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

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ADVISORY BOARD ON RADIATION AND
WORKER HEALTH

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WORK GROUP ON TBD-6000

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TUESDAY
AUGUST 28, 2012

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The Work Group convened in the Zurich Room of the Cincinnati Airport Marriott, 2395 Progress Drive, Hebron, Kentucky, at 9:00 a.m., 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
ROBERT ANIGSTEIN, SC&A
PATRICIA JESKE*
JOSH KINMAN, DCAS*
JOHN MAURO, SC&A*
DAN McKEEL*
JIM NETON, DCAS
L. MICHAEL RAFKY, HHS*
JOHN RAMSPOTT*
WILLIAM THURBER, SC&A*

*Participating via telephone

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

(9:00 a.m.)

MR. KATZ: Good morning, everyone. This is the Advisory Board on Radiation and Worker Health, TBD-6000 Work Group.

Let's get started with roll call. We're dealing with a specific site, GSI, so also speak to conflict of interest. And we'll begin roll call with Board Members, with the Chair.

(Roll call.)

MR. KATZ: Okay. The agenda and related materials for the meeting that were received in time are posted on the NIOSH web site.

And, Paul, it's your agenda.

Please, for everyone on the line, mute your phones except when you're addressing the group. Press *6 to mute your phone, and press *6 again to take your phone off of mute. Thank you.

MS. JESKE: Excuse me. This is

Patricia Jeske. I was on mute and couldn't get it off on time. I'm the petitioner for GSI.

MR. KATZ: Oh, good to hear from you, Patricia. Welcome.

CHAIRMAN ZIEMER: Okay. Thank you, everyone, and I'll call the meeting officially to order.

The business today focuses on the issue that came before the full Board at its last meeting. And I will just remind you of what the recommendation of the Work Group was, and this recommendation was approved by the full Board and it is this: we recommended that the discussions relating to the residual period and the desire of the Work Group to confirm the appropriateness of the use of TBD-6000 model of uranium site facilities as a surrogate for the handling of uranium at General Steel Industries,

the Work Group recommends that the Board not take action on the SEC Petition

00105, but defer action until the next full Board meeting, and that this recommendation was based on the discussions that the Work Group had relating to the residual period and the desire to confirm the appropriateness of the use of the TBD-6000 model of uranium site facilities as surrogate for handling of the uranium at General Steel. And it was also indicated that this applies both to the operational period as well as to the residual period.

So, that recommendation was approved by the Board, and as a consequence of that, we are meeting today to focus on that issue and its implications.

Also, I'll just remind you that the issue of the surrogate data focuses primarily on the uranium contamination part of the dose reconstruction; that is, the internal dose component for the operational period. And of course for the residual period, the internal dose would be the primary issue of

concern.

So we have asked SC&A to review the NIOSH use of surrogate data in this instance and SC&A has done that. We also have a response from NIOSH on SC&A's recommendations and we also have comments from the petitioner relative to this issue.

So we can begin with SC&A. And I know you all have seen the paper that Bob has developed on behalf of SC&A.

And, Bob, you may want to highlight your findings and what SC&A's evaluation was.

Then we'll have an opportunity for NIOSH to respond and also for the petitioner to make input on this issue.

And just a slight delay here. I think we're getting some information up on the screen here. Stand by a minute.

Okay, Bob.

DR. ANIGSTEIN: Yes. All right. So as Dr. Ziemer pointed out --

CHAIRMAN ZIEMER: Can you speak loud, because the mics have to pick you up and the people on the phone need to hear you?

DR. ANIGSTEIN: Okay. I'll bring this maybe a little closer.

CHAIRMAN ZIEMER: Bob is fighting a cold. There might be a little difficulty here.

DR. ANIGSTEIN: Okay. My voice isn't quite as strong as usual.

All right. Well, first, in all fairness, the original Appendix BB came out in year 2007. The uranium dust loading in the air and deposition was based on surrogate data which was taken from the TBD-6000 which had been issued the year before, which was a review of about five different sites and described the work practices at these sites, sites that handled uranium metal.

Subsequent to that, the Board issued -- actually, it was SC&A drafted and the Board adopted a set of five criteria for the use of surrogate data. So in all

fairness, the initial use of the surrogate data was several years before these criteria were issued. Nevertheless, that is the standard by which we will be reviewing it.

And the Criterion 1 is the hierarchy of data. First I'll give you a summary and then I'll go through in detail how each criterion was evaluated. And one critique we have is that there was data. In 1993, there was a FUSRAP survey of the contamination of the floor of the old betatron building. It was a very detailed survey. And this was at the very end of the residual period, so it's contiguous to this period of operation. I mean, the entire period of evaluation starts in 1953, which is presumed to be the beginning of AEC operations at GSI, and it ends with the clean-up in, I think it was June, 1993, 40 years later. But anyone working during any of that period was potentially exposed to radioactive contamination/radiation and therefore subject

to have a dose reconstruction.

So there was one piece of data, one set of data that was not utilized. Furthermore, there were adjustments that were made to the surrogate data that was used that in our opinion were not appropriate for the uranium handling scenario with GSI, and I'll go into more detail.

Criterion 2 is the exclusivity constraint. One of the subsets in that criterion is that any use of surrogate data needs to be stringently justified, that's the wording in the criterion, and we feel that they were not stringently justified.

Criterion 3 deals with site or process similarities, and we find there were dissimilarities. First, a scenario that was adopted by NIOSH was the stamping of slugs produced by powder metallurgy. Now this is quite different than the physical form, the metallurgical form of the uranium that was actually handled at GSI which were either

recast ingots, meaning first they had cast them -- they called derbies, smaller shapes, and then they were melted and cast into ingots, or they were directly reduced dingots, uranium tetrafluoride which reacted with magnesium to directly form uranium metal. Neither of these is similar to making slugs with powder metallurgy.

The processes. In the one case numbers were stamped on the slugs, and that was a source of uranium dust. And that again had no similarity to what's going on at GSI.

Next there were -- we say that, yes, NIOSH did review data at four additional sites. We just got this review last week. We performed some cursory review of these data, but not -- when I'm talking about use of surrogate data, I'm talking about the original slug stamping scenario, not the new data that NIOSH has just presented.

And finally, there was not sufficient data about the surrogate site,

where were the stamping -- it was not identified where the slug stamping took place, and therefore we have no knowledge of that site. There was a -- earlier we had -- we were concerned about the temporal considerations, which is the time period of the measurement that was cited and the time period at GSI. And that has since -- we just needed a satisfactory explanation. And in fact, NIOSH has subsequently furnished such a satisfactory explanation, so that is no longer an issue.

The biggest issue is probably the plausibility of the model. We feel that the calculation of the surface contamination from the uranium aerosol, airborne uranium, was not scientifically plausible. And the statement, the assumption that the surface contamination resulted only from the slow deposition of aerosols did not conform to workplace plausibility. And we go on in detail.

Okay. Back to Criterion 1,

hierarchy of data. "Data should only be used" -- and I'm quoting. This is a quote. Everything in italics is quoted directly from the Board criteria. "Surrogate data should only be used if the surrogate data have some distinctive advantages over the available data, and then only after the appropriate adjustments have been made to reflect uncertainty inherent in their substitution."

Now normally to my mind, and this can be -- I would like verification from John Mauro on this, because he was the author of this. I would assume that the adjustment for certainty would mean an increase in the value. Well, we're not certain where it is, so we take the 95th percentile or some upper level.

Is that -- John, am I correct in my interpretation?

DR. MAURO: Yes, the main thrust is that, you know, you're never going to get a perfect surrogate. And once you recognize the differences between your real situation that

you're dealing with and the surrogate situation, if at all possible, try to make accommodations to deal with that, one of which, of course, you just pointed out, has to do with, let's work with the high-end values to be sure that we're not missing. But there may be other factors. But there's a lot of judgment made, and this is what makes the surrogate process difficult and, you know, when have you struck that balance? But anyway, yes, you're correct.

DR. ANIGSTEIN: Okay. Thank you. And then the other is that there is -- here are the data that were available. There was no monitoring. We all agree there was no monitoring of the uranium intake at GSI. The only hard data is the duration of uranium handling operations, which is based on the Mallinckrodt purchase orders.

The Mallinckrodt purchase orders said, we're going to pay you so much during this particular time period, say $500 or --

I'm just making it up -- one number that was cited -- for doing radiography during these three months and we're going to pay you at the rate of $16 an hour. So it's easy to estimate the hours, simply taking the total amount and dividing it by the hourly rate. So that is on firm ground.

However -- and then there were interviews with former workers, a number of interviews. NIOSH was involved. SC&A conducted additional ones to get an idea of the uranium handling operation. However, the data on the -- the very thorough survey of the old betatron building, the only place where uranium contamination was found, was not utilized by NIOSH, and that should have been part of the hierarchy of data.

Now the adjustment --

MEMBER POSTON: Before you go on, and maybe John would like to jump in, isn't what you've said so far really a subjective judgment? I mean, it's like arguing over

which baby is the cutest, I mean, in terms of --

DR. ANIGSTEIN: Well, unfortunately it is subjective, but the data, the site-specific data should take precedence over data that was borrowed from another site. And these site- specific data were not utilized by NIOSH.

MEMBER POSTON: I was talking about the appropriateness of the statement that you added to that.

DR. ANIGSTEIN: The appropriate -- well, I will get to -- oh, yes, let me get to it. That's on the next slide.

MEMBER POSTON: And the fact you talk about the uncertainty in here and then the substitution. I mean, those are subjective things as far as --

DR. ANIGSTEIN: Yes, but this is the Board criteria. This was adopted by the Board.

MEMBER POSTON: I understand.

DR. ANIGSTEIN: So I'm saying -- perhaps that might be clarified, because the point is the uncertainty -- this is what they said: There should be some adjustment for it, for uncertainty. However, the adjustment that was made by NIOSH was actually to take the values and substantially lower them. So that's not a question -- that's uncertainty -- usually my understanding is that when there is an uncertainty, you resolve it in a claimant- favorable manner, meaning if you're not sure of the dose, you either use the range or you give it some like 95th percentile of the range, some upper amount.

Here the value was actually substantially reduced over the measured value, the measured reported value. So that's why I'm questioning the appropriateness --

MEMBER POSTON: Okay.

DR. ANIGSTEIN: -- of the -- okay. So even though it's not given in the report, actually this does appear in TBD-5000, which

TBD-6000 refers to. And there is an equation, which I -- actually, I derived it separately from a statistics textbook, for the relationship between -- if you have a log normal distribution, there is a -- you can calculate the geometric mean and you can also calculate the arithmetic mean, and this equation gives you the relationship between the two. Anyone who has the earlier -- the actual report that was issued last month, there was an error had to be corrected, we left out the parentheses squared part.

So anyway, the geometric mean calculated by this formula using the arithmetic mean of 590, this was what was reported in the Harris and Kingsley report on the slug stamping scenario, but this form -- but then the geometric mean is only 162 if you assume a geometric standard deviation of 5, which again is -- you can call it surrogate data because its base is an assumption used by NIOSH, but it's certainly not relevant here

necessarily. There's no basis for applying it here. It's just a default assumption in TBD-6000.

So if you do that, however, because of an error in arithmetic in all of the TBD-6000 -- as a matter of fact, fortuitously I made the same error and I left out the square here and they got to 264 by leaving out the, you know, sigma, log sigma g squared. So that was an error and I believe NIOSH confirmed that that was an error.

So if we continue the calculation as described by NIOSH, this is a statement that I took -- I quoted from the recent NIOSH report, that "the use of the geometric mean is an attempt to prevent the value from being unrealistically high." Again, we don't agree that this is a valid adjustment. I just simply say we don't -- we think this value of 590 is too high, so we're going to calculate another value that's lower. And the problem we have with that is a single number was

reported. The only number that was reported was 590. Whether that's an average or a single measurement, we're not sure. More likely it was a single measurement, from my reading of the original source document.

However, what NIOSH assumed is, well, we're going to say this is an arithmetic mean. And because we tend to use log normal distributions, we'll derive a log normal distribution from that. Well, that's possibly defensible. But what is not defensible in our opinion is to then say we're going to use a single value of this artificial geometric mean that was from this -- derived from this geometric, this log normal -- the whole thing comes out of one number. You have one number and suddenly you're getting all of these values coming out of that. We feel that this is the number that you have. You have the 590. That's the number you use. If you need to use a value, you use the value that was reported.

And then the next adjustment that NIOSH made was to say that the operator was exposed only 75 percent of the time. Well, and the purpose of TBD-6000, if you're looking at an eight- hour exposure -- I'm not going to state an opinion, you know, state a position on that, but it's plausible. You know, the guy's not going to be at his station where the maximum concentration appears all of the time. He goes away. He does other duties, whatever. But that's like for an eight-hour day.

Here the operation was assumed by NIOSH to only take half the time. So if we calculate - - and I'm just making up a number, say one hour a month of this actual uranium handling based on the purchase orders, then they say, ah, but only half an hour is actually spent handling the uranium. The other half hour is spent in the control room while they're doing the radiographic exposure with the betatron. I don't necessarily agree

with that, but even then -- so you're already reducing it by half. You don't take 75 percent of that 50 percent. That seems again an unreasonable reduction in the exposure. This was done just to be consistent with TBD-6000, but it does not apply -- or we don't feel it applies here. So this is -- Criterion 1, we feel is not justified.

The exclusivity constraint of 2 is -- this would simply suggest that simply selected there were five scenarios in TBD-6000, and for Appendix BB, the one that produced the lowest concentration was selected because it is the lowest concentration. Again, that does not appear to be stringently justified, and I believe that I read the report that was recently issued where NIOSH agreed that it's not stringently justified.

The next is the site or process similarities. First of all, the uranium slugs produced by powder metallurgy simply do not have the same metallurgical properties as the

cast uranium ingots or the dingots. We have actually two -- these two forms are different, but they were both handled at -- they were both produced at Mallinckrodt, so we assume they were both handled at GSI, but neither of these conform to powder metallurgy.

Then the next part of this criterion is: are there other sources of surrogate data that were not used? So NIOSH has since reviewed the four additional types. I think actually that three sides were -- measurements were reported and a fourth one which was done for a temporal time curve.

Finally, are there adequate data characterizing the site -- assume the site that was selected, the site where the powder -- the slug sampling took place, to support its application? And the work site is not identified clearly. I think there was a suggestion it may have been Fernald, but it's not -- Harris and Kingsley list a number of operations and they list a number of sites,

but they don't say which operation was at which site.

And then finally, the criterion, do surrogate data reflect the type of operations and work practices at the facility in question, meaning GSI in our opinion? No, it does not. So we feel that criterion 3 is not satisfied.

Criterion 4, I'll skip over because that's been resolved. The dates are rather different. It was earlier, but NIOSH made the argument that since there were no particular measures taken to reduce the exposure to uranium dust at GSI, there is no reason to believe that that would be different and that's -- we agree that that's not a significant issue. I'm just mentioning it for the sake of completeness.

And finally, the one about the plausibility. Now I have to briefly go over the NIOSH model. The NIOSH model is that somebody comes into the room, let's say the

old betatron building, that's where we took measurements, I mean, it could be anyplace, and starts handling the uranium. Then the moment the time, the clock starts with the uranium handling, immediately a dust cloud appears. Some aerosol is generated and it starts at that moment. The uranium handling goes on for half an hour. It gets repeated periodically, but at any one time it's half an hour, again according to the scenario that's provided in Appendix BB. So for half an hour this dust is suspended in the air. It's inhaled by the workers performing this work. And most important, it settles to the ground, but it only settles to the ground for 30 minutes.

So if you know the concentration, this assumed concentration based on TBD-6000, and then you take the settling velocity of I believe it's 7.5 times 10 the minus 4 meters per second, then you can tell that during this time so much dust has settled. Basically it's

like a belt that the thickness of that belt would be the speed times the time. How far can a uranium particle travel during this time? And it's that and only that amount of uranium that settles to the floor. Because at the end of 30 minutes, poof, it's gone. So that's not a realistic scenario.

If we assume that this five-micron AMAD is the average -- the mean particle size and we agree that 7.5 times 10 to the minus 4 meters per second is the settling velocity, and if the dust was to extend to the roof of the old betatron building, it would take about 4 hours for it to settle. And this is not as absurd as it may seem, because, going back over some of the documents and worker recollection, there were heaters well high up on the wall which would have created an updraft with the space heaters, and they said there was an updraft -- so that there would have been some vertical movement of the air, so that the dust may very well have gone

certainly higher than a few feet, which would be the 30-minute settling time. So more dust would have settled to the floor than would have been calculated by that model.

And then more recently, in the latest report -- I'm not going to put up it right now, but there was a figure, a curve from Simonds Saw which shows how the air concentrations changed. And this is in the NIOSH response to our report. How the air concentration changed -- function of time. And it took days for it to go away, not half an hour. Because they showed a significant concentration after about two days later still, maybe one-third as much. And then in TBD-6000, there is a statement that it takes 30 days to achieve equilibrium between dust -- that's not during dust generation. But if there's dust on the floor and you stir it up, of course it gets resuspended. Then it starts settling again and there is a constant back and forth. It goes up, it goes down. And it

takes about 30 days to achieve equilibrium. So that's certainly not inconsistent with saying that it happens in 30 minutes.

And then the other problem with the model that we have is that the surficial contamination is based on deposition during one year. First it's the year, because at the very beginning there seems to be an increase in the frequency of radiography. Then there is a maximum year of July '61 to June '62. And so then NIOSH assumes whatever deposit during that year is constant for all time afterwards. No increase, even though there's additional uranium being handled and no decrease. So we just think this is not a scientifically-correct model. It needs to be -- I mean, the one year, if there is a basis for it, that this should be demonstrated.

Then the final --

MEMBER POSTON: Can I interrupt you just for a second?

DR. ANIGSTEIN: Yes.

MEMBER POSTON: I think you misspoke, and I want to clarify. You said it's not inconsistent with 30 minutes, and I don't think you meant that.

DR. ANIGSTEIN: It's inconsistent with the 30 minutes.

MEMBER POSTON: Okay.

DR. ANIGSTEIN: A double negative. Thank you.

MEMBER POSTON: Because that would mean, if it's not inconsistent --

DR. ANIGSTEIN: I thank you very much.

MEMBER POSTON: And you said that, I'm pretty sure.

DR. ANIGSTEIN: I'm sorry. Thank you for the correction.

And then finally, the workplace plausibility; because these are the two in the Criterion 5, is: are the assumed processes and procedures plausible for the facility in question? Have all the factors that could

significantly impact exposure been taken into account? Is adequate information available about the facility in order to make a fair assessment?

So again, we feel that the aerosol levels from the handling of uranium ingots are not comparable to uranium slug stamping. We also feel that the surface -- that even if we didn't have other problems with the model, the surficial concentration cannot be calculated from the airborne concentration because the settling of the aerosol is not the only source. You have these uranium ingots coming in. There would be loose contamination on the surface, contamination meaning -- it's not really contamination. It's just uranium, uranium oxide, and you could have large flakes falling down during the handling.

Now these flakes would not contribute to the inhalation of the worker that's doing the handling. However, they would contribute to the amount of uranium on

the floor. And immediately afterwards, that would not become airborne because it's strong. But here you have these little pieces of uranium lying around. Eventually they get ground underfoot. The forklift trucks go back and forth. We have a picture of trucks actually being used inside the extruding room. And this contributes to the resuspendable surface contamination layer. So again, we believe that this is not fully -- and then I show the 30 days required for equilibrium, which I mentioned before. So we feel that this plausibility, workplace plausibility criterion is not satisfied.

So the one alternate scenario before seeing the recent information from NIOSH is -- as we were directed, we looked at the other five scenarios in TBD-6000 and they also were not applicable to GSI. However, we did find there was the Adley report on the handling of uranium -- of the melt plant building at Hanford. And here you had a

series of operations which were simply handling uranium rods. No machining. No bending. Later on they were bent, they were straightened. But here was just the pure handling. And simply unloading the rods from the truck with a forklift gives you a far higher concentration.

This is the measured concentration now. So 3,900 dpm opposed to, what was it, 590 -- 560, I think, in TBD-6000. So we have 6 times as high and 20 times as high as the value that was actually used by NIOSH, 198. So this would be a more limiting scenario and simply unloading uranium rods. Well, that's probably as close as we could find. I'm not saying we did an exhaustive study of the literature. This is something that we just came up with. That was far more restrictive. So, and even if this was to be adopted, we would still have concerns about the model that was used.

So shall I go on now to the

alternate model or, any other discussion?

CHAIRMAN ZIEMER: Let's stop for a minute to see if there are any questions that the Work Group Members have on any of the items that you covered here so far. John?

MEMBER POSTON: Two questions: one, these are all measured values, you say? Was that what you said?

DR. ANIGSTEIN: Say again?

MEMBER POSTON: The values you're showing there on this table, are they all measured values?

DR. ANIGSTEIN: Yes.

CHAIRMAN ZIEMER: These are the Hanford values?

DR. ANIGSTEIN: Yes.

MEMBER POSTON: Yes.

DR. ANIGSTEIN: Yes, this is correct. Yes, that is in the Adley report. These are the measured values.

MEMBER POSTON: Can you help me understand, I'm assuming that the third entry

where the concentration is 88, these rods have been straightened and --

DR. ANIGSTEIN: Yes.

MEMBER POSTON: -- perhaps machined.

DR. ANIGSTEIN: I believe, yes.

MEMBER POSTON; So they have no surface?

DR. ANIGSTEIN: Right. That's probably the case.

MEMBER POSTON: Yes. But what's the difference between the first entry and the second entry?

DR. ANIGSTEIN: I can't tell you. This was simply -- it was not identified in the report.

MEMBER POSTON: Because they both say unloading the truck.

DR. ANIGSTEIN: One is unloading and the other is -- I think there were two different work locations. One is unloading the truck with a forklift and the other one is

I guess; I'm speculating now, the rods will be transferred with a forklift and to another location. And then there will be another monitoring at that location where they were received.

MEMBER POSTON: Would you speculate that --

DR. ANIGSTEIN: This is simply taken -- there was no further information there. This is many pages of tables in that report of many measurements during many operations and this was the one set of measurements --

MEMBER POSTON: Were these the maximum values?

DR. ANIGSTEIN: And I believe these were simply single values.

MEMBER POSTON: You didn't answer my question.

DR. ANIGSTEIN: Excuse me?

MEMBER POSTON: You said there were multiple tables with lots of data --

DR. ANIGSTEIN: Oh, oh --

MEMBER POSTON: And I asked you --

DR. ANIGSTEIN: No, no. I'm sorry. There were --

MEMBER POSTON: -- if these were the maximum values selected from those tables.

DR. ANIGSTEIN: No, these were selected on the basis of -- everything else involved melting uranium and handling -- you know, this was the one was selected as were simply handling cold uranium metal as opposed to uranium being heated, uranium being melted. So out of these numerous tables these would be the ones --

CHAIRMAN ZIEMER: You felt this was more like GSI?

DR. ANIGSTEIN: Pardon?

CHAIRMAN ZIEMER: This was more like GSI, is what you're saying?

DR. ANIGSTEIN: Right, this was the closest. My colleague Bill Thurber, who's not on the line, went through other sources

and found -- this was the one place he found that was the closest in this particular report. This is again one of the reports that was the basis for TBD-6000.

MEMBER BEACH: So this isn't something that SC&A came up with? This is one of the scenarios that NIOSH put out, correct?

DR. ANIGSTEIN: No, no, no.

MEMBER BEACH: No?

