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

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ADVISORY BOARD ON RADIATION AND
WORKER HEALTH

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NEVADA TEST SITE WORK GROUP

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THURSDAY
JANUARY 5, 2017

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The Subcommittee met at the Marriott Cincinnati Airport, Montreal Room, 2395 Progress Drive, Hebron, Kentucky, at 9:00 a.m. Eastern Time, Bradley P. Clawson, Chair, presiding.

PRESENT:

BRADLEY P. CLAWSON, Chair
WANDA I. MUNN, Member
GENEVIEVE S. ROESSLER, Member*
PHILLIP SCHOFIELD, Member*

*participating by telephone

ALSO PRESENT:

TED KATZ, Designated Federal Official
LYNN ANSPAUGH, SC&A
BOB BARTON, SC&A
ROSE GOGLIOTTI, SC&A*
JENNY LIN, HHS*
ARJUN MAKHIJANI, SC&A*
JOHN MAURO, SC&A*
JIM NETON, DCAS
MARK ROLFES, DCAS
GENE ROLLINS, ORAU Team*
MATTHEW SMITH, ORAU Team*
JOHN STIVER, SC&A*
DENNIS STRENGE, ORAU Team*

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

8:54 a.m.

Welcome

MR. KATZ: Welcome, everyone, in the room and on the line. This is the Advisory Board on Radiation Worker Health. It is the Nevada Test Site Work Group. We haven't met in quite a few years. We have got some work ahead of us.

The agenda for today's meeting and all the papers that we could clear, I haven't checked recently, but most of the material for today should be posted on the NIOSH website. So for folks on the line, from the public, for example, who don't have those papers directly, you can go on the NIOSH website for this program, schedule of meetings, today's date, and you can follow along with all the papers and perhaps the presentation. I'm not sure if that is posted yet. If it is not posted, it will get posted after this meeting. That doesn't help you as much but you can listen along and then see what that presentation looks like as soon as it does

get posted.

Roll call, let's do that.

(Roll call.)

MR. KATZ: Okay, then. I think that takes care of preliminaries. I would just remind all you folks on the phone to mute your phones, except for when you are speaking to the group -- star 6 to mute your phones, star 6 to come off of mute.

And Brad, it is your meeting.

Chair's Opening Remarks

CHAIR CLAWSON: I appreciate that. First of all, I would like to tell everybody thanks for getting with this. One of the things that I have to do is I have to go back. It has been since 2013 that we had a Work Group on this. So things might be a little bit old. So I just want to make sure that we take the time that we need to be able to discuss these issues.

With that, I am going to turn it over to Mr. Anspaugh and we will start into it. And I

believe we are going to be talking about the resuspension.

DR. MAKHIJANI: Brad, this is Arjun.

CHAIR CLAWSON: Yes, Arjun.

DR. MAKHIJANI: Before we start, I would like to make a correction in the update that went out from SC&A. It was a cut and paste error. On item 20 it says review continuing. It should actually have said issue resolved. I was just trying to cut and paste the heading for each one and I cut and pasted the whole thing. I'm sorry about that but item 20 should say issue resolved.

MEMBER MUNN: That's the kind of change we always like to see, Arjun.

MR. KATZ: Thanks, Arjun.

DR. MAKHIJANI: You're welcome.

CHAIR CLAWSON: Okay, I appreciate that, Arjun.

Lynn.

Response to NIOSH's Review of SC&A's Nevada Test Site Resuspension Issues Status Report

DR. ANSPAUGH: Okay. Well, thank you. It has been a long time since I have been here. I think 2008 was the last time. So, we all have a little bit of a problem with trying to remember what was going on and try and resolve some of these issues.

The first slide just shows my title slide. And the next one we start out with just to remind you what SC&A's task was related to resuspension. This was item number five on the long-standing matrix and that cascades down into several other issues that are shown too, I think item 6 and 7 plus some others.

So the basic task that SC&A was given related to the Nevada Test Site was basically about resuspension. And so our task was to review the calculation of the doses from the resuspension the radionuclides deposited on the ground. And the issues mainly relate to the resuspension of short-lived radionuclides previous to 1972 or 1971 when the start of measurements of airborne

plutonium were started.

So besides just the resuspension, this also involves the review of ORAUT 2012, which is the Technical Basis Document on Environmental Occupational Dose, Revision 3. That is a very complex document and difficult to follow, as we will get into.

And also, there was a review of how doses were actually being constructed by the dose reconstructors and Bob Barton will make some additional presentation on that later.

The next graph just indicates some fairly recent documents. So we started out with this Technical Basis Document that was dated 2012 now and I didn't know that there was a new update of this famous matrix that occurred in May 2015. I don't know if that has been released in a PA-cleared version or not but I hadn't seen it until last night when reading Bob's computer.

MR. ROLFES: This hasn't been cleared yet. It has a bunch of claim information that we

have reviewed and provided. So that is why it wasn't emailed because there were a number of individuals who didn't have government emails as well or government email access.

DR. ANSPAUGH: Including me.

Anyway, going on, there was from SC&A a review of the issues and comments matrix. And then we had two additional what are known as White Papers. The first was from Strenge and I am glad to hear that Dennis is on the line, which was NIOSH's response to short-lived radionuclide issues raised in comment 5 in one of the resuspension issues report.

Then we also have a report by Rollins related to the inconsistency issues and I am also glad to know that Gene is on the line.

And then we have the document that is in your hands, I hope, which I was the main author on it, the response to NIOSH's review of the Nevada Test Site resuspension issue status report, which that report goes back to 2015.

And then we have, finally, Bob Barton's memo about the comment 8, the White Paper given by Rollins.

So just to remind you that I would like to go back to the next side, which shows major events at the Nevada Test Site and maybe this is a good refresher for us all. Looking at the major events that occurred at the Nevada Test Site, the first one, of course, is January 1951 when atmospheric testing began. And then in January 1962, atmospheric testing stopped. And finally in September 1992, all testing stopped.

And there were two SEC petitions, both of which were eventually granted. The first one, 55, carried us from January '51 through January 19 -- well actually, the end of December 1962. And then the second petition, number 84 went from January 1963 through December 1992. And there was a lot of -- I think controversy is a reasonable comment, about whether or not it was possible, given the data on hand to reconstruct internal dose

based on the information that was available. And the ultimate resolution was that from the period of January 1951 through December of 1962, for both SEC classes, NIOSH decided that they were unable to calculate internal dose.

In an effort to calculate some dose for people who did not have presumptive cancers or perhaps did not work 250 days, there was an effort established to try and calculate occupational environmental dose. And the way it stands right now, there is what I call the NIOSH resuspension window which carries from January 1963 up to about 1972 or '71, when the measurements of airborne plutonium were actually started at the Nevada Test Site.

So in order to look at this resuspension window, it is necessary to try -- and I would use the word bootstrap the measurements of airborne plutonium into all the other radionuclides that were present. And the way that is being done is to make use of what we call the RIDP data, RIDP

standing for not a frog but for the Radionuclide Inventory and Distribution Program, which took an inventory of all radionuclides on the test site. Those measurements were made in the 1980s.

And so that plus the Hicks tables, as used by NIOSH, takes us back to January 1963, according to the present calculations. And it has always been a bone of contention of what happens to the people who were on-site in the latter part of 1962 after atmospheric testing stopped but before January 1st of 1963. So the way it stands right now, these people who are shown in this circle who represent claimants who were at the Nevada Test Site working from January 1962 through the end of 1962 and these people are not getting any occupational environmental dose, which means I think that these people are not being treated equally as others.

And just to remind you, the next slide indicates that there was a very busy time in the latter part of 1962 because 30 underground nuclear

tests were conducted during that six months and also the preparations were underway for 46 additional nuclear tests in 1963. And I think you all understand that digging the tunnels and then placing the devices and taking care of diagnostics requires that there be a lot of work being done at the site. So, even though atmospheric testing ended in July 1962, there was a lot of activity going on in the latter part of 1962.

So, the next slide indicates our Recommendation 1 to the Members of the NTS Work Group, which is change the time period of reconstruction of occupational environmental dose to January 17, 1962 through December 31, 1992. So, that would take care of those workers who are enclosed in that small circle who are not getting any dose calculated for them in terms of occupational environmental dose.

One of the reasons for this recommendation is that the same methodology can be used just as it is used to get back to January 1,

1963. That same methodology can be used to get back to July 17, 1962.

MR. KATZ: Lynn, can we -- let's stop there with that issue because it is not an issue that we can really -- there is any point in spending much time on.

DR. NETON: Yes, I think that this, although technically Lynn is correct that the methodology could be used, the decision was made in the evaluation of SEC 55 to purposely add that extra six months period after atmospheric testing stopped to allow for the stabilization of the source term after atmospheric testing stopped. So, we specifically didn't stop the SEC 55 in July of '62. We extended it six more months intentionally and added that to the entire Class so that no dose reconstructions, by definition, could be done under SEC 55. I mean it was added under those -- that thought process.

So it is an SEC. No dose reconstruction could be done, whether it is a

resuspension model or not. It is already decided. It is a fait accompli, essentially. We can't go back and change the definition of a Class at this point.

MR. KATZ: You would have to withdraw the Class, basically and that is just really not realistic.

DR. ANSPAUGH: You know, I understand what you are saying but I don't really think that is correct. If we could go back one to my slide, you know, the SEC Class runs continuously from 1951 through December of 1992. There is really no distinction between what can be done for people in the two SEC classes.

DR. NETON: Well if you look at the two Evaluation Reports, SEC 55 spoke nothing about a resuspension or environmental model. The SEC Evaluation Report for SEC 84 clearly had the environmental model in there moving forward from 1960.

I mean it is pretty clear in those two

separate reports, if you look at them, that is the way it was treated. I mean there is no environmental -- as a matter of fact, there was no environmental model in the Site Profile prior to its revision after the SEC was added.

So, it was not considered to be possible. And again, the language is very specific in Section 4.5 of the SEC Evaluation Report why we intentionally added that six month period after atmospheric testing stopped. So, it is a policy decision not a scientific decision.

DR. ANSPAUGH: Exactly.

MR. KATZ: It is not a Work Group activity, unless SC&A is recommending that the Board retract part of that Class, which honestly is not a realistic path forward.

DR. MAKHIJANI: This is Arjun. I have a question for Jim Neton. When you say no dose reconstructions, not even partial doses or anything?

DR. NETON: I'm sorry, Arjun. I meant

to say no internal dose reconstructions unless there is bioassay data, you know the normal --

DR. MAKHIJANI: Right. Right, I understand that and agree with it except that here we are talking about environmental dose.

MR. ROLFES: I would disagree with that because testing is going on and there is resuspension occurring, you know it is an outdoor environment, an outdoor workplace. So I would consider it to be an occupational exposure, not necessarily an environmental one, which would be more reflective of work in mercury, for example, outside of a trailer in an area that is not having active weapons testing going on.

DR. MAKHIJANI: Right. So I am not understanding the difference between environmental dose in this six month period and in the subsequent period when you have similar activities going on. You are distinguishing between occupational internal dose and occupational environmental dose.

DR. NETON: Well, I could read you the language in 4.5, where we added that six months after atmospheric testing stopped. In the Evaluation Report for SEC 55 it said the extension of the SEC period through December 31, 1962, approximately six months after the last atmospheric test allows for the stabilization of the source term and for decay of a shorter-lived radionuclide associated with the final atmospheric test. That was our conclusion at that time in 2006.

DR. MAKHIJANI: Right.

DR. NETON: So we added, we intentionally added that six month period to allow for stabilization of that rapidly changing source term and we said we cannot do, with sufficient accuracy essentially, any dose reconstructions for internal dose during that period unless we have bioassay data. That is the way we have been behaving since 2006 and that is the conditions under which the Class was granted.

DR. MAKHIJANI: Yes, I am not questioning it. I am just confused. I am just trying to clear up the confusion is that I am still not understanding the difference between the environmental dose and the six month period. Not occupational dose. I understand -- not occupational, internal dose. But the environmental dose between that six month period and the subsequent environmental doses that we are agreeing we should try to estimate.

DR. NETON: In SEC 84 there is an Environmental Dose Report because an environmental dose is described how that is done. That is not considered in SEC 55 at all.

DR. MAKHIJANI: Okay.

DR. MAURO: This is John Mauro. I do have one additional observation that I think I would like to bring to the table. There is nothing about I guess the work we have done related to that six month period that would imply withdrawing or making modifications to the definition of the SEC.

That is you cannot reconstruct doses. So it still would be covered under the SEC. The only real question is for those workers that are not covered by the SEC, is it possible to assign some dose, as you do starting in January '63, is it possible or plausible to assign some dose to that six month period from July '62 to January '63.

I just want to make it clear that there is nothing about the comments we are making that have any impact on the --

DR. NETON: John, there is a policy decision that we couldn't do doses at that time. You can concoct any scientific model and go back and demonstrate that something could be done but the decision has already been made that it can't be done.

DR. MAURO: Okay, that is an important point and that is why I jumped in. Really what we have here is a judgment that is being, I guess, discussed on where I believe it would be fair to say that SC&A's position is that we believe using

your very same methodologies some dose could be assigned for that, at least a portion of that six month period, because you have a certain methodology in place. That can actually be extended back in time. However, it is NIOSH's position that you probably can't because of the complexities associated with the rapid decaying and perhaps in-growth and change of radionuclides perhaps in time and space. It is a very fluid time period.

