's consistent with NIOSH's procedures, start that scenario in 1963. And in both cases through

1966. And what we mean, the middle of the 1966 when the period of operations ended.

Then I would say the major thing of this agreement is we both agree that the layout man who is working in the No. 10 building just outside the betatron -- and this is a realistic scenario in that often they would -- radiographic casting, look at the -- and then they would take it out of the betatron room because they wanted to get another casting in, and unload it nearby. And the layout man would then literally crawl over the casting, if it was kind of a shape, and with the film or the previous exposure in hand, mark the areas where the grinders and chippers and welders would have to repair the casting.

I made this analogy at the previous meeting: very much like a dentist takes an X-ray and says, a-ha, here's a cavity. This is where I have to drill and put in a filling. It's a very close analogy. They have hidden cavities and they grind them out and then the welder fills

them in.

Okay. So there's more than a twofold difference in the dose that we calculate. We calculated in roentgens. NIOSH, I'm not sure what units they used, because in one report they say R or mR. In another report they say rem. And the number is identical, so I think they're using the units interchangeably.

And then the neutron dose comes from exactly the same scenario, because the electrons hitting the platinum target in the betatron -- the primary purpose is to generate X-rays, but they also generate neutrons.

The reason for this disagreement, NIOSH used 15 different shooting scenarios which we do not agree with. Some of them are at a 45- degree angle to the axis. It's a cylindrical casting that we use as an example, but we haven't had detailed information on it, even photographs of it. So we just use that as a typical casting. And they would not make

radiographs at a 45-degree angle to the axis. It just would not provide any useful information. So there are several of those scenarios. Those 15 scenarios, a number of them have that angle, I think maybe 10 of them, which we think are unrealistic.

The betatron positions, they've just put the thing arbitrarily in several different positions, three or four different positions in a betatron shooting room. Again, we found that an arbitrary selection.

But the main argument we have is that out of those 15 scenarios NIOSH made the stipulation that we have the -- I should have shown it. I did have a drawing. Just a second. I did have a drawing. Well, here is -- I don't have it shown here. I have it another place. But somewhere in this region where you can see my mouse moving was a storage area. It was a storage rack for the film badges that were kept there when the workers were off duty, or at least left the betatron room.

And NIOSH's position is that those badges could not have received more than 10 millirem a week because the vast majority of the badges have a reading of M for minimal, which means 10 millirem or less. And therefore they say that no matter what the scenario is, those badges could not have been exposed, the control badges. And that is contrary to information.

First of all, we do know that's where the film badges were stored. The NIOSH model treats this whole region as empty space. In reality there are walls there. There is furniture there. There is equipment there, of which we have no detailed information. So the MCNP model calculated the exposure to the film badge rack as incomplete. It also assigns an unrealistically low density to this brick wall.

Now, we're responsible for that, because in the original analysis we performed back in 2007 we were trying to maximize the dose to the control room operators. So since we didn't know what this wall was made of, we gave

it the lowest plausible density. We have since learned, based on the information that was obtained from NRC through Dr. McKeel's FOIA request, there was more detail and that these were filled, as you would expect, from good practice. These were substantially thicker walls or denser walls than we had originally assumed and that NIOSH used in their model. So there are many reasons why the exposure to this film badge rack in the NIOSH analysis would have been overestimated.

But the most important reason is the information that we obtained directly from Landauer - it just so happens one of our associates is a former officer from Landauer who has good contacts and relations with the current Landauer staff, and he confirmed -- I mean, he was not there in the 1960s, but he did obtain information from the records. He asked the current vice president in charge of operations to check the record.

And the story they came up with,

which I included both the summary in my report and a copy of his memo to me as an appendix, is that essentially any exposure to the badges while they were not being worn was zeroed out. The control badge was zeroed out against itself. I know that sounds illogical, but that's what they did. They took the control badge and subtracted that reading from every other reading, including that of the control badge. And only in the case if the control badge read more than 50 mR and was higher than half of the other readings, then they would say, okay, here we have an anomaly and they will report that to the customer.

But absent that, we really don't know what the exposures to those unworn badges were. And you cannot use that, in our opinion. We cannot use that as a basis for limiting which of these 15 exposure scenarios can be applied. So we disagree with the 15 exposure scenarios to begin with. And then we disagree with the method in which they were selected.

And another area of disagreement is the location of the layout man. This area, that's not shown here, but this is actually a diagram produced by the MCNP program itself. So here is the position of the betatron. Here is a cross-section of this hollow casting. You see the horizontal planes, those two lines. And here would be the railroad track on which the casting enters and leaves, a straight track. So the NIOSH model has the layout man in the center of the railroad track. Well, that's unrealistic because then you could not have any railcars coming in or out.

In our analysis we put it on either side of the railroad track. And it came out, and it's logical in retrospect, that this was the most exposed position, because you have -- except for the fact that there is a thin sheet metal roll-up door probably 16th of an inch seal here, you have direct line of sight from here to the betatron in this orientation, which is realistic. We do have information from a

now- deceased betatron operator that those castings were shot on the railroad track at times and that this was one of the castings.

So whereas the beam is strongly focused for -- it's not collimated; it is strongly focused forward. But nevertheless, there is some stray photon radiation coming off in this direction and the neutrons are probably -- I'm not that familiar with neutron generation, but the neutrons are most likely omnidirectional. So you do get your neutrons drifting in this direction. So that's the main basis for our disagreement.

There was also a badge called betatron control. We have no knowledge about this. I mean, it is listed in the film badge reports. Towards the end they stopped using it. It may have also been in the old betatron building, because the supervisor, who's also now deceased, we interviewed, had his office -- and he was there until about -- he left the betatron operation something like November 1964. And he

had his office in the old betatron building. It's just as plausible that the betatron control badge was in his office. We don't know. It's sheer speculation to say that it was kept in the betatron control room, absent any such knowledge.

And then, finally, the NIOSH model included a heavy steel door. I saw in their earlier MCNP files it was about 0.85 inches, which is pretty thick steel, whereas the workers say that it was just a thin sheet metal door like the kind you would have in a garage door, you know, a roll-up door on a garage. So, again, we disagree with the model for that reason.

Now, coming to beta exposures. I have to make a comment about the report by Dave Allen that just came out, that was distributed. I didn't get it until yesterday afternoon. There's a statement in the report that states that the NIOSH report came out in August, which is correct, and that the SC&A report giving other values, our values of beta doses came out on

October 6th. Now, literally that is correct. However, there is a misperception that this was new information.

In fact, the beta doses that are listed in this slide and are in the report, our October 6th report, were first reported to NIOSH and the Work Group in March 2012. And that analysis has not changed. And we have repeated in at least one other -- there was at least a presentation made in April to this Work Group which had these same numbers. So these numbers are not new information. And it is, I think, misleading to imply that NIOSH only saw this for the first time on October 6th.

Sorry to have to take that tone, but there was an implication there that I think is misleading.

Okay. That aside, the other statement made in Dave Allen's report is that because he inferred that by looking at our steel doses he found that if you doubled the steel doses you get better agreement. No, if you

double NIOSH's steel dose -- he didn't see our steel doses separate. If you double the steel -- I'm sorry, I'm skipping around.

There are two components of skin dose for the betatron operator. And that is handling uranium and handling irradiated steel. In other words, the uranium they have to handle while setting it up the first time. So it's just natural uranium.

We do assume the Putzier effect where the edges of these round slices, cylindrical slices, have this enhanced beta activity due to the migration of the short-lived uranium, the other products to the surface of the casting. And then you have the greater component which is the activation, or more correctly you create uranium-237 and -239, which are both short-lived beta emitters. So that's the other source of exposure.

And then, since the hours of uranium operation during each year are limited and they're based on the purchase orders from the

Mallinckrodt Chemical Works, the rest of the time the operator is presumed to be irradiating steel. And since there are many repeat shots -- I mean, you have the film, they use the standard chest X-ray film, so it's 14-by-17 inches, if I remember correctly.

So a large casting can be many feet across, so they keep shooting the same casting over and over again with overlapping shots. And so the operator is exposed to the activation products in the steel from the previous shots. So that's the second component of his exposure.

So Dave Allen's yesterday's report claims that if they double the exposure from the steel, then they will come close to matching SC&A's numbers. And they concluded that SC&A must have failed to take into account that the exposure -- that they would only be exposed, you know, at close range to the steel 50 percent of the time. That is an incorrect assumption.

I verified our calculations and we calculated it and there is a factor of two,

divided by a factor of two to account for the fact that it's not likely that the entire time the operator is near the steel, in the shooting room with the steel casting, that he would be up close to the steel.

For the uranium we actually have him at one foot and at one meter, 50/50 for the whole body, and contact at one meter for the skin and forearms. For the steel we didn't bother with the one meter because it's such a low exposure that we just essentially gave a zero. And we just had at 50 percent at one foot. So that is not the explanation.

The only plausible working method which can resolve this would be -- again we sent NIOSH, as requested, the calculations we had made back in 2007-2008. We have revised them simply because that was a trial version of MCNPX during the activation. And they have refined and put out a final publicly-released version which produces much higher concentrations of the activation products in the steel. And those we

have not exchanged between us and NIOSH.

So this would require an exchange of information. You know, if the Work Group and NIOSH so desires, we can get NIOSH's MCNP files. I assume they probably use Excel spreadsheets like we do, for the follow-up calculations, and we could examine those. And we can share ours with NIOSH and we can find out, you know, where the difference lies.

And I agree that the difference is most likely in the steel. Simply looking at these numbers on a percentage basis, the differences are smaller in the early years when the uranium -- with heavy uranium work, and that by far dominates the beta dose. And then it gets smaller. And here towards the end there is little uranium work, so most of the dose comes from the steel. And so now it becomes like a factor of two, almost a factor of two.

There is a consistent difference for the dose -- this is for the contact dose -- with the dose at other skin, which we assume would be

one foot apart, and we don't know what that is. It could be the thickness of the clothing. I know we had at one time, in connection with another site, there was an error in a NIOSH -- what we would consider a NIOSH analysis of what is the thickness of a T-shirt or a sweater that was much -- it was just an unrealistic number that was -- or I don't know if they're still using that. I really shouldn't speculate. I don't know what the reason is.

So I think that's -- okay. Oh, and then internal, for an internal exposure, we have come close to agreement -- this is my last slide -- to internal exposure based on the last several meetings. The one thing we point out is that at the last Work Group meeting, at the last teleconference, when I went over the notes, I have it on record that at least Jim Neton agreed with our proposal that during what we call the dark years from October 1st, '52, to February 28th, '58, there are no purchase orders. So we do not know what the exposure should be during

that time.

SC&A proposes, based on the subsequent purchase orders, that there is a maximum time, which happens to cover a 12-month period, but it's not a calendar year -- it's, you know, July 1st to June 30th -- I believe it was in '61, I think it is, where the maximum for the year was 437.5 hours based on, you know, we're paying you so many dollars and you're getting so much per hour.

