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

WORKING GROUP MEETING

ADVISORY BOARD ON

RADIATION AND WORKER HEALTH

NEVADA TEST SITE

The verbatim transcript of the Working Group Meeting of the Advisory Board on Radiation and Worker Health held in St. Louis, Missouri, on June 23, 2008.

STEVEN RAY GREEN AND ASSOCIATES
NATIONALLY CERTIFIED COURT REPORTERS
404/733-6070

C O N T E N T S

June 23, 2008

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-- (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.

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P R O C E E D I N G S

(10:00 a.m.)

WELCOME AND OPENING COMMENTS

DR. BRANCHE: Excuse me, we're going to start now -- hello, hello, hello, could you please un-mute the phone?

UNIDENTIFIED: Hello, hello?

DR. BRANCHE: Yeah, that little microphone thing.

Good morning. I'm Dr. Christine Branche. I have the pleasure of being the Designated Federal Official for the Advisory Board on Radiation and Worker Health, and we are about to begin the Nevada Test Site workgroup meeting. It is Monday, June 23rd.

Would someone who's on the phone please tell me that you can hear me?

UNIDENTIFIED: Yes, I can hear you.

DR. BRANCHE: Thank you so much. Mr. Green, are you ready?

THE COURT REPORTER: Yes.

DR. BRANCHE: Mr. Presley, are you ready?

MR. PRESLEY: Yes, ma'am.

DR. BRANCHE: Would all members of the working

group -- Board members who are part of the working group please announce your names?

MR. PRESLEY: Robert Presley, chair.

DR. ROESSLER: Gen Roessler.

MR. CLAWSON: Brad Clawson, no conflict.

MS. MUNN: Wanda Munn, no conflict.

MR. SCHOFIELD: Phillip Schofield, no conflict.

DR. ROESSLER: I have no conflict -- Gen Roessler.

MR. PRESLEY: This is Bob Presley, I have no conflict.

DR. BRANCHE: All Board members have been cleared for their conflict on this, but go ahead.

Any other?

UNIDENTIFIED: Josie.

MS. BEACH: Josie Beach, no conflicts.

DR. BRANCHE: Let me go over that number again. I've got Claws-- excuse me, Presley, Munn, Schofield, Roessler -- whose name didn't I call -- Clawson -- is that it in the room?

Okay, then we do not have a quorum. We can proceed.

Would NIOSH staff who are in the room please announce your -- sorry, are there any Board

members participating by phone?

(No response)

Thank you. Would NIOSH staff who are in the room please announce your names and please state if you have a conflict with Nevada Test Site.

MR. ROLFES: This is Mark Rolfes, NIOSH health physicist. I have no conflicts.

MS. ADAMS: Nancy Adams, no conflict.

MS. CHANG: Chia-Chia Chang, no conflict.

DR. BRANCHE: Any NIOSH staff participating by phone, would you please announce your names and state whether or not you have a conflict for Nevada Test Site?

(No response)

Thank you. ORAU staff who are in the room please announce your names and state if you have a conflict with Nevada Test Site.

MR. CHEW: Mel Chew, ORAU -- ORAU staff, no conflict.

MR. SMITH: Billy Smith, ORAU staff, conflicted.

MR. ROLLINS: Gene Rollins, ORAU staff, no conflict.

DR. BRANCHE: ORAU staff participating by

phone, would you please announce your names and state whether or not you have a conflict?

MS. HOFF: Jennifer Hoff, ORAU team, no conflict.

DR. BRANCHE: Thank you. SC&A staff in the room, please announce your names and state whether or not you have a conflict.

(No response)

SC&A staff participating by phone please announce your names and state whether or not you have a conflict.

DR. ANSPAUGH: This is Lynn Anspaugh. I have a conflict.

DR. BRANCHE: Thank you, Dr. Anspaugh, for announcing your name.

Other federal agency staff in the room, would you please announce your name, state whether or not you have a conflict.

MS. HOWELL: Emily Howell, HHS, no conflict.

DR. BRANCHE: HHS staff participating by -- I'm sorry.

MR. MCGOLERICK: Robert McGolerick, HHS, no conflict.

DR. BRANCHE: Sorry. Any other federal agency staff participating by phone, would you please

announce your names and state whether or not you have a conflict?

MR. BROEHM: Jason Broehm, CDC Washington office, no conflict.

DR. BRANCHE: SC&A staff in the room, would you please announce your names and state whether or not you have a conflict for Nevada Test Site?

DR. MAURO: John Mauro, SC&A, no conflict.

DR. MAKHIJANI: Arjun Makhijani, SC&A, no conflict.

DR. BRANCHE: Thank you. Are there any petitioners or their representatives who would like to announce their names?

MS. GLENN: Reini Glenn.

MR. FUNKE: John Funke.

MR. WHITE: Peter White.

DR. BRANCHE: Any workers or their -- thank you. Any workers or their representatives who -- would you please state your names?

UNIDENTIFIED: (Unintelligible) --

DR. BRANCHE: Any -- I'm sorry -- I'm sorry, sir, would you please announce that again?

MR. WHITE: Peter White.

DR. BRANCHE: Are there any members of Congress or their representatives, would you please

announce your names?

MS. OH: Katherine Oh in Senator Reid's office.

DR. BRANCHE: Would you please say that again, please, for the record?

MS. OH: Katherine Oh, Senator Reid's office.

DR. BRANCHE: Thank you, Ms. Oh. Are there any others participating by phone who would like to announce their names?

MR. ROGERS: Keith Rogers, Las Vegas Review Journal.

MR. RICH: This is Bryce Rich with ORAU team. I came on just a bit late. I'm conflicted.

DR. BRANCHE: Thank you for announcing your name.

Before we formally begin I would ask that everyone participating by phone mute your lines. It is important for the quality of the participation for the phone participants that every single person participating by phone mute your lines. If you do not have a mute button, then please use the star-6 to mute your phones. We would value your interaction and when you are ready to speak please use that same star-6 to un-mute your phones when you are ready to speak. Again, it is important that everyone

participating by phone mute your lines because, if you do not, then other people participating can't hear the infor-- the discussion.

As well, for phone participants, if you must go away from the call, please do not put this call on hold. That's -- that subjects all of us to whatever music or sound your hold function has for us. And I thank you for your cooperation and your observance of phone etiquette, and we'll get started.

MR. PRESLEY: Just had somebody else enter the room.

DR. BRANCHE: Would you please announce your names and tell us if you have a conflict?

DR. ANIGSTEIN: Robert Anigstein, no conflict.

DR. BRANCHE: Mr. Presley, you are welcome to begin.

INTRODUCTION BY CHAIR

MR. PRESLEY: All righty. Good morning, I thank everybody for being here. At this time I want to open any issues that we have had that are still on the table in the past for discussion. John, I think you had one.

ENVIRONMENTAL OCCUPATIONAL EXPOSURE

DR. MAURO: Yes, over the past week I read

through the new Chapter 4 in the TBD, the one dealing with environmental occupational exposure, and -- and -- and I'd like to first say that it does contain all of the new strategies that were discussed in the white papers before, so it's a -- it's basically a rewrite. And I reviewed it carefully and then I checked some numbers just to see if -- you know, if things seemed to ring true. And -- and I did come up with one issue, but it's a fundamental issue, that I wanted to leave with the working group. In fact, over the weekend, given that -- I didn't just want to drop new information. I did call Jim Neton and Robert Presley just to brief them about what my concern is. I'm not s-- now -- so what -- concern goes like this, and correct me if I misunderstood anything that's in Chapter 4. The fundamental strategy for doing inhalation doses from airborne particulate radioactivity post-1963 is to take advantage of the enormous amount of air sampling data that was collected beginning in 1971. And my understanding was continuous air samples collected in many locations and -- for pluton-- I'm particularly

talking about plutonium-239 right now, 'cause that's the hook and I like that as your hook. Now I looked at the numbers and I see that from site to site -- Area 1, Area 3, 5 -- you know, all the different areas -- the -- the level of plutonium in the air is on the order of -- just a rule of thumb -- about ten to the minus four, I think it was picocuries per cubic meter. I got it wri-- in fact, if you give me a second we can see how I -- yeah, three times ten to the minus four, five times ten -- anyhow, and -- and it's area to area, year to -- by year, and you -- these are air samples, continuous air samples, as I understand. So you've got a really good handle starting in '71 of what the airborne dust loading -- so the way I look at it is, you know, for -- for reconstructing inhalation doses from plutonium.

And then on top of that you say okay, what we're going to do is we're going to use the plutonium as a hook for the other isotopes by prorating, 'cause you know what the mixes are, and that's a good idea and that works.

And so -- and so my first impressions, and this -- by the -- this is the first time even the

SC&A people are hearing this because I really did this over the weekend, and not everyone -- that's the way it is. You know, there may be some disagreement. And remember now, it's very important to realize we're talking about environmental occupational exposures. These are areas where people are working and -- and these are not radiation control areas now. These aren't areas where -- there's a fence. There's a contr-- access control. This is just the area -- and there are people out there working. So -- so -- so I want to make sure we make a distinction in this wa-- the reason -- between people who are under some type of radiation control and people who are working outdoors doing their jobs and just breathing in resuspended dust that's blowing all over the place and it's just out there.

And I say okay, my first perspective is, starting in '71 it looks like you've got a lot of data and you've got the wherewithal to factor in other radionuclides that are in -- in the air. But then I said let me go back to -- okay, now how are we going to go back to '63? And here's where I start to run into some

problems. I say okay, let me go back and look at the activity that's on the ground, say, in becquerels per meter squared, of plutonium-239. And there's -- I forget the fella's name, starts with an M, McCaldwell -- there is a -- an author, one of your -- what's his name?

UNIDENTIFIED: McArthur.

DR. MAURO: McArthur, McArthur has lots of reports and we -- we've seen these before where you have becquerels per miter squared on the ground, and -- and certain assumptions could be made to convert that into becquerels per gram. And in fact you've done that in the past by assuming what they call a relaxation link over some two and a half centimeters -- in other words, some reasonable assumptions, that are probably accurate within a factor of two or three, of what the picocuries per gram of plutonium-239 is in the soil in all the different areas in 1963.

DR. MAKHIJANI: McArthur dates from the '80s, John.

DR. MAURO: Okay. Whe-- when you look -- the really reason I felt that you could --

DR. MAKHIJANI: If I remember it correctly.

DR. MAURO: Yeah. But the plutonium numbers seem to be very rob-- the -- the -- in other words, when you look at the air sampling data, they're almost like -- they don't change that much from '71, '72, '73, '74, '75, so -- in those later years, so -- and that's coming from resuspension. I mean you have your tables -- I have them in front of me, in fact, and -- you know, they vary from ten to the minus four to ten to the minus five, that's about it, no matter where you look, no matter what area you look in. So it's almost like the dust loadings, starting in '71, are all in the range of ten to the minus four to ten to the minus five.

I say okay, so this -- this tells me that at least in those years, what I call the later years, you've got a pretty good handle on the airborne dust loading. Now -- and I said what do I do -- how do -- how do I get a handle on '63? Now -- 1963 -- the approach, as I understand it, that NIOSH used -- say listen, we realize that if you go backwards in time from '71, when you do have air sampling data, to '63, which is the time period that starts

our area of interest, you've got to bring the numbers up a little bit because we know that the availability for resuspension declines as a function of time, is the -- the Ans-- the Lynn Anspaugh curve. All right? And you'll -- but you basically have done, as I understand it, is say well, we're going to take the airborne concentration that we observe in 1971 and multiply it by three -- 3.12, to be exact -- and that's the concentration we're going to assume is in the air of plutonium-239 in 1963. Okay, I say -- then I say to myself, and this is where, if you're following this so far, where I ran into a problem. I said okay, now I have -- I have activity on the grou-- on the -- in the soil of one times ten to the fourth becquerels per meter squared measured in the later years. I'm going to say, just for the sake of argument, let's assume that's pretty con-- that doesn't change that much. Okay? That -- and that activity is distributed over a given area and has a certain vertical profile. Okay? And I realize that is a crude assumption, but we're talking about less than an order of magnitude crude assumption,

(unintelligible) factors of two or three, you're never going to get better than that. Then I say okay, so now I have a handle on the becquerels per meter squared in the soil in '63 -- 1963. And I have your airborne concentration that you would predict would be in the air in 1963, and I -- and I say okay, what -- then I back out -- what would that resuspension factor be? In other words, in effect what I'm solving for is a resuspension factor, and does it ring true with my experience -- and it's quite a bit -- with resuspension factors. And I come up with a resuspension factor that's on the order of ten to the minus nine, maybe approaching ten to the minus eight, per meter. So in effect -- this is where I ran into a problem.