And I guess I would like to try to get to the nub of the issue. I believe, and correct me if I am wrong, the nub of the issue is there is a point when the complexity of the problem is such that it is really beyond that you could reasonably perform the dose reconstruction and it is a judgment call. And I guess the judgment call on SC&A's part in our work is that, well, it appears that you could do it, even though it gets a little bit more complicated. While it is NIOSH's position that not only do you have your definition

of the Class but also there is a technical underpinning and that underpinning goes toward the fact that you are dealing with a very complex, changing dynamic situation for that six month period.

That is my understanding of the nub of the issue and it really becomes not only -- I don't want make it sound as if somehow we are challenging the definition of the Class.

MR. KATZ: Well, John, it is Ted. I mean but it doesn't -- the technical business doesn't matter. It doesn't even matter because it is a policy bright line. It is not a NIOSH discretionary matter. It is the Secretary signed on the bottom line and drew this line and it applies. And we are beholden to the Secretary's decision.

So, it is not a NIOSH discretionary matter as to whether dose is being reconstructed for that six month period.

So that is why I say that the only way

for dose to be reconstructed for that six month period that we are talking about here is for the Secretary to reverse the Secretary's decision and remove that period of the Class for this cause, basically, for this element.

MS. LIN: This is Jenny with OGC. I would agree with Ted's assessment as well.

DR. MAKHIJANI: Well, in that regard I think we should just defer to OGC and NIOSH, in my opinion.

MS. LIN: I am not sure why we need to defer it. I think the message is very clear we agree with Ted that if the dose reconstruction -- if now the Advisory Board is making a decision about or wanted to make a decision that dose reconstruction is feasible, that would need to be escalated back to the Secretary. That is not a NIOSH discretionary issue.

DR. MAKHIJANI: And I guess what I am saying is that we go along with that opinion because we have been saying something different,

presumably. I mean I am still confused but --

MS. LIN: I think we should move on from this discussion.

DR. ANSPAUGH: Well is it fair to say that scientifically we could do it?

MR. KATZ: Well, it is just --

DR. NETON: Well, can you do it with sufficient accuracy? That is the question.

MR. KATZ: And there is no point in debating that.

DR. NETON: There is no point in debating. The decision about doing it with sufficient accuracy has already been decided.

MR. KATZ: Yes, this is not unique. There are other classes we have established where we have similar analogues where something could be done and we can't do it because it is just a legal bright line. So, it is not worth the Board's time I think to spend on this matter because it is out of our hands.

CHAIR CLAWSON: Well also, too, back to

my predecessor before me, the late Robert Presley, we basically pushed for this six month time period past it because at the Work Group, we were concerned with these radionuclides going past that time period. There wasn't that drop dead off and actually, NIOSH agreed with us on that. And this is where we got that six month period.

And now to come back and say -- it really is out of our hands now but what we were doing at that time period was taking that SEC and we were wanting to make sure that the people were covered under that. That is where that whole six months came from.

MEMBER MUNN: It's not as though this hasn't been debated in Work Group and in the full Board before.

CHAIR CLAWSON: Correct.

MEMBER MUNN: We have covered it very thoroughly, granted, in early years, but there was a great deal of discussion on exactly this point what you can do and what you can't do, as Jim said,

with sufficient accuracy. That was always the sticking point.

CHAIR CLAWSON: And this six month period, we felt, gave the petitioners their best opportunity because this other SEC came in later. And if it would have all been one thing, it may have been a little bit different.

My personal opinion is that it is really out of our Work Group's hands to be able to do this. We have already addressed this and already gone through the Secretary and we can't do anything about it.

MEMBER MUNN: It is actually out of the Board's hands completely.

CHAIR CLAWSON: Correct.

MEMBER MUNN: It is a fait accompli.

MR. BARTON: I just think their confusion was, when we read the decision, it was based on the inability to reconstruct internal dose because of the same definition for both periods.

CHAIR CLAWSON: Correct.

MR. BARTON: On the second period, we can do environmental dose. So we were sort of asking the question could you possibly do environmental dose. But it seems like what we are all hearing is that for that first SEC it is the inability to do occupational and environmental internal dose; whereas, for the second SEC, it is occupational internal dose but we can do environmental.

CHAIR CLAWSON: Correct.

MR. KATZ: Correct.

MEMBER MUNN: Yes.

MR. KATZ: Okay, then.

DR. ANSPAUGH: Okay, so we will move on.

MR. KATZ: Thanks, Lynn.

DR. ANSPAUGH: Okay, the next one is a small point but we were somewhat taken aback by a very optimistic statement about the performance of bioassays during the period of time addressed when people are doing some dose reconstruction. And so

the second recommendation was simply to provide a modified statement.

Instead of saying very positively that people who entered the underground test areas had their name on the roster and so forth, and that if they were exposed then they would have had bioassays, we would like to change that to the wording on this slide that says -- the next one -- that these workers may have been identified on the rosters that were published before the event and these workers may have had bioassay results. I think this is the more accurate reflection of the truth. We know that some people had their names on the rosters and never showed up and we have seen evidence of people who were in the tunnels who did not have their names on the rosters. Plus we do know that lots of people, rosters did not have bioassays, which is why the SECs were granted in the first place.

So, that is just a very minor second recommendation.

MEMBER MUNN: You'll have to forgive me. I do not remember the details of our years ago discussions about this particular point. But do we have any evidence that there is a significant number of people who were not on the roster who were actually in the tunnels? I know a significant amount of effort was made at the time on-site to try to assure that they had a good handle on who went in and who did not because early days or not, it was well understood that this was hazardous area and that records needed to be kept. So I don't recall. You may have seen this much more recently than I. What is our concern with respect to how many people may or may not have been admitted to the tunnels without any indication that they were in fact going to be working in that hazardous area?

DR. ANSPAUGH: Well I certainly can't tell you how many. I did look in detail at one particular accident of some note, which is the Yuma accident in 1963, I believe. There were a lot of people's names on the roster who were known not to

have been there. There were several people who were there who did not sign in and did not have their names on the roster. And I think, further, it is clear that people who may have been on the rosters didn't necessarily have bioassays done because we went through a very long analysis of who had bioassays and who didn't. And it turned out that the bioassays were very selective, only directed towards radcon workers -- I shouldn't say radcon -- rad protection people and also security people.

DR. NETON: I agree with that but it makes no difference at all. We are doing dose reconstruction at this point. If a worker has a bioassay, we are going to use it; if they don't, they are in the Class already.

All workers are in the Class for presumptive cancers. If they have non-presumptive cancer, then they will get a dose reconstruction using available bioassay data. If they don't have it, we don't do anything. I mean this wording may be more accurate but it makes no

difference in how we are doing dose reconstructions at all.

DR. ANSPAUGH: Well, I understand that but I object to the very optimistic statement that they would have been on the rosters that they would have had bioassays.

DR. NETON: Well and I suspect that that is a holdover from before the Class was added and it is easy to change that "was" to "may." I mean that is not a problem but, again, it makes no difference in dose reconstruction at all.

MR. BARTON: As Lynn said, it is kind of a minor point. And I think that is exactly what we are talking about.

DR. NETON: I don't think that we would revise the document just to make this one change but if we are doing it for -- we are going to obviously have some other changes. We would be happy to put that in there. It is not a problem.

CHAIR CLAWSON: That would be good. We have learned a few things through the years.

And I agree with Lynn on the "may" but also, too, I agree with you that it doesn't make any difference in the dose reconstruction. But if we do make a change or whatever, it would be nice to be able to put that in because I look at the perception from the petitioners and so forth.

DR. NETON: I don't disagree it is more appropriate wording.

CHAIR CLAWSON: Okay.

DR. ANSPAUGH: Okay, so then we will go on.

Again, this is just a reminder of the NIOSH calculations. The goal was to reproduce concentrations of radionuclides in air. And we will stick to 1963 through 1971, when the measurements of plutonium in air started in 1971.

So then the problem was that -- again if you remember, that the measurements of plutonium were sort of singular. There weren't measurements of a lot of other radionuclides. The corrections were made for long-lived radionuclides based on

measured concentrations in soil. And just, as I recall, in addition to plutonium-239 and -240, those measurements included things like cesium-137, strontium-90, europium-152, -154, and -155, long-lived radionuclides that were there many years afterwards. That was in the 1980s.

And so the key issue thing here is you get a ratio of plutonium to these other radionuclides and then you can infer the concentration of these other radionuclides in the air. So, that takes care of the situation in terms of long-lived radionuclides.

Then the situation gets much more complicated of how do you correct for the short-lived radionuclides that were present during the early times and which were not present in the soil in the 1980s. So, those corrections were made on the basis of the Hicks tables, which indicate the presence of short-lived radionuclides at various times after deposition.

Now the Hicks tables do not include data

on plutonium-239, -240 and this is for classification reasons. They do include data on a long-lived radionuclide of use, which is strontium-90. So the idea is you have measured air concentration of plutonium in 1971, you get a ratio of plutonium to strontium-90 based on the RIDP measurements, the Radionuclide Inventory Project, and then you get measurements of strontium-90 compared to all the other radionuclides based on the Hicks tables.

So, it is kind of a complex chain. You start with measured air concentrations. You have concentrations in soil. Then you have concentrations inferred from Hicks. That gets you back to the point of being able to reconstruct the concentration in air of all these radionuclides, including the short-lived ones. So, it is a complicated process.

The other thing about it is it is well-known that resuspension decreases as a function of time. And so the way the correction

was made for the decrease of time was using a resuspension equation that was actually developed by me based on measurements at the Nevada Test Site back in the 1970s.

And just to show you how this equation looks, this is actually taking from an ORAU report, the resuspension factor is a function of time. And you see that it decreases very rapidly from a level of one times ten to the minus five per meter down to about five times ten to the minus nine or something by 150 days or so. And eventually, the resuspension factor goes down to what is assumed to be a constant value of ten to the minus nine per meter after a long period of time. So, it does indicate that during very early times after deposition you have very high levels of resuspended activities, which decreases very rapidly.

The next one is just an example of the Hicks tables. You may not have seen these things in person before. And this is just to show you an example. This is a very complicated process but

it started out with Harry Hicks, who had access to all the classified information on every single device fired at the Nevada Test Site and in the Pacific in terms of what were the fissile materials in case of a thermal nuclear event. What were the devices? What were the materials around the device that would have been activated? And one of the things that was very much present was tungsten, which is used as sort of a mass thing to keep this whole thing together for a picosecond or two.

So, if you look at the complete set of radionuclides, you see there are a lot of tungsten isotopes for some events and not for others.

One of the key things about the Harry Hicks tables was these values were all normalized to an mR per hour at H plus 12 and that the document we submitted goes through some rationale for why this was done and the reason, basically, was that we had all kinds of measurements of mR per hour downwind of the test site because people knew in advance exactly when this would occur. The

monitors were all placed out there with their meters. They measured mR per hour. What we wanted to do was to get the deposition of radionuclides on the ground so that it could be referred to that mR per hour.

And so what you see here is the Hicks tables which do exactly that. And if you wanted to look at something in particular like cesium-137, you can see what the deposition of cesium-137 in terms of millicuries, microcuries per square meter.

This was also important because if you knew the presence of one radionuclide at a given point in time, then you could reconstruct the presence of all the other radionuclides. So, these were a very important calculation.

But in order to actually achieve this, the Hicks tables were normalized, as I mentioned before, to an external gamma exposure rate of 1 mR per hour at H plus 12 hours with use of Beck's tables.

Then the next one gives you an example of what the Beck's tables were. This is Harold Beck from the U.S. Department of Energy's Environmental Measurements Laboratory.

And so this gives -- for every radionuclide within the Harry Hicks tables, this gives the microR per hour per millicurie per square kilometer. So, the Hicks tables, as derived on the basis of fissile materials and so forth, in order to be normalized, you had to know the amount of emission of different radionuclides in order to come up with this normalization. So, it is not just the Hicks tables by themselves that are important. It is also the Beck tables, in order to derive that.

The next one is just a reminder of what these Hicks tables were for. They were not derived by or for NIOSH. They were derived for the offsite radiation dose reconstruction activity that was carried out by the Department of Energy in the 1980s.

Now one of the things that NIOSH has keyed in on was the Small Boy, which was one of the -- almost the last test at the Nevada Test Site. And another point about the Harry Hicks calculation is that they had to take into account fractionation. And this, again, gets into something that is very complex because it is known that as debris carries downwind, say from a nuclear explosion at the Nevada Test Site, the volatile elements which condense later are enriched in the material that goes downwind. And so what the problem is, in terms of this kind of dose reconstruction is if the volatiles are enriched downwind, that means that the refractories are missing downwind.

So then, where are the refractories, the missing refractories? Well, the missing refractories have to be on-site at the Nevada Test Site. So, in order to use the Hicks tables for on-site at the Nevada Test Site, you have to correct for this fractionation that was done to facilitate

the use for the people downwind. So that means that if you want to correct for what is on-site, the best way to do this takes four steps and the first two are shown on this slide.

So for example, for Small Boy, we start with calculations for 0.4 of the refractories present downwind. So in order to get back to even situation, then you need to add back in the 0.6 or 60 percent of the refractory radionuclides that were missing to create an unfractionated source term.

And then it is necessary to renormalize the unfractionated source term to 1 mR per hour at H plus 12.

Then the next two steps, you start with the now unfractionated source term and then you add back in another 60 percent of the refractory radionuclides that are presumed to have been on the Nevada Test Site and then you need to renormalize again.

Now, this is a situation where,

clearly, ORAU has gone through steps one and three with adding back in the refractories but I don't think the renormalization was done.