So these were the maximum hours. And it was my understanding that NIOSH agreed to use those hours under the concept -- you know, this is like the co-worker model -- if you don't know, if you have an unbadged worker, one alternative is assigning the highest dose of the badge workers, so to assign the highest annual hours during that period. And NIOSH used what they considered a more characteristic of 337.5. So we're 100 hours apart.

And so therefore, since we agreed on the inhalation parameter, we have agreed on the

exposure time, you know, the fraction that the workers would be inhaling, we agreed on the concentration. I believe it was 68.7 dpm per cubic meter. And we agreed that they would be exposed 100 percent during the working hours of the uranium. So we have, you know, not surprisingly, a higher intake when it's averaged over a calendar year of 113 per day versus 91 per day.

And then we have another period. And here we have an exact number that we calculated from the purchase order for that exact four- month period, March through June '85, where we have 375. That's annual. So, I mean, we take those years and prorate them. So one-third would be 125 hours. So it comes out to 375 hours per year, again higher than the NIOSH number. And so again we come up with a higher inhalation during that period. For the remainder of that time, we agree with the NIOSH calculation for '58 through '66.

Where we strongly disagree is the

residual period. We agree with the assumption about a 30-day settling of the -- I mean, it's not mechanistically realistic, but it's at least acceptable results that we agree with, calculating the floor concentration as if the 95th percentile airborne concentration settled out over a period of 30 days at 0.00075 meters per second. So we're in agreement there. And we're in agreement on the resuspension factor of 10 to the minus 5 during the operation.

Now, the day the operations ceased, the only difference is the contract ended. So GSI was no longer radiographing uranium. The activity on the floor on June 30th, 1966, is exactly the same as on July 1st, 1966. And we agree with NIOSH on that. However, the resuspension factor suddenly drops in the NIOSH analysis from 10 to the minus five -- I should have said per meter down here. It suddenly drops from 10 to the minus 5 per meter to 10 to the minus 6 per meter. That's not reasonable and not realistic and it's not

claimant-favorable because the resuspension is due to activity in the betatron room.

And the betatron room, since there were only something like 13 hours during that last 12- month period, the vast majority of activity was radiographing steel. And they continued radiographing steel. And they were just as busy as they were during the operational period. There were just as many men walking across the floor, stirring up the dust, forklift trucks coming in, wheeled vehicles stirring up the dust. So there is no reason why the resuspension factor would drop.

The ten to the minus six per meter as the resuspension factor came out of an NRC report. I believe it was numbered NUREG-1720. And it applied to a quiescent area that has been decommissioned, has been decontaminated to the extent reasonable and is basically in a caretaker status. And that's a reasonable upper-end value to use for a facility such as that, because the purpose of that particular

NUREG was to do a radiological assessment of decommissioned facilities. And that's what they agreed on.

This would not apply here. Certainly there was no major -- we don't know about the clean- ups. We've heard anecdotal information that one time or another there were clean-ups subsequent, but we don't know that they specifically were clean-ups. They were certainly not clean-ups under the supervision of a health physicist or a health physics technician who had monitored the ground and said, "okay, guys, here's some contamination. Clean up here. No, we're okay here."

So our position is that they should continue using a 10 to the minus 5th resuspension factor. But we do agree that you should apply this exponential decrease in OTIB-70. So every day it decreases by a small fraction. So like by the end of -- I'm just trying to quote a number out of my head. But by the time of the FUSRAP clean-up in 1993 there would just be a small

percentage left. So the doses would decrease, but not by the sudden drop, n-fold drop on the day that the operation period ended.

So, okay. That pretty much winds up the presentation.

CHAIRMAN ZIEMER: Okay. Thank you very much, Bob. I know that Dave had some initial responses, some of which you've already sort of referred to, particularly the issue of the 50 percent and the 100 percent.

But, Dave Allen, why don't you give us your comments at this point now based on what you heard and what you'd seen before?

MR. ALLEN: Okay. There's a number of issues there, and I think the first one, or one of them on the list, is the number of uranium work hours. I mean, I used the work hours that we had been using before because I did not recall any agreement from NIOSH on that particular issue from the last Work Group meeting. So then after I saw the SC&A report from a few days ago, I went back to the transcripts and I still didn't

gather that from the transcript.

But I'm considering that particular issue not a major one, and I still think it should be the value that essentially started in the period where people -- you know, where we had a record and not the one year a few years later where it jumped up and using that.

DR. ANIGSTEIN: I can give you a page reference to where Jim Neton specifically was referring to 400 hours.

MR. ALLEN: Yeah, and I read that and I still didn't get that out of it. But that's beside the point. If the Work Group wants to use those SC&A hours, and if that's the only issue holding things up, I definitely don't want that to hold anything up and I would agree to use the SC&A values. No problem.

DR. NETON: Yeah, this Jim. I'm okay with those values. I guess, is this consistent with what we're using for the external dose assignments as well?

DR. ANIGSTEIN: Well, it's not --

DR. NETON: Because, I mean, it shouldn't be inconsistent with that.

DR. ANIGSTEIN: Of course not.

DR. NETON: And so what are the years that SC&A has decided -- or what are the hours that SC&A is using?

DR. ANIGSTEIN: The same hours as here. The same hours as for the internal, obviously. I mean, it should be obvious.

DR. NETON: If they're consistent, I agree with Dave: I don't think this is a show stopper. And given that we have no records back in that time period, I'm okay with going with the number of hours that SC&A -- and apparently I agreed to, although my memory is a little dim from that meeting, but it makes some sense to me. You know, given the lack of information in that time period, to go with the highest value is somewhat consistent with how we've behaved at other facilities when we were lacking information. So I'm okay with that. So I think that issue is no longer an issue, in my mind.

CHAIRMAN ZIEMER: Okay.

DR. NETON: That may be the easiest one.

CHAIRMAN ZIEMER: Well, you can agree to that one. Go ahead, Dave. What other items did you want to address?

MR. ALLEN: Okay. I'm taking things a little bit out of order, but I think we'll go for the easy ones first. And next one on my list is the date of the regulation change.

And I'm assuming Bob's correct on that. I went with the date that the NBS publication came out, and it makes perfect sense that it took a couple more years before those regulations were propagated. So, 1961 -- or through the end of 1960, as I understood you, we would use the 15. And then starting January 1, '61 we would use the --

DR. ANIGSTEIN: Twelve.

MR. ALLEN: And we're okay with that. Just making a note here before I go too far.

CHAIRMAN ZIEMER: Okay. Yeah, and I can confirm what Bob said, because I was involved personally when that was changed in terms of being a licensee. So I know that that occurred. So we have agreement on that. Okay. Proceed.

MR. ALLEN: And then the next easy one is Bob pointed out that in my White Paper I had the radium era ending at the end of -- I'm sorry, what did I do? I had it ending at the end of '63. And as Bob, I think, speculated on the phone there, that was probably just a mistake. And that's what it was. It was a mistake on my part. The radium era should be -- I want to get this right -- through the end of '62, and layout man dose starting January 1, '63. I think that's what SC&A --

DR. ANIGSTEIN: Yes.

MR. ALLEN: Yes. We're in agreement on that, too. That was a mistake.

CHAIRMAN ZIEMER: Thank you. Proceed.

MR. ALLEN: Okay. The rest of them not so easy. First with the resuspension factor, I know we've continued to have disagreement with SC&A on that on a variety of sites, et cetera. But, I mean, the basis that Bob's talking about just now is that it's an abrupt change. And that's true, but that's an abrupt change in an estimate. The truth is, with just a few hours in 1966 there that they working with uranium, we think the more realistic would be ten to the minus sixth, but we're using ten to the minus fifth because we don't know at what points in there they work with it, et cetera.

And, I mean, the basis for that comes from that NUREG, and as far as the studies that were used to develop those numbers, and from at least one of those studies it was a uranium facility and a study was conducted on the weekend of an operational facility. So it seems like aged and activity, because they did a similar activity without actually the uranium, a

simulated vigorous activity or something to that effect. But I think, in that if we're going to start saying what is aged and what's not, we might have to define that, if that's what you're saying.

DR. ANIGSTEIN: Well, other references indicate that contamination essentially weathers in and the resuspension factor gradually, exponentially goes down with time. And that is exactly accounted for by the OTIB- 70. Whether we say that the resuspension factor goes down or whether we say that the contamination level goes down, it's the same effect.

And then, actually, if you were to take the OTIB-70 approach and say, okay, this is the NIOSH assumption as to the activity on the floor at the end of operations, you know, June 30th, 1966, let's decrease it by the fraction. And it so happens it's 27 years for the final FUSRAP clean-up. So we decrease it by that fraction in OTIB-70. You actually come out with

one-tenth the average activity on the floor than what was measured by the FUSRAP in the old betatron building.

So, if you want to use that, you can say it doesn't go down as quickly as OTIB-70 assumes. But if you say, well, this is a combination of the actual decrease of the contamination level and the gradual decrease in the resuspension factor, then the factor of 10 exactly works out.

MR. ALLEN: Well, I think you're talking about the direct readings of contamination.

DR. ANIGSTEIN: I'm talking about the -- in an earlier report I took all the numbers where they took measurements on the floor of the old betatron building, the random, not the biased measurements, but the random measurements. With the biased measurements they were of course looking for contamination, so naturally they found more in localized hot spots. And it's much higher numbers. It did

not go down by the OTIB-70 fraction.

MR. ALLEN: Yeah, my point --

DR. ANIGSTEIN: But that's okay. I mean, we're still willing to accept the OTIB-70 fraction as a calculational tool, because built into that -- whether it was intended or not -- built into that is both the gradual removal of the contamination, the gradual exponential decay; not radioactive decay, of course, of the contamination; and the weathering in of the remaining contamination, which sort of makes sense. Obviously, the looser dust goes away more quickly and the more tightly bound dust stays longer.

But I think you end up with a much more realistic estimate, I would suggest, with this approach, because it doesn't give, you know, unrealistically high release rates because of the OTIB-70 decrease. If we were to say that it's 10 to the minus 5th and then the concentration stays constant for 27 years, I would agree that that's unrealistic, that it's

an exaggeration. But I think the other is a reasonable compromise.

As a matter of fact, in the original Appendix BB -- and I'm quoting from memory now -- it gives a different concentration on the floor and then it cites a number, a single measurement, that is cited in the -- let's see, the final was 1993 and there was an earlier investigation in 1988. I think I've got those years right. And they cite a number. And the Appendix BB says, oh, it's half of what we estimate, so our estimate must be a good number. Well, actually that's not a logical conclusion because it should go down by a lot more than half if you use OTIB-70.

So I'm not sure I'm making myself clear. If you use OTIB-70, we get a tenfold disagreement between the actual measured concentration, the average concentrations measured in 1993, and the predicted ones based on the assumption of the floor contamination that NIOSH and SC&A agrees on.

But if you increase the resuspension factor by a factor of 10, the net effect of the airborne concentration and the intake cancels out and we come out with a pretty good number.

MR. ALLEN: Well, I have to admit you did lose me about halfway through there. I'm following about half of that argument.

You are saying that the FUSRAP contamination surveys, compared to our production contamination estimate reduced by the OTIB-70 value to that time frame -- you say it came out higher or lower?