I came into a problem that says the approach that's been embraced in the new Chapter 4 effectively adopts a resuspension factor for the contamination of the plutonium in soil for 1963 that's on the order of ten to the minus nine to ten to the minus eight. My experience with resuspension factors outdoors, especially if there's any type of anthropomorphic activity

going on -- like trucks, people walking and working and digging and whatever it is -- is closer to ten to the minus five, maybe ten to the minus six, certainly ten to the minus four would be conservative.

So I walk away from this saying I would have come up with air dust loadings that were several orders of magnitude higher for 1963, and that -- and when I -- when I run into order of magnitude disparities, with my experience and knowledge of a subject, I think that's -- to me, that's -- we've got to zero in on that. Everything else that I saw in the write-up were factors of two and three. And I'm not -- and I've got to say that we could work with that. We could always say well, maybe we should be a little more conservative here or -- but when I see two, three, four orders of magnitude possibili-- I'm only saying this is a possibility -- concern, and it's based solely on my review of literature dealing with resuspension factors, I get concerned. And I -- and I called Ji-- and I said -- and I said -- I called Jim and I called Robert and I expressed this to say listen, I wa-- I don't

want you to hear it for the first time at this meeting, but -- everyone is, but some haven't. And I also came up with an idea for a way to find out whether I'm off base or whether maybe I'm right, and this is my idea. When the air samples were collected in '71, '72 -- 1971, '72 and '73 to determine picocuries per cubic meter of plutonium in the air, I'm pretty sure they must have weighed -- they took the filter paper off the fil-- off the air sampler and weighed it, and they know how many milligrams or micrograms of dust there is in the air per cubic meter, and these were continuous samplers collected over the course of a year at all these locations, many, many of them, so there's probably out there a pretty rich database giving you some realistic estimate of what the milligrams or micrograms -- I mean the numbers I'm used to seeing are a low of maybe ten to 20 micrograms in a pretty quiescent area to easily milligrams per cubic meter. Now someplace in there is probably where typical time-averaged dust loadings are for the -- for the Nevada Test Site at any time. So see, I look at the dust loading in milligrams or micrograms per

cubic meter as a characteristic of the site, whether we're talking 1960s, 1970s, a characteristic of the site -- of course not when they were detonating the above-ground, but after -- you know, we simply have a site with this normal wind blowing, anthromorphic -- pomorphic activities going on all the time. There -- there are dust devils, as I understand, that happen every so often, and you have this long-term situation where you -- if you have a long-term air samples, you could start to get a pretty good feel of what the long-term dust loading is in micrograms per cubic meter.

Well, in my mind, if we can get a handle on that number and its variability and wha-- its range, maybe it even differs a little bit from area to area, and we also have becquerels per meter squared on the ground, I think we go back and revisit the dust loading approach using realistic dust loadings.

Now previously we had this conversation, about six months ago, where the idea was embraced but one of the assumptions that were made was that well, we'll assume it's five milligrams per

cubic meter all the time to all people everywhere. And I'd be the first to admit, that's not real. You're going to come up with a dose that's off by at least a factor of ten. So I had -- when -- when you decided to, you know, walk away from that strategy, I fully appreciated and understanded (sic) because that's not plausible.

But what is plausible is something on the order of -- again, this is -- if you could actually have the measurements, we're low. It may turn out the average annual dust loading in the air is only 20 or 30 micrograms per cubic meter, and then you've got a rock to stand on. You say listen, we now it's -- this is the dust loading and we know the becquerels per meter squared. We've got a pretty good idea of the relaxation length over which that plutonium is distributed, so we have a pretty good idea of what the upper level of, you know, becquerels per gram is in the soil in Area 1, in Area 2, for 1963. We multiply that by a dust loading and you've got the problem solved. And I think you're going to come up with exposures which are several orders of magnitude higher than the

numbers you currently have in this report. So that's what I walked away with from reading this report, and I put that on the table.

DR. ANIGSTEIN: I'd like to make a couple of additional comments. First of all, one thing that they have -- according to this Chapter 4 -- going back into the '60s is gross alpha measurements. And so I was surprised that they didn't think to try to determine what fraction of those gross alphas were plutonium. For instance, based on -- assuming that they continued taking gross alphas after they -- at the same time they were taking plutonium samples, then it would be relatively straightforward to say okay, for this level of plutonium, this is what the gross alpha count is. And then when we have only gross alphas, you could prorate those super-- and to get an estimate of plutonium.

The second point, separate from this, is in terms of this Arthur data on the inventories of plutonium on the ground, EG&G did a fly-over survey in 1982 of -- I happened -- they did several areas; I happened to look at Area 11, which is Plutonium Valley -- they call it

Plutonium Valley and it's not exactly the same as Area 11. It's most of Area -- it's part of Area 11, goes a little bit into Area 3. But anyway, they came up with inventories that were ten times higher than -- in the Arthur report than what is here in Chapter 4, curie inventories. They came up with something like 240 curies for Area 11, of -- of plutonium, and this -- and here we have something like 29 curies. So that's a significant difference, which can't just be dismissed.

They also had a ground -- EG&G had also done a ground level Fiddler* survey which came up with lower numbers, but it was a very much smaller area. So one of the reasons why the two would not coincide is the fly-over had a very, very wide angle of view, so it may not localize pro-- properly. However, for the ar-- for the area as a whole, it should be fine.

(Unintelligible) the (unintelligible) be better 'cause it does automatically average this out. So I think that's something that should be considered.

And the other point, which is in slight disagreement with -- I mean (unintelligible)

consistent with what John said but perhaps -- I mean any -- okay. I have another point.

MS. MUNN: Your voice is very soft in general --

DR. ANIGSTEIN: Oh, I'm sorry.

MS. MUNN: -- and even for those of us at this end of the table --

DR. ANIGSTEIN: Okay.

MS. MUNN: -- it's a little difficult for us to hear.

DR. ANIGSTEIN: I'm sorry.

MS. MUNN: Project just a little more --

DR. ANIGSTEIN: Okay.

MS. MUNN: -- and we will appreciate it.

DR. ANIGSTEIN: Will do.

MS. MUNN: Thank you.

DR. ANIGSTEIN: The air sampling data, whether it's plutonium or whether it's dust, isn't area-wide. Now what we're concerned with, we're -- for dose reconstruction is the breathing zone sample, which of course weren't taken. But by -- but conceptually, this is where the person actually is. If this is where the person is, he's going to be stirring up dust. He's going to be walking, he's going to

be working, he's going to be digging and whatever else they do -- driving a vehicle. There's going to be a lot more dust where that person is than this wide, empty, uninhabited space over the course of a year. So immediately there is a bias there which is claimant-unfavorable in using either the dust loading approach or the resuspend or the air sampling approach.

And then finally, in terms of the actual dust levels, the only thing I would -- I happened to come across, I only worked on this for a few days, is a 1993 cost benefit analysis for cleanup put out by DOE. And there they just make reference to the fact that, taking four samples from widely dispersed areas, they said typical rural dust loadings which would be applicable to the Nevada Test Site are 20 to 40 micrograms per cubic meter. That's -- it's just a statement that's in there, but it's -- it's the only place I've found a number -- an actual number where dust loadings were referred to, so that's -- that's it.

DR. MAKHIJANI: If I might -- this is Arjun. If I might supplement that, Lynn Anspaugh

brought up a similar point on a number of occasions. I believe we actually have something shows a -- heavy equipment that Lynn sent around. We haven't printed them out or anything, it's just part of our review that's ongoing and -- I don't know -- Lynn, are you on the phone? I guess he's not --

DR. BRANCHE: Well, give him a moment to un-mute. Give him a --

DR. ANSPAUGH: Yes, I'm here. I just had a little trouble getting my mute button adjusted.

DR. MAKHIJANI: Okay, so I -- I didn't know whether you were on the phone because I came a little late, so I don't need to stand in for you.

DR. ANSPAUGH: Okay. I appreciate the comments that were just made. In fact, I have some very similar comments that I made from time to time, and one -- one of my chief problems that I mentioned several times is where were these samples located -- air samples -- where were they located and what was the purpose for taking them. And I've read a lot of these reports and -- for example, in 1964 there were 14 samples -- samplers, and I would just like

to read to you the location of these samples. In Area 3, it was the cafeteria; Area 5 is (unintelligible), another Area 5 is Gate 250; Area 6 was dispensary; Area 9 was dispensary; Area 10 was Gate 700; Area 12 was cafeteria; Area 16 was dispensary; Area 18 was Camp 17 dispensary; Area 20 was dispensary; Area 23, which is Mercury, was Building 214; Area 25, which is NRDS, was LASL H-8 facility; Area 27 was dispensary; Area 51 was dispensary. And then there was a comment made by the people who were writing these reports that said specifically results of environmental surveillance sampling activity values obviously cannot be used in calculating personnel exposure doses.

So I think the comment that people who were out in the field that -- bulldozers and dragging drill rigs from one location to another cannot be represented by these stationary air samplers that are located mainly in -- adjacent to cafeterias and dispensaries.

And one other thing I wanted to mention was that it's frequently stated that atmospheric testing stopped in 1962, and that's not exactly

true. We had four plutonium dispersal tests in 1963 that were just beyond the Test Site, but nevertheless they did have significant effluents that were detected even off-site, so there could have been a -- I'm sure there was a major perturbation of the plutonium levels -- airborne plutonium levels in 1963.

In addition to that, we had five PLOWSHARE experiments, which were permitted underneath -- under the treaty as long as they didn't cross international boundaries, and so we had five tests that took place in the Test Site between '64 and '68 that substantially contaminated the area. In fact, some of these shots even contaminated the drinking water supplies.

So I -- I think Chapter 4 is -- is not claimant favorable by any means.

MR. ROLFES: Okay. This is Mark Rolfes. Before I address some of these questions and concerns that SC&A has raised, I would like to thank everyone that has provided information to NIOSH so that it can be incorporated into the site profile. I know that John Funke has specifically been spending a -- a bit of time to ensure that we have put together the most

complete and scientifically valid site profile to use for EEOICPA dose reconstructions for Nevada Test Site. I would like to thank him and the other people that have made contributions to our work.

The current approach that we have in our Nevada Test Site environmental intake chapter does rely upon air monitoring data which started in 1971 at Nevada Test Site. These were ambient air samplers that were set up in various areas, as Lynn Anspaugh has mentioned, in Area 1, 2, 3, 5, 6, 7, 9 and 10, 11, 12, 15, 16 -- let me make sure -- 18 -- excuse me -- 19, 20, 23, 25, 27 and 28. From thousands of air sample results, in order to be claimant favorable -- now mind you, we do only have air sampling data in complete sets beginning in 1971 and continuing through 2001. From those thousands of air sample results NIOSH has hand-selected the single highest plutonium ambient air sample result to use for reconstructing historical radiation exposures. We have taken that single air sample result for plutonium and decay-corrected it back to 1963, so we've chosen the single highest air sample result which occurred

-- it was documented in 197--

MR. ROLLINS: Area 9 -- Area 9, 1972.

MR. ROLFES: Okay, thank you, Gene. It was Area 9, 1972. We have singly -- we have picked out that single highest sample result, used that to decay-correct back to 1963, and then applied a maximum scaling factor to add in other radionuclides in ratios to the plutonium-239. So we've taken the highest sample result. We've applied the highest scaling factor, and we've also assumed that a worker was exposed to that concentration for essentially twenty-- is it 21 --

MR. ROLLINS: (Off microphone) Forty hours (unintelligible) --

MR. ROLFES: -- 40 hours per week --

MR. ROLLINS: -- (unintelligible) 600 cubic meters per year.

