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

convenes the

THIRTY-THIRD MEETING

ADVISORY BOARD ON

RADIATION AND WORKER HEALTH

VOL. I

DAY ONE

SUBCOMMITTEE MEETING

The verbatim transcript of the Subcommittee Meeting of the Advisory Board on Radiation and Worker Health held at the Knoxville Marriott, Knoxville, Tennessee, on October 17, 2005.

C O N T E N T S

October 17, 2005

TRANSCRIPT LEGEND

The following transcript contains quoted material. Such material is reproduced as read or spoken.

In the following transcript: a dash (--) indicates an unintentional or purposeful interruption of a sentence. An ellipsis (. . .) indicates halting speech or an unfinished sentence in dialogue or omission(s) of word(s) when reading written material.

-- (sic) denotes an incorrect usage or pronunciation of a word which is transcribed in its original form as reported.

-- (phonetically) indicates a phonetic spelling of the word if no confirmation of the correct spelling is available.

-- "uh-huh" represents an affirmative response, and "uh-uh" represents a negative response.

-- "*" denotes a spelling based on phonetics, without reference available.

-- (inaudible)/ (unintelligible) signifies speaker failure, usually failure to use a microphone.

P A R T I C I P A N T S

(By Group, in Alphabetical Order)

BOARD MEMBERS

CHAIR
ZIEMER, Paul L., Ph.D.
Professor Emeritus
School of Health Sciences
Purdue University
Lafayette, Indiana

EXECUTIVE SECRETARY
WADE, Lewis, Ph.D.
Senior Science Advisor
National Institute for Occupational Safety and Health
Centers for Disease Control and Prevention
Washington, DC

MEMBERSHIP

ANDERSON, Henry A., M.D.
Chief Medical Officer
Occupational and Environmental Health
Wisconsin Division of Public Health
Madison, Wisconsin

DeHART, Roy Lynch, M.D., M.P.H.
Director
The Vanderbilt Center for Occupational and Environmental Medicine
Professor of Medicine
Nashville, Tennessee

GIBSON, Michael H.
President
Paper, Allied-Industrial, Chemical, and Energy Union
Local 5-4200
Miamisburg, Ohio

GRIFFON, Mark A.
President
Creative Pollution Solutions, Inc.
Salem, New Hampshire

MUNN, Wanda I.
Senior Nuclear Engineer (Retired)
Richland, Washington

PRESLEY, Robert W.
Special Projects Engineer
BWXT Y12 National Security Complex
Clinton, Tennessee

ROESSLER, Genevieve S., Ph.D.
Professor Emeritus
University of Florida
Elysian, Minnesota

STAFF/VENDORS

LASHAWN SHIELDS, Committee Management Specialist, NIOSH
STEVEN RAY GREEN, Certified Merit Court Reporter

AUDIENCE PARTICIPANTS

ADCOCK, DAVID, Y-12
ANDERSON, LYNN, SRS
BEATTY, EVERETT RAY, SR., FERNALD ATOMIC COUNCIL
BEHLING, HANS, SC&A
BEHLING, KATHY, SC&A
BOGARD, RHONDA, Y-12
BROEHM, JASON, CDC WASHINGTON OFFICE
BRYNESTAD, ASTRIS, Y-12
CLELAND, REINA
CROWE, BILL, X-10
CROWE, ED, X-10
DEHART, JULIA
DUNCAN, ASPASCIA, CLAIMANT SPOUSE
DUNCAN, THOMAS N., BWXT Y12
ELLIOTT, CHARLES C.
ELLIOTT, CONNIE, ENERGY RESERVE CENTER
ELLIOTT, LARRY, NIOSH/OCAS
FITZGERALD, JOSEPH, SC&A
HALL, HUBERT T.
HALL, LAWRENCE R., X-10
HENSHAW, RUSS, NIOSH/OCAS
HINNEFELD, STUART, NIOSH
HOMOKI-TITUS, LIZ, HHS
HOWELL, EMILY, HHS
HUMPHREYS, IDA, NIOSH
ISHAK, LAURIE, OCAS
KATZ, TED, NIOSH
KERR, ,GEORGE, KERR CONSULTING
KOTSCH, JEFF, DOL
LAWSON, J.H., BWXT-Y12/ATLC
LAYMAN, RANDY D., CLAIMANT
LIPSZTEIN, JOYCE, SC&A
MAKHIJANI, ARJUN, SC&A
MAURO, JOHN, SC&A
PIERCE, WILLIAM, X10-Y12-K25 K33
PRESLEY, LOUISE S., WIFE OF ROBERT PRESLEY
ROBERTSON-DEMERS, KATHY, SC&A
RYAN, MAXINE, Y-12
SAMPSON, BOB, GAO
SCHAUER, DAVID, NCRP

SIMS, JOHN, IGUA
SMITH, KAREN D., Y-12
SNELLINGS, HOWARD
TOOHEY, RICHARD, ORAU
WALKER, ED & JOYCE, BSAG
WOODY, RUTH, CLAIMANT
WORMSBY, PATSY E.
WRIGHT, BETTY R., Y-12
ZIEMER, MARILYN

P R O C E E D I N G S

(10:00 a.m.)

WELCOME AND OPENING COMMENTS
DR. PAUL ZIEMER, CHAIR
DR. LEW WADE, EXECUTIVE SECRETARY

DR. ZIEMER: Good morning everyone. I'm going to call the meeting to order. This is the subcommittee for dose reconstruction and site profile reviews. This is not the full Advisory Board committee meeting. I hope everyone's aware of that. The subcommittee, the way this board is structured all members of the board are permitted to participate in the subcommittee meetings. So you have a good number of board members are actually here this morning although it is actually a subcommittee meeting.

Anyway, we welcome you to Oak Ridge. I feel like I can actually welcome people to Oak Ridge since this is kind of a homecoming place for me. Marilyn and I spent the first year of our marriage -- I say Oak Ridge, welcome to Knoxville which is a suburb of Oak Ridge. But Marilyn and I spent the first year of our marriage in Oak Ridge, so it's kind of homecoming, old stomping grounds. We've always loved this area. We love coming back here to the general area and Smokey Mountains and surrounding lakes and so on.

We have a rather diverse agenda for the subcommittee. It includes things ranging from Bethlehem Steel in New York all the way to Rocky

Flats, Colorado, and things in between. So a number of items that we will be discussing some of which will be updates, some of which will help us define some of the issues relative to NIOSH's reports and the reviews by our contractor.

Before we get into the main agenda, we do have a few business items and things to take care of. Let me begin with the usual things, and that is to remind everyone to register your attendance with us. There's a registration book out in the corridor. If you've not already done so, please do that at the break. This is everybody, board members, staff people, members of the public.

Also, if you are a member of the public and wish to make public comment during the public comment session which will be a full board session late this afternoon, please register for that as well in the appropriate spot there in the corridor.

Our designated federal official is Dr. Lewis Wade, and he's going to make a few remarks at this time, Dr. Wade.

DR. WADE: Thank you, Paul.

Well first, let me welcome you on behalf of the Secretary and the Director of CDC and John Howard, the NIOSH Director. We appreciate your service.

It's a wonderful time to be alive. It's a wonderful time to be here where we are enjoying this beautiful weather and doing this most important business. I think this board has shown its ability to take on unbelievably difficult issues and work them through with quality and dignity. And I thank you very much for that. It's been a pleasure to be associated with the board.

I will report to you that Secretary Leavitt signed the designation of the later years of Mallinckrodt on Friday and has sent it on to Congress. So a long ordeal I know for you and for many, but it's been brought to fruition with the Secretary following your recommendation and sending that recommendation on to the Congress.

A couple of things I'd like to talk about, and I'll talk about them again tomorrow as we sort of sit here now and look forward. We have a board meeting scheduled for the end of January. At that board meeting we very likely will be looking at SEC petitions for Rocky Flats and also for the later years of Y-12.

As I said, we look forward to January. On that agenda we're very likely to have SEC petitions in front of the board for action for Y-12 and for Rocky

Flats. I don't have to remind any of you of the pain we went through in terms of Mallinckrodt when we were trying to do a site profile review at the same time we were trying to do an SEC petition review.

I think it's critical, therefore, that as the subcommittee and then the board looks forward towards the end of January that we put in motion everything we need to have done to bring us to a position where you can make a decision on the SEC petitions for Y-12 and Rocky Flats at the end of January. We've got time between now and then. We have good people doing work for us, and I think it’s terribly important that we imagine a strategy that will bring you to late January and position the information you need to make those most important votes.

We also have in front of us a longstanding issue relative to the Bethlehem site profile or Technical Basis Document. Again, it's been a long and arduous journey that's taken us to this point. We need to think hard about those actions both today and tomorrow.

And then we can't forget there are many other things that are sort of sitting without direct attention, and we need to put our minds to those. There are petition, there are reviews of site

profiles for Hanford and the Nevada Test site, and we'll talk about those through this week. But there's much work for the board to be done.

So I congratulate you on the work you've done to this point. We've come through some very difficult issues with regard to SEC petitions for Iowa and Mallinckrodt, but there's still a great deal to do. And I think we need to take the lessons learned, particularly from Mallinckrodt, and apply them to the things that are in front of us. Thank you.

DR. ZIEMER: Thank you.

On the agenda you have an item called approval of minutes, and we're going to defer action on those since the copies are not to you at this time. We will pick that up later in the meeting.

CONFLICT OF INTEREST
MS. LIZ HOMOKI-TITUS

There are a couple of other sort of housekeeping items, however. The first has to do with conflict of interest, and we're going to call on counsel to give us the latest views on that. So Liz is approaching the mike now, and Liz, please define what the issue is for the group, and what the Department's position is.

MS. HOMOKI-TITUS: Thank you, Dr. Ziemer.

Good morning. I just wanted to let you know as

far as the site profile reviews and recommendations that you all will be making to the Secretary, the Department has determined that it is very important that we have the comments of board members who may have expertise at those sites during the discussions. But we ask that each board member who has a issue with a site or has worked at a site as it's listed in your waiver letters, recuse yourselves from approving and voting on recommendations that are going to the Secretary. So again, we ask that you remain in the discussions and provide your expertise, but since those site profiles will affect dose reconstructions and SEC reviews that could affect you from those sites, we ask that you recuse yourselves from voting on those issues.

DR. ZIEMER: Thank you.

Board members, are there any questions on that? And just let me ask one, Liz, for clarification. For example, Y-12 as a site, this means that Mr. Presley could be involved in all of the discussions on that, but if we had a specific action, either on the recommendation on the profile itself or on an SEC petition dealing with that site, then he would recluse himself from a vote. Is that correct?

MS. HOMOKI-TITUS: That's partially correct. For

the site profiles he can participate in the discussion but not vote. For an SEC petition, he would have to recuse himself from the whole thing.

DR. ZIEMER: From the whole discussion.

MS. HOMOKI-TITUS: Right.

DR. ZIEMER: So site profiles the discussion is a different level of concern than --

MS. HOMOKI-TITUS: Right.

DR. ZIEMER: And then we will also at some point need to determine what it means, for example, for the Chair who officially worked for Union Carbide at X-10 but spent a week at Y-12, what that means also. And you don't have to answer that now, but I will need to determine that.

MS. HOMOKI-TITUS: Right, well, we can discuss that.

Yes, ma'am.

MS. MUNN: I noticed in observing my own documentation that my only listing did not include this full site. It only included the portion of the site where I had been employed. So rather than the Hanford site, my documentation indicates Westinghouse Hanford Company which did not come to the site until 1970 and did not, and I did not come to the site until 1977. So...

DR. ZIEMER: Perhaps what we need to do as individuals, if you have questions as I did and Wanda does and maybe others do, we can work these out with counsel individually.

Go ahead if you have an answer.

MS. HOMOKI-TITUS: No, I don't have an answer. We'll have to look at your specific waiver. Again, it may be best that you participate in the discussion since you have expertise but recuse yourself from voting just so there's no kind of outside looking in saying she has a conflict there. Why is she voting.

DR. ZIEMER: Thank you.

Then we also have another item that we wish to bring before the group. It's from the Congressional liaison person for NIOSH, but he'll be back a little later, and we'll pick that up at that time.

BETHLEHEM SITE PROFILE REVIEW

Then we're ready to begin our deliberations on Bethlehem Steel, the Bethlehem site profile review. And to get us up to date we have two presentations. We have the presentation from the board's contractor, SC&A, and then we will have a presentation and response by NIOSH and Dr. Neton. And so we'll begin with John Mauro for SC&A.

SC&A PRESENTATION, DR. JOHN MAURO

Welcome, John. And I think we have, yes, John's presentation, board members, is in your booklet, and I believe there's copies for the public on the table if you need to get a copy for yourself.

DR. MAURO: Good morning, everyone. I've very happy to be here in the sunshine. Those of you who live in the northeast, I would say for at least two weeks, maybe three weeks, we had six inches of rain that didn't stop. It stopped yesterday but flying in here today, I looked out at all the green and lush. Feels good today. I feel like I came back to life.

Okay, Bethlehem Steel. Let’s see if we can get the next slide going. There's some history here on Bethlehem Steel, and I thought it might be a good idea to sort of go back very quickly and come up to date. As you recall, there was an original NIOSH site profile for Bethlehem Steel that goes back to about 2003. I'm not sure of the exact date, but it was the first one, Rev. zero.

When that report came out, we performed the review, the limited review about a year ago, about October 2004. And there are many issues we raised, but when all is said and done, our main concern as you recall is NIOSH adopted an approach where they used what was called a triangular distribution, data

that they acquired from Simonds Saw. And it basically said well, we're going to use one size fits all, so we're going to say that it's a triangular distribution, starts at 0 MAC, Maximum Allowable Concentration, which is 70 DPM per cubic meter, goes to a peak at 2 MAC and then goes all the way out to 1000 MAC, big triangle.

We reviewed that, and I guess we walked away with the feeling that, you know, we have a problem with that approach on several levels. And this came out in our critique of the site profile. We felt that when you look at the data, the Simonds Saw data, it sure looked like, pretty close to a lognormal distribution. And when you take the approach that is used at triangular distribution, and you sample from it, the way we looked at it is by sampling from the entire distribution, one could argue well, that's really not necessarily claimant favorable. It' sort of claimant neutral.

Claimant favorable would be, okay, you took the distribution maybe, and then you pick off the high end. So okay, if you're going to have one size fits all, you really want to pick the concentrations that are more toward the high end, especially if you don't know really where a worker worked. So you say, well,

we had the full distribution of, going from zero to let's say 1000 MAC, but the problem is if you don't really know where the worker worked, it's possible that he could have worked in a high end location.

And what we did find out since that is that workers did, in fact, work at particular locations the whole time. In other words there were workers that worked at the rollers, and that's where they stayed, and they worked there those ten hours. By the way if you recall, they worked on weekends. The approach is every one weekend a month for two years or perhaps four years.

So the concern we had was, in essence, with the original report. The triangle distribution really wasn't the correct distribution to represent the data. And second, given that you could construct a more realistic distribution, perhaps lognormal, if you're going to go with one size fits all, you've really got to go with some high end number, maybe a 95^{th} percentile value as a one size fits all rather than sampling from the whole triangle which is really claimant neutral.

Now appropriately NIOSH pointed out, but wait a minute, now the Simonds data is very, very conservative as applied to Bethlehem Steel because

there's lots of reasons, and they're listed in detail, of why Simonds was a very, very, I guess, conservative representation. And it mainly had to do with the salt bath versus the lead bath versus no bath. When you have a salt bath which is what Bethlehem Steel used at least in the later years or later year, you really reduce the potential.

So the original approach -- now, you've got to keep in mind the original approach was use Simonds' data, construct triangle distribution and sample from it one size fits all. And we gave our criticism on that basis. Let's move on now. In fact, let's keep going.

Then Rev. two came out from NIOSH basically reacting to some of these comments. And Rev. two did something which basically got rid of the triangular distribution and went toward the lognormal. It took the Simonds' data, took all the data, fit it to a lognormal distribution, and plucked off the upper end, 95th percentile, which was 550 MAC, okay? That was a big change, a very claimant favorable change. And they said we're going to use that number. We accept SC&A's criticism about the triangle. We accept the idea they really should pick a high end. So they went ahead and did that.

But then they did something else, and this was a little bit, this is where things start to get interesting. They say well, we're going to use Simonds Saw data for 1949-1950 and pluck off the 95th percentile, 550 MAC, one size fits all for those two years. But for 1951 and 1952 we're going to use Bethlehem Steel data. We're not going to use Simonds Saw data; we'll use Bethlehem Steel data, and we're going to do the same thing.

We're going to take that data from '51 and '52, throw it in the pot, fit it to a lognormal distribution, pluck off the upper 95th percentile from that, and use that as a one size fits all for 1951 and 1952. That concentration turned out to be 20 MAC. So what we have now is as of the Rev. two to the TBD was really a two-step process now. We use 550 MAC for 1951, 1949 and 1950 and 20 MAC for '51 and '52.

Now there was a lot about that that troubled us. In fact, we submitted a report on September 27th, relatively recently, which was our critique of that work. It took some time for us to get back and to look at that issue. And our concern with that -- well, let me go through a process that took place.

We issued a report on the 27th. Then there was a

series of interactions whereby once we issued that report, and in effect, the report said the following: Our report said when we look at the Bethlehem Steel data that's available to us, we've got some problems with it. In other words, we're a little concerned that you went from using solely the Simonds' data, now we're using Simonds' data for two years and Bethlehem Steel data. So we took a really close look at the Bethlehem Steel data, and we had some problems with the data.

The problems with the data really stemmed from the fact that the data for 1951 was illegible. So there was a lot of numbers for '51 that we couldn't, we didn't know what they were. We also looked at the data from the point of view of it looks like the data that you do have is heavily general air samples and light on breathing zone samples. And we also noticed that when you do make, we look at the breathing zone samples versus the general air samples, it looks like there really isn't much difference between the two.

Now if you look at the Simonds' data, and you look at the breathing zone samples, and you look at the general air samples, you consistently see a large difference whether you look at the highest values; you look at the average values, look at the 50

percentile, median values. Whatever values you look at the breathing zone samples collected from Simonds are always higher than the general air samples at Simonds. But that's not what we saw in looking at the Bethlehem Steel data.

So we had two problems with the Bethlehem Steel data. The problems being we really couldn't read the data for 1951, which was an important year because that was the transition year where they were installing the new technologies to try to lower the dust loading. And so we're missing some data, and this ratio just didn't make sense to us. That is, why is it that the breathing zone samples are not substantially higher than the general air samples?

Well, so that was our reaction. Well, what happened was -- and that was our report on September 27th. The next thing that happens is October 4th, NIOSH was able to track down what they called the onion-skin original sheets that really were prepared back in 1951 where you could read the numbers. So we got a hold of the numbers; NIOSH got the numbers; we got the numbers, and you could read the numbers. Aha! A big problem with the Bethlehem Steel data that we had where we were missing all these 1951 data, we have them now. That's important.

So now we've got to regroup and say, well, wait a minute. We've got a lot of data now. Maybe we could use it to see what we can do with it. Let's work with it. We had a meeting on October 6th. We had a working group meeting. Mark was there and others were there. I didn't make it. I was out. I had the flu.

But then, and now remember now, we had these data as of October 4th. And if you look at one of these data sheets, you'll see there's a whole list of numbers and right next to each number, the air concentration, it gives you the location that it was taken from. Our crew sat together and said well, what do these locations mean? I mean, let's let the data speak to us. What do they tell us? We couldn't figure it out, so we don't really know what each of these little identifiers mean.

So Arjun, on Sunday on the -- what date was that, the Sunday? The ninth? The ninth -- flew up to Buffalo, met with Ed Walker and two of his coworkers who worked at the facility for many, many years. And they sat down and they talked through each label. What do these labels mean, the shearer, a whole bunch of different names of, that are given to where the sample was taken. So we're starting to get an

appreciation what the data say and where they were taken. Let's keep going.