DR. ANIGSTEIN: No, this is SC&A. This is SC&A looking at alternative -- the original NIOSH scenario was simply the slug stamping, the uranium slugs that had been produced by powder metallurgy, where you simply took a powder and pressed it together under high pressure and temperature and then stamping numbers on it. That's the scenario that NIOSH took. We explored alternatives and we came up with one alternative that was both more -- a better surrogate and also more claimant- favorable.

CHAIRMAN ZIEMER: Other questions?

MR. ALLEN: I got one comment I would make. That Adley -- that report includes a map and then the area there. And I mentioned in my White Paper response to SC&A's thing is, one of the issues with trying to find this kind of information is that you have much higher airborne causing evolutions in the air.

CHAIRMAN ZIEMER: Other things going on.

MR. ALLEN: And there is a map in Adley that shows that this is a fairly small area. And as Bob shows -- or mentioned, there's quite a bit of airborne samples from other operations that are quite a bit higher than that. It's really impossible to say that this is the kind of airborne you get from cold metal uranium handling --

CHAIRMAN ZIEMER: Per se.

MR. ALLEN: Per se. I mean, it could very easily be interference from these even higher airborne samples 20 feet away.

And as far as temporal, whether this happened while the other stuff was going on, it's hard to say. I mean, that's the bottom line, is you really can't say. You don't know.

MEMBER BEACH: So it's just another best guess?

MR. ALLEN: Yes, but you -- in this particular case and in so many others, you have a very reasonable chance that there's a lot of interference from something happening in open air. And the Adley report does mention summertime, doors and windows open for ventilation. And you know there was, you know, higher airborne measured. And these are 20, 30, 40 feet away from higher airborne causing evolutions. It's really not a good set of data to use.

DR. ANIGSTEIN: Well, actually that's why we were suggesting, and I'll get to it, an alternative model which doesn't -- we realize that whatever surrogate data is out there, at least that NIOSH has come up with

that we've found, is probably not that good.

CHAIRMAN ZIEMER: John?

DR. ANIGSTEIN: And that's why we think that a model based only on site-specific data with just a couple of default parameters based on observations in many other places would be more defensible and claimant-favorable, more like a plausible upper -- I'm already jumping ahead.

CHAIRMAN ZIEMER: John, were you making a comment? John Mauro?

DR. MAURO: Yes, I was going to sort of step back a little, because I think a lot of information has been put on the table right now. It's always good to sort of pause and say, well, you know, what's all this saying, speaking to us?

I think that what we're hearing is the starting point for the surrogate model that NIOSH used, this, the slugs and the stamping goes back a long ways. At the time SC&A was supportive of it. I was the one who

said, yes, that's probably okay, because in all likelihood the handling of these ingots was not going to be as aggressive as the slugs. So, and we say, yes, that's probably going to bound it. And that was the end. That was the extent, especially at the time. All the attention, of course, was on the betatron and the radium. And the last thing in terms of the hierarchy of important issues and scenarios was this residual dust.

So we sort of started out accepting that. But, you know, as we focused in, we said, well, we'll take a little closer look. And I think the important message that comes out of what we just talked about was a starting point is the key. Starting with the dust loading of -- I think it was 550, was the number you cited that came out of the backup documents, the TBD-6000, Harris and Kingsley. And so I think the first place to look is, is that a good starting point, or are there better starting points if you're going to go

with the surrogate model? That's really the question.

And of course the conversation we're just hearing now is, well, there are other starting points that we might want to consider in light of everything we just talked about. Now, it's important to separate the starting point from then the mechanics. Given this starting point -- you heard a lot of discussion -- whatever starting point you decide to pick, if you decide to go with the surrogate approach, then there are the mechanics of it. And what do we do with that number?

And you heard a lot of criticism of the approach, the mechanics used in taking the single number and then, you know, assuming it's a geometric mean, then applying some factors to get -- or assuming it's an arithmetic mean, then applying some factors to get it to a geometric mean, and so on and so on and so on. And there's all this mechanical

treatment of that one number that we started with. I think that's a separate question. So it's important to separate are we happy with the starting point, you know, or are there better starting points?

And the other question is once we -- if we do find the place where, you know, we sort of like this starting point better, then the mechanics of how you treat it and what do you do with that number, I think that's a separate subject. And I think the -- you know, and it's important to separate those two, because in one case, if you can't find a good starting point you're really in trouble if you have no starting point. If you can, you've got a tractable problem. It's just a matter of now agreeing on the mechanics we're going to use, the mathematics, the assumptions in order to come up with a reasonable dust loading.

Now, we're about -- I'm only doing this sort of to set the table a little better.

If it's determined that you're not very happy with the starting point, Bob has come up with a whole different approach which comes in from a different direction entirely where you don't use surrogate data. But then the question becomes: what's the strength of that approach?

So in effect, there are two different strategies before the Work Group. One is to try to start with a surrogate point, the surrogate data as your starting point that you feel is reasonable and understanding all the strengths and limitations of it.

The other approach that you haven't heard from yet is, well, there's a whole different approach that could start -- your starting point, instead of being surrogate data, it could be some real data that we have in the 1990s of what was residual and then work with that.

The reason I only mention this is I like to sort of sometimes step back and collect everything up so that we can move

forward from the same perspective. I hope that's helpful.

CHAIRMAN ZIEMER: Yes. Okay. Thanks, John.

Why don't we go ahead and hear from NIOSH? And have before us their responses and their view on the extent to which the approach that they're proposing meets the criteria. Dave, you want to -- we have your document. You want to highlight some issues or amplify some things --

MR. ALLEN: Okay. Well, I mean --

CHAIRMAN ZIEMER: -- in response to what Bob has said?

MR. ALLEN; Well, I mean, I started that document off with trying to give a background -- this is handling cold uranium metal. This is nothing really specific to one particular site or one particular era. It is a very common task that's done in a lot of different areas. Unfortunately, when you go to all those sites where this was done,

there's not a lot of air samples for handling of uranium metal, and largely because it's not much of an airborne- causing evolution.

I worked myself at a uranium foundry for 14 years and I know for a fact you can handle uranium metal and move it around with a fork truck, et cetera, and there's just not much airborne caused by this. Unfortunately, like I said, it's difficult to show because, you know, a few air samples and everybody's convinced there's nothing and they don't take any more. So there's not a lot out there.

I pointed out some of the problems. In my White Paper I pointed out some of the problems, you know, number one, there's not a lot of air samples because of that. People concentrated on the more airborne-causing evolutions in the area. And, two, when they did take a sample or somebody unloading something or handling cold uranium metal often they were unloading it for a

reason, and that's for, you know, putting it in a furnace or some other manipulation that ends up causing quite a bit more airborne in the vicinity. Those two things cause a lot of trouble trying to find the information.

What we did with TBD-6000 and for GSI, we used that slug production. And one way of looking at this is, we used that slug production, which also included other evolutions with uranium or airborne-causing evolutions, and the table in Harris and Kingsley demonstrates other operations. Some were causing higher airborne and some were not. And part of that -- one step of that task included handling an ingot of uranium metal. And made the assumption that the other tasks were going to be higher airborne-causing evolutions and therefore took the median of the -- all the airborne in the vicinity.

Actually, it was the median of the higher one, which ended up being somewhat of a median of all those tasks and said that the

task of handling uranium metal is on the lower half of all the steps in this process. Therefore, we used the median as a constant. That's not a real mathematical or scientific argument there. It's, like I said, some means of trying to keep it from being implausibly high, which is yet another criterion we had to deal with. In my --

CHAIRMAN ZIEMER: So this is a judgment that says --

MR. ALLEN: It's a judgment.

CHAIRMAN ZIEMER: -- typically, as I understand it, typically in addition to any handling of the type done at GSI, most of the other data available is handling plus something else?

MR. ALLEN: Right.

CHAIRMAN ZIEMER: So the values that you have available would tend to overestimate the handling part because there are other operations. Is that what you're saying?

MR. ALLEN: Yes.

CHAIRMAN ZIEMER: Okay.

MR. ALLEN: So they took one operation that included handling, which all of them inherently include handling.

CHAIRMAN ZIEMER: Right.

MR. ALLEN: And decided that essentially the median -- it couldn't be -- the whole encompassing part of the cold uranium metal handling could not be as high as the median of the other -- the entire task, is what it amounts to.

In my White Paper response, I pointed out -- I went through a number of places looking for some air samples, most of which have some issues. Like I said, either don't take air samples or take them in the vicinity of other operations. And I pointed out about three -- I think it was three sites where we found some that were -- you know, it's not a great deal of information, but it's somewhat relevant to the conversation anyway

and gives you some idea of what I'm talking about.

The one that I thought was the most pertinent to GSI, that was only three samples, is the last one. It's the last page, I believe, of my White Paper where it's a BZ of an operator hooking a hoist to a billet and placing it on this machine. And it shows three air samples. One is nine dpm per cubic meter. The other two are non-detectable, so they're even less than the nine. That is the kind of airborne I was used to seeing from handling cold uranium metal.

I did have some other ones. They had their own issues where there was other operations going on, but they show still well below -- or at least below -- even the highest one is below what we were using in Appendix BB, but it does demonstrate uncertainty. There is uncertainty with what the number is because there was not a lot of samples, because the samples we do come up with end up

having operations in the area, you know, often, or more important, you'll know whether there's another operation going on.

So it is looking at these. And from past experience and from the logic I'm seeing with the slug production from TBD-6000, I think using that median as a constant seems to be a bounding approach, probably high, but there's enough uncertainty with the limited number of samples of where it's justified to use that kind of a number.

CHAIRMAN ZIEMER: Okay. Board, you have any comments or questions?

DR. ANIGSTEIN: I have two comments, and one is the -- I don't want to sound overly pedantic, but again, using the median as a constant there are two assumptions: one, there is an assumption that the single value that is reported is in fact an arithmetic mean which is a single value and that there is an underlying log normal distribution with a GSD, geometric standard

deviation, of five that contributes to that arithmetic mean and that you can then calculate the geometric mean, which also for the log normal distribution is the median, from that. These are simply not supported by the information.

I mean, this is just -- I don't mean to -- there is a fourfold decrease from the corrected value if you account for the arithmetic error. There is a fourfold change from the reported value to the value that would be used if that calculational error was corrected. And there just does not appear to be just a -- I mean, that should be -- making an argument, plausibility argument, well, we think that it should be lower because there are some other -- there are some additional mechanical agitation going on. So we think it should be lower.

Well, you can think it should be lower, but you cannot make a number out of that, with this kind of a manipulation,

without a sound scientific basis. We just don't feel that. I mean, I would say if anything, use the number and say, well, this is a bounding upper value because we really think it should be lower, but we're going to use this as an upper bound. So that's fine. You can use a number and then make a qualitative statement that this is a conservative number because here the following number should be lower. But not to then say we think it should be lower, so we're going to find some mathematical manipulation to just lower it to where we think it should be. Then you might as well pick the number out of a hat in the first place. And also --

CHAIRMAN ZIEMER: You feel it's still considered plausible if it includes --

DR. ANIGSTEIN: Say again?

CHAIRMAN ZIEMER: In your mind, do you still consider it to be a plausible number even if it includes --

DR. ANIGSTEIN: No.

CHAIRMAN ZIEMER: -- the --

DR. ANIGSTEIN: Pardon me?

CHAIRMAN ZIEMER: -- things beyond the handling? You're saying use the --

DR. ANIGSTEIN: No, I'm saying that the -- I'm arguing at several levels at once, which is a little difficult. It's a little bit difficult to maintain that position.

MEMBER POSTON: It's harder to understand.

DR. ANIGSTEIN: Pardon?

MEMBER POSTON: And harder to understand.

DR. ANIGSTEIN: Oh, okay. I'm sorry.

CHAIRMAN ZIEMER: Well, I think that SC&A is --

DR. ANIGSTEIN: But I mean they keep going back to -- first of all, the slug stamping scenario is not necessarily a good surrogate. Second of all, the way it's used

-- I guess the way I'm doing it because my charge was to compare it to the five criteria and one of the criteria is: were the appropriate adjustments made? And I feel that, no, the adjustments that were made were not appropriate.

And also, to jump ahead to something else that Dave said about the other sites that he looked at, one of the ones in his Figure 3, the Chambersburg Engineering -- it's a little hard to read. I dug out the original document. If you were to use the same philosophy that TBD-6000 used, and they picked, for instance, for the slug stamping, they did not the average, but they took -- there were like -- in Harris and Kingsley there were maybe five different values reported; I'm just going from my memory, and they picked the highest one.

Well, if you look at the Chambersburg and you pick -- and they give you high, low, and average for nine operations.

And if you pick the highest -- never mind the highest value, just the highest average value of those nine, it's 895 dpm per cubic meter. So that's not -- that belies, that contradicts that, oh, well, all of these other operations were lower. This is higher. The reported value is higher than the one, the slug stamping operation of 560, which is a single value, so it's taken to be the average. So here they give you the average and it's about one and a half times higher.

MR. ALLEN: Bob, that is for controlling the impactor. That's after it comes out of the furnace and is forged.

DR. ANIGSTEIN: Oh, okay. Impactor. Yes, and these are the inspector. No, I read the document, the discussion in the document. And they said, yes, this is interesting that the inspector gets a higher exposure than the actual workers.

MR. ALLEN: Well, I doubt that says inspector. I think it says impactor

operator.

DR. ANIGSTEIN: If you were to read the document, it says "inspector." I'm sorry, "impactor." I'm sorry, the inspector is somewhere else. I take it back. I stand corrected.

DR. MAURO: This is John. I'd like to jump in a minute.

I think that where we are is -- you know, we think that the starting point; that is the stamping of the slugs, when all is said and done, which launches your surrogate approach. The process we just went through, both SC&A and NIOSH, in asking ourselves the question: are there better surrogates out there? And I think that where we're coming out is, it certainly appears that there are. What I mean by better, that is other data that's out there as a starting point that would appear to be closer to the kinds of handling materials that are associated with GSI.

So it then becomes a matter of, among the suite of alterative starting points for a surrogate are there any that fall into place. And this is how I'm looking at it, that seem to become -- and this is where the judgment comes in -- that seem to be a more reasonable starting place -- starting point? And then once you're -- if you could come to that place. And that's a simple question. If you can't come to that place, well, the whole surrogate approach, you know, then comes in question. Notwithstanding the mechanics that follow; the geometric mean, the arithmetic mean, the occupancy times, et cetera, et cetera, you know, that all becomes moot if you can't find the starting point.

So I think it's important that now that we have a fairly nice collection of other starting points, some of which sound as if they're pretty close to the kinds of things that were done at GSI in terms of handling ingots or handling bars, that's really -- if

you can't get past that, we're really -- we can't make too much progress. But if we can find it, a place that people feel, well, this looks reasonable, then after that it becomes a matter of, okay, what do we do with that number next in order to make sure we're comfortable with it as it applies, as we go to use it at GSI.

And I think that it's so easy to get lost in bouncing back and forth between these two subjects. I think we got to get through the first one. If we take it through the first one, it doesn't even make sense to talk about the mechanics that follow.

DR. ANIGSTEIN: If I can add something to the question about the use of the Chambersburg data, I agree, I confused the inspector and the impactor. Nevertheless, what they handled at Chambersburg were half-inch by three- quarter-inch slugs. Little tiny pellets, half an inch diameter, three-quarters of an inch long. The total

quantity of uranium handled over a two-day period was 75 pounds. And the smallest piece, the slices, for instance, that they would have done at GSI would have been hundreds of pounds. If they had been whole ingots, that would be even higher. So these are not -- again, this is not a comparable operation.

CHAIRMAN ZIEMER: As far as handling is concerned is what you're saying?

DR. ANIGSTEIN: Yes.

CHAIRMAN ZIEMER: Yes.

DR. ANIGSTEIN: I mean, the material itself.

CHAIRMAN ZIEMER: Now my understanding, Dave, is that your selection of the geometric mean was focused on the plausibility issue that if you use the main -- or the tail end value, you would get an implausibly high number. Is that -- am I understanding correctly?

MR. ALLEN: Yes.

CHAIRMAN ZIEMER: And then we get

into this judgment issue of: what is it you select? Because that slug operation that you're using as a surrogate, which is the lowest in that hierarchy of possible applications in TBD- 6000, is still not just handling alone. It's handling plus something going on. And in the handling part, I mean, everybody agrees, is really at the very low end of all of this. And you're saying that if I use the slug thing, I'm implausibly high if I use the numbers as they are because they are handling plus something. Is that correct?

MR. ALLEN: Yes.

CHAIRMAN ZIEMER: Now, I suppose one could argue, but nonetheless they are still bounding. But then you have that plausibility issue which you're dealing with, and I sort of want to ask SC&A, because I started to ask you, Bob, do you consider it plausible to use the tail end value, the upper value of that? Or, John, if you want to chime in.

Or -- because it's not the stamping per se that is the issue. It's the handling. Now, the stamping thing is stamping plus handling. We're trying to get a handle on the handling part. Is there a way to get that? What would really make me uncomfortable is to say we know something is very low. And dose-wise we know that even if you use the implausibly high one, it's a very small fraction of the doses you assign from the external exposures. I don't recall the numbers.

MR. ALLEN: It would vary with different organs.

CHAIRMAN ZIEMER: Yes, it varies in different organs. But anyway, is there a way to get a handle on that and meet the criteria?

DR. ANIGSTEIN: Now, see, the additional problem is it's not just the airborne exposure during the handling. There is the accumulation of the uranium dust on the

floor or uranium chunks which become grown into dust and the resuspension. So what isn't mentioned here is also the fact that we disagree with the resuspension factor. I mentioned that earlier. Ten to the minus six is not a plausible number for an active area where people, whether it was foot traffic or vehicular traffic -- it should be much higher.

And we disagree with the model. Regardless of what number you use, the model of how the uranium concentration builds up. We think it could be higher. So the argument NIOSH made is, well, this is not an important contribution, the resuspension. Not according to their model. But we would argue that it is in fact, becomes an important contribution if it's treated in a different manner and in a way that we think is more defensible.

CHAIRMAN ZIEMER: Well, I don't think you're using 10 to minus 6 in the --

MR. ALLEN: Yes, we don't.

CHAIRMAN ZIEMER: For both

periods?

MR. ALLEN: For resuspension, yes.

CHAIRMAN ZIEMER: For the active period as well?

MR. ALLEN: Yes.

DR. MAURO: This is John. The plausibility, I think that that's causing difficulties for us all. When the plausibility clause, the last one, was added, the reason it came in; bear with me a little bit on this, is not for the reasons we're talking about now. It came in because there was a time when we were doing Texas City where there was an operation going on that in many respects was similar to what took place at Blockson in terms of using -- making uranium yellowcake.

And the reason the plausibility clause came in; and this came in late, was, Blockson was being used as a surrogate for Texas City. Blockson was handling thousands and thousands of pounds of uranium. And the

whole idea was, while Texas City only handled 300 pounds over a short period of time -- what I'm getting at was, plausibility did not relate to the model.

We're talking about -- Bob, you used the term "plausibility," that the model was not plausible. I just want to make -- let everyone understand, when we came up with the term "plausibility" was the site you picked, the operations that you picked were -- and when this originally happened was -- you really can't use Blockson as a surrogate for Texas City. Because the amount of material that was being handled at Blockson was perhaps a factor of a thousand times higher over a different time period and it was very unrelated and it would be inappropriate to just throw some big number like Blockson at Texas City. And it was not a plausible scenario that somehow Blockson could be used as surrogate for the Texas City because of the -- just the sheer quantity of material that

was handled at Blockson.

Now we're here today many years later and the plausibility clause is here. And I think that, Bob, you're referring to the model itself, the way in it's structured as being implausible. And I think that's true, but that wasn't really the intention. I think "implausible" means that you just can't use this particular facility as a surrogate for another facility, or that this exposure level of dust -- for example, you can't pick a level of dust that's very, very high, extraordinarily high and say it's bounding if it was implausible. And I think really that went to the heart of the whole plausibility issue.

You just can't pick some very, very big number and say that, well, it's so big we know it could never have reached that, and then you walk away and you say, okay, no SEC because you can bound it. Well, that was the real reason it wasn't plausible to assign

such a high number and walk away and say everything is fine.

I think it's inappropriate then to then extend the plausibility argument, at least as it was originally invented to, oh, you didn't model it the best way you could have modeled it. So I want to help out a little bit there. If we have a dispute over the model, I think we should discuss that, because that's solvable, you know, but -- and not refer to it as a plausibility issue. I think the plausibility issue is throwing some very, very big number and saying everything's fine. We got a big number we know is going to bound this, if it's impossible that that circumstance could have ever existed at GSI.

And I think the stamping operation itself is just not applicable. And it certainly sounds to me that a number of other operations have been identified that seem to be a lot better. They also have their weaknesses, but they certainly appear to be

better. And to try to force-fit the stamping operation onto GSI seems to be a stretch.

CHAIRMAN ZIEMER: Any comments?

MEMBER MUNN: Yes.

CHAIRMAN ZIEMER: Okay, Wanda?

MEMBER MUNN: This entire discussion is extremely difficult. And one of the reasons it's so difficult is because we are talking as if we are dealing with an extremely hazardous material for which any exposure is problematic.

In point of fact, the science surrounding uranium is quite well established. We know a lot about uranium. We know a lot about exposures that are subsequent to it. We know a lot about its suspend-ability. We know a lot about sizes of particulates that are necessary in order to create a biological hazard. And yet, we settle upon administrative issues surrounding processes that we've established here in the Board's deliberations at one time or another.

We're looking at details surrounding a possibility that is dependent directly upon an element we know a great deal about and that we can make legitimate statements about, supposedly without having to prove time and time again that something did not happen. It is -- I'm unsure how to focus this discussion on large known issues surrounding the processes we're talking about, the physical processes we're talking about as opposed to the details of whether models are correct, whether activities did or did not generate dust, what kind of dust, how much, was it resuspended, was it not, what happened after the picture was taken? These are debates that assume there is a significant hazard in handling this material.

And if we can't come to some reasonable assertion with respect to how to approach the larger questions rather than spending such a great amount of effort focusing on small issues, as we are right now

-- I recognize, the normal response of that is, well, the devil's in the details. Yes, that's true. One can't argue that. But we really are focusing here on whether or not we're dealing with models and minutiae, and I'm unsure how to get us off that.

But plausibility is something that is not limited to surrogate data. Plausibility is a matter that we dealt with at the Board level and not always satisfactorily to the perception of all. I'm not sure that we can ever do anything about that, but we have done a remarkable job of holding up each one of these issues to the light and looking at it. I don't know how much longer we can continue to debate it. That's all I have to say.

CHAIRMAN ZIEMER: Okay. Well, yes, thank you for those comments. Nonetheless, we're going to have to deal with --

MEMBER MUNN: No solution.

CHAIRMAN ZIEMER: -- the issue before us.

MEMBER MUNN: Yes.

CHAIRMAN ZIEMER: For example, on this first item I think we Work Group Members and eventually Board Members will have to decide whether they believe that the use of the stamping operation scenario or some other, if NIOSH wishes to propose something else, is a suitable surrogate. And you justified the surrogate in terms of using that versus some later data and back-extrapolating.

MEMBER BEACH: The 1993?

CHAIRMAN ZIEMER: The 1993 data and those kind of things. So the Board will need to decide whether or not that is a parameter, on the hierarchy of data, whether surrogate data is better in this case than actual data. That's on this --

MEMBER BEACH: Well, actual data from 40 years past.

CHAIRMAN ZIEMER: Right, 40 years

up the chain and --

MEMBER BEACH: And possible cleaning in between, so --

CHAIRMAN ZIEMER: Right. Right.

MEMBER BEACH: Yes.