MR. ROLFES: Okay. So -- which is roughly -- we basically have assumed --

MR. ROLLINS: (Off microphone) A standard -- standard breathing (unintelligible) 40 (sic) hours a day --

MR. ROLFES: Okay.

MR. ROLLINS: -- five days a week.

MR. ROLFES: So for the entire year we have assumed that that individual was exposed to that air concentration, that single highest result with the single highest multiplication factor, scaling factor, for other radionuclides. We haven't taken any credit for respiratory protection. And that was our basis for dose reconstructions.

I'd like to call everyone's attention to the revision on page -- oops, let me -- on page 75 of the Technical Basis Document we have compiled a list of the organs for which internal doses are calculated that had in excess of one millirem from 30 years of inhalation and ingestion at this level. And Table B-1 shows the internal doses resulting from these ambient intakes at the Nevada Test Site. If you take a look, for example, the lower large intestine dose would be one millirem per year from this level of exposure for 30 years of exposure.

We do acknowledge that there's uncertainty associated with the measurements that we have used. However, we feel that this -- this approach is claimant favorable and that it's

defensible, meaning that we've hand-selected the single highest ambient air sampling result. We can look into additional information that would allow us to refine our dose estimates. However, the amount of work that would be necessary would not significantly contribute to higher internal doses.

I believe -- Gene, do you have any additional information --

MR. ROLLINS: Let me make one --

MR. ROLFES: -- to add to that?

MR. ROLLINS: -- one observation. I -- in addition to assigning intakes for plutonium-239, if you go to Table 4.4-6 on Table 23, we're also assigning intakes of other radionuclides, and one of those happens to be cobalt-60. If we -- just as a -- as a thought, if we increase those intakes shown in this table by a factor of 100, then those intakes would be readily seen by whole body counting, and we have no evidence that any positive cobalt-60 whole body counts were observed at NTS. So I don't think it's a factor of 100, John. It's something lower than that.

DR. MAKHIJANI: Well, I have a question for --

couple of questions for Mark, just on what you said. When you say decay-corrected for plutonium back eight years, what decay correction?

MR. ROLFES: Not very much.

MR. ROLLINS: (Off microphone) Most of those decay corrections were (unintelligible) to all the others here.

DR. MAKHIJANI: Okay, so for plutonium anyway there's no decay -- essentially no decay correction. The -- the other thing is, the other radionuclide question has been raised -- and again, this was raised by Lynn but I don't want it to fall between the cracks -- that there's a fractionation problem in terms of relative amounts of various radionuclides. I don't know whether you use the Hix* Tables --

MR. ROLLINS: That's been corrected.

DR. MAKHIJANI: It's been corrected.

MR. ROLLINS: It's been corrected.

DR. MAKHIJANI: I just wanted to --

MR. ROLLINS: We've enriched the (unintelligible) field with refractors.

MR. ROLFES: Right.

DR. MAKHIJANI: Okay, so that's been -- is that

on the old --

MR. ROLLINS: (Off microphone) (Unintelligible)

DR. MAKHIJANI: Oh, it's in there, okay. I haven't looked. Thank you.

DR. MAURO: Let me pick up on that 'cause that's good. I didn't realize -- if I'd read more carefully -- in effect, I looked at the table, the -- the central numbers for all these couple of hundred numbers here, around ten to the minus four, you picked ten to the minus three. That's the highest number in the table.

MR. ROLLINS: Yeah.

DR. MAURO: So you're about a factor of ten higher --

MR. ROLLINS: Yes.

DR. MAURO: -- right off the bat. Now -- and -- okay, now -- but still -- and -- and that -- you know, that's good that you're trying to find a way to accommodate the uncertainties, accommodate this time variant issue. But in my mind, you don't have to resort to that. You could just go back and look at what the dust loadings are if they're out there. In other words, every single one of these samples probably has a microgram per cubic meter, a

number that ti-- we'll start to get a sense for what is the average annual dust loading at the site -- notwithstanding Lynn's point, by the way. I wasn't aware that, you know, there was this concern that perhaps the air samples were not taken where the people were doing this mechanical work. I mean that's -- that's a separate issue.

Right now I'm operating on the premise, given that the air samples that were collected were collected at a place where people are and is generally representative of the dust loadings that people experienced -- given that, and I'm not -- now from what Lynn said, that may not entirely be the case. But if it is, and if you do actually have information on what the real dust -- when I say dust loading, milligrams or micrograms per cubic meter, you don't have to resort -- it may turn out that that's too conservative. You see, I'm ready to go to the point where I say I might be entirely wrong for the reason you just said, but everything I know about resuspension factors tells me that ten to the minus nine, ten to the minus eight, is not a good number.

MR. ROLLINS: May I comment on that?
DR. MAURO: Yeah.
MR. ROLLINS: This is Dr. Anspaugh's own model based on empirical data from the Nevada Test Site. And you've seen this curve --
DR. MAURO: I -- and I --
MR. ROLLINS: -- and you see what the number is.
DR. MAURO: And I see why it happened. It drops three orders of magnitude within the first hundred days, (unintelligible) --
MR. ROLLINS: Let me -- let me comment on that.
DR. MAURO: Yeah.
MR. ROLLINS: The last ato-- the last atmospheric shot was July 17th, 1962. If we're starting our area of interest in 1963, that's practically 180 days. We're off the hump --
DR. MAURO: But I don't buy this --
MR. ROLLINS: -- according -- according to his -- to his model.
DR. MAURO: See, I don't necessarily agree that this curve is -- is applicable to the problem that we're talking about where we have people physically -- and we have Lynn on the line. Lynn, please, you cor-- I mean we -- this is

the first time we -- we're engaging this issue, but --

DR. MAKHIJANI: No, we -- we did this before.

DR. MAURO: We -- no -- yeah, and we -- we did, okay. We did do it before.

MR. PRESLEY: Yeah, this -- this -- let me tell y'all something -- this is Bob Presley. This discussion started in March of 2007. This issue was closed in December of 2007, so this has been discussed before, and a lengthy discussion.

DR. MAURO: It -- it was closed when it was five milligrams per cubic meter. Then a -- then a reversal occurred.

UNIDENTIFIED: Well, let's --

DR. MAURO: And that's okay --

UNIDENTIFIED: -- let's --

DR. MAURO: -- that a reversal occurred, but now -- so we're really back -- okay, we're returning to the resuspension factor approach. And granted, Lynn's curve is here. I'd like to hear a little bit -- in effect, according to Lynn's curve, you've got this enormous elbow that occurs at 180 days, and we -- and -- and you take -- and it's working very -- serving

you very well 'cause your adjustment factor's only three.

DR. ANIGSTEIN: But --

DR. MAURO: In fact, if you -- okay, Bob.

DR. ANIGSTEIN: But Lynn just said the testing did not stop in '62. There were tests in '63 and through '68 that -- that were responsible for -- they may not have been violations of the treaty, but responsible for dispersion of the plutonium, particularly the safety -- the safety tests, by definition, were not nuclear bomb tests 'cause they did not have a detonation.

DR. MAURO: And that --

MR. SMITH: Those detonations were not on the Nevada Test Site.

DR. ANIGSTEIN: Oh.

DR. ANSPAUGH: Those detonations were just off the Nevada Test Site, but Billy, they were detected off-site and they certainly were detected on-site.

MR. SMITH: Lynn, the wind blows generally northeast, so they couldn't have -- they could not have been detected on-site.

DR. ANSPAUGH: Well, they were.

DR. MAURO: Well, see -- wait, wait, see, we're operating (unintelligible).

DR. ANIGSTEIN: The safety -- according to the DOE report, the safety tests were done in -- in Plutonium Valley.

MR. SMITH: That's Area 11 on the Nevada Test Site.

DR. ANIGSTEIN: That's right.

MR. SMITH: He's talking about a place that's off of the Nevada Test Site.

DR. ANIGSTEIN: But the safety tests were --

DR. ANSPAUGH: No, we're -- we're -- we're con- - we're confusing the tests in 1955 and those in 1963.

DR. MAURO: Wait a minute, we -- Lynn, there's layers of issues that -- I -- in other words, you're raising issues related -- on one level. I have a really fundamental issue. My fundamental issue is that a resuspension factor of five times ten to the minus nine -- basically that's what you effectively adopted - - is being applied to the surface contamination, notwithstanding whether we -- you know, given that the surface contamination in becquerels per meter squared is in fact a

robust, reliable model, and given the assumption that you can establish a vertical profile for that -- which I believe you can -- I find it very hard to believe that N to the minus -- five times ten to the minus nine is a good resuspension factor for this circumstance, notwithstanding Lynn's curve. So I might right now be, you know, crashing heads with Lynn. I don't buy that resuspension factor as applied to this situation. I think the resuspension factor is going to be closer to ten to the minus six.

MR. ROLLINS: Comment.

DR. MAURO: I mean and that's what I'm saying.

MR. ROLLINS: Com-- comment, please. My model does not assume a resuspension factor. The only time I bring in resuspension factors is to account for short-lived fission products, fission and activation products. My model is built on empirical air measurements.

DR. MAURO: But -- but your model, in the end, results in a resuspen-- in other words, yeah --

MR. ROLLINS: It's an implied resuspen--

DR. MAURO: It's an imp-- of course, and that's how I checked the number and I said -- whenever

I check a number I say does it ring true, how do I come at this number, and ask myself does it ring true for me. 'Cause on face value in your -- this looks great. But then I said but I know something about resuspension factors, and I say does it hold up. And I went back and I did a calculation and I said my goodness, they got a resuspension factor that's -- that's -- well, I didn't work with the 4.3 to the minus three, by the way. I worked with the 3.7 -- I worked with one of the numbers and just checked it, and I came up with five times ten to the minus nine as a resuspension factor. And at that point I said I've got a problem. And it wasn't some, you know, deep, penetrating -- I says that just doesn't sound right to me, and that's when I immediately wanted to communicate this concern, this -- to Jim and to Robert, and I wanted to put it on the table. So -- and now -- now on top of that, obviously, we've got other layers and -- see where I'm starting. I'm starting at giving -- basically accepting a lot of information. I'm accepting the becquerels per meter squared number. I'm accepting the air sampling data as being taken

in the right areas and -- and are representative. Given all that -- I mean accepting that --

UNIDENTIFIED: You mean for the sake of argument.

DR. MAURO: For the sake of argument, I'm accepting it. For the sake of this discussion, let's just start at the simplest level, and at the simplest level I'm saying even accepting all that or on -- on face value, I have a problem with the resuspension factor that's implied in the model.

Now, you know, once we could get by that -- and maybe we can, and one way to get by that is to check what the dust loadings actually were, which I believe the numbers are out there -- and we may find out, if you pull the records from when they took those air samples in '71/'72, that we know what the milligrams or micrograms per cubic meter is and we may find out that your approach is right on the button. Or we may find out that no, you're low by two orders of magnitude. And we could find that out.

Now whether or not that data are available, but

-- but in my mind, it should be -- that data should be available because every time I ever took an air sample I always weighed it. I take the Wattman filter paper, you know, you -- you weigh it before, you weigh it after, so it's got to be in there somewhere. If it's not, that's the end of my story. But if it is, you've got a hook on -- on what the dust loading is. And once you've got a hook on what the true milligrams per cubic meter are in the air at this site, you have a very, very strong platform to stand on, say now we're going to apply that to what we know to be the activity in the soil of plutonium-239.

Now we do have some questions and maybe we don't know what the plutonium is, but that's now a second -- to me, now we're moving up the ladder on the -- on -- but the very beginning -- to me, the ground -- the rock you're standing on is -- is that, you know, you believe you have an appreciation for what the potential for resuspension is, and I'm saying I don't think you do.

MR. CHEW: John, to -- to -- to move a path forward -- go forward on -- picking up in your

discussion here, we have to make some -- probably some big assumptions here that, number one, they weighed it. Okay?

DR. MAURO: Yeah, that's --

MR. CHEW: And then -- and then secondly, if they didn't weigh it and then we have to either go find those samples, probably no longer exist here, and the reweigh them -- right? And so I just asked Billy -- I said Billy, do you happen to know the knowledge of the very fact that -- did they weigh those samples or not?

MR. SMITH: No --

DR. MAURO: They don't weigh --

MR. SMITH: -- they were not weighed. The activity was based on the air volume that went through the air sampler so the activity was activity per cubic meter of air.