Now so we got to the point now I think we understand what the data are. And given that we have these data now, and we consider now a complete set of data. We understand them, in fact, we actually made a drawing. We sent out our report on Friday afternoon electronically.

I realize it was like the eleventh hour, but our report, a letter report would go out electronically. And we actually had the drawing back there. What was done. This is the first time I think, anyone made an attempt at trying to sketch out what Bethlehem Steel looked like. And that was done, we really have to thank the folks that Ed Walker, his buddies, worked with us to lay out what does this place look like.

DR. ZIEMER: Could we identify, is the drawing called Bethlehem Steel Ten Inch Bar Mill line diagram?

DR. MAURO: Yes, that's correct. And that's Figure 3-1 in the letter report dated October 14th, from SC&A to Dr. Ziemer.

DR. ZIEMER: Right, thank you.

DR. MAURO: Okay, now where we are with this report now. Now it's caught up. There were

originally six issues, if you recall, that were on the table. One of them has gone away. The one that went away is the oronasal breathing issue. And the reason that went away is NIOSH made a very compelling case that the uncertainty introduced associated with having to model mouth breathers as opposed to nasal breathers, or oronasal breathing, the uncertainty introduced by that and what it means is very, very small compared to the uncertainty in the distribution of the concentrations in the dose.

So it's almost like it's not important. It might be important at other sites so, in effect, that at this site we could sort of put that one on the shelf. It's not important. That doesn't mean we don't have to revisit it again, but because it becomes a, I refer to it as a policy decision.

There's no doubt -- just as a quick aside, there's no doubt if you assume a person's a mouth breather and there's a large fraction of the U.S. population that are mouth breathers, that you're going to get a different dose than if you breathe through your nose or breathe through you nose, oronasal breathing, when you start to work hard.

So whether or not mouth breathing is part of a model that should be factored in, that's really a

policy call. And but there's no doubt, everyone would agree, we agree that there's a large segment of the U.S. population that are mouth breathers. And we also agree if you're a mouth breather, your dose to your lungs are going to be somewhat higher, maybe a factor of two or so -- we did the numbers -- than if you were just a regular breather like everybody else. So it becomes a policy decision whether or not that should be explicitly included in doing dose calculations or not.

But for this particular issue it becomes a non-issue because it really is not an important contributor to the uncertainty in the dose calculation. There are other issues that are by far dominate or contributes to the uncertainty.

So we're left with these five issues. I'm going to spend most of my time talking about the first issue. We'll quickly go into the other four and that'll take five minutes. We're not very far apart. In fact, we're not very far apart at all on the second to the last four bullets there where we have come at it a little bit different. We think our approach is a little bit more scientifically robust and a little bit more claimant favorable. But certainly it is not very far away from the position

that NIOSH has taken.

The place that we are still very far away on is the first bullet, and that has to do with the following:

As I understand it -- well, let's start where we agree. The data that came in where we were able to see legible data for 1951, it's clear now that there's a very significant difference, perhaps a tenfold difference in the concentration distributions between the 1951 Bethlehem Steel data and the 1952 Bethlehem Steel data. I think we have agreement on that. I think both NIOSH and SC&A agree, yes, there is a difference there.

So the idea that we have to parse it one more time, say, in other words, we started off there was a one size fits all. Took the Simonds Saw and at the very beginning and applied it across all four years. Now we moved into a mode where we say, well, we've got the first two years, '49-'50, which is Simonds Saw and then in one of the reports then there was a second set of data which was the 20 MAC data which is '51-'52.

Well, we've moved away from that I believe. Now we're at the point where we're saying, well, we need something for '49, '50 Simonds Saw. We need

something for 1951, and we need a separate one for '52 because there's a big difference in the Bethlehem Steel data between 1951 and 1952. So we've got agreement there.

Now here's where we've got -- item number two. Here's where the difference of opinion exists. We believe that you can't take the 1951-1952 Bethlehem Steel data on face value, and we're prepared to discuss this with the working board today.

I'm looking forward to Jim and his position because I realize Jim doesn't agree with this. See, we say look at the 1951-1952 Bethlehem Steel data, and we say there's a paucity of breathing zone samples. Unlike the Simonds Saw data where that wasn't the case. NIOSH would like to take all of the '51-'52 data, put it in a pot, plot it out, semi-log, plot it on a lognormal distribution and pick off the upper 90 percentile. Or alternatively, take the 1951 data and take, separately from the 1952 data, and plot each on log paper and pluck off the 95 percentile from 1951 and a separate log for '52.

That would be an approach that's very compatible and consistent with what was done with the Simonds' data. The only problem we have is when you look at the Bethlehem Steel data, you see a paucity of

breathing zone samples which tells us that they didn't catch the full range of exposures.

So we believe that where the action is in terms of protecting the workers, where the workers got the most exposures, were at the roller locations. This is where the dust was generated, okay? So we're saying that we need breathing zone samples from the roller locations for '51-'52 for us to start to get a, to feel confident that we can bound it.

In an ideal situation if we had lots of breathing zone samples from roller locations in '51-'52 from Bethlehem Steel, plucked off the 95th percentile for those, I think we've got it knocked. But we don't have that. We have general air samples for those locations. So we think that, yeah, you've got really good data for '51-'52, but you don't have enough breathing zone samples from the roller locations because that's where I think the action is.

So we came up with the idea that perhaps you can take the data, general air sample data from the roller location for Bethlehem Steel, '51-'52, and apply an adjustment factor. To say, okay, if we were to take, if they did, in fact, take breathing zone samples when they took the general area samples they also took breathing zone samples in '51-'52 from the

roller locations, we'd probably see numbers that are a little bit higher.

And we believe that the numbers would be higher by about the same factor of the ratio that we saw at Simonds Saw because they did have breathing zone versus general air. And there was a several-fold difference. It turns out the numbers ran from, I think, a low of five to a high of twelve-fold difference but consistently different. So we're saying, okay, let's use the Bethlehem Steel data, but I think we need to tweak it a little bit. We've got to make an adjustment for the paucity of breathing zone samples.

Finally, this slide, I really told my whole story by looking at -- we're going to back up a little to reinforce some of the points I've made. This plot gives you -- we call it the period one, period two. I refer to it as 1951, 1952 data plotted for the air sample. You could see that the red line which represents period one, '51, is substantially higher, especially when you go out toward the higher end of the distribution to the right-hand side. So there's really two different populations of numbers here. These don't come from the same populations. So this is the rationale which I believe, NIOSH agrees, we

need to make a distinction between those two years.

This is, again, points out that, the important point is we're looking at the breathing zone samples from Bethlehem Steel. You'll notice as we go down on the right-hand side, it gives you a number of samples. Then you look at the right-hand side, you'll see no breathing zone samples, no breathing zone samples. It's a recurring theme, and that troubled us.

This is another breakout of the data, but in this case we show different percentiles. That is, how the plots, it's the same way to show there was a difference by period between '51 and '52. It gives a little bit more richer breakdown of the data, but it makes the same point. These two years are different years.

We also know from reading ICRP 75 that we would expect to see a substantial difference between breathing zone and general air samples. So between the Simonds' data where we really see it consistently, a difference, and then, of course, there's the generic literature ICRP 75 that says you're going to see, you should expect to see differences, on that order by the way. You know, five to ten or perhaps higher differences, not as the

literature says.

So we walk away with the sense that if you're going to use the Bethlehem Steel data, you've got to do some adjustments in order to correct for what we consider to be a paucity of breathing zone samples especially in the vicinity of roller stands. But I think an important point is that we're zeroing in on that population group, the folks who worked at the rollers. Those are the people you want to make sure you understood and what kind of exposures they received.

The second period, the, it turns out when we -- just as an example, if we take the '51 Bethlehem Steel data, and we take the general air samples in the vicinity of the rollers, and we multiply the general air samples by eight which is the ratio that we saw at Bethlehem Steel and which is a ratio that's not inconsistent with ICRP 75, lo and behold, the upper 95th percentile concentration is 540 MAC, basically the same 95 percentile concentration that we saw as the upper bound at Simonds Saw.

So in effect what we're seeing here is it looks like the dust loadings in '51 in the Bethlehem Steel data are really, at the upper end concentrations, are really not that different than the concentrations

that were seen in Simonds Saw. But then when we do the same thing for 1952, we take the same batch of data, Bethlehem Steel, take the general air samples, multiply by eight, get them, get them up. So now you say now we've got something representing free breathing zone samples. And we take the upper 95th percentile, we come up with 22 MAC.

So in other words, there is, so where we're coming out is -- and we're not saying those are the right ratios, but we did that to explore, see what would happen if we just went with that. But we do think that something has to be done with the data. So we come out that we have three different time periods; we use Simonds Saw 540 MAC or 550 MAC, 1951 Bethlehem Steel data where you have a number which is probably going to be on the order of hundreds of MAC. Maybe not 540 but certainly not 20.

Then we move to '52 where we're saying sounds like about 20 might work which is the number originally they wanted to use for '51 and '52. So the real difference is I think we're coming down in a different place for 1951 Bethlehem Steel data. What's the right dust loading to use for that year?

And I think that we're coming in, if you adjust the breathing, the general air samples of '51 by some

appropriate factor, you're going to come in with a number that might be somewhere in the vicinity of 500 or 400 or 300, you know, depending on what you justify. And we didn't take the analysis that far, recognizing we didn't run these numbers until we got the data back on October 4th.

So but I think we have a theme that says I think we've got a handle on this problem. We've got a tractable problem. And the difference between NIOSH and SC&A is that we think some adjustment is needed on those data, and we'll hear from Jim about why, perhaps, we don't. But right now I haven't heard the rationale, the why we don't have to make an adjustment factor. The data telling me, yeah, we need an adjustment factor, and I haven't heard that rationale, but perhaps we'll hear some more about that.

Another point that's important is there is actually a generic TIB for AWE facilities. And interestingly enough it recommends 100 MAC as your default value. In other words, if you're going to do an AWE facility, and you don't have any data, go with 100 MAC. So that's part of the soup, so to speak.

So we've got all these numbers floating around. We're troubled by the 20 MAC that originally, that's

proposed in Rev. two of NIOSH's report for the reasons we gave. One of the reasons also is that the generic TIB itself argues for 100 MAC as being the default value when you don't have data. So that's sort of part of the play.

So now -- I'm just about done. Thank you for the amount of time you're giving me here. I want to move on to the other, a couple of other issues. If you remember we made a big fuss over cobbles, you know, cutting up cobbles, lots of airborne dust. And we learned something. We learned something on our visit on the ninth with Ed Walker and his friends. We found out that when the rods were going through the rollers, and they cobble up into spaghetti, the guys working the rollers, they don't get involved in that.

See, we had this thing in our heads, you make these models in your head that the guy is working the roller and this dust is coming up. Cobble hits, he has to go in there now and cut the cobble making all sorts of fumes which the concentration’s going to be real high, so he's getting whacked by the dust loading from the rollers when things are going well, and then when he has to go in and cut the cobbles. Well, it turns out it doesn't work that way.

The way it works is there's a crane that comes,

whisks the cobble out, puts it over far away from where the roller operation is, puts it on a lay down area, and someone else takes care of the cobbles. So it's not an additive thing. It's almost like a different crowd that's going, he'll get exposed. Now we don't know how they cut those cobbles. We still don't know. We don't know whether they used some kind of shear or snipper, or whether they did cut it with an acetylene torch.

Jim made a great point at our working group meeting, they can't cut that stuff with a torch. It's, you know, it's pyrophoric. I don't know. We made some inquiries. We spoke to Ed Walker and his friends if they had any experience, personal experience and seen how it was done? The answer is no information.

But you know what? We've sort of put that on the margin right now to a certain degree because it's a separate crowd of people, and yeah, they'll get their exposure from cutting up cobbles however they did it, but it's not the same people that are at the rollers. And right now it looks like the rollers are the guys that we're worrying about. So if you're going to have one size fits all, it's the roller guys.

I need to keep going. I'm almost done. We had a

couple of other issues that had to do with ingestion scenario, the dust ingestion, and also resuspension that goes on all the time. Both those, these are different than the air inhalation scenarios where that inhalation from direct puff coming up while you're rolling or cutting is happening only on the days when you're rolling.

But the ingestion scenario and the resuspension scenario in theory could be going on all the time. During the week while they're working with steel, you still have some uranium dust co-mingled in the steel filing dust. So there theoretically was something going on that we just can't ignore. Well, we came up with, NIOSH came up with their approach, and we came up with our approach.

Ingestion, our approach basically starts with the premise that the data compiled by EPA and others is sort of accepted 100 milligrams per day as being, in a dusty environment one would expect inadvertent ingestion of soot on the order of that. Now, the numbers could be higher or they could be lower, but 100 milligrams per day looks like a fairly well documented, as good as you can document these things.

Now our position is okay, so on the day of the rolling the guy is working. He eats 100 milligrams

of uranium dust inadvertently because that's when the rolling is occurring, that's when he's covered. And if he's going to ingest 100 milligrams that day there's probably going to be predominantly uranium dust or filings or oxides, flakes.

However, as each -- now that's on, let's say that's a Sunday. When Monday comes along they're not rolling uranium any more, they're rolling steel, and Tuesday they're rolling steel and Wednesday. So what happens is the approach that NIOSH took was okay, but one day out of 30 is uranium and the other 29 days is steel. So if you got 100 milligrams a day, we'll take 100 milligrams, divide by 30, you come up with 3.3 milligrams per day as your average ingestion rate.

We took a little bit different tack. We said, you know, it doesn't really work that way. On day one, it's 100 milligrams of uranium. On day two, it's 50-50, 50 milligrams of uranium, the second day 50 milligrams. On day three it's one to three, on day four -- so it's sort of like a summation.

If you do it that way, I think we come in with a number a little bit higher, maybe a factor of two higher, maybe, I'm not sure of the exact number. But the differences are small. But we felt that approach

was more mechanistically true to the process that was taking place and it ended up being a little bit more claimant favorable.

Finally -- no, the next item is resuspension. The approach that NIOSH adopted regarding resuspension, again, now we've got all this dust all over the place, iron and uranium co-mingled together throughout this facility being resuspended. Now what NIOSH did is say okay, let's go figure out how much dust might be in the air from resuspension.

So they went to data from Simonds Saw which is a couple of days after they stopped operation at Simonds Saw. They stopped operation. Nothing going on. They go in two days later. They take out an air sample, the position being any air dust loading we see must be from resuspension because they weren't rolling, and they came up with .5 MAC.

We've got a problem with that because there was nothing going on. No one's kicking around working, stirring up dust, so we're not really comfortable with that. They also validated that .5 MAC by taking what they knew to be measured activity on the surfaces, on the floor and the tables and applied a resuspension factor approach of ten to the minus six per meter, and they came up with a number that wasn't

all that far removed from the .5 MAC.

I've got a problem with ten to the minus six per meter as a resuspension factor. You look into the literature, that's on the low end. In a dusty environment, a place where there's soot, people are working, they're kicking up dust, it's a lot higher than that, at least a factor of ten, maybe a factor of 100 or higher. So right off the bat we're a little bit uncomfortable with the strategy taken for resuspension.

So we came up with a different approach which may be erring on the other side of conservativism now. I'll be the first to admit that. What we did is we said let's go into Simonds Saw, find out what the air sample level is in the general air, not in the breathing zone now, in the general air, and we're going to make a very conservative assumption.

We're going to assume that the levels you generally receive in the general air samples are from resuspension. Now we know that it's really a mixture of resuspension and direct puff, but let's just say it's resuspension. And we're saying, okay, so that's the amount of dust, milligrams per cubic meter, from resuspension. We're going assume that based on general air samples from Simonds. Then we're going

to say that let's assume that's the amount we're going to get from resuspension at Bethlehem Steel. That is from resuspension.

And now we're going to dilute that, that's on the day of the rolling, in other words, if you have your day, rolling is stopped. There's no rolling. Let's assume that at that day the activity that's airborne that we measure at Simonds Saw is, in fact, the dust loading from resuspension from Simonds, okay? Then we're going to say on day two, the next day, you cut that in half because now you're doing iron, and you cut that in half, and you cut that in half, and you keep doing that just like we did with the ingestion.

And what happens is you come up with a different approach. Turns out when you do it that way, that approach -- by the way, in our original report we used the 95th percentile dust loading from general air. That was wrong. We made a mistake. The right answer, not the right answer, a better answer would have been to go with the average or the median which we did. We fixed that.

When you do it that way, and do our approach that way, you come up with a dose from inhalation from resuspension which is about four times higher than the .5 MAC approach that NIOSH came up with. So

we're not that far away.

Now finally, and I'm done, we have some concerns with external exposure contact handling. Jim gave us some very compelling arguments why that's not an issue, and we agree.

The only place that's a little residual is that we're a little concerned that more explicit consideration, not so much for contact where workers actually handled the rods, you know, touched them, picked them up. And the only other thing that sort of stays residual on that last issue is clothing contamination. That is, if there's dust embedded in the person's clothing and sort of stuck on the clothing on non-rolling days, whether or not that's an issue or not. But again, that's not an issue that's going to be important in terms of reconstructing doses, but it's the last place where we have some concerns. That wasn't really addressed.

And I think that's it. And I'd like to thank Arjun, Bob Anigstein and Harry Chmelynski. I mean, they're the ones who crunched the numbers, who worked real hard. I basically sat there listening to the story to make it all make sense to me so I could try to communicate it to you. So I'd like to thank Arjun and the crew for that.

DR. ZIEMER: Thank you very much, John. Let me start a question here, and you can answer or maybe Arjun or maybe Mr. Walker. Do we know where the general air samples were collected relative to the rolling operations? I can imagine cases where general air samples and breathing zone samples might, in fact, be quite similar depending on where the workers were relative to the mills and where the general area samples were collected. Do we know where they were there or at Simonds relative to the workers?

DR. MAKHIJANI: At Simonds though all of the data points are either labeled on the data sheets and where their data points are not labeled either general air or breathing zone, we have the AEC documentation that indicates that the samples that were taken at the rollers that are not labeled either general air or breathing zone were considered by HASL to be breathing zone samples in their calculations of time-averaged data.

DR. ZIEMER: If they were close to the roller.

DR. MAKHIJANI: Yeah, and since they were using them as breathing zone samples and you had NIOSH present some testimony from HASL about that, about the Simonds sampling, in this analysis we simply

accepted that although with some uncertainty with a question mark around it. We accepted that those were breathing zone samples.

For Bethlehem Steel we have four different types of samples that were taken. There were breathing zone samples that are labeled as such and the table of that is in your, in the report. There were general air samples that were labeled as such.

Of the labeled samples, there were only three breathing zone samples that appeared to be at the rollers. They're all associated with the low dust salt bath process it seems to us from September '51. There are no breathing zone samples associated with the lead baths at the rollers which were clearly the dustiest location in the early period.

The unlabeled samples, there are a number of unlabeled samples that are set, taken near the rollers. A lot of them are in the second period, but because we don't have any descriptive documentation or time-averaged calculation, it doesn't appear to be claimant favorable to assume that breathing zone.

This set of samples seems to me, I've poured over the sample descriptions quite a bit, to have been taken in the AEC description itself. At least indicates that these samples were taken with the idea

of understanding the continuous rolling process at Bethlehem Steel which was, you know, experimental. It was new so it wasn't taken, so we have the difficulties arise in interpreting the data partly because they're unlabeled and partly because there's very little associated documentation in terms of calculations.