MEMBER MUNN: You see, again --

DR. ANIGSTEIN: I guess, would this be a good time to -- because the alternative model is not just throw everything out and start from scratch. It shows -- it builds on the discussion we've had now. And I think I could bring this to a conclusion by showing how it compares to the other --

CHAIRMAN ZIEMER: Well, I'm concerned that we -- if the Advisory -- or if the Work Group here decides to recommend the use of the NIOSH approach, then that becomes a moot point, I suppose.

DR. ANIGSTEIN: I see. Okay.

DR. MAURO: This is John.

CHAIRMAN ZIEMER: Yes.

DR. MAURO: One thing, in working

with Bob along this and reading David's response, the light went on for me that said, you know, Bob's work, which we'll eventually get to, in a way could be looked at not as a replacement for, but as a validation. What I mean by that is we have this -- now we have a number of different starting points for surrogate data. We have the mechanics of processing that, modeling it, for better or worse, maybe improving on the model, and in the end coming up with some estimate with certainly a degree of uncertainty, for the airborne dust loading as a function of time using a surrogate data approach.

And I like the idea that, okay, Bob comes and says, you know, wait a minute, there's a whole other way to come at this thing. And I'm thinking that that's not a replacement for -- and, Bob, you may not agree with me, but it's -- let's look at it more as where do we come out if we come at the problem from this direction, and do we come within an

order of magnitude when we come in from the other direction from the range of values that we're looking at from the surrogate?

I almost see the two complementing each other, not being antagonistic to each other and I think that -- you know, so I've been looking at more as a whole picture and how does all this -- once you're done with all of this, something emerges from it and you start to get a degree of comfort that these independent lines of inquiry, different starting points, different assumptions. And then coming up with that -- do they all start to ring true and bring you to a central attractor in all this chaos that seems to say, yes, no, this is not a bad number?

Anyway, I wanted to preface that if we do move into Bob's work, because we could look at it from that perspective.

CHAIRMAN ZIEMER: Well, I guess I'd like to hear from NIOSH on the other criteria, too, and any comments you want to

make on any of those, Dave or Jim.

MR. ALLEN: Let me get to my report here and see what the --

CHAIRMAN ZIEMER: Well, on exclusivity constraints.

MR. ALLEN: Yes, exclusivity restrain -- constraints. If -- and correct me if I'm wrong, if I remember right, the main comment on that was that the use of surrogate data was not stringently justified in Appendix BB, and we agree. We didn't think that small of a source needed much justification, but it definitely didn't use the criteria that was developed after the fact. And any revision in the future does need to include more of a justification for that use, so I don't disagree with that. The framework for that justification is right now intended to be this White Paper.

As far as the site processes, we've pretty much discussed that before. There is no task of -- you know, process of

moving cold uranium metal. It is inherently included in any -- almost anything you do with uranium metal. And we can discuss it more if you want, but I think we've kind of discussed that one to death already.

The temporal considerations, I think Bob said he now agrees. And as I pointed out there, it's a physical property of uranium metal. It's not really site- or era-specific.

And then the plausibility again. That's -- you know, we've --

CHAIRMAN ZIEMER: We've been discussing that.

MR. ALLEN: We've been discussing that one, so --

MEMBER POSTON: Paul, I think --

CHAIRMAN ZIEMER: Yes, John?

MEMBER POSTON: I think we have to recognize that we've just been re-calibrated by Wanda. Because if you think about uranium, it's not the dose that really plays the role.

I know that's the important thing because that's what the legislation says, but we're talking about damage to the kidneys. That's the important thing. And that has nothing to do with radiation at all. And so we're arguing --

CHAIRMAN ZIEMER: Well, it's chemical hazard, right.

MEMBER POSTON: So we're arguing about insignificant kinds of things, if you really want to get down it, and not focusing on the details that we need to be. Because we know what happens when you ingest or inhale uranium. We know where it goes. We know what the organs are at risk, and those are the kidneys from the nephrotoxicity of the uranium, not from the radiation dose. So we're arguing about things that, in a lot of ways, are not important.

MEMBER MUNN: Because they're outside the scope of the --

MEMBER POSTON: They're outside the

--

MEMBER MUNN: Of our charter.

MEMBER POSTON: So it seems we ought to be able to resolve this thing.

CHAIRMAN ZIEMER: It's true, but nonetheless NIOSH, for example, has to do something.

MEMBER POSTON: Well --

CHAIRMAN ZIEMER: If there is an SEC, then it goes one way. If there's no SEC, they have to be able to reconstruct that part of the dose.

MEMBER POSTON: Yes.

CHAIRMAN ZIEMER: And --

MEMBER POSTON: Well, you cut me off --

CHAIRMAN ZIEMER: And --

MEMBER POSTON: I said we should be able to resolve this.

CHAIRMAN ZIEMER: Right.

MEMBER POSTON: We spent a lot of time discussing models and, I mean, the data

that Bob showed, if you look at the three values that he showed under different situations, they vary almost by a factor of 50 because they go from 88 to 3,900-and-something. So how do you -- I mean, are we going to just pick blindly a value or something? I mean, Dave's picked a value and tried to justify it based on his 20 years of experience. And I'm not willing to argue with him because I have no experience in terms of the rolling and dealing with uranium. And I have been in a couple plants, but just on tour. So I can't argue that he's not justified those numbers simply based on his experience. Experience has to play a role here. And I'll shut up now.

CHAIRMAN ZIEMER: Okay. Other comments?

MEMBER BEACH: But that's also why we have the criteria to verify what surrogate data is being used. I mean, that has to come into play, too, which is why we're having this

discussion.

MEMBER POSTON: I agree. I agree. I understand that.

CHAIRMAN ZIEMER: Okay. We need to -- yes, before we discuss further, we need to give the petitioner an opportunity to comment on the SC&A document and on Dave's document. We need a break first.

Dan? Are you there, Dan McKeel?

DR. McKEEL: Yes, sir, I'm here.

CHAIRMAN ZIEMER: Yes, I didn't realize the time. We're going to take a 15-minute break and then, if you would be prepared to address your issues on the surrogate data.

DR. McKEEL: That would be 20 minutes of nine? I mean --

CHAIRMAN ZIEMER: Yes, about 20 of. Yes. Yes.

DR. McKEEL: That would be 15 minutes from now?

CHAIRMAN ZIEMER: Yes.

MR. KATZ: And we have Patricia on the line, too. So she has the opportunity, too.

CHAIRMAN ZIEMER: Right. Sure. Sure. Okay. Thank you.

(Whereupon, at 10:26 a.m., the above- entitled matter went off the record and resumed at 10:41 a.m.)

MR. KATZ: The TBD-6000 Work Group is back from a short break.

CHAIRMAN ZIEMER: Okay. We want to have an opportunity to hear from the petitioners on the surrogate data issue. And actually both Patricia and Dan -- and John Ramspott of course, as a site expert, may also want to comment. But in the -- Dan, I know you -- we have a number of comments we've received in writing from you, and I think all of us have those, but why don't you go ahead and add whatever comments you want to at this time on the issue of the surrogate data?

DR. McKEEL: Paul, this is Dan

McKeel. I guess I've got to say this: the remarks that I've prepared today were on the materials that were sent to me, which is the 7/16 SC&A paper, the 7/25 alternate model paper.

CHAIRMAN ZIEMER: Right.

DR. McKEEL: And then I also sent a set of new data about the residual period.

CHAIRMAN ZIEMER: Right.

DR. McKEEL: And --

CHAIRMAN ZIEMER: I think we all have those.

DR. McKEEL: Yes. And really my comments address all of those. So I'll try not to dwell on the alternate model because I understand that that has not really been discussed in detail, but the comments I have overlap all the things that we were talking about this morning. And I do want to get before you all the new information about the residual period, because I think it has an important bearing on how dose reconstruction

is done.

So my overall conclusion about the surrogate data findings by SC&A are that I agree with SC&A. And I have said since 2008 actually that the slug facility does not meet the criteria. And basically it's amazing to me that this issue has arisen so late in the game, but I'm glad it did.

I also want to comment that it's my opinion that the motion to examine surrogate data use at GSI was really too limited. It's been my opinion, still is, that the models that are based on computer codes and not validated by real measured data, which is very scant at GSI, are themselves instances of surrogate data and they should have been scrutinized as such.

I heard this morning a lot of discussion of uncertainty and I just need to add again for the record that, yes, there's a tremendous amount of uncertainty, but statisticians have developed a very formal

discipline of uncertainty analysis that can be applied to a variety of situations just like the things we've been discussing again today. In order to do that and to make it a plausible analysis, you have to have real data to base it upon or you really can't define the uncertainty components. And my strong feeling is that there is not the data needed to define uncertainty by any technique at GSI.

Dr. Poston just pointed out that even using the grouped surrogate data from other sites that NIOSH has recently collected and that Dr. Poston -- I mean that Dr. Mauro mentions as, you know, much better than what was used in Appendix BB, even there, there is a very large range between the low and the high values.

The other thing I've go to say as a general comment to what's been talked about this morning is that I frankly as co-petitioner and speaking for the GSI work force, I'm shocked to hear uranium dose

considerations described by words like unimportant, trivial, that the science is well known, that the issues we're talking about this morning are insignificant, when in fact TBD-6000 is designed expressly to consider uranium doses at uranium metal facilities. So for this program that we're dealing with, EEOICPA 2000, uranium dose is central and certainly is not a side issue.

As far as the known-ness of information about uranium, my comment still is that that is true. And that being true, then a lot more certainty should have been achieved in the years we've taken discussing Appendix BB and SEC 00105 about exactly what the doses were at GSI from uranium exposure. And I would say that the -- Dave Allen's paper and his response, which I do want to comment about, ignores a lot of this hard data. In fact, it starts off ignoring the fact that, as he puts it, GSI dealt with uranium billets, when in fact they dealt with ingots, dingots,

a few billets and betatron slices.

Anyway, I have this comment about Dave Allen's response to the SC&A surrogate data findings: the first thing, which is extremely important for the Board in making their recommendation is that Dave Allen agrees that the slug facility uranium operations were not rigorously justified for comparability to GSI under Appendix BB, Rev 0. His argument, however, that this was acceptable because Appendix BB was issued before the surrogate data were -- criteria were ratified by the Board, I think is a -- that that analysis misses the point of what's going on here.

The NIOSH uranium intake model for 1953 to 1993 is getting its final assessment now as far as appropriateness of adhering to the Board's surrogate data criteria. That's in August of 2012, and that's a couple of years after the Board's surrogate data criteria were finalized. So I just think that Dave Allen's argument does not pass muster on

that point.

I want to make a comment about the SC&A new alternate intake model, and that is a point that I've made before, that while I understand that SC&A's job is to do evaluations, it seems to me that this clearly is development of a new model. And in spite of what John Mauro said, the intent of the new model is to replace the TBD-6000 NIOSH model to be used in surrogate data, and the paper actually says that.

The SC&A new model is supposedly based entirely on GSI data, but it's not. And Dr. Anigstein actually alluded to that this morning, saying that a few parameters were borrowed from the literature. I would add that a few of the parameters used in the alternate model were not defined as to what their source were. Basically, the SC&A model relies on differential equations and there are constants in those equations that need to be defined pretty precisely in order to even

evaluate the model that SC&A proposes.

Anyway, Dave Allen responded to both the surrogate data paper and to the alternate uranium intake model proposed by SC&A. And in that paper he dismissed the SC&A model in one sentence basically saying you cannot back- extrapolate data 40 years from 1993. And while I agree with that statement wholeheartedly, Mr. Allen, in saying this, also invalidates NIOSH's own June 2007 Appendix BB uranium intake model that relies partly on uranium activity associated with a small industrial vacuum sweeper that was back-extrapolated from the ORNL DOE cleanup under FUSRAP in 1993 at GSI.

The new information that I'm going to present to you all on the residual period years 1978 to 1993 provides very compelling evidence, I believe, that bounding the airborne uranium at GSI with sufficient accuracy for SEC 00105 Class members will be difficult or impossible. Dave Allen says the

new SC&A model has way more uncertainty than the surrogate data model that NIOSH uses from the slug factory. However, in saying that, he ignores SC&A's findings that use of the slug facility data has failed the five Board surrogate data criteria. And then he employs -- and I don't mean to be impolite, but this is a scientifically absurd argument and basically his argument was that if one has no or insufficient real intake data, as is definitely the case at GSI, then one can use inappropriate surrogate data that violates Board SD criteria to bound uranium intakes.

Mr. Allen also does not mention application of NIOSH's own surrogate data criteria in OCAS IG-004 to the slug facility, nor does he capture any differences between the NIOSH criteria and the Board surrogate data criteria, and I find that hard to understand.

Finally in his paper, Dave Allen does offer some new surrogate data sites that

could possibly be substituted for the slug facility used in TBD-6000, but it's interesting to me that two of those facilities make slugs which have just been ruled out as passing the Board criteria, and one of the sites deals with uranium billets. Neither one of the three sites actually dealt with uranium ingots and dingots which have a thick outer crust, which have magnesium fluoride coating them, which was subject to the Puzier effect that's not been mentioned.

And so there's a lot of incorrectness and inaccuracy in accepting anything except data measured from handling uranium ingots and dingots in the way they were handled at GSI along a long transport path with chains and cranes. And there was a lot of indefiniteness expressed this morning about uranium that's well known. For instance, Dr. Anigstein mentioned that ingots and dingots were heavier. Well, their weight is known. They weight 3,000 pounds apiece.

And so it required big chains, big thick chains and big cranes.

And they were placed on railway transfer cars which were never cleaned. So those cars were contaminated. The rail tracks that passed through building 6 and 7 along the foundry, through 8, 9 and 10, then went into railroad tracks into the new betatron building, railroad tracks branched outside and went into the old betatron building, that entire pathway was contaminated with uranium and none of that was measured. The only thing that ORNL and DOE measured in their cleanup at the end of the residual period was data from the new betatron building -- or they looked for uranium in the new betatron building. Didn't find any. And they found some residual uranium in the old betatron building.

So that brings us to the final point that I want to go over, and that is the new information that primarily Mr. Ramspott has developed about the GSI residual period

from 1978 to 1993. And I've gotten that together for you all and transmitted that to the Work Group and the full Board.

In brief, there were two known extensive cleanups, power pressure washings and rewiring and renovation campaigns, to the new betatron building.

The first was in August 1978 by Michigan Metals Processing, who had a three-year contract with National Steel. The new betatron facility was cleaned up, rewired and power washed by the Power Blasting Company in August of 1978 and used thereafter for offices and classrooms. The Michigan Metals Processing contract work also included cleaning up buildings 8, 9 and 10 during the three-year period.

And had ORNL, in my opinion, surveyed what they should surveyed, they would have been able to measure residual contamination in those buildings as well, but they didn't even try to do that. Apparently

none of the MMP workers wore protective clothing or respirators and an eyewitness states the subject of possible uranium contamination during this cleanup work was not mentioned.

There was also a power washing by the Power Blasting Company to the old betatron building interior in 1984. And it was illuminating to me, Mr. Ramspott has photographs before the power washing was done and during the 1990s cleanup by DOE, and interestingly in the early photographs you can see that the walls of the betatron building are painted white. And in the photograph from DOE in 1993 or thereabouts you see that the concrete walls have almost been entirely stripped of the white paint. So this was a powerful blasting operation and I'm sure that mightily disturbed the uranium that was in that building.

We believe a company named Affiliated Metals occupied former GSI building

6 during part of the residual period and continued the steel pickling operation that Michigan Metals Processing had initiated in 1978 through '81. Thus large areas of the former GSI building complex and both betatron facilities were extensively renovated, cleaned, power washed with sufficient force to strip paint from the walls, rewired, paneled in the new betatron building and re-purposed for classroom work and pickling operations. This must have created massive disturbance of the surface dust on floors, walls, ceilings and in air vent ducts.

It is difficult to imagine that this entire scenario could be modeled accurately both along the uranium transport and the NDT betatron paths even if you had monitoring data. However, during the same time period no workers were badged and there was no monitoring for uranium done except for the ORNL DOE FUSRAP survey of the old and new betatron buildings.

When they found some uranium in the old betatron facility, I think it's not surprising, knowing that all this was done, that they found no residual uranium in the new betatron building. The problem is there is no monitoring data, so you can't make conclusions about how the uranium levels varied during the residual period, and it would just be impossible to calculate the exposure to all the workers who worked at that plant from cessation of the AEC contract in 1966 through 1993.

So my conclusions are that NIOSH has no acceptable uranium intake model for GSI after three attempts. NIOSH rejects the SC&A July 25th, 2012 alternate intake model for uranium. Airborne uranium levels varied widely because of renovation and cleanup and re-purposing work in the GSI betatron buildings and buildings 6, 8, 9 and 10. Therefore we believe the conditions described cannot be modeled absent -- almost complete

absence of measured data at GSI.

And that would conclude what I have to say about everything that was put before me for this meeting.

CHAIRMAN ZIEMER: Okay. Thank you, Dan. I wonder if Patricia Jeske has any comments.

MS. JESKE: Actually I do, and this is news to Dr. McKeel and John Ramspott as well, because I just found out this morning. They are not sure about Affiliated Metals being there. I have a brother-in-law by the name of [identifying information redacted] that worked there at that time. I just got the information. And I don't know what building he was in, but he would walk through the tunnel just like my dad did at the old Commonwealth. So it was on State Street, and they had to wear gas masks and they could not take them off until they were outside the buildings. We don't know why, but we'll find out. That's really all I have to say right

now. Thanks so much.

CHAIRMAN ZIEMER: Thank you. And, John Ramspott, did you have any other comments? Where's John? If you're commenting, you're probably on mute. We're not hearing you.

MR. RAMSPOTT: I'm sorry. Yes, I was.

CHAIRMAN ZIEMER: Okay.

MR. RAMSPOTT: I was on mute. This is John Ramspott. The cleanup information that Dr. McKeel is referring to came to me from various sources, some of them known to this whole Board. One gentleman in 1978 worked at Michigan Metals just happened to tell me this situation by pure accident at an automobile dealership that I was at for service. Found out that he worked at the old Commonwealth or GSI plant in 1978-81. And he was actually one of the cleanup people, power hose guys or the water blasting operation. He personally did it, and he is definitely

available for an interview. He has no claim, no connection to this program until I happened to meet him a month or so ago.

And one of the issues he brought up was he did know another gentleman that did it there and that gentleman happened to be the son of the man we all know was the last employee at GSI who had told us this story. Because if we check transcripts I'm sure I said something about power washing in that betatron building probably four or five years ago. So this is not new news. This is just now totally again verified news.

The second cleanup that Dr. McKeel was talking about in the new and old betatron, after I'd heard the story from this guy, I recalled that an individual, of course known to us again -- he was a Dow worker who then went to school at Granite City to be retrained as the result of a work reorganization at Dow. While he was there getting ready to do the wiring, actually had to shut down that wiring

the first day and had the building totally power washed, cleaned. And he's known to everybody, too. And I'll be glad to share those names off-line. I know you don't like to have them now. But that's absolutely no problem.

So there's three totally different people from different directions all telling the same story and at different times. And what's really important about the times, these are all prior to the FUSRAP cleanup. So what FUSRAP walked in and saw was definitely 100 percent not what was there during the contract period and the residual period.

And if I recall correctly, at our last meeting, and I think it was Dr. Ziemer along with others that pretty much agreed, I think Mr. Neton, you can't separate the contract period from the residual period when it comes to certain things. You just can't separate them when you're going to try and talk about surrogate and slug usage.

So these cleanups all have to do with the contract period as well as the -- you know, we're talking about a little shop vac vacuum with some residual in it and trying to use that as the basis. Something that dawned on me yesterday, we don't even know when that vacuum was put in there. We don't know if Michigan Metals put it in there. Did Affiliated put it in there? Who put that vacuum in there? There's no mention of it in the GSI auction listing in '74.

Now, I'm not saying there wasn't probably some sort of cleaning mechanism in there before because GSI workers told me how they just about slipped and broke their neck on little pellets and BBs and dust in the betatron building. So we don't have good information on the maybe vacuum sweeper. So, you know, that's just -- this cleanup thing really concerns me. And it's all unprompted from individuals from totally different walks of life. Thank you for your interest.

CHAIRMAN ZIEMER: Okay. Thanks, John, for your input on that.

Okay. Now, Board Members, we need to now consider some alternatives as well. We did sort of commit to SC&A that we would listen to some ideas that emerged out of their initial study. And I do want to go on record as pointing out that neither Ted nor I tasked in advanced SC&A to come up with an alternate model. That alternate model, we were informed of that when they had completed their preliminary review that they had considered some other ways of looking at the surrogate data issue. And in the process had -- I don't know if you'd say developed, but at least thought about an alternate way of doing things.

They were never tasked to come up with an alternate model. I just want to make sure on the record that that's clear because I think that there are some that think that somehow we had tasked them to do that. And

this alternate model I don't think has been fully developed, but it's an idea that apparently emerged out of their analysis of the surrogate data issue. And I don't know to what extent it has even morphed from its original version. It sounded like from what John Mauro was saying earlier on the phone that perhaps they're thinking of it more as a different version of the surrogate data approach.

But in any event, Bob, do you want to kick this off and tell us what your thinking was on that?

DR. ANIGSTEIN: Okay. Thank you, Paul. Well said. Just what happened was as we were reviewing, I was reviewing the surrogate data as we were tasked to do, I started thinking, it seems like there was some -- there should be another way of approaching this that would solve some of the problems using some of the -- using all the information we knew. There seemed to be something that

was being overlooked. And the something that was overlooked was the recently adopted OTIB-70 which gives default assumptions -- recommends default assumptions regarding resuspension rate -- is why we first looked at it. And also about a decay rate, or a removal rate, should be said, not to confuse it with radioactive decay.

And they recommended a removal rate. And the removal rate also has to be consistent with the resuspension rate. So here you have two numbers that are linked together. And the fact that there was this detailed -- because I don't believe there was any drastic changes made in the old betatron. It was well explained why there was no residual contaminated powder in the new betatron building because it had been cleaned up prior to the FUSRAP. But the old betatron building more than likely remained -- I mean, if there was any cleanup, it would only -- right. It seemed to me -- what I'm giving you

a preview was, we have found something that's internally consistent.

So the -- oh, I'm sorry. Wrong slide. Okay. So here's a little picture I found. I didn't create it entirely. I modified it somewhat. Here's what happens in the typical -- with this -- this whole thing, it's very easy to talk about resuspension factor, re- deposition. What does it really mean?

So here you have a little house, a cartoon of a house and you have something coming in from the outside. Doesn't matter what it is. So it comes in by infiltration through the walls, tracking, which is really not relevant here. Might be somebody literally picks it up on their feet and tracks it in like somebody tracking mud into the house. So here's this contaminant. Now also there may be some coming from outside. There may be some generation which is really relevant here.

Some material is being -- airborne material is being generated. And this stuff gradually falls by deposition. And then you have resuspension, which is two-way actually. I added the other arrow to show it's going down and it's coming up again. Under equilibrium and absent anything else, these two would be the same. And then you have cleaning, ordinary housekeeping and exfiltration. You have a little bit on the person coming in. So this is how any material can accumulate inside the structure.

And to model it, we first go back and look at what comes in. Well, here are the uranium handling times based on the Mallinckrodt purchase orders. This is similar to what you find in Appendix BB, except maybe a couple of changes. They made an assumption that one of the later periods was -- they extrapolated one of the later periods back to the beginning. Since there are no purchase orders from 1953 until end of February 1958,

the conservative assumption is to assume that it's the highest rate of any of the years for which we do have purchase orders.