DR. MAURO: But the sam-- once you pulled the piece of paper --

DR. ANSPAUGH: I'd like to make a few comments on that, if I might. I think the resuspension factor of ten to the minus eight, ten to the minus nine, is okay for the -- for the conditions under which those air samplers were taken, which was nearby a cafeteria or a

dispensary. I -- I agree with John that if we're dealing with a bulldozer operator or a construction guy or somebody dragging a drill rig across the desert, that value is not appropriate and a mass loading approach would be much better.

I -- I also think Billy's absolutely right that those filters were not weighed, and in order to get representative values we probably would have to go (unintelligible) the material that was done for the -- the Yucca Repository where they did make a lot of measurements of mass loading and so forth in order to build the predictive models. So there are results available very close by the Test Site that were taken at later times on mass loading.

DR. MAURO: (Off microphone) I think (unintelligible).

MR. CHEW: I'm just trying to -- John, we need to probably discuss what -- what the appropriate path forward here to resolve this issue here because, you know, we -- let's say example we have -- we -- we cannot find those samples again to weigh them. That would be -- that would be another thing that we could -- is

that -- what -- and then I'm just listening to Lynn about finding some representative -- then that's got to be something that we need to agree upon, that's got to be representative of what we're (unintelligible) --

DR. MAURO: Unfortunately, what I'm hearing is that --

MR. CHEW: -- that's not easy.

DR. MAURO: -- even if we were able to get this mass loading --

MR. CHEW: Right.

DR. MAURO: -- associated within -- it may not serve us well because --

MR. CHEW: Exactly right.

DR. MAURO: And so maybe it is my id-- my idea of how to come at this thing may not work if in fact the samples -- the air samples were taken at locations where -- that were quiescent, when in fact we're interested in the areas that weren't quiescent, areas where there is physical activity going on.

MR. CHEW: Well, I think we need to go back to the conservatism that Mark has been talking about, taking the highest samples, assuming that the people were there continuously here,

and there are several factors -- orders of magnitude built into that, too, as you well know.

MR. ROLFES: We haven't considered the other 99 percent -- or greater than 99 percent of the data which indicated lower air concentrations.

DR. MAURO: No, but you're only a factor of ten -- in other words, you see, I would have been okay with that. In other words, what I -- we -- in fact, as soon as you said that, I went right to the --

MR. ROLFES: Sure.

DR. MAURO: -- and I said --

MR. ROLFES: Sure, but that would still --

DR. MAURO: -- that would, yeah.

MR. ROLFES: The other -- the other conservatisms that are built into that are the assumption that that individual was exposed for his entire year of employment in that area --

DR. MAURO: That's true. That's true. I agree with that.

MR. ROLLINS: Let me --

DR. ANIGSTEIN: But wait --

MR. ROLLINS: -- let me make one more comment.

DR. ANIGSTEIN: -- there's a --

MR. ROLLINS: How many square miles is NTS?
MR. SMITH: 1,350 square miles.
MR. ROLLINS: 1,300 square miles. Now these source terms are spread rather -- rather well, from what I can tell, based on these air sample results. It's spread pretty much over the 1,300 square miles. So at any point in time most of it's going to be quiescent and the resuspension's going to be occurring over quiescent areas. So the -- the site -- that's the average, but we've chosen the highest.
DR. ANIGSTEIN: I have -- I have a comment on that.
DR. ANSPAUGH: Well, you -- you've chosen air sampler that may have been located by a dispensary or someplace that does not represent the situation that would be claimant favorable.
MR. ROLLINS: Let me make a comment on that, and Dr. Anspaugh mentioned in 1964 what the sampling locations were, but as I understand it, the air sampling program was in its infancy in 1964 and they were just coming to the conclusion that they maybe needed to start measuring what the actual ambient concentrations were out there in areas that

were not affected by testing. And this is a quote that came out of the 1971 annual report, and it says (reading) In 1964 REECo established an environmental surveillance program at NTS that was designed to measure radiological conditions throughout the site, without regard to nuclear testing. That is, the collected data was not -- was not to relate to specific tests, but general conditions of radiation.

The short-term objective of the program was to minimize casual personnel exposure to radiation by locating and identifying localized radiological environmental conditions by type and quantity of contamination.

In other words, they were concerned that people might be being exposed to -- to areas that they didn't -- that they were working in that were not known to be contaminated. So it seems to me that they were trying to design a program to prevent this type of casual exposure. And I don't think putting air samples inside a dispensary would -- would accomplish that objective.

UNIDENTIFIED: I don't think we're --

DR. ANSPAUGH: Well, the -- I -- they were

located -- the location is given as dispensary or cafeteria for more than half of the samples.

MR. ROLFES: That's exactly what ambient exposures -- that's exactly where you would want to sample for ambient exposures. These -- these are not occupational internal exposures per se. For individuals that were working directly with radioactive material and were exposed to airborne radioactive material, those people were typically participants in a bioassay program.

DR. ANSPAUGH: We have -- we have serious questions about that, too.

DR. MAURO: We'll get to that next.

MR. ROLFES: Okay. The air samples that were set up that we are using, these would be reflective of essentially background concentrations that an individual that was not working in a radiologically-controlled area would have been exposed to.

DR. ANIGSTEIN: But it's -- but that still neglects --

DR. ANSPAUGH: Well, I -- I -- I certainly agree with that statement, but what is the definition of a radiologically-controlled area?

It certainly doesn't include everywhere that these people were out in the field driving bulldozers.

DR. MAURO: What I'm hearing is maybe Bob -- I mean Bob pointed this out to me over the weekend, this alpha -- gross alpha. See, what we're struggling with right now is we have these air samples and what do they really mean and can they serve our purposes, and lots of questions have come up. One angle of trying to come to grips with it would be if we can track down the dust loading. I'm hearing that can't be done. And even if we can do it, it may not mean very much if those air samples were taken in places where people were not working.

Now Bob, you had mentioned that you ac-- saw, which I wasn't aware of, gross alpha measurements were collected in 1963 --

DR. ANIGSTEIN: Right.

DR. MAURO: -- which is the time period we're interested in.

DR. ANIGSTEIN: Yeah, that's what it says in the report.

DR. MAURO: And -- right, and any sense of wh-- why they were taken and where they were taken?

DR. ANIGSTEIN: Yeah, they were taken -- I don't know where, I'm just getting it out of Chapter 4, but they were taken for the same purpose. That was the initial environmental monitoring and then they went -- got more refined and started doing radiochemical analysis of plutonium. So it would certainly help, but it would still have the same limitation. It's only as -- in other words, these are very good results for -- they were very good measurements of what they were measuring, and they were measuring the air concentration in that particular location. And all of these -- you know, we had this same -- the same problem looking at things like Bethlehem Steel. Breathing zone samples are the only thing that means anything (unintelligible) that's where the person actually is. The person stir-- the presence of the person, regardless of what he's doing, stirs up dust, particularly in a desert environment where the soil is very loose. Walking, driving a bulldozer, driving a truck stirs up dust. You know when the -- you know, you can look off in the distance and before you

see the truck -- before you realize there's a truck coming, you see the cloud of dust -- oh, there must be a truck coming.

DR. MAURO: You see, I think that originally you -- we were very much in agreement when you had the five milligram per cubic meter strategy. But we also agreed right around the table that that was off the charts high to assume someone has got five milligrams per cubic meter eight hours a day, you know, 2,000 hours a year.

DR. ANIGSTEIN: Maybe not, if he's really working. If he's really -- if he's working earth-moving machinery --

DR. MAURO: But that -- we (unintelligible) they also put it at the worst place. There was one -- all these different areas. You had one area that was by far the worst area so you assume that area with that activity (unintelligible) --

DR. ANIGSTEIN: In that particular year. It changes year by year.

Also I had a question about that. You made the statement that you picked the worst of the worst, the highest of the highest. That's not

according to what this -- looking at Attachment A; what it says here in the footnote to Table A-1 is for the site maximum -- values represent the maximum of the average area concentrations for '71 through '78 and the maximum of the maximum for '89 through 2001. So that's only -- that statement was only half correct.

MR. ROLFES: Well, that's correct. I believe what we've done with those, we've taken -- these are compilations of air samples that were collected -- was it -- Billy, was this monthly air samples that were compiled?

MR. SMITH: Yes.

MR. ROLFES: And we've taken the average of those monthly results, I believe.

MR. ROLLINS: Monthly or weekly? (Unintelligible) weekly.

MR. SMITH: Monthly.

DR. ANIGSTEIN: In one place it said weekly.

(Whereupon, Mr. Rollins, Mr. Smith and others conversed among themselves.)

MR. SMITH: I'm not sure relative to the environmental surveillance program. Some of the air samples ran for a month, some ran for shorter periods of time. For instance, if you

look at the volume of the air that we pulled you can estimate the period of time that they ran, based on the flow rates, but I think it was monthly -- as I recall.

MR. ROLFES: Okay, what you're referring to is footnote B on page 51 of the --

DR. ANIGSTEIN: Correct.

MR. ROLFES: -- Chapter 4.

DR. ANIGSTEIN: Correct.

MR. ROLFES: And it says values represent the arithmetic average of the area average concentrations for years 1971 through 1988, and the arithmetic average of the area maximum concentrations for the years of 1989 through --

DR. ANIGSTEIN: No, I'm --

MR. ROLFES: -- 2001.

DR. ANIGSTEIN: -- referring to -- I was referring to footnote C.

MR. ROLFES: Okay, I'll get to that in just a second. But anyway, that was the footnote pertaining to the site average.

DR. ANIGSTEIN: Yes.

MR. ROLFES: Footnote C pertains to the site maximum, and footnote C reads (reading) Values represent the maximum of the average area

concentrations for years '71 through '88 and the maximum of the maximum area concentrations for the years of 1989 through 2001.

Once again, we've ignored thousands of previous results which indicated lower exposures.

DR. ANIGSTEIN: But the one that was used for the early years is really the -- the 1972 site maximum, which happens to be Area 9, so what -- so what you took was the average for Area 9 to characterize 1972. If you look under 1972 column for -- column under 9, so that's the average.

MR. ROLFES: Okay.

DR. ANIGSTEIN: And then you took -- so basically the assumption was that it's the average concentration in the worst area for that year.

MR. ROLFES: Okay. Sure, okay.

MR. ROLLINS: Let me ask a question. As I pointed out a few moments ago, if we increased these intakes by a factor of a hundred, now we -- now we're into the range where the cobalt would be easily detected in whole body counting. So if we believe that, then we must understand that we -- we're not off by more

than a factor of 100.

DR. MAURO: When were the whole body counts taken and how many people were (unintelligible)?

DR. MAKHIJANI: They were not -- there were al-- there are almost no whole body counts before 1967.

MR. ROLLINS: Fine, but there were plenty afterwards.

UNIDENTIFIED: Sure.

MR. ROLLINS: But when -- if this phenomenon was going on, it would continue.

DR. MAKHIJANI: No, no, no. We're talking about exposures in 1963 and whether -- if the exposures were at the level that John was talking about, the cobalt would have been detected. We're not talking about the cobalt exposures in 1972 from the measurement you have in the site profile.

MR. ROLLINS: I think I can demonstrate to you that that would still be detectable.

DR. MAKHIJANI: It would be detectable, but it wouldn't have been detected because there were no whole body counters so you have no way of actually calibrating against -- with an actual

measurement whether the plutonium result that you're talking about is correct or John -- John is talking about is correct because the cobalt reference of hundred times being detectable by whole body counting is unverifiable. There's no measurement to calibrate this assertion.

MR. ROLLINS: (Off microphone) (Unintelligible) whole body counting start at NTS, Bill?

MR. ROLFES: It was roughly 1966 and it was operated by PanAmerican. We had spoken with a health physicist regarding --

DR. MAURO: So 13 years later --

MR. ROLLINS: '66?

MR. ROLFES: '66.

MR. ROLLINS: '66, right.

DR. MAURO: Oh, '66?

MR. ROLFES: '66.

DR. MAURO: Okay.