DR. ZIEMER: So the assumption is that if the sample was taken close to the operation, it probably got labeled as a breathing zone sample. By implication the general air samples were away from the source of generation of the aerosol. Is that --

DR. MAKHIJANI: Well, we don't know how much away. I mean, the general air sample on April 26, 27, 1951, must have been taken fairly close to the rollers because there are quite high dust concentrations. But when you look at ICRP 75, there was quite explicit disc -- on earlier ICRP documents and other documentation. There's quite explicit discussion that samples that are fixed samples, taken in the general air near the working location, can be an order of magnitude or two or there's a magnitude less than a lapel breathing zone sample.

So that's where, it's not that they were taken very far away. We don't think that they were. But

the problem comes in is what's, is there an order of magnitude or more difference or could there have been. And since we don't know, this is a kind of...

DR. ZIEMER: And the ones that were labeled breathing zone samples are not likely to be breathing zone samples in the way we would do it today as far as lapel samplers and so on.

DR. MAKHIJANI: Well, Jim may address this to some extent and we also looked at it. For the Simonds' data I think there seems to be no controversy or difference of opinion between us and NIOSH. In this case we accept the breathing zone samples that are labeled as such as breathing zone samples. However, when I interviewed this one worker who wanted not to be named for privacy reasons, but as I said on a conference call of October 11th with NIOSH -- I just interviewed him on October 9th.

He's very clear. He has excellent recall as you can see from that drawing that he made. He recalls every detail of what went on. I mean, it's really quite extraordinary. He worked in that area for a very long time which is part of the reason. Part of the reason is very clear. He worked at the shears during the period of uranium rolling, and he remembered, he knew at the time that it was uranium.

And he does not remember, he might not have been there on the day the nine shear samples were taken in the breathing zone. So that's one possibility. But he does not remember anybody coming very close to him to take what he would consider that he was breathing although he remembers people sampling in the area. So we have much more uncertainty about Bethlehem Steel data. That's the short of it.

DR. ZIEMER: Wanda Munn.

MS. MUNN: I think I heard in that but just want to be sure that we actually do not know where the general air samples were taken.

DR. MAKHIJANI: Ms. Munn, the samples usually have a description of the general area where they were taken so it will say roller number three or roller number one and so on. Now some of those samples are, or shearer, and some of those samples will be labeled breathing zone, and we assume that they're equivalent to a lapel sampler. The ones that are not labeled we assume they are near, but because there is a big difference between near and a lapel, there's a question of how do you interpret those for calculating intake.

MS. MUNN: In my mind the answer is no, we're not sure just exactly where the general air samples were

taken.

DR. MAKHIJANI: Yeah, we don't know how far they were taken although we know the vicinity in which they were taken.

MS. MUNN: That's what I thought.

DR. ZIEMER: Mr. Walker, can you add anything to that for us?

MR. WALKER: Thank you. You had some, in the interview and you asked the question, I didn't, I know Arjun is very familiar with what we done on that conference call, but you asked where the breathing zone samples were taken in relation to the rollers. I got a set when I asked from Oak Ridge, a breathing zone sample. Well, I got samples, air samples, and the breathing zone samples are something like nine out of ten were taken at the shear.

And that shear actually from the rollers is approximately four to five hundred feet away from the rollers. And the rollers are considered the most contaminated and dusty area. I got a nice set of samples, I believe NIOSH had sent it to me, and I looked at those, and most of them were taken at the run-off. And I can't remember exactly, but like maybe four or five were taken at what they call the run-off which was another hundred feet, or

approximately a hundred feet, beyond the shears. And those were breathing zone samples.

And it's my understanding that there was only two breathing zone samples taken at Simonds Saw. These are the issues that trouble me. I hope it clears it up on just how far away from the basic rollers and the dusted area. I hope that cleared it up a little.

DR. ZIEMER: Thank you.

(Whereupon, Mr. Griffon joined the meeting after which the following transpired:)

DR. ZIEMER: Additional questions or comments? Yes, Mr. Presley.

MR. PRESLEY: Bob Presley. Do we have any data that shows that the areas were cleaned up prior to the rolling? Knowing how valuable this material was in '49, '51, '50, do we have any data at all that shows that the areas were cleaned up prior to the rolling of the uranium on the weekends and then cleaned up again after the rolling was done?

DR. MAKHIJANI: There are two kinds of residual uranium about which there's some pretty clear evidence. One is the crop ends that were not useful were all packaged and taken away in 55-gallon drums.

And then there's the question of scale that fell to the floor and so on. The scale from the scale pit appears so the steel and uranium scale would have been mixed up together, would have been loaded off onto flatcars and taken off. The areas do appear to have been cleaned from time to time in terms of hosing down and so on, the best as we can tell.

DR. ZIEMER: Yes, Roy -- oh, Ed. Ed's got a comment on the --

MR. WALKER: Again, with the information and what happened to the scaling, I have witnesses that I talked to that worked at the plant. And the scaling as it rolled, you can see in the picture from the plant, the dust from the floor, three, four inches thick. That went down and mixed with the steel dust. It was run into a big pit outside and the scale was loaded onto a railroad car.

And one of the claimants, not knowing what I was talking about uranium, he was describing what happened during that time. He worked there at the time. The railroad car took it and dumped it on the site. And the reason that we know it was left on the site is, that we had good reason to believe, because the gondolas that they dumped the scale in, the railroad cars had no air lines on them. Without air

lines on these vehicles, you could not take them onto the main track and out of the plant. So we're sure they were dumped on the site, and I forgot the other question, Bob.

MR. PRESLEY: That's fine.

DR. ZIEMER: Roy DeHart.

DR. DeHART: In mention of digestive calculations, you used a progressive dilution. On Sunday, we had 100 milligrams of uranium and then progressively did over time. However, you didn't use a progressive dilution of the steel that had been processed. You're assuming on Sunday that it's full. That whole 100 milligrams is uranium. Well, in fairness, shouldn't you consider the fact that there would have been steel contamination in dilution as well?

DR. MAURO: That's correct. Ideally, we had to start at a point. We said, listen, how do we start this. The day of the rolling we could say that you already have in place some inventory of iron oxide scale that you're going to add uranium to. We decided that since the uranium during the rolling is going to be falling and accumulating on top of that while you're working, yes, there would probably be some co-mingling and using the 100 milligrams per day

on that day all uranium is conservative.

There's no doubt that there's going to be some co-mingling with pre-existing, but for two reasons, one, we were not quite sure how best to deal with the fact that the stuff is falling on top of it, fresh. So we said, listen, let's err on the side of the claimant and assume that the, of the 100 milligrams, given the 100 milligrams, at least on the day of the rolling, the majority of what they inadvertently ingest is going to be uranium.

Granted it's going to be something less than 100, but we started at that point not knowing how far down to go. What kind of co-mingle you would have. Then from then on, then we did a sequential dilution. So you're absolutely correct. There's a conservatism built in starting at that point that there was 100 percent -- milligrams of uranium on the day of rolling.

DR. ZIEMER: Because the counter argument, of course, is if uranium is 50-50 on the day following, then the day following steel, steel should be 50-50. So you have a factor of two right at the front end, I think, is your argument.

Henry Anderson.

DR. ANDERSON: Yeah, the 100 is basically taken

from the exposure handbook?

DR. MAURO: Yeah, two places, Exposure Factors Handbook and NCRP both recommend that as being a good value, two different places.

DR. ANDERSON: There isn't any general dust measurements made in the facility?

DR. MAURO: No, the ingestion? The amount ingested?

DR. ANDERSON: Well, I mean a hundred --

DR. MAURO: A hundred milligrams a day is a generic, an inadvertent dust soot ingestion, soil ingestion, used widely.

DR. ANDERSON: Yeah, no, I understand that. I just wanted to know. I mean, that's a starting point, and it's as good a guess as any, I guess.

DR. MAURO: Yeah, there are others. For example, in the same report, gardeners, people who, 480 milligrams a day or so. And other places you hear people use 50. So yeah, there's uncertainty there.

NIOSH PRESENTATION, DR. JIM NETON

DR. ZIEMER: We do need to move along here. We need to hear from Jim Neton from NIOSH.

Jim, let's go with your presentation.

DR. NETON: Thank you, I assume I have at least 15 minutes here.

DR. ZIEMER: Oh, yeah.

DR. NETON: I'd like to commend SC&A for their tremendous vigor at which they pursued these investigations. I mean, they're paid to turn over a lot of issues and discover things, and in fact, they do a commendable job at trying to poke holes in some of the stuff that we've done. I think though in fact in some cases they've gone a little bit too extreme, and I'm here to discuss that.

I'm glad to see that we're coming closer, although every time we come closer it seems that a couple more issues pop up. But if you recall -- I'll go back and do a little historical review of my own. If you recall the first review of SC&A, they speculated that there may have been as high as 4000 MAC exposures out there in the plant and people were breathing submicron ten nanometer particles possibly. People were cutting these cobbles out with a torch in the middle of the process. Oronasal breathing could have a significant impact on the dose.

All these issues have sort of gone away at this point. So their original estimates, I believe, were high-sided. I think we agree that our original estimates may potentially be slightly low-sided. However, I'd like to point out that if we ended up

with even SC&A's assertions as they stand now, this 500 MAC per year, an allowance for like 20 MAC in the fourth year, it is not substantially different than the triangular model that we proposed a year ago.

That model ended up giving a worker about 330 MAC effective per year, so 1300 MAC hours, if you will. SC&A's model now would say 1500. We're less than 20 percent apart, I think, on these issues. So the differences are not as great as they sound although conceptually we still have some issues with what they're talking about.

I'd like to address this TIB-0004 issue a little bit as well. TIB-0004 is a profile that is used not to make compensation decisions in both directions, but it's a profile that is used to, if one can make a determination that a hundred MAC is truly a bounding estimate at that facility. And which Bethlehem Steel is not considered one of the facilities it's applicable to, then one can make a determination that that probability of causation is less than 50 percent for that claim. It's a very different issue, and I'd like to separate those two if I could.

If I can go to the first slide, these are the issues that John went over in some detail so I won't go through all of them again. But I will point out

that this first issue here, of course, is the one where we have the most difficulty accepting some of their assertions, and I'll get into that in a little bit.

And actually the first two sort of get in there. I was encouraged to hear that John is now willing to accept the fact that these cobbles are truly, if they did happen and were cut in a different location, and in fact, the concentrations at the rollers may indeed be bounding for the highest exposure or the 95th percentile workers. And that's a major step forward I believe.

Oronasal breathing, I think we've agreed, at least in this case as John indicated, is not an issue for Bethlehem Steel. It's a small contributor, albeit not negligible, but a small possible contributor, and it is something that we need to take up as a policy issue.

I'll just move on, and I'll go over these as I go. The rollings in 1951 and 1952 are indeed different. The original review that SC&A did asserted that we should use, I think it was 500 MAC air at Bethlehem Steel for all four years even though it was pretty clear from the data samples that Bethlehem Steel was, in '51 and '52 at least, in the

business of reviewing or looking at process improvements related to salt baths and lead baths to reduce worker exposures.

And in fact, I think I'd like to clarify a little misconception. John spoke of, Dr. Mauro spoke of 1950, two time periods, those early rollings and late rollings. In fact, the Simonds Saw and Steel rollings that were used from October 27th, 1948, were neither salt bath nor lead bath rollings. They were furnace-heated uranium.

That was the only one that we could find had been done that way. They realized early on that that was an extremely messy process. And in fact, that's where the highest air concentration we've seen between Simonds and Bethlehem occurred. And that was 1000 MAC air at roller stand. Well, the first roller, there really only was one roller where they kind of repetitively moved it through.

But the first pass through of that furnace-heated uranium that had a lot of oxide scale on the outside is where we observed these processes, at least this high sample. Because of that we think that Simonds is certainly a very large bounding estimate or a bounding estimate for ‘51 when lead bath was used at Simonds, or at Bethlehem Steel.

I would also point out that all the rollings at Bethlehem were lead and salt baths, and I'd like to address a little bit of this issue of BZ versus GA samples, breathing zone versus general area samples. I've had very little time to review their report. Their report came out Friday at close of business, and I've looked at it, but we've not had time to review any of the statistical analyses that were done in any detail, but I do have a few comments.

The first issue that's apparent to me is that there were really only two samples at Simonds Saw and Steel out of the 41 that were taken that were listed as general area samples. The other samples were listed as roller stand number one, roller stand this, you know, so they were not explicitly called out as breathing zone samples.

SC&A rightfully points out that in their analysis, the AEC or HASL went and used these in their calculation of total exposures, but they were not the traditional sample where it was taken over the shoulder. These are very similar in the descriptions of the samples that were taken at Bethlehem Steel.

In fact, the ones that are not breathing zone samples at Bethlehem Steel are the GA samples that

are listed as over roller stand number one, over roller stand number three. They were very close to the roller stands. At least, they seem in our mind very conceptually similar to the process that was taken at Simonds Saw and Steel. It would have to be a leap of faith to assume that from 1949 to '51, the HASL program became more lax and just started taking general area samples at the stands that were not as representative of the breathing zone that could be used for calculations in '51 as they were in '48.

We have data over seven rollings. There are over 200 air samples that were taken at Bethlehem Steel. We only have 40 samples, 41 taken at Simonds. We believe that the data are informative of the bounding estimates of exposures at Bethlehem Steel.

One thing that -- we do agree that we need to clarify this issue, the difference between BZ and general area samples, and we've gone to some lengths to do that. As of last Thursday we've been trying to make contact with Al Breslin who was the architect of the HASL breathing or the HASL air sampling program. Through a series of cold calls around the state of New Jersey, we have finally located Mr. Breslin. He is alive and well and living in New Jersey, and he has agreed to discuss with us exactly where these

samples were taken, and what they were intended to represent.

So until we make that contact, we have Federal Expressed to Mr. Breslin the air samples that he assigned. We've sent him a copy of the revised site profile, and we look forward to discussing with him in the very near future exactly what HASL's intent was for these samples. This is assuming that he can remember.

We've talked to him briefly. We've not had any substantive conversations with him, but he does remember Bethlehem Steel. He remembers the process, so we're encouraged that we may be able to shed some light on this issue and make a determination as to how representative or how useful these samples are.

In fact, it's interesting that the samples that Al Breslin took over the salt bath had the same exact description as the samples that were taken over the lead bath by Mr. Miller. And Breslin labeled the sample a process sample. Miller labeled the process a GA sample. I believe what we have here is sort of a difference in nomenclature for these samples.

But the fact that at Simonds they took the same sample near the roller and used it as representative of the breathing zone I think also speaks to the fact

that these were either breathing zones that cold be used, represented the breathing zone, or in fact, I suspect that they may have actually been process samples.

And the other issue that we had with SC&A's review was it's difficult to speculate or postulate a mechanism that could sustain continuous air concentrations at greater than 500 MAC. This equates to about 30 milligrams per cubic meter. SC&A by their own calculation in their first review has determined that around 500 MAC is an area where continuous exposure to that environment would result in some adverse health effects to the workers as far as respiratory distress and that sort of thing. And we buy that. We believe that's probably true.

So the rolling of these samples were somewhat episodic. It took two to five minutes per roll to run through. This was a continuous mill, and that was the whole point of this was to rapidly process a rod through without having to manually feed it.

So for instance, on the rollings of April 27th and 28th, I believe 72 billets were processed. If you took about three minutes a billet, that's 210 minutes worth of rolling. Possibly one could get this high. We have no air samples. The highest sample we have

during that period was around 400, I think, of all the 200 and something samples that were taken. So to postulate that 500 MAC air existed for a duration of 20 hours when only 72 billets were rolled, this does not seem credible to us.

The SC&A analysis of the BZ and GA samples is sort of interesting. They kind of had a double approach here. Up till now we've been fitting statistical distributions to the data set and picking off the 95th percentile. When SC&A chose to analyze the ratio of BZ to GA, they did a rank order test, found as close as they could to the 95th percentile, and then linearly interpolated between two points linearly which, and in the data are lognormally distributed which ended up a much higher value than if one fits a statistical distribution to -- in fact, their most recent report demonstrates that.

You'll see the ratios are much lower if you fit a statistical distribution, in fact, closer to one to one than if you do this rank order test, so one can do with statistics what one wishes, but it certainly alternate interpretations are there for making a case that a GA sample at Simonds is very different than a GA sample at Bethlehem.

And in fact, if one, the highest rollings at

Simonds of 70,000 DPM, the 1000 MAC, was listed as roller stand number one, it was not listed as BZ roller stand number one. SC&A has interpreted that as a BZ sample. In fact, that has seriously jacked up the ratios to make their case. Well, I think there are alternative statistical analyses that could probably prove almost exactly the opposite of what they've come up with. We've not had time to look at them.

This is similar to what Dr. Mauro presented. This is the difference in the lognormal distribution of the lead/salt bath or the lead bath versus the salt bath. And one can clearly see that there are very large differences in the airborne concentrations in general.

I don't have it on here, but if one plotted the Simonds Saw data, it would be closer to the lead bath, but the upper value would be, well, it was 1000 MAC. The upper value, 95th percentile for the Simonds' data would be 550-something MAC. The 95th percentile of the lead bath data at Bethlehem Steel approaches around 200 MAC, I think. So there is a substantial difference in those two operations.

This is just a depiction of the various locations where samples were taken in a box and whisker plot of

the distribution of those samples. And you can see that there was some very serious interest in the shears. These breathing zone samples were taken typically when a worker was doing something that required some movement, and he was in close proximity to the source. So you'll see a lot of breathing zone samples when you're taking it out of the salt bath and loading it into the first stand or one is physically cutting a rod of uranium with a shear generating large airborne.

So there was a lot of interest in BZs where workers were in very close proximity with potential to generate airborne. The stands which are located here, again, they weren't labeled as such as BZ samples, but in the early days we know for a fact that there were no workers positioned at the stands. It wasn't until the cobbles started to become a problem, and we've got some documentation on it, that they positioned some workers at the different stands. And in fact, we don't believe that their face was right in over the stand. They were with crowbars trying to make sure that these things went into the next location.

Just a little more to finish up here, episodic events. Rolling, you know, SC&A has made the issue

of these episodic events, and it sounds like that the cutting of uranium and the cobbles might not be an issue at this point. But we were of the opinion that rollings are episodic events in themselves. So to take the highest concentration, 95th percentile, at a rolling stand and assume a worker breathed that for ten hours a day, we believe in our mind, is a fairly, is a very claimant favorable assumption.

This torch cutting of uranium we still believe is not, it's a bad idea to cut uranium with a torch. We were interested to talk to Mr. Breslin who was present during all of these rollings when cobbles were taken out to ask his, find out what his opinion is of what happened with these cobbles.

I'd like to talk a little bit about the ingestion model. SC&A has recommended, as John talked, this 100 milligrams ingestion as a starting point. And, you know, I spent some time reading literature on this. Steve Simon has put out a very large paper on this to evaluate ingestion. It is an ingestion recommendation for people exposed to soil. It's an EPA-type document, and gardeners and such, they can ingest soil, you know, a total mass of soil. One does need though to make, take account of the percentage of the material ingested is the relevant

contaminant.

And SC&A has taken the position that the uranium that comes off the process makes this veneer contamination of uranium on the ground that is available instantaneously for uptake. Our problem with this model is that it does not have, take into account at all the source term. In other words, if I were theoretically rolling five pounds of uranium or five tons of uranium, I have the same potential for ingestion regardless of how much airborne there was. SC&A has made the opinion that these are large particles that kind of get deposited locally and that doesn't kind of make a case then because these things do not distribute and become widely available for ingestion.

We have proposed a model that is based on the amount of uranium that is put into the air, and we've taken the air concentrations right at the stands so that's the material that can drop down to the ground in the near location where the workers are. And we believe it to be more credible. It's a consistency point of view that we're trying to do this.

I mean, if we have another location, can we assume 100 milligrams? For instance, in this model a worker would receive 100 milligrams ingestion in 1952

when if, as SC&A contends, 20 MAC was the highest air concentration than if you were working in 1949 when 500 MAC was the air concentration. That just doesn't make intuitive sense to us. It just does not seem proper.