MR. STRENGE: This is Dennis Strenge. You are correct, we did not normalize -- renormalize because the normalization is not necessary because the only thing we needed was the relative activity among all the radionuclides. And you could take -- well, Hicks prepared a 1981 report called Calculation of the concentration of any radionuclides deposited on the ground by oxide fallout from nuclear detonation. I'm sure you are aware of that one. And in there, he has the equations for the normalization and it shows quite clearly that every activity value in that table is corrected by the same factor. So, the normalization that he did did not change the relative activities. And for that reason, we have not done the renormalization. We did do the corrections you mentioned, quite clearly, that we removed the refractory fractions and we corrected

back to -- not to times zero but to the initial site. So, those corrections, we did do.

DR. ANSPAUGH: Okay I understand. The question of whether or not the renormalizations should be done in order to get the correct relative things is something that I think needs to be looked at. And that gets to our Recommendation Number 3 that we would like to see the details of the result of that calculation so we could actually check that very issue.

One of the things that is missing from both the ORAU team and also your report, Dennis, was the results of the recalculation process. And in order for us to verify the calculations were done correctly, we would really like to see the results of those calculations.

MR. STRENGE: Well I guess we just felt that dividing by the listed refractory fractions wasn't that complicated and the tables would be huge. So those things, I am sure, could be provided.

DR. ANSPAUGH: Okay well, we would certainly like to see that. My feeling is that it would be necessary to renormalize but I am open to being convinced otherwise. But I would really like to see the results of the calculations. In essence, you would be reproducing the Hicks tables, which is not that big a file and certainly not nearly as bad as what we are going to ask you to do later on.

MR. STRENGE: Okay.

MEMBER MUNN: This is a fascinating scientific puzzle. The question that arises in my mind immediately is would the additional steps to normalize these data, as has been requested, make a significant difference in the calculation of dose? That is the bottom line from my point of view.

I can understand -- as I said, it is a wonderful science puzzle and it would be great fun to delve into that for a few months and play with the numbers but whether it makes any significant

difference to the claimant is a key for us in the Work Group I think. Does anyone have any feel for what normalization --

MR. ROLFES: This is Mark. As of right now, we are calculating doses for non-presumptive organs, essentially, those that don't fall into the SEC.

MEMBER MUNN: Right.

MR. ROLFES: The environmental intakes, the environmental internal doses that are calculated with the current model for many of the non-presumptive cancers are less than a millirem per year.

MEMBER MUNN: Yes, that was my concern. Is this really going to -- and less than a millirem a year is certainly not going to affect our final dose reconstructions. So that raises another aspect of the question, I suppose.

MR. BARTON: Mark, this is Bob Barton. Does that same concept apply to the less than 250 days where even for presumptive cancers you apply

the environmental doses? Would they all be very low like that or did that make a difference for less than 250?

MR. ROLFES: I have a table somewhere in the past several years that might provide example calculations of the resulting internal doses from the various organs. I don't know where that is in my emails or files at the moment but I could dig it up again.

I think we provided that to the Board in the past.

DR. NETON: Well I think the question right now is does this normalization need to be done or not. I mean it sounds -- I didn't understand completely what Dennis was saying but it sounds to me like it was a pretty simple logic argument that showed that it wouldn't make a difference because it is all relative.

Couldn't we start with that and maybe try to convince SC&A that that normalization is not required? Can you put a page of writing and you

could go check that yourself to verify that it is appropriate not to normalize, rather than redo all the calculations for the normalization that may not be required anyway, which is what you are proposing, I think.

DR. ANSPAUGH: It certainly is possible to check that and determine whether it would make any difference or not. Yes, it could be done.

DR. NETON: I would rather start with that, rather than have you guys redo all the calculations using a normalization that might not be required. I mean the first step would be to say is it required yes or no. And I don't know what could be provided to convince you it is not required or how we need to guide you down that path, but clearly it is more than just Dennis saying that is so.

DR. ANSPAUGH: Well, I would like to see the results of Dennis's calculation and then I would like to redo it myself, if the Board asked

me to do it. I don't want to wander off.

DR. NETON: Well what I am saying, though, is it really necessary to redo them using the full normalization, if it is not required? That would be -- wouldn't seem to be worth the time to do that if one could convince others that it is not a required step.

CHAIR CLAWSON: So, Jim, it wouldn't be that much from what I hear to show what Dennis was saying about whether the normalization does not matter.

DR. NETON: Dennis, is it possible for you to put together some brief discussion write-up that would lead us down that path better?

MR. STRENGE: Well maybe I have been looking too closely at it. It is really simple to me because if you have a set of numbers and you multiply them all by the same value, which is what the normalization does, you are not changing the relative values at all. It is sort of like you didn't change the relative values, you are going

to get the same answer out when all you are using is the relative values.

DR. ANSPAUGH: Well I would disagree that you are multiplying all the values by the same number. The numbers are different, depending on whether it is volatile or refractory.

MR. STRENGE: Could you go through that? I don't really understand that.

DR. ANSPAUGH: Well if you look at your table, which I believe is Table 1, you see the numbers are not all the same. Some of them are 1 and some of them are 0.4. And there are a few oddballs that are in-between.

MR. STRENGE: Oh, those numbers are not involved in the normalization that Hicks did for the 12 hours.

DR. ANSPAUGH: Well, we have a disagreement there. Maybe it would be better for you and I to try and resolve that offline. I don't know.

MR. STRENGE: Okay. Do you have my

email?

CHAIR CLAWSON: Actually to be able to come to this, both sides fill further out. But I agree with you, Jim, that I don't think we have to go through this.

I think what we need to be able to do -- but I would like to see whether the Board will concede to a write-up, a White Paper, whatever you want to call it of why or however we want to be able to do this.

But there is a disagreement here. Somehow --

MR. KATZ: Why don't we have -- Dennis and Lynn can talk in a technical call offline and write up a memo just summarizing that discussion and, if there is still a difference, what the difference is and why. And if there is concurrence, explain that. And we will have that, distribute that to the Work Group.

DR. NETON: Yes, I think that sounds reasonable.

MEMBER MUNN: That would be helpful.

MR. KATZ: And the other matter I think that is equally relevant is when Mark goes back and looks at his files, if he finds that this has no bearing on doses, it is a non-issue.

DR. NETON: Well, I agree. I mean a 250-day exposure limit is not going to provide a tremendous amount of dose.

MR. KATZ: Right. Right. I think the bottom line is the Board doesn't want to spend a bunch of money on an issue that has no bearing on doses. So, if that is the case then it doesn't even really matter to resolve the -- if it --

DR. NETON: I mean to me, though, if there is a technical issue, we should address this.

MR. KATZ: I know, but I am just saying Lynn doing a bunch of recalculating and so on, that kind of spending real time and real money on an issue that has no bearing on dose, to me, is a waste of the Board's money and time.

MEMBER MUNN: As I said, it is a

wonderful exercise and would be great for him to do but the bottom line to me still is does it actually affect the dose reconstruction. That is our job here.

DR. ANSPAUGH: Well I think the other bottom line that I have been wondering about from the very beginning is how many people do we really have who would benefit from such a calculation to the point where they might actually be compensated. I don't know whether there is anybody.

DR. NETON: Well the problem with that logic, and we have gone down this path before is if there is one --

MR. KATZ: Yes, we want to do right by everybody.

DR. NETON: But, you know, I looked at those doses before the meeting and they are pretty small. These environmental doses, these resuspension doses, especially the non-presumptive cancers, remember these are --

MR. KATZ: No, I know. Mark already

addressed that.

MR. ROLFES: Yes, there was a specific example, I guess, that SC&A had asked us to provide a sample dose calculation for a thyroid cancer and we had redone that and I am trying to find the results here.

MR. KATZ: But you don't need to scurry, Mark, because we will have this technical discussion offline, we will have a memo. So you have time. You don't need to dig it up now, Mark.

MR. ROLFES: I was just going to say the doses were very low and it wouldn't have made a difference in the compensation decision.

DR. ANSPAUGH: The other thing is that if you presume that the calculations were done correctly, the doses are very low except for ET and the doses were up to a level of around for ET, I believe --

DR. NETON: Oh, for ET1 and ET2? Yes.

MR. ROLFES: Yes, I think I had looked into the number of nasal cavity cancers as well and

I didn't see any at the time. I think that was also combined with the issue of 1962 because you had asked us to extend the internal dose calculations back to 1962. Since we are not doing that, the doses received -- the dose is really only important in that first year for the ET1. Virtually 99 percent of the internal dose is all delivered in that single calendar year. And we are not going to be doing any internal dose calculations for 1962 because of the SEC determination.

As I said, I didn't see any individuals with employment in 1962 or 1963 that had cancers of the nasal cavity. So, there wasn't anyone for which the model would apply.

MR. BARTON: When you say the calendar year, you mean just 1962 or you mean July '62 to June '63?

MR. ROLFES: I have looked specifically, I believe, into 1962 because, at the time, SC&A had requested that we had redo the model, essentially, to start calculating internal doses.

Back -- develop our resuspension model, recalculate intakes dating back six months earlier.

MR. BARTON: Well, I didn't know if you were talking about that six month period or actually into 1963 where we are doing the environmental doses.

MR. ROLFES: It was during 1962 is what I had done for. Looking back at an email because I had looked into this. Let's see.

It looks like the doses were calculated. I'm just looking at an email. The doses were pretty low, with the exception of those that would potentially be received by a hypothetical claimant. We didn't have a claimant in our -- not this database at the time I had looked at this. The exception was the ET1 region, the nasal cavity, and the high doses from the short-lived fission and activation products for ET1 and other organs were almost entirely delivered in the year that the intake occurred.

So this issue was really only an issue that would affect non-presumptive cancer cases with employment in 1962. And that was when we were considering re-estimating intakes back to July 31, 1962.

And I agree there were some high doses to ET1 from the short-lived fission and activation products.

DR. NETON: But even if you put a rem dose into a nasal cavity, it is not going to be compensated. I mean it is not even close to being close to a 50 percentile case I don't think.

MR. ROLFES: That was one of the highest exposed organs, ET1. I think some elevated doses to surfaces and other metabolic organs.

DR. ANSPAUGH: Well, I guess it is an interesting question whether or not any of this is worth doing but if it is worth doing, I suppose it is worth doing right.

DR. NETON: Yes, I can't disagree. I

think we should resolve this first question, which is the normalization factor. I mean if it needs to be completely -- first we can decide whether it does not need to be completely redone and if it does, then we can decide how to proceed I think after that.

DR. ANSPAUGH: Well my understanding of what the recommendation is that Dennis and I are trying to resolve this offline.

MR. KATZ: Yes. So we will set up a call. Work Group Members can listen in and you folks can discuss this. If Dennis wants to send you a piece of paper first, that is great and that should be back to the Work Group Members. You will have your call and then we will report out with a memo the results of that.

DR. ANSPAUGH: Dennis, are you still in Richland?

MR. STRENGE: No, I am in Washington State but I am in Western Washington. I moved to be close to grandkids.

DR. ANSPAUGH: I see. Okay. Well, we'll figure how to get in touch.

MR. KATZ: Yes, we will set that up. It will be a conference line so that other folks can listen in.

MEMBER MUNN: Terrible choice, Dennis.

DR. NETON: Yes, this will be a conference call Ms. Copeland will set up, to get the number --

MR. KATZ: Yes, we will set that up.

DR. NETON: -- and Board Members can listen in. Because we need to do this somewhat transparently.

MR. KATZ: Yes, absolutely.

DR. ANSPAUGH: In order for Dennis and I to be prepared for such a conference call, I think we need to exchange some data first.

DR. NETON: Yes.

MR. KATZ: Send -- you are welcome to email and just copy me in the process of emailing so that I can share that with the Work Group

Members.

DR. ANSPAUGH: Okay.

MR. KATZ: By all means.

MR. STRENGE: Yes, that is good.

CHAIR CLAWSON: Well when we get to that point, we kind of know the background of how we got to where we did.

MR. KATZ: Exactly.

CHAIR CLAWSON: We will look through the emails, then also the transparency of it. Because it is hard for us, as Board Members, too, to be able to come in when these decisions are being made and we are still wanting to know how we got to that. So, if we are involved with it, it would be good.

DR. ANSPAUGH: Well, I guess the bottom line is this is very complicated and difficult to comprehend stuff. It doesn't matter whether you are a Board Member or a flunkey. It is still hard to trump in.

Okay, well, let's move on to what I

think is also a central problem that we have been discussing here. I find that these calculations, as presented by NIOSH and also by Strenge, the calculations are not transparent and I can't follow them. After we got to the point where we are now, the intermediate results are not shown and the descriptions, I think are, in some cases, they are either not clear or conflicting. And we will go through and show some of these things.

And the next one is my favorite cartoon, which is the way I feel after looking at how these calculations have been performed, is that we go along just fine and I understand everything completely and then it is seems like the miracle occurs and I can't follow it.

So, the next slide indicates to me what is the miracle. What I infer from what is said in the documents is that IMBA was run to determine the relative importance of 177 radionuclides to 26 organs for ten one-year periods. That gets up to be 46,000 IMBA runs. And then Dennis did it for

five scenarios, which is 230,000 IMBA runs. And then if you add in ingestion, that is up to half a million IMBA runs.

And so my conclusion is that I am not understanding what was actually done or running half a million IMBA runs really is a miracle.

MR. STRENGE: Yes, well, this data is -- I like running IMBA but not that much.

In the Technical Basis Document in the attachment A.6, what Gene indicated there was the dose from the short-lived fission products was actually taken from ICRP 68. So, we didn't need to run IMBA for all those radionuclides.

And in my White Paper, I indicated that I did run IMBA but that was only to get the annual dose values from a unit intake, one becquerel per year of strontium-90, which I then used to generate the final results.

So, we did use IMBA but it was only one run. And, fortunately, when you run IMBA you get the results for all the organs in one run. So, that

simplifies things also.