DR. ANIGSTEIN: Ten times higher.

MR. ALLEN: The actual measurement?

DR. ANIGSTEIN: The actual measurement. I did it two ways: the way I reported it, I took all the non-detects and assigned them the MDA value. But even if you just look at the detects and ignore those, you know, it's the same rough number. It's within a factor of two. And in both cases it comes out roughly 10 times higher than you would predict

by taking your number and multiplying it by the 27 years, which is something like three percent, if I remember correctly from OTIB-70.

DR. NETON: Bob, this is Jim. Have we seen that analysis? I mean, I hate to ask, but --

DR. ANIGSTEIN: No, no. No. No.

DR. NETON: Yeah.

DR. ANIGSTEIN: Sorry. This is something that John Mauro and I kicked around in a conversation and I did not include that. I agree it should be. We can send you a little memo on that.

DR. NETON: I think that might be appropriate, because if what you're saying is true, I think you've got something there.

DR. ANIGSTEIN: Okay.

DR. NETON: And, you know, I think we even went back and modified TIB-70 to talk about resuspension factors and said that we would do it on a case-by-case basis.

And maybe this is one of those cases

where, if there are data there, we definitely need to consider it. So I think that's where I'd leave it at this point, because, you know, it sound reasonable off the top of my head, but I'd like to see the data. I'm sure Dave would as well.

CHAIRMAN ZIEMER: So, Bob, SC&A has already done this analysis, you say? Well, it sounds like you have. I mean, is it in a written form that you could provide it pretty --

DR. ANIGSTEIN: Well, we do have the concentrations from the floor. That I have. That is actually part of what I call the alternative model, which was not accepted. But the data is there. Just our theory about working backwards from that. And I can certainly excerpt that and forward it. And the rest of the calculation will, you know, take a few minutes to write up. Yeah, I can prepare something.

DR. NETON: Yeah, I don't think you need to put anything elaborate together, Bob. I

think we understand the logic. Just sort of the data, you know, calculation?

DR. ANIGSTEIN: Yes, will do.

DR. NETON: Okay.

CHAIRMAN ZIEMER: And provide that to the Work Group as well, just so we have that in our records.

DR. ANIGSTEIN: Of course.

CHAIRMAN ZIEMER: Now, I want to make sure I'm understanding, though, in terms of the 10 to the minus 6th versus 10 the minus 5th issue, your argument initially was that nothing really changes on the day we go into the residual period, so why should that value suddenly change by a step function? And there's a certain logic to that. I think the 10 to the minus 6th, of course, assumes that a place has been cleaned up and it's sort of a quiet work area.

DR. ANIGSTEIN: Yeah, or a non-work area. Sort of a custodial.

CHAIRMAN ZIEMER: A custodial area. Would you be proposing that the 10 to the minus

5th remain throughout the residual period, or that --

DR. ANIGSTEIN: Yes, the 10 to the minus 5th remains throughout the residual period and it would be sort of counterbalanced by the gradual decrease by OTIB-70. So that when you come to the time of the FUSRAP clean-up, if you use the greatly reduced floor concentration according to OTIB-70 and the NIOSH assumptions scenario and apply 10 to the minus 5th, you will get approximately the same predicted air concentration as you would if you used the actual measured numbers and 10 to the minus 6th.

CHAIRMAN ZIEMER: Yes. So this would be sort of part and parcel to what you're talking about in the analysis that you would provide?

DR. ANIGSTEIN: Yes, exactly.

CHAIRMAN ZIEMER: Because, you know, intuitively, one would think, all right, I agree, the step function at the front end doesn't make sense, but is there some point at

which it reaches the other mode where it really is 10 to the minus 6th? But maybe your analysis sort of compensates for that. Anyway, you're going to provide that for NIOSH and --

DR. ANIGSTEIN: Yes, I'll try to get it out next week, early.

CHAIRMAN ZIEMER: Do I have any?

MR. ALLEN: What was that?

CHAIRMAN ZIEMER: Questions or comments on this issue from the Board.

MEMBER MUNN: Oh, we didn't hear that from you. At least I didn't hear it here.

I will look forward to seeing Bob's analysis. It's not intuitively obvious to this intuition exactly why that would be so, but I think that I'll be able to follow his analysis just fine. Thank you for being able to provide that for us, Bob. That would be helpful for us, I think.

CHAIRMAN ZIEMER: Okay. Dave, other comments on the other issues?

MR. ALLEN: Yeah, moving on to the

other issues I think where we have disagreement that's not new here is layout man gamma dose and the beta dose to the betatron operator.

The layout man gamma dose, we have looked at the e-mails or the correspondence that Bob had, you know, about the Landauer dosimetry and --

DR. ANIGSTEIN: I'd like to add, if I may interrupt, I also had telephone conversations. So I provided a summary of the discussion, and not every single item is in that memo that he provided to me.

MR. ALLEN: Okay. But what is in the memo, I mean, it's not super clear to me, but it did seem to contradict some of the other stuff you're saying.

DR. ANIGSTEIN: Well, it's not as clear as it could be. And I did have a discussion with him afterwards and he confirmed my interpretation or my understanding. I shouldn't say interpretation. He confirmed my understanding of it. Then I asked him to please

write a memo. And it was some time before he got around to it, so maybe --

DR. NETON: Yeah. Bob, this is something we might want to get on a technical call with, because, first of all, as you've indicated, it's sort of counterintuitive as to what you're suggesting that their process was. And it seems to be contradictory in the attachment that Mr. Zlotnicki wrote. When he talks about the Landauer procedures, he talks about how they subtracted the base fog density from everything --

DR. ANIGSTEIN: Yes.

DR. NETON: -- including the control badges, which is fine.

DR. ANIGSTEIN: Yes.

DR. NETON: And he said if they subtracted the base fog from the controlled -- then he said in a normal situation -- if the control badges were stored in a low background area, he said in a normal situation this meant the client controls that

were stored in a low background area would report as minimal or effectively zero.

Which I would agree with. If they were low background and they subtracted the fog and they came out below the detection limit, they would report as zero. But it specifically says they would report them as minimal.

In the very next paragraph, when they're talking about the client badges, it says the residual dose remaining on the client control would be subtracted from the batch of client badges, which makes sense. The next sentence, "effectively," it says, "the control badge was set to zero."

DR. ANIGSTEIN: Yes, exactly.

DR. NETON: That to me is interpreted to mean they were set equal to the background dose. It doesn't say that they were made zero.

DR. ANIGSTEIN: Well, okay. I agree with you that this is a little bit inconsistent and you only have sort of my -- you

know, I did provide a documentation. And I think a technical call -- and if we schedule it appropriately, Mr. Zlotnicki, excuse me --

DR. NETON: Yeah, because Mr. Zlotnicki's --

DR. ANIGSTEIN: -- can certainly -- you know, he's on our -- he's available to us and I'm sure he will be happy to participate.

DR. NETON: Yeah, I was thinking about actually having this before the call, but we didn't get around to it. But I think this is a critical area because essentially it is the determining whether those badges are useable. And I would agree that if they really made those badges M, which I find very hard to understand why they would do that, then, you know, the badges would not be useable. But, again, I think this is a critical issue.

DR. ANIGSTEIN: Okay. But most likely it's not something that we can resolve prior to the Board meeting.

DR. NETON: I don't think so. But it is a critical issue, I think.

DR. ANIGSTEIN: Yes, I agree.

DR. NETON: And after reading this very closely, I don't get the SC&A interpretation out of this. And I take your word for it you had more detailed conversations, but I guess --

DR. ANIGSTEIN: No, no. I'll be happy to. I agree with you completely and, you know, I will be happy to arrange that.

DR. NETON: Now, I'll say that, given that this remains to be the only outstanding issue related to the dose of the layout man. If the other issues can be agreed upon and this becomes the last issue, then we need to do this.

DR. ANIGSTEIN: Yeah. Well, and the beta dose.

DR. NETON: Well, and the beta dose as well. But what I'm saying is, as far as the layout man dose goes, this is one of I guess about

three issues that come into play: the shielding of the control badges and the angle of the shots that Dave chose. Those are two other issues that need to be resolved as well.

And, again, we have to discuss those two issues, and if this is the only one remaining, then we pursue this. Okay.

CHAIRMAN ZIEMER: This is Ziemer. I'm going to suggest, if you can -- I'm not sure, were you just talking about a technical call with this Joe Zlotnicki?

DR. NETON: Zlotnicki, yes.

DR. ANIGSTEIN: The English pronunciation is Zlotnicki.

CHAIRMAN ZIEMER: Yeah. Now, is Joe formally affiliated with SC&A?

DR. ANIGSTEIN: Say again?

CHAIRMAN ZIEMER: Is Joe formally affiliated with SC&A?

DR. ANIGSTEIN: Oh, yes, he's an associate. He's an SC&A associate.

CHAIRMAN ZIEMER: Might be of

value --- I'm not saying don't use Joe -- but might be of value to have someone who's independent, from Landauer involved as well. If you could get Craig Yoder, that would be good.

DR. ANIGSTEIN: Yes, well, let's see, I would suggest --

CHAIRMAN ZIEMER: He's been their technical guy for many years.

DR. ANIGSTEIN: Yes. I would suggest, if that's -- I know he's not on the phone now -- that Stu Hinnefeld, you know, is personally acquainted, I think that they were in school together, with Craig Yoder, who is currently an officer with -- and I believe that he was the contact that Joe Zlotnicki uses. I mean, he contacts him and then maybe it gets passed on to some technicians who look up the records. But it might be more appropriate for Stu to contact Craig and ask him if he would like to participate.

DR. NETON: I would suggest that it may be as simple as an email posing the question

directly. I mean, we only have one question.

DR. ANIGSTEIN: Okay.

DR. NETON: How were the control badges reported in this era to the client?

DR. ANIGSTEIN: All right.

DR. NETON: Were they automatically reported as M or did they actually report the dose?

DR. ANIGSTEIN: Should that email come from us or from NIOSH?

DR. NETON: Well, it depends on -- I guess if you're -- and you suggested it might be a reasonable idea that Stu contact Craig Yoder. Maybe he should make the first contact. I don't want to speak for Stu, but we can investigate that and see if Stu feels comfortable pursuing that way. If not, I don't know.

DR. ANIGSTEIN: No, because NIOSH actually at one point had a contract with Landauer.

DR. NETON: I understand. Yeah.

DR. ANIGSTEIN: So this would just

be -- I know he's no longer active, but at least they would, you know --

DR. NETON: Yes, and this might be something that could be cleared up in just a single email exchange.

DR. ANIGSTEIN: Okay.

DR. NETON: I mean, very simple. So I think that I will pursue that with Stu and see if we can get this to be answered, you know, fairly quickly. And I'm only going to ask one question: you know, how were the control badges results reported to clients during this time period? Okay. We'll take that action and I'll see if we can get that done quickly.

I do know that Craig Yoder is a very busy man, and in the past when we've been dealing with him it's been hard to get in touch with him because of his schedule. So that may be a limiting factor, but we can --

CHAIRMAN ZIEMER: Craig Yoder is at the vice presidential level, so that is why it becomes difficult.