DR. MAKHIJANI: We've -- we've -- we've taken your Table 7-1 in the evaluation report in which there are 100 cases and compiled the data for 53 of the hundred, every -- every alternate one plus three test compilations, just to get the tables in order, and there are a couple of measurements before the mid-'70s -- and I don't

have the exact number in front of me. We're -- we're still compiling all this data and proofing it. But there are very, very few whole body counts before the mid-'70s and, as Billy said, you know, it started in '66 so it's moot before 1963 anyway.

DR. MAURO: I would -- I would agree that if there was widespread whole body counting looking for cobalt-60 in 1966, and you see nobody with a body burden that's substantially higher -- in other words -- in other words by a couple of orders of magnitude -- yeah, that means my -- my --

MR. ROLFES: Uh-huh.

DR. MAURO: -- my intuition --

MR. ROLFES: Uh-huh.

DR. MAURO: -- and experience that these resuspension factors could be at least a hundred, probably more of a thousand times higher, would be disproved.

MR. ROLFES: Uh-huh.

DR. MAURO: Now -- but -- so I'm not -- I'm going to -- I -- I mean my reaction to this, and this is, you know, a real time discussion --

MR. ROLFES: Uh-huh.

DR. MAURO: -- my reaction to this is that heck, you show up and you show me a large number, a large fraction of the workers that were out there running around out in the field, doing all the things that they do, and you have a significant fraction of those workers had a whole body count looking for cobalt-60, and you're not seeing any cobalt-60 when you would have seen it if it was at the levels that we're talking about -- that I'm talking about, you've just -- you just shot -- you just blew -- you know, just -- just shot down my argument. I mean and I'm -- I'm fine with that.

MR. CHEW: No, we didn't -- we're not shooting down your ar-- we're refining it.

DR. MAURO: No, no, no, I'm okay with that. I'm okay with that. I mean to me -- see, when I see something that just doesn't ring true, I say geez, it doesn't ring true and it's bothering me. But if you could show me why it's true because you come at it from that angle, I -- I walk away immediately. I say you're right, I'm wrong. But right now I don't have that.

Now I've got to tell you, we've been looking at the -- the inte-- the bioassay and whole body -- in other words, the internal dosimetry issues, gathering a lot of data -- yeah, we'll get to that, and it's pretty sparse, and I'm going to -- I -- I'll stick my neck out a little -- okay.

DR. MAKHIJANI: (Off microphone) (Unintelligible) get to it (unintelligible).

DR. MAURO: Okay, I won't leap yet, but I would agree with the argument you just made if that -- that record exists.

DR. ANIGSTEIN: Now what about -- does the cobalt necessarily stay with the plutonium in the soil?

DR. MAURO: My sense is yeah. In other words, they're going to be -- they're going to be --

DR. ANIGSTEIN: It doesn't migrate --

DR. MAURO: They're -- they're both --

DR. ANIGSTEIN: -- to different --

DR. MAURO: -- relatively refractory.

DR. ANSPAUGH: I think you have to be careful about generalizing about cobalt-60 because there were some shots that were deliberately loaded with cobalt-60 and there a large amount

around, whereas other shots had almost none.

MR. ROLFES: What's your source for that, Dr. Anspaugh?

DR. ANSPAUGH: What's my source for what?

MR. ROLFES: For -- for the loading of a device with cobalt-60.

DR. ANSPAUGH: Well, for ex-- for example, the Sedan event was loaded with, I don't know, maybe a hundred cobalt-60 sources of -- a curie or so, because they were going to do some diagnostics on the -- the bay surge* and the throw out* and all that stuff, and it just so happened that of approximately a hundred sources that were contained there, they never could find more than one or two of them.

MR. CHEW: You're right, Lynn, they did do that in Sedan.

MR. ROLFES: Thank you.

MR. CHEW: But -- but that doesn't keep us from going path forward. I'm just trying to figure out how we're going to resolve this thing here, John, because the arguments -- talking about conservativism, several factors of ten, and even using -- people spending the entire time there is probably another factor of ten, so --

DR. MAURO: Well, not exactly. Remember, we're talking about -- we're talking about this wide area, a big area -- we're not talking about controlled areas. People -- I don't know how many people are working out there, and you've got numbers that -- it could be -- for example, let's just look at Area number 9. You've got numbers that range -- there are several places where they're on the order of ten to the minus three, in Area 9 -- number 9, as a function of time. You've got a lot of areas in number 9 -- in Area number 9 that are on the order of ten to the minus four, and a couple that are on the order of ten to the minus five. So as a function of time, it's highly variable by -- I would say we're talking one to two orders of magnitude, just in that one area. Right off the bat, that alone belies Lynn's curve. Now it was Lynn's curve that predict-- you know, it's -- you know, you saw the li-- how it curves. It's a flat line. Well, obviously it's not. I mean it's all over the place. It's a couple of orders of magnitude -- that's just in one area. And then when you go between areas, I see more or less the -- a variability

that goes from ten to the minus four to ten to the minus five, and a couple of places ten to the minus three. So in a funny sort of way, in looking at this table, Table -- very important table, Table 7-2, what we have is your estimates of airborne activity, which shows that, whether you within group or cross group, the dust loadings spread from ten to the minus three to ten to the minus five. I don't care whether you're going within group as a function of time or across group. And you went ahead and picked a ten to the minus three number, something that certainly errs on the side -- and I would say in general that would do it for me, except that I know that a resuspension factor of ten to the minus nine is not -- it could be off by three, maybe four, orders of magnitude if in fact there are people working in an area disturbing the soil, even moderately -- even moderately. We -- I've seen resuspension factors on the order of ten to the minus two in areas that are heavily disturbed.

MR. ROLLINS: Episodic events.

DR. MAURO: They're very much episodic --

UNIDENTIFIED: Yes, they are.

DR. MAURO: -- that's correct, and I agree with that. But what I'm saying is that -- so it's no-- I would not have even brought this up if I -- we were not talking about many orders of magnitude concern, which could be put -- on -- and I'm looking for a way to put this to bed, and I thought I might have found it by the dust loading approach, but what I'm hearing is that's not going to do it.

UNIDENTIFIED: Well, we've driven down this --

DR. ANSPAUGH: I -- I'm not so sure about that, John, and you know, obviously there's no perfect solution to this problem because we don't have the data we'd really like to have. However, I -- I think the present calculational method is -- we can argue on several bases that it's not claimant favorable, and I believe that we would -- most of us would feel more comfortable with the mass loading approach in terms of it being claimant favorable, although that's not perfect, either. But I think it's much more claimant favorable than what we've got right now.

DR. MAURO: Yeah, I would say if there's a way to place a plausible upper bound on the chronic

dust loading over the course of a protracted period of time, in milligrams or micrograms per cubic meter -- originally when you picked the five milligrams per cubic meter, my sense was that's pretty high. I mean I -- I don't see that often. I see that as a transient situation, although Bob might argue -- others might argue that well, people are working in an area, that's what you get. I don't --

DR. ANIGSTEIN: There would have been measurements various places, like unloading trucks full of soil or gravel on the surface and other places that close to five is not uncommon.

DR. MAURO: While that activity is going on, yeah.

DR. ANIGSTEIN: Yeah, while -- yeah, while the activity's going on. Usually I haven't seen anything higher than five, but three to five is no-- you know, measured data is not uncommon.

DR. ANSPAUGH: Those -- those data have been reviewed extensively by the Yucca Mountain people and there are nice summaries of that data available, so we can use it.

DR. MAKHIJANI: We -- wha-- I'd just like to

just point out some order of magnitude things. If we're talking about the difference between ten to the minus nine and ten to the minus three or ten to the minus two, we've got six orders of magnitude. And it doesn't -- it doesn't help to say you're assuming somebody's present for 2,000 hours a year because that's three orders of magnitude -- still got another three orders of magnitude. That's one issue. The other issue that I'm a little concerned about is there's a difference between what Lynn was saying and what John was saying. Lynn was saying we can find a more claimant favorable approach, and that may be okay but -- in the TBD context. But also we're dealing simultaneously with a Special Exposure Cohort petition. And if there is no solution, then that is a solution. And I just -- I just want to say that this -- if -- if there -- I don't have a position on this 'cause I'm not reviewing it, you know. Joh-- John, you're the point person for this, so it's not my call. But if there isn't a scientifically valid way to put an upper bound on this based on the available measurements, and if a back

extrapolation from '72 backwards, for instance, is not -- not a sensible way to do it, I'd be interested in seeing what the monthly variations were if this -- if 1972 was an average for the -- for the whole year, it would be interesting to see if, on the same spot, the monthly variation was a factor of two, factor of five, or two orders of magnitude. That would make quite a lot of difference, and I presume that we have -- we have the raw data for that.

DR. MAURO: Well, we actually have the annual --

DR. ANIGSTEIN: Well, the annu-- the annual variation is tenfold --

(Whereupon, Drs. Mauro, Anigstein, Makhijani and others spoke simultaneously.)

DR. ANIGSTEIN: But the annual variation is tenfold and it doesn't -- and it's not steady.

UNIDENTIFIED: Sorry?

DR. ANIGSTEIN: The annual variation for a given area varies by a factor of ten over these years in the 1970s.

DR. MAKHIJANI: No, what I'm talking about --

DR. ANIGSTEIN: No, I know you're talking about

monthly.

DR. MAKHIJANI: -- on what you said is if, for any year in a particular spot, the number is an average for that spot and that year, it would be instructive to see what the monthly measurement variation was for that spot and that year because it -- it may show you under different weather conditions --

DR. MAURO: (Off microphone) (Unintelligible)

DR. MAKHIJANI: -- which might -- which might be buried in the annual average, what the resuspension in the absence of -- in the absence of equipment and worker disturbance was. That's -- that's all I'm saying. So there are -- so there are multiple --

DR. ANIGSTEIN: Why would monthly be better than annual?

DR. MAKHIJANI: No, al-- all I'm saying -- I'm not saying one's better than the other. All I'm saying is putting the monthly data on the table allows you to see how the resuspension varies within the year, even though the measurement in itself is a monthly average. If we're talking about episodic exposures, and we would be talking about episodic exposures, you

may be talking about somebody that is exposed for a few hours a month so a few tens of hours a year they may be dragging heavy equipment. I wouldn't go anywhere near a thousand hours, or 2,000 hours, but if you're talking many orders of magnitude, then -- then none of the other adjustments make any difference because you can't get there from here. And then the question is do we have a scientifically sensible way of going from an average measurement for a year for one spot, even if it's maximum, backwards. I would suggest at least that we look at the monthly variations for the spot that you've picked to see what those variations are. It won't solve the problem, but there is -- I just want to say but there is a solution, we can't solve the problem.

MR. PRESLEY: Y'all excuse me but we need to take about a five-minute break. We will be back in here at 20 minutes after. One thing (electronic interference) that I am going to remind you all of that we have to be out of this room by 12:00 o'clock and that gives us approximately 40 minutes -- would somebody

please mute their telephone?

DR. BRANCHE: All right, I'm going to cut the line and dial back in, so we're going to close off and come back in -- fortunately someone -- no, we're going to start all over again.

MR. PRESLEY: Everybody's got five minutes. We're going to start --

DR. BRANCHE: At 11:20?

MR. PRESLEY: -- at 11:20.

(Whereupon, a recess was taken from 11:15 a.m. to 11:20 a.m.)

DR. BRANCHE: We've dialed back in. Can someone who's participating by phone please let me know that you can hear me?

UNIDENTIFIED: We can hear you.

DR. BRANCHE: Thank you very much. I'm going to ask again that everyone participating by phone please mute your phones until you're ready to speak. If you do not have a mute button, then please dial star-6 and then you would use that same -- same star-6 to unmute your line. Again I stress how important it is that everyone participating by phone mute your phones so that everyone can hear. Believe me, even the slightest click of your mouse or your

keyboard interrupts the sound for the people participating by phone.

And again, please do not put us on hold. Thank you.

MR. PRESLEY: This is Bob Presley, chairman. At this time I'm going to call a halt to the discussions that we have had.

John, I have one question. I would like to know what it will take to satisfy SC&A on this issue, so think about that where that we can come up, we need -- this -- this is a question that we've beat to death. We need to come up with some type of a answer and move on.

Arjun, I understand that you have another problem that we need to discuss?