DR. ANSPAUGH: Okay well that is a factor of 26.

MR. STRENGE: Yes, right.

DR. ANSPAUGH: That does help.

MR. STRENGE: Yes.

DR. ANSPAUGH: Well you know the bottom line of all this stuff is that I think it would be very helpful if somehow or another in this process that there would be a very clear description of exactly what was done and with the intermediate results shown so that people like me, and I presume some of the other people here could understand exactly what was done.

MR. STRENGE: Yes, I can certainly understand the difficulty in going through that because when I did the work for the White Paper, I started with some of the files and information from Gene Rollins, who did the original calculation and it took quite a while to figure out what was going on. So, I can really understand where you

are coming from.

DR. ANSPAUGH: Okay. Well, maybe we can skip over some of this other stuff and get down to --

MR. KATZ: Well, can I just ask do we have a path forward into what will address your concern at this point?

DR. ANSPAUGH: Well, if we could skip over to slide 26, this is our recommendation to the Members of the Work Group. And that is basically what we just discussed. I think it would be very helpful to me and I think to everybody else that this recommendation is that NIOSH and their contractors should be very specific about how the calculations were done and to provide the intermediate results so that we could understand exactly what was done and also do some verification, independent verification.

And I also understand that this could be voluminous in amount of material that may not be presentable in a written form but it could be

done on a DVD or some method of communicating things that are other than paper.

MEMBER MUNN: Well the volume may be reduced significantly, once you and Dennis carry on some conversations.

DR. ANSPAUGH: Hopefully so.

MEMBER MUNN: A great deal of this may be easily explainable verbally so that the written result can be much clearer for all concerned.

The first step is for you two that know what is going on to talk about it.

DR. NETON: Well this is a little different from that last conference call we were talking about.

MEMBER MUNN: Yes, it is. It is.

DR. NETON: It would seem to me if you provide all these calculations, it is still going to need a roadmap of some type because, obviously, they are going to be just massive, I assume, things like spreadsheets and whatnot that were done.

DR. ANSPAUGH: Well, I am presuming

that the spreadsheets are not going to fit on a piece of paper. I think that is the problem.

(Simultaneous speaking.)

DR. NETON: But if they are not annotated in sufficient detail, it would be difficult for this third party to look at and decipher because they probably weren't developed with that intent. I mean I shouldn't speak for Dennis. Maybe they are.

MR. STRENGE: There are a lot of spreadsheets and they are fairly complicated. It might be possible to do some extractions for say one organ instead of all 26 and put something together that can show the numerical progress.

DR. NETON: That is what I am thinking is maybe just an example, maybe a once through for an organ to show the concept or the process that was used.

I think if the concept can be shown to be accurate, then I'm not sure we really need to go and verify every single cell of every

spreadsheet is valid.

DR. ANSPAUGH: Well, I certainly wouldn't have that kind of intent but I would certainly like to be able to go through a calculation say for an organ just to be able to follow the calculation.

MR. ROLLINS: This is Gene Rollins. I am the author of all this stuff, originally, I guess.

When I handed this off to Dennis to do a third-party review on it, I didn't give him much information about how to follow the calculations and he is a pretty smart guy but he figured it out. And I believe Dr. Anspaugh could figure it out, too. I don't think it is going to require that much for him to understand what we did.

DR. ANSPAUGH: You overestimate how smart I am but thank you anyway.

DR. NETON: I guess then the question is what can we provide, other than just a data dump of everything you have done to SC&A and Lynn

Anspaugh that will allow him to review?

MR. KATZ: Well you suggested he provide an example and maybe he could be on the line and walk Lynn through -- on the phone with Lynn and walk Lynn through the process, so that Lynn doesn't have to decipher it. We could do that, right?

Bob, you could use a little of it, too, because you could help facilitate at least Lynn getting the spreadsheets and all of that.

MR. BARTON: Oh, absolutely.

DR. ANSPAUGH: Well, Bob is a lot smarter than I am.

MR. KATZ: So anyway, why don't we do that? It is almost -- it is not really a technical call, per se, but they can walk Lynn through the spreadsheet for an example.

DR. NETON: That is a clarification type issue.

MR. KATZ: And then Lynn can ask questions and sort that out. And then if we need more follow-up after that, we will have more

follow-up.

Is that okay, Lynn?

DR. ANSPAUGH: That's fine.

MR. KATZ: Yes, okay.

DR. ANSPAUGH: Okay, we are almost done. So, let's go to Recommendation Number 5. And I had made this recommendation several times that I would like to see NIOSH and contractors also consider the source term for the Sedan event and the reason is this is a very large event that occurred on July 6, 1962 almost at the end of testing.

And the Sedan source term is very different because it was a large thermonuclear event, less than 30 percent fission and 70 percent thermonuclear. And this results in quite a different mix of radionuclides. And just as one example that I pointed out in the paper was if you look at the relative amount of some of the tungsten isotopes, it differs by five orders of magnitude larger for Sedan than for Small Boy, for example.

And the next slide just shows that Sedan was a major event. Even though it was 600 feet underground, it created a huge crater that was 1280 in diameter and 320 feet deep. And so it produced a massive amount of fallout, which was falling clear across the U.S. and also had appreciable residue on the Nevada Test Site. It created large amounts of activation products, in particular. So, this is just a suggestion that I think it would be helpful to see what the differences would be if we considered Sedan in additional to Little Feller I or Small Boy.

So, that was Recommendation 5.

DR. MAURO: This is John Mauro. Just a quick clarification for my benefit. So, the essence of this is that the construct that was just described with regard to the Hicks tables and the relative amounts and how that was back calculated out, are you saying that if Sedan was -- the incident because this is also in more or less the same time period I believe, would that change the

whole paradigm? I guess I am not quite sure of the implications of looking particularly at this event that was, I guess, one of the tests that contributed to the residual radioactivity that was actually observed in the soil and in the air. Does this change the paradigm?

DR. ANSPAUGH: Well, it is going to change the radionuclide mix in a substantial way and I don't know if that means it is going to be worse or better. Well, if I had to guess I would say it was probably better because there is less fission product but I don't know.

MR. ROLFES: Gene, this is Mark Rolfes and I know we have discussed this issue as to whether or not we would. We looked at the Sedan event and I don't recall -- I'm looking for a write-up or anything in my email but I am unable to find anything. I know we discussed this. Do you have any recollection of this issue as to whether the source term would be significantly -- from what I recall, I thought that what we were

currently doing would have resolved it in more claimant-favorable mixes of radionuclides to use. But I might be imagining that.

Do you --

MR. STRENGE: This is Dennis. Yes, when I saw the comments on Sedan, it made me curious. As Mark indicated, I actually did do some calculations but they were never written up. What I found, briefly, was that -- for 1963, some of the values went up but it was, at most, a factor of three.

And then when you get to 1964 and beyond, there is really not much difference and I think the reason is is because the three tungsten radionuclides, the longest half life is like 120 days. So, even though they start out at a high amount, in a few years, they do decay away and don't contribute that much.

MR. ROLFES: Thank you, Dennis.

DR. ANSPAUGH: Well, you know if Dennis has already done this and it could be written up,

I think that would be helpful because Sedan was a very major event.

MEMBER MUNN: Atypical.

DR. ANSPAUGH: Atypical, right.

DR. NETON: Yes, I agree. It sounds like we could just put that in writing, form a White Paper.

MR. STRENGE: Yes, one question I have on that, for the underground event we took the Hicks tables and backed out the refractory fraction twice. Is that -- I'm not sure how that second removal of the refractory fraction really physically is described when you have an underground event. It is kind of hard to imagine.

DR. ANSPAUGH: Well, the way Hicks did it was the same as the Small Boy, as I recall.

MR. STRENGE: The same refractory fraction, yes. It was 0.4.

DR. ANSPAUGH: Right.

MR. STRENGE: But when we apply it, take it out the second time, we are, in effect,

saying that at the site of the detonation, the refractories are enhanced relative to the volatiles.

DR. ANSPAUGH: Correct.

MR. STRENGE: Well, I guess that would still be applicable because I am sure the volatiles were probably just blown out and most of them went a ways. So maybe it is still valid.

DR. ANSPAUGH: I think it is still valid.

MR. STRENGE: Yes, okay.

MR. KATZ: Okay, so then Dennis, you will write up a little White Paper on that analysis.

MR. STRENGE: On the Sedan, yes.

MR. KATZ: Yes.

MR. STRENGE: I will have to go through and check on my calculations again but I will do that.

CHAIR CLAWSON: Yes, if I remember right, back -- and I have to agree with Mark, I thought we looked at kind of all the Plowshare

programs because that is where we got some of the larger releases and stuff like that. And there wasn't that much difference.

But you know it is a question we have if we have got a write-up that we could have, then it has been addressed and taken care of.

MR. KATZ: Okay.

DR. ANSPAUGH: Okay, well the next slide indicates that sort of what we were just talking about, actually. You mentioned, Brad, that there was a large number of underground shots that actually vented 1963 to 1970. We had these five Plowshare events that released from 100,000 to a million curies. The Baneberry event released 10 million curies and then we have all these tests of nuclear rocket engines.

The question here is just what does this mean relative to how we calculate the doses. And I don't have an answer to that and I am not sure that it has been seriously considered. But the next slide is the photograph of the Baneberry

event, which, as you can see, it was, indeed, a very massive event and it nearly resulted in getting the test site shut down because this was detected well beyond the borders of the United States, which was in violation of the treaty, of course.

So, the Recommendation 6 then is have we really considered these impacts of the hundreds of other releases in a serious manner enough that exposures to claimants are really considered fairly. And maybe NIOSH has already gone through that calculation. I don't know but my impression is it hasn't really been considered seriously.

CHAIR CLAWSON: I thought we had because that was one of my question is what created what is referred to as the Plutonium Valley. And that is where the write-up Mark brought out of looking at this.

If my memory serves me right, it was addressed and that this was all put in, that we were good on it. But I guess we will have to resurrect that paper, Mark, if you can find that. Because

this was a big issue. The reason why is because that was one of my issues because I had dug into quite a bit of this and I wanted to know all of these, especially Plowshare because of the release into the environment and that is where I remember Mark did a write-up that also SC&A reviewed and they both came up with the same thing. But maybe we need to bring it back.

MR. ROLFES: Yes, I will see what we can do to find the previous document that we discussed.

MEMBER MUNN: I recall a lot of conversation about this during our site visit but I don't know about the documentation for it.

DR. MAURO: This is John. I have a question, conceptually. When I was working with Lynn on this, I was thinking in terms of the whole motivation behind this array of calculations was environmental. We all recognize that there were occupational exposures during this time period which were transient situations where it was impossible to try to reconstruct doses to

individual workers who might have been involved in the individual occupational activities, along the lines of backdrilling and on-site work.

Now, we are bringing into the picture something that in my mind is of -- philosophically or conceptually similar. Namely there were transients related to tests that were -- and I consider these field activities transients also, where a person goes out and does a certain job, which are occupationally related.

The question I have is are these matters, such as these venting occurrences, occurrences that appropriately belong to what I would call this chronic environmental exposure that everyone was exposed to throughout this long time period, where you were trying to assign some environmental dose to those workers who are not covered by the SEC.

Now, what we are doing here is superimposing on that well there were also these transients that would also have occurred. Do

these scenarios fall within the purview of exposures that cannot be reconstructed and therefore a part of the story that says well, that is why there is an SEC. There are certain scenarios that we can't reconstruct doses to individual workers. Or do these fall into the idea that well, this is part of the chronic environmental exposure that Strenge and Rollins constructed to at least assign some dose, environmental doses to workers who may have been present on-site, where we don't know where they were necessarily, or what their -- but at least there is a way to assign them some internal dose.

So, I guess I would like to just raise the question. These particular incidents of where there were these transients that occurred during the time period of interest, do they fall into the category that is appropriately considered part of the chronic environmental exposure that you would like to try to reconstruct or do these represent scenarios that may have been transients but they

can be reconstructed, as opposed to the other situations of occupational exposures that cannot be reconstructed? I'm not quite sure if you understand the question.

MR. KATZ: John that was a very clear explaining of the question.

DR. NETON: This is Jim. It is not clear to me because I don't recall this evaluation that was done for these events, to be honest. But I don't know when we are talking about atmospheric inhalations or whether these would be resuspension models, deposition and then add to the resuspension model. To me, if they were atmospheric it would be related to the occupational environment. I mean if they are just released to the environment -- I mean released to the atmosphere versus this environmental model, which is essentially just a residual contamination model, I don't know you could do that. Could you really --

MR. ROLFES: I think where we are at right now with the current resuspension model to

assign environmental intakes, it is the product of many years of our work and the Work Group's work. So, we are trying to refine the model once again or going back to a previous discussion that we have had in the past with an earlier version of the environmental intake models because we have gone full circle.

MR. ROLLINS: This is Gene Rollins. I would like to say something here.

A resuspension model was not used to estimate these doses, these intakes. We keep getting back to that and it seems that Dr. Anspaugh seems to think that I am using his model to estimate atmospheric concentrations and I never did. The only atmospheric concentration I ever used in any of my calculations was the highest Pu concentration measured at the site in 1972 in Area 9. That did not require a resuspension model. That was empirical data.

So resuspension is really not an issue here.

DR. ANSPAUGH: Well, if I recall what you wrote in your report, you certainly have higher values in 1963 and to get there, you did use a resuspension model.

MR. ROLLINS: No. No, I did not.

DR. ANSPAUGH: Then I totally misunderstand what is in your report. And you also have a graph of the resuspension model in your report.