John talked about this. The SC&A suspension model relies on 95th percentile. They backed off on that a bit and now have said, well, no, maybe the median value of the general area samples are relevant. We still have a problem with this since these are the samples that were taken during rollings. This is going to be, by their own admission, an overestimate of the ingestion. And in fact, their first model came up, I think it was 13 or 14 MAC constant air for four years was higher than the air samples that were measured in some of the rollings, have higher than the median rollings at Bethlehem Steel.

We need to look at this a little closer. This is not some place where we're really far apart, but we're not convinced that using the median value of the general area samples is representative of the resuspension. In fact, if one looks at some of the samples at Bethlehem Steel where they've taken a sample before they're, you know, as the rolls go

through, the air samples drop down pretty precipitously in between rollings, and they'll take another sample, and it'll be very low. That drop in itself indicates that something like this is probably a very large overestimate. And you just can't sustain uranium in the air at the levels that this model is predicting.

Finally, I'll finish up, the external model, we see some criticism on our external model. It wasn't high enough. We estimated that if a worker were continually exposed for a 2000-hour year, it would equate to a dose of 133 rem, a fairly large, substantial dose is not seen in most DOE facilities. I think SC&A has looked at that and agreed that that's a very claimant favorable assumption.

They still maintain that we need to address residual contamination on clothing. I think we've done that. I would ask them to go back and look at our revised site profile, and in fact, we looked at some dose rates coming off of contaminated clothing at Simonds Saw and Steel and adopted that for our use. And I think we ended up assigning about 150 millirem a year of residual contamination on clothing.

So we need to get together and talk about that,

but I think we've addressed that issue. Whether they agree with it or not remains to be seen, but we're interested in talking with them about it.

I think that's all I have, open to questions.

DR. ZIEMER: Thank you very much, Jim. Let's open the floor now for questions on Jim's material.

Henry.

DR. ANDERSON: I mean, it seems all of this is working off, you know, the both groups are using the same data, and I guess it was unclear to me and in the earlier presentation it seemed there were smaller numbers. Is there agreement on what samples represent what between the two of you? Are you assuming, you know, breathing zone when they're not, and --

DR. NETON: Right, I think that's the crux of the issue is, you know, we have GA samples at Simonds Saw and Steel taken at the rollers that we believe are similar, if not the same, as the type of samples that were taken at Simonds Saw and Steel. SC&A has made the determination that for some reason the Bethlehem Steel samples were taken either further away or not as representative as the samples taken at the rollers at Simonds Saw and Steel. And we have no indication they aren't. We believe that the program took the

samples in a similar manner between the two facilities.

DR. WADE: Jim, could you restate that? I think maybe you were using different Simonds Saw and Steel a couple of times.

DR. NETON: We believe that the GA samples taken at Bethlehem Steel can be used to indicate the representative exposures, if not bounding exposures, for workers at Bethlehem Steel. SC&A's position is that those GA samples are not representative or cannot be shown to be representative.

DR. ANDERSON: Because it all seems to stem from that and then it's a matter of which model you use. And it's all, you know, beyond that how you treat that data, it's sort of how reasonable is your speculation, and then how do you bound that, and then are they, how confident are we that the ultimate exposure --

DR. NETON: I think the issue really is pretty simple. I mean, if we pick the 95th percentile and assume the GA samples are representative, not bounding of the exposures at Bethlehem Steel, we would suggest then that somewhere in the vicinity of 200 MAC air was possible at Bethlehem Steel at least up through, I think it's October 1st because they only

rolled lead bath uranium up to October. After October every indication is that it was all salt bath rollings.

SC&A, at least I don't want to speak for them, but at least in their original models they suggested 500 as an upper bound for all four years. Now I don't know. I'm not sure where they're going to fall now because now they're saying the air samples could be eight times higher so that would put them at 1600 MAC. And then I'm not sure what factor they would accept as reducing these by, you know, they come with this magic factor of three that all of a sudden makes a four, five hundred MAC. I'm not sure. I don't want to speak for them, but --

DR. ZIEMER: Jim, could you clarify your understanding of the differences in the ratios of the breathing zone to general at Simonds and breathing zone to general at Bethlehem Steel? Do you interpret that differently than what we heard from SC&A?

DR. NETON: One certainly could come to a different statistical interpretation depending on whether you use a linear fit, a linear fit to the log model or use this rank order analysis and interpolate between the 90^{th} and 95^{th} percentile.

DR. ZIEMER: Was that in your mind the source of

the difference in the ratios?

DR. NETON: Yes.

DR. ZIEMER: The selection of that.

DR. NETON: The selection of that particular way to do the test is what drives that ratio high. Even in their last report which I've not had a lot of time to look at, I think the ratio's about one to one if you do a linear --

DR. ZIEMER: Has anyone looked at the ratios, for example, based on the mean values of those distributions or anything like that?

DR. NETON: SC&A looked at all the parameters, the mean and 95th percentile.

DR. ZIEMER: John, did you want to --

DR. MAURO: If I may.

By way of clarification, in our report we're not recommending one method of interpolation or the other. In other words we're saying we have a collection of data.

DR. ZIEMER: Yeah, and you've raised a question on it.

DR. MAURO: Right, and now the question becomes okay, if you want to pick off the 95 percentile of a data set, that's what we're talking about. Okay, now let's say we have a data set. Now we basically say

in our report we like the idea of picking off what I call an upper end. And for want of a better term it looks like 95th percentile's the kind of numbers that we all agreed represents an upper end.

Now the question is how do you get there? We present both and say listen, you can get there by a rank -- if you don't think it's a lognormal distribution, and there's some question whether we have a lognormal distribution because it does deviate quite a bit from lognormal. So one could argue well, listen, one statistician would say listen, I don't think you really have a lognormal distribution. And if you don't have that, what do you do? Well, what you do is you do a rank order approach.

In other words that's non-parametric. You don't make any assumption regarding what type of distribution it is, and you pick off, and there's an interpolation technique. You can get to a 95th percentile that way. Or you can get to the 95th percentile another way. You can take your data, fit it to a lognormal, get your best fit, and pluck off parametrically at the 95th percentile. We present both. So we do not recommend or adopt, although we do make a point though.

It does look like the fit breaks down on a high

end as a pro lognormal. In addition, the ratio of this eight to one. We're not recommending that. We used that in our conference call mainly as a way to discuss the issue. We believe that we have some questions related to the definition of what's a breathing zone; what's a general air. There's ambiguity. If you go with, well, a more conservative interpretation, listen, we're going to do the best we can to parse it, which one is basically a breathing zone, and which one's a general air, which is what we did. Then we do see this difference, a robust difference, for Simonds Saw.

And it turns out to be eight-fold at Simonds Saw. We're not saying that's the right number. We're saying there is a difference. Now and all we're really saying is when we move over to Bethlehem Steel, it looks like there's a paucity of breathing zone, but there are some arguments that perhaps those general air are really breathing zone, but we didn't see it. But we think there is.

There's no doubt we're erring more on the side of the claimant, and then that means that we do need an adjustment factor. What that adjustment factor is, you know, we're not in a position to nail that number down. All we did is get, for example, the difference

is a factor of eight at Simonds, and we sort of stopped there.

DR. ZIEMER: Jim, do you know if Mr. Breslin had experience at Simonds as well as Bethlehem Steel?

DR. NETON: Yes.

DR. ZIEMER: So he would be in a position to address the breathing zone and general air sample issues for both sites perhaps?

DR. NETON: We believe so if his memories can --

DR. ZIEMER: But at least he --

DR. NETON: -- over 50 years. He would certainly be the one in the best position to represent what they did.

DR. ZIEMER: And Mark, you had a --

MR. GRIFFON: That was my discussion was the ratios.

DR. NETON: I'd like to point, I mean, John keeps saying well, it's not eight to one, and then he says it is eight to one, and that's their argument. One has to buy into this linear interpolation of these admitted outliers to begin with. And the other issue is that one has to believe that the air samples taken at the rollers at Simonds, which were listed as not BZ samples, are materially different samples than the samples taken at the rollers at Bethlehem Steel.

DR. ZIEMER: Yes, Arjun and then Jim.

DR. MAKHIJANI: Dr. Ziemer, there are a lot of issue about these samples. I'd just thought to make a few factual clarifications about what's in the data sheets, and what SC&A did.

First of all, the report that you have before you, both the rank order and lognormal analyses are done on all the data. And if you look, there's one, there's only one data set. If you look at Figure 2-6, you'll see there's really --

DR. ZIEMER: This is 2-6 in the letter report?

DR. MAKHIJANI: In the letter report.

Where all the lognormal fits in various ways are shown in the figures in Attachment Two. If you look at those fits, you'll see there's really one data set which sticks out which is the general air in period two with, where the upper percentiles don't at all fit a lognormal, and where a rank order approach is more appropriate.

In the other cases you can go either way, and there is a very consistent pattern in the 95 percentile ratios at Bethlehem Steel, but apart from that one that does not have a fit with lognormal. That if you take the ratios of breathing zone to general air by either method, you do have, the

general air is tending to be higher than the breathing zones which is not what you expect.

There's one very important fact in looking at the data sheets at Simonds and Bethlehem Steel that it's important to note. The highest sample value at Simonds Steel, that famous 1000 times MAC, is an unlabeled sample as to breathing zone or general air which the text of the AC discussion interprets as breathing zone. And so it's not entirely claimant favorable maybe, but it's reasonable to accept it.

And the reason we felt comfortable with that is that the result is also compatible with this 30 milligrams. So there's an independent kind of check to that. The highest Bethlehem Steel sample of 400 and odd MAC is labeled as a general air sample so that there we do have to, we felt, you have to pay some attention to how you interpret that specially the general airs are showing higher than breathing zone.

And the last point I'd like to make, Dr. Ziemer, is that you have to look at whether the sampling was done to be representative of worker intake or whether it was done for something else. And Merril Eisenbud himself said in 1951, and this is quoted in a TBD, quote, dust samples were taken at Bethlehem Steel

plant to evaluate continuous rolling of uranium.

And so from a worker intake point of view, the samples are biased. From the point of view of evaluating experimental processes, they would be appropriate. So now you're faced with the problem of how you take this data set that was done to evaluate processes and then back fit it into something to a different use, to put it to use to evaluate intakes. This is a big part of the problem.

DR. NETON: I would remind Dr. Makhijani that the intent of the Bethlehem Steel rollings, one of the principal intents, was to develop a more worker-protective process. I mean, so this was not, there was some production value to be gained here, but one of the major intents was to make the process more worker protective.

DR. ZIEMER: Yes, Dr. Roessler.

DR. ROESSLER: I have two questions, Jim. You mentioned meeting with Al Breslin. I'm wondering what the procedure is then. After you meet with him, and whether he can or cannot shed some light on the samples and what they represented, then do you get back with SC&A, and then how does the board or the working group get involved? It seems like that could be a very important meeting.

DR. NETON: Yes, we haven't worked out the mechanics of that, and I'm not sure whether we should go along; we should go with SC&A and a member of the board. I mean, that process needs to be worked out. But certainly, we would need, we don't want to overwhelm Mr. Breslin either with a large cadre of folks. I mean, he is in his eighties, I believe. But after that meeting, we let the chips fall where they may. If it looks like we can't come to any conclusive evidence, that these are as we believe they are, or he confirms what SC&A's opinion is, we're willing to modify our profile as appropriate.

DR. ZIEMER: Do you anticipate a number of NIOSH people or is it possible that --

DR. NETON: No, I think a small --

DR. ZIEMER: --someone could represent SC&A at that meeting so that it's more efficient and both hear the same thing?

DR. NETON: I don't anticipate a large number of NIOSH people, one, maybe two tops.

DR. MAURO: If it's going to be in New Jersey, it's very convenient for me. So I know John is --

DR. ZIEMER: Right, we'll work that out.

John, an additional comment?

DR. MAURO: I do want to -- we've been talking

about our differences here. I think it's very important to say let's look at where we're not different. We're very close. I think the only thing we're saying is this is 1949, 1950, 550 MAC, surrogate for 1949, 1950, no problem. Nineteen fifty-two, they come up with 20 MAC as being a number. I'm sure that's pretty close to where you're going to be. So where's the problem, 1951.

And what I just heard is that if you take all your numbers and you come up with, without any adjustment factor. See, we're saying we need an adjustment factor. You're saying we don't need an adjustment factor. And in the end Breslin may be able to give us information that says, yes, you need an adjustment factor or no, you don't to create, to keep parity.

But either way we're close. I think I heard you say for 1951, your 95th percentile value is 200 MAC?

DR. NETON: I'd guess at that, but it's pretty close to that, I mean, in the upper one hundreds.

DR. MAURO: Now if you go with my, our example that we only use for the sake of our conversation during our conference call was to take the roller breathing zone samples. I'm sorry, the roller general air samples. Get the 95th percentile to that.

Multiply by eight. So that represents the high-end exposure that might be at the breathing zone when rolling is going on, and that's going on about 30 percent of the time. The other 70 percent of the time it's general air.

So what I'm getting at is I don't think we're more than a factor of two apart. Even if we go with that approach on that order, a factor of two apart on 1952. That's it. That's our difference. Now if we can get some clarification, I think that gap closes.

DR. ZIEMER: I think Roy gave you the factor of two already in his revamping the model, but let me get Gen's other question here.

DR. ROESSLER: I think that might have answered my second question, but I'll make it very specific. There's so many numbers that have been going around and so many multiples and so on, is there any case where it's going to be greater than 500 MAC? I mean, is that any --

DR. NETON: I really can't speak for -- in our opinion, no, but I can't speak for SC&A.

DR. ROESSLER: -- because it seems like you mentioned health effects that would occur at that level or higher, and so I'm wondering about if that's a possibility, medical records or anything that Mr.

Walker could remember about the workers and whether they --

DR. ZIEMER: You're talking about general health effects from breathing stuff?

DR. ROESSLER: Something in that, this breathing stuff. I think you said that if it were greater than 500 MAC that the workers would have shown some difficulty breathing or some --

DR. NETON: This was SC&A's analysis by Dr. Van Pelt, but --

DR. MAURO: Yes, one of the things we did look into just how many milligrams per cubic meter can a person work eight hours, ten hours a day and not experience respiratory distress. And based on our work, two separate people looking at this question, we came in at around 30 milligrams per cubic meter as being you're putting this person in -- never mind the radioactivity, just the dust loading --

DR. ZIEMER: Just the dust loading...

DR. MAURO: -- as being in a stressful situation. So, and that turns out to be around 600 MAC. I mean, so that was one of the reasons why we felt that we could cap this problem. In other words, given all these uncertainties from the radiological measurements point of view alone, because we realize

that the sampling, how representative it is, these are all questions. But one thing where we've come to a degree of comfort is based just on the respiratory distress aspect, we probably could put a cap on the problem, and I think that's very important.

DR. NETON: But one thing I, a question is one would have to get the 1600 MAC if you used this adjustment factor of eight for three hours a day or for seven hours a day or something like that.

DR. MAURO: Well, as I said when we worked the factor of eight as our example, we took the 95th percentile for the general air roller, '52, '51, multiplied by eight and said you're going to get that concentration for 30 percent of the time.

DR. NETON: Which was 1600 MAC.

DR. MAURO: Right. And then the other, right --

DR. NETON: But that's higher than any sample that has ever been measured in --

DR. MAURO: So you're making a counter argument. Well, that example isn't too robust because eight may get you to a place that doesn't make sense.

DR. NETON: Right.

DR. MAURO: And I'm not going to disagree with you here. If eight gets you to a place that doesn't make sense, you have to acknowledge that. So that's

why I keep saying, we used eight only because that's the number that we walked away with. So you're making a counter argument of well, I hear what you're saying, some adjustment factor. Well, I guess I haven't heard you say do you think an adjustment factor is needed here. You're saying until we talk to Dr. Breslin, we really are not sure.

DR. NETON: Right.

DR. MAURO: And I think we all agree with that.

DR. ZIEMER: That sounds like the next step.

I want to ask, we had a working group that met last week -- or last week -- earlier this month and also discussed some Bethlehem Steel things.

And Mark do you have any comments?

Mark chaired that working group.

Are there any other issues on Bethlehem Steel that you want to bring to this subcommittee that came out of the working group at this point? Because this will be back on our agenda for the full board tomorrow to discuss.

MR. GRIFFON: I think one thing that struck me was, which is where we've had most of this discussion, was that was the main item where we had some disagreements and questions. And if we can figure out what these samples actually represent, I

think all of that can go away.

What I thought we sort of concluded in the work group was on a number of the other findings, such as oronasal breathing, and I missed the earlier discussion. I apologize. But it seems to me that we came to a pretty good conclusion that for that it was okay for the Bethlehem site profile; however, there's still some outstanding stuff that from a programmatic side we might want to have NIOSH address the policy because it's going to come up again.

And I think that's true also as I look on the ingestion and resuspension, you know, in fact, and I'm not sure how far apart these are, but in fact, I felt like these numbers were getting pretty close in either SC&A's approach or NIOSH's approach. The question was also then, well, what's the best method to use and maybe for consistency going forward that still needs to be worked out. But as far as the overall impact on the Bethlehem site profile and the doses assigned I don't think it would have major impact so.

But I think where we ended up was that -- and Wanda was there as well and Bob. And I think where we ended up was that that first issue really was the hardest one with the BZs versus the general area and

how that was going to be worked out. And that had a larger impact on the doses so that was the most important one to nail down.

DR. ZIEMER: Okay, thank you.

Yes, Ed, did you have an additional comment for us, Mr. Walker?

MR. WALKER: Thank you, Doctor.

I just wanted to mention to Jim that you mentioned that the cobbles weren't cut. That's still in question because when those cobbles got into the rolling stand, and there were six stands at Bethlehem compared to two at Simonds Saw, and they run much faster. These cobbles got tangled. Now they were, they could not just hook a crane on them from up above and pull them out. Somehow, they had to be cut, some of them. Maybe some of them they could pull out with a crane.

But somehow when they get tangled around, those rods go through that rolling system at that speed, and Arjun can testify to this. The crane operator that was working 50 feet in the air above a cobble was hit, maybe not with uranium. That I didn't clarify, but he was hit in the crane when the cobble shot 50 feet in the air, red-hot rod an inch and a half. So some of those cobbles had to be taken out,

in the roll stands, parts of them, maybe not all. Some of those cobbles went down into the basement area, went around. It was just like a snake when they cobble. So I learned that --

DR. NETON: I am concerned about the cobbles. We have some documentation from HASL AEC talking about the cobbles. And the curvature of those cobbles doesn't seem to be as great as maybe happened with steel. I don't know. They talk about some radiuses (sic) of curvature five inches, bends that are not that great. But they've taken them back and put them in the salt bath and re-ran them through the process. I don't know whether --

MR. WALKER: Some of them probably --

DR. NETON: -- uranium would be a much more dense metal, doesn't cobble as much, you know, as steel might --

MR. WALKER: I don't know. There was many cobbles, and I can testify to that. Some of could have been, I think we had talked about that.

The other one was the men stationed between the stands. There is documentation that many times, and the fella's name was mentioned, Mr. Harper, had to get, he wasn't outside the stands with a long bar, had to get between each rolling stand, and again,

there were six of them, to open up the jaws to receive the rod because they wouldn't hit and go in. They had to open up, it was a pressure spring that closed down on the rod, and many stands also where they had to feed them in with a sledge hammer at number one stand. I just wanted to clear that up.

DR. NETON: But the fact is did he stand at that jaw the entire ten hours or was he moved around? The air sample standing right here on top of the rolling station.

MR. WALKER: Yeah, that's right. I can't say that he was there every minute of the day, right, but there had to be people placed there. I wouldn't want to be there, and I don't think --

DR. NETON: Nor would I.

MR. WALKER: Thank you.