DR. NETON: Yes, I'm not complaining or anything. I'm just saying that --

CHAIRMAN ZIEMER: Yes, right.

DR. NETON: -- he's busy. But I think we'll try that. And, again, this maybe can be answered in a single email exchange over a day or so. So we'll try that, if that's acceptable.

CHAIRMAN ZIEMER: Thank you very much, Jim.

DR. NETON: What's that?

CHAIRMAN ZIEMER: Thank you very much.

DR. NETON: Okay.

CHAIRMAN ZIEMER: Okay. Let's go on to the additional questions. Dave, you want to -

DR. McKEEL: Dr. Ziemer, this is Dan McKeel.

CHAIRMAN ZIEMER: Yes, Dan?

DR. McKEEL: I would like to put in

a timely comment to that, if I may.

CHAIRMAN ZIEMER: Sure. Oh, of course.

DR. McKEEL: I endorse the idea. I think it's a very good idea to get in touch with Landauer directly, but if the overture to Mr. Yoder either doesn't work or it's taking a long time, I want to remind everybody that when I originally got the annual GSI Program 2084 film badges from Landauer, I called and spoke directly to a man named Chris Passmore, P-A-S-S-M-O-R-E, and he engaged a woman named Emily Quirke, Q-U-I-R-K-E. And we had telephone calls and exchanged letters. And for a while they were very helpful and they sent me the annual film badge records. And then Larry Elliott, who was the OCAS director at the time, wrote a letter which informed Landauer that petitioners actually had no special entree to such records. And so after that time it became more difficult.

But I would strongly suggest, since

Chris Passmore and Emily Quirke were the two people who actually did the research, found the badges and sent them to us -- I'm not sure if they're still at Landauer, but they might be, and they would also be two people that could be interviewed about this.

I agree that this is absolutely, absolutely crucial. And, you know, I cannot underscore how important it is to get the badges straightened out.

One of the other points I want to remind everybody about, while it's fresh on my mind, is that Bob Anigstein said that the drawings indicated that the film badges were kept on a rack. Singular. And Terry Dutko, who is now deceased, a betatron operator at GSI you all well know, sent us drawings; and they've been circulated and you all have them, I'm talking about everybody, the Board, NIOSH, SC&A, that actually carried through two locations for the film badges in the betatron facilities. And he clearly said that the film badges were kept at

both facilities.

But also there was unanimous opinion backed up by affidavits of the GSI workers that they were unaware of anything referred to or called a control badge among the film badges at GSI. And our affiants included, for example, [identifying information redacted], who was the clerk who managed the film badge distribution program at GSI for several years. But all of the workers said they simply aren't aware of that.

So I think it's important to not only get -- I think we need to get Landauer's full comments in writing and then make certain that that full set of comments is put on the record. And I don't think a phone call actually accomplishes that purpose. I don't think a technical call where Ted Katz writes up a summary of the call -- I don't think that accomplishes that purpose.

I don't think there's anything that will substitute for a letter on Landauer

letterhead signed by a person or persons with -- I think it would be better even if you talked to Craig Yoder, that he ought to consult with Chris Passmore and Emily Quirke and make sure that the information we get from Landauer is as complete and accurate as possible on this issue, which I couldn't agree more with Jim Neton is absolutely crucial. So I appreciate you letting me make that comment.

CHAIRMAN ZIEMER: Well, thanks for those additional names, Dan, because particularly if Dr. Yoder isn't available, certainly can follow up with these folks and maybe all of them will get involved.

But the ball's in NIOSH's court then to follow up on this. And initially this will be in writing, email. If we need to get a formal letter at the other end, we can certainly do that as well.

But, okay. Let's proceed. And then, Dave, do you have some additional items now to respond to?

MR. ALLEN: Yes, I think the last one on the list there is the beta dose. And I'm going to apologize to Bob right off the bat. My original draft that was sent out yesterday I believe it did have “speculate” in there, that they didn't use that 50 percent. And in my rush to edit things and get that piece of information out, I'd somehow changed the “speculate” to “concluded.” And that wasn't intentional. That was my fault. But my main reason for trying to get that out was if it were that simple, we could possibly put this to bed. And that's why I wanted to get it out.

Bob has pointed out in his review that it's not that simple and that's not what happened. And unfortunately I don't see any real way around that other than us trading files, like Bob said, to try to figure out where the difference is.

CHAIRMAN ZIEMER: Okay. Well, the analysis that SC&A is going to provide is the one on the contamination levels, but what do you need

on the beta?

MR. ALLEN: Well, we just simply don't know what the difference is, why they're getting some numbers and we're getting another set of numbers.

DR. ANIGSTEIN: Which way should we --

CHAIRMAN ZIEMER: So both of you both think that you're using the same parameters and the same calculational methods, right?

MR. ALLEN: Right, that's why there shouldn't be a huge difference like that.

DR. ANIGSTEIN: Well, okay. Which way? Should it go both ways, or, Dave, would you like to send us the files and we have our MCNP people here, myself and then a couple of my consultants, who can review them and, you know --

MR. ALLEN: Well, I'd love to see yours and I'm willing to send you mine.

DR. ANIGSTEIN: Okay. Well, you see it's not that simple. Ours is a set of interlocking spreadsheets which would require

some explanation. I mean, I'll be happy to send them, but I'm just warning it's not an immediately transparent process.

MR. ALLEN: Well, I agree. There's complicated calculations. I'd probably have to provide you some explanation as to mine, too. So let's --

DR. ANIGSTEIN: I'm open to direction, whichever way you want.

CHAIRMAN ZIEMER: Just trade between the both of you.

DR. ANIGSTEIN: All right.

MR. ALLEN: Yes.

DR. ANIGSTEIN: All right. It's going to be a little while before we do that. I mean obviously it's not going to be before the Denver meeting.

MR. ALLEN: Yes, between explaining them, getting them traded and somebody else analyzing, it's not going to happen by next -- what is it, Wednesday?

DR. ANIGSTEIN: Yes.

MR. ALLEN: Right. That's clear.

MEMBER BEACH: This is Josie. Is that something that would maybe require a technical call, or would you just do that via email, explaining your numbers?

MR. ALLEN: Well, the explanation I was talking about is just explaining what's in a spreadsheet, because sometimes, especially me, I'll write these up with numbers and, you know, the headers are cryptic, you know? So for somebody else to make any sense of it, I'll have to say, okay, in this column, you know, this is what we did and stuff, so they could have a better chance of actually following through that.

CHAIRMAN ZIEMER: So you just need to prepare the information in a way that they can understand what you're did. Sounds like it's written information rather than a technical call.

MR. ALLEN: Right. I don't think that's something you want to try to explain on a call, at least the first shot around. And then

maybe some clarification if it's not clear.

DR. ANIGSTEIN: Yes, I agree with Dave that we exchange information. Then I would say, probably, on our end, I would say we issue a report, a brief report or a memo saying this is what we found.

MR. KATZ: Yes, Bob, this is Ted. Once you've both done your analyses of each other's spreadsheets, you probably need to trade some emails before you put out any final report to make sure you each understand each other's material.

DR. ANIGSTEIN: Sure.

MR. KATZ: Yes.

DR. ANIGSTEIN: Or we can put out sort of a draft report and send it, if we don't already do that, because it's easier. I personally find that writing a report sharpens my thinking. So if I write it down and then I can send it, we can have -- you know, before issuing it officially we can exchange it and have it commented on, if that's acceptable. It's the

same thing basically. You know, an email with an attached note to it.

MR. ALLEN: Well, I think some emails may probably be warranted just to make sure we understand each other's --

DR. ANIGSTEIN: Yes, right, right, right. But it won't necessarily -- it might be an email attachment.

MR. ALLEN: Yes, I mean just a clarification type of --

DR. ANIGSTEIN: Exactly.

MR. ALLEN: What did you do here versus --

DR. ANIGSTEIN: Yes.

MR. ALLEN: -- an evaluation? Not an evaluation in the emails, but some sort of a clarifications-type --

DR. ANIGSTEIN: Well, in email, I mean the first be a transmission of the file with a note explaining what we did.

MR. ALLEN: Right.

DR. ANIGSTEIN: And then once we get

your material, we'll send you another email with comments and questions.

MR. ALLEN: Right.

DR. ANIGSTEIN: So there will be an opportunity to respond, to clarify, respond, acknowledge. And then in the end perhaps each can issue a report saying we're right, you're wrong, or you're right, we're wrong.

(Laughter.)

CHAIRMAN ZIEMER: Okay. Well, that's certainly a way to get a handle on why you're seeing these differences. You know, if it's just a simple calculational thing versus some major underlying assumption that is very different, we need to identify that.

Are those the only items now where we have to address on the original paper here then? Is that the last one, Dave?

MR. ALLEN: I think it was, yes.

CHAIRMAN ZIEMER: Jim Neton, did you have another item that was on there that you had a question on?

DR. NETON: Well, you know, there's still a couple outstanding items on the badge rack issue, you know, unrelated to the detection limit of film badges, and that is the model that Dave Allen used to generate the photons at the badge rack.

And Bob Anigstein's comment on the shielding between the betatron room and the control room -- I mean and the badge rack. I don't know whether, you know, those are worth discussing today before we decide this. You know, if the film badge issue is as SC&A portrays it, then I guess the other arguments are not worth discussing, because unless Dave Allen can correct me, I think that that's the key issue. So maybe we have to decide if this is still -- solve this issue first. But those are the only two issues I can think of.

CHAIRMAN ZIEMER: Yes, and it's not clear to me whether that issue needs to be resolved before we get the -- I think we still need the Landauer information.

DR. NETON: Yes, I agree. If the Landauer backs up, is exactly the way Bob Anigstein has interpreted it, then I think the other issues may be small potatoes compared to this.

CHAIRMAN ZIEMER: Yes. So I guess my question is do you want to do anything further on this now, or hold this off until you get the other information?

DR. NETON: Well, after thinking about it some, it's probably best to solve this issue first, I think.

CHAIRMAN ZIEMER: The badge rack issue?

DR. NETON: Yes, because we could debate a lot about the other two issues and this one would trump the other two, I think.

CHAIRMAN ZIEMER: Okay. Well, let's go to it, then. Let's see.

DR. ANIGSTEIN: I'm sorry, I'm losing track. The badge rack issue. Which other two issues would be put aside?

DR. NETON: Well, I think the key issue to answer right now is the reporting practice of Landauer --

DR. ANIGSTEIN: Yes.

DR. NETON: -- for the badge rack control.

DR. ANIGSTEIN: But the beta dose is completely separate, independent of that.

DR. NETON: The beta dose?

DR. ANIGSTEIN: Do you want to proceed with that, or you want to not proceed with it?

DR. NETON: You're talking about the beta dose with the MCNP files?

DR. ANIGSTEIN: Yes, I mean that's completely separate from this.

DR. NETON: Oh, yes. No, that needs to be pursued. I have three issues down. And, you know, the --

DR. ANIGSTEIN: Right. Okay.