MR. ROLLINS: Right and that was used to estimate the effects of short-lived -- of early resuspension. And that was just a multiplication factor that I used based on that 1972 air measurement.

DR. ANSPAUGH: Well, I understand, but the ratio is based on resuspension, as stated in your report.

MR. ROLLINS: Well yes, I integrated to a function on how important it could be.

DR. ANSPAUGH: Yes and you applied it.

MR. ROLLINS: But I did not use it to

estimate airborne concentrations. I used it generally as a factor I could multiply the derived intakes by to account for early resuspension.

DR. ANSPAUGH: Well that is using the resuspension model in my book.

MR. ROLLINS: Well okay but I didn't use it calculate airborne concentrations. I used it to develop a multiplier to multiply the intakes that I derived to take care of the early resuspension.

DR. ANSPAUGH: Well I agree and maybe we are just differing on some minor terminology but you did use a resuspension factor model to increase -- well, to create your multiplier for the intake. Right?

MR. ROLLINS: Yes, for 1963, '64, and '65.

DR. ANSPAUGH: Right. Okay, so we are in perfect agreement. Yes?

MR. ROLLINS: Right. But nowhere in the intermediate calculations did any air

concentrations were ever used, except for one measurement that was used that came from 1972 Area 9 for plutonium-239.

DR. ANSPAUGH: Okay, yes, I understand.

MR. ROLLINS: Okay.

DR. MAURO: This is John again. So, my understanding is that the issues of these venting occurrences can be thought of within the concept of there is the actual airborne releases that occurred at the time of the event and there is no -- during this time period -- that is unrelated to resuspension.

And the issue is not that we are trying to reconstruct those doses. The point that is being made here is the fact that those events occurred deposited radioactivity in the soil, as was done during atmospheric testing. It has, in fact, been captured by plutonium data that was measured in the air and by the soil activity that was measured because that is the rock you are standing on.

MR. ROLLINS: Right, John. It's all based on the soil measurements that were made in 1981, which would have included all these releases, they came after 1963.

DR. MAURO: Okay, good. That is why I am raising the question. I am trying to say that I think you might be okay.

MR. ROLLINS: Yes, I think so too and I am going to tell you why. Because we backed all that data in 1981. The case record goes back to 1963. So, there were a lot of persistent radionuclides that were put on the ground by these ventings that occurred after 1963 but really weren't there in 1963. So in my way of thinking, that makes it claimant-favorable.

DR. MAURO: And that is why I bring the point up. And I think my sense is I am agreeing with you. That is, what is being said here is that these transients, at least to the degree to which they contributed to the activity in the soil and the associated resuspension, aren't captured by

the data that you base everything on. And you are not trying to reconstruct the doses from the actual airborne ventings but you are saying that, effectively, your methodologies have captured it because you are working with data from the soil that was collected and data from the airborne plutonium that was mentioned that would reflect these transients that have occurred and resulted in soil contamination and resuspension. That is what I understand that -- why you are okay, I guess I am saying that.

MR. ROLLINS: Well I appreciate you have a complete understanding of it, John.

DR. MAURO: Okay.

MR. ROLLINS: Thank you.

CHAIR CLAWSON: But this would be part of the environmental, wouldn't it? Because back in the days, I remember because we were talking about this, we wouldn't be able to do these exposures as they were. This would become environmental because we were talking about the guy

that was going out there with a CAT in the Plutonium Valley making all of this.

And so this is where I believe this paper that Mark was talking about and also where they were talking about that they back calculated this. And the reason why we didn't address it is because it wasn't an SEC issue. It was a Site Profile issue. And this is what we are back to now.

Is that painting a good enough picture for you, John?

DR. MAURO: I believe so. And I guess I would want to defer to Lynn because we did not -- Lynn and I did not spend very much time discussing this one particular issue. And now that it is on the table before us and I just articulated my understanding of the issue, I would like to hear if Lynn agrees that my characterization of it is fair, where I am, in effect, agreeing with Gene and whether Lynn would agree that conceptually, in effect, these incidents, at least the resuspension in soil

contamination aspects of their contribution to dose as environmental is, in fact, a correct representation or did I miss something.

DR. ANSPAUGH: Well you know the last major contaminating event was Baneberry and that occurred in 1970. So, I think Gene is correct in the sense that the measurements of the RIDP program went on for years and years but, basically, it was during the 1980s. And so in that sense, anything measured by RIDP in the 1980s would have included most of the major events -- well, all of the major events. So I think probably in that sense, we are okay.

The Baneberry event was also very unusual. It was both contributed to the environmental background but it also created some quite large exposures to workers who were evacuated and they were measured. They did have bioassays. So, I think Baneberry goes through both situations. And I think the large exposures to individual workers who were taken care of because they were,

in this case, identified and they were bioassayed or they had external badge measurements. So, I think Baneberry was unusual in that it encompassed both a contribution to the general environment and also very specific exposure to workers.

CHAIR CLAWSON: So do you feel good about this? Because my question is because this was a very personal one to me because I wanted to know how they were going to be able to address all these releases. And this is when Gene came in because my memory is starting to work again back here, since I have slept, that they back calculated everything and it brought all this in.

But this was more of an environmental dose because, as you said with Baneberry and stuff, they have the bioassay. So, the people were taken care of. But my picture was was how are they going to be able to address all these releases to the general people, people that were just out there working and so forth. And this is when they came up with this and this came down to the environmental

dose of it.

So, do you feel good about what --

DR. ANSPAUGH: Well you know in thinking back, I am feeling better. I will tell you what. You know what Gene did originally in his calculations was very conservative. He picked the highest concentration of plutonium ever measured, which appeared in 1972. And for some of these other radionuclides in the ground, he picked the highest concentration measured in any area. So, there is quite a large amount of conservatism built into there already.

And then considering that the measurements of the Radionuclide Inventory and Distribution Program were made in the 1980s, it should encompass all these residual activities. And so by the time you move them back to 1963, I think we are fairly well covered.

DR. MAURO: This is John. There is something important that has happened here that I just want to make sure I understand.

When it comes to Baneberry, what I am hearing is that there is a lot of data, which means that the exposures from that, where you have bioassay data which was collected, it sounds like fairly extensively, you will have the data to reconstruct the doses for those workers not covered by the SEC, which would be the way you would always deal with any circumstance where you have data. But there is no intention here to say let's see do we want to address exposures for workers that may have been exposed to Baneberry airborne venting -- not the resuspension part now. Remember we like to separate the two ways of thinking. One is the resuspension aspect, which is this chronic exposure from the actual event where there was venting.

Here is a circumstance where everyone agrees, yes, there was significant venting. Yes, we do -- where there was direct airborne exposure and not resuspension and that we do have lots of data where, at least for a significant number of

workers, we have data where we can reconstruct the doses for those workers not covered by the SEC but no attempt is being made here to build a coworker model to reconstruct doses to other workers that may have not been bioassayed but exposed to Baneberry direct releases.

Is that a correct statement?

MR. KATZ: Yes, lots of heads nodding, John.

MR. ROLFES: This is Mark Rolfes. John, there is a specific matrix item on the discussion of Baneberry. It is matrix item 13 and SC&A had a comment that the method for estimating iodine-131 exposure due to the Baneberry venting does not appear to be claimant favorable. A similar approach for other ventings may also underestimate dose. And the suggestion was the development of a method for assigning more claimant-favorable partial iodine-131 doses appears to be warranted.

The NIOSH response -- following the

December 2014 Work Group in the matrix that is from May 15, 2015 -- the NIOSH response, we agreed that the iodine concentration measured at Orange Road on December 18th -- there is a typo, it says 1968 but it is supposed to be 1970 -- at 9:30 a.m. may be a more appropriate concentration to use to estimate bounding doses to unmonitored workers. Had the concentration 3.5 times 10 to the negative seven microcuries per cc been used, an intake of 0.835 microcuries for two hours would have been calculated, resulting in a dose to the thyroid of 6.5 times 10 to the negative fourth rem. That would be the maximum exposed organ and that would be less than one millirem, which is already accounted for in other doses assigned in a dose reconstruction for an NTS employee.

DR. NETON: But see I am not even sure those doses should be reconstructed, based on what we just talked about here.

MR. ROLFES: True.

DR. MAURO: That's why I brought it up.

I think that what is important about this is that we understand the bioassay. You always use that. What we have here is a circumstance where you have other data of airborne sampling that would allow you to -- now I don't know if you have considered this a coworker model, I don't think it would, but you are saying you have some other data which is air sampling data that perhaps might be useful in reconstructing the doses from the direct releases.

But, Jim, you just pointed out that -- well, perhaps we should not do that.

DR. NETON: I think if you look, and this is in the SEC 84 time period, yes, I am pretty certain that the designation for the Class, the determination language, talks about the unavailability of bioassay and adequate bio air monitoring and all those kind of traditional monitoring data that are not sufficiently present to reconstruct doses. So, it already acknowledged that the source term -- not the source term -- that the monitoring information is not sufficient,

including environmental samples.

DR. MAURO: Okay, that was my understanding also.

DR. ANSPAUGH: Mark, what was the date of that measurement?

MR. ROLFES: The date was December 18th and there is a typo up here that says 1968 but the Baneberry event occurred in 1970.

DR. ANSPAUGH: It was on December 18th?

MR. ROLFES: Correct.

DR. ANSPAUGH: Because at one point in time, you had a measurement that was made six days after the event. So if that one was really made on the 18th of December, that is better than one six days after the event.

MR. KATZ: So, the Work Group can close this one.

CHAIR CLAWSON: Well, from what I have just heard from SC&A, we are in agreement that we can close this one, correct? But I leave it up to you.

DR. ANSPAUGH: Yes.

DR. MAURO: Sorry, the discussion we just had was that, Mark, the fact that you were able to do a calculation that showed doses were negligible based on those air samples, I am going to use a word that is not meant to be pejorative, the fact that you were able to go through that exercise and show that these doses were negligent, really has no play because it has already been agreed we are not even going to try to do that. Is that a fair statement?

MR. ROLFES: Right, I agree.

MR. KATZ: Yes, it is not pejorative.

DR. MAURO: Pejorative.

DR. MAKHIJANI: This is Arjun. Just for clarity, so we are talking about item 13 being resolved?

MR. KATZ: Yes.

DR. MAKHIJANI: Thank you.

MR. KATZ: Yes, thanks, Arjun. Right, we do want an updated matrix.

So, you have to ask your Work Group fellow Members for their concurrence.

CHAIR CLAWSON: Wanda, how do you feel?

MEMBER MUNN: I concur.

CHAIR CLAWSON: Okay, Phil?

MEMBER SCHOFIELD: I think that is settled.

CHAIR CLAWSON: Gen?

MEMBER ROESSLER: It sounds settled to me.

CHAIR CLAWSON: Okay. And I agree, too.

MR. KATZ: And then we have a petition for a comfort break. Some of you may want one as well, on the line. So, why don't we take a ten-minute break?

(Whereupon, the above-entitled matter went off the record at 10:37 a.m. and resumed at 10:49 a.m.)

MR. KATZ: Okay, we are all back here in the room and I think ready to go again, assuming

we have folks back on the line. Do we have Phil back and Gen?

MEMBER ROESSLER: I'm on. This is Gen.

MEMBER SCHOFIELD: I'm on. This is Phil.

MR. KATZ: Okey-doke.

So, Bob, is it your turn?

SC&A's Position on comment 8 -- Resuspension Issues at the Nevada Test Site

MR. BARTON: Okay, so we have just gone through essentially what was SC&A comments 1 through 7 on the resuspension issue, which Lynn did excellent work on. There is also comment 8, which was the subject of a separate White Paper response by NIOSH. And just to give a little back story on what that was, in addition to looking at sort of the technical aspects of how we derive these environmental intakes, we also ask the question, well let's go take a look at some actual claimant dose reconstructions and see what is happening

there as far as application of it. So, I am sure you all will be relieved that you can take your analytical hats off for a few moments.

And what we have found is discrepancy or -- inconsistency is a pretty harsh word because there are a couple of different, I guess you could call them methods that were being employed, one which is OTIB-18, which sort of a generic document, among many sites that employed an air sampling program to assign internal doses without actually constructing a coworker model, sort of an efficiency measure. So, we saw that that was actually being used a couple of times during the SEC period.

Also sometimes the environmental intakes were being applied. Sometimes there would be overlapping employment with Tonopah Test Range and in most cases, we saw that Nevada Test Site was being applied. Sometimes the TTR intakes were being applied. So we said you know this might be an opportunity to sort of shore up some of the

consistency because one of the great tenets of this program that we strive for is, to the extent we can, theoretically if a number of dose reconstructions were to look at the same case, they would all come to the same conclusions, within reason. I mean, obviously, every individual case will have its own nuances that will necessarily require some professional judgment but we felt that in some cases a more standardized procedure would be beneficial so that, again, let's say you had two claimants that were pretty much doing the same job, the same work history and are looking at their dose reconstruction report. Are they actually getting assigned the same thing within reason?

NIOSH responded to that and provided a really excellent discussion, which I think is beneficial not only to the Work Group, us and other interested parties such as the claimants themselves, about sort of the evolution of how doses get reconstructed and how necessarily they are not always going to be up to date with what the

TBD says. Just that is the nature of the beast. TBDs can't be revised. Every day you have a change.

But anyway, I don't have a presentation but I think just for everyone's benefit I am going to throw up our memo in response, which is on the website for everybody and just for ease of everyone seeing it here. So give me just one minute here.

Okay, can everybody see? It should be showing a PDF file, page two of SC&A's response memo. Is that up on the screen?

MR. ROLFES: Yes.