DR. ZIEMER: Thank you very much. This will conclude our discussion of Bethlehem Steel for this morning. We are actually a little behind schedule, and we do have one item, well, this kind of relates.

CONGRESSIONAL ADDRESS
MR. JASON BROEHM

We're going to hear from our congressional liaison.

And Lew, why don't you introduce Jason to the group and explain what Jason does.

And then, Jason, we'll have you read those statements.

DR. WADE: Well, Jason works for CDC, and he works in the area of Congressional Relations; and therefore, interacts with many of our friends on the Hill related to this activity and others. And Jason has been in touch with some of the people of the New York delegation who have some things they would like to be on the record for this meeting.

Jason, introduce yourself and then go ahead.

MR. BROEHM: I'm Jason Broehm, and I work in the Centers for Disease Control and Preventions, Washington office. As Lew said we deal with congressional offices on a number of issues, and I represent NIOSH in that office.

I have this morning a statement from Senator Charles Schumer from New York that he would like read before the board, the subcommittee of the board. And I also am expecting a letter that I have not yet received from representative Louise Slaughter which I anticipate reading tomorrow before the full board as well as this letter again. So this is the statement of Senator Schumer.

Mr. Chairman, thank you for allowing me to submit testimony to the board regarding Bethlehem Steel.

Thousands of New Yorkers labored during the late 1940s and early 1950s in ultra-hazardous conditions at Department of Energy and contractor facilities while being unaware of the health risks. Workers at these facilities handled high levels of radioactive materials and were responsible for helping to create the huge nuclear arsenal that served as a deterrent to the Soviet Union during the Cold War.

Although government scientists knew of the dangers posed by the radiation, workers were given little or no protection, and many have been diagnosed with cancer. Despite having one of the greatest concentrations of facilities involved in nuclear weapons production-related activities in the nation, western New York continues to be severely under served by the Energy Employees Occupational Illness Compensation Program.

I'm aware that many positive steps have been taken in the past few months regarding the Bethlehem Steel site profile, but I do not feel that worker concerns are being adequately addressed or that workers are going to be adequately compensated. Eddie Walker has been a tireless advocate for former Bethlehem Steel workers, and I share many of his concerns.

For example, has residual radiation between rollings and after rollings been evaluated to the fullest? I have a hard time believing that such a large steel mill could be completely cleaned of uranium dust simply by using a vacuum. Without proper decontamination after a rolling, it is likely that uranium dust would still be present throughout a plant of this size, therefore, making residual radiation a hazard for all workers.

In the latest S. Cohen & Associates report on Bethlehem air data released on October 14th, 2005, an interview with a former worker states just this. Quote, the repair and machine of the rollers which would carry residual dust from the rolling area was done in the machine shop according to the schedule of the shop, which means that it was likely that it was done on days which uranium was not being rolled. Unquote. In meetings I have had with former workers, they tell me that they were surrounded by uranium billets and/or dust all day long. Some even told me that they had to remove uranium flakes from inside their coffee mugs.

Have site expert information and worker interviews truly been taken into account? S. Cohen & Associates has repeatedly stated that airborne dust

was unlikely to be the main contributor to ingestion dose, both in the first interview in October 2004, and in the latest document on October 14, 2005. Worker interviews done by S. Cohen & Associates also state that workers were required to be at the rolling stand all day even during lunch. Many workers ate their lunch in the rolling area because adjustments to rollers were constantly necessary.

I cannot stress how important it is to speak with former workers and site experts to come up with a proper ingestion model. The bottom line is this latest document from S. Cohen & Associates clearly supports what Mr. Walker and other former workers have been saying from day one. If an accurate dose reconstruction model cannot be formulated from Bethlehem Steel information, then these workers should be awarded a special exposure cohort plain and simple. Using air sample data from Simonds Saw and Steel in place of Bethlehem Steel data is based on assumptions rather than on sound science.

On July 27th, 2005, Senator Clinton and I along with our colleagues in the House of Representatives introduced S-1506 which would amend the Energy Employees Occupational Illness Compensation Program Act of 2000 to include certain former nuclear weapons

program workers in a special exposure cohort under the Energy Employees Occupational Illness Compensation Program. Our bill would correct years of injustice for western New York's nuclear workers after the sacrifice these Cold War heroes made for our country and have waited far too long.

Being added to a cohort means that these former employees do not have to go through a dose reconstruction process. Instead, if a person had an eligible cancer and worked at a facility when weapons work was performed, their cancer is presumed to have been caused by workplace exposure, and the person's claim is paid. This bill would finally put the former workers on the path to getting the recognition and compensation they deserve, and this is how we should correct this wrongdoing, not by endless bureaucratic red tape.

Again, I thank the Chairman and the board members for allowing me to submit testimony on behalf of the former nuclear workers in New York.

DR. ZIEMER: Thank you, Jason, for bringing that to the board this morning.

We have reached the magic hour of noon, and we are going to proceed with our lunch break. What I would like to ask is if you're able to try to get

back by 1:15 instead of 1:30 so that we can move into our afternoon session in a timely way and catch up a little bit as it were. So we will recess now then till 1:15.

(Thereupon, a luncheon recess was taken and the meeting resumed at 1:30 p.m.)

DR. ZIEMER: We're ready to proceed with the afternoon session. We actually have to carry over one item from this morning before we get to the Y-12 information, and that is the Savannah River site material. So we're going to proceed.

SRS SITE PROFILE REVIEW

We have two brief presentations on Savannah site profile review, and basically this will constitute a sort of status report. We'll hear first from SC&A, and I guess Kathy DeMers is going to make the presentation, and then we'll hear from Jim Neton for NIOSH.

So Kathy, if you want to begin.

SC&A PRESENTATION, MS. KATHY ROBERTSON-DEMERS

MS. DeMERS: Good afternoon. As Dr. Ziemer said, my name is Kathy Robertson-DeMers, and I work with the Task 1 team for Sandy Cohen & Associates. And

really this Savannah River review was a team effort, so I may call upon some of the team to answer some of the questions. It's been awhile since we've submitted this report, and we submitted it back in March of this year. So we're just going to go over some of the basics.

The Savannah River Site is located on 300 square miles of land in the middle of a forest actually between Aiken, South Carolina and Augusta, Georgia. DuPont originally constructed the plant in 1951. They then took over operations of the plant and remained the prime contractor until March 31st, 1989, at which point Westinghouse Savannah River Company took over.

It had multiple missions. The site had a heavy-water plant to produce the heavy water for the five production reactors. It had a nuclear fuel and target facility. It had two chemical separations facilities, tritium processing facilities, test reactors, and then, of course, the Savannah River Laboratory where a lot of research and development occurred. Their primary mission was to produce plutonium 239 and tritium. Their current mission is remedial action, D&D and storage of fissile material.

This is kind of an overview of the Savannah River

process. Basically, they fabricate the fuel, put it into a reactor. Depending upon what the target material is, depends upon where it goes at the facility. They have F-Canyon which processes the plutonium 239 as did H-Canyon through 1960 at which point they started to process plutonium 238 for the heat source program.

As you can see, if we have a pointer here, the uranium retrieved from the separations facility ended up in Oak Ridge, and the plutonium buttons went to the Rocky Flats facility. Then the remainder of the material went to the tank farms.

There were several documents that we reviewed in the course of this analysis. One of the unique things about the Savannah River Site profile was that it was a single document rather than six separate TBDs, but it included the information that you see up on the slide.

In addition to this there were some supporting documents, some technical information bulletins that dealt with maximizing of the internal dose for non-compensable cases, assignment of tritium dose, interpretation of the external dosimetry records and room to assign neutron exposure at the Savannah River Site. In addition since we've put out this report,

NIOSH has released one additional technical information bulletin on the assessment of external dose via the coworker method.

This was one of the first site profiles that NIOSH put together and the first site profile that SC&A reviewed for a DOE site. The document that was reviewed was revision number two. NIOSH is now on revision number three which was released in April of 2005 shortly after we issued our report, and with the differences being that they incorporated a model for consumption of contaminated food. With that completeness and adequacy of data, technical accuracy, and the assumptions and methods, consistency with other site profiles. Although that was fairly limited since we hadn't, at that point, done a lot of site profile reviews, and compliance with the dose reconstruction requirements.

The report was divided into strengths, things that we thought were good about the document; findings, things that represented deficiencies in the site profile and that we believe should be corrected; and then there were observations, which simply raised some questions for which NIOSH should consider it would clarify certain things in the TBD and may improve it.

And prior to me getting into the findings and observations, I think it's important that I tell you some of the strengths about the Savannah River Site profile. As I previously mentioned, they supplemented the site profile with technical information bulletins which gave additional information to the dose reconstructors. The site profile was divided into 30 areas, and they made a concerted effort to characterize each of those 30 areas with respect to internal and external dose.

They did have meetings with trades workers, and it appears as though they have incorporated some of the suggestions into at least revision three for the site profile. Their method for assigning missed dose for those with zero dose we concur with, and in the case of assigning the dose for internal exposure, they used the most conservative solubility class. And we'll get into this in a little more detail here.

We had seven findings, the first of which is related to what's called the high five approach, and I'm going to kind of explain that to you. Savannah River Site had an internal dosimetry registry which was put together by Roscoe Hall. In the '90s, Tom LaBone of Savannah River went back and calculated the dose based upon ICRP 30 methodology which is the

requirement that he has to follow.

It was a best fit analysis. It contained acute doses primarily, some chronic doses, and a variety of radionuclides, primarily plutoniums and uraniums although some fission products. And I need to clarify that this methodology would primarily be used for someone who would not be compensated. So that's kind of a little clarification, but there are some issues, technical issues with this method.

For example, many of the radionuclides, if you started with the bioassay data and used the ICRP-60 methodology versus starting with an intake which was calculated by Savannah River, they used the top five intakes by radionuclide, averaged them, and then put that value into the IMBA code. So if you start with the bioassay versus starting with the intake data, there are some situations where the methodology underestimates the dose.

Another concern we had was that not all of the positive bioassay samples, people with bioassay samples, are included in the IDR, or the internal dosimetry registry. And in some cases we found through data banks and incident files that even the highest recorded intakes for a particular radionuclide were not included.

NIOSH did a comparison of the intake retention fractions, and when the intakes are used to calculate the organ dose, in general, the solubility type, the soluble type is most claimant favorable for a systemic organ. When you use bioassay results, many times the soluble material represents the higher dose.

So there's kind of a mixing and matching of the two methodologies. The most claimant favorable approach would be to start out with the bioassay samples then apply the ICRP-60 methodology. And finally on this issue, we were concerned that the intake values were calculated with ICRP 30 rather than the most current models, and this seemed inconsistent with the requirements for dose reconstruction.

With respect to environmental dose there was really no explanation of how they determined the significant radionuclides. The analysis for environmental dose was limited and did not include many of the radionuclides that were also documented by the Savannah River Site in public reports as being released from the site.

They applied a resuspension factor for calculation of dose of ten to the negative nine per

meter. And based upon the studies that we looked at, a more appropriate value would have been ten to the negative five to ten to the negative six. And those values are highly dependent upon the level of activity that somebody participates in.

In this particular TBD they did not address recycled uranium which, of course, has impurities in it such as fission products and transuranics which can add to the dose. And it may not seem that these are pertinent in such small concentrations, but they do have the potential to concentrate in different organs than say the uranium. And in addition to this, the impurity concentrations in the recycled uranium were somewhat variable because the sites did not want to commit to a particular level of impurities in their recycled uranium until much later in the process.

With respect to the beta gamma dosimetry, a dosimeter calibration was based upon an incident angle of zero. This is typically not representative of what goes on in the field. The uncertainty value that they assigned of 30 percent didn't appear to include field radiological conditions and environmental uncertainties. And we felt that this was too low as a result.

The on-phantom correction factor was also too low at the lower energies of 30 to 250 keV, and those values were taken from calculated values by the Savannah River Site shortly after they got their DOELAP accreditation and they're primarily laboratory errors.

There was no guidance for how to assign shallow dose, and they also did not refer to the TIB which tells the dose reconstructor how to assign shallow dose.

With respect to the neutron dosimetry, the geometric mean and standard deviation for the post-1971 neutron-to-photon ratio are not technically defensible or claimant friendly. The TLD recorded neutron dose for 1971 to 1995 and pre-1971 neutron doses derived from a neutron-to-photon ratio have a high level of uncertainty which needs to be taken into account. One way to do this would be to use the 95th percentile value for the thermo luminescent neutron dosimeters.

And as I mentioned to you previously, all the bad stuff that they didn't re-collect went to the tank farms which had a large number of tanks. With respect to the tank farms, the radionuclide lists were incomplete for both internal and external

radiation. And it was kind of odd because there were inconsistencies between the Attachment A, where it described the radionuclides in the tank farms, and the main text of the document.

Early workers' incident and contamination records, we believe, may be seriously incomplete. For example, when you look at the tank farms data bank which lists spills, incidents, high radiation incidents, and you look at the numbers for the 1950s and 1960s, they're very small. And this can be seen in our table in the report. And then all of a sudden you have a great increase in incidents where they really started to document things better.

Another problem with the tank farms is the exposure geometry. In this particular case you may have somebody standing on top of a source term rather than in front of a source term where their badge would more effectively measure their whole body dose. There was a potential for internal and external exposure to unmonitored workers in unposted areas. If they did not go into a radiation zone, they weren't badged. However, there was a lot of contamination around the tank farms, and they didn't always get it posted appropriately.

The completeness and adequacy of the tank farm

data bank we talked about a little bit. What NIOSH needs to do is put together a master incident database. There are several sources of information that can feed into this database. There is, of course as I mentioned, the tank farms data bank. There is something called a special hazards investigation. This is like a field report, radiation report, prepared on an incident. And these are items that are not being sent to NIOSH by Savannah River Site.

There is also a very complete incident database which I'm sure NIOSH knows about which is not directly owned by Westinghouse but is owned by one of their subcontractors who is responsible for their safety analysis program. And site experts indicated to me that this is where all the detail is down to the minor incidents.

And that brings us to the last finding. There needs to be further investigation on the solubility assumptions used to estimate organ dose from urine. The most claimant solubility for the radionuclide and organ of interest should be considered in the context of calculating the dose from this urine.

With respect to ingestion, I mentioned earlier that they had incorporated foodstuff intakes into

their latest revision. I really believe that there may be some other modes of ingestion, for example, during solvent burning, plutonium solvent burning in the tank farms area. And then we briefly talked about oronasal breathing this morning, so I won't bring that up.

NIOSH along with Savannah River really has excused the dose from special tritium compounds. These are organically bound compounds or special metal tritides which can give a higher dose than the standard tritiated water. This is particularly pertinent to the tritium production facilities and to the D&D workers. This is a subject that is not specific to Savannah River and should be looked into.

Savannah River participated in a number of special campaigns. I may have mentioned earlier that the reactors produced 14 radionuclides. These special campaigns involved the reactors, the separations facilities, and of course, the laboratories. Some of these radionuclides were Californian 252, thorium 244, plutonium 242, and americium 243, and then also cobalt.

What we would like to see, and what we have seen in later onsite profiles, there's a list of incidents, and then added to that a list of high risk

jobs to alert the dose reconstructor to this type of situation. For example, with respect to the high risk jobs, you have the construction workers, the subcontractors, and the D&D workers. And this is also mentioned down below in observation seven, and really they are, NIOSH is working on this through the Center to Protect Worker Rights. The doses to the trades workers were not discussed in revision two or revision three. And Jim can speak to this better, but it is being processed, the investigation.

There was involvement with uranium 233, with beryllium recovery processing and recovery. There were also off normal or unauthorized practices. For example, there’s a great pond on the Savannah River Site, there's lots of tritium and some cesium, but it also has great fish. So the workers like to fish in this pond. That would be an example of one of the unauthorized practices. And then there's significant unrecognized or unreported exposures such as opening of the tank risers for visual checks.

The dose to early worker seems to be a recurrent problem in these site profiles primarily due to the early dosimetry in the early monitoring practices. For example, at Savannah River in the early years, although they did use neutron monitoring, it was not

a routine program as was the beta-gamma program. Also, until very recently as a result of an audit, the field was responsible for determining who got what bioassay including who got follow-up bioassay.

The, each area on the Savannah River Site was like an entity in itself so the rules followed by the 100 Area or the reactors may be different than the rules followed by the 200 Area, or the separations facilities may be different from those followed by the 300 Area. And that is why it's important to know that your field is determining what bioassay you're getting.

There needs to be a review of how comprehensive this early data is. For example, if they didn't use --

DR. LIPSZTEIN (telephonically): Hello? Hello?

DR. ZIEMER: Is this Joyce? Joyce, are you on the line?

DR. LIPSZTEIN This is Joyce Lipsztein. I'm on the line.

DR. ZIEMER: Okay, you're on the speaker phone here. Kathy DeMers is still presenting.

Kathy, go ahead.

MS. DeMERS: Okay, for example, if you have someone who was not routinely monitored for neutron

working in a reactor area, you need to ask yourself, why and when they could have potentially been exposed to neutrons while they were not monitored.

And finally, I would encourage NIOSH to go back to Savannah River and make sure that they're getting all the data that they need. For example, there are neutron logbooks out there which Savannah River was not providing the quarterly data for that you ought to be getting your hands on. And the reason that they're probably not giving you this data is because it's stashed away in the records repository and needs to be pulled back.

Another area that we would like to see NIOSH consider that doesn't come in the standard radiation personnel file is the multiple dosimetry results. They did use multiple dosimetry for especially high risk jobs. The dose recorded in the dose record is dependent upon the time period. Sometimes they used the highest of the multiple dosimeters; sometimes they applied correction factors. But you can see if they wore a dosimeter on their head, for example, when they were in the PIN room, and they have cancer on their head, how that might be relevant.

And that's basically all I have to say.

DR. ZIEMER: Thank you very much. We have time

for a couple of questions. Kathy, on the neutron logs that you mentioned, are those individual records, are they area surveys or what, both?

MS. DeMERS: They're listings of neutron dose by badge number and --

DR. ZIEMER: But are these logs that don't appear in the individual's personal record?

MS. DeMERS: No, and I don't believe they're being provided to NIOSH because they weren't readily available to the Savannah River Site records organization.

DR. ZIEMER: Do we know why those were kept separate from individual dosimetry records or is that not something that --

MS. DeMERS: Well, Savannah River does provide information, cycle information to NIOSH out of the logbooks.

DR. ZIEMER: Well, I meant why does Savannah River not include those in their own personnel, for example, if Dr. Anderson worked at Savannah River, would his personnel record not include that or would it have been kept --

MS. DeMERS: It would have been like each of you have a badge number, and your dose of record would be on one page. So they didn't segregate that page into

several different people.

DR. ZIEMER: I see, thank you.

Other questions right now?

NIOSH PRESENTATION, DR. JIM NETON

Okay, if not, let's go to NIOSH and Jim Neton has some comments. Jim has told me that their comments at this point are still rather brief, so, but we'll hear what they have.

DR. NETON: Thank you, Dr. Ziemer.

Dr. Ziemer's right. We have not evaluated in extreme detail the Savannah River Site profile. We acknowledge that we've had this profile since the March time frame, but other competing activities such SEC petitions and other items have prevented us from going into the depth that we really need to at this point. I'd like to acknowledge that I think Kathy's done an excellent job summarizing the issues that were discovered by SC&A in their review, and they, as usual, have done an extremely thorough job.

It does bring one point to mind though, and we've been discussing this all along as we go with each of these site profiles. They're not intended to be a complete compendium of all facts ever known about the site. These are not the definitive treatise on activities at Savannah River. They are guidelines

used by the dose reconstructors to take an appropriate path forward, and where there are data gaps, to fill those in using claimant favorable assumptions. It's been our experience, in fact, that in doing so it ends up that we overestimate doses more often than not when we start pulling the thread and finding out what the real facts are because we tend to be conservative in those assumptions.