MR. ROLFES: I did want to -- before we leave that, I just wanted to point out that this is an occupational ambient source of exp--

MR. ROLLINS: (Off microphone) (Unintelligible)

MR. ROLFES: Okay, excuse me, an ambient source of exposures for individuals that worked at Nevada Test Site. This is typically not going to affect compensation decision for a claim. This level of dose is very small in comparison to that which we would assign to an individual

who worked in a radiologically controlled area and was directly handling radioactive materials. That would be considered occupational internal exposures. That would be the larger source of internal exposures that an individual would likely receive at the Nevada Test Site.

DR. MAURO: And Robert, if it's acceptable to you, I'd very much like to work with Mark and Gene and others to pursue this together as strategies for, you know, finding a way to lock this thing up. The cobalt-60 might be the answer. The answer may be, no matter what assumption we use, the doses are going to be less than a millirem a year, I don't know. So -- but I'd be happy to do that and we could try to do that quickly.

MR. PRESLEY: I would appreciate that, very quickly.

DR. MAKHIJANI: You might want to do a technical call, which is -- which is properly summarized --

DR. MAURO: Oh, absolutely.

DR. MAKHIJANI: -- (unintelligible) working group.

DR. MAURO: And -- and if anyone on the working group wants to sit in on any of these technical calls, I certainly will announce it or -- Gene -- or --

MR. PRESLEY: Would like to do that --

DR. MAURO: -- Mark would do that.

MR. PRESLEY: -- and I would like to have the -- the announcement for the call more than a few hours, please. If we set the call up, we need to give everybody a chance to kind of adjust their schedules. Arjun?

INTERNAL DOSE SITE PROFILE

DR. MAKHIJANI: Yeah, Mr. Presley, I don't have a problem, I was just following up on the direction that we got last time in May when we met, I believe it was a Board call, and NIOSH had said that they would publish new versions of their site profiles, and we got a direction to take a look at them. So whatever I'm saying is -- is not -- you know, not a carry-over from some previous working group meeting but essentially a new internal dose site profile was published and I was tasked with beginning to review that.

As you know, we've also been simultaneously

looking at the SEC evaluation report and the internal dosations associated with Table 7-1. We have a pretty careful review of the data in Table 7-1 of the evaluation report because there it just said that these are the workers for whom we have sufficient workers. Looking at -- we had some issues -- we have not finished our evaluation or review of the new TBD -- it's a complete rewrite of a pretty difficult area of inquiry at NTS for all the periods -- but I can give you some preliminary -- preliminary comments. In looking at the internal dose data from -- and maybe John will pass that summary around. This is not even a complete summary. This is something we've put together. What I'm handing out is -- is fairly preliminary. We are looking at -- we've looked, as I mentioned, at 53 of the hundred cases. We've compiled all of the internal dose data available for those 53 workers. And we looked specially at plutonium and iodine data to examine adequacy and completeness issues for dose reconstruction. And this is a preliminary set of comments that I'm making. Obviously you can see there are a lot of blank -- blank

columns, we haven't finished our compilation, but just based on the 53 out of a hundred, the -- the data for plutonium for 1963-'67 are -- are quite sparse. Of the 53 workers, I think 50 or 51 workers actually worked in that period.

DR. MAURO: Say -- say, Arjun?

DR. MAKHIJANI: Out of -- out of 53 workers that -- for whom we compiled the data for -- from Table 7-1, 51 actually worked in the '63-'67 period, and out -- out of 51, only six had any plutonium bioassay data in that period, and so it's less than 12 percent -- less than one in eight workers had any plutonium bioassay data. The -- the total number of workers indicate -- last time we discussed what might a routine sampling be, and I believe Billy Smith said that that would mean at least an annual -- annual sampling for -- for plutonium, if I remember it correctly, those who were part of the routine sampling program. And in the '63 to '67 we did not find any worker who had an annual plutonium. There were -- there were other bioassay results; I'm just focusing on plutonium.

The iodine data were even more sparse. I think only -- don't know where my result went here -- only two workers had any iodine data in the '63 to '67 period. So we found that period to be prelim-- on a preliminary basis -- now this is not sorted by occupation. As -- as we noted by NIOSH, most of the results -- most of the results are for rad-safe health physics type of personnel, and these are said to be representative of the group with the highest exposure potential.

Did I get that right, Mark?

MR. ROLFES: Yes, correct. Yeah. Uh-huh.

DR. MAKHIJANI: And -- so we're trying to find how that statement can be validated and (unintelligible) little bit of our time because the results for other categories of workers are very sparse. And that's why you see a lot of effort being put into actually compiling the data for other categories of workers, so we can actually make some comparisons. That work is not complete. In fact, that work is more or less at the beginning. And I -- we've designed a program so we're able to make some reliable statements about that.

So that's -- that's sort of one set of issues. The other -- the other issue that there was a reference in the site profile, the new site profile document, version one, to a REECo document from 1993 that said that this was the protocol for sampling from 1970 onward. I can't -- you know, I haven't had time to review -- it's a -- it's a pretty complex document. I haven't read every word of it, but I tried to go through it and, from what I could tell, the REECo document really states this -- states the protocol as of the date of that document, '90 -- early '90s. So-- for some things you can discover that it -- the measurement protocols or equipment go back to the early '80s and it's stated in the document.

Now for the -- for the minimum detectable amounts, NIOSH actually has extensive documentation as to what they were, going quite far back. And I found that the TBD has quite extensive reference-- I haven't checked all of them, but I presume that those references would check out. But for who was monitored and what the monitoring protocol was, I -- I could not validate that it went back to 1970. So this --

this is obviously a concern in that -- in that the TBD appears to rely on an idea of a certain monitoring protocol that's extended backward to 1970 that at least I was not -- on a preliminary review -- able to validate. That's very important because in 1993 even only 300 out of 12,000 workers were in a routine bioassay program. And our initial review of the early periods indicates very sparse routine coverage of plutonium -- other radionuclides are more common -- and we have to look at what that might mean for dose reconstruction ability.

So that's our second significant issue that arose directly out of our review of -- of -- now I've already mentioned iodine. Let me -- Lynn -- Lynn had some comments that he made. Do you want to go through your comments, Lynn, or should I go through them?

(No response)

Is Lynn on?

DR. BRANCHE: Give him time to unmute.

DR. ANSPAUGH: Hello?

DR. BRANCHE: Yes, we can hear you.

DR. MAKHIJANI: Would you please go through

your comments 'cause I read them rather -- I got -- only got them last night and I read them rather rapidly, so I -- I'd prefer if -- if you went through your comments rather than me trying to represent --

DR. ANSPAUGH: (Unintelligible)

DR. MAKHIJANI: -- a quick reading.

DR. ANSPAUGH: Okay. Now this is related to iodine -- potential iodine exposure in Baneberry, and if I understand the TBD correctly, on page 52 the comment was made that Baneberry was the most significant venting and you used that to make your bounding calculation on the concentration for iodine dose estimates. Correct?

MR. ROLLINS: That's what it said.

DR. ANSPAUGH: Okay. And again, as I understand it, you used one measurement of an air concentration at Camp 12 which was taken on December 24th, 1970, and then you decay-corrected that back to December 18th, but the critical assumption was made that the concentration, except for radioactive decay, on December 24th was the same as it was on December 18th. And this is not a -- a

reasonable assumption because Baneberry was a very prompt, massive event, and it had stopped venting in 24 hours, according to the REECo report on the subject. So assuming that a concentration six days later represents what was there on December 18th is not a reasonable assumption.

And it goes on -- on page 38 there's a comment that this leads to a dose of less than one millirem to the thyroid, and that is supposed to be a bounding calculation. But the actual data from the Baneberry event where people -- 900 people were evacuated and they all had their thyroids screened and the actual calculated thyroid dose based on the screening was 3,730 millirem, not the one millirem that was assumed for the bounding calculation.

And then there were other situations where there were some very high values of thyroid doses that -- for example, in Uba the dose was 593,000 millirem and there was another situation on the Merlin event where there was a dose of about 30,000 millirem, and also the Wishbone event, which -- and maybe Bryce Rich is on the phone, but he -- he was very much

involved in that, which again was a dose of about 9,000 millirem. So if I have understood what your bounding calculation was intended to be, then I -- I don't think it's a bounding calculation at all but it's a very serious underestimate of some of the doses that were observed, even following the Baneberry event itself.

MR. ROLFES: Dr. Anspaugh, this is Mark Rolfes, and we certainly do acknowledge that there were other exposures that exceeded what we've put in our ambient environmental exposure Technical Basis Document.

MR. ROLLINS: (Off microphone) (Unintelligible) Chapter 5.

MR. ROLFES: Oh, this is Chapter 5, okay, thank you. The cases that you have mentioned where there were larger iodine exposures, we're certainly aware of that, and those individuals participated in the bioassay program. That's how we know that there were such large exposures, because those individuals did have thyroid scans and participated in a urinalysis program to screen for gamma emitters and fission products. Yes, there were, for

example, the Uba event where they had drilled back into -- into some contamination and there was a radioiodine release. The DOE response files that I've received for the people that were involved in that event did indicate that there were in fact large thyroid exposures. That information is typically contained within an individual's DOE response file which NIOSH receives for every claimant, and that would be the most important piece of information, rather than the information in the Technical Basis Document. The information that's contained within an individual's DOE dosimetry response would be the most important piece of information for us to reconstruct that individual's dose.

DR. ANSPAUGH: Well, you are using that as the bounding calculation and your bounding calculation was a factor of 4,000 off just for the Baneberry people themselves.

MR. ROLLINS: Comment -- comment on that. It was meant to be a bounding calculation for someone who was unaware that they had been exposed.

DR. ANSPAUGH: Right.

DR. MAKHIJANI: Okay. I mentioned earlier in our discussion that -- you know, we -- we took our cues in the more general investigation of internal dose from Table 7-1 of the evaluation report where the internal dose data was said to be sufficient to calculate internal dose and there was a relationship between external dose potential and internal dose potential -- there are a number of statements that are made over there. When we actually compiled more than half of the cases in the Table -- we're going to do all of them, but so far we've compiled, more or less randomly, you know, choosing every alternate one and then three more than that. As I said, for 1963-'67 we found only two workers who had any iodine monitoring at all. I think -- we did not find very extensive evidence that people were checked -- people's thyroids were checked or screened on exit from tunnel areas, independent of job classification. So how -- so leaving aside the people who, during the Uba incident and the Baneberry incident -- which I agree are documented and you can find these doses, so obviously --

MR. ROLFES: Correct.
DR. MAKHIJANI: -- we know what the doses were --
MR. ROLFES: Sure.
DR. MAKHIJANI: -- I have no argument with that.
MR. ROLFES: Of course.
DR. MAKHIJANI: I don't -- I don't have a problem with what you've just said. But for more general -- for -- for a more general case, we are -- at the present time, as I said, we haven't finished our data compilation. I'm only giving you a preliminary look, just to report where we are in what turned out to be a more complex investigation than imagined, that in terms of plutonium and iodine, specially for the earlier period just after the SEC has already been declared, up to the end of '62, we're having a hard time finding a significant amount of data for either iodine or plutonium monitoring. And -- and that's just -- it's just a -- in term-- in the spirit of what Mr. Presley asked me to do, I'm just putting the issue on the table before having concluded. The other -- the other issue that I mentioned

in regard to whole body counting, which we already discussed, there -- the whole body counting really seems to have gotten underway in the mid-'70s in terms of more frequent counting. Out -- off these 53 workers that we've looked at, there were only two workers who had any whole body counting information, only two counts in -- in the earlier period before the mid-'70s, and so it's not of much help in terms of determining who should have been monitored further or -- as an indication of where you might go with bioassay samples. That gives you kind -- kind of an idea of -- of -- of the state of our investigation.

DR. MAURO: Arjun, I -- I'd like to go over this table that was circulated.

DR. MAKHIJANI: Sure.

DR. MAURO: Everyone sh--

DR. ROESSLER: First could I ask a question?

DR. MAURO: Sure.

DR. ROESSLER: On that table, where do I find Table 7-1? I was -- I'm on the internet on the CDC/NIOSH -- no, where in there do I find that table so I can look at the table from which you derived this data?