We take that the handling -- we just take the number of hours in this -- any one of these periods and take the number of hours that Mallinckrodt was paying for. Divide one by the other and you get a percentage. So it ranks a maximum of almost 5 percent and a minimum of 0.15 percent at the very end when there wasn't much going on.

And here's the mathematical formulation. I've just -- it can be -- it's not as -- it's really simpler than it looks. This is simply the rate of change. This is a generic equation. This is the rate of change of the contamination level on the floor. And this is -- and there is some removal fraction; I called it the Greek letter mu, which is proportional to the rate of -- to what is on the floor, that some fraction is being removed.

And then there's a deposition that is a constant hourly rate or daily rate, just to keep the units consistent, which we don't know yet, but multiplied by the fraction of time. Well, the fraction of time is simply these fractions that are here for each time period. So this is different. So this is the sigma sub i. There's a number of time periods. Basically these 3, 6, 9, 11 time periods. So it's different for each time period, but we model each one separately.

And this is the removal rate of 6.7 times 10 to the minus 4 per day in OTIB-70 that's recommended as the default removal rate, and it's based on several places where these measurements were made. And I don't have it in front of me, but they don't span that big a range. It's a pretty robust number because the numbers maybe change by a factor of two between the highest and the lowest. I'm just guessing now by memory.

So then when we do the

mathematical manipulation of this to solve the equation, you come up with an expression for the contamination due to each of these time periods. Now so they're modeled separately and then we add them together. And this is the actual surface activity on the floor at any given time due to summing the contribution from all of these individual time periods.

Now you notice I said nothing here about how many -- or what is -- what fraction of the time it's actually being handled. Because of this here, we know -- this is the fraction of all -- this is the total handling, and NIOSH assumed 50 percent. We don't need to know that because we do know that it's simply -- this is how it varies from one year to another, which is all you really need to know for the purpose of this model.

Then you need some real data. Okay. This is the drawing of the old betatron building. And they made measurements, two sets of measurements. The red -- I'm just

giving for matter of interest. This is the biased sample. This is where they went with a meter and said, wait a second, there's a hot spot here. And then they take their detector that will measure the alpha activity and make a measurement at that spot. So these are biased samples. They're deliberately looking for the high spots.

Then separately from that they took an unbiased. There is a procedure called MARSSIM. It stands for manual -- unfortunately I can't remember the exact acronym. It's a basic guide used by all the government agencies that contributed. NRC, DOE, EPA contributed to it. When you go in and you want to clean up a facility, how do you sample to see what the average activity level is? And there is a way of selecting locations. And these are the blue. These are the randomly selected locations that are designed to give you a cross-section, a representative picture.

You can't sample each and every spot, because we're talking about something with about three -- a little disc about three-and- a-half-inch radius. And based on these random samples, most of them albeit were below minimum detectable activity. Minimum detectable activity was assumed -- was 50 dpm/100 cm2. The reason they use 100 square centimeters is because that happens to be the active area of the detector, 100 square centimeters. So it's how many dpm on a detector.

MEMBER BEACH: Bob?

DR. ANIGSTEIN: So by standard procedure we assume that it's half.

MEMBER BEACH: Excuse me.

DR. ANIGSTEIN: Yes?

MEMBER BEACH: Would you remind what date those samples were taken?

DR. ANIGSTEIN: Yes, these measurements were made in I believe June 1993. That's by definition the end of the residual

period because that's when they cleaned it up.

MEMBER BEACH: Thank you.

DR. ANIGSTEIN: However, they did get a number of real measurements. So if we take the MDAs and say, well, 50 is the least they could detect, so we'll just assume that it's half that; it's a pretty standard procedure, and we assign it to 25 dpm/100 cm2, then I convert it here to becquerels per square meter to be consistent with the model, it comes out to 43.6. You know, most of them were based on if we just took the ones where we had meaningful data, it would not be a very robust model.

So taking that, we can actually calculate the hourly rate of accumulation during the period of uranium handling operation during the intervals. And it comes out to -- knowing what this term is solving the equations, that it was June 7th, 1993; to answer your question, Josie, we find that the average -- that the measured was 43.6

becquerel per square meter. We find a deposition rate of about 1,200 becquerel per square meter per day. We prorated to a day. The actual operations take less than a day. So you could divide by 24 and have an hourly rate.

And so now we know the rate at which it had to be deposited. And the only two things that go into this, the only two assumptions is that removal rate of 6.7 times 10 to the minus 4 per day; that's one datum, and the other one is the 43.6 average activity at the end of the residual period. And with these two things you say this had to be, given the pattern of activity -- so much in one year, so much in another year, increasing, decreasing, all of these add up to a single number.

And now with this number, we can say what would be in the air due to resuspension. Well, we know that 1 times 10 to the minus 5 is a good resuspension factor.

There is a huge body of literature on resuspension factors. They vary a great deal. Ten to the minus six, which was used by NIOSH, was adopted by NRC for a facility that has already been cleaned up. It's a decommissioned facility that has been cleaned up to the extent practicable using the ALARA, as low as reasonably achievable, and they say that after it's been cleaned there's still going to be some residual on the floor and there will be some possibility of resuspension. And they picked 10 to the minus 6 as a good number.

But that's something -- the concept is important. There is very little resuspend- able material left. It's all, like, hardened. Everything that's easily resuspend-able -- because also the same could be swept up, vacuumed, washed off. So that's a number -- that's for an inactive facility. A facility where the stuff is being deposited every day or every few days, that's not a good

value.

So 10 to the minus 5 seemed like a good compromise value, because, true, now some of the activity will be old because it's depositing year by year and there's some removal -- some unspecified removal mechanism. So it seems like a good, reasonable number. And I'll go a little further and demonstrate why it's a good -- it's not just our opinion.

And then there is a second source of activity, which is of course what happens -- this takes place all the time, you know, every day of the year. This takes place only during the uranium handling period. And now that we know what we just derived are the rate of the accumulation, you just divide by the velocity, this average velocity of 10 to the minus -- 7.5 times 10 to the minus 4 and you can calculate what would be the activity due to the uranium handling. So during this time the actual activity in the area is the sum of these two. But in between the uranium

handling, you get only this.

And then finally, what do we end up -- what's -- basically what's the bottom line? What is the intake? Well, we find that year by year -- we just did this in case -- just to show that this can be used for dose reconstruction. At the very beginning, at the first year there's very little resuspension because nothing has accumulated yet, not very much. So all the activity, almost all the activity is due to uranium handling as year after year, it accumulates on the floor. And now the resuspension becomes a bigger factor up until you get to about 1963 where it's the dominant factor because there is much less handling.

And so how we come up against NIOSH, here is Appendix BB, the total dpm per calendar day, and we come up with more than 10 times that during the very busy time up through about 1963. And then we start falling off. And then during the far part of the

residual period -- in the original report, it's tabulated every year. I condensed it here so it fits into one slide. So you start -- you end up by 1988 -- we actually end up with lower simply because NIOSH does not have this decline. They have it constant and we have the decreasing year by year.

Now the -- why does this make -- why is this plausible? First we compare what would be the derived, that is before the -- during the actually handling operation? We calculate 11 -- by the way, we -- excuse me. Forget that. So we have 1,100 dpm per cubic meter. How does this compare with the measured value? Well, it's less than the unloading from the truck at Adley.

But if you just take these three values and take a geometric mean, that makes sense because it's -- you know, when they're so different, it makes more sense to do a geometric mean rather than an arithmetic mean, 563, well, within a factor of 2. For the

model starting out from that, not bad. And it's not that different, by the way, from the slug stamping operation, which also is somewhere around 560. So we're within the right ballpark.

Then next: a verification. This is not to -- this is only a verification calculation. What kind of an air exchange would you need if you were to assume a resuspension factor of 10 to the minus 5 and if you were to use the fractional removal rate of 6.7 times 10 to the minus 4 per day, and you were to assume no other removal; no cleaning, no washing, that the only removal was that the dust accumulation on the floor gets suspended in the air and the building ventilation takes it out? And you would get -- you would need an air exchange rate of approximately one-quarter of the volume per hour.

For a large building that's not a bad estimate because the larger the building,

typically the lower the air exchange rate is simply because the air exchange rate depends on the wall area; the windows, the roof, the wall area. Now the surface -- the ratio of the surface to volume gets small with large buildings. So therefore, for a very large building, less than one-quarter of an exchange per hour is not unreasonable. A more common one is one per hour, but it would be for a smaller building. There have been buildings where as little as one-tenth of a volume per hour has been measured. Actual measurements.

So this looks -- all we can say is, you know, are these numbers -- you know, I'm shooting myself in the foot now, you know, are these numbers exact? No. It's not possible to have an exact number, but these are plausible upper bounds. And they're based on documented data, so it's not just something we like this -- we pull this number up out of a hat because we like it.

And we think that this is a

reasonable way of approaching the problem which would give -- which would be bounding and yet not implausible. It gives you -- say this 1100 dpm per cubic meter for natural uranium corresponds -- I did the calculation -- to less than a milligram per cubic meter. That is not an unreasonable dose concentration for indoor air. They're on the order of micrograms up into the hundreds. This is already in the high end. It's not -- it doesn't get to the point where we had like with the Bethlehem Steel. I know we actually did calculations. It gets to a point you can't even see across the room it would be so thick.

So I'm just suggesting that this is a plausible model which is not dependent except for the air exchange, for the removal rate is not dependent on external calculations. That's it.

CHAIRMAN ZIEMER: Bob, what is the implications of the cleaning that Dr. McKeel

mentioned on this particular --

DR. ANIGSTEIN: I don't believe it's on the -- I believe they were referring to the new building, to the new betatron building. All of that -- I read over the material.

CHAIRMAN ZIEMER: I thought that they were all cleaned. Is that not correct?

MR. RAMSPOTT: Dr. Ziemer?

CHAIRMAN ZIEMER: Yes.

MR. RAMSPOTT: This is John Ramspott. Since I'm kind of the cleaning expert; the gentleman told me I could use his name, [identifying information redacted] from Dow was there in '84 and saw the cleaning of the old betatron building. He was also there in the same time frame and saw the cleaning of the new betatron building. The old betatron building, in looking -- well, Dr. Anigstein, his drawings up there, if you look at the one with the red dots, blue dots, it's pretty interesting. Take a look at that, if

you would. That's from the cleanup material, the FUSRAP cleanup material. If you notice that berm that's shown on there --

CHAIRMAN ZIEMER: Yes.

MR. RAMSPOTT: -- that wasn't there during the GSI dates. That came in after the fact, after that plant closed because they stored leaky electronic transformers in there. That's in the FUSRAP report. I kind of find it amazing there's nothing in there. Well, if I was there in '93 and doing a cleanup for FUSRAP, I probably wouldn't want to climb in that PCB-contaminated oil that they built. And the berm was about an 18-inch wall. We have pictures of it from a DOE cleanup, some really good color photographs that was provided to myself and Dr. McKeel from the Department of Energy. That area, 25 percent of the shooting vault area, and it's not touched. No one's looked at it.

Then the other series of red dots

over in the right-hand corner, I think from the FUSRAP drawings that's where the vacuum cleaner was. I kind of find it amazing that there's nothing in between that and the berm. That's where the guys worked. That berm is right in the work space. That's where according to FOIA drawings a lot of work was done. If they had a source, that's where they worked. That's where the betatron crane would hang. And there's nothing there. I find it kind of unusual because they wouldn't be examining those dingots on top of the vacuum sweeper. I guarantee you that.

And then the other series of red dots on the railroad tracks, I find that kind of amazing too because the betatron doesn't go there. Those red dots got there somehow. And more amazing is they're in between the control room and the vault and the control room is shown as totally clean. Now if the guys walked through those red dots, I mean, every day, every moment into the control room, you'd

think there would be something in the control room.

Now that building definitely, according to [identifying information redacted], was cleaned. And we had heard the story about the power cleaning from another gentleman, [identifying information redacted], [identifying information redacted] is his nickname, who was the last guy at GSI. It was his son who did that cleaning with this other individual I happened to meet at the car dealer.

So that building -- Dr. Anigstein, that building definitely was, according to [identifying information redacted], cleaned as well. And he explained the cleaning. [identifying information redacted] said they cut power to all the buildings. [identifying information redacted] was an electrician. They cut the power completely so they could squirt or power blast the roof, the walls -- or the ceiling, the walls, the floors. And

that -- now there was a major, major disheveling of the interior of that building.

And also from FUSRAP they show two betatrons sitting on the floor of that building. Those things weigh 4,000 pounds apiece. You don't just flip them around. I'm sure the second one that was in there came from the betatron building. We know that now. But there was only betatron in this old betatron building originally. You'd have to bring that in with a forklift, high lift, something. That floor had been -- that floor was definitely not what it was when GSI workers were working there during the contract period and the residual period. That changes everything.

CHAIRMAN ZIEMER: Right.

MR. RAMSPOTT: Because this whole alternative is somehow being based on 1993 samples. That's a little late. So I appreciate it. I'm open to any questions.

And all of these people I'm talking about are willing to be interviewed. Thank you.

CHAIRMAN ZIEMER: Okay. Thanks, John. My understanding of this drawing is that these are the locations where they sampled. This is not necessarily where they found activity.

DR. ANIGSTEIN: No, the red ones --

CHAIRMAN ZIEMER: The red is a biased sample, which means they didn't select the location randomly. They selected it intentionally.

DR. ANIGSTEIN: Right. No, the red -- my understanding with the red ones, they went around with an alpha-beta -- with a beta- gamma meter --

CHAIRMAN ZIEMER: Okay.

DR. ANIGSTEIN: -- rapidly surveying. And where it chirped, then they would get out the --

CHAIRMAN ZIEMER: Oh, I got you.

DR. ANIGSTEIN: -- detector and do it and take a reading.

CHAIRMAN ZIEMER: Got you. Okay.

MR. RAMSPOTT: Dr. Ziemer?

CHAIRMAN ZIEMER: Yes?

MR. RAMSPOTT: There is another drawing in the cleanup report that really clearly shows the same red dots as being hot areas that had to be cleaned. And I might add that when we visited the site, Dr. McKeel and myself and some workers with the new owner who let us go in there and photograph it, those are parts of the floor that are definitely scarred. I mean, there was -- I assumed there had to be dust there, but some of that was so ground in they actually used a -- I think they called it scalping.

CHAIRMAN ZIEMER: Yes. Yes.

MR. RAMSPOTT: You could see the gouges.

CHAIRMAN ZIEMER: Got you. Yes.

DR. ANIGSTEIN: Let me --

CHAIRMAN ZIEMER: Yes, I see them. Yes. Thanks. Appreciate that clarification.

DR. ANIGSTEIN: John? Let me correct something, John.

MR. RAMSPOTT: Sure.

DR. ANIGSTEIN: John Ramspott. The red dot -- you're going to see the red dots there. I put the red dots there, the red and blue dots, to correspond to the readings on the table, that if you look on the next -- my -- you know, on another page where it has -- you have the original report. You don't have this one. The alpha activity concentration, you see sample locations north and east. So those are the coordinates of the locations where they made the measurements. And I simply plotted those coordinates on here. So this is a --

MR. RAMSPOTT: Ah, okay. I see.

DR. ANIGSTEIN: This is a composite.

MR. RAMSPOTT: Yes, I saw -- the

drawing is definitely in the cleanup report.

DR. ANIGSTEIN: I took the -- that's where I got it from.

MR. RAMSPOTT: Does that mean there's also a drawing just like this that shows the actual sites, not just numeric?

DR. ANIGSTEIN: Yes, there was harder -- but they were much harder to interpret, so I recalculated them.

MR. RAMSPOTT: The other one is more exact, I understand that, but the fact is the material is where your red dots are and the material is where the uranium was on the drawing that's in the cleanup.

DR. ANIGSTEIN: Right.

MR. RAMSPOTT: Okay.

DR. ANIGSTEIN: Yes.

CHAIRMAN ZIEMER: Okay. Thanks for clarifying that.

Okay. Questions?

DR. McKEEL: Dr. Ziemer?

CHAIRMAN ZIEMER: Oh, yes?

DR. McKEEL: This is Dan McKeel.

CHAIRMAN ZIEMER: Oh, yes, Dan. Go ahead.

DR. McKEEL: Could I just quickly say one sentence?

CHAIRMAN ZIEMER: Yes.

DR. McKEEL: So one thing that I need to emphasize, I listened to that very nice exposition by Dr. Anigstein of his alternate model, but my comment is that the SEC recommendation that the Work Group and the full Board must make, it is really based entirely on what NIOSH can bound with sufficient accuracy.

So with all due respect, NIOSH and Dave Allen have already said in their response paper that they do not accept the SC&A alternate model and will not use it. And all NIOSH can recommend is that they will take the three additional sites, the two slug facilities and the billet facility, more surrogate data and make some kind of an

adjustment to a revised Appendix BB.

And so really the bottom line is that -- and I hope you all will consider is that NIOSH will not accept the SC&A alternate model, however elegant it may be. And what the Board and the Work Group has to concentrate on is what can NIOSH do. And that's the sole criteria for making a recommendation about SEC 00105.

MR. KATZ: Well, Dan --

DR. McKEEL: Thank you very much.

MR. KATZ: Dan? Thanks, Dan. Let me clarify. That is actually not the Board's charge under the regulations, which is the Board's charge is to determine whether doses can be estimated with sufficient accuracy. There's no qualifier such as you're suggesting as to whether NIOSH's method is applied or any other method is applied. But the Board's charge is whether doses can be reconstructed with sufficient accuracy, end of statement.

DR. McKEEL: This is Dan McKeel.

I have to respond to that. We can't complete this argument, but I could not disagree more completely with what Mr. Katz just said and I think that it is very well understood that the role of the Board and the role of SC&A is to evaluate NIOSH's methodology.

MR. KATZ: Dan, I'm speaking from the regulations. We don't need to continue this discussion. But these are what the regulations specify and lay out for the Board.

CHAIRMAN ZIEMER: Okay. We're not going to discuss the regulations today. I think certainly we have to respond to NIOSH's proposal, and one of the things Board does is in fact indicate whether they agree or disagree. And we need to find out actually what NIOSH's approach will be. I mean, one of the reasons we meet is to hear each other's ideas. And historically in all of the sites, we try to come to some point of -- where we can agree, we'll agree. If we can't, we disagree. But, and I don't know if we're

there yet, but I don't think it's the petitioner's position to have to say what NIOSH will do. NIOSH will have to state what they will do.

And so, Josie, you have a comment?

MEMBER BEACH: And just an addition to that, because I know Dave's going to speak here. I was wondering if you're going to use this new information on the facility cleanup, or are you going to consider it, and how?

MR. ALLEN: Well, right now the position I have in that White Paper was to essentially say the 198 dpm per cubic meter we used in Appendix BB is bounding. I tried to justify it by the surrogate data criteria and come up with a few other data points from other sites to point out that it is indeed a bounding value.

We have not, despite what was said here, completely dismissed Bob Anigstein's model as a -- you know, a showstopper, but I

do think there's other sources of uncertainty in that model such as the cleanup which was pointed out which changes that depletion rate, not to mention the heterogeneity of the contamination after you do that cleanup.

And I know he didn't use the biased samples. He used the random samples to try to avoid that, but I think there's still a lot of heterogeneity in cracks and crevices, expansion joints, railroad tracks, you know, et cetera, where power washing can drop all that in there and virtually fix it in there to where it doesn't change over a number of years.

So currently I believe the surrogate data is a better approach, a more accurate approach. And I think the data that we've pulled up from the other sites that is limited, points to the idea that the numbers we are using are actually very conservative.

And my intent at this point, my true intent at this point is to see what the

Work Group feels about this, you know? But my recommendation at this point is we use the same number that's in Appendix BB, the 198 dpm per cubic meter. There may be some adjustments if we want to discuss as far as how that it is used in a model, but as John Ramspott -- or, I'm sorry, as John Mauro said, the question is the starting point right now.

And then the other items we're talking about are the mechanics that are essentially -- you know, one's an SEC issue. And after that you're into the TBD issue part of it. And I don't think we've gotten really any feedback from the Work Group yet as to the starting point and whether it's worth pursuing the mechanics of it. Did that make any sense?

CHAIRMAN ZIEMER: Also, I just want to emphasize that whatever the Work Group does is simply recommendation, but the Board could go in another direction. So the Work Group doesn't speak for the Board specifically. Part of our function is to get

the parties together and talk about the issue so we can in part see if there is some level of common ground. I don't know that -- you know, what we say is not the final word on this. Certainly the Board will listen to the recommendation of the Work Group, but the Board is always independent of the Work Group in a sense and will make its own judgment.

But to the extent that we're able to find some common ground I think is always helpful to the Board, and that in part is why we want to look at some possibilities here. I think that, you know, the Board has the option of saying we'll go with an SEC for both periods, or we'll go with an SEC for one and not the other, or we'll go for dose reconstruction for both. There's a lot of possibilities here.

But to the extent to which NIOSH and SC&A have some level of agreement -- and they certainly don't have to agree, but that also sometimes helps. And of course the -- if

the -- and I don't think it's necessary for the Work Group members to necessarily agree. We can have -- we see this in different ways, and so on.

So what we want to be able to do though is to make sure that we can present clearly to the Board what the issues are. If there's disagreement, why it's there and what the options are.

Now, Bob, do you have a comment? Or, Jim, I don't know if this sort of hits --

DR. ANIGSTEIN: I have a couple of comments.

CHAIRMAN ZIEMER: I don't want to put you on the spot, but I'd sort of like to hear from you sort of independent of Dave in terms of whether you think NIOSH would be in a position to utilize some of the ideas that have been brought forth by SC&A and either incorporating them or if you feel that they're not useful, or we need -- I think it would be helpful to know that as well.

Bob?

DR. ANIGSTEIN: Yes, I've got an observation to make about the most recent development, and that is it's been five years now since SC&A, and on some occasions joined by NIOSH, has been interviewing mostly former GSI workers. One case a site expert was a contractor who worked on the GSI site. These were facilitated by John Ramspott, for which I -- to whom I'm quite grateful, because otherwise I would never have been able to have the kind of information about this site that I have.

Even though this model is something we came up with just recently, nevertheless there was mention of the cleanup. As a matter of fact, in Appendix BB, there is a reference to measurements made in the old betatron building, slightly earlier in 1989, when it was first considered for FUSRAP cleanup. And it's even -- the measurement was cited as validation of the values that were

derived by NIOSH because it was I believe something like -- 1100 was the dpm per for 100 square centimeters was the -- or I -- there was -- I don't want to start quoting numbers. I'm probably getting the units wrong. But there was a value that was used by NIOSH and they said, look, years later the highest value was about half that value, so that shows that this was a good assumption. And no one ever challenged that. There was no mention made of a cleanup.

Suddenly, after five years we're discovering -- we're finding a new person who has information on this and it seems like there's been a lot of time. There was a time when we would have had the opportunity; and I'm sure we're taking the opportunity, to interview these people on the telephone, perhaps even in person, get a better picture. Now we're getting a very last minute second-hand information of quoting someone. It seems a little late to be starting down

that path at this late stage.

And the earlier -- at the time I developed this model, which was just a month ago, six weeks ago, my only information was that the new betatron building had been cleaned up. And that was plausible because, first of all, I know -- I already know that when the Granite City Steel acquired the property; not the business, but the property of GSI, of the -- it was then called the -- it had been formerly called the Commonwealth Foundry, they converted the new betatron building into an office space. Of course they would have cleaned it up.