The high five approach is a good example of that. I think I'll start with the first slide. I'll just summarize the seven findings I think that Kathy has gone over so I won't bother to discuss those. What I will go over is where we're prepared to make some general comments about some of the findings that were made by SC&A in their review.

I think a fairly important one is this issue of the high five approach. We've used this to a considerable extent in doing dose reconstructions, and in particular for workers who, from our estimation, appear to have had jobs with low potentials for exposures. A good example of that are administrative workers, workers who infrequently visited the process areas, those types of workers. And in fact, it's only used to make a determination that even under the, what we consider bounding

circumstances, the dose is less, or the probability of causation is less than 50 percent.

The intakes, as I mentioned, are assigned to unmonitored workers with low potential for exposure or for workers who have bioassay data, and it can be demonstrated definitively that the bioassay data points themselves are below the values that would be predicted based on this large intake. So what we've done -- and Kathy's again done a good job summarizing this -- is looked at the historical intakes that Tom LaBone put together.

It wasn't our intent that these be exact intakes. All we were suggesting, and I think the SC&A review itself acknowledges, that for workers for low potential for exposure, the intakes that we're assigning are bounding values. For example, one would assume for an administrative worker or a person who infrequently visited a process area, an intake on day one, the first day they were at the site, an intake of a couple hundred nanocuries of plutonium -- I'm not sure if it's a couple hundred nanocuries, but it's in the hundred nanocurie range, a very large intake that would be very unlikely to be received by a person who had a low potential.

We've done that time and time again in calculated

doses for these what we call non-metabolic organs, and it's worked to our advantage. So whether or not ICRP 30 was involved, we didn't use ICRP 30 to do dose reconstructions. All we're portraying here is that these intakes are large. They're implausibly, not implausibly large, but very large, not likely to have been received by these folks. And under those conditions we can make a very definitive determination of the probability of causation falling on which side of the bar.

I think more has been made of this than needed to be, and we need to engage in some conversations with SC&A. We have not yet gone to the table and engaged in the so-called six step process that needs to be undertaken to address these issues.

The oronasal breathing issue, the more we hear about this the more I'm convinced with John Mauro that this really is a policy issue that we need to come to grips with. It falls in a line of what is used. ICRP models, for example, are reference man models. They assume a standard adult weighs 70 kilograms. He has a certain lung size, a certain ventilation rate. All those parameters that one needs to use to do a calculation.

Oronasal breathing sort of falls in that

category. Is it appropriate that we assume, a priori, that all claimants breathe a hundred percent of their time through their mouth when, in fact, a fraction of the workforce does. We've looked at this is some detail. I've commented on this at the St. Louis meeting, been to St. Louis so many times. I'm sure it was one of the St. Louis meetings -- that ICRP has evaluated this.

And in fact, what their conclusion was that the variability among individuals just from normal breathing is greater than the variability within an individual whether he breathes through his mouth or his nose, and it wasn't worth considering in the big picture. So again, I'm not going to purport to solve this issue here, but these are some discussions that I think, us, the board, SC&A, others need to undertake to put this issue to bed.

Another recurring theme in the SC&A analyses of profiles, incidents and ingestion is not covered. Now we have adopted a policy of primarily, almost exclusively, we'll use some type of bioassay data to conduct a dose reconstruction for monitored workers. In that scenario incidents and ingestion are covered. We've gone through this with Mallinckrodt. I thought we had demonstrated to SC&A that when one assumed the

chronic inhalation model, you cover a large magnitude of issues that could have occurred as far as these incidents. We're not saying it covers a hundred percent, but by and large it is claimant favorable, and it addresses incidents.

Ingestion is also included in there if one is ingesting uranium that would be coming out in the urine. For the most part we believe for chronic exposure models, the inhalation pathway provides the highest dose per unit intake. We can discuss a little bit about the solubility type F that SC&A has raised in the Savannah River profile. That's a special circumstance, and in fact, when one evaluates that completely, the issue kind of goes away. I mean they've kind of focused on one narrow issue.

So again, you know, the profile gives the dose reconstructor an approach to look at detection limits, bioassay samples, all that sort of material. It doesn't do it in a vacuum. We don't, I think, nor could we have every possible incident and addressed in these documents. It's just not possible.

Much was made in the Savannah River as the first site profile review done by SC&A, the first DOE complex site profile of the uncertainties in external dosimetry. I believe that we use the standard

uncertainty of plus or minus 30 percent that's included in the National Resource Council review, the DTRA Program. It's pretty consistent of laboratory practice analyses. What can one measure? What type of radiation can one measure within a certain uncertainty under laboratory conditions?

And SC&A has made some comments that that certainly doesn't include angle of incidents. It doesn't include environmental factors that may affect these TLDs and film badges. And we agree that we need to address that. I mean, there is an analysis ongoing as we speak to address another issue related to exposure geometries, the locational geometry that was brought up in another review. And we're going to fold that analysis into one big technical information bulletin, and we'll get back to the board with further information when that's complete.

DR. LIPSZTEIN: (Inaudible).

DR. NETON: Hey Joyce, this is Jim Neton. Did you have something to say?

DR. ZIEMER: Joyce, are you still on the line?

DR. LIPSZTEIN: Hello?

DR. ZIEMER: Yes, Joyce, are you still there?

DR. LIPSZTEIN: Hello?

DR. ZIEMER: Can you hear us, Joyce? It doesn't

sound like she's hearing us.

DR. LIPSZTEIN: (Inaudible) I can hear, but I couldn't hear anything after (inaudible).

DR. NETON: Sorry you can't hear, Joyce. I'm not sure what we can do to rectify that. I can try speaking more directly into the microphone. Does that help?

DR. LIPSZTEIN: (no audible response)

DR. NETON: Apparently not.

Well, I only have one more bullet here so I'll be quick.

The final issue here is the use of the 95th percentile for neutron-photon ratios. Kathy, you brought this up, and there's a recurring theme here where SC&A consistently recommends the 95th percentile. We're a little bit concerned that if one keeps compounding 95th percentile, 95th percentile, 95th percentile, one's add up to at a fairly implausible exposure situation which I'm afraid we may be reaching.

In this particular case we constructed the ratio of neutrons to photons based on some monitoring data that we had and assigned an uncertainty distribution about that. So our best estimate was the median value and put uncertainties about it. And we

proposed and have used that value in reconstructing not really missed neutron doses, but what we believe to be unmonitored neutron doses.

SC&A's opinion is that one needs to go out to the upper tail, the 95th percentile, multiply all those doses by a factor of four, and then one will be sufficiently claimant favorable. We're not convinced that that's the best approach to take. We certainly, again, have not discussed this with SC&A. We need to get to the table and work these issues out. But I wouldn't say we strongly oppose that, but we'd certainly like to have a more detailed discussion of where this 95th percentile really needs to be used.

And I think there's a couple areas that should be quick, areas I've called in agreement. We don't always disagree, so I thought I'd bring those up. We do agree that for consistency in dose reconstructions even though the dose reconstructors might include recycled uranium in the calculations, we need to have additional development in that area so that, again, consistency could be reached.

And we also agree that the exposure conditions at the tank farm definitely needs to be fleshed out in more detail and portrayed in a better light or better detail.

And that concludes my remarks.

DR. ZIEMER: Thank you very much, Jim.

And Joyce, if you can hear us...

On the line, for the benefit of those who are in the audience, let me tell you Joyce Lipsztein is a member of the SC&A team, our contractor. Joyce is on the line with us today from Brazil. So I'm not sure Joyce if you are hearing this or not at this point, but --

DR. LIPSZTEIN: I could hear you, but I couldn't hear Jim.

DR. ZIEMER: Couldn't hear Jim. If you have any comments on, I don't know if you heard your colleague's presentation, but at least you have a copy of it, I guess, and know what she said. Do you have anything to add on the Savannah River presentation at this point, Joyce?

DR. LIPSZTEIN: I couldn't hear anything (inaudible).

DR. ZIEMER: Okay, thank you. Hopefully, you'll be able to hear the subsequent presentations a little better.

Board members, do you have any comments or questions for Jim?

Yes, Mark. And Mark also while you have the mike

if you can report anything from the working group. Did the working group --

MR. GRIFFON: We really didn't discuss it, so that's an easy one.

DR. ZIEMER: That's easy, okay, thank you. Mark Griffon.

MR. GRIFFON: Yeah, I just have a question as we're thinking toward the six step process, Jim, that, I mean, I'm wondering if it would be more useful to have sort of this how a dose reconstruction is going to be done because I don't think that's, you know, you said the site profile document is not intended to be prescriptive necessarily.

So I think that's where we've had trouble in the past, I think, is that we're pulling some of these things apart in the site profile and then we come to hear from NIOSH that, well, we're not really using that aspect of the site profile to reconstruct doses. Well, then why am I wasting my time? You know. I think that might be useful for us as we go forward in the six step process to --

DR. NETON: You raise a very good point, Mark, and I think SC&A recognizes in the last meeting where we talked about when they do the site profile review, they've been doing it without the benefit of having

these workbooks available to them and what really goes into a dose reconstruction. And I think, I've said all along, and I believe more strongly than ever, that one can't review a site profile in a vacuum without looking how it's applied in a dose reconstruction.

We certainly have many, many Savannah River dose reconstructions have been completed. I think it's in the hundreds if not a thousand. And so it would certainly be good at this point for SC&A and NIOSH to jointly look at those and see are we missing major issues, we always hear about these incidents, minor incidents. We take coworker data. We take bioassay data, and we try to give the benefit of the doubt where we can and I think to a large degree we address these issues by using these coworker databases that tend to overestimate on doses.

MR. GRIFFON: That's what I, I mean, I'm trying to understand. I think it would be useful early on in the six step process to have that instead of going through a lot of this and then at the end of the day, the board saying, Jim, can you provide us with a few sample cases, right after we've been through four meetings of this, you know what I mean?

DR. NETON: I agree.

MR. GRIFFON: I think, and especially maybe not only the worst case high five approaches but also some best estimate cases.

DR. NETON: Exactly.

DR. ZIEMER: John Mauro.

DR. MAURO: Thank you.

Yes, we're about to move into what I consider to be an extremely important phase of the work we've been doing for the board and that is the workbooks. It's, in effect, we're about right now, we've just begun the Rocky Flats workbook is on the web on the O drive, downloading it, we're starting to work on it.

Now we're engaging in this, some of these are extremely sophisticated, complex tools. I think we, what I'm going to offer is that the workbook review process somehow become part of the working group operation. That is, in effect, if I understand the workbooks correctly, all of these questions are solved. That is, it is a mechanical process where all of the lower limits of detection, all of the decision points, everything, or as much as humanly possible, is built into some of these extremely complex workbooks, maybe all of the, many of these questions on Rocky Flats which is right before us right now.

We're concerned that once we dive into these workbooks, we're going to find it's going to be an enigma. And that it might be something where we're going to have to work very closely with your spreadsheet workbook people. The degree to which, because we've built workbooks that are enormous, and the documentation that goes with a workbook to understand not only how to use it correctly, but the rationale behind every layer and every sheet and every link is not always apparent unless it's very carefully laid out.

Now my sense is that a lot of these workbooks were put together to help the dose reconstructor get through a complex process quickly and efficiently and consistently. But perhaps, you could tell me perhaps the level of documentation and guidance and training is very advanced that you have put in place to make sure that these workbooks are being used correctly. I'm concerned that we have not been part of that so that here we are. We're going to download the Rocky Flats workbook and try to dive into it, and we might be diving into an ocean that we're going to try to swim.

I think that we need to work more closely with the working group and with you. And you're in a

position to judge. Some of the workbooks may be very straightforward. For example, the 28 radionuclide workbook is straightforward, but I'm not sure, for example, Rocky Flats is going to be very important. And I'm not sure how advanced and how complex that workbook is. But if it is like the Savannah River one, for example, which is a set of three I believe, that are incredibly complex, I think we have to start thinking in a different paradigm to use an overused word on how we're going to engage. And so I'd like to put that before the board as to how we're going to proceed with the workbooks.

DR. ZIEMER: And that's not a discussion we'll have right now. I want to return to Savannah River and see if there are further questions specifically on this presentation. And Savannah River will be back on our agenda tomorrow as well so we'll have a chance to talk about next steps.

Yeah, Mark.

MR. GRIFFON: Just one more thing along the same lines that I just mentioned. There's, in the procedures review that's coming up later in the agenda, there was some -- this is to exemplify some of my confusion on this -- some of the NIOSH responses, and I think I may have these procedures,

technical information bulletin. I think it's TIB 0007.

One NIOSH response indicated possibly canceling that TIB and including the detailed information in the site profile. So I'm wondering if, you know, I thought the procedures were going to be more prescriptive of what a dose reconstructor would do. And now some of that's being put back into a site TBD. So I guess, you know, I guess it all does come down to these workbooks. But is there any written guidelines?

DR. NETON: I think it's always been our intent that as we develop these technical information bulletins they get rolled up at some point into the site profile because they would tend to be more generic. I mean these are generic approaches to addressing a varied series of complex details that occur. And a lot of thought goes into these TIBs to say, well, yes, we acknowledge that SC&A thinks that there's all these incidents around these areas and solubility's an issue. These TIBs tend to be bounding-type calculations that do allow for economy of a person's time to do these things. And so --

MR. GRIFFON: So then I guess the notion would be that if you had site-specific guidance related to a

certain TIB, it would go in the site profile. But also if it had certain parameters such as John was discussing, it would be built into the workbook --

DR. NETON: Right.

MR. GRIFFON: -- and carried through the...

DR. NETON: I guess enough said on that.

DR. ZIEMER: Thank you very much.

Y-12 SITE PROFILE REVIEW

We go on now to our next item on the agenda which is the Y-12 site profile review. Again, we have two presentations. This says John Mauro, but I think it's going to be Joe Fitzgerald it appears. So Joe Fitzgerald who's here on behalf of SC&A will present their review and Jim Neton will have an opportunity to respond.

DR. ZIEMER: Joyce, can you hear now?

DR. LIPSZTEIN: Yes, I can hear Joe now.

SC&A PRESENTATION, MR. JOE FITZGERALD

MR. FITZGERALD: Thank you for the opportunity. We discussed this back in July, so I'm going to really kind of hone this down to some of the issues I think are going to be important over the next couple of months that we've already started a dialogue with NIOSH a couple of weeks ago.

I'm going to go over five of the eight or nine

findings that we have, and of course, you have the handouts. First of all, this is one of the earliest site profiles, and as with Savannah River, we understand we're looking at a snapshot that's almost two years old and there's been a lot of work since then in terms of TIBs and workbooks, and you name it. So to some extent we're acknowledging the fact that there may be some developments that weren't captured by looking at the TBD itself. And we're certainly open to clarifying that.

Just off the top, one issue that has continued to be of concern is this notion, this worker population of maintenance support workers, service workers. A plant like Y-12 historically had a large group of workers that weren't on the line or assigned to a facility per se, but really were available for the entire site. And Y-12 being a kind of a self-contained close-in site, you could, you know, give people assignments and move them around the site during the work week where they were needed.

So you had a group that, for want of a better word, in fact, they called themselves the outside, you know, support staff as opposed to the inside support staff. The inside support staff were maintenance workers and people like that that were

actually assigned to a facility, to an operation, say a 92-12 maintenance person would have been certainly associated with that particular operation. And in those cases, those workers, were effectively badged and monitored as were the process workers.

This group of workers though, these so-called outside workers weren't monitored. And again, let me tell you, we interviewed something like 40 to 50 workers split fairly evenly between the maintenance and support types and the production folks and with vintage -- I guess that's not the right word -- with tenure going back into the late '60s and early '70s. So I was actually pretty surprised we were able to find people that went back that far.

We really didn't have a whole lot of luck, and this was an expedited review. As you recall, the board wanted to have this review done relatively quick so we started in July and finished probably mid-August. But we interviewed these workers, and thankfully they certainly corroborated that they weren't routinely bioassayed before the mid-'90s, and of course, weren't externally monitored before '61.

And the concern that we have effectively is that these workers were given assignments throughout the different operations over the different time frames.

They supported not only the Y-12 operations but also the Oak Ridge National Lab tenant operations that were onsite as well. Well effectively, they were exposed to just about everything that was going on in the plant at some given time. And were, in fact, likely exposed.

There's no way to really know since they weren't monitored very effectively and exposed to the recycled material. They were exposed to certainly the limited PU that was handled at the site and certainly thorium operations. They effectively did all the cleaning, all the mopping up, and effectively some of the dirtier operations because again, they were in the maintenance class, and they tended to have to do those kind of operations.

And we were able to get, I think, a number of recollections of the types of activities that were underway. But they tended to be what I would characterize as more dirtier activities from the radiological standpoint.

And the concern certainly is that we couldn't find any specific records and the activities these workers were involved with, in my view certainly, and certainly from their standpoint, were unlike those in the production operations. If they came closest to

any type of worker, it was probably the maintenance workers that were supporting specific line operations but only to the extent that they certainly were exposed to some of those source terms.

But these workers, which makes them rather unique, were also exposed to source terms elsewhere in the plant which the specific inside maintenance workers would not be nor would the process operations people that were working specific facilities like 92-12 and 92-06. So we did spend the time we had trying to nail down whatever, you know, records would kind of stipulate where these people were and where they worked and what time frames.

But as they would attest, those kind of records were not kept in any systematic way. I mean, they truly were an available resource that was used almost on a daily basis for whatever support activities would be needed at that time. I make the notation that, you know, this is analogous to, I think, your transient worker, in a sense outside contractors that would have come down to --

DR. ZIEMER: Your slide, Joe. I think you're a slide behind.

MR. FITZGERALD: Sorry about that.

The last bullet sort of acknowledges that this is

similar but not identical to the situation you'd have with a transient worker. But the difference is, of course, the transient and the construction workers were outside contractors that would be coming in as subcontractors. These were Y-12 employees. And I think the notion here is that in addition to being a coworker challenge, this is a worker population that is kind of a unique group that beyond not being monitored, were doing operations that were fairly specific and different than the production employees.

Now finding two is something we did have a chance to talk about two weeks ago. And I think Jim was beginning to provide some feedback on this. So the process of beginning to understand the issues it has started. But I would also add to what Mark Griffon was saying earlier. We do need to move right into a six step process to begin to converge on this.

But in this case once you get beyond the question of the worker category and to what extent you can pin down that category and understand what may be a potential pathway as far as a coworker assignment, you get into this question of whether on the external side you can actually assume that the individuals that were badged pre-'61 -- and Y-12 is a situation where '61 was truly a threshold on the heels of the

criticality accident that happened in the late '50s. They went from virtually having only a quarter or less of the worker group monitored. I think it was two to 25 percent to almost everybody being monitored after '61.

And so the question is can you frankly as the TBD suggests, use the badged personnel and assume that the badged personnel before '61 represented the maximally exposed individual, this so-called ten percent. And we had a number of questions on that, really clarification questions. We didn't see a high degree of corroboration in the TBD, couldn't see the basis for making that assumption, and this is something that we're looking to NIOSH to do.

And I think Jim has certainly some information on that. But that's certainly a concern because on the external side for both worker population in general and the support workers in specific, that's going to be a crucial issue on the external side. On the bioassay side, clearly, that's going to be a problem in terms of workers that were not bioassayed, and there were certainly a large segment that were not.

Now I might turn to Joyce and as an experiment see if we can do this from Brazil.

Joyce, do you have our slides in front of you?

DR. LIPSZTEIN: I couldn't hear you, but I have your slides.

MR. FITZGERALD: Okay, we're going to attempt this, and if it doesn't work out, I'll just take over. But Joyce was a key author, in fact, the key author in this section on internal and certainly had a number of concerns that, again, we wanted to clarify how these were addressed, what assumptions were driving the internal dose TBD. We did have some exchanges with ORAU, but I think we still have some concerns that have not been resolved.