MR. BARTON: Yes, okay, great. So what we are looking at here is after the discussion that NIOSH, which really provides a lot of clarity on how the dose reconstruction methods sort of evolve over time, they did agree that, and I will read this into the record, "NIOSH agrees that, over the years, the lack of detailed instructions and the evolution of project- and NTS-specific guidance has resulted in inconsistencies in the

manner in which OTIB-18 and environmental intakes are assigned and the resultant doses evaluated." And this next part is definitely important, "However, these inconsistencies have not resulted in the discernible effect on case decisions." Essentially, the compensation.

But they did agree and they drafted up -- and I will scroll so everyone can see -- these eight sort of instructions. And I will back it up here just a little bit so you can see all eight.

And these are taken out of NIOSH's White Paper response. And as you can see, it really is kind of a step-by-step instruction for the dose reconstructor about how you apply these different things, such as OTIB-18 environmental intakes, when you have overlapping periods of employment at Tonopah and NTS, how you deal with all these different facets of it so that you kind of standardize the process, which is really what we were looking for.

And so we took a look at these essential

steps or punch list and all we have is we came up with five, I guess, comments or maybe suggestions, two of which are really sort of suggestions on how the TBD could be improved, if it were to be revised so that it is a little more clear to readers such as the claimants, who might ask similar questions that we ask when we do these reviews, or just other interested parties, or just for clarification to improve the document and the program as a whole.

So as we see these instructions, I am going to leave them up there because it is just easier to talk about them when everybody can see them.

Our first comment, and this is one of those that is sort of just a suggestion, but the first instruction says: Assign environmental intakes for all employees who were issued dosimetry at NTS between 1963 and 1992 and to all employees after 1993.

SC&A's comment on that was sort of for clarity and we already discussed sort of the

atmospheric period and that six months after testing and why that can't be reconstructed. And obviously there is the legality of it but there is also the technical reasons behind it, why that was chosen. And I think if you were reading this as a claimant, you might look at it and say well, why are we doing environmental intakes for that latter period? What about me? I work in the earlier period. And, obviously, for, I am sure, several valid reasons, it is just impossible during the atmospheric period to separate out source term that is purely environmental versus occupational. And so we completely understand why the decision was made not to try to tackle the environmental dose problem.

And our only suggestion is that maybe some text could be added to the environmental TBD to sort of flesh that out and explain why we are only talking about 63 to 92 and why we are not talking about the earlier period.

So, again, that is just a suggestion to

improve the TBD. I think it provides clarity, especially for outsiders who are reading this and they are wondering the same question.

So, again, that is just a suggestion, sort of an editorial suggestion on how the TBD could be improved. It doesn't have an effect on dose reconstruction.

Our second comment is related to instructions 2 and 3. And so instruction 2 is: Beginning in 1993, OTIB-18 can be applied in lieu of environmental intakes as an overestimating technique for cases that do not require a best estimate.

And number 3 is: Beginning in 1993, OTIB-18 intakes may be applied in lieu of evaluating claims that had bioassay results (in vivo and in vitro) that were less than the minimum level of detection or had relatively-low positive results reported.

It was a little confusing to me but I think I got it. So what we are saying is if it is

just a best estimate and you want to apply environmental intakes, you would use OTIB-18 to apply those environmental intakes. But I guess what I was confused about was it seems that OTIB-18 is being used to assign occupational intakes in that post-1992 period. So, I was a little confused about how does that work. I mean, would you still be assigning some sort of occupational intake, based on bioassay or would that just --

MR. ROLFES: Well, if a person has bioassay data and we want to provide an overestimate of that person's internal dose, if they had several non-detectible bioassay results, non-positive bioassay results, the application of OTIB-18 would be a claimant-favorable overestimate.

MR. BARTON: It was just confusing because it said, I guess, in lieu of environmental intake. So environmental intake would not -- well, we will get to that point.

Part of the comment there was that,

obviously, it is always preferable to construct a coworker model for any unmonitored doses but we looked into, specifically, the technical basis from EG&G from 1993, which was basically their document on showing how they are in compliance with 835 and they even ay in there that only about 2.5 percent of the NTS worker population was on any sort of routine bioassay schedule. So, it is a very, very small portion.

And we went in and look at 100 random claims and we found that only five of those had routine gamma whole body counts. So, it is pretty much in line with what was said in that 1993 document. So, we certainly find it maybe implausible to construct any sort of coworker model. But again, this is a recommendation to improve the TBD to say the reason that we are not considering a coworker model to specify unmonitored doses versus the OTIB-18 approach is that it is simply not feasible for reasons A, B, and C. So, again, that is --

DR. NETON: Well, I think that is a little different logic, though. What they are saying here is OTIB-18 -- if a worker were going to be assigned environmental dose, that decision would have been made up front. They were not a general worker in the area. They were environmentally exposed.

And OTIB-18, you are right, is an occupational assignment but it clearly would bound any environmental intake that would be assigned. So, it is an overestimating technique. It is an efficiency process almost.

MR. BARTON: Right. I guess my question is then for unmonitored workers because that was the impression I got from this. If you are an unmonitored worker, then you would get OTIB-18. Do I have that correct?

DR. NETON: Only if the case is not compensable. OTIB can only be applied for noncompensable cases.

MR. BARTON: Okay so if you were trying

to perform a best estimate on a case --

DR. NETON: You would assign environmental dose.

If you can assign an OTIB-18 intake, which is way above an environmental intake and it is still not compensable, it is not compensable. It is just quick that way, rather than go on through the details of environmental.

MR. BARTON: Okay. I guess I was thinking of this in terms of how you assign the occupational portion. My impression was --

DR. NETON: Well, if it is environmental intake, it is not an occupational. The person would -- it would already have been decided that he is not going to get an occupational dose.

MR. BARTON: Okay. Maybe this is just not applicable to the environmental TBD. But again, if you had an unmonitored worker who was considered a rad worker out at the site post-1992 --

DR. NETON: He wouldn't get an environmental intake.

MR. BARTON: He would be able -- OTIB-18, right?

DR. NETON: Well, OTIB-18 could be used to overestimate his dose.

MR. BARTON: Okay.

DR. NETON: OTIB-18 is an overestimating TBD -- TIB. You can say we don't know exactly what this guy's exposure was. Clearly, it was less than what was it, ten percent of the MPC or DAC or whatever it was at that time.

MR. BARTON: Well, post-1992 you have to assign occupational dose.

DR. NETON: If it is an overestimating technique, you just say it is less than ten percent. You got less than ten percent of the DAC after 1992 and if it is not compensable it is okay. If it is compensable under those conditions, you can't use OTIB-18. OTIB-18 can only be used as an efficiency process. It is not used to compensate cases.

MR. BARTON: Okay, I guess I am still confused. So, you have a case where you have a rad worker that is not monitored outside of the SEC period --

DR. NETON: He cannot be assigned -- he should not be assigned environmental dose. He would be assigned an occupational dose.

MR. BARTON: Which it is not a coworker dose, right? There is no coworker model.

DR. NETON: No. You would take his bioassay --

MR. BARTON: But unmonitored.

DR. NETON: All right, we are getting into the post-835 compliance era.

MR. BARTON: Yes.

DR. NETON: And we are probably going to take that up at the next Board meeting.

MR. BARTON: Okay.

DR. NETON: We have some approaches that we have developed to deal with this requirement and 835 is everybody that had

potentially received 100 millirem CEDE in a year is required to be monitored.

MR. BARTON: Okay.

DR. NETON: And if you can demonstrate that they met that compliance requirement, then by definition everybody that wasn't monitored receives less than 100 millirem CEDE.

MR. BARTON: Okay.

DR. NETON: And we are working ways to deal with that universally --

MR. BARTON: Okay.

DR. NETON: -- as long as you are in the 835 compliance era.

MR. BARTON: Okay. Yes, that one was really concerning, that post-835 period. That is why I had the question about it.

MEMBER MUNN: I have a question. Give me an example of an individual who is classified as a rad worker but is not badged.

MR. BARTON: We're talking internal dose, so it would be not bioassayed.

MEMBER MUNN: Okay.

DR. MAURO: This is John. Just to confirm my understanding, the reason OTIB-18 is bounding is that you actually compare the outcome of this ten percent DAC doses, which are too with the workers that were monitored and you found that they are grossly overestimated. In other words, for the places where you do have data, bioassay data and you can reconstruct the doses on that basis, those doses were always much lower than what you would have gotten if you assumed OTIB-18. That is what makes it bounding.

DR. NETON: That would necessarily be true, yes.

DR. MAURO: Yes, all I am trying to look for is you chose OTIB-18 because you felt it was a bounding analysis that can be used for the purpose of denial. And we know it is conservative because when you compare the results for places where you do have data, it is always conservative.

DR. NETON: Well, this item does refer

to Table 7-1 and -2 of ORAUT 2005 and I'm not sure exactly what that table is. But yes, that calculation would have had to have to have been done for us to use that approach.

DR. MAURO: Right. That is all I wanted to confirm.

MR. ROLLINS: John, Gene Rollins. I think you need to understand that when you compare OTIB-18 to when you have data, it is typically compared to negative data. We don't use OTIB-18 if we have significantly positive data.

DR. MAURO: Oh, no, and I understand that. No, I was just looking for the reason you believed OTIB-18 was always going to be bounding as a way of assigning a dose and the reason is when you look at the people where you do have data, it certainly demonstrates that it is a very high dose that you are assigning by using OTIB-18.

MR. ROLLINS: For instances where the results are negative or very strong, positive.

DR. MAURO: I guess I am a little bit

lost on that. I'm sorry. I didn't quite understand.

MR. ROLLINS: We can only use OTIB-18 if we have like a plethora of negative data or in certain instances and they are called out in OTIB and they give values, that the processor has to remain below those values to be able to apply OTIB-18 and assure that you are getting a conservative answer.

DR. MAURO: Okay.

MR. BARTON: Okay, well it sounds like the issue of post-'93 is still in the works. So, I guess my main comment there was that for unmonitored, and so this is not environmental intakes, I thought that the approach I was reading there of using OTIB-18, which may not be the truth anymore, I thought the TBD would benefit from a discussion of why a coworker model was not being used there. And again, it is for the benefit of the claimant or other interested parties looking at it and saying well other sites get coworker

models for unmonitored workers. What happened here is really the situation that you only have 2.5 percent of those population actually on a routine monitoring program. So that is a very small population.

DR. NETON: Oh, just to clarify John's question, I did look up Table 7-2 in TIB-18 and it definitely has values that were evaluated that would indicate that the TIB-18 is an overestimate against whole body counts and exclusion data. So, that was done as part of a TIB-18 exercise.

DR. MAURO: Thank you.

MR. BARTON: And again, that was meant to be sort of an editorial suggestion.

Alright, anyway, moving on to comment 3 and this has to do with instructions 4 and 8. And this is a situation where -- instruction is: When OTIB-0018 intakes are assigned, environmental intakes do not need to be assigned but may be assigned for claimant favorability.

So, that is the situation where the dose

reconstructor themselves is making that choice. Obviously, adding on the environmental intakes is going to be claimant-favorable. Not including them is probably still claimant-favorable but, again, it is one of those situations where is this an opportunity to take that decision out of the dose reconstructors' hands so that, again, if you have a bunch of people looking at the same dose reconstruction, we don't have one electing to assign environmental intakes and another one electing not to do it. Does this have significance from a compensation standpoint? No, because we are talking overestimate cases. But again, it is the issue of trying to make these dose reconstructions as consistent as possible so that if we were to look at them, compare cases, we see that the same decision is being made in the same situations.

And 8 is very similar to that and also it has very built-in claimant favorability that says if you have no external dosimeter, the

assumption should be that the EE is not on site and environmental takes should not be assigned; however, you can assign them for cases that do not require a best estimate.

So, those are two of the instructions that kind of leave it out there for the dose reconstructor to do it or not do it, based on probably the individual preferences of the dose reconstructor. And I think that is something that could be shored up.

It is not going to make much of a difference but, for example, if you are saying well you can apply environmental intakes if you want to but you don't have to, I mean you can always make it that if it is an underestimate you don't apply it; if it is a best estimate, you do whatever the best estimate methodology is and if it is an overestimate, you apply it. You know something standardized like that to take the decision out of the dose reconstructors' hands, even though it probably doesn't make any difference in

compensation because we are talking about non-best estimate cases. It is sort of an example of one of those steps that maybe we could standardize even more.

Our fourth comment had to do with instructions 5 and 7 and this was the whole employment overlap between Nevada Test Site and the Tonopah Test Range.

And I think this probably is just the way it is written but instruction 5 is: If the employment periods at TTR and NTS overlap for less than a year, we will apply the NTS intakes, which is claimant-favorable. They higher than the TTR intakes.

And then you get to 7 and it says: If there was only employment at the TTR and no overlap with NTS, then, obviously, we are going to go with TTR. We know they were there.

I guess my only question there was the whole notion of if there is an overlap of less than a year, we are going to assign NTS and, I assume,

although it is not written, if there is overlap of more than a year, NTS would also be assumed as the claimant-favorable assignment.

MR. ROLFES: NTS would always result in a higher internal dose than Tonopah.

MR. BARTON: Okay. I mean that one is easy.

The last comment is the only one that I think really might need some discussion. Let me see if I can find the spot in the report. Just bear with me, folks. I'm getting a little lag here.

Okay, in the section of NIOSH's response, and it's titled Path Forward for the Application of Ambient Environmental Intakes at NTS, it says that the environmental intakes should be applied as a constant because they are considered over estimates. And that is not new. That is right out of the TBD. It says that right in there.