DR. NETON: -- ten to the minus sixth versus ten to the minus fifth you're going

to send us data.

DR. ANIGSTEIN: Right. Right. Right.

DR. NETON: We're going to try to poll Landauer on the practices of reporting --

DR. ANIGSTEIN: Right, yes.

DR. NETON: -- during that era. And then the trade files for the MCNP.

DR. ANIGSTEIN: Right. So these are all independent?

DR. NETON: Yes, they're all independent. What I was talking about was the other two issues related to using the badge rack --

DR. ANIGSTEIN: Oh, I see. I got you.

DR. NETON: You know, that made the exposure --

DR. ANIGSTEIN: Yes, I understand. Yes. Yes, there will be -- once you don't use the badges, then the rest is moot.

DR. NETON: Exactly.

CHAIRMAN ZIEMER: So the question is what do we need to do on the badge rack issue now.

DR. NETON: Well, I think the issue is to contact Landauer and get hopefully a clear answer as to how they behaved.

CHAIRMAN ZIEMER: Right. And once you have that, then we can determine whether this other needs to be pursued then.

DR. NETON: Yes, and the other ones still may be okay. You know, it just depends on how accurate they are and what adjustments may or may not need to be made to make them more accurate, yes.

CHAIRMAN ZIEMER: Okay. Let me ask again, Work Group Members, any questions on proceeding in this way?

MEMBER MUNN: No, that sounds perfectly logical to me.

MEMBER BEACH: I don't have any either, Paul.

CHAIRMAN ZIEMER: Okay. John?

(No response.)

CHAIRMAN ZIEMER: I was talking to John Poston.

MEMBER POSTON: Can you hear me?

CHAIRMAN ZIEMER: Yes, there you go.

MEMBER POSTON: Yes, I'm okay with it.

CHAIRMAN ZIEMER: Okay. Now I want to give Dr. McKeel a chance to comment also on the Dave Allen paper and the related issues that we've talked about here.

And, Dan, we have your document, a critique of Dave Allen's August GSI White Paper. And I think there's some follow-up. I think you had another one a day or two later. Well, that was information on an abstract. And your papers are also on the website. But why don't you go ahead.

DR. McKEEL: Okay. Are you hearing me now?

CHAIRMAN ZIEMER: Yes, go ahead,

Dan.

DR. McKEEL: Okay. Good. Well, I have some comments about what's been discussed in the meeting and then I had a few things that I knew needed to be addressed. So if you don't mind, I'm going to kind of take it in reverse order and wind up with the comments about the immediate discussion today as the last item so I can get through these.

I want to stress that I sent this Work Group four papers. Two of them were from August of 2013 and one of them was the rebuttal paper that I had to Dave Allen's White Paper on the GSI estimated doses. So I'll address that in a minute. The other papers were two reports from the Health and Safety Lab of the AEC New York Operations Office, and that's NYO Report- 4699. And there is a 1957 original paper and there is a Supplement 1 from the next year.

And what's interesting about those papers is the AEC conducted in the '50s -- it may have gone on later, I'm not sure, but in the '50s

they had these two reports about what they call the Accelerator Surveillance Program. And these two reports report their information on going to at least 38 sites that had accelerators of various kinds: cyclotrons, most importantly betatrons, larger accelerators.

But the highly interesting part of these papers is they went to extensive lengths to carry their measuring instruments to the site. And so they measured the photons and they measured neutrons, and they spent a lot of time discussing the neutron results. And of most interest and highly pertinent to General Steel Industries, in fact so important I would rate this as maybe the most important paper about betatrons that we have yet seen about GSI.

But in the Supplement 1 paper, which I review pretty extensively in my White Paper, they include data on three 22, 25 MeV betatron sites. One is at Memorial Sloan- Kettering Hospital and I assume it's the same machine that -- the Health Physics Society president was

the head of that department -- and was later donated to the Smithsonian Institution. And then they have additional data on two betatrons, both of them at the University of Illinois. One of them I gather was used for research purposes and the other was definitely used in the medical school.

And for all of the accelerators the reports had highly interesting data. Number one, they included pictures, two-dimensional drawings, some photos of the machines being used, but also of the facilities themselves. And these were like the ones we have for GSI. They were not blueprints, but they were sketches, and very informative sketches.

They also had and collected film badge data, real measured film badge data from the workers who operated those accelerators. And then they had extensive photon measurements from the operating accelerators and that included not just the machines themselves, but also the facilities and most interestingly in

the case of the University of Illinois Medical School betatron, of the surrounding buildings. And so this is the only place that I'm aware of where they have -- where an AEC/HASL/NYO team went out and made extensive actual measurements of betatron photons and neutrons and included with that film badge data from the workers that were involved.

And I assume from what Paul told me that all of you all had those papers and have read those papers. I've got to tell you I'm surprised since I sent those out in August that -- you know, and all of September went by, some of August and up until today. I've gotten no feedback from either NIOSH or the Board about those important papers. And so I'm assuming that you all have all read them. I certainly don't have time to go into them right now.

The latest two papers I sent to you all are just informational really, but they also have something that's new and needed, I believe. Those two papers are -- on May the 17th, HHS

approved an administrative review for GSI, and the three panel HHS members have been working on that since May 17th. I have not heard from them. I didn't expect to. So it's been five months. They have not made their decision apparently, nor have I heard from HHS what their recommendation was and what Secretary Sebelius' final decision was on that matter.

And then on the 7th of October, I sent you an addendum paper to the administrative review that did several things. One is the first paper, the administrative review had 44 errors I cited for the three-member panel. And they were errors of omission, commission, policy matters, as well as technical and scientific matters. And to those I've added 20 new errors I think that have been made since the Board voted 9 to 8 to deny SEC on December the 11th, 2012.

Ted has distributed that document to the entire Board and I sent each of the Members of the Work Group, the Board Members a copy as well. I sent copies to NIOSH as well. And I

assume that Ted also distributed copies perhaps to SC&A.

The other thing I did in that last paper was -- I have been highly interested of course ever since the Board voted to see when the Appendix BB and transferred SEC issues that were made part of the Appendix BB matrix would be addressed. And so far I think it's fair to say they have not been addressed since 12/11/12. So I made a list in there of the 19 still open issues; that is, issues that were either transferred, or were marked as in progress, or were marked as open, but that were not definitely closed by all Members of the Work Group.

And, you know, it's my understanding, I think everybody agrees, that all of these issues have to be systematically worked through before NIOSH can be even in a position to revise Appendix BB Rev 0, which was, you know, put in in June of 2007.

I also have to note that in those two matrices that I reviewed, again the Appendix BB

one is from 11/26/12, the SEC matrix issue is from 12/5/12, they have detailed timelines which elegantly lay out what's been done about the GSI TBD-6000 and Appendix BB.

What I was interested in is really from a scientific and personal view and the way business is conducted is there's no mention at all of the fact that between 2007, July 2007 and today I've submitted 52 White Papers that I authored and have posted to docket 140 for GSI and shared with the Work Group and the Board about GSI. And I personally think that petitioner input should be weighted higher than that and it certainly should have merited an entry into the timelines of the decision matrices on the important issues.

My third point is that I thought that the NYO-4699 papers were so important because they were the first and only measured photon, neutron and operator film badge data that we had on comparable betatrons to the GSI ones. So I ask that he task SC&A to review those papers.

Paul said that everyone had read my papers. And so he didn't think that SC&A needed to review them. But I notice today, for instance, in the discussions that have taken place so far, none of those papers have really been mentioned at all.

I think it's extremely important that this Work Group look particularly at the neutron doses. The authors of the NYO-4699 stress how significant that was, and they cite for instance, at the University of Illinois Nursing School, that there were still overdoses of the neutron from betatron vaults that were shielded similarly to the ones used at GSI. But there was spillover of neutron doses into the nursing facilities, into the hallways of the living areas of the adjacent dormitories.

And unlike the modeled doses, it is extremely interesting that these papers detail the neutron-measuring devices that they used, and in some accelerators they used up to three different devices to triangulate and make sure

that the doses they measured were as accurate as possible. It's also a good primer on how difficult it is to measure neutrons from different kinds of accelerators using, quote, standard methods. In fact, they found you really couldn't do it. You had to have several sources, all of which when combined gave you a much clearer picture.

So anyway, I encourage everybody to look at that and discuss it and make it part of the agenda for any next meeting there is of this committee.

Then I want to turn very briefly to Dave Allen's GSI dose estimate paper. You know, I found in my rebuttal that there were just numerous things I disagreed with, and the first one relates to what I've just been talking about, and that is that he speaks of -- and also the first comment today by Dr. Anigstein -- they both agree that the radium era doses to workers should be bounded by the two radium sources.

But what's omitted from that fact,

at least Bob Anigstein and SC&A acknowledge that there was a 22, 24 MeV betatron operating at GSI from October the 5th, '52 through 1962, so during the entire radium era. What they both ignore is the fact that the radium sources didn't give off any neutrons. And so they ignored the betatron neutron doses during the radium era. They need to be modeled, but the model needs to be validated using the NYO-4699 measured neutron data to compare with.

We all know that this is not true, but if you read Dave Allen's paper, you would think that the only source at GSI, the only source term was the two radium sources. So not only was the old betatron ignored, but so were the two 250 kV X-ray machines and so were the iridium-192 sources. And I've sent you data from Paul Sinn recently that he estimated that there were 25 to 50 uses of the St. Louis Testing Lab's iridium-192 sources at GSI. He's not very clear about the dates for that, and it may be that it started after the radium era, but those

up-to-50 iridium source exposures are not mentioned in Dave Allen's paper at all.

I wanted to comment that we're spending a lot of time retaining this term layout man at GSI. We have established I think now conclusively that although this was an operational term, that actually no person, no employee of GSI ever held this job as an exclusive job category. So whatever you assign to the layout person, they also accumulated dose due to other types of exposure.

I noticed that in this paper, although -- and I've noticed very carefully since we've supplied data and NIOSH supplied data from October 1952; our data was from November and December of '52, that this Work Group has not even mentioned the papers that went into arriving at that conclusion showing that the 24 MeV old betatron was used in conjunction with the AEC and Mallinckrodt in an experimental program they had to develop better imaging of uranium using uranium billets, actually

sections of uranium billets, and using a uranium shield that Mallinckrodt had designed and brought over to GSI in order to improve the quality of the radiograph team.

Nobody's ever calculated those doses. Nobody's ever modeled those doses. And of course there are no actual purchase orders for those dates either. There are statements from the AEC operations report that that work existed.

Anyway, there are lots and lots of other objections I had to that paper and I'm going to have to trust that you all have read that.

The fifth thing, next to last thing I want to talk about is an awful lot of the dose assignments from the years 1958 to 1962, before the Landauer Film Badge Number 2084 Program began, is based on film badge reports from one part-time radiographer, [Identifying information redacted], and SC&A has detailed

that data quite elaborately.