Whereas my understanding was that the new -- the old betatron was left off by itself. It was used -- that is correct. The two -- the other betatron instrument was brought in and it was just a storage. And I -- we don't know whether it was cleaned up or not. I'm really going to put it very bluntly. It would take a lot of investigation to find

out. And it would seem odd that there would be so much activity left if in fact there had been such a cleanup when the new betatron building was cleaned up, and they could not detect any.

And these measurements were made also to -- there was sort of an allegation of competence there. These measurements were made by the ORISE, the Oak Ridge Institute for Science and Education, which has been taking the lead on cleaning up and surveying -- actually they don't do the cleanup -- of surveying. They perfected this MARSSIM manual, which is the guide to all government agencies for cleaning up radioactive sites. And these were the -- they've given the training. These are the most competent people in the business, the most reputable, competent people in the business.

And the picture of theirs, the berm -- yes, the berm was -- it's well acknowledged the berm -- obviously the berm

was added later and -- but it does not affect the measurement on the other parts of the floor, because the other parts of the floor -- each one of these was a measurement of dpm per 100 square centimeters. So you simply average over whether some of the areas could not be sampled. It's not 25 percent. It looks like -- to my mind it's looking more like 10 or 15 percent, but that's a quibble. This looks like reliable data. I rest my case.

CHAIRMAN ZIEMER: Okay. Thank you.

MR. RAMSPOTT: Dr. Ziemer, may I respond to that?

CHAIRMAN ZIEMER: Sure.

MR. RAMSPOTT: I'll keep it brief.

MR. KATZ: Yes, go ahead, John.

MR. RAMSPOTT: We invited everybody to visit that site, just like we did. And I'm sure more information would have been found if those invitations had been taken, but I know people are busy and it just

didn't work out.

But there was mention of power washing, and I'm sure I can find it in the transcripts from years past. And, you know, I think -- maybe I'm wrong, but I thought the law said if you find new information, you're supposed to submit it. I think I've seen that in the law. And that is what we're doing.

And I also think that I saw all radiation must be considered in dose reconstructions. Well, I think that also is part of this argument, too. If there was a cleaning and that material was moved and what you're seeing now is what was there after that was done, how much was there before it was done? I mean, that's logical.

And I did look at those drawings. They definitely are in the -- I'm looking at it now. They're definitely in the FUSRAP report and they show that material there. And, you know, I've talked to people that are familiar with concrete work. Now that berm,

you don't just put a berm in there. You got to clean the surface of the existing concrete in order to put new concrete on it, especially if you're going to hold oily matter, like transformer oil, which is exactly what this says. Transformer storage area. I'm looking at the drawing now.

So I really can't agree. When you bring in, I guess, whatever it took to build that berm and you bring in another betatron from a new betatron building, you go down that walkway, those railroad tracks, you're going to disturb something.

Now I respect -- or I guess the Oak Ridge National Lab, but in reviewing the cleanup documents again last night, if you look in there -- and, Dr. Bob, I know you used their drawing originally, too, they said there was a huge door in the new betatron building. And I'm just referring to the new betatron building not as part of this argument or this building. But they said the new betatron

building, the main door; that is their quote, is to the left of the drawing or photograph they have. And we all know now that hole was knocked in the wall well after the contract period.

And we also note from their drawing there's -- they never even walked in the 10 building. That's where the uranium had to come in from. There was no other way to get uranium into that building except through 10 building.

So I think they did a fair job, but they were wrong. They weren't totally accurate. They weren't totally complete. With all due respect to them, if I was coming in 40 years later, I might have missed it, too, because they didn't have some site experts and some people to talk to and some workers. I guess I wonder why they didn't talk to any workers. They just talked to Granite City Steel management. If I'd had Granite City management -- those people were

probably kids when all this was going on. So I don't think they did such a good job. I have to disagree with you.

But, you know, I respect what they did. And I probably would have made the same mistakes, but now it's time to correct it. We've got people telling you the facts, you know? So I'm sorry, that's just the straight-up truth. Thanks a lot. I appreciate the chance to comment.

CHAIRMAN ZIEMER: Thanks, John. I want to give -- Jim, I'm kind of putting you on the spot, but do you have any sort of reflections on NIOSH's approach here in terms of what you heard so far today?

DR. NETON: Well, I think Dave's put together a pretty, I thought, compelling argument that the exposures associated with the movement of uranium, the sole activity of the handling and movement of uranium throughout a building is inherently -- is pretty low. The values are low.

I think the most compelling sample is the one that was at LeBlond where they were actually -- I think there were three BZ samples where they hooked a hoist to the billet and placed the billet into position on the machine. And then they removed it from the machine nine dpm per cubic meter. I think that's consistent with Dave's experience, and my experience has been working at a uranium foundry.

And the other two instances he's demonstrated that even though there were ancillary activities going on in addition to the movement, the values were still around less than 200 dpm per cubic meter, which is what we suggest.

There are other values out there that SC&A have raised that talk about movement of uranium where the much higher -- I think they're worth looking at, although the value of 3,000 I think is going to be rejected almost based on being really high. I've

looked at some values while we were talking and there are some air samples in Harris and Kingsley where your chiseling billets, just actively chiseling billets, and you get around 4,000 dpm per cubic meter. I just find it implausible that you can generate fifty MAC air by just moving rods. I mean, that's higher than the values that were measured at Bethlehem Steel during active rolling operations in the 1951-52 period where they were taking heated uranium, you know, and moving and pushing it through machines. So I find that value to be a little bit out of range.

The 500 dpm ones, you know, I don't know. Maybe, you know, that's worth considering, I suppose, but I think we would all -- I believe that somewhere in there, in that range where we put -- Dave has suggested or maybe looking at another value is a bounding value. We know that movement of uranium generates some dust. And I think the

range is somewhere in those values, rejecting the -- I would reject the 3,000 one as being implausibly high.

I get -- SC&A hasn't said this, but I get the sense that they also somehow believe that it could be bounded in some way, whether we pick a value that's a better surrogate value or whether relying on their model, they believe -- I think it's their sense also that this activity can be bounded using the data that we have available to us.

DR. MAURO: Jim, this is John.

DR. NETON: Yes?

DR. MAURO: I agree with that statement.

DR. NETON: Okay. So I think both SC&A and NIOSH agree that the value can be bounded. It's how you come about it and what's the appropriate exact value you use. I sense that SC&A's position is that 200 may be a little low for their comfort level. But again, I think we're in a situation where we

agree that it can be bounded somehow and the exact value maybe can be debated to some degree. But I think within a factor of two. I mean, I don't know. I think 200 is bounding, in my own opinion, but we're open for discussion on other values.

CHAIRMAN ZIEMER: John, did you want to expand on your comment there at all? John Mauro.

DR. MAURO: Yes. You see, we went through a process here which brings me to a place I said, listen, you know, we look for other starting points and we see a range of them that are in many -- in some cases not unreasonable as applied to the circumstances. I for one feel the stamping of these slugs is being not at all like the kinds of things. So we are troubled by the starting point.

Now the interesting thing is outcome. If you would -- let's say we'd start with that 500 number that in our opinion is really -- it's one number associated with the

type of operation that really is not analogous to what types of things that went on. So, you know, we are troubled by that as a surrogate, but there are others that are apparently -- certainly look better. And I agree with Jim, going to 3,000 doesn't ring true given the amount of information we have regarding the airborne dust loading associated in all types of operations.

So it seems to me that if I were doing this, I would go with a different surrogate that perhaps -- and not go with the adjustments that were made and the way they were made. But I think that we can pick one that -- out of the data that now is available, something we didn't look at before. It wasn't until this process where we said, listen, can we find some better surrogate for handling of uranium? And it seems that we have some numbers out there are better as a starting point than the stamping process that -- for these slugs. And I have to say that I was

very pleased with the process that Bob went through to say, okay, let's go backwards in time.

Now of course John Ramspott now brought up a point now, if he's correct. Let's see there was a thorough cleaning of this building before these measurements were made. Well of course, then you got to throw the model out. I mean, that's all there is to it. You know, you can't avoid the argument that, listen, if that's true that it had been scoured, what do you do with that? I don't know what you do with that.

But if it wasn't, then Bob's approach leads you in a place that says, listen, this is another way to come at the problem that's reasonable. And it comes in at a place which -- what's interesting to us is that I think that there's a -- if you went over and looked at it and looked at the concentrations in air as a function of time, you know, the numbers that we're seeing are

compatible with these other numbers we've been talking about.

So that's the reason I feel that we're converging into a place. And I think we can converge on a place that at least in my mind is one that would place a plausible upper bound on these exposure scenarios. So, yes, that's the reason I agree with Jim.

CHAIRMAN ZIEMER: Okay. Thank you.

DR. McKEEL: Dr. Ziemer, may I?

CHAIRMAN ZIEMER: Yes, is that --

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

CHAIRMAN ZIEMER: Yes, Dan, go ahead.

DR. McKEEL: I'm going to say three short things about what I was just thinking on. One is John Mauro just said that if -- if it was true that there was a heavy-duty power washing of the betatron building in 1984, as [identifying information

redacted] said there was, then they would have to throw the model out. And then we went on to explain how you could use the model.

Well, I've got to speak for all honest, truthful, well-intentioned GSI and other workers in the entire nuclear weapons workforce who tried to give the Board their honest opinions on things. Dr. Anigstein criticizes us for bringing this information forward late in the game. Well, you know, you could do the same thing and criticize him for bringing the alternate model to everybody's attention here at the very last minute as well.

But I've got to say this: [identifying information redacted] saw what [identifying information redacted] saw with his own eyes. That's not hearsay. That's not a secondary thing. The Board has heard from [identifying information redacted] before on the Dow SEC and they can pick up the telephone this afternoon and confirm what we just said.

So the fact is, whether it's convenient or not, the old betatron building was power washed in 1984.

The second thing I can do, and I'm sure either John will do it or I will do it immediately after this is over, is we will send you the proof photos which show GSI workers that we know were there during the operational and residual period and left in 1973 inside the old betatron building showing the interior was painted with white paint, which John assures me in those days was white lead paint.

Now, for white lead paint to be 75 percent, 80 percent removed, then a power washing has truly got to be what the name of the company implies, power blasting. And John Ramspott reminds me that these days; you know, that's later than 1984, that power washing as a technology in fact can be used to cut hard materials. So this is like a laser blade -- a laser beam that's cutting through things. And

it did demonstrably take off white lead paint that was coating the betatron doors -- I mean, the walls.

The other comment that I want to make is that throughout this discussion the comment that the only thing that happened to the uranium, the only thing that we need to consider is that this was, quote, cold uranium metal. Well, nothing could be farther from the truth. Because for half of its life at GSI, that may be true. It was cold uranium metal ingots, dingots, billets and betatron slices.

However, the game changed completely and set this site apart from all other sites that are covered under EEOICPA in that the sole purpose of bringing the uranium to GSI was to irradiate it with a 24 to 25-MeV betatron beam. And the petitioners have sent this Board numerous articles from the literature, peer-reviewed, well-respected journals; they sent you another one actually,

that shows that around 6 to 10 MeV betatrons or linear particular accelerators operating in X-ray mode can cause both fission and activation of uranium, and do that. And they convert a measurable portion. Yes, it's only a few percent, but they convert uranium-238 mass into daughter products of fission and activation.

And so for the latter half of its life at GSI this was not cold uranium metal. This was hot activated fissioned uranium metal, and that fact and those doses need to be factored into the dose reconstruction at GSI. And I understand that in the technical documents that have been produced so far that activation dose, the fission dose have been referred to -- this doesn't have the things that we don't know about the uranium exposures at GSI as trivial or insignificant. Well, all I can say to that is, in scientific terms, that's really not acceptable reasoning. OCAS-IG-003 and everybody's interpretation of

the Act says that during the operational period, you have to count all doses. So you just have to.

And it's wrong today to talk about uranium metal as being cold uranium metal. That's just not the truth except for 50 percent of the time it was at GSI. The rest of the time it was quite hot. And to say that these doses -- try to dismiss them, which I hope nobody will do, as being insignificant, inconsequential, under the Act, they're highly consequential and they must be accounted for. And they must be accounted for by someone with sufficient accuracy, and I don't think the models that we've been hearing about today do that at all. So thank you very much.

CHAIRMAN ZIEMER: Dan, I just want to emphasize one point: when we talk about hot metal, we're not -- we're talking about thermally hot, which is a very different condition from the sort of jargon that nuclear people use, that is something being

radioactive as being hot. It's not thermally hot.

DR. McKEEL: I understand this --

CHAIRMAN ZIEMER: Well --

DR. McKEEL: -- and you have criticized me for such semantics --

CHAIRMAN ZIEMER: Well, it's --

DR. McKEEL: -- problems, but I'm telling you --

CHAIRMAN ZIEMER: It's not hot --

DR. McKEEL: -- let's take the word hot out of there.

CHAIRMAN ZIEMER: Well --

DR. McKEEL: Let's say that this has been -- the uranium has been previously fissioned. I don't think you could disagree with that.

CHAIRMAN ZIEMER: No. No.

DR. McKEEL: And it's been subject to betatron activation.

CHAIRMAN ZIEMER: Right.

DR. McKEEL: And that both of

those products generate new daughter products which are radioactive. And I don't think you could argue with that point.

CHAIRMAN ZIEMER: No, and we've agreed to that and --

DR. McKEEL: Okay.

CHAIRMAN ZIEMER: -- NIOSH has calculated those activities and the dose rates from them. It's in the external dose model, so they have accounted for that, and the internals as well.

Okay. We're going to take a break for lunch. You all have a lot to ponder over the next hour while we eat. And then we'll come back and try to move towards some sort of closure on these activities.

MR. RAMSPOTT: Dr. Ziemer?

CHAIRMAN ZIEMER: Yes?

MR. RAMSPOTT: I mean during your break --

CHAIRMAN ZIEMER: Yes?

MR. RAMSPOTT: -- I would actually

-- that word if really bothers me. I would -- about the power washing of the old betatron. That word if is a big word. If you guys would like, and you would allow it, I'd actually try to call [identifying information redacted] at home and ask him to call in on this meeting and you can hear it right from the individual.

CHAIRMAN ZIEMER: Well, I'm not personally disputing the -- I don't believe that's necessary. I'm not disputing the testimony that --

MR. RAMSPOTT: Excuse me, I'm just the carrier of the messages from the men.

CHAIRMAN ZIEMER: Yes.

MR. RAMSPOTT: I know what they told me.

CHAIRMAN ZIEMER: Right.

MR. RAMSPOTT: And --

CHAIRMAN ZIEMER: I think --

MR. RAMSPOTT: -- working around these guys, this --

CHAIRMAN ZIEMER: I think the Work

Group accepts that the cleaning occurred. Is that not correct?

MEMBER MUNN: Yes, that's correct. Absolutely.

CHAIRMAN ZIEMER: Yes. Yes, I don't --

MR. RAMSPOTT: If you would reason the cleaning occurred in the old betatron, I heard John Mauro say he had to throw everything out.

CHAIRMAN ZIEMER: Well --

MR. RAMSPOTT: John, is that not --

DR. MAURO: I don't know, does anyone else have a perspective on this? I know -- Bob, would you agree with that? I mean, if the starting point has been cleaned up extensively that, you know -- I don't know. You know, I hate to step on your toes, Bob, but it seems to me common sense would dictate that.

DR. ANIGSTEIN: I was kind of

questioning his statement.

CHAIRMAN ZIEMER: Well, in any event, yes, you may have to ponder what the implications of that are, but certainly --

MR. RAMSPOTT: If [identifying information redacted] is available --

CHAIRMAN ZIEMER: Yes.

MR. RAMSPOTT: [identifying information redacted] has no ax to grind in this program.

CHAIRMAN ZIEMER: Yes. Well --

MR. RAMSPOTT: He worked at Dow. He didn't work at GSI.

CHAIRMAN ZIEMER: I don't think we're disputing his --

MR. RAMSPOTT: I haven't talked to him in a year until I --

CHAIRMAN ZIEMER: -- testimony that that had occurred, so --

MR. RAMSPOTT: -- talked to him about this matter here, because I remember him saying he was an electrician. I had no idea

he was involved in seeing the cleanup or how it was -- he named the guy from Water Blast by name because he went to high school with him.

CHAIRMAN ZIEMER: Yes.

MEMBER BEACH: John, this is Josie. We do appreciate your bringing that information forward. Thanks.

MR. RAMSPOTT: Thank you very much. These people, three, four people different people telling the same story different directions, don't talk to one another.

CHAIRMAN ZIEMER: Yes.

MR. RAMSPOTT: I try to triangulate everything I present to you. I always have. And I wouldn't have said it if I wasn't comfortable with it.

CHAIRMAN ZIEMER: Yes.

MR. RAMSPOTT: Thank you very much.

CHAIRMAN ZIEMER: We thank you. Okay. Let's break for an hour and come back

at 1:15.

(Whereupon, the above-entitled matter went off the record at 12:15 p.m. and resumed at 1:17 p.m.)

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

1:17 p.m.

MR. KATZ: Good afternoon. This is the Advisory Board on Radiation and Worker Health, TBD-6000 Work Group. We're just reconvening after lunch. Let me check on the line for a few people.

John Mauro, are you back with us?

DR. MAURO: Yes, I am.

MR. KATZ: Great. And how about petitioners Dan McKeel and Pat Jeske?

DR. McKEEL: I'm here. This is Dan McKeel.

MR. KATZ: And, Pat, are you back with us, too?

(No audible response.)

MR. KATZ: And, John Ramspott, are you back with us?

MR. RAMSPOTT: Yes, I am. Thank you.

MR. KATZ: Great. All right then.

CHAIRMAN ZIEMER: Okay. What I'd

like to do here as we begin the afternoon session, I'm trying to get a feel for the extent to which there's some mutual ground between SC&A and NIOSH on the issue of surrogate data. I've gone back and looked at the five criteria. I believe, if I'm not mistaken, that there was already agreement now on criteria 2 and 4. Am I correct, either Bob or John Mauro, on criteria 2 and 4? You both have agreed with NIOSH's approach on those two items, is that correct?

MR. ALLEN: I was going to say I think Bob said that on --

CHAIRMAN ZIEMER: Four was temporal.

DR. ANIGSTEIN: Four we agreed with.

CHAIRMAN ZIEMER: Well, and 2 had to do with a more robust analysis.

MR. ALLEN: Yes, I was going to say I think that's provisional or whatever.

CHAIRMAN ZIEMER: It's going to be

provisional. You would have to include that in the Appendix BB.

DR. ANIGSTEIN: Yes. No, we did not agree with No. 3.

CHAIRMAN ZIEMER: Oh, I didn't say 3. I said 2.

DR. ANIGSTEIN: Oh, 2? I'm sorry.

CHAIRMAN ZIEMER: Yes. Now, what I want to ask now is on process, site or process similarities, did I understand that were NIOSH to use one of the other sites; maybe it's one that you guys cited, SC&A, that were the surrogate, that you might be comfortable with criteria 3? I'm just asking. I may have misinterpreted.

And also that if that were to concur and you agreed that the process, site or process similarities were appropriate that you would feel more comfortable with using the surrogate data versus the later data which John now has indicated has some questions on because of the cleanup, so that you would

agree to the hierarchy issue as being -- that the surrogate data would be appropriate if it has distinct advantage. I'm just -- again I'm asking those two.

And that if the appropriate site or process similarities issue was addressed, that plausibility might be more acceptable. I'm trying to get an extent to which -- you know, our starting point was has the criteria been met and I'm trying to see to what extent if these changes were made would that take care of that? We would still have to agree on what the surrogate data selections would be. And then we would still have to decide whether we want to accept surrogate data criteria or accept that for the two periods.

So I'm trying to get a feel for that.

DR. NETON: I would suggest criterion 1, the hierarchy data -- if it's true that the facilities were cleaned up, which it seemed to be, then SC&A would

probably agree that that criteria was fulfilled. If you can't use the FUSRAP data, then it can't be --

CHAIRMAN ZIEMER: It can't be better than the --

DR. NETON: We would have used the appropriate surrogate data -- it would have been appropriate to use surrogate data if the building was cleaned up.

DR. ANIGSTEIN: Well, but the adjustments are not -- were not made appropriately.

DR. NETON: What's that?

DR. ANIGSTEIN: No, under criteria 1 the problem is that the adjustments -- are we talking about 1?

DR. NETON: Yes.

DR. ANIGSTEIN: Okay. Under criteria 1 the main problem was that the adjustment was not appropriate. Going from -- first of all, there is a --

DR. NETON: Well, the Criteria 1

is --

CHAIRMAN ZIEMER: Hierarchy of data.

DR. NETON: -- is you only use surrogate data if there are no other suitable data to be found.

DR. ANIGSTEIN: And then only after appropriate adjustments have been made.

DR. NETON: Right.

DR. MAURO: Yes, Bob, I agree with you.

Jim, I know you folks are headed -- there's -- I've come to a place now where in light of the stipulation that there was cleanup, that means the use of the hierarchy of data has been demonstrated. We can't go that route. The only data, site-specific data we had was this FUSRAP data and, you know, it sounds as if that there's agreement that there's good reason to believe that there was this cleanup which puts us in a place where, okay, now we have to resort to surrogate data.

And so therefore within that context, Jim, I agree.

DR. NETON: Right.

DR. MAURO: We now have moved into the realm where I think a justification has been provided that it's appropriate to use surrogate data. We've exhausted our effort to try to use site data. Of course now once you move into that realm. So if you want to call that agreement on 1. But as Bob points out, that's coupled up also with, you know, how you use that data.

So but, yes, I understand what you're saying and I would agree that it's time to move off the model and move to the surrogate data strategy and talk about, okay, how do we get through that process and converge on agreement on what I would call the starting point with a good surrogate data set. And then of course the way in which you apply that data and the adjustments you make.

DR. NETON: Right. Okay.

CHAIRMAN ZIEMER: Now, the adjustment issue is only an issue if you were adjusting the data set --

DR. NETON: The slug data.

CHAIRMAN ZIEMER: -- the slug data, which you may not be doing if you end up selecting a better -- and I'll call it "better" for lack of a better term -- a different site. So which has --

DR. ANIGSTEIN: I don't know, I mean, the question of what John just said on the phone, there seems to be a disconnect between the level of contamination found in the old betatron building by -- during FUSRAP cleanup by ORISE and this aggressive cleaning that was being described. It just doesn't seem to make -- it doesn't seem to be plausible that they would have found the contamination levels that they did in the light of such a cleanup. Whereas the new betatron building, that was much earlier. It was stated that there was a cleanup and in

fact there was nothing found there.

But the pattern of contamination, the fact that it was like along the -- around the railroad track, seemed to be consistent with the use of that building. And it just seems to be not consistent with the assumption of the cleanup of that -- of a very aggressive cleanup. That's just an observation which is -- I mean, we can't ignore.

DR. MAURO: Well, you know what; this is John, to get the process moving forward, I guess the question is do we want to stipulate? You know, it's a legal term that I thought to use. We understand that there are questions that -- in other words, I hear what Bob is saying. And there seems to be -- you know, if there was such a cleanup, why are we seeing what we're seeing, et cetera? But I think, you know, for the purpose of this meeting rather -- you know, all I could say is that rather than try to -- you know, we need to -- I think we need to accept John

Ramspott's and Dr. McKeel's argument that this is true. There was a cleanup. The degree of cleanup we -- it certainly occurred, it sounds like, and the degree of cleanup, and what the implications are. It really means it's hard to use our model.

And then I'm -- you know, I'd like to be able to say, yes, we can use our model, but it seems to me we need to set that aside, unless you want to go down the path of digging into that, and I think that that will be a never ending task. I'd sooner think it's better to set our model aside, notwithstanding the fact that, you know, it may have some validity, but I think I would recommend to the Work Group that we pursue the surrogate line. Let that one go. It was our work. You know, we did the best we could. But I think that it served its purpose, but now I think it's time

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to let it go. I do think we should be working the surrogate data line.