What is new is the language in here that says if it is a best estimate, we are going to use

ten percent of the environmental intakes and assume its distribution with GSD of three. And that is the first time I had seen that before this White Paper exchange. And we didn't really understand the technical basis of the ten percent and we weren't sure if this is a new sort of programmatic approach. I know that in other situations, we often use a reduction factor for cases where you have job titles and exposure potentials. That is different. For example, sometimes you will have a coworker model where the rad worker will have 95 percent of the constant and non-rad workers or partial rad workers would get the 50th percentile in distribution.

Or I think there were situations, and John Mauro maybe you can remind me, in the old TBD-6000 methodology where the rad worker got 100 percent, the supervisor who was there part of the time got 50, and the secretaries and administrative who really didn't enter that ten percent.

So, there were other reduction factors

used but I have never seen it used based on the type of dose reconstruction.

MR. ROLLINS: Let me explain before we get too wrapped around and too technical because there is really not much technical involved here. This is Gene Rollins.

The way I came up with the ten percent was pretty simple. If you go back and look at the air concentrations, the maximums to the averages for all the areas, it works out mostly to be less than 20 percent. In other words, the average is less than 20 percent of the maximum. And in the case of plutonium, it is much higher than that, which is what we based everything on initially.

So, there is almost a factor of ten there to get from a maximum to an average. And as you know, I used the maximums to come up with these environmental intakes. That is why they are bounding.

Now, further, when you go to add in the other radionuclides, there was measurement ---

excuse me. So you look at the average, the maximum of the four concentrations and they average between 0.21 and 0.63, with an average of about 0.31. So there is another factor of three right there.

So, I felt like that just by assuming ten percent to be closer to an average than values that were in the OTIB for bounding was reasonable. And that is all the analysis I did.

MR. BARTON: Okay. Well, I am sure you understand my confusion because the original TBD had no such language in it. It was just we are going to assign these environmental intakes as a constant to the workforce. And now we are doing a ten percent reduction and it sounds like it is a reasonable, maybe not exactly quantitative reduction. It is just something I had never seen before and it certainly gave us pause because I mean essentially what you are saying is the real environmental intakes are the ten percent of those derived values in the TBD.

MR. ROLLINS: In my opinion, we would

be closer to the average values that we see out there instead of the bounding values that I included in my calculations.

MR. BARTON: I'm not sure I have any more to comment on that. Again, it gave me pause that the reduction factor, which what I am hearing is it is not a reduction factor, that is the true value that NIOSH believes should be used. So, it is not a reduction factor and it is not based on the type of dose reconstruction, per se. It is what you are saying is that what is in the TBD now is only for overestimates.

MR. ROLLINS: Correct. And it is stated as such, that it is a bounding estimate. And so this came about when we had to do a best estimate and we had some discussions about this and I went back and looked at the original data. And based on observation, what I just told you, the differences between the soil concentrations maxed then -- maxed average and air concentrations maxed average, the ten percent seems a reasonable

assumption to reduce those maximum values is why I came up with the best estimate.

MEMBER MUNN: More accurate data, more accurate assessment.

MR. ROLLINS: Excuse me, Gen. I couldn't understand you.

MEMBER MUNN: I just said it is a more accurate assessment.

MR. ROLLINS: I think it is more reasonable for a best estimate.

MEMBER MUNN: Yes.

MR. BARTON: Well, like I said, it was certainly new to us and I wanted to bring it to the Work Group's attention in case they had questions about it. I'm not sure if there is anything more that should be done about it. Maybe I guess a more analytical response, beyond what Gene just provided, to sort of justify the ten percent because I mean it kind of -- it might be a reasonable number. It is just we didn't know where it came from and, again, we had never seen a reduction

factor used for -- based on the type of dose reconstruction. But like you are saying, that is not what is happening. You are saying that were the TBD to be revised today, it would say assigned ten percent as a distribution for best estimates and then if it is not a best estimate, we are going to assign the original environmental intakes.

MR. ROLLINS: That would be my recommendation.

CHAIR CLAWSON: So, if I follow this right, the reason why we went down to the 90 is because the original was a best estimate. Correct?

MR. ROLLINS: The original was a bounding. What is in the TBD now is bounding.

CHAIR CLAWSON: Okay and you reduced it by ten percent why?

MR. ROLLINS: The justification for that was going back and looking at the raw data, the empirical data --

CHAIR CLAWSON: Okay.

MR. ROLLINS: -- and comparing the maximum values, which I used in my calculations to the average values.

CHAIR CLAWSON: Okay, now I understand.

MR. BARTON: The reduction of 90 percent. They are using ten percent of the value.

MR. ROLLINS: We got a reduction of 20 percent when you just look at the air concentrations and then you get another reduction of a factor of three when you look at the maximum to average soil concentrations when you enter the other radionuclides back in.

CHAIR CLAWSON: Okay.

MR. ROLLINS: We put those two together and I came up with ten, which I think is still too high but I think it is reasonable.

CHAIR CLAWSON: Okay.

DR. NETON: And you also have the GSD of three built in there, which does not apply to the bounding value.

MR. ROLLINS: That is correct.

DR. NETON: The standard deviation is three, which would put the 95th percentile somewhere around six times that.

CHAIR CLAWSON: Okay.

DR. NETON: So, when you are sampling it and the compensation is the 99th percentile, it is going to use more than just the ten percent. It is going to sample that distribution up to the 95th percentile around 60 percent, I think.

MR. BARTON: Okay. This is a site-specific approach. This isn't something that is programmatic.

MR. ROLLINS: Correct.

CHAIR CLAWSON: No, and this is something we found out. Each one of these sites are unique in their own aspects. And Nevada Test Site has been one of those sites. There is not that many places you go and blow stuff up.

MR. BARTON: Thankfully.

CHAIR CLAWSON: Yes.

MR. BARTON: Okay. Well, then the reason I brought it up is we hadn't seen it before and it kind of was kind of included in the response about consistency. So, we weren't sure where that came from and NIOSH has provided their explanation. I don't know if there is anything more the Work Group wants to say about it.

CHAIR CLAWSON: No, I am understanding why we got that. I was looking at it and I and I am just saying that with being everybody explaining to me, I now see the picture.

MR. BARTON: Well those were the five comments. And I guess in summary, and I kind of saved my summary paragraph, I think this is exactly the type of thing we were looking for where to standardize what we can standardize within reason so that, again, the goal is if you have a bunch of different dose reconstructors looking at the same case, they would all evaluate it the same way. So, I really appreciate the response here by NIOSH.

CHAIR CLAWSON: Well also, too, with

Ron and myself where we sit on the Dose Reconstruction Work Group, this is something that we deal with every time and that is getting it to where we can actually -- SC&A or whoever else could come in and take a dose reconstruction and have enough information in there to be able to understand and go through the process. And that is a continuing thing.

MEMBER MUNN: Without completely removing the discretion of appropriately trained professionals to do their job.

MR. BARTON: Right. There are certain things that just can't be dictated by a proceeding. I understand that.

MEMBER MUNN: Absolutely.

MR. BARTON: That is all I had on comment 8.

CHAIR CLAWSON: Okay.

MR. KATZ: So now we go to the rest of the matrix.

CHAIR CLAWSON: Did you have anything

else?

DR. ANSPAUGH: No, that was it for the issues that were before us related to resuspension or environmental/occupational dose reconstruction.

CHAIR CLAWSON: Okay. Okay, so I think we only closed one in the matrix, right?

MR. BARTON: There was a number of them that were kind of cascaded from the other issues.

CHAIR CLAWSON: That is what I was --

MR. BARTON: There is still some technical things to work out.

MR. KATZ: We closed a couple.

MEMBER MUNN: Yes, we definitely closed 13.

CHAIR CLAWSON: So where are we at on the number one issue, the Nevada Test Site matrix? Do we agree workers -- I'm trying to see if we have got a --

MR. KATZ: Are you working off of the SC&A matrix or both that and the NIOSH?

MR. ROLFES: There is three matrices out there.

There is an SC&A matrix that was recently sent. There is also the resuspension model matrix that had the eight comments. And then there is the original bigger matrix that is out there.

MEMBER MUNN: I appreciate Ted's suggestion that they be combined.

MR. ROLFES: Yes.

CHAIR CLAWSON: I have the one that is dated 1/29/2016. Do we have a --

MEMBER MUNN: I am working off Arjun's.

DR. ANSPAUGH: That is only related to resuspension. So, that is a small subset.

MEMBER MUNN: Yes, it is.

CHAIR CLAWSON: Okay, who wants to start working through the --

MEMBER MUNN: It seems to me that the one Arjun sent out on the 29th was, at least, recently up to date.

MR. KATZ: Yes, except it lacks the most recent upgrade.

MR. ROLFES: I don't think SC&A's matrix contains the response that NIOSH provided on May 15, 2015.

MR. KATZ: So you are going to have to work with both those matrices to go through this.

DR. MAKHIJANI: This is Arjun. Might I suggest that -- I guess we are now below item 7, after Bob and Lynn's discussion. And a number of items are closed. If we could go through them in order, ones that are closed. And I would suggest for the ones that are open, we should work from the NIOSH 2016.

MR. KATZ: Right, that sounds good.

DR. MAKHIJANI: So that way, we kind of go in order and also address the open issues.

MR. KATZ: Right. Arjun, that sounds like a good plan.

MEMBER MUNN: I had a hard time getting to the --

CHAIR CLAWSON: Okay, so you can take care of that, Arjun?

Review of Closed and Remaining Open Items

DR. MAKHIJANI: Sure, Brad.

So, starting with item 8, so what I had done is I went through the meeting transcript and the previous materials to come up with this. And of course, the Working Group should endorse, if they see fit, these comments.

So, item 8 about 1967 external dose data was closed and that was discussed in December of 2014. And I think NIOSH agreed with that.

DR. NETON: So which matrix are we working from?

MR. KATZ: Arjun, one second. So, we are working from Arjun's matrix but we will refer to the latest NIOSH responses for items that are not already closed.

MR. ROLFES: We started at 8 on the SC&A matrix, the memo there.

MR. KATZ: The full matrix, not the

resuspension matrix.

Okay, is everybody -- okay. Sorry, Arjun. Go ahead.

DR. MAKHIJANI: Yes. So, according to the December 2014 transcript, the issue was closed. We haven't formally closed it in the matrix. So, that is the reason for the comment. So, maybe the Work Group can indicate whether it should be formally closed.

MR. KATZ: So, Arjun, when you say that you closed it and if the Work Group closed it in 2014, then it is closed.

DR. MAKHIJANI: That's right. You are right. The Work Group did close it and that is the reason for the comment.

MR. KATZ: Yes, so it is closed.

DR. MAKHIJANI: Okay.

So, number 9 is similarly closed from the Work Group transcript and discussion.

So, number 10 is pending some action by NIOSH regarding pre-1963 external environmental

doses. I think we covered this earlier.

CHAIR CLAWSON: Wasn't that discussed today?

MR. KATZ: Yes.

MR. BARTON: Well this says it is external. I know we covered internal.

DR. MAKHIJANI: External environmental doses.

Yes, so as I read the transcript, and as I read, going to the NIOSH 2015 matrix update, that NIOSH is going to revise the TBD according to the discussion that was held in 2014, December.

MR. ROLFES: Yes, and our response indicates that we are going to add a missed dose into the coworker doses in the TBD when we revise the external dose TBD. I guess the coworker doses currently don't have missed dose incorporated into them.

DR. NETON: So this is environmental dose. Is it environmental doses?

MR. KATZ: External.

DR. NETON: It was the external environmental dose.

DR. MAKHIJANI: I think the missed dose should solve the problem, in my opinion. But it is for the Work Group to --

DR. NETON: Okay. Well then this one would essentially become an abeyance if the Work Group agreed to our path forward but we have to revise a TBD.

MR. KATZ: Right.

DR. NETON: So mark that one in abeyance.

MR. KATZ: Right. Yes, so that is a current response and the Work Group agrees.

CHAIR CLAWSON: We agreed to --

MR. KATZ: Well, you already did that --

CHAIR CLAWSON: I thought we did.

MR. KATZ: -- in '14.

MR. BARTON: You know is this really an environmental dose? I mean after 1957, everybody

was badged at the site, right?

MR. ROLFES: Right.

MR. BARTON: So we would really be looking at unbadged people who probably should have been badged.

MR. ROLFES: Right.

MR. BARTON: So that would be unmonitored occupational, really, right? I don't want to pick the nit here but I think that we were talking about occupational doses at this point, right?

MR. ROLFES: The original issue says external environmental dose but in our response we have indicated external dose.

DR. MAKHIJANI: Okay, and I think that is actually correct because it is the external dose TBD that needs to be fixed.

MR. BARTON: Right, we are talking about coworker models.

CHAIR CLAWSON: Okay.

MR. BARTON: I think it is just the

semantics of how it is written.

MR. KATZ: Alright. So, we'll close that one. The TBD is updated. Is that on schedule?

MR. ROLFES: Not that I am aware of, just because everything -- we hadn't received any kind of response.

MR. KATZ: Yes, of course. That wasn't a leading question. Okay, thanks.

Go ahead, Arjun.

DR. MAKHIJANI: Yes, so item 11 was open and NIOSH was to provide the basis for the beta/gamma ratio of 1.04. NIOSH yesterday, day before yesterday, sent a spreadsheet. I've only had the briefest chance to look at it. It seemed to change this ratio slightly and provide the basis for that.

MR. BARTON: Arjun, NIOSH has a response in their matrix which I think it would probably be appropriate to have them --

DR. MAKHIJANI: Yes, right. I agree.

We should go to NIOSH's matrix now, since this is an open item.

MR. ROLFES: Okay, I believe the reference that you are referring to, Arjun, was sent out about a year and a half ago, along with the updated issues matrix from May 15, 2015.