[Identifying information redacted] also supplied John Ramspott and I with the same set of his data. And so it includes not just that one page that has the 18 quarters of data and so forth, it also has reports for later years that show a dose received by him of zero. And [Identifying information redacted] worked at GSI until 1973, when it closed. And the complete Landauer data set that SC&A and NIOSH are privy to includes all the weekly data through 1973 as well.

Well anyway, my original annual report from Landauer also has annual reports up through 1973. And it is possible even though a lot of information is redacted from those early -- not a lot actually, but some, the names are redacted, you can follow through -- well, even not all the names are redacted from that set. So but you can follow through this one particular individual's data in the data set

that I got from Landauer, and there are non- zero numbers in those later years. So there's a discrepancy.

The result of all this was we have urged [Identifying information redacted], even though he has not filed a claim, to obtain his Landauer film badge data. And we helped him do that. He initially contacted NIOSH about getting his report in June. They wrote back to him and said that he could do that, but he would have to send them some forms attesting to who he really was and in compliance really with the Privacy Act laws. [Identifying information redacted] did that and those papers were mailed back to NIOSH in mid-July of this year to a woman named Mrs. Aquino, A-Q-U-I-N-O, who had sent the original letter to [Identifying information redacted].

[Identifying information redacted] tells John Ramspott and I as of yesterday, he has heard nothing back from this request for his

Landauer film badge records. So it's been from mid-July through August, through September and now October. So it's been almost three months and he's not received his Landauer film badge data. And it seems to me that that needs to be addressed immediately by NIOSH and to get him those film badge data.

Final thing I want to say is just a couple of comments that relate to things that were said today during the meeting. I've already pointed out that both Dr. Anigstein and Dave Allen feel that radium is bounding for 1952- 1962 exposure, external exposures. And that totally neglects the fact that radium gave off no neutrons, but the betatrons were giving off neutrons that entire period. So the radium gamma protons certainly don't bound the contribution to dose from betatron neutrons.

Second point is we spent a lot of time -- Dr. Anigstein spent a lot of time going over material that has already been discussed in

great detail, including the information about the 15 NIOSH scenarios and the angles used and SC&A objections to all that, but during that discussion he also mentioned the presence of a storage rack. And I want to reiterate and underscore that there were two racks.

So when you model the exposure to the control badges, you don't know which of those racks those badges were. You have to model them both. They were in two different locations on two different walls 90 degrees apart. And as was said, the betatron control room was just one of many rooms in that structure. And the film badges were in another room on different -- in two other rooms actually on two different walls of the building. And you have those drawings so you should be able to model them.

I want to reiterate that I do not think that accepting telephone information from Mr. Zlotnicki, who was employed by Landauer, but he's not employed by Landauer now -- I don't think that's sufficient. I think and agree that

we need to speak to Landauer and get an answer to them in writing.

And as you know, we have put on the record numerous objections why the film badge data for those 89 betatron operators of a workforce of 3,000 people is not representative. So we don't think you should use that film badge data for anyone but betatron operators, and that means you don't have any way to calculate. There is no film badge data. There is no bioassay data, no monitoring data of any kind for the rest of the people at that plant.

My same comments I would say about the models that were developed for the layout man. You know, again, this was just a rehash of things that were discussed extensively and here we are months later and they're still listed as an open issue. And it was clear from the discussion today that NIOSH and SC&A don't agree about that.

There was a comment today about a math error in TBD-6000 that was going to be

fixed, but when you actually read that matrix issue, NIOSH says they will fix it when Rev 1 of TBD- 6000 is revised. And at least based on NIOSH's experience with Appendix BB Rev 0, they may not revise TBD-6000 for a long time.

Next comment I wanted to make is there's been a lot of discussion about the resuspension factor. I wrote a paper about why I thought TIB-70 was not a good model for what happened at GSI. And the primary reason was -- and I didn't think even the ten to the minus fifth number was necessarily the best one to be chosen.

And what I pointed out in that paper, and John Ramspott provided volumes of information about this, is we know that not only the betatron facilities where the uranium was shot, but that in all of the buildings that the uranium passed through when it came in on railroad trucks -- and that would include Buildings at least 5, 6, 7, 9 and 10 -- what we referred to in all our papers as the uranium

transport path -- that there was uranium all along that transport path in the railroad tracks, around the railroad tracks. And we know that, in the 27 years of the residual period, there were multiple businesses, steel businesses that came in and had operations in those other buildings. So we believe that the resuspension and settling of uranium at GSI during the residual period was a cyclical phenomenon.

The other comment is that everybody seems to have forgotten that in the original discussion of the resuspension factor, John Mauro actually argued that there were instances in the literature; and this was also argued when the Procedures Review Committee took up TIB-70 recently, this year -- that there were statements in the literature that a resuspension factor could be as high as ten to the minus two per meter or ten to the minus three, ten to the minus four. So even higher than the ten to the minus five.

My conclusion: ten to the minus five is totally arbitrary. You have no idea what it was. And, you know, the buildings that the uranium was suspended in at GSI included small rooms and it included very large rooms, Buildings 8, 9, 10, 5, 6, 7 were all interconnected. They were basically one big roof with some steel walls in between them which weren't complete.

And finally, I wanted to put in my two cents' worth about what was actually said at the June 2013 meeting about uranium hours. If everybody remembers, as soon as that meeting was over, I was so struck by the lack of clarity on the uranium hours issue that I actually submitted my annotated notes on that meeting. And one of the things that I highlighted was that there certainly was a discussion about the 400 hours, and there was a discussion which I took to be that the highest numbers of hours, the 437 hours in '61, was going to be used throughout that period.

And it is my recollection that that was basically what Dr. Neton was saying, but after rereading that transcript over and over, I had to say to myself -- and I put in my report it wasn't clear what was agreed upon. So, you know, that's a general comment for all of these issues.

I think at the end of a Work Group meeting there should be a definite statement just like you do for why the Board recommends denial or approval of an SEC that explicitly gives action items that are to be followed up on. And it really would be good, when we're having meetings that are all about do SC&A and NIOSH agree, to put the areas of agreement and disagreement in that list of action items at the end of the meeting.

Anyway, once again I sincerely appreciate you giving me some time to address the group and I look forward to the rest of the discussion.

CHAIRMAN ZIEMER: Okay. Dan,

thank you for those comments. I want to follow up a little bit on the document dealing with the survey of the accelerators. First of all, I'll tell you that I did read the document and went through -- it's a totally extensive document. But some things occurred to me as I read it, and I want to maybe ask Jim and Dave this question, and also SC&A folks can respond to it also.

But it occurred to me as I looked at those surveys done by the AEC, and they were done decades ago, where they have information about the shielding of these accelerators and the radiation levels at different locations, but would it be feasible and/or even useful to take the MCNP model and see how it predicts the actual readings based on the output of those machines and the shielding that is provided. Now I'm not saying to do it for all of those, but perhaps a couple individual ones that would be similar to the GSI one.

The reason I'm thinking about that -- and I think in part Dr. McKeel has

suggested this would be a kind of independent validation of the usefulness of the code for this type of application. But maybe, Jim or Dave, you could respond. Is that something that is feasible or useful? And maybe, John Mauro or Bob, you could also respond.

MR. ALLEN: Well this is Dave Allen. I can start responding. And when I first got a hold of that document, that was what my intent was. And it is lacking more information than what we had for GSI, I mean as far as dimensions. And I thought I could maybe guess at some dimensions, et cetera.

Then you start looking at thickness of the shielding material and what that material is. And you know for the ones that aren't next to some sort of window that I'm going to guess is a lead window or something. For the ones with just a thick wall I could almost guess concrete there. But then on most of them I don't even really have a beam orientation. I mean I could toss a number of orientations in there to try to

reorient the numbers or whatever, but in the end it's going to end up being, you know, a number of guesses on here. And I didn't know how much worth that would be for anybody.

CHAIRMAN ZIEMER: Well, I wasn't sure whether the information was adequate to be used for that purpose. It just had occurred to me. And of course the actual survey was done really to -- for a couple reasons. One was to determine adequacy of shielding throughout these different facilities. And of course one of the things they found in general, it seemed that the shielding was not adequate. This was largely the case almost everywhere they went. I know Dr. McKeel mentioned the nursing facility. That was a good example where they really didn't have adequate shielding.

The other part of course was the neutron issue. And my understanding of when you say the radium sources are bounding, you have already taken into consideration both the gamma and the neutron component of the betatrons. Is

that not correct?

DR. ANIGSTEIN: This is Bob. The 2008 report specifically modeled the 24 MeV betatron in the old betatron building and neutron doses to the operator are listed. And that has not changed. I mean we have had no reason to change that.

DR. McKEEL: Dr. Ziemer? Dr. Ziemer?

CHAIRMAN ZIEMER: Yes, go ahead.

DR. McKEEL: This is Dan McKeel again. Well, yes, they have modeled the neutron doses in 2008. That's interesting to me that the 2008 data is mentioned, because I've said for a long time that all these reports on summary doses by SC&A and NIOSH should also include that early data. It's almost like that data never actually was generated. It was generated. But again, this is modeled by MCNPX.

I have sent this Work Group three papers just as illustrations to support a point that I've been making all along; and that is that

when you formulate an MCNPX dosimetry model and you want to get that accepted in a respected scientific peer-reviewed journal, that number one, you aren't going to get that accepted unless you have validating measured data.

Now, I understand that the measured data is in some senses limited for the betatron installations that I mentioned in NYO-4699. However, it is the same kind of betatron that was used at GSI. They do give accompanying film badge data. They do give diagrams of the facilities. They do give wall thicknesses. And in fact in many of those instances they do mention what the wall thicknesses were. So it's my opinion that instead of people making off-the-cuff ad hoc comments on what those papers showed, that's the very reason that I think it's still highly important to have SC&A take NYO-4699 and to review that paper.

And I really think it's important for Dave Allen and NIOSH DCAS to take those papers and to explain, particularly for those

three sites -- Memorial Sloan-Kettering Hospital in New York City, the University of Illinois Medical School and the University of Illinois Research betatron -- and say exactly why you can or cannot use that as surrogate data to validate the MCNPX models at GSI. I think you can.

What you're relying on -- now let's remember, there is no data at all, real data except from that one radiographer for 1952 to 1962 -- '59. So you know, what everybody's relying on is AEC radiation limits. That's like saying that if you said how fast do drivers in the United States drive on the super highways? And you say, well, we've taken an average and, you know, the average speed limit is 65, but in some places it's 70, in other places it's 55. So we're going to say that 67 miles an hour, that's the average speed limit in the United States, and we're going to multiply that times the number of drivers per year, and that's the miles driven in the United States. That's the speed, the miles

per hour on average driven in the United States in one year.

Well, nobody would accept that, obviously. And yet you're trying to do the same thing. You're taking the statement of an individual at GSI who had many reasons to be favorable in his comments and accepted that as the gospel truth and set the limits for 10 years of this site. So I guess that's what I want to say about it.

CHAIRMAN ZIEMER: Okay. Thank you.