And Joyce, I'm going to try this. Can you walk down the five or six points beginning with the fact that we don't believe Type F as a solubility class was addressed adequately.

DR. ZIEMER: So basically, you want Joyce to go finding --

MR. FITZGERALD: Can you go down finding three?

DR. LIPSZTEIN: I'm sorry, can you repeat again?

DR. ZIEMER: Finding three, Joyce, can you lead us through that page?

DR. LIPSZTEIN: Oh, okay.

First of all on the intake model that was used for the coworkers, the Type F uranium compounds were not considered, and that's, but there's no

information why Type F uranium compounds were not considered since it is clearly stated as the internal dosimetry. (Inaudible), but many workers (inaudible) Type F compounds. And when you go (inaudible) the dose in the body, in the different organs of the body, systemic organs, then Type F many times it's more conservative than the other types of compounds from Type M and Type S. So there was no explanation why Type F uranium compounds were not considered. One has to remember that when you go back from bioassay results, it's different than analyzing some intake. So when you analyze the data from the intake, then Type F is less conservative than Type M and S. But when you go back from (inaudible) that was used in the (inaudible) workers, then type F certainly should have been considered.

The second thing that was a problem is that all particle sizes were considered equal to five microns. And then again, the TKBS says that there were many particle sizes ranging actually (inaudible) considered eight microns instead of five. And then again, when you go back from bioassay results to doses, then the larger the particle size, then the doses are higher.

The third thing that was not considered was the

ingestion pathway. Ingestion pathway should be considered especially for people that have cancer found in their GI tract organs. This should have been done at least for the people that have cancer from the GI tract.

Then there's more good analysis on the uncertainty in bioassay measurements and the capacity to detect intakes from urine samples after exposure to Type S uranium compounds. There are some samples are saying that we don't know how the bioassay measurements were drawn. We don't know how the change from microrems to 24-hour intakes. We don't know this and don't know that, but then it's not addressed in the TBD.

And then this fourth thing, the fifth thing is the 48 hour delay in the routine urine sample. This was not addressed, and it says clearly that there was -- in two pages at least in the document 005, that the samples were taken after 48 hours, and this makes a big difference. It could be type F, it can be up to four times more, the dose can be four times higher if you consider that samples were taken after 48 hours.

And then the thing that I consider one of the most problematic things is the use of the 50th

percentile intake rate for coworker. I think, I strongly think that this is not a claimant favorable approach. It says clearly that there was a lognormal distribution. We didn't analyze the data from where it came because we didn't have access to it from the (inaudible) database. So we assume that NIOSH was right in saying that there was a lognormal, a (inaudible) lognormal distribution.

(Inaudible) lognormal distribution is characterized for urine activity results being positively skewed each month. So each month (inaudible) high urine activities for the workers. So there is a group of workers, I don't know if it is the same group or not, but there is a group of workers that clearly had a high positive urine results every month. Which means that there was a contamination every month. Then what NIOSH does is to use 50th percentile. When it uses the 50th percentile, you get tremendous, you assign to all workers that were not monitored values that even are below the detection limits for natural uranium (inaudible). So this is clearly not a claimant-favorable approach.

DR. ZIEMER: Thank you, Joyce.

Do you want her to continue on?

MR. FITZGERALD: No, no, I can continue.

DR. ZIEMER: Thank you, Joyce.

MR. FITZGERALD: The only two things I would add is on the ingestion pathway, particularly for a uranium plant like Y-12, but for other uranium plans, clearly ingestion is, I understand what Jim was saying earlier, but in this particular case it's a key pathway that we believe needs to be included. It won't rival inhalation perhaps, but certainly for certain cancers it would be a major contributor.

The second thing is on the 48 hour delay, clearly this was a practice that was changed in the '80s, mid-'80s or so according to the interviews with workers. I mean, the explanation was there was a desire to wash out the soluble uranium before taking the measurements, and that practice was reversed sometime in the mid-'80s. It wasn't an exact date, but so this wouldn't be going into the '90s and present but certainly back in that era.

The fourth finding is again something that I don't think is really in contention, frankly. We've had this discussion with the ORAU team and also with NIOSH. And we acknowledge and recognize that the TBD did not originally address nuclides beyond uranium, but there's an active effort underway to include

thorium, we understand, uranium 233 and recycled as well. We would maybe add a couple more to that list, but again, we're trying to provide a complete assessment of the TBD which is now over two years old and at that time certainly it was limited.

And that has implications again because certainly workers were exposed to elements more than uranium. And of also concern was, there was a great deal of effort by the facility, to its credit, to segregate operations like thorium from uranium operations because of the higher activities involved with thorium. But there were excursions on occasions, and they did have track-outs. So again, it would certainly be helpful to have that additional information in it.

The next finding, this is kind of an interesting issue. I guess we keep coming back to this question almost at each site, and it seems like we also have a slightly different bent to the NTA film issue. In this case the notion is that the NTA film can be relied on because the energies involved with the neutrons of question were relatively high; and therefore, it was felt NTA film would pick up those neutrons.

However, we're concerned that there isn't in our

view enough spectral measurements to firm that conclusion up enough, but we feel that there's some evidence that we do have more some -- I don't want to call them soft -- more moderated neutrons, neutrons of lower energies that would not be seen by the NTA film. And also, again, trying to pin down the reliability of NTA film.

Those of us who were looking at the Iowa site profile sort of remember this debate and there's sort of the 700 to 1 MeV range that was the area of acknowledged value of the NTA film. Even there I think there was some desire to not use the NTA film and let's go for the neutron-photon as a better way to go. Here it's just the reverse that the NTA film seems to be the valid way to go. And by the way, we can actually see the neutrons down to 500 keV. So certainly a question of consistency.

We're concerned about, you know, exactly where does that fall? Are we that confident that the energy spectra at Y-12 historically, in fact, would have been picked up by the NTA. We have some doubts, but certainly if one came up with more energy spectra measurements, I suppose you could bracket this and be firmer about it. In any case...

Since we did cover this in July and have had a

chance to chew on this a bit, I'm not going to spend a lot of time on this. I think the focal point that likely the board and we will be faced with were amongst those four or five issues. These issues we certainly feel are legitimate, but maybe not as important to resolving questions before we get to January, and in Lew Wade's words, trying to converge on some of the key pathways. But certainly there are elements of scope that could be addressed and should be addressed but ones that we think are incremental and ones that have been raised before.

With that certainly Joyce and I will take questions from the group and certainly welcome any questions.

DR. ZIEMER: Any questions for either Joe or for Joyce?

Joe, do you know if there was any effort on the part of the Y-12 health-physics group to take the NTA film readings and adjust them according to the spectrum in the location of interest? Because typically, even in certainly in the late '50s and early '60s, facilities did that. I don't know what was done there, but...

MR. FITZGERALD: We certainly didn't see that. Again, this was covering a lot of ground in a short

time, but no, we didn't see any evidence of that.

DR. ZIEMER: Other questions or comments?

Wanda Munn.

MS. MUNN: Just wondering about the time element that we're looking at now, Joe. Have we started our six step process? Are we underway yet or just...

MR. FITZGERALD: You can consider today the launching of the process in a sense because we certainly have had a couple of very brief verbal exchanges of which this is going to be one today. But I think what's going to help this is to get into a more formal, documented process over the next month or so. And I would also add we haven't plowed into, as John was saying, the workbooks.

So to some extent maybe some of these questions will be answered by looking through some of the guides and workbooks that we have not seen because a lot of our questions are ones where the TBD seemed to be silent, wasn't clear. And we don't want to jump to the conclusion that there isn't a basis, but we can't see it in the TBD at this point.

MS. MUNN: I was heartened by the comments in the monthly report and the comments that John just made with respect to perhaps having a little experience under our belts now and being able to resolve some of

these issues which, to the outside observer, would appear to be major stumbling blocks but which really and truly aren't if we just have the right communication.

MR. FITZGERALD: Right, we have a full expectation to converge on a lot of these issues over the next month or so and we certainly take that to heart.

MS. MUNN: Thank you.

NIOSH PRESENTATION, DR. JIM NETON

DR. ZIEMER: And the SC&A review was dated September 19th, and so Jim's folks have had just only a few weeks to look at that, and they do have some initial responses which we're going to hear next so Jim, if you want to take the floor and have at least the first round of responses.

DR. NETON: I knew I should have gotten some throat lozenges for this board meeting. I'm going to be doing a lot of talking and hopefully you won't get tired of hearing me.

Again, I'd like to commend SC&A for doing a very fine job of turning over all the rocks and finding the warts that they might perceive in our profile. They do a tremendously thorough job.

As Dr. Ziemer just mentioned though, we only

received this profile on September 19th, and typically if any of you haven't looked at these in any detail, they're generally around 200 pages. That seems to be the target number SC&A shoots for and buried within them are a lot of individual observations.

And I'm encouraged from this conversation that for NIOSH to go and address literally the tens of observations that go into rolling up these findings, I'm not convinced is the best use and the way to proceed. I was very heartened to hear John Mauro suggest that this workbook issue may help us get to some closure on these issues because the fact is SC&A is eight site profile reviews into this that if history holds will be about 1600 total pages of material to address and many probably dozens, if not hundreds, of points.

So I will go through and briefly summarize what our take and observations have been on the findings to date. I've reproduced these here again, I guess more for my own edification, and I'll get right into our comments.

This monitored workers prior to '61, SC&A has raised some questions, and they were, we haven't really corroborated our contention over, the profile's contention that these workers were the

maximally exposed workers. This is somewhat critical. To me it's probably the most critical observation made because it speaks to our ability to do dose reconstructions.

And as one knows, we're in the throes right now of doing an SEC petition evaluation for workers in this time period. So if we look at this, SOL who put the profile together, took a sort of a three-part tack here and first did interviews with Y-12 staff which were primarily the health-physics staff, asking them what did you do? Did you really contend the monitored workers who were the most significantly exposed?

And the answer was a very resounding yes. They felt that yes, we did. We tried to monitor all workers with the potential to exceed ten percent of radiation protection guidelines. Of course, that in and of itself is not proof because as we know, health-physicists may be biased trying to cover up past sins or practices. But we went in and pulled out a few reports. ORAU has -- SC&A has not had an opportunity to see these reports yet.

These are mentioned in the historical dosimetry practices document that covers the beta exposures, skin doses. And there's a couple documents in there

that speak very nicely to this issue. That is, in 1958, around the same time period ORAU -- SC&A has found a Patterson document 57.

In that same time frame we see a couple reports by McLendon and one by Reeves that talk about let's get together with management and see does this make sense? Among the issues that they discussed with line management were what types of exposures were expected in these workers, what was the exposure potential, what were the typical exposures in the last year, the bracketing exposures low and high, what were the expected changes in the exposures in the near term, and what were the limits of detectability could we really see what we were trying to measure? And this was all in the context of trying to go from a weekly badge exchange to a monthly. So they really did try to consider the issues relevant with the line management to who should be on these monitoring programs.

The third leg of this analysis was the trend analysis of the monitoring data, and I think it speaks nicely and completes the little analysis, and if I can get the slide. It's very confusing. Don't glaze over on me here, but the blue line's what you have, the red dots on the very top are the maximally

exposed workers per quarters starting in 19, say, ‘48 or '50, I guess is where we had data here, going through 1980.

Now in 1960, as Joe Fitzgerald accurately pointed out, that they went to virtually a hundred percent monitoring of workers. What you see here is when they went to a hundred percent monitored workers, you see no change in the maximally exposed workers. One would assume that if the monitoring practices had changed, then in this time period the maximally exposed workers were not monitored, you'd see them starting to, you'd see the trend analysis over here. You don't. In fact, what you see is a downward trend in the analysis which is this is represented by the red line going out past 1960. So that tends to corroborate the analysis.

With a much better analysis of this ongoing, it's more straightforward and a little simpler to make the point. But I just put this up here as supportive evidence of the other two issues, you know, the other two areas that were investigated, that is, the health-physicists said they were monitored, and there's documentation that said they were monitored the highest, and here in fact, the data tend to support that issue. Again, I can't overemphasize the

importance of this. I think we need to be able to come to grips with this, and hopefully SC&A and NIOSH can discuss this and come to some mutual understanding here.

The other issue is this 50th percentile for bioassay data. I'm not sure where Joyce was coming from. I don't know whether she maybe misunderstood what our approach was, but what we're suggesting here is that we would apply the 50th percentile bioassay data to workers who were not monitored that in our opinion did not need to be monitored.

Again, these were the administrative-type workers, those who infrequently visited the process areas. These were not the people, the chemical operators with their nose in the operations, grinding, cutting, you know, purifying, doing things that would have a tendency to generate large airborne concentrations.

We believe that this value is very claimant favorable for those who were not monitored. They weren't on the program. We know that the program, given that they took almost a million air samples during this time frame, were very acutely aware of what the exposures, potential exposures, were in the workplace. We agree that certain maintenance

workers, staff, those types of folks, could have had exposures, but we don't believe that it's appropriate to assign them at the 95th percentile of the monitored workforce.

What you have in effect then, is a situation where you're assigning larger doses to 95 percent of the unmonitored workers than what the monitored workers ever received. So if you have a person with a complete bioassay record, 95 times out of a hundred, you would assign a larger dose to someone who had no monitoring. It just does not make sense to us. I went over this we gave the workers with the highest potential for exposure were monitored, and we need to discuss this with SC&A.

The other issue is a chronic exposure model was assumed here. We've talked about that. How we believe that this tends to cover incident-type exposures. Many results even among the monitored workforce were less than the detection limit. And again, I suggest that incident data is included in the analysis.

DR. LIPSZTEIN: Hello?

DR. NETON: Yes.

DR. ZIEMER: Joyce, you have a question?

DR. LIPSZTEIN: No, I couldn't hear.

DR. NETON: It must be my voice. I don't think I'm talking any more softly than others, but maybe Joyce is just being kind.

I'm getting into some issues I think that Joyce might be interested in hearing. We believe that the internal dose model adequately addresses chronic exposures. One thing that we need to look at, and we've taken some look at this. We have roughly, after 1952 or '3, I believe, about 30,000 urine samples per year. I mean, this was a fairly well monitored workforce. Well, the workforce that a lot of samples were taken on. I don't want to make any characterization judgment here.

But SC&A assumed, or they didn't assume, I think they inferred from our analysis that all samples were taken on Monday morning, and there is something to that effect in the profile. We've gone back and looked at the data, and in fact, probably it looks -- I can't remember the exact figures, but somewhere in the 20 to 30 percent of the samples were not taken on Monday morning. This makes a huge difference in the chronic exposure model.

I think that the SC&A review indicated there's a factor of three to four difference if you took it on Monday. It's sort of a, there's usually sort of a

worst case bracketing analysis. You're exposed all year to a chronic exposure. You take Saturday and Sunday off and you take one sample. That's a pretty rare occurrence in this program where most workers were monitored at least quarterly, more likely if they worked with uranium, weekly.

But again, the samples, about 30 percent of the, 20 to 30 percent were taken during the workweek which tends to minimize or mitigate that issue with this two day layoff. And in fact, a lot of that Type F stuff kind of gets smeared into the background when one does the analysis. We're still working this. It's not nearly as straightforward as the SC&A analysis implies. These workers were not exposed seven days a week, primarily there were five maybe six with a day or two off, sometimes overtime. So we're looking at this closely, but we don't believe it to be as big an issue as the review seems to indicate.

One issue here is the variability of the bioassay techniques. Joyce alluded to the variability of the sampling techniques and maybe the urine volumes and all that sort of thing. When you have 20,000 or 30,000 samples, and you generate a coworker distribution, it is our opinion that the inherent

variability in that distribution encompasses all those other sources of variability. In other words, you have the same people who maybe worked side by side and inhaled the same amount of material, so you've got a hundred people, variability is going to be inherently included in that analysis of the distribution.

So we need to talk about that with SC&A. We believe that this is a very appropriate technique to account for uncertainty. It's really the strength of large numbers. When you have 20, 30,000 or something, you have a pretty good feel for the variability of the process.

The solubility of material, there was some issue made about Type F versus Type S. The profile has adopted this Type M material. It's based on some pretty solid investigations by the Y-12 staff. We believe Type M to the greatest extent is representative of the typical exposure of the workers. We're not suggesting that we would ignore a special situation where we knew a person was working with uranyl nitrate solution or something like that and account for it.

But by and large the analyses that we've looked at -- and we'd be certainly happy to sit down with

SC&A and discuss them -- indicate that the Type M at Y-12 is a very reasonable approximation of the exposures. And again, this chronic exposure model we believe tends to bound incidental exposures.

We get into the issue of neutrons, I think Joe had a pretty fair depiction there. I think the profile review questioned whether we could see below an MeV, an MeV energy. There is a new report out ORAU Report 33, which I believe was available at the time this profile was evaluated, that addresses these issues. I think that we make a strong case that the nuclear track emulsion-type A film, that's what NTA stands for, is sensitive down to about 500 keV although we acknowledge that there are suppression factors that need to be applied.

As you go below one MeV, the film is less sensitive and the Report 33, I believe, includes some tables that indicate the extent that that correction factor needs to be made. And in fact, there are some recommendations from ORAU, and I'm sure whether it's in this report or not, that actually speak to the correction factors that should be applied on a exposure-dependent basis, whether it was a cyclotron or a californium source or something of that nature. So we think we've got a better handle on this. I

think ORAU has done a good job with this Report 33.

I might add SC&A, Y-12 has the most information written on it of any of the sites so far. If you add up all the pages that NIOSH has written with the site profile and all the technical information bulletins and the reports, we have amassed about 580 pages of written material on this site. If you add the couple hundred pages that SC&A has done, you're approaching 800 pages of written documentation on the Y-12 site. So I think it's some site we know quite a bit about although sometimes the more you know, the more it encourages debate and discussion which is good.

I think that covered, I know I have a couple areas of agreement that Joe talked about. We do agree from time to time. We do believe that the profile lacked significant discussion of thorium activities. There was a, this is what surprised me, actually, how much thorium activities there were at Y-12 at one point. In fact, I was surprised when we found the database that had 90,000 air samples for thorium exposure and 10,000 individual lung counts. So clearly, it was an operation that they knew had a hazard associated with the thorium as a pretty large dose per unit intake-type nuclide. And so there is a technical bulletin being written right now that

fleshes out this issue into much more detail.

We also agree that additional discussion is required for these radiation-generating devices. It's sort of a unique situation at Y-12. I mean, it's, there's three sites very closely involved, X-10, Y-12 and K-25. The resources were shared. It appears to us that a lot of the exposures from these radiation-generating devices were really under the purview of the National Laboratory. They happened to be located at Y-12, but it was National Laboratory personnel.

We're rethinking where the exposures need to be addressed. Is this really an X-10 issue or is this a Y-12 issue? And we agree that that needs to be discussed in some better detail. And wherever this characterization ends up, we recognize that the energy spectra are somewhat different than your normal, garden-variety Y-12 spectra with cyclotrons and x-ray machines and that sort of thing, and we agree that that needs to be fleshed out.

Now, I think that finishes my formal comments.

DR. ZIEMER: Thank you, Jim.

Now again questions from board members.

MR. GRIFFON: Jim, just one quick one. On the 50th percentile bioassay distribution. Does that, I

mean, is that, I think that's the data I just received.

DR. NETON: Yes, right.

MR. GRIFFON: Is that done on an annual basis or is it all the data put together to get a 50th --

DR. NETON: It's a monthly basis.

MR. GRIFFON: Monthly basis.

DR. NETON: We took the distribution of bioassay samples every month.

MR. GRIFFON: And does it include zero values or below detectible?

DR. NETON: Yes.

MR. GRIFFON: They're included in the distribution?