And whether or not we could find the starting point, and I say I think we can given what I've seen so far. And in light of -- once we have that starting point, I think

we could converge on an agreement of what types of adjustments and what types of models. How do we use that starting point? So I see a very tractable problem here.

It certainly would have to -- so what I'm coming down to is that all of the issues associated with surrogate data, you know, we do have to go through the process now. You know, how do we converge and agree that, yes, you picked a good starting point? Yes, the adjustment factors are the appropriate ones, that sort of thing. And I think we could work through that.

CHAIRMAN ZIEMER: John, or John and Bob, if a suitable set of surrogate data; and I say "suitable" in terms of site or process similarities, were identified, and you have at least one that looks awfully close, does that in your mind also then to some extent address the plausibility issue? Because if it's an appropriate surrogate site, then in a sense it seems to me you're saying

that the values that you gain from that are plausible if the site in fact is a good surrogate for GSI. Does that follow in your mind logically? I'm asking John or Bob.

DR. ANIGSTEIN: Well, the plausibility criteria is not to the value of the -- you know, the particular value of the parameter, but it's the reasonableness of the assumptions. And the model -- it's a question of the models.

CHAIRMAN ZIEMER: And so --

DR. ANIGSTEIN: So here where we have --

CHAIRMAN ZIEMER: -- you couldn't address that until you knew more specifically how you were going to use --

DR. ANIGSTEIN: Yes.

CHAIRMAN ZIEMER: -- the surrogate data? Okay.

I know -- I think, John, you pointed out in the past we've also thought of plausibility in terms of is it -- does it make

sense reasonably? Like dust loading of air can only reach so much and then a person can't breathe it anymore.

DR. MAURO: Yes, let me -- in the historic use of plausibility the question that we would ask ourselves is we've picked a surrogate site where the starting point is some dust loading that's -- let's say it turned out to be -- let's say we picked, you know, Bethlehem Steel for a dust loading, okay, where they rolled steel. Okay? And you got these enormous dust loadings. The plausibility was meant for that purpose, that is originally. That's the original narrow use that we used when we started. That doesn't mean it's not evolving. And we would say, no, it's not plausible that -- and in fact they're plausible circumstances.

Those circumstances did not exist at GSI. So therefore -- so I would argue the plausibility issue goes toward is it plausible to have dust loadings associated with -- you

know, is it a plausible circumstance that we're taking from one location to another? And you could have concentrations of that elevation.

So what I'm saying is once you pick a surrogate that represents a set of operations and their associated dust loadings, that seem to be plausible as applicable. Let's say applicable and -- plausible circumstances that -- and I say that I think that amongst the new ones we're looking at we have that.

The broader interpretation of plausibility as used by Bob, and certainly not unreasonable to do that, but I typically don't do that, goes toward the plausibility of the model. In other words, in my world -- and Bob and I, you know, we're sort of showing -- you know, airing out our dirty laundry here, but I would not refer to that as plausibility. I would simply refer to that as, listen, you start -- you pick a good starting point.

And I think we can pick a good starting point. And then we have to agree on how do we model -- use that information to model the concentrations in the air as a function of time throughout the operational history of GSI during operations, and then of course during the residual period? And what models and assumptions should be used? I typically don't use that as a plausibility, but you can. Either way it's just semantics.

The question really becomes given -- we need to first and foremost agree on a starting point that we all agree that, yes, that particular dust loading is a set -- it represents a set of circumstances that is applicable to GSI and perhaps plausibly bounding. And then once we could agree on that, then we could talk about what would we do with that data, that starting point in order to feel confident that we are placing a plausible upper bound on the exposure that were experienced by or might have been

experienced throughout the operating life of the workers at GSI?

So I mean, I think it's -- you know, the hard part is getting that starting point. After that it's just, you know, digging -- rolling up our sleeves and agreeing on the mechanics. How do you do the modeling? And Bob was very attentive, as you noticed, when he went through the back calculations at the starting point. But a lot of what he has described in terms of the mechanics is important; that is, that we had episodic generation of dust.

And then of course it -- and it -- so I would say that this is all very tractable if we agree that there is a starting point. And I think that we have enough experience from handling of uranium metal at other facilities that we could pick one of those as being -- representing a plausible upward bound of what might have been experienced during the handling of the metal at GSI.

CHAIRMAN ZIEMER: Okay. Thank you. Any other comments? Board Members, questions, comments? Dave or Jim?

MR. ALLEN: I was just going to reiterate what John was just saying. I was just looking at the Board's surrogate data criteria and it's kind of like we're saying it's an -- surrogate data is an SEC issue, and it's really both because the criteria deals with not only can we use surrogate data, but how is it used?

CHAIRMAN ZIEMER: Yes.

MR. ALLEN: And I think the only question here with this plausibility part is how we are using it, which is more of a TBD-type of issue, not an SEC-type issue. I don't know if that makes a lot of difference in our Work Group right now, but I think there is a -- I think that's what John's trying to say, is separating that slightly.

DR. ANIGSTEIN: Yes, the plausibility -- according to the Board

criteria, the plausibility refers not to the value, but to the manner in which -- I mean, yes.

MR. ALLEN: The manner that it's used.

DR. ANIGSTEIN: The manner in which the surrogate data are to be used must be plausible with regard to the reasonableness of the assumption that's made.

DR. MAURO: And I'm okay with that broader interpretation.

DR. ANIGSTEIN: So it's the manner -- it's not the value. It's not the data. It's the use of it.

CHAIRMAN ZIEMER: Right. Okay. Thanks.

DR. ANIGSTEIN: That's how it -- whether this is right or wrong, this is what's in there.

CHAIRMAN ZIEMER: That's what it is, yes. Wanda?

MEMBER MUNN: What we've been

doing here is almost a poster child for the reason for the Appendix to begin with. This is why Appendix BB was put together, so that we would have -- not have to do what we have just been doing here for the last year or so in this Work Group. That Appendix was put together so that when you have an AWE or sites like this one where information is not clearly available, you have the weight of knowledge of the entire process and the entire background of nuclear knowledge with respect to these metals and how they behave. We have -- that knowledge is known to us and it's available to us.

And that's why we have the Appendix so that we do not have to do the kind of detailed parsing that we attempt to do when we don't have adequate firsthand measurements to rely on. We already know what the material does and we know how these processes affect it. So it is to our benefit to come to a conclusion on whether or not we will use that

material as it was I think intended to be used when we put the Appendix together.

MEMBER BEACH: Well part of our criteria also states that we have to use sites with process similarities. And I think at this site it's been brought up numerous times that there are dissimilarities between the processes. There's more equipment, the way it was sliced, cut, dingots, ingots. I mean, there's just more that I don't believe we know, and that's part of the reason we're talking about surrogate data. And part of our charter is to make sure that that surrogate data being used is similar. And I still don't feel that we have a good handle on what went on, how long it went on, what was cleaned up, what was not cleaned up at GSI.

MR. ALLEN: Well right now the models and et cetera, you know, other than that alternative model, don't count on that cleanup and everything else. But as far as the process similarities I don't think anybody

envisioned calling movement of metal a process. I mean at some point you cut the thing so thin that you have to say, well, you moved it with a Yale fork truck, but not a Ford fork truck. You know, I mean, at some point you got to say we have an overall task that includes these other items, issues.

MEMBER BEACH: I don't know if we've done that though completely.

MEMBER MUNN: We have surrogate information from not just one site, but from multiple sites with respect to what the bounding doses could be for certain kinds of activities and certain kinds of metals. And of all the metals in the world that have been studied, uranium probably tops the list in terms of what we know with respect to bounding doses. And bounding doses ultimately are what we rely on to be able to say, yes, this was a hazardous environment for these workers or it was not.

CHAIRMAN ZIEMER: Okay. Other

comments?

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

CHAIRMAN ZIEMER: Yes, Dan?

DR. McKEEL: Well, I would like to comment on the comparability of the three sites that David Allen came up with and GSI and just comment that it seems to me that comparability of process and site also includes how the uranium metal -- not only was it transported, how it was picked up and transported from one site to another.

I mean, you can say that that's all one operation, but in fact that's not true. I mean, there are different amounts of uranium released when a forklift picks it up with its sharp teeth and say when a grappling hook picks it up, or when a chain is wrapped around an ingot and picks it up. I don't believe that. And somebody mentioned data earlier today from one of the surrogate proposed sites where a forklift gave very high

numbers compared to the 590 dpm. I think it was 3,600 dpm. So this idea that there is uniform data about the airborne levels achieved after various types of handling, that's not true.

So saying that no site can possibly pass the similar process test for -- and this is what I've been saying since 2008 many times, is the fact that uranium for Mallinckrodt went over to GSI to be subjected to high-MeV betatron irradiation which caused fission and activation. And that was not the case at any of those other sites. It just wasn't. Two sites made slugs. That's the very reason -- the two sites that Dave Allen brought up made slugs. That's what they did.

And here today, in really an amazing, I want to say illogical manner -- but the logic of the surrogate data test has really -- you all have tried to bypass it and ignore it and not apply it. I mean, the SC&A findings were that the slug facility data

flunked all of the tests including plausibility and the only one that was really resolved to everybody's satisfaction was the temporal criterion. So that left four that were not satisfied. And I don't believe today that there has been a definitive coming together of the minds on that.

What you're now saying is -- John Mauro must have said a half a -- I mean, a half a dozen times, two dozen times that what we need to do is pick a starting point. Well, I don't think you can pick a starting point, because even though you might say that the billet production facility, No. 3, was pretty good; words like that have been used, and not bad; words like that, actually only a very small and unknown fraction of the uranium that was processed and handled at GSI was a billet, which was a smaller type of uranium object than the 3,000-pound ingots and the 3,000-pound dingots. And none of the billets were subject to betatron irradiation and fission

and activation.

So the idea that you could say that any of those other facilities came close to GSI, much less be stringently justified as being similar, it really defies all the intelligent reasoning that I've collected in the last 73 years. And I hope very much that, you know, there will be a consensus. This process similarity has just not been achieved and won't be achieved. And when John Mauro repeatedly says this problem is tractable and resolvable, then I challenge him. Show me another site where there's surrogate data where the airborne levels of uranium were actually measured, real data on uranium that had been subjected to high-MeV fission and activation. Please just show me that.

DR. MAURO: I think it's applied --

DR. McKEEL: A wide range of daughter products are generated during both of those processes and one of our objections; and

I'm talking about the petitioners' objections, to Appendix BB from the very beginning, in June 2007, was that it does not account for that full range of radionuclides that are produced.

And we've repeatedly cited the paper by Dr. Ziemer and Guo where they subjected surgical instruments to a linear accelerator and showed a similar very wide range of activation products in those metal instruments. And NIOSH has not modeled all of those. They have not come up with good numbers. They've underestimated the amount of fission products and underestimated the number of activation products that are in every article that we've supplied to you by Sugarman, by Kuttemperoor, the most recent one by Crowley, who discovered promethium in 1945. His article, which I sent you the abstract of, all had a much wider ranger of radionuclides than you all have accounted for.

And I don't think any of those other sites that Dave Allen's come up with, or anybody's come up with, had anything to do with what went on at GSI. There was a graph shown from what happens to suspension at Simonds Saw before and after rolling mill operations. Well, that has nothing to do with GSI. Yes, it took 30 days for it to come down to a good level, but that was in a rolling operation. GSI didn't do rolling operations and they didn't do rolling operations on Mallinckrodt uranium.

So I would strongly disagree with John Mauro. I don't think there's a good starting point. And I think you should go back and rigidly apply the Board surrogate data criteria to each and every substitute surrogate data site that you say is good and a good starting point. And I think that it will flunk. All those sites will not pass the test on at least one or more of the surrogate data criteria.

And if you tell me that it's plausible to use a site that has no betatron fission and activation as a surrogate for a site that does, where that's the sole purpose, and you tell me that's plausible and passes the plausibility test, I'd say the criteria are basically worthless. And I don't think they are worthless. I heard all those discussions. I know why plausibility was put in there. There was some disagreement about exactly what they meant. But I would point out to you right now that 10 years after this program in EEOICPA was instituted, there's still an argument about defining sufficient accuracy, a core principle that governs dose reconstruction. That's still not defined carefully, nor is plausibility.

So I'm saying that in the ordinary way that human beings and scientists and trained scientists and intelligent scientists use the word "plausibility" -- that to say that a slug production facility, a billet

production facility that had no radiographic examination of that uranium is comparable to GSI, it is just logic that's too tortured and it's certainly not defensible in my view. So I guess that's all I want to say.

DR. MAURO: This is John. I do have to help clarify a couple of things I think that are very important.

The real question we're asking ourselves -- there's a metal that we're handling, uranium, and when you're handling it, you're going to have the potential to generate aerosols. What Dr. McKeel pointed out is we're really asking can we -- are there handling -- metal -- uranium handling operations where we've measured the number of milligrams or micrograms of uranium in the air while that metal was being handled? And could we just say that those represent a plausible bound for the uranium handling that took place, the metal uranium handling that took place at GSI?

The composition -- now, here's -- and, Dr. McKeel, please, here's where I have to disagree with you. You're really posing a question regarding the composition of that metal. In other words, the fact that it has been irradiated means that, yes, you might actually have some activation products. So it's not just uranium that's airborne anymore. It's uranium with perhaps some fission products, all of which was modeled and in a way that I think we've been through. So really we have to separate the two. It's very important to make a distinction.

DR. McKEEL: Yes, but --

DR. MAURO: The only question we're asking ourselves -- let me finish and then I'll stop --

DR. McKEEL: All right.

DR. MAURO: -- is are there metal -- are there uranium metal handling activities out there that we could say are of a nature that generates aerosols that we could say are

plausibly bounding? The only problem we're having is that most of the metal handling operations out there are too high. That is because not only do they handle the metal, they also machined it and they also did other things with it that would generate even more aerosols. And that's -- you know, so that's one of our dilemmas.

But I think that David and SC&A have found a few where we could say, well, this looks like some -- a place where it was primarily handling the uranium metal. And granted, it's not exactly the circumstances, whether they handle it with a chain or a forklift, or how many times a day did they turn it around? I mean, there's always a place you could find where you could parse it and say, well, you don't really have a comparable circumstance. And that's up to the judgment of certainly the observer; yourself, the Board. But in my mind if you can't find a surrogate for handling uranium; and I'm going

to be a little outrageous, you can't find a surrogate for anything. This is as classic, as simple a problem as you could have in terms of looking for surrogate data.

Unfortunately, in the original start of this whole process NIOSH picked what I -- we now -- SC&A believes is a poor surrogate. It was not really uranium metal handling. It was a different kind of thing. But that doesn't mean we can't find one. And I think we've already found one. And if we look harder we could probably find more. But, boy, we're talking about the simplest of things, handling uranium metal. So I for one feel strongly that we could find and agree upon a surrogate for the GSI handling of those ingots. And the fact that they were irradiated with the betatron is not relevant to this question that we're talking about right now.

DR. McKEEL: I would like to reply, please.

CHAIRMAN ZIEMER: Sure.

DR. McKEEL: No, you're not getting what I'm saying correct. I am saying that that's part of the equation, the fission and the activation. But what I'm also saying is the metal itself -- there is a difference between a slug, a billet and an ingot and the dingot. And what is the difference? The difference is that the ingots and the dingots that were sent over from Mallinckrodt had not been cropped, had not been shaved on a vertical lathe. They still had their bomb; and I'm using that as the furnace bomb that was used to produce those metal -- those uranium metal forms. And as you know, as we all know, and as is inadequately treated in TBD-6000, which I pointed out when this Work Group was revising that document, still not treated correctly in TBD-6000 Rev. 1. There is an irregular magnesium fluoride crust that has various impurities in it, including a small concentration of thorium which

accumulates at the surface.

So in several different ways an ingot and a dingot compositionally, chemically and physically, the surface of them is different from a clean pure uranium metal product. And a slug is a good example of a pure clean uranium metal product. It doesn't have a magnesium fluoride crust. And you know that that magnesium fluoride crust doesn't -- it has different compositions. I suspect it's not as hard a metal. It's not as hard a substance as the inner uranium core, which is very hard -- when they sent uranium through a extrusion press and heated it, one of the big problems is it would fragment. And the people at Dow knew that. They could tell when uranium was being processed. Even when the name of the metal was hidden from them, by its characteristics in an extrusion press it would fragment. It was difficult to do.

So, no, I'm not at all saying this is not one of the simplest things that you can

model. And in all due respect to everybody, including Wanda Munn, I have also given you all data of a nice campaign that was done where they were studying uranium dingots from Mallinckrodt that were sent to Hanford for the Hanford reactors, where she worked, and they had done various things to those dingots. They had various amounts of trace metals that were added to them to see if they could change the structural stability of those ingots in the reactors themselves. And eventually they abandoned them.

So, you know, but there was a beautiful table in the data that I sent you all that showed the individual variances of individual dingots, and they varied quite a bit. And some of them worked and some of them didn't work, and some of them deformed and some of them didn't deform. You know, so this idea that it's one product and it's simple is absolutely contrary to what the scientific literature shows. And I agree with Wanda

Munn. I like to stand on published peer-reviewed scientific literature.

And I would say this: I don't think there is a site where you can produce a paper of scientifically peer-reviewed literature that has a process where they are handling fission and photo-activated uranium that's been produced as an ingot or a dingot by a derby melting two-step process or a one-step dingot process like it was from Mallinckrodt Destrehan Street and later from the Weldon Spring Plant in Saint Charles County.

So, no, I respectfully disagree. I don't think this is the simplest product and I don't think it's the simplest situation. And I would say because of the outer bomb crust that was adherent to the ingots and the dingots, then you have to pick a surrogate data site that processed ingots and dingots. And so the three sites that Dave Allen came up with wouldn't pass that test. They made slugs

and billets. So that's my comment.

CHAIRMAN ZIEMER: Thank you. Yes, Bob?

DR. ANIGSTEIN: Okay. First, as far as the nature of the uranium, starting off with the irradiation of the uranium, starting off with the fact that the uranium was irradiated, a microscopic, submicroscopic fraction of the uranium atoms are actually involved. We're talking about activities with short term -- yes, there is some radioactive radioisotopes. Two of them actually are uranium isotopes, so they would have the same physical properties as the natural constituents of uranium in terms of -- so say mechanical properties.

The fission products we have calculated. They are in the -- absolutely in the minuscule range. I can't quote you a number right now, but they're order -- many, many orders of magnitude below unity, below the concentration of uranium, though there is

just not enough there. They are far lower than the ordinary impurities in the uranium metal. So that is not an issue.

It's an issue for reconstructing doses, and as it turned out, it's a very small amount. We have mathematically derived that and we have used all of the latest physics models. We don't have to go back to papers from 1945. We have the very latest. So that is simply not an issue. I mean, you can -- this can be brought -- any issue can be brought up just to try to discredit what's being done. But that is not a valid issue.

As far as the uranium coming with slag and with fluoride, with magnesium fluoride attached to it, there is no basis for believing that except the assertion that is being made now that because that is how it was produced, it is being assumed and asserted that that's how it was shipped to GSI.

The literature --

DR. McKEEL: Dr. Anigstein --

DR. ANIGSTEIN: Excuse me. Let me finish, if I may. The reports that we have read are that the slag and the fluoride was cleaned off and knocked off immediately after it was made. The purpose for shipping it to GSI were two -- there were only two reasons for shipping it to GSI: There was the slices where they were looking for imperfections in the center of the metal which would affect later on when it was being rolled into rods, and it was also to look at how much of an imperfect end could be sawed off.

The only firsthand information about the end shots was from one worker whom I interviewed who came in on this day shift and he said the night shift had told him they were doing these corner shots. And I made a drawing in response to his account and sent it back to him and said is this what you meant? And the only -- the corner shots would be to just determine how much of this imperfect metal that you get at the end -- you always

get that in a casting. There is air, there is -- slag gets included and they have to saw it off. They don't want to saw off too much, so they cut off on a band saw. And they did the radiograph to tell them how many inches from the end to cut off.

The lathing on the vertical lathe would have been done. That is -- you cannot -- unless you take hundreds of radiographs you would not know how much to take off on the lathe. And there's no need to, because the machinist sees it. When he's bare metal, he quits.

So that assertion is being made without any firsthand information, even any firsthand testimony. This is just an assertion to try to say you can't model this because this is how it was shipped. It was not the way it was shipped, because our metallurgist who has worked with uranium said this made no sense whatsoever. This is not the way things would have been -- this is not

the way uranium would have been fabricated.

DR. McKEEL: Dr. Anigstein, this is Dan McKeel. Do you have firsthand information from any source about the condition of a dingot that -- I sent you a letter from within the Atomic Energy Commission that that -- actually it was from the -- it was from the ORNL cleanup program that was talking about -- that the primary product that was sent from Mallinckrodt to GSI was dingots. Do you have any information that those dingots had been cleaned off before they went over to --

DR. ANIGSTEIN: Yes, there was the report on the Mallinckrodt site which described the processes.

DR. McKEEL: And it said that -- no, I don't believe so because -- I wish Mr. Ramspott would weigh in on this. He has a report from Mallinckrodt that says just the opposite, that one of the purposes of sending the uranium over there was to define that

slag/uranium interface. So I don't think --

DR. ANIGSTEIN: The ends. The ends, not the surface. And there is --

DR. McKEEL: Well, we --

DR. ANIGSTEIN: I cannot produce this at this moment.

DR. McKEEL: Well, we have a picture that we also sent you recently of -- you're right, of uranium dingots over at Weldon Spring from the Weldon Spring Interpretive Site Museum. And this is -- who knows how long that dingot has been made, but you know, it's quite clear that the outer surface is rough coated slag and not smooth shiny uranium.

So I believe that you're making an assertion and I believe -- and, you know, that's why we write all this down in our technical papers. I can't go back and reconstruct every single bit, and I think it's wasting the Board's time to do that. We have provided that information. We believe that

based on technical reports that we've furnished that one of the main reasons -- that has been ignored by SC&A and by NIOSH, that those dingots and ingots were sent over to Mallinckrodt -- I mean, from Mallinckrodt to GSI was to define that interface.

And so, regardless of what Dr. Thurber says and your metallurgy experts and so forth, that's their opinion. We have our opinion. I think we've backed it up. And so, I think the Board will have to decide. Thank you.

MR. RAMSPOTT: Dr. Ziemer, this is John Ramspott. May I add to this a moment?

CHAIRMAN ZIEMER: Sure.

MR. RAMSPOTT: I actually do have that article. Matter of fact, I'm looking at it and wrestling with it a little -- with my email right now. So I will get this again forwarded to Mr. McKeel so he can send it to you. But it actually states in here -- and I have shared this actually with this Work

Group, with the Board, with SC&A, with NIOSH. It clearly states in here the reason -- you use high -- it's actually the symposium -- let me get the exact -- you can pull it up, too. It's online. Non-destructive Test in the Field of Nuclear Energy. And they actually state you use high-energy X-rays in order to figure out how thick the crust is so it can be taken off with the lathe. I mean, there's no doubt about it.

We also visited Weldon Spring. We have a picture of a dingot.

DR. McKEEL: Okay.

MR. RAMSPOTT: It still has the crust on it. No ifs, ands or buts, it's on there. But I'm going to come back to one -- if I could, just one important comment, and it goes to everybody. We're talking about surrogate material. You mentioned one site that had 75 pounds of uranium over, I don't know what the period of time was, and that was one of the surrogate sites. Chambersburg, I

believe. I have a more serious question for you: Look at all those purchase orders. Do any of those purchase orders tell you how many pounds of uranium went to General Steel?