DR. McKEEL: And, Paul, I guess I've got to ask you again. I would like you to put on the record why it is, given my last comment, why you still do not think it's necessary to have SC&A review this very important paper? I will say one thing, too: you made an error when you said that that paper shows that the shielding was inadequate at most sites. As a matter of fact, if you read that paper carefully, it says overall that the accelerator radiation safety programs

in place are pretty good. And the main place that it has problems are some very specific sites in those things.

For instance, in one of the betatrons, they had a hot spot right next to the control room door. Well, in many of the other areas though they were below-the-limit reading. So actually that's not true to say that they found universal poor shielding. It wasn't true at all.

And when you characterize the study as decades ago, actually decades ago, it's '56, '57, right there in the period we're talking about, right there in the middle of the radium era at GSI. So that makes it even more relevant and it fulfills the Board criteria for surrogate data for being contemporary with the exposures at GSI.

So again, I am asking and I would appreciate an explanation of why you don't think SC&A should review this paper and why NIOSH shouldn't respond to it in a formal way in

writing.

CHAIRMAN ZIEMER: Well, I can't answer for NIOSH. I think I told you why I didn't think I should task SC&A to review the paper, but that if the Work Group wished them to do that, then we could certainly consider that. And in fact I've raised this issue about whether or not we can use that information, which if we could for the purposes of validating the use of the model, then that would certainly involve both NIOSH and SC&A doing this.

DR. McKEEL: Well, you've given your opinion that it doesn't need to be modeled, but you really haven't asked the other Members of the Work Group whether they think SC&A should be tasked to review these papers that I feel are of absolute paramount importance. So I'm not tromping you on your prerogative, but I still wish you would do that and let them put themselves on the record. If they say no, fine, they say no. But I don't think it's on the record.

CHAIRMAN ZIEMER: Okay. Certainly the other Members of the Work Group can comment on this. I certainly don't object to tasking if the Work Group wishes to do this. What I told Dr. McKeel originally was that I felt that all of the participants have copies of that information to evaluate as they proceed through with the other documents. It hasn't been our practice in general -- I'm not sure about the other Work Groups -- to do tasking outside of the tasking of the NIOSH work products, but we certainly have the other information. But, Work Group Members, you're certainly welcome to chime in on this.

MEMBER BEACH: Paul, this is Josie.

CHAIRMAN ZIEMER: Go ahead.

MEMBER BEACH: From what I understand here, NIOSH looked at it. Dave Allen said that there were some problems with him being able to look at that analysis because of missing points. And I haven't heard from SC&A. Is there any merit to taking the time to look at

this?

DR. MAURO: This is John. I don't hear anything from Bob. Bob, are you on line?

DR. ANIGSTEIN: Sorry, I was on mute.

DR. MAURO: Oh, okay.

DR. ANIGSTEIN: Yes, I have not had the time, I have not had a chance to review the papers that Dr. McKeel submitted. Right now from what I've heard from Dave Allen, if he says he doesn't feel that there are enough -- he's certainly familiar with what is required for an MCNP analysis, and if he thinks that there is not enough information and there's not enough specific data, I would be inclined to accept his opinion until I've had a chance to -- you know, until I find out otherwise.

Mostly having set up many, many, many MCNP analyses, both for the NIOSH project and other work, the limiting factor is always lack of information, lack of data on specific materials, densities, composition. And so the

MCNP becomes a model, I think a very accurate model. The MCNP Code has been amply -- it's been in use for something like on the order of close to 50 years. It's been amply validated in many, many, many field studies. I mean this is the Los Alamos National Laboratories, one of the foremost research institutions in the world when it comes to nuclear science. The MCNP Code has been used to design nuclear weapons, for better or for worse.

And, but the model -- I'm trying to get to -- I'm being a little roundabout -- is MCNP accurately models the information that is presented to it. If the information is inadequate, is not precise, is not what is really in the real world, then the model -- in other words, it's only as good as the input data. You know, there's a saying in computer talk: garbage in, garbage out. So if the data is not adequate, then all we can do, as Dave said, is take a guess and then it's a matter of luck, maybe. Maybe the MCNP results will confirm the measurements.

Maybe they won't. It will not say anything one way or another about the ability of MCNP. It only is a reflection on the input data.

So my off-hand opinion is that it's not likely that this will produce information that will be useful for GSI, especially since the limiting scenario, as we just discussed, in the SC&A analysis for exposure to betatron photon radiation is the layout man who's in an essentially unshielded location. And so it's a very, very simple model. He actually has line of sight, except for a thin sheet metal door, which is essentially transparent to high-energy photons, so you can say line of sight of the betatron target. So it's a very simple analysis.

We have a detailed drawing thanks to information that was obtained by NIOSH under contract from a former Allis-Chalmers engineer. We have a very detailed drawing of the platinum target. We have a fairly good idea of what the intensity of the beam was based on the

measurements on the X-ray beam, so we can back-calculate the electron beam. And therefore, the physics there is fairly straightforward and very, very well known.

So I don't think that there will be any value. There might be sort of a value in appearance. If we happen to come up within a reasonable fraction of the measurement data, that will be fine. And if we don't, it won't prove anything.

DR. McKEEL: Dr. Ziemer?

CHAIRMAN ZIEMER: Yes? Yes?

DR. McKEEL: Dr. Ziemer, can you hear me now? This is Dan McKeel.

CHAIRMAN ZIEMER: Yes. Yes, go ahead. Go ahead.

DR. McKEEL: All right. Well I have to just reply to that, as I have done many times before, but there is something a little bit new.

I've sent the Work Group by now three peer- reviewed scientific journal articles in

which MCNPX was used to model various aspects of radiation dosimetry. In all of those articles, just as I have stated, the model was tested against real measured data. We can call it empirical data. Whatever you want to call it. I call it measured data. In all of those instances the agreement between MCNPX and the measured data was 2 to 20 percent, plus or minus 2 to 20 percent.

In 2008, 2012 actually, NIOSH and SC&A were modeling the betatrons. Even though in some cases they were sharing input files to MCNPX, the closest they ever could come was 200 percent, twofold. And in many papers they said, well, the agreement is reasonable. No, that agreement is not reasonable. It's not good enough.

And we have another example. We have an example here of beta dose. You know, they're comparing beta skin doses now and they can't agree on what those doses should be. And then there are a number of instances, if you look

back over the last 17 or 18 Work Group and technical meetings where SC&A and NIOSH were sharing models, sharing files and their results didn't agree.

So I've just got to say that, you know, we've already found out that at least Dr. Anigstein did not have a chance to read this paper, so he's making comments about a paper that he has never read. And I can promise you from my 36 NIH grants where I was on different sides of the table, but the ones that I had where I was being grilled, if I'd given an answer like that to that review committee, my grant would have never gotten funded.

So I think that at the very least, you know, it's imperative that everybody read those papers and then in some fashion maybe they come to the Work Group meeting and present a review, a verbal review of NYO-4699.

I want to put this on the record to be very, very clear: I'm not a health physicist, that's true. I'm not a physicist,

but I am a physician and I have been at this now for a long time, since 2005 with GSI, and I think I understand a lot about betatrons. And I have contributed a lot of original information that this Work Group never would have gotten had I not put forth that effort. John Ramspott has contributed a lot as well.

I think at this stage of the proceedings with the crucial nature of those film badges and the fact that the NYO-4699 papers do excellent measurements of the neutron fluxes, that at the very least somebody on the Board, on SC&A, at NIOSH needs to review those papers and send us a review and say what they think of those papers. And as a matter of fact, they can critique my paper if they want to. If they think I've got it all wrong, fine, do that.

But my own opinion is I think this is a ridiculous argument that Dave Allen makes where he says that he doesn't have enough data to model the results achieved using MCNPX. Nobody said you have to use MCNPX to arrive at

data. You know, let's not forget NIOSH is now normalizing their external betatron data for the film badges, which even today, 10 months after the Board voted, they say the film badge data may not be any good. Well, if the film badge data wasn't any good, GSI should have gotten an SEC back in December.

So anyway, I just don't think that's okay. I mean what the NYO-4699 paper does do is it gives doses, you know, in millirems or rems per year. And one column in all of those tables gives the fraction of the total dose that's accounted for by neutrons.

Now, my feeling is I produced a paper that in my opinion NIOSH and SC&A, who have both been working on betatrons since 2005 -- they should have had those papers. You know, they are not new papers. They are on OSTI. OSTI is one of the main sources that NIOSH researches in just getting basic information together for their scientific papers. And they didn't get those papers. Or if they knew about them, they

didn't let on to this Work Group that they knew about them.

I asked Josh Kinman are they listed in the SRDB? And he said he couldn't find them, either one. Jim Neton was kind enough to send me the URL for the second, the original NYO- 4699 paper, and I put that URL in my paper.

So I believe there is data in those papers that is directly relevant to SC&A's dose calculations and to NIOSH dose calculations. And I'm not talking about modeling everything in MCNPX. The reason that you're modeling anything in MCNPX is because you don't have full or nearly full, or anything like full bioassay data. And as far as the film badges, you know, you have '63 to '66 of photons, period. No neutron data. No beta data. The film badges weren't read for that at GSI.

So I'm glad you all put this on the record. I think every person in that room, if they don't think these papers ought to be reviewed and the reviews put on the record, I

think is making a huge scientific blunder. And anyway, I won't take up any more time. That's just the way I feel about it and it's a very, very strong feeling.

CHAIRMAN ZIEMER: Okay. And that is so noted, Dan. We appreciate your input on that. In my mind if there were to be a formal review, it would be my impression that the responsibility would be to NIOSH as a starting point. In my mind I wouldn't be thinking about tasking SC&A to review this paper, per se. If it has importance, NIOSH needs to take a look at it. I think they have started to. I don't know whether it's of any value outside of the modeling.

Dan, I heard you sort of imply that it might be thought of in terms of surrogate data.

DR. McKEEL: Absolutely.

CHAIRMAN ZIEMER: Yes, and I don't know if that's a possible consideration. Certainly NIOSH is here at the table. They're

aware of the paper. They can certainly consider that, I would expect.

And, Jim and Dave, you certainly would want to take a cursory look and maybe a further look to see whether or not there is a possibility of additional usage of this. You apparently have looked at it from the point of view of the MCNP modeling, but is there any useful surrogate data there that would be of value as well?

But let me hear from the other Work Group Members.

MEMBER MUNN: This is Wanda. I have real reservations about the Work Group itself taking a position that we should be instructing either the Agency or our own contractor as to how they should pursue their investigations.

We are charged with the responsibility of seeing that adequate attention is being paid to the issues that are brought forward and I believe that we've

certainly devoted a reasonable amount of time, probably more than reasonable, to hearing the concerns and to hearing the exchanges between the parties involved with respect to issues that have been raised. Absent the group feeling that adequate attention is not being paid, then it appears very unwise for us to establish a precedent of telling any of the parties involved which material they should and should not be addressing. That's all I have to say.

CHAIRMAN ZIEMER: Okay. Josie? John? Any other comments?

MEMBER POSTON: Paul, I really don't have any substantive comments. I think, you know, before I would make any suggestion or having input I'd like to go back and reread the documents.

CHAIRMAN ZIEMER: Josie?