DR. MAKHIJANI: Dr. Roessler, it's in the evaluation report for the second SEC petition, which is dated September 25, 2007 -- and I'll give you a page number, if I remember it's page --

MS. MUNN: Page 34 -- 33, 34.

DR. ROESSLER: Okay, that'll help.

DR. MAURO: The reason they --

DR. ROESSLER: Okay.

DR. MAURO: Yeah, and I think I -- that's -- that's --

DR. MAKHIJANI: It starts on page 36, the -- the table.

DR. MAURO: That table is very funda-- our understanding is that's fundamental to being able to do dose -- internal dose reconstructions post-1962. And our mandate was let's take a look at the data, let's see what kind of bioassay data are out there. And it's -- it's really not an interpretation of data, let's just get the facts correct, and since there's -- so -- so that's what we're doing. Now the table I handed out captures perhaps hundreds of pages of database in one page, and let me explain what you're looking at so you

can understand what it is we're doing. What you're looking at is -- there's a column that says cases from Table 7-1. What we did here is say okay, we went in -- and Table 7-1 effectively has 100 workers who had the highest external exposure, and those 100 workers are the workers that are -- the data represent the workers whose bioassay data are being used as a core* model for all workers between '63 and I guess '67 and beyond. All right?

So our first question is okay, let's take a look at that data and what is -- what -- and now how robust, how rich is it, what does it cover, and we -- and for each worker we have pages upon pages of his records in our database and I -- the author of the-- this work, two of the folks who work for SC&A, have -- I asked them to -- could you please summarize this vast amount of information on one page, which was quite a -- an achievement. The first column you're looking at basically says listen, row number one, there are 100 claimants in Table 7-1. Row number two said to date SC&A has captured, downloaded and put into a relational database all the bioassay data for 53 of those

100 randomly --

DR. MAKHIJANI: Near--

DR. MAURO: -- selected --

DR. MAKHIJANI: -- nearly all.

DR. MAURO: Near--

DR. MAKHIJANI: (Off microphone) A couple of cases that were (unintelligible) are not totally (unintelligible).

DR. MAURO: Okay, so --

MR. CHEW: Yeah, I get...

DR. MAURO: -- that's where we -- so you get an idea on where we are on that.

MR. CHEW: Uh-huh.

DR. MAURO: Then we go on -- then we say okay, the number of bio-- you can see column after column -- basically this is sort of a way to summarize the data that we're capturing and putting in place, without any interpretation, just a way to reveal to the working group and the Board what's out there. So -- and you march down and you'll see, out of the ca-- twen-- 53, we looked at the number that have whole body counts, the number that have whole body counts between '63 and '67 -- an important time period, as we know -- the number of

plutonium analyses. Well, out of the 53 we see there was six. We -- then underneath that, we have the highest result in microcuries per cc. The highest number we saw, 6.13 minus seven microcuries per cc -- and so on down the row. So what we've done here in a -- very much a summary form is try to capture the essence of what kind of information we have in that Table 7-1.

Now one of the criticisms -- or not criticisms, one of the concerns SC&A has, and I think the working group had, was how do we know that those 100 workers who were selected based on the highest external exposure do in fact capture the workers that had the highest internal exposure.

MR. CHEW: Uh-huh.

DR. MAURO: So we came up with a strategy -- in fact, I think it was Dr. Lockey who came up with the strategy; he recommended it and then we followed up; I believe it was part of the discussion in one of the work meetings -- why don't you go and sample according to different categories of workers, because Table 7-1 doesn't really make an effort to look at

miners, radiation safety personnel, laborers. It goes in and just grabs the workers who have the highest external exposure -- okay? And it's possible that there is a limited relationship -- maybe it is a very weak relationship, we don't know -- between external exposure and internal exposure, we don't know. And so on -- on -- so it's important to find that out because if it turns out that that presumption -- high external also means high internal --

MR. CHEW: Uh-huh.

DR. MAURO: -- if that presumption turns out to be not entirely correct, we've got a problem.

MR. CHEW: Uh-huh.

DR. MAURO: That means the population of workers would not necessarily represent your bounding set and therefore it's going to be difficult to use them as your coworker model. So in order to test that, we went in -- now there are 1,500 claimants in the database for '60 -- post-'63 -- I think '63 to '67, or post-'6-- I'm not sure exactly the time period. And we said okay -- and it turns out you can go into the database and sort and say download --

very quickly, download for me all the miners, all the workers that claim they are miners and we -- and you say -- and we got a whole bunch of those. Then we went in and randomly selected 20 miners, and that's what the second column is.

These are -- and now we're into SC&A's work. SC&A went in and said well, let's go grab 20 miners and create the same -- and -- same record --

MR. CHEW: Uh-huh.

DR. MAURO: -- database, and summarize it in this column. And one of the things it tells you right off the bat -- I mean one of the interesting things -- it gives you an idea of how many miners were -- what percentage of the miners that we sampled were bioassayed, what percent were bioassayed for plutonium, and also what the result is. And you can see -- and -- well, first interesting observation -- all preliminary, by the way; all preliminary, and this is just -- let the data speak to you. In other words, we're not saying it, the data's saying it. All right? It says okay, we're looking at the highest miner that we saw, at

least in that -- in the sample of four cases that we grabbed out of the 20 'cause only four out of the 20 miners had bioassay data -- was orders -- the concentration was lower.

DR. MAKHIJANI: Be -- be -- be careful. The one -- the one doesn't have a volume unit attached to it and -- and this -- and the other does, and --

DR. MAURO: Oh, no, I'm looking at miners, not radiation safety.

DR. MAKHIJANI: Yeah, but you're comparing the miners to the radiation safety --

DR. MAURO: No, no, I'm not, I'm comparing the miners -- see, to me, I think it's important --

DR. MAKHIJANI: -- or you're comparing the miners --

DR. MAURO: -- miners -- see, to me, the way I look at it is --

DR. MAKHIJANI: -- between the other --

DR. MAURO: -- there's a lot of things that this data could sh-- to tell us, that's why I think it's important and I think we have to finish it. One is -- one question is well, listen, if we picked those 100 -- if you, NIOSH, picked those 100 in Table 7-1, what

confidence do we have that, for plutonium, we've got the -- we've got the big hitters? Well, take -- you look -- right off the bat we say well, let's take a look at the -- let's do another sample of miners. Well, so far our miner sample -- by the way, none of this has been QC'd; this is right hot off the press over the weekend, produced over the weekend. Well, to the extent that -- you know, it's -- we've got -- we've captured the data reliably and faithfully. It says that well, at least in the sample that we looked at in miners, it sure looks like the miners' plutonium concentration was well below. The highest -- the highest miner plutonium concentration was well below the highest concentration of the workers in Table 7-1.

But when you -- now we -- we go over to the next column called radiation safety, we did the same thing. We went into the 1,500. We sampled 20 out of the 1,500 and we sampled 20 workers who were radiation sa-- designated as either radiation safety, radiation monitor or health physicist, and we compiled all their data and summarized it here.

And here -- again, just for the sake of discussion -- it looks like that the highest plutonium concentration -- that's in microcuries, now -- not -- it's not microcuries per cc. I'm not quite sure what that means right now.

DR. ANIGSTEIN: Is that just a misprint, do you think?

MR. CHEW: I don't think the cc -- you know, is in either the miner or the case, it's just microcuries.

DR. MAURO: Okay, here -- so -- but -- but I think the impor--

DR. ANIGSTEIN: So -- so what -- what would -- excuse me, what would that be, just the entire sample?

DR. MAURO: I don't know. In fact, I won't even speculate right now. There's no need to do that. You see, what we --

DR. MAKHIJANI: (Off microphone) (Unintelligible)

DR. MAURO: We will eventually. We will eventually.

DR. MAKHIJANI: (Off microphone) (Unintelligible) to hand this out because --

DR. MAURO: No, no -- no, Arjun, I think it's important because we want the working group to understand what we're doing.

MR. CHEW: I know where you're going.

DR. MAURO: You know what we're doing. See, I -- we're doing -- now, we -- we're -- we're in-- we're going to be finishing up this table, laborer, so in the end -- everyone -- now -- and after we QC and check it and everything, we're all going to sit around a table, we're going to look at this data and tell -- ask ourselves what does this tell us, because in the end this is it. This -- this table's going to say, one, do we really have a robust set of data for internal dosimetry to reconstruct not only the workers who have data, but to build a coworker model. Second -- out of the Table 7-1. Second, does the workers from Table 7-1, do they appear to be the bounding ones. Right now I'd say, you know, we really can't tell yet but, you know, at least, you know, if you -- if you start to compare the tritium -- for example, you know, there might -- they may be okay, that's what I'm getting at. Table 7-1, when you start to compare the other categories

-- what -- remember what we asked ourselves, do we feel confident that the workers in Table 7-1 do in fact capture the high end workers. And by -- by looking at these other categories, sorting the data from a different direction, by worker category, it'll start to give us what I call the weight of evidence. You start to get comfortable. And every-- in other words, if every one of the mi-- all the miners, radiation safety, laborers -- if all their plutonium concentrations for everyone that we were able to capture are lower than the highest one for the one that's from the Table 7-1, you know, you start to get a warm and fuzzy feeling, not bad.

Now -- now that doesn't mean you've got yourself a really good database, but it means that when you picked that Table 7-1 workers, it's looking pretty good.

If we see there's a -- one of the -- let's say one of the -- the welders, we didn't get to the welders yet, but we find out the highest welder is two orders of magnitude higher in some category than the highest 7-1, we've got a problem. We've got to talk about it.

So that's what we're doing what we're doing. And I think in the end we -- it'll be in front of everybody to look at. And of course behind this is hundreds of pages and -- and then we can do any sorts you can imagine on it. We -- we just sorted this way for the purpose of this meeting so that everyone can have a good idea of what it is we're doing and why we're doing it, and that's all I wanted to communicate right now.

MR. CHEW: (Unintelligible)

DR. MAKHIJANI: One -- one other comment, and I presented some of the data earlier, I think the data need to be divided into periods because, at least from the first 53 that we've compiled from Table 7-1, it seemed there's a dif-- significant difference in the period as to how much plutonium monitoring went on. I'm not so sure whether the different -- about iodine monitoring, but in plutonium monitoring it does appear to be a difference. And so we will probably have to parse this (unintelligible) --

MR. CHEW: John, I -- I'd like to -- I'd like to speak to what you were discus-- I'd appreciate it. I just want to correct the

first thing technically, then I'll talk about the whole program and set you -- how we got this started here, and -- and by the way, it cannot be three times six times ten to the minus 11 microcuries 'cause that's two orders of magnitude below the limit of sensitivity 'cause you -- look -- think about it. At -- at five times ten to the minus seven, that's about a tenth of a picocurie. Okay? And that's about a -- less than a dpm. You can't count that low.

DR. MAURO: Per cc.

MR. CHEW: Yeah.

DR. MAURO: Per cc, but the samples are (unintelligible) liter.

MR. CHEW: Well -- yeah, but for -- for a full liter, exactly right (unintelligible) --

(Whereupon, Dr. Mauro and Mr. Chew spoke simultaneously.)

DR. MAURO: So you've got to multiply by 1,000.

MR. CHEW: By 1,000, right, right, and so we've got to, you know, compare equals.

Well, let's start to think about the -- where the program started from. When -- when we first -- looking I says where can we find -- in

the lack of going back to individual records and polling to do a full coworker study, we actually went to -- says let's go look at NOCTS and see what's there. All right? And so we said there's probably a fairly good assumption, and I'm sure we can argue about this, that, you know, Nevada Test Site different than plutonium facilities like Rocky Flats -- Nevada Test Site, people were exposured -- exposure to probably the highest gamma exposures probably equates to potentially internal exposure, 'cause that's the kind of activity that went on at the Test Site. Obviously Lynn will say there's a couple of safety things, shots, that may be an exception there, but we started with that particular premise. All right?

Now we look at the program at that particular time, there was clearly -- you can see -- Billy can assert to this -- that the -- the people who were monitored for bioassay -- they was trying to get a -- represent sev-- representation of who was po-- potentially the highest exposure because there was -- as you said, there was many people at the Test Site. And at that time, I think -- Billy, please

correct me if I'm wrong -- but the radiation safety, the health physicists who were there pretty much for all the shots were -- were probably a good representation because you well know the majority of the Test Site did things to pre-- prepare for the shots and not necessarily were participating in the events. Okay? And so the health physicists (unintelligible) representation.