DR. NETON: Correct. Yeah, that's the only way one can do it. I mean, if you fit these lognormal distribution curves, you allow, you could take advantage of the fact that the first frequency point, it may be, and it may actually be in certain cases that the distribution, the 50th percentile, is actually below the MDA or the (unintelligible), the critical level of the analysis. I think that's pretty instructive though. I mean it shows that you can almost infer what the people are being exposed to even though it was below the detection limit because

it's a power of numbers.

If you have 30,000 samples, and your detection limit is say one DPM, and you have 80 percent of your samples less than one DPM, I think you have a pretty good feel of people being exposed to less than one DPM. And we can discuss that in some detail. I'd like to go over this with SC&A. This is not something NIOSH made up. I mean, this is a literature-type review stuff that is not commonly encountered, but it's a valid technique we believe.

DR. ZIEMER: Any other comments?

Okay, thank you very much.

So we're off to a good start on the Y-12 process of getting issues out there and resolving them. Hopefully, we'll be able to move along on this at a good pace and bring it to closure in a timely fashion.

ROCKY FLATS REVIEW STATUS REPORT

DR. WADE: I listed Rocky Flats on the agenda for this. John Mauro had sent out a note raising a Rocky Flats issue that he thought was particularly important to get before the subcommittee and the board, and that was the high five plutonium issue. And John had asked that he be given an opportunity to raise that.

I think there's been some subsequent developments, John, that maybe have tempered that, but it is on the agenda.

DR. ZIEMER: Yeah, and Rocky Flats is simply going to be a status report, and I think we have that issue from SC&A and probably don't have anything specific from NIOSH on Rocky at the moment.

But, John, you did raise that issue. I think the board was e-mailed on that at my request. You notified the board members of that issue at the time although it's involved a bit, but remind everyone what it is, and...

DR. MAURO: I just wanted to point out as we go through this process, and as we identify areas that we think are especially important to bring to the attention of the working group especially, and of course, NIOSH, what I did at that time, I think at that time I sent out an e-mail to, as sort of a heads up regarding two matters.

One was Bethlehem Steel and the fact that we had this concern about the need for an adjustment factor. And the other was to give an update of where we are related to some of the things we're seeing regarding Rocky Flats. In fact, the person who's actually alerted me to it has to do, the issues have to do

with lower limits of detection, urinalysis, three important points that I think we're going to be moving through. As we move through, you'll be receiving our Rocky Flats report. Actually, I have the full document in my briefcase, and I'm doing a read on it right now. And so it's moving through the pipeline.

But there are three areas that I think by way of a heads up that are important. And to the extent you want to get into at least some introductory aspects of it, three areas. One is urinalysis. Namely, in the early years of Rocky the main one which you were able to evaluate plutonium exposures, which were urinalysis. And we're finding that the lower ends of detection that you can see using urinalysis is associated with fairly high doses, very high doses to the lung. And that's going to be an important point.

The second point has to do with high-fired plutonium and its relation to urinalysis. You could almost envision if you inhale plutonium, and let's say it's a Type S. It has a certain kinetics, a certain amount will go to the urine. Now we raise the question of well, there are forms plutonium could take, and without getting into details where the clearance rate might be a lot slower and if it's of a

high-fired nature.

What are the implications of that? Because if you take a urine sample, and you see something, or you don't see anything, or you see something just above the lower limits of detection, how do you interpret that in terms of the dose to the lung or any other organ? If you're not sure whether it's Type S or it's Super S, and there are implications there. So that was the second area of inquiry that we're right in the middle of working very closely on.

And the third area has to do with when they, as the program matured, they took chest counts whereby in addition to urinalysis, which as I mentioned, you really can't see very, you have to have fairly high exposures, you get very high doses to start to see anything in the urine. But with chest counts, you see the americium 241 that's coming off.

And the issue has to do, and that really improves your ability to see a body burden, a lung burden. But one of the areas we're exploring right now, in fact, we discussed in one of our conference calls, working group conference calls, is that it's possible that you don't always have americium 241, and there was (unintelligible) of plutonium depending on the campaign. The amount of americium 241 that might be

associated with the plutonium 239, I'm sorry, associated, the amount of americium 241 that might be associated with the plutonium 239 could vary quite a bit.

So how you interpret the lung count where you're looking at the americium 241 because you're blind to the plutonium 239, so then you have to infer what the body burden or chest burden is of the plutonium 239 from the reading you're getting from americium 241. But if there's uncertainty regarding the relative amounts of plutonium 239 versus americium 241, then there's a lot of uncertainty regarding what the chest burden is.

So these are the three areas that we're looking very closely at and I think that at some point a working group meeting along these lines is probably going to be in order to pursue them a little further. And I just gave you a real snapshot of it. The people that have been looking extremely closely at this issue are Hans Behling and Joyce Lipsztein. So I mean, if you want to hear a little bit more about that certainly we can call Hans to the microphone and tell you a little bit more about where we are on making those investigations.

DR. ZIEMER: Thank you very much.

And on the high-fired plutonium issue, conceptually, I believe, the assertion is that the turnover rate would be so low you may not see it in the urine and still have a fairly hefty lung burden. In fact, the turnover may be so slow that if somebody asserts that, in fact, they, if a claimant says this is my issue, a claimant might not come up with it that way, but if we said a claimant may be in that category, it seemed to me that we ought to ask the question whether or not that claimant could not be subject to a lung scan and determine, now again, depending on the americium issue it may not be even determinable in that case.

But there also is a legal issue, I guess, as to whether or not you could require such a scan of a claimant. And I asked this question and wondered if somebody could at least look into that issue. This would be a case where you'd say if such were the case the high-fired plutonium should still be there, and therefore, be detectible possibly.

DR. NETON: I'm not sure that NIOSH would go to that extreme --

DR. ZIEMER: Well, that's what I was really asking.

DR. NETON: -- exposure. I think, you know, we

have taken a very serious look at this high-fired, super insoluble plutonium issue, and we need -- there's merit to it. I mean, the Rocky Flats certainly has situations where the plutonium is very insoluble. Our contention is though that if the material is so insoluble that it never leaves the lung, then the dose to systemic organs would be very small.

And if we assume that it's Type M material using a detection of limit for the bioassay meaning it leaves more rapidly, we'll be overestimating the doses to the systemic organs. If it -- we use Type S, right now to this day, I cannot think of any case that has not been compensated even just using moderately insoluble plutonium. So --

DR. ZIEMER: Well, let me ask, and I don't know the biology of this well enough, but could you have clearance to the lymph glands through macrophages?

DR. NETON: Yeah, that's an area we're looking into, whether the super insoluble material ends up being sequestered in the lymph nodes or remains sequestered in the pulmonary compartment. It turns out, this is a very, very valid, interesting scientific issues. It turns out then for instance in the (unintelligible) workers in Russia who were

exposed to very large amounts of plutonium, the plutonium does not appear to leave the pulmonary compartment. Now one does not know whether that's an inherent property of the insolubility of the plutonium or it may have more to do with the lung damage sustained either through the alpha activity in the lung itself or the concomitant chemical exposures that occurred along with these materials. So the short answer is we don't know.

DR. ZIEMER: Thank you.

Are there any other questions pertaining to this particular issue?

Okay, thank you. I'm going to ask the board members if, do you need a break before we -- I see levels of discomfort around the table. Let's take a ten minute break and then we'll reconvene.

(Whereupon, a break was taken after which the following transpired:)

SC&A CONTRACT TASK III REVIEW, NIOSH PRESENTATION

DR. ZIEMER: Our next topic is an item which is carried forward for awhile. It's the procedures review. One of our early tasks was to ask SC&A to review the procedures that NIOSH and ORAU use in dose reconstruction. And so we have the, and that was

Task 3 of the procedures review. That review has been completed for quite some time, but in a sense got back-burnered because of more pressing issues, dose reconstruction and SEC issues.

But now in the meantime, NIOSH has had a chance to review the findings of SC&A, and they in turn have provided responses. And our work group has gone over the NIOSH responses and is going to be recommending to us, and I think we'll actually do that tomorrow, what action the board should take on each of the items.

And board members, although we're not going to cover it in detail right now, you should have, there are three versions of the so-called findings matrix that's called Summary of Task 3 Procedure Findings Matrix. The first version of that, I think, was distributed, I don't know, a few weeks ago, and it had the findings of SC&A, and it cross-referenced the finding number and where it was in the report and so on.

Then NIOSH recently generated their response and so the matrix has the NIOSH responses included, and then Mark Griffon has generated a recommended action one. So the final version which, was that distributed today?

MR. GRIFFON: Yes.

DR. ZIEMER: Hot off the press, has a column even entitled Board Action which is a recommendation which comes from our, out of our work group, and we'll have a chance to act on that tomorrow. But we'll start out with the NIOSH response, as it were, and Stu Hinnefeld is going to bring us the presentation for NIOSH. Stu, I don't believe, is going to go through each individual item, but will --

MR. HINNEFELD: I'm not.

DR. ZIEMER: -- give us an overview of their response to the findings.

So Stu.

DR. WADE: Just a clarification. We have the complete work on the external dosimetry.

DR. ZIEMER: Oh, yes, right. The portion of the matrix that's been completed, by completed I mean includes NIOSH response and board action recommendation, is only roughly the first half of the matrix dealing with external dose reconstructions.

MR. HINNEFELD: Thank you.

I started out with just a little history of the project that was performed by SC&A. The final draft report initially was delivered back in January, and this was prior to any of the dose reconstruction

review reports, the dose reconstruction review reports that SC&A subsequently embarked upon, so they got the procedures and read the procedures.

And I think that they would also tell you, the reviewers would also tell you that after having reviewed some dose reconstructions, they now have probably a better understanding of the structure of the procedures and may have written things a little differently, which is not to say that necessarily anything they wrote is particularly wrong, but they may have focused on different things had they done the dose reconstruction reviews first then gone on to the procedures.

I've read some of the timeline up here. The first compilation or matrix of the findings was prepared, I think it was August 22nd. That was the date I found in my notes. It was just prior to the previous board meeting. And that activity or that process was extraordinarily helpful to us to take the report and to condense it on to a series of sort of cogent statements of the finding. It certainly made it much easier for us to provide some evaluation response to the finding in a more structured format.

Now on October 4th, the entire completed matrix was distributed and that was just prior, yeah, that

was just prior to an October 6th public meeting in Cincinnati of the working group where we went through the findings on the matrix from where we had our initial responses back from the, on the external dosimetry findings and discussed potential resolutions. And now I've not actually seen the recommended board action column, but that was generated at that meeting.

And then there were a few minor edits from the matrix that were provided a little bit later, but those were just strictly, they were mainly typo kinds of things or clarifications. I don't think there's any particular substantive change that came out after the October 6th meeting. But the version that was distributed has those included.

Our view of this is it was really thorough. It's big, and it was a very thorough and thoughtful review of procedures. I think it provides valuable feedback throughout. There's very little in the report that we take issue with, maybe a couple things that we think aren't particularly supported well. I think there might be some comments that expect more detail in a particular document than we had expected to put there. So there will be some things like, that might fit into that category, but generally, we were, we

thought it was a nicely done and well explained piece of work.

We have a number of corrections and clarifications and evaluations that we're doing as a response to that as well as probably canceling a couple documents that have probably been overcome by events. And these documents are maybe three years old and in the interim, you know, better procedural documentation is put out and better organizational assignment of responsibilities is identified. And so there are probably some things that were obsolete that we'll probably just do away with.

I've gotten on at the end of the distribution or in here the list of the procedures that were reviewed from the external dosimetry standpoint. I guess from our standpoint of, I don't know, there are about three pages of these and they are in the handouts, but these were the external dosimetry procedures that were reviewed as part of the process.

I guess I would categorize our initial response as, of course, we're pretty wordy too, so our initial responses are fairly long. But I kind of put them in maybe about five categories. There are a few comments that we would think are not well enough supported that we just don't think that they warrant

a revision. We think that we don't necessarily agree with the comments, but there are not very many like that.

There's some comments that we believe call for more detail in a particular procedure than what we intended that document to include. So we also would feel like there will be some procedures in that category, or comments in that category. Well, we wouldn't necessarily take issue with the comment. We understand why they would make the comment, but they may call for more detail at a specific point in our documents. And we would expect our document to have, so we would perhaps not propose to do that.

Plus, there are some findings that we feel point out a, they raise a serious technical issue that needs at least careful evaluation and quite likely modification to the documents we've published and then probably a retrospective look at what might be affected by this technical change in approach.

There's a category of comments that we don't think requires a technical change to the work completed. It doesn't cause us to change our technical approach to the work that we're going to do in the future. But it does require a much clearer description of the technical work we're doing because

the document doesn't provide that as it's written today.

And then we have a category of findings that we would consider valid findings. You're right; I'm not going to dispute the comment you made. But we don't know that there's a particularly high priority to go change this. If we can make a revision easily that corrects this, what gave rise to this finding, we'll go ahead and make that correction. Some of the revisions may not be that easy, and so we may not pursue those corrections, at least not any time in the near future.

Now let me explain a little bit what I meant. This would be a finding that the procedure is poorly organized, has extraneous information, has a lot of fluff in the middle and the important information is in the back, and you really ought to reorganize that. And reading it and looking at the procedure I'd say, yeah, that's probably right. That procedure should be, could be, better organized. But that's a fairly significant thing to do. That's a fairly difficult thing to do is to reorganize a procedure and put it in a system, you know, put it back in a format that you like better and rewrite the whole thing.

That's fairly significant and the people who are

working on the program, we don't have a lot of turnover of people working on the program either on our side or the contractor's side. They're familiar with how they're structured now. They know how to use and the way they're structured now, and so we don't know that we gain a lot by making those kinds of revisions.

And then, but there are some procedures that, or some comments that would say things like, well, you say that this document is giving an illustrative example, but it doesn't really seem to say that. It seems to be giving definitive guidance and the numbers that are there are wrong. And we would be, well, we should probably emphasize the illustrative example. We've not translated those numbers, and it's usually in the implementation guide, the high level document. We've not translated those into actual practice.

We're not actually using them anywhere so it's not like we've made a technical mistake or a technical error from having that document written that way. But that's a fairly easy amendment to make, edit to make. You insert a little language that says, this is included for illustrative purposes only and these numbers are not to be utilized as

guidance for this site. So that's a fairly easy edit to make while it's still in the relatively low priority case we think because we've not made technical mistakes, and we're not proceeding technically in accordance with those numbers.

So that's sort of a characterization of the types of our responses, and unless, I don't think you probably want me to read the titles of the procedures, and the projector clearly doesn't want me to either, so...

DR. ZIEMER: Thank you very much, Stu.

Let me ask Mark, do you want to add any comments now or just some general things out of the work group in preparation for tomorrow's actions on this document?

MR. GRIFFON: Yeah, I think the board actions go along those lines that Stu just mentioned with low priorities, medium priorities, high priorities. I'm not sure in some cases that I distinguished what he just said there in the last two examples where you might have a low priority but it might require a great deal of work as opposed to a low priority, fairly easy change. But I think that is reflected in the board action.

I guess the other thing that struck me, and when

you review these tonight I think it might be useful for the board members to look at this. I think in many cases we've sort of pushed the ball down the road on a lot of these, and that concerns me a little bit that we recommend a certain action which is to clarify a document, for instance, but it doesn't say exactly how it's going to be clarified. So we don't really have an answer to some of the technical issues. For example DCF, I think everyone agreed that they want to look into it further, but it's not clear that we have an answer there yet. So again, we've got this issue of tracking these findings over time and not losing track of them.

DR. ZIEMER: Let me make sure I understand. Your point is that although NIOSH may have agreed to modify or change something, we don't actually know what the modification or change would be other than saying, yes, we agree that this is an issue and we will change our document.

MR. GRIFFON: Right.

DR. ZIEMER: Nonetheless that, for the first round of doing this, that may be suitable. It may be that we will want to have a follow-up with you at some point and say, okay, what was actually done? I think that's what you're saying.

MR. GRIFFON: Yeah, yeah.

DR. ZIEMER: You don't really know what the final outcome would be. It could be modified in a way which really is unacceptable also is what you're suggesting.

MR. GRIFFON: Right.

DR. ZIEMER: It seems to me that that probably is some kind of a follow-up action on our part otherwise this goes on indefinitely. I think if NIOSH agrees that there's an issue, the issue's been identified, the implication is they will make the modification or change to correct or address that issue appropriately. Now there's still a possibility that you'd say, yeah, but you really missed the mark.

MR. GRIFFON: Right.

MR. HINNEFELD: Well, we expect to have an action list after this, once we have a board recommendation or a board action on these, we expect an action list for us.

DR. ZIEMER: Which would delineate actually what you did, and that would be kind of a final step --

DR. ANDERSON: Report back to them.

DR. ZIEMER: Report back with what actually was done. And that would be a good closure point, yeah.

Any other comments, Mark?

MR. GRIFFON: Yeah, the only other thing I was going to say is just when I put these board actions together, and it wasn't really obvious to me in the work group meeting, but on page four we break off from OCAS-IG-001 to ORAUT-PROC-0006. And basically, there's this default line that says that all the findings before that sort of apply to PROC-0006 also.

And I just wanted a clarification in my mind because it seems to me that the implementation guide is this high level document, but procedure six seems to be, at least in my way of thinking, it would be more prescriptive, maybe not prescriptive, but more prescriptive than the implementation guide. So I'm not sure that all the board actions are the same on six as they would be on those, on the IG items. And maybe Stu can help.

MR. HINNEFELD: Well, if I may just a little bit, procedure six actually repeats a lot of the information verbatim out of IG-001. And so to the extent that any of those comments in IG-001 are reproduced verbatim in procedure six, those would, it would be that kind, it would be relevant to procedure six as well. Procedure six has additional information. Typically, it's oftentimes included on an attachment. It gives some specific instruction on

how to proceed so, and since barring anything specifically on those, then we wouldn't consider findings to have been, you know, written against those.

But I believe this was captured this way, and perhaps I'm not the person to speak to it, but I believe it was captured this way to reflect that a great deal of the information in IG-0001 was captured verbatim in the procedure six which was copied over. And so if you got one, you got a finding in that and it's reproduced somewhere else, you got a comment that it's a finding on that procedure as well. I think that's what happened.

MR. GRIFFON: Any maybe we need to ask SC&A on this one, but I just wasn't sure if we inadvertently glossed over something that needed a more -- needed a fix in PROC-0006.

DR. ZIEMER: Hans, do you to --

DR. BEHLING: I will concur with what Stu has just said. That procedure six is really an abridged version of the implementation guide with the exception that there are a series of very, very specific attachments which then guide the dose reconstructor in terms of what he should do. But for the most part the central core of procedure six is a

facsimile of the implementation guide, and as Stu has already mentioned, what applies there in terms of criticism, would also apply to procedure six.

MR. GRIFFON: So none of the findings were for the specific parts that you talked about.

DR. BEHLING: No, no.

MR. GRIFFON: Okay, that was my question.

DR. ZIEMER: Thank you very much.

Any further comments or questions on this?

Thank you, Stu.

We will again return to the detailed comments when we deliberate at the, later in the meeting this week.

We're going to move almost immediately to the public comment period. In order to do that, I'm going to adjourn the subcommittee, and I declare the subcommittee adjourned; however, you may not leave.

(Thereupon, the subcommittee meeting was adjourned at 3:50 p.m.)

C E R T I F I C A T E OF COURT REPORTER

STATE OF GEORGIA

COUNTY OF FULTON

I, Steven Ray Green, Certified Merit Court Reporter, do hereby certify that I reported the above and foregoing on the day of October 17, 2005; and it is a true and accurate transcript of the testimony captioned herein.

I further certify that I am neither kin nor counsel to any of the parties herein, nor have any interest in the cause named herein.

WITNESS my hand and official seal this the 4th day of December, 2005.

_____

STEVEN RAY GREEN, CCR

CERTIFIED MERIT COURT REPORTER

CERTIFICATE NUMBER: A-2102