DR. ANIGSTEIN: No, John, you know they do not because you --

MR. RAMSPOTT: That's correct.

DR. ANIGSTEIN: -- have them.

MR. RAMSPOTT: Correct. How can -- I'm going to back extrapolate this a little bit. How can you pick out a surrogate data for GSI when you don't even know what GSI had? How do you do that?

Now one ingot was 3,000 pounds. A slice is maybe one-fifth of that. You don't know many ingots, dingots, slices went to GSI. You don't know much they weighed. Are you going to take 75 pounds of uranium and whatever was with that, take that times 50,000 in order to get GSI's magic number? If you don't know what was at GSI, how can you use any surrogate data? I don't care what site,

you don't know what went to GSI.

I know some hours. And I question the hours because nobody has ever broken down by year the steel worker's salary. We've taken some generalities, given out by employees. They were guesses. They don't know. They got a paycheck for 500 bucks.

DR. ANIGSTEIN: Excuse me. I just want to clarify that point. I don't want to interrupt you. It wasn't -- it had nothing to do -- John, it had nothing to do with the salary. The purchase order said we will pay you $16 an hour for your -- for the radiography and we will pay you $500 for three months. So they specified -- it had nothing to do with what the workers actually got. This was the charge that was negotiated between GSI and Mallinckrodt, so they simply presumably would submit a record. We spent so many hours and this is how much we charge you at $16 an hour. It was always $16 a hour.

MR. RAMSPOTT: -- matter because

you eat up $150 pretty quick if it's $20 an hour. You don't eat it up very quick if it's $1 an hour.

DR. ANIGSTEIN: Where do you get -- they said -- $16 an hour was on every single purchase order except the very last one or two when it went to $35.

MR. RAMSPOTT: Yes, but you don't have any purchase order for the early years. The workers told me they made two bucks an hour over there, three bucks an hour.

DR. MAURO: It's important not --

MR. RAMSPOTT: It's very important.

DR. MAURO: No, no. It's important not to get lost in the woods.

MR. RAMSPOTT: John, the biggest item that I'm bringing up, you don't know how much uranium was at GSI.

DR. MAURO: I would say -- I postulate this for consideration by the Work Group. The real question is how many hours a

day, how many days per year were people handling uranium metal? The uranium metal could be a dingot, which is a large object. It's very big. Or they could be handling a large number of rods or billets, or other forms, physically shaped forms. So I would say that it's really -- we have to make sure we got a pretty good handle on -- you know, if you assume the person is doing it full time.

The real question is when people are handling uranium metal there is airborne dust. Okay? And so this business of the -- whether it's the tons and pounds, that's not -- I don't -- in my opinion, I think the real question is when you're handling metal, there are a lot of different kinds and shapes and sizes of uranium metal that you could handle.

But I do believe that the question that Dr. McKeel raised, well, isn't -- sometimes we don't really -- there may -- I will say at this point that certainly some of that metal may have had some type of oxide or

dolomite scale to it. Some of it may not. Some portions of it may. Some portions of it may not. And in theory that surface of the metal could be influential in the degree to which you could get aerosols generated.

For example, if you've got pure uranium metal that is -- and it's -- you know, we know that it has the potential to flake, cause oxides. On direct -- some conditions you get sparking. If you've got the outside coating -- I don't know if Bill Thurber's on the line. He may have a sense for it. But if you have this coating on the outside that's associated with the originally formed; I guess it would be a derby or a dingot, it may actually have a different type of surface. And I would be the first to say, yes, there may be a difference in that. So I concur that that's worthy of deliberation.

And the degree to which by using the surrogate data that we currently have before us, the degree to which that might be

non- representative of let's say handling a dingot, I think that's -- and so I like to try to keep complete open and clean on this.

So, I don't know, Bill, are you --

DR. THURBER: Yes, I'm on.

DR. MAURO: Did you have -- I know that you've looked at this a bit. We've talked about this in the past.

DR. THURBER: I haven't been privy to all the conversation today; I'm sorry, but I would make one or two comments.

If you look at the worker surveys that were done at the places that -- where the bomb reduction was done, one of the operations and one of the dirty operations was the chipper. And obviously the chipper was the guy who was cleaning up the surface of these dingots or ingots before they were moved for further processing. So, you know, it seems to me that any product that goes out the door has already had some chipping and cleaning of the

surface to remove easily friable material, if you will.

And of course the other question is, well, what is the composition of that material? I mean, nominally it's a magnesium fluoride. So those are a couple comments I would make.

DR. MAURO: And let me close the loop a little. What we have here is a very good question. It goes toward, well, what are we going to use for the surrogate, because we have data that just about captures every aspect of every -- of operations of uranium under every circumstance. I'm thinking the Adley report is a great example, and there are many others by Christifano and Harris, Harris and Kingsley. There's a collection of an immense amount of information of uranium in various stages being handled under different conditions.

The real question is -- and, Dr. McKeel, I appreciate your pointing this out,

because there may be some question as to the material. And I can't say for certain whether there's a question or not, but there may be reasonable questions. Was this dingot or ingot or this slice -- it may have -- actually the -- for example, the slice. The slice may -- of course on one surface be the uranium metal that's without any scale, but the outside edge may very well have had some. So what you're really posing; and this is a good conversation, what are we going to choose as a surrogate?

For example, all we're really doing now is going a little deeper. Because when we started out, SC&A was concerned with the slug and the stamping. Well, what you're saying is, well, let's be -- okay. Good. Let's -- this is SC&A talking now. You know, if we -- SC&A would say, well, listen, the slug and the stamping, we don't think that's a very good starting point. Not that the number you come out with at the end is a bad number,

but to start with that number, it seems that really is very little parity in the way in which the uranium is being handled. And it seems that there are other places, other handling operations that might be more analogous to the handling operations.

And so now we're getting -- we're sharpening the analysis further. What you're saying is, okay, we're with you. And you're saying, Dr. McKeel, that, well, we don't especially like the fact that you're using these particular cases, the ones that we've cited earlier, discussed earlier. There may be better ones. And I would be the first to say, yes, there might be better ones. But I think there are --

DR. McKEEL: No, I didn't say that. I said I don't think are any others that are comparable.

DR. MAURO: Fair enough. You know, all I could say is that when I reviewed that literature, and let me -- I spent eight

years diving into that literature. It's out there. And right now I could say that if it's bare uranium metal, we already have a few that we've identified. And now you're saying, well, let's assume, no, it's something that might have some kind of dolomite crust on the outside. You know, we'd go into the literature, find -- you know, and see how we can do -- if that turns out to be the predominant form, yes, this is an uncertainty.

Is it manageable within the context of dose reconstruction or sufficient accuracy? Certainly once we come up with our case, our arguments, I think all we've managed to explore at this point is that you're raising a question regarding this alternative surrogate, just like we did. We did the same thing when we started this out. We raised a question regarding the stamping operation. What you're doing now is you're raising a question regarding the bare metal. And I think that's a reasonable question to ask, you

know, and it needs to be answered.

MR. RAMSPOTT: Dr. Ziemer, may I --

DR. McKEEL: John?

MR. RAMSPOTT: -- finish my original --

DR. McKEEL: Can I make a comment just to follow on, because I --

DR. MAURO:

CHAIRMAN ZIEMER: Hang on.

DR. McKEEL: -- think I need to keep this thread in continuity.

CHAIRMAN ZIEMER: Go ahead.

DR. McKEEL: I need to respond. I appreciate what Dr. Mauro just said. I need to respond to what Bill Thurber had to say. And that was that we agree, there was a job called a chipper. And when the bomb reduction was completed and it cooled down enough to be opened, which could take quite a while, somebody had to clean the loose crust, slag, scale; it's been called different things, but

the magnesium fluoride had to come off. And some of it was easy to take off, or relatively easy, and other of it was tightly adherent to the surface of the uranium metal. And that's the point I tried to make.

Bill Thurber said it quite well: Some amount of the magnesium fluoride had been removed. But the point we're trying to make is in all the pictures that we've seen it was not completely removed by the chipper. And that's the reason why in that symposium on non-destructive testing of uranium metal the comment was made that there needed to be some radiographic guidance as to where the interface was.

And I believe that same chapter goes on to describe that, you know, when you were going through with a vertical lathe, sure, you could take extremely small cuts, millimeters, but time was money then as it is now, and they wanted to get through more quickly. And if they had a radiograph, it

would define the crust thickness, both at the top and the bottom. And the bomb was adhering around the sides, too, not just at the top. So they had to do a top crop and a side crop and a bottom crop. And we're saying that that's what took place.

So I'm saying that a long time ago we introduced that information. Three years ago at least we introduced that information. And I've thought all along, it won't be possible, from what I know, to find a site that did similar things for similar reasons, because I don't know of a site that we're defining -- Fernald would be a good one. We don't know how it was defined at Fernald. I think that would be interesting. We've suggested looking into that. Was there a betatron at Fernald, et cetera? I don't know now the answer to all those questions.

But the other point, huge point that I want to make today is John Mauro is saying that we can work this out, but the vote

that's supposed to take the recommendation of this Work Group is supposed to take place today, in a few minutes. And I don't think there's any more time to do that. I think that you should make your recommendation based on a solid method that you have now, that you're convinced will allow NIOSH to bound with sufficient accuracy all -- all doses during the operational period and the residual uranium doses during the residual period and all of the doses during the operational period.

And so, you know, I don't think there's time to explore around and look for other data. And I would suggest this: If nobody has found an exact comparable site to GSI and what they did in the seven years since 2005 when all of us have been aware of the betatrons and what was done at GSI, if nobody has been able to do that in seven years, it's highly unlikely that that's going to be done in the foreseeable future. I have email from

-- between John Ramspott and John Mauro that exploring a betatron model was something that was underway at SC&A as far back as 2006.

So I'm saying that today is the day. There needs to be a recommendation. I think the recommendation is there is no solid model for calculating intakes. The Work Group certainly can decide that the intake model that NIOSH proposed in Appendix BB is satisfactory. But against that, I think they have to say is the very solid recommendations of SC&A before this discussion began today. And what was written down in the work that they were supposed to do for the Board was to see whether the surrogate data criteria were appropriately used for the slug facility in TBD-6000, and the answer was, no, it was not. And I don't think there's any more new data that's been presented today since the data about what happened during the cleanup period.

And I am pleased and think it's important that it has been stipulated in a

legal sense by both sides that the GSI old betatron building was power washed to an extensive extent in 1984, at least.

And so I think that's where things are today and I do appreciate the time to address the Work Group.

DR. THURBER: This is Bill Thurber.

CHAIRMAN ZIEMER: Go ahead, Bill.

DR. THURBER: Dr. McKeel said, well, this magnesium fluoride slag or whatever it is did go through a chipping process and what was left was very adherent. If what was left was very adherent, then the likelihood of it being removed during handling was of course substantially diminished, or it would have been removed.

The second point I would make, Dr. McKeel said, well, it's very important to the production to be able to take cuts as deeply and quickly as possible. That fails to recognize that it takes some pretty robust

equipment to these vertical lathes. And the question is, well, how much of a cut can you take at a time and how much are you going to be saving in productivity if you knew if there was some way that the X-rays would show you the depth of contamination that you couldn't discern by eyeballing the surface?

CHAIRMAN ZIEMER: Okay. Thank you.

DR. McKEEL: Well, all we know is that there is the Non-Destructive Testing Symposium book that says that's exactly why radiographic NDT examination of uranium was done. And so --

DR. THURBER: Was that --

DR. McKEEL: -- I guess the answer is --

DR. THURBER: Excuse me. You know, I'm not familiar with that, but was that symposium- specific --

DR. McKEEL: Well, it's a major publication. I mean, I --

DR. THURBER: Excuse me.

DR. McKEEL: You know, we have presented that.

DR. THURBER: I beg your pardon. May I finish, please?

DR. McKEEL: Of course.

DR. THURBER: I was asking the question about something that I wasn't familiar with. I'm not familiar with everything that's gone on with GSI. I've been working on other things. But I would ask you this question: To what extent was that symposium which you've quoted several times specifically connected with the work that was done at GSI?

MR. RAMSPOTT: Dr. Thurber?

DR. THURBER: I don't know. I'm asking for information.

MR. RAMSPOTT: Dr. Thurber, may I answer that for you?

DR. THURBER: Surely.

MR. RAMSPOTT: That document

actually names Mallinckrodt. And I'm going to put that -- matter of fact, that's not the only information. The chipper -- you're the first guy I ever heard talk about a chipper knocking the slag off, and you are 100 percent correct. I have a picture of that person doing that at the Weldon Spring site, which would be dingots, where the Mallinckrodt chipper is knocking the big heavy slag off of the bomb with the uranium laying on the ground and it shows him chipping. Then -- and that's in the Post-Dispatch 1959 article that I have. It's about a four-page article. It's really great. I'll get you a copy of that.

I also then have the book that we're talking about, the non-destructive testing one. And they again -- they name Mallinckrodt in there. They name a lot of other sites. That's why this thing is not just applicable to GSI. But they name Mallinckrodt. They name the -- or they actually state that high-intensity X-rays are

used to figure out how much crust is left on there.

DR. THURBER: Does the article speak to specifics of what can be done, or is it a generic comment that it would be --

MR. RAMSPOTT: No, it goes into specifics. And then there's an additional magazine that I found as a result of that that goes with it and it actually shows those dingots having the crust taken off by a turret lathe. Mallinckrodt is named again in that article.

DR. THURBER: Right.

MR. RAMSPOTT: There's three different articles that will confirm everything we're saying. And the one that you brought up is 100 percent correct. And what Dr. McKeel is saying apparently is 100 percent correct. And I gathered that information and I'm going to put it together again. I was looking at it the other night and one thing they said, where the chipper was at Weldon

Spring there were extensive, I think is the word they used, exhaust systems at Weldon Spring for that. And I know you guys have been involved with Weldon Spring, so you may have seen these. But I'm going to put together that package. I'll do it in the next day and get it to everybody because it comes directly to what we're talking about right now.

DR. MAURO: Let me say something. It's so easy to lose sight of what we're trying to do. It's too easy to happen. If the folks on the phone are saying that, well, there are aspects to this operation -- let me give you an example: Let's make believe that there was some chipping going on at GSI. I'm not saying there was. You could -- I don't -- see, what I was getting at is whatever you want to postulate, well, they may have been doing some cutting, they may have been -- whatever it is, that it was dolomite crust, it wasn't, it was naked, it was heated, it wasn't

heat. Whatever you want to postulate as being a possible scenario that took place at GSI, I could say right now we will find a bounding surrogate to apply to that circumstance. That's all I'm trying to say.

You brought up that, well, it was this. No, it was that. Well, whatever it was, there's so much data on airborne dust loading associated with every type of operation you could dream of on uranium that once you tell -- if there's uncertainty as to what exactly -- what was handled and how it was handled, we will then go the next step up the ladder and pick a little worse scenario that we would say plausibly bounds that circumstance. That's why I say it's a tractable problem.

MR. RAMSPOTT: John, when I was speaking before when you first entered that thought, there's one thing I got to come back to, and I have to respectfully disagree with you on the size of the uranium object can

determine the amount of dust. The example I'm thinking of; you tell me if I'm wrong, you got a slice, you got a dingot. You have a cup of flour, you have a bucket of flour. You drop the cup, you drop the bucket at the same time, where do you get the most dust? Where do you get the most flour?

DR. MAURO: It's really the surface area, not the weight.

MR. RAMSPOTT: Oh, no, I'm not talking -- I'm talking about surface area. There's more surface in a bucket than there is in a cup.

CHAIRMAN ZIEMER: John, we understand that point. I don't think we have to belabor it.

I want to make one other comment on this in terms of the idea of looking for a surrogate, that in my mind it does not have to be a process where the uranium is irradiated with a betatron or anything else. I fail to see how that has any impact on what the

surface properties will be in terms of what can be removed. The amount of atoms changed in that surface is so minute in that activation process, it's virtually impossible to change the properties. So that the idea of how much uranium can be removed is not related to the fact that it's been irradiated with a betatron.

DR. McKEEL: Dr. Ziemer, do you agree that a surrogate site would have to have employed ingots and dingots --

CHAIRMAN ZIEMER: Oh, sure. Yes.

DR. McKEEL: -- and slugs and billets?

CHAIRMAN ZIEMER: I think we have to take into consideration both the size and the kinds of surface materials, but not the fact that they had been irradiated, yes.

DR. McKEEL: Okay. Well, then let's not forget the size and the volume then.

CHAIRMAN ZIEMER: Yes, okay.

DR. McKEEL: That's also

important.

MR. RAMSPOTT: Dr. Ziemer, that's my question: Would you agree that the size -- you just said volume. Would you mean size?

CHAIRMAN ZIEMER: Well, generically I think --

MR. RAMSPOTT: Or quantity.

CHAIRMAN ZIEMER: -- we're all in the same boat on that. And generally you're looking for similar kinds of operations. I don't know if we're going to reproduce size exactly, but it would certainly make a difference if you're talking about a few pounds of uranium versus these big ingots and dingots. So, yes, it would help.

MR. RAMSPOTT: Doctor, that's the point I was trying to get to. At GSI you don't know what was there.

CHAIRMAN ZIEMER: Yes, but you can assume --

MR. RAMSPOTT: You don't have any quantities. You don't have any shipping

manifests. You know nothing about the uranium that was at GSI in terms of quantity.

CHAIRMAN ZIEMER: No, but we do have the possibility of knowing what kind of activities are generated by other facilities that handled similar kinds of materials.

MR. RAMSPOTT: My understanding was Weldon Spring was the only one that had dingots. You guys will have to correct me if I'm wrong.

CHAIRMAN ZIEMER: No, we don't know. I don't know. Anyway --

MR. RAMSPOTT: I had an article that said that.

MEMBER POSTON: It's 2:30 and --

CHAIRMAN ZIEMER: Yes, we need to --

MEMBER POSTON: -- we still haven't had a Work Group discussion.

CHAIRMAN ZIEMER: Yes, we need to -- well, we've been discussing, but we --

MEMBER POSTON: No, we haven't had

a Work Group discussion.

CHAIRMAN ZIEMER: We do need to reach a point where we have a recommendation for the Board of some sort for the full Board meeting. That recommendation can take the form of accepting in some way the NIOSH approach. It can take the form of disagreeing with -- what I'm calling the NIOSH approach now I believe is sort of a modified picture of this surrogate thing that you presented in terms of some modifications of the -- looking at which one it is here -- of the criteria 3. And I think -- I believe that's what SC&A was agreeing to as well, that if you could -- it's basically the starting number issue.

Now, if you were to make that recommendation, I think what Dr. McKeel says is true, that that would have the effect of extending things, because we're not at a -- you know, we don't have something specific then at that point. Although, if the Work Group felt it was a tractable problem, then it

no longer becomes an SEC issue.

MEMBER POSTON: Right.

MEMBER MUNN: Well, the key to what we've been discussing boils down I believe to its essence; perhaps it's being missed here by me, is whether we will or will not continue to rely on any information that we have received from the FUSRAP data as to whether or not that is the basis for what John calls the beginning point, or whether we will use -- we will request that NIOSH consider the possibility of using surrogate data in a different manner than has been proposed.

CHAIRMAN ZIEMER: Well, I think we need to ask NIOSH directly in terms of what you heard today and what is your position, or are you prepared to say --

DR. NETON: Well, this mag fluoride issue with the dingots, I'm not sure where that will take us. In my opinion, if there's a mag fluoride crust on there that has lessened amounts of uranium, the potential for

inhalation of uranium goes down --

CHAIRMAN ZIEMER: Yes.

DR. NETON: -- because you diminish the source term. You protect the uranium metal itself. And in my opinion, the bare uranium metal is probably the highest source term potential there is. So I don't know what fruitfulness there would be in researching exposures from dingots. I think you've bounded it using a bare uranium metal source term.

DR. MAURO: And, Jim, you know, earlier --

CHAIRMAN ZIEMER: What I was asking about -- or I think Wanda was asking --

DR. NETON: Hold on. Hold on, John.

CHAIRMAN ZIEMER: Hold on, John. I think Wanda was asking about the data from the cleanup, whether --

DR. NETON: Well, I think we need to separate two things: One is can we

reconstruct the dose during the covered period?

CHAIRMAN ZIEMER: Right.

DR. NETON: I think that needs to be established first. If we can't, that brings another issue into the cleanup period. So I think we need to maybe go in a step-wise fashion and say can we or can we not reconstruct the dose during the contract period? And then we could take on the second issue, which would be can we do anything during the cleanup?

John, you were saying something about --

DR. MAURO: Well, I was saying that I think the slices are a good example of the dilemma we're dealing with. Once the face of the slices are in all likelihood bare uranium metal, not unlike the bare uranium metal at other places that are handling rods and slugs, not unlike other bare uranium metal. Then you have the edges which may or

may not -- and I -- it sounds like that some of them may have had some of the dolomite. I think that's magnesium fluoride crust on the outside. And so, I mean, we're dealing with -- that's what we're dealing with.

DR. NETON: But, John, that's not radioactive. That's --

DR. MAURO: No, no, I'm just about to say that. Now that is -- so, you know, one could argue that the -- that's -- if it's -- you know, if you got this crust, you probably reduce the potential. But, no, I hate to be the one to say that, because I'd be just speaking from what I would say first principles, but without any direct knowledge of this.

But, so, you know, we have both these circumstances. And if -- you know, and where we are right now is -- whichever the circumstances are, if it is judged at the end of this process that the bare metal has more likelihood to spark and make airborne aerosols

than let's say the dolomite crust, well, we have surrogates for that. And if it turns out that a case could be made that, yes, the dolomite crust might be a little more friable, I don't know if it is, but it isn't uranium. It would be a little uranium.

So, I mean, you know, at some point you've got to say can we wrap our arms around and wrestle this thing to the ground or not? And I'll say it again: With the data we have out there, you know, we have a way to bound it. It may be simply that let's go with some bare metal that has been handled. And we have examples right now that we've been talking about when we started this conversation as perhaps being the one that is bounding one of those cases. The dolomite issue, I think it's a red herring.

DR. NETON: Yes.

CHAIRMAN ZIEMER: Other comments? John Poston, no comments?

MEMBER POSTON: I think we ought

to move forward.

CHAIRMAN ZIEMER: What does that mean? What are you recommending?

MEMBER POSTON: Do what --

CHAIRMAN ZIEMER: I'm looking for a recommendation from the Members of --

MEMBER POSTON: We have a suggestion to try to bring these two together.

MR. RAMSPOTT: Dr. Ziemer?

MEMBER POSTON: I know it's extra work, but --

CHAIRMAN ZIEMER: Yes?

MR. RAMSPOTT: If I can make a quick comment, I'm asking you to please double- check, I believe dingots are a product of Weldon Spring. That would mean 1958. I think ingots from Mallinckrodt Chemical Works are what went to GSI. I think there's a clear distinction. '53 to '58 I believe dingots plus other items -- or I'm sorry, ingots plus other items, slices. But I'm pretty sure -- and I've seen the literature and I'll look for

it again. And I know you guys can find it. You guys know Weldon Spring better than I do. You think that's where the dingots got started.

CHAIRMAN ZIEMER: Okay. Thank you.

MR. RAMSPOTT: If you're talking about the dolomite or whatever crust and how that changes, you need to take that into consideration, I think. I might be wrong. But I still come back to the quantity. You don't know how many dingots, ingots or slices.

CHAIRMAN ZIEMER: Okay.