We also did look at the first 100, as you said in the -- and that's where you first -- your starting point, and then that's probably why -- and these are only in NOCTS. Okay? These are only in the people who are claimants. But they are also the top highest exposed people, too. Okay? And --

DR. MAURO: Yes.

MR. CHEW: -- and -- and out of that particular --

DR. MAURO: Highest external expos-- right.

MR. CHEW: External exposure, correct. And so -- so that -- the reason probably why you would not see as many of the other categories in those highest exposure that did bioassay, because that's not how the program was set up

to monitor who for bioassay. Now Billy, maybe you want to speak to that, huh? (Unintelligible) saying this correctly here?

MR. SMITH: Yes.

MR. CHEW: Okay. And so --

DR. MAURO: But that would argue for your approach --

MR. CHEW: Right.

DR. MAURO: -- you see -- you know, when we fini-- see, when we finish fleshing this table out and we -- and we may very well find that the num-- the laborers, the wiremen -- you look at the actual bioassay numbers for that, and we could look at the highest value (unintelligible) distribution --

MR. CHEW: And there'd be only a very few, that's what I'm saying --

DR. MAURO: Only a few --

MR. CHEW: -- exactly right.

DR. MAURO: -- and the highest ones, if they continue to consistently come in lower than let's say your Table 7-1, I would say that -- that starts to give weight to your approach.

MR. CHEW: Now for the people who were in the other categories who were bioassayed, they were

probably due to or most likely is due to an episodic event. Okay? We know that they got -- potentially was involved with some exposures, that's why they did that. Right? But in order to do what you (unintelligible) say to -- to represented as a coworker -- right? -- then the health physicists and the radiation people are probably truly representative 'cause they were the highest exposure and they were the ones who were monitored, and that's basically how the program was set up. I think we -- we need to go back to think about how and why the program was set up that way, because of the limited bioassay that was -- that was done.

DR. MAURO: Well, would -- would this table show us that, demonstrate it? I mean in effect what I'm hearing is the premise that you're working on, which may be well-founded, should reveal itself in this table. In other words, we will find that the highest exposures, the most thoroughly monitored -- bioassay monitored -- would be the radiation safety people and they -- and the numbers we get for them would be comparable to the ones in Table 7-1 in terms of the bioassay, and we should also be able to

use this very same information to draw correlations between external exposure and internal. And for example, I could see a plot of external exposure versus plutonium levels in bio-- in -- in urine. Other words, it -- so it -- what I'm getting at is ultimately -- lots of statements made, presumptions made, perhaps on very good grounds, that are in the evaluation report and site profile, this table will basically either tend to support those conclusions and say yes, it looks like all those generalizations or judgments that were made were well-founded, or there's going to be sufficient disparity revealed by tables like this that will say hmm, maybe some of those assumptions don't exactly ring true, and it should come out from here.

Now if -- if you don't believe this database generation -- by the way, I'd like to point out, just so you know, 'cause there's a budget involved here. It takes about four hours per case. We're doing 120 cases, so what's that, 480, so we're investing 480 work hours to do this.

MR. CHEW: And we did the same thing, too, by

picking the top 100 and not just --

DR. MAURO: And I was very favorably impressed. I was surprised that they were able to do that in four hours. So in my mind, for relatively modest cost, we're going to get to the bottom of this thing. And when we're done I think we're going to be able to say something very insightful about the power of the Table 7-1 or its limitations and be able to present it to the Board and the Board's going to make its own judgments. The table will speak for itself.

DR. BRANCHE: To the Board or to the workgroup?

MR. PRESLEY: Yeah.

DR. BRANCHE: To the Board or to the workgroup?

MR. PRESLEY: To the workgroup.

DR. MAURO: I'm sorry, the workgroup. Of course I mean the workgr-- I -- the workgr-- other words, I'm trying to get to the place where the data speaks to the workgroup, and each member of the workgroup could look at it and we could all sit around and look at the data and discuss it and understand it, and you could lend your insight into why this number's here and this number's there, so -- so rather than us making judgments and speculating like

we just did before -- one of the problems with the conversation we just had is a lot of speculation -- worried about this, worried about the suspension factor, all the -- but this is not that. This is (unintelligible) --

MR. PRESLEY: John --

DR. MAURO: -- data, this should answer questions for us.

FUTURE ACTIONS

MR. PRESLEY: -- excuse me. It's 12:00 o'clock. We have to break. It's obvious to the chair we are not -- I repeat, not -- going to be able to come up with any kind of a decision that I had hoped to do and give to the Board this time. What I would like to ask -- and Christine, correct me if I'm wrong -- I would like to ask SC&A and NIOSH to discuss their concerns and findings and make sure that everything is taken care of. At this time I am not going to ask for a scheduled meeting. I want to give both sides time to think about what they're going to do. Let's get -- let them get together, iron out the situations, problems, issues, whatever you want to call them. But the next time that we get back to

work as a working group, I would more -- and I want to bring all the issues to the table and let's make a decision on this. We have people that are not being paid, they're not being compensated, they're dying. I want to get this issue taken care of so these people can get their due.

DR. BRANCHE: Point of order, Bob. There was a discussion about a technical call. Who from your workgroup do you want to participate in that call --

MR. PRESLEY: I want --

DR. BRANCHE: -- as they schedule it?

MR. PRESLEY: I want the whole workgroup notified about that --

DR. BRANCHE: Okay.

MR. PRESLEY: -- so that the workgro-- anybody on the workgroup can be on that technical call if they want to be on it. And agr-- and again, I ask you to please not call at 9:00 o'clock in the morning and expect somebody to be on an 11:00 o'clock technical call that day. We all have very, very busy schedules. So when you schedule these, give us two or three days to correct our schedules.

DR. BRANCHE: Who from -- who from NIOSH and who from SC&A will essentially handle the scheduling of this technical call? Mark, I presume.

MR. ROLFES: I -- I would be the NIOSH point of contact to coordinate with whoever from SC&A.

DR. BRANCHE: Looks like John.

DR. MAURO: Just call me and I'll make sure our folks are available.

MR. ROLFES: I would like S-- I think it would be appropriate for SC&A to do the scheduling. I think that that would be the easiest thing to do, so...

MR. PRESLEY: Yeah.

DR. MAKHIJANI: Well, this is a point of information. I think -- I think it might be useful -- we also have Joyce Lipsztein working on this because she is our --

DR. BRANCHE: You -- okay, you all can dis-- sounds like Mr. Presley -- giving you pres--

MR. PRESLEY: Yeah, this is (unintelligible) --

DR. MAKHIJANI: (Off microphone) I (unintelligible) the -- I (unintelligible) the schedule so it might be a few weeks before we can actually get to the point of having a

substantive (unintelligible) --

DR. BRANCHE: Well, I think Mr. Presley's simply asking that you give sufficient notice --

DR. MAKHIJANI: Sure.

DR. BRANCHE: -- and ample dates.

MR. PRESLEY: That's why I have not scheduled a meeting. I want everything to be completed. Now, does any Board member have a comment? Wanda?

MS. MUNN: Just -- just a question, and I was going to bring up the issue of what time frame we're actually discussing here. You've just said a few weeks. A few weeks, to me, can mean anything from two to nine, and I'd like very much to be able to put a tighter frame on that. If we're talking about 480 hours of work, I assume it's distributed among a variety of people, so what are we thinking in terms of completion of this table?

DR. MAURO: This table in particular, I would say we're a month away from completing the table. Okay? But, once the table is completed, it goes to Joyce 'cause Joyce is going to say well, what is -- what

(unintelligible) -- we have this data here, what can we do with it? Can we reconstruct doses? So -- so -- I would -- we -- but that doesn't mean we can't -- once the table's completed doesn't mean we can't talk. So between now and a month from now I'd like to be able to engage Mark with our folks in -- in working the table, but actual --

DR. BRANCHE: Okay --

DR. MAURO: -- (unintelligible) table to database (unintelligible) --

DR. BRANCHE: Okay --

DR. MAURO: -- is going to take a month. We're not done yet. We just -- we're -- in effect, now we've got the dataset in front of us. Now we have to --

MR. PRESLEY: Hey, John --

DR. MAURO: -- interpret that data.

MR. PRESLEY: -- the table and all that we're talking about is SEC stuff and not site --

DR. MAURO: Yes, we are.

MR. PRESLEY: -- profile. Okay?

DR. MAURO: Yes, we are.

MR. PRESLEY: So let's don't get these two mixed up. We are trying to get the site

profile completed and recommended to the Board.
DR. MAKHIJANI: Mr. Presley, I'd like some clarity on -- on that because I -- I -- I must admit I'm a -- I'm a little confused, because in -- in the TBD there's a dose reconstruction method put forward, and if there's a finding that the dose reconstruction method put forward does-- doesn't have sufficient information to be able to do a good dose reconstruction, it's -- automatically overlaps with the SEC issue because it can't be resolved within the framework of the TBD and -- which is why we're actually proceeding with the two documents in -- in parallel because -- or almost overlapping because that's the only way that we see these reviews can be efficiently done in terms of the claims that NIOSH has already put on the table about how internal dose calculations are to be pursued. So I'm quite confused about that.
MR. ROLFES: What confused me, Arjun, was that I felt we were going to have a discussion to address the environmental exposures. I thought that was --
DR. MAURO: We did.
MR. ROLFES: -- the whole purpose of our call,

to address the site profile issue. This is a separate issue. This is now the SEC issue, so --

DR. MAURO: I think we need to talk about both. Other words --

MR. ROLFES: Okay.

DR. BRANCHE: There are two -- there are two issues.

MR. PRESLEY: Yes, there are two issues.

DR. MAURO: I think we need to talk -- there's a third -- we didn't even talk about -- I hate to do this to you, but there's still the badges left behind.

DR. MAKHIJANI: Well, that's an SE--

DR. MAURO: That's --

MR. ROLFES: Once again, that's --

DR. MAURO: -- purely an SEC issue.

MR. ROLFES: -- an SEC issue.

DR. BRANCHE: But it --

DR. MAURO: I understand you don't -- we don't engage that issue in the -- within this particular framework that we're talking about now, just the two issues, Chapter 4, Chapter 5 -- basically, environmental dose and internal dose using -- basically using Table 7-1 -- even

those 7-1 is in the ER, it is certainly an internal dose reconstruction issue.

DR. BRANCHE: All right, gentlemen, we do have another point of business. We do need to --

MR. PRESLEY: Brad --

DR. MAURO: Mark, we'll talk.

MR. PRESLEY: -- Brad, do you have anything?

MR. CLAWSON: No, we --

MR. PRESLEY: Phil, do you agree with what we're doing?

MR. SCHOFIELD: Yeah, let's get --

MR. PRESLEY: Gen?

MR. SCHOFIELD: -- some discussion --

MR. PRESLEY: Do you have a problem? Is everything all right?

DR. ROESSLER: I don't understand what we're doing, but I think we need to --

MR. PRESLEY: Yeah.

DR. ROESSLER: -- pick a time where these people get together and -- and --

DR. BRANCHE: Is there someone who's going to outline what the technical call's going to be about and send it out to the workgroup?

MR. CHEW: Yeah, I was going to say, what's the bottom line?

DR. MAURO: I would be happy to put together a draft of my perspective on the path forward --

MR. PRESLEY: Do that.

DR. MAURO: -- and I'll work with Mark on that, making sure we both agree on what the path forward is and get it off to the workgroup.

MR. PRESLEY: Okay.

DR. BRANCHE: Okay, that -- that sounds --

MR. PRESLEY: Let's call an end to this meeting.

MR. CHEW: What are we trying to achieve?

DR. MAURO: That's all (unintelligible) framework of what we're trying --

DR. BRANCHE: We're going to close this call. The meeting's adjourned.

(Whereupon, the meeting was adjourned at 12:07 p.m.)

CERTIFICATE 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 June 23, 2008; 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 10th day of Aug., 2008.

_____

STEVEN RAY GREEN, CCR, CVR-CM, PNSC

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