MEMBER BEACH: Yes, Paul, it's Josie. I agree with your thoughts on it, that it would have to come from NIOSH and possibly they would be willing to take a look at it and

address it and let us know as a Work Group, you know, how it would fit in for GSI.

DR. McKEEL: Paul, this is Dan McKeel again. Well, I would strongly endorse that. And what I was trying to point out is I wasn't -- I sent this paper in the beginning to NIOSH, you know, and I haven't heard a word back from them. I haven't even had the courtesy of them saying thank you for sending this interesting new paper; we'll look over it and appreciate your efforts. That's just common courtesy. In the scientific community not only is it common courtesy, it's de rigueur. You have to do that. That's just part of the process. And it wasn't done.

So I would say my request has been, is, still is today I would like them to take this paper and to consider it. But, you know, words alone -- for example, Dr. Poston, whom I deeply respect, he said he'd like to reread the paper. Well, you know, that's why I sent it to you in the middle of August so you'd have plenty of time

to read the paper. You assured me that everybody had read the paper. Bob Anigstein hadn't read the paper. So I have no confidence this morning who's read the paper.

CHAIRMAN ZIEMER: Dan, I don't think I assured you that everyone has read it. I said everyone had it available to read.

DR. McKEEL: Of course they did, but everybody --

CHAIRMAN ZIEMER: Yes, but I certainly didn't take a survey. I don't take a survey to see what people have read and haven't read. I can't assure they've read anything.

DR. McKEEL: I know, but that's the reason you told me that it didn't need to be tasked to SC&A. And that's your prerogative. But you didn't --

MEMBER POSTON: Well, it seems to me that --

DR. McKEEL: You didn't --

MEMBER POSTON: It seems to me that using the word read implies that I've read the

paper, I've had a lot of things I've read over the last few weeks that I couldn't tell you exactly what's in them or so forth. I'd have to go back and review them. And that's all I was suggesting that I do.

MR. RAMSPOTT: Dr. Ziemer?

CHAIRMAN ZIEMER: Yes.

MR. RAMSPOTT: Hey, this is John Ramspott.

CHAIRMAN ZIEMER: Yes, John.

MR. RAMSPOTT: Can I make a quick comment?

MR. RAMSPOTT: You bet.

MR. RAMSPOTT: I've been listening to this and I have read that paper, and I mean General Steel is not the only place with a betatron or a cyclotron or any of the equipment that's in this paper. I'm amazed someone had not found it before Dr. McKeel did. And I think I even asked in one of the meetings, and the transcripts would probably bear it, and I think Dr. McKeel asked it, too, is there a good

published paper that anybody knows of that actually tells what happens when a betatron does what it does at GSI? And I never heard anybody say yes.

So I think everybody owes Dr. McKeel a -- I mean I personally thank you. I've never seen this paper. And for people not to be willing to maybe take a little time with it when it probably applies to 80 or 90 percent of the sites that you people are reviewing daily is pretty amazing to me. This is the first good shred of information.

And I do know that if you go to the University of Illinois Research Lab, or Research Library like I did, you'd probably find those floor plans for those buildings that those betatrons were in. I have no doubt. I mean that's where the betatron was invented. I've seen documents about those betatrons, but I bet the floor plans are there if we looked a little harder. And it's a research lab, or a research library open to the public.

But the main thing is that TBD-6000, which we were talking about earlier, I don't even think it says anything about non-destructive testing. That I still find amazing. This looks like a good direction towards that issue, too. It seems like GSI is the only place, you know, a device like this is really investigated only because of Dr. McKeel and my efforts. And now it's been picked up by everybody.

So just a comment. I'm just amazed. Thank you.

CHAIRMAN ZIEMER: Okay. Thanks, John. I guess I want to hear from Jim Neton.

Jim, can you give us some indication of the feasibility of NIOSH taking a broad look at this from a point of view of possible use as surrogate data, or have you already looked at it from that point of view?

DR. NETON: I've looked at the paper as well, principally like Dave did, from the perspective of its utility for what Dr. McKeel would like to see as a validation of the

model. And I agree with Dave, I don't see any usefulness apparent in doing that because again of the lack of detailed specifications in those papers that would allow us to have to make a lot of assumptions.

With regard to the surrogate data issue, I'm not quite clear what would occur here, keeping in mind that the SC&A model using the MCNP is already assigning a nine rem exposure per year to the layout person based on the MCNP model. And personally I haven't looked at it exactly, but I cannot believe that there was anyone in those facilities receiving more than nine rem per year from betatron operations. And that has to do with shielding and that sort of thing. And I just don't see as a good fit for surrogate data. I'm not seeing it. I could be wrong. Someone could point to me a better use of it, but you know, surrogate data would have to be under the same conditions, the same shielding conditions just like we've talked about, and I don't see that.

DR. McKEEL: Dr. Ziemer, this is Dan McKeel. I must respond to that, please.

CHAIRMAN ZIEMER: Sure.

DR. McKEEL: All right. Well, let's see. So Dr. Neton says it has to be the same kind of data from the same source and so forth. This Work Group was perfectly willing to accept data from a cobalt-60 80-curie source used in the new betatron building in 1971, past the operations period at GSI, to use that as a model to predict external exposures to betatron operators, layout man, and things like that.

Now, everybody admits that a cobalt-60 source is not the same in many ways as a betatron for the same reasons. You know, yes, it has photons, very little neutron dose, an omnidirectional source. Betatron is highly focused in a tight beam.

And then the comments about assuming doses. As I read EEOICPA, the language is very explicit. They say that NIOSH has to be able to reconstruct the dose for every kind of cancer for

every worker that works in the facility. The concept of bounding is not really -- I've gone into this before with this group. I strongly disagree that you can point to, for example, doses from the new betatron and assume that that bounds the doses from the old betatron. You have to model that and then show that.

And I want to give an example of another thing from our part-time radiographer [Identifying information redacted] from the data that SC&A has analyzed extensively and put in their report. The same report that shows the 18 quarters of photon data that they extrapolate from 1963 back to 1958 has another entry in there, and it's called Pittsburgh Testing. And it says that this same individual, prior to his dose at GSI, got 7.2 rems in 2 quarters while working at Pittsburgh Testing Company.

So we've interviewed [Identifying information redacted] about that. Pittsburgh Testing is a company that does non-destructive

testing radiography work in many states. Its headquarters was headquartered in Pittsburgh. He did jobs while he was there. He said he exclusively used an iridium-192 source. So based on that testimony, which I have relayed as well, he got 7.2 rems from an iridium-192 source in 2 quarters. Now you can consider that as either an incident or just a really dangerous job where you get really high doses, you know? And I don't need to explain that 7.2 rems in 2 quarters extrapolated to 4 quarters is 14.4 rems for that year.

All I'm trying to say is the iridium-192 sources trivialize cobalt-60 sources as a source of significant exposure, trivialize neutron doses, everything trivialized. All of a sudden, radium-226 is it. And there's not one shred of measured data for the radium sources. There's not even good MCNPX data for the radium sources.

So I think that a lot of these a

priori items saying no value for MCNPX, that could be NIOSH's opinion. And not useful as surrogate data, that's scientifically absurd. And I use that word very carefully for everybody there. That's a really bad scientific statement to make. There couldn't be any better surrogate data. It's fulfills all the criteria just right off the bat. University of Illinois.

And again, all you have to do is read the paper which obviously everybody has not carefully done. And I would say this: I have taken the time to write a paper about NYO-4699 and I think you all owe it to me that you read my paper and include that in your analysis. So again, I'm not going to prolong this anymore. I'm sure you don't want to either.

I'm going to ask NIOSH and Jim Neton to look at that paper, please, with those two aspects in mind. Is it valueless as a validating tool when it's the only measured data available for betatron with film badge records and neutron data, and you can call that valueless

as a model validation, and it's valueless as surrogate data, I would like to see that in writing and the reasons why, as a scientist.

And I have to say this: and this may seem presumptuous, but I would say as a scientist with a curriculum vitae that probably matches all the people in that room, you know -- so on that level I think it is from one of your peers that's asking you to do this. And I'm asking you to do it today. And that's really all I do have to say. Thank you.

CHAIRMAN ZIEMER: Okay. Thank you, Dan. Okay. NIOSH has heard your request. As the Work Group Chair, I'm not going to demand that they do that, but they've heard the request.

I think one point Jim was making was that the proposed modelers would probably end up assigning a dose than assign a few years as surrogates, because in the most part -- and I think you've pointed out, Dan, for the most part the exposures would not be excessive. So they were probably much lower. I don't recall exact

numbers, but there were a few cases where they're higher. But in any event, NIOSH has heard your request and we'll let them proceed as they see fit.

DR. McKEEL: Well, can they give an answer as to what they're going to do? You know, Paul, one of the problems with this Work Group is you bring things up and then there's never a conclusive answer to them. I'd like to have an answer. Are they going to review the paper, yes or no?

DR. NETON: I will keep that under consideration, Dr. McKeel, and put out an answer, but right now my feeling is the use of an academic or a medical facility as a surrogate exposure model for a steel facility is not an appropriate comparison. That's my opinion, but we will issue an opinion on whether we're going to review it or not.

DR. McKEEL: Thank you.

CHAIRMAN ZIEMER: Okay. Thank you. It's now five minutes to 2:00. I don't

think we're even going to get into the Appendix BB matrix. We won't have time to do anything on that.

I'm going to prepare a report for the Board meeting which will simply be a summary of what we've covered today and what the deliverables are going to be. I'll put that in writing so there's no question on it. And then we will try to set up a Work Group meeting in the fairly near future, assuming the government is still in operation and we can do that.

MR. KATZ: So, Paul, this is Ted. Do you need any help from SC&A for your presentation?

CHAIRMAN ZIEMER: I don't think so.

MR. KATZ: Okay.

CHAIRMAN ZIEMER: I think I'll just summarize what we've covered here today.

MR. KATZ: Okay.

CHAIRMAN ZIEMER: Okay. Any other comments at this point?

DR. ANIGSTEIN: Yes, I have one.

This is Bob. I have one comment about Dr. McKeel's comments, and that is I have the same exposure history record that he was referring to for the radiographer who had worked at Pittsburgh Testing and his dose was simply assigned to him on the AEC record on the assumption that he got the maximum allowable dose. It was not a measured dose. The 7.5 rem for 2 quarters was simply --

DR. McKEEL: 7.2 rem.

DR. ANIGSTEIN: It says right here calculated at 3.75 rem per quarter, which is one quarter of the 15 rem maximum limit at that time. There was no measurement.

CHAIRMAN ZIEMER: Okay. There is no particular reason to debate that at this moment.

DR. ANIGSTEIN: Okay.

CHAIRMAN ZIEMER: But I'm going to adjourn here unless there's any pressing issue that we need to raise.

(No response.)

CHAIRMAN ZIEMER: If not, I thank you all for your participation. We'll see many of you in a week or so and then get information back on when the next meeting will be. Thank you very much.

MR. KATZ: Thanks, everybody.

(Whereupon, the above-entitled matter went off the record at 1:58 p.m.)