DR. ANIGSTEIN: Yes, according to the Technical Basis Document for Mallinckrodt dingots were made at Mallinckrodt. I didn't say exclusively, but they were made at Mallinckrodt. There's a very detailed description of how it is removed from the bomb, allowed to cool off and how the slag is chipped off and reused, conveyed away on a conveyor belt.

MR. RAMSPOTT: At Mallinckrodt?

DR. ANIGSTEIN: This is at Mallinckrodt. I have not read the Weldon Spring report lately.

MR. RAMSPOTT: Okay. I'll double-check my literature and I'd ask you to do the same, if you would, please.

CHAIRMAN ZIEMER: Go ahead, John.

MEMBER POSTON: I know that SC&A, wherever you are, is supposed to be working for the Board and NIOSH is also -- has their responsibility, but it seems to me that we've got enough discussion and enough interest in doing this that we ought to ask them to get together and provide a solution to this conundrum that we have.

CHAIRMAN ZIEMER: A solution in terms of the issue of what would be a suitable surrogate?

MEMBER POSTON: Yes.

CHAIRMAN ZIEMER: Is that what you --

MEMBER POSTON: Yes, exactly.

CHAIRMAN ZIEMER: And if we were to ask that, what is your recommendation or what we would recommend to the Board at our next meeting?

MEMBER POSTON: Well, unfortunately the Board meeting's coming up here.

CHAIRMAN ZIEMER: Because time-wise I -- and keeping in mind there's a holiday coming up and then both groups are getting ready for the meeting itself with other preparations, so this is not something that's likely to occur before our full meeting, I don't believe, if we were to do this. I don't know if you're making that as a motion or just getting the idea on the floor, but --

MEMBER POSTON: Well, I will make it then as a motion, but my concern is not whether or not the Board is meeting or not. I mean, I think that we ought to do this

correctly once and for all and have something that we can stand by and recommend to the Board. And it takes until the next Board meeting in whenever it is, December or whatever, then so be it. But I don't think it makes any sense to rush through something because we have to have something from the Board. We can give them an update on what's going on, tell them about our discussions that we've had, the disagreements and trying to get things together to reach something that's useful. But if you want a motion, I'll --

CHAIRMAN ZIEMER: Well, I think we need a motion to get some of these specifics here and get some -- to go on record. So I'm not sure I got the full motion, but I got --

MEMBER POSTON: I've moved every part I could. All right. Let me think.

MR. ALLEN: If I can just make one comment. I mean, I came into this meeting thinking that the White Paper basically justified that the 198 dpm per cubic meter I

used was a bounding estimate. And I -- speaking for Bob; and he can correct me if I'm wrong, I think he came into the meeting thinking the alternative method was probably the best thing on the table at that point. If I'm not mistaken, the Work Group, at least from the discussion, seems to have shot both of those down.

CHAIRMAN ZIEMER: Well, I think the -- I believe that in a sense SC&A has withdrawn that position because of the cleanup.

MR. ALLEN: Okay.

CHAIRMAN ZIEMER: And I think I heard John Mauro say that.

DR. MAURO: If we stipulate -- and I use the term "stipulate" -- simply means, you know, that that being the case, I think we can't use the model.

CHAIRMAN ZIEMER: Yes, that is --

DR. MAURO: So, I mean, I don't think anyone would disagree with that.

MEMBER BEACH: Well, I don't know if Bob agrees 100 percent with that.

DR. MAURO: Well, no, I think if he -- Bob, would you agree that if it turns out there wasn't --

DR. ANIGSTEIN: If that were the case -- we just heard -- it was just in the past few days that we heard about the cleanup at the old betatron. I certainly would not -- if there had been evidence of cleanup at the old betatron, then we certainly wouldn't have gone down the path we did.

DR. MAURO: That's all I --

DR. ANIGSTEIN: I think that to make such a radical abrupt change in direction I think that this cleanup needs to be looked into, just like -- I mean, we spent -- and I guess I should say I -- with all due modesty, I can say I spent a good portion of the last five years tracking down what went on at GSI, and I think we got a pretty good picture. Speaking -- I spoke to a number -- I mean, I'm

not talking about this particular thing. I'm just saying in general I spoke with a number of people, two of whom are no longer with us, who gave different views, not always the same, not always identical. But you talk to enough people over a long enough period of time, you get a pretty fair picture. You get to the point where you're 90 percent sure I think this is most likely what happened, even though there may be one or two disparate opinions.

And here we have not gone down that route. We have just been shown this information, been -- not shown, been told this information very, very recently, had not had a chance to look into it, and I think it's a little fast. Because as I said, my problem is not that I take ownership of my model, but that to say there was a -- this -- I said this earlier, that it was this aggressive cleanup just does not seem to be consistent with any model and the ORISE survey data, the independent verification data. It just --

it's simply -- when things don't add up, one has to look at the information and say which of it -- you know, how do we deal with it? And we have not had the opportunity to do that here.

Here is a sudden new piece of information or a report, a secondhand report which is given to us which we have not had a chance to investigate, to consider, just like we do all the other information, the plausibility, the correctness of it. So I would not want to be that hasty, because the fact is that this model or some other -- you know, I'm open to suggestions. There may be another approach. There may be some other assumptions. There may be some modifications possible to it. But a model that is based on site data gets away from all of this -- all of these questions of how many tons of uranium were there? What was the exact shape? What was the exact -- all of that is -- all of these questions are eliminated.

All we need to know is what was the concentration at some period of time and what was the time history, the temporal history of how much was done in each year? Because if it was all done in the last year, it would be very different than if it was all done the first year. But we know how with a reasonable -- as well as we can do, with a reasonable degree of assurance how it was spread out over the various years. And the idea of the model solves some of these dilemmas.

And the fact that the parameters can be put together to give us a ventilation rate which is plausible, I mean, it's not that it's the answer. To my mind it's a plausible upper bound. I think it's on the high side. I think it's probably likely higher than what was experienced, but it's a plausible upper bound that if you adopt something like this or some variant of that, you're not going to appreciably underestimate anyone's dose.

CHAIRMAN ZIEMER: So, John, I think your colleague is saying that he's a little nervous about accepting that right away.

DR. MAURO: Oh, and I understand exactly where he's coming from. I hate to abandon a perfectly good model prematurely, but you know, it sounded like, you know, if we're going to -- now we have two -- unfortunately, Paul, we have two paths. One is can we -- in other words, given that NIOSH would say, well, perhaps we could find a better surrogate. And I'm not sure if they're ready to say that. But if they were, then we'd have to go down that road and find a better surrogate. And that was where we were sort of headed.

However, listen, if we're not -- all I said is if you want -- if you stipulate and say, no, we're going to accept it as true there was an aggressive cleanup, well, of course we have to abandon our model. But Bob

brings up a good point, the point being why are we ready to abandon this model based on what we just heard today? You know, maybe we're abandoning it prematurely. This is really a call that the Work Group has to make, you know, to pursue both lines a little further. You know, are both worth pursuing, not just the surrogate data question?

CHAIRMAN ZIEMER: I think the Work Group has already agreed that they were accepting that the cleanup occurred.

DR. MAURO: Okay.

CHAIRMAN ZIEMER: I'll ask again. John and Josie?

MEMBER BEACH: Yes.

CHAIRMAN ZIEMER: I think Wanda said she accepted that and I do. So --

MEMBER POSTON: Accepting what?

CHAIRMAN ZIEMER: We're accepting that the cleanup occurred.

MEMBER POSTON: Oh, okay. I see. All right.

CHAIRMAN ZIEMER: I mean, one might get into details on how aggressive it was and so on, but I think on that basis we're going to proceed.

MR. RAMSPOTT: Dr. Ziemer, could I add something on cleanup?

CHAIRMAN ZIEMER: No, John, we're trying to come to closure. We've already accepted that it's occurred, so --

MR. RAMSPOTT: I just wanted to correct Dr. Anigstein -- a few months ago -- I'm going to read the transcript note. I'm looking at it.

CHAIRMAN ZIEMER: No, John, we --

MR. RAMSPOTT: Yes?

CHAIRMAN ZIEMER: You don't need to do that. Okay?

MR. RAMSPOTT: All right.

CHAIRMAN ZIEMER: Yes.

MR. RAMSPOTT: Well, it's there.

CHAIRMAN ZIEMER: Okay. So I would like to get something specific from the

Subcommittee on a recommendation for the Board. If you want me to recommend that we ask NIOSH and SC&A to try to collaborate on a better surrogate or a more appropriate surrogate, we can do that. If you want to recommend something else, we can do that.

MEMBER BEACH: Paul, let's be clear that surrogate data is for what time period?

CHAIRMAN ZIEMER: Well, it would first of all have to cover the main --

MEMBER BEACH: The main time period?

CHAIRMAN ZIEMER: -- the covered period because there is a contamination component there.

MEMBER BEACH: Right.

CHAIRMAN ZIEMER: I mean --

MEMBER BEACH: So it would --

CHAIRMAN ZIEMER: -- previously we had already in a sense recommended that we go ahead, or that we --

MEMBER BEACH: Right.

CHAIRMAN ZIEMER: -- recommended to the Board that the covered period -- that we accept the NIOSH approach. That was before this whole issue came up. But this issue covers both periods, the issue of surrogate data. So we would first have to establish it for the covered period. If we can't for the covered period, then that already has implications for the residual period, I guess. Yes. So, I think it would have to be both. It basically, you know, in my mind reopens the whole issue of the earlier period in terms of a recommendation, because we can't recommend to the Board that the SEC be denied for the earlier period if we have this open issue on that component. So I think it's both. Do you agree, Jim?

DR. NETON: Yes, any model that would be developed for exposure handling the uranium would ultimately end up being the basis for some model of the residual period --

CHAIRMAN ZIEMER: Whether it was -- yes.

DR. NETON: There would be no more handling of the uranium material because that's all gone, but it would --

CHAIRMAN ZIEMER: It would be the start of the --

DR. NETON: -- be the basis for the starting point for the resuspension of material in the building.

CHAIRMAN ZIEMER: Right. So you heard sort of a vague suggestion from John. I say sort of vague because --

MEMBER POSTON: Yes, I'm not quite clear. I don't think we've ever done that, this joint approach before. I'm a little --

CHAIRMAN ZIEMER: I think you're -- are you thinking of it more like a technical meeting, or -- I mean, why would they meet apart from the Work Group?

MEMBER POSTON: Well, I want to understand a little bit better what is the

most appropriate surrogate data to use. I want to understand what model is going to be used. You know, we have two models basically. We have David's model and Bob's model. And maybe --

CHAIRMAN ZIEMER: Well, Bob's model will be off the table --

MEMBER POSTON: Yes, all right.

CHAIRMAN ZIEMER: -- if we would agree that the cleanup occurred --

MEMBER POSTON: Yes. Okay.

CHAIRMAN ZIEMER: -- which I think we've agreed to.

MEMBER POSTON: I understand that, but there may be components of Bob's model that may be --

DR. NETON: Well, I think what John was saying earlier, we have to pick a starting point. We have to decide is there some data available that can be used as a starting point to bound the exposure of the workers based on whatever source term type

material was there.

MEMBER POSTON: Yes, I agree.

DR. NETON: And the rest will fall into place after that. The specifics of the -- like the -- as Bob was talking about, the resuspension model and everything will just fall out from that. But I think it would be -- SC&A and NIOSH would need to agree or should agree at some point that there are data available, surrogate data available that could be used, and what is that? What's the starting point?

CHAIRMAN ZIEMER: Well, let me propose something to the Work Group. At this point I guess I would report to the Board two things: One is that SC&A's review of the surrogate data issue was that they do not believe that NIOSH met the surrogate data requirement, at least on three or four of those issues, certainly on three for sure, maybe on the fourth, number one.

DR. NETON: For the specific value

we propose.

CHAIRMAN ZIEMER: Right. Number two, that we have an extensive discussion about what would constitute an appropriate surrogate, and that in the process of the review and the response by SC&A some additional potential surrogates have arisen that need to be looked and that this covers both periods, the covered period and the residual period, and that we would like NIOSH to -- I think the ball's in NIOSH's court then to specifically tell us whether there is a different surrogate group that they can recommend or not. And once that's done, then I think SC&A would have to come aboard also and say, okay, we'll look at that, too, then. But I think we've all --

DR. ANIGSTEIN: I mean, if we had -- are you saying --

CHAIRMAN ZIEMER: I don't think you do it in -- I think NIOSH does it and --

DR. ANIGSTEIN: Sure.

CHAIRMAN ZIEMER: -- then --

DR. ANIGSTEIN: And then we would then go back and review it against the Board's criteria.

CHAIRMAN ZIEMER: Which means this is going to extend this out, but it does -- it reopens everything for both the covered period and the extended period.

MEMBER BEACH: Or we could go to the Board and just suggest that we vote on an SEC for the early period, which would be --

CHAIRMAN ZIEMER: Right, we --

MEMBER BEACH: -- my vote.

CHAIRMAN ZIEMER: We can --

MEMBER BEACH: Because I think we've gone over and over this.

CHAIRMAN ZIEMER: Right.

MEMBER BEACH: So --

CHAIRMAN ZIEMER: And so, I would simply report that to the Board, that that's what SC&A -- that's what their report was, that this is a possibility, or that the Board

can decide if they want to go ahead and vote now on the issues. And I don't know if this Board right now, if you want to make a specific recommendation or simply have me report that. And certainly at the meeting if -- and Josie is -- you can certainly make the motion. I mean, I don't personally object to having the motion made, if you want to make it at the meeting.

MEMBER BEACH: Right.

CHAIRMAN ZIEMER: Or I'll simply report that, unless you all want to vote here. We can take a vote here on whether or not -- well, number one, is everybody comfortable if I report it that way?

MEMBER MUNN: Yes.

CHAIRMAN ZIEMER: Because I'll just report what we did.

MEMBER MUNN: Yes.

CHAIRMAN ZIEMER: Number two, to give the Board -- say you have an option. If you would like NIOSH to proceed and focus on a

better surrogate, then you can say so. Or you can move to close this and vote for --

MEMBER MUNN: And I would prefer not to vote it, because I understand what you're saying, Josie, but by the same token what we did here today was to agree that reliance on the FUSRAP data was not going to be as desirable as we had originally intended because of the cleanup, the magnitude --

MEMBER BEACH: The potential cleanup.

MEMBER MUNN: -- of the cleanup activities that went on --

CHAIRMAN ZIEMER: Which was new information.

MEMBER MUNN: -- in between that time; this was new information, and has fallen out of the activities that are going on and the exchange of information that's occurred between NIOSH and SC&A during the last activity period.

MEMBER BEACH: Well, and on top of

that I didn't agree with using 1993 data to back extrapolate it --

CHAIRMAN ZIEMER: Anyway --

MEMBER MUNN: Yes.

MEMBER BEACH: -- anyway.

MEMBER MUNN: Yes, so --

MEMBER BEACH: So, regardless.

MEMBER MUNN: -- that is now -- as I understand it generally, based on what the conversation has been here, has been set aside.

The position that I was trying to make and the few comments that I made was that we know an awful lot about the uranium metal and how it operates and all of the proposed concerns that people have with regard to what transpired in this particular site. We know a great deal that has -- we have not relied upon because we have been looking in a different direction to extrapolate information that we did have.

And now we're talking about going

to look specifically for the kinds of surrogate information that is on record for sites that have handled this same type of uranium over a period of something like 50 years. Knowing that information creates a different basis for making the decision. And until we have specifically requested NIOSH to go that direction, we don't have that information. We have it, but it has not been finalized and it has not been placed before us in a rigorous manner. I would prefer to have that done, because we do know a lot about uranium and we can do this. It can be done.

CHAIRMAN ZIEMER: Okay. Other comments? John, are you okay with that as a general report?

MEMBER POSTON: Yes, I agree with Wanda. Wanda said it --

CHAIRMAN ZIEMER: And, Josie?

MEMBER BEACH: I still say that we should bring it to the Board also with the third option of voting for an SEC.

CHAIRMAN ZIEMER: Yes, I would be glad to present that as an option. I mean, I understand that I'm going to report what we did here, indicate that as an option. I think the options are to proceed to have SC&A -- NIOSH -- yes, I know, hard to tell them apart. Yes, right. Have NIOSH proceed and see if they can identify a more suitable surrogate data set. But if the Board wishes, it can proceed to vote yea or nay, up or down, without going any further.

MEMBER BEACH: So will you go further from now, or will you wait until after the Board meeting, Dave, just as a curiosity?

MR. ALLEN: I'll start now. I don't think it will -- you'll have anything before the Board meeting.

MEMBER BEACH: Okay.

CHAIRMAN ZIEMER: Yes, you're not going to have that much time with other things --

MR. ALLEN: I've got other Work

Groups, too, you know?

DR. ANIGSTEIN: And we certainly won't have time to review it.

CHAIRMAN ZIEMER: Let's see. So we're all agreed to proceed on that basis?

MEMBER POSTON: Yes.

MEMBER MUNN: I think that's unfortunate, personally, but then the Board voted with incomplete information before.

MR. KATZ: So is there a need, Paul, for -- so that lays out a presentation by you --

CHAIRMAN ZIEMER: Right.

MR. KATZ: -- to summarize what's been done today.

CHAIRMAN ZIEMER: Right.

MR. KATZ: You need -- I mean, right now we have lined up things as we did with the last Board meeting where SC&A as well as DCAS had opportunities to present, as well as you. Do you want that supporting them at this --

CHAIRMAN ZIEMER: Well, I will summarize what we did. I'm trying to think whether or not we need any further presentations.

MEMBER MUNN: We gave a pretty complete presentation last Board meeting.

CHAIRMAN ZIEMER: Yes, I think they can be available for questions. I don't know -- and I think the Board has all been kept apprised of all the documents, including the petitioners' and our documents as well. So the Board should have a pretty complete set of --

DR. ANIGSTEIN: So, we don't need to go.

MR. KATZ: No, no, I think you need to be available at least --

CHAIRMAN ZIEMER: At least by phone.

MR. KATZ: -- by phone. You need to be available to answer the questions of the Board.

CHAIR ZIEMER: But I mean I don't think you need to make a presentation.

MR. KATZ: You don't need to make -- that's what I was trying to get clear. So if you don't need -- you don't need to make a presentation and --

DR. ANIGSTEIN: I see. But I already have my ticket, so should I go, or just cash it in and --

MEMBER POSTON: Sure. Good skiing.

DR. ANIGSTEIN: I think I got to go.

MR. KATZ: You can choose.

DR. ANIGSTEIN: Okay.

MR. KATZ: It just depends on whether you're comfortable answering questions by phone. But you're not giving a presentation.

DR. ANIGSTEIN: Yes, okay.

MR. KATZ: Okay?

CHAIRMAN ZIEMER: Now, the other

thing we said we would do as we have time, and we're already at the 3:00 hour, I do want to point out just before we leave as far as the matrix or the matrices; there are two of them, but the SEC --

MEMBER BEACH: The last day was June 1st, correct, on that one? Yes.

CHAIRMAN ZIEMER: June 1st was the last update. And just very quickly, I'm telling you you can look -- if you have your report, it's on page 45 of the status summary. Issue 1 had been previously closed. Issue 2 actually -- and all of these are -- the open ones were all transferred to the Appendix BB matrix, but they don't show up specifically on Bob's latest version, which is the July 28th version. They don't show up there. But I'm just telling you that issue 2 of the SEC matrix, as far as I can tell, is part of issue 1 on the other matrix. So if you want to make a note of that and --

MEMBER BEACH: Well, if you go

into the body of these, the end is the ones that he transferred. Like issue 2, it says on 3/28/12, transfer to Appendix BB.

CHAIRMAN ZIEMER: Right, but --

MEMBER BEACH: He didn't write it on the summary though.

CHAIRMAN ZIEMER: It doesn't show up on Appendix BB.

MEMBER BEACH: Right.

CHAIRMAN ZIEMER: But I'm telling you that it --

MEMBER BEACH: So you're going to ask --

CHAIRMAN ZIEMER: -- it's covered by issue 1 of Appendix BB.

MEMBER BEACH: So are you going to ask him to converge these?

CHAIRMAN ZIEMER: Well, yes, I'll make up a chart to do that.

Issue 3 we closed. Issue 4 is closed. Issue 5 is closed. Issue 6 really becomes part of issue 11 of Appendix BB. But

in essence the recommendation was to transfer it. And we hadn't officially closed it, but it, you know --

MEMBER BEACH: And go back to 3. Three was actually transferred to BB, not closed.

CHAIRMAN ZIEMER: Well it says here it was recommended moving it and closing it.

MEMBER BEACH: It was recommended, but we didn't actually officially do that.

CHAIRMAN ZIEMER: Oh, we may have to actually do that. I thought in doing the recommendation that we were closing it, but we can formalize that.

MEMBER BEACH: Not to confuse this anymore than it is.

CHAIRMAN ZIEMER: Right. We have to do the same on issue 6.

MEMBER BEACH: Yes.

CHAIRMAN ZIEMER: Issue 7, it shows it in progress. Essentially we closed

it, but NIOSH has to actually show the update in their new version before we officially close that.

Issue 8 was recommended to be closed, but right now it's simply transferred. It's one of those again transferred and had been recommended that it was closed.

And issue 9 as far as I can see becomes part of issue 6, and that should be closed as well.

The other thing I'll just point out, if you look at the SEC findings, there were no specific findings on the residual period.

MR. KATZ: Right.

CHAIRMAN ZIEMER: So all of these findings have to do with early period. But there are still some items open on TBD-6000 that I think we can close most of. Some of them are fairly straightforward, but to some extent it will depend on our time. We need to focus on this SEC, but I just wanted to make

sure that we're on the same page on the issues matrix. But even though they showed transferred, they haven't shown up as specific separate items.

Okay. Any other questions before we leave?

MR. KATZ: So, Paul, do you want SC&A to amend the matrix accordingly, or --

CHAIRMAN ZIEMER: I think they should amend it, but I want to make sure -- I think I'll interact -- I think I'll prepare a little chart and what I --

MR. KATZ: Yes, I'll wait for you to have -- for your chart.

CHAIRMAN ZIEMER: Right.

MR. KATZ: And then I'll send that along to SC&A.

CHAIRMAN ZIEMER: This is where I think they go and make sure we're on the same page.

MEMBER MUNN: It would be helpful --

CHAIRMAN ZIEMER: And then I'll ask them to update it.

MR. KATZ: That sounds good.

MEMBER MUNN: No, I thought it would be helpful, but this is getting awfully cumbersome.

CHAIRMAN ZIEMER: Right. Well, see there's a lot of these findings that look similar but they're worded slightly differently. So there's a fair -- there's always overlap between SEC issues and TIB issues.

Okay. With that we stand adjourned. Thank you. Thank you everyone on the phone for your input today and we'll hope to hear from you all at the full Board meeting. You'll have an opportunity again to have input there.

MEMBER MUNN: Oh, you're not going to make an effort to schedule another meeting at all?

CHAIRMAN ZIEMER: Well, yes.

MEMBER MUNN: I mean, not knowing what we're waiting for.

MR. KATZ: I think it makes sense to wait.

CHAIRMAN ZIEMER: I think we need to know where -- yes, what --

MEMBER MUNN: Who's on first.

CHAIRMAN ZIEMER: -- action the Board takes, number one. And then we'll find out what NIOSH's stand on it is.

MEMBER MUNN: Timeline for that, yes.

CHAIRMAN ZIEMER: I think we're going to be talking probably into November before we --

MEMBER MUNN: I would imagine so.

CHAIRMAN ZIEMER: Yes, we're adjourned.

(Whereupon, the meeting was adjourned at 3:09 p.m.)