DR. MAKHIJANI: And you sent the spreadsheet recently, right?

MR. ROLFES: No.

DR. MAKHIJANI: Or am I mistaken about that?

MR. ROLFES: No, that was sent in 2015, May of 2015.

DR. MAKHIJANI: Oh, okay, sorry about that.

MR. ROLFES: Let's see. I will have to look back at our response here. Let's see. I can go through our response. It says the issues regarding correction factors for skin dose are addressed by using the beta/gamma methodology summarized in Section 6.4.2.1 and discussed in

detail in Attachment C of the Nevada Test Site occupational external dose TBD. We go on. I won't read the rest of that.

We had searched through several original files to derive a beta/gamma ratio of 1.04 to 1 originally and that original file could not be located. NTS data from 1966 to 1986 was reanalyzed using current EEOICPA data files and a value geometric mean of 1.16 with a GSD of 2.15 and a 95th percentile value of 4.09 was derived from the data.

The published value had a GSD of 2.41 and a 95th percentile value of 4.59. The change in the current value compared to the published values is due to the additional claim data available for analysis at this time.

Gene, did you want to add anything? I don't need to read everything that we have added here verbatim. Is there anything that you have to add or are there questions about the reanalysis of the beta/gamma ratios?

DR. MAKHIJANI: I had a question about the spreadsheet. This is Arjun. You have dates in the spreadsheet that said date of claim year. Is that -- I don't understand what claim year refers to.

MR. ROLFES: Claim year?

DR. MAKHIJANI: Yes, that is not the claim made under EEOICPA.

MR. BARTON: Yes, Arjun, it is.

DR. MAKHIJANI: It says 1966.

MR. BARTON: Right and then there is a claim number next to it, which I won't read out but --

DR. MAKHIJANI: Yes.

MR. BARTON: -- what seemed to have happened was --

DR. MAKHIJANI: What is the date? It can't be 1966 for the claim.

MR. ROLFES: That was the year that the dosimetry was recorded.

DR. MAKHIJANI: Oh, okay. Alright.

So, that clears that up.

I had a question, though. One of the items that we discussed in December 2014 was the significant difference between these ratios and the ratios that show up in the Hicks tables and the reason for the difference, which I didn't see discussed.

MR. ROLFES: The difference between recorded dosimetry data and Hicks data?

DR. MAKHIJANI: Yes.

MR. ROLFES: The dosimetry data, which are directly used for dose reconstruction, not calculated values.

DR. MAKHIJANI: Yes, I mean Hicks data are representative, I presume, of environmental dose, which is what this was about.

MR. BARTON: Yes, at the last meeting, if you go through the transcript, there was a pretty interesting conversation about it. Because if you look at the Hicks data, which was actually provided in an Appendix, I believe --

DR. MAKHIJANI: Yes.

MR. BARTON: -- the ratios were quite high compared to close to unity. I mean we are talking six, seven, eight. And what Hicks had come up with for beta/gamma ratios, and of course it is always better to use the actual empirical data than some sort of construct, but I think the Work Group is certainly curious why we would have such a large discrepancy between what he was predicting and what we were seeing based on the original analysis, which had a ratio of 1.04 and the newer one which is 1.16.

So, that was part of the discussion last time. And I think Arjun is correct. I don't think it really addresses it here. Like I said, it is always better to use empirical data.

I think we were curious why -- maybe it is just an academic issue but why would we see such large differences between those Hicks numbers which are in the TBD and just I don't think they are being used.

DR. MAKHIJANI: If I might just supplement Bob's comment -- this is Arjun -- is that these doses which are in the spreadsheet are the badge doses, ratios derived from badge doses, which are integrated for all kinds of exposure. And I think the Hicks tables are explicit for environmental dose, if I am understanding the situation correctly.

And for addressing environmental dose, the question is shouldn't that be applied. Aren't the Hicks tables relative?

DR. ANSPAUGH: Well, the Hicks tables don't directly have beta to gamma ratios. It would take some appreciable manipulation of Hicks to get ratios like that. And I guess it depends whether the refractories were added back in or not.

DR. MAKHIJANI: Well, Lynn, my understanding is that NIOSH did have them back in for the discussion that we have just had.

DR. ANSPAUGH: Well, yes, at least for the material we were discussing before. But in

terms of what the Hicks ratios may have been derived, like I say, it would take some manipulation. And I am not sure which version of the tables were manipulated.

MR. BARTON: I can say that the Hicks data that we are looking at is in Attachment C, page 118 of the NTS external TBD. And just looking at some of the values, I mean it can get as high as almost 60. And so that is why we are certainly curious as to why we would be seeing such large beta/photon ratios in these predictive tables admittedly and not in the actual badges.

MEMBER MUNN: I may be missing a salient point. Time dependency?

DR. MAURO: This is John. If I recall correctly, the Hicks tables give you picocuries or becquerels per meter squared, the numbers. And if you are looking to say let's take that value and convert that to what the field would be in terms of photon and beta, you are going to have -- it is going to be difficult to do because of the vertical

distribution of the radionuclides.

And Lynn, am I correct about that?

DR. ANSPAUGH: Well, the Hicks tables do assume some partial shielding by the soil, yes.

DR. MAURO: Oh, okay, so that is built in. My recollection of the Hicks tables is the mR per hour that you would have at a given point in time is what is measured and from there, you could go and figure out what the becquerels per meter squared is.

But you are saying that you could also get picocuries per gram as a vertical and, therefore, somehow get to what the ratio of beta to gamma might be.

I'm just trying to follow the logic sequence here of the relevance and applicability of the Hicks tables to getting this ratio. I am having a little trouble with that.

MR. ROLLINS: John, if I may make a comment. Gene Rollins here.

I think what we are seeing is layering.

I think some of the beta particles are being shielded by soil that has been deposited on top of it. But that is the only thing I can think of.

DR. MAURO: But that is where I am coming from.

By the way, I think there were -- are we working from the -- I think there was a matrix dated mid-2015 that we are working through because I don't see it on the screen.

MR. ROLFES: Correct.

MR. KATZ: That's right, John.

DR. MAURO: And is it something that cannot be put up on the screen? It would be a little easier to follow the discussion. Because I know I read through that and the description that we just heard of the rationale and the curves, the data, which was a spreadsheet, was in a couple of files that I looked at over the last day or two. And it is not here in front of me on the screen. Is there any reason why we can't put that up? It might be helpful to everyone if that was in front

of us.

MR. KATZ: There is no reason not to put it up. It is just that we will be bouncing between two matrices.

DR. MAURO: Oh, okay. I thought we were in the 2015.

MR. KATZ: Well, we are going through -- we are for this item. But in general, we are running through Arjun's matrix.

DR. MAURO: Oh, okay.

MR. KATZ: Where needed, we are going to this one. So, if Bob can handle it, that's fine.

DR. MAKHIJANI: This is Arjun. I am looking at the third revision of the external dose TBD in Appendix C that Bob Barton just talked about. And there is a Figure C-2 there, which shows the calculated beta/photon ratios for skin and then there are also immersion calculated beta/photon ratios.

And Lynn is right, these had to be calculated and the reference given here is 2006.

And all of these values, they are time dependent, I think as Wanda just said, if I recognized the voice correctly, but they don't necessarily go down with time.

The ratios for skin actually vary in the first hundred days quite a bit and then they seem to go up and then go down. So, it is quite a complicated time evolution.

And similarly -- yes, so there is nuclear rockets, too, but that is not in the present discussion.

MR. BARTON: A question. It appears that these are based on the annual, the ratios that have come off the dosimeters, they are annual. Are they annual average ratios?

MR. ROLFES: I believe it was an annual. Let's see. Yes, it was per year, by year.

MR. BARTON: And this is everybody who had a positive beta and gamma reading?

MR. ROLFES: Yes.

MR. BARTON: I imagine you couldn't do

it month by month if you have so many detection limit, you need positive results to actually get a ratio.

DR. MAKHIJANI: So could that be part of the explanation that maybe there are situations in which the beta dose is high but the gamma dose is below the detection limit and so when you take the badge data where both are positive, you get smaller ratios than calculated when all the radionuclides are taken into account on a time evolution basis?

MR. BARTON: That was one explanation. I think when we came out of that meeting, we were all kind of curious and are hoping to have a clearer explanation. I'm not sure we do have one. Good theories. I'm not sure.

DR. MAKHIJANI: Yes, obviously, what I am saying is just a theory.

MR. KATZ: What is the path forward here?

CHAIR CLAWSON: Yes, what is the path

forward?

DR. NETON: How large are these environmental doses that we are assigning?

MR. ROLFES: The environmental doses, I don't recall that there is any elevated external environmental doses being assigned. Gene, is that correct? Gene?

DR. NETON: He might be on mute.

MR. BARTON: Could this be a situation where environmental is not the right word again?

DR. NETON: That is what I am wondering.

MR. BARTON: Because I mean if you don't have a dosimeter then actually you are getting environmental dose.

MR. ROLFES: Right. Essentially, everybody entering the site is going to be badged for external dosimetry, to be assigned.

MR. BARTON: So are these beta/photon ratios, though, I mean are they applicable to the actual badge readings? Because I don't think

every badge had the open window element.

MR. ROLFES: I think the issue was prior to 1966, when they weren't reporting beta doses or only in certain situations, I believe.

And so what we did is analyze the 1966 through 1986 data, where individuals had positive recorded external dose for both photons and beta to develop a ratio that can be used prior to the time period when beta doses were reported.

MR. BARTON: And we looked at this data in-depth. Wasn't the analysis done to see if certain -- because we essentially end up with one number for that entire period and we are also looking at claimants. So, I don't know, we might have some claimants who have 40 comparative results and you might have some that only have one.

Again, we have, unfortunately, did not have a chance to really delve into this data set prior to this meeting. And I am wondering because that could be a way that it might be biased towards certain people doing certain jobs that had a lot

of data versus someone who only had maybe one data point because they were only employed in 1966 or something along those lines.

DR. NETON: Was it their annual beta/gamma?

MR. BARTON: Yes, annual.

DR. NETON: Is it 1.1 for every year or does it vary by year?

MR. ROLFES: It was done as a summary of all the data from 1966 through 1986.

DR. NETON: Okay.

MR. ROLFES: We developed a minimum/maximum and we have developed a distribution.

DR. NETON: And you assigned the distribution?

MR. ROLFES: Let's see. Yes, the published value had a GSD of 2.41 and a 95th percentile of 4.59.

DR. NETON: I guess my question is are we really talking about environmental dose here or

are we talking about occupational?

MR. ROLFES: This is essentially occupational external dose but it is under the common text of initially environmental.

DR. NETON: And I guess, Gene, are you still on the phone?

MEMBER MUNN: I think we may have lost Gene.

MR. ROLLINS: No, I'm here.

DR. NETON: I guess the question we had is how are we assigning -- what kind of environmental doses are we assigning in this early period before 1968?

MR. ROLFES: Yes, Gene, this is Mark.

MR. ROLLINS: Are you asking are we assigning beta dose to that?

DR. NETON: Well, environmental, yes.

MR. ROLLINS: I guess we would typically -- currently, we are not doing that, no, if there is strictly gamma doses that are being assigned.

DR. NETON: And how large are the gamma doses that were assigned?

MR. ROLLINS: I would have to go look it up but they are not huge.

DR. NETON: That's what I thought.

MR. ROLLINS: They are in the 25,000 to 30,000 millirem per year range.

DR. NETON: Alright.

MEMBER MUNN: And you are certainly not anticipating that beta doses will be higher.

DR. MAKHIJANI: Well, that is the debate. So, in the period where beta doses were not measured, as Mark was just indicating, if we are going to go with a beta/gamma ratio what should the dose assignment be. And I think Jim Neton is right that this is the overall assignment of a beta dose and not just environmental dose in a pre-1967 period.

MR. ROLLINS: That's correct. And as we pointed out before, the ratio that we are currently using of 1.04 was based on a series of

empirical measurements with film badges.

MR. BARTON: Which has changed, though, to 1.16 after it was reanalyzed.

I'm just looking at the data here and for annual doses, at least in 1966, most of them are in the hundreds of millirem, one at 1.9 rem. And I don't know if it goes down to the years subsequent.

DR. NETON: Those are occupational doses, though.

MR. BARTON: Yes. Yes, well I mean if everybody as a film badge entering the site, I'm not sure we have. I mean this is what we discussed before.

DR. NETON: Okay, well, that is --

(Simultaneous speaking.)

MEMBER MUNN: More than inclusive.

MR. ROLLINS: Because they are badged. We don't assign environmental or external because they are all badged.

DR. NETON: Right.

MR. BARTON: But prior to 1967, they didn't have the open window measurements.

MR. ROLLINS: That's correct.

MR. BARTON: So there is no measurement of air.

MR. ROLLINS: So we put out a beta/gamma ratio.

DR. NETON: Right. This is actually for all badged workers. I mean so I think we are okay.

MR. BARTON: Yes, if we change it to occupational.

DR. NETON: Yes, it is all badged workers, period. It seems they were all occupationally-measured doses.

DR. MAKHIJANI: Yes, I agree with Jim and Gene. So, the question is is the number calculated from badges that have both photon and beta measurements after 1967 the right number to use? Because the gamma doses in most badges were zero, right, as I understand the history? And so

we don't know what the beta doses in those situations would be, especially in the early months after a test. And so since we are talking about that specific period before 1967, I think some kind of idea as to why the Hicks tables numbers should not be used on the theory that there might be significant beta doses when the badge registered no gamma dose.

DR. MAURO: This is John. Just to get a little orientation here, the important point that we are talking about here is all of the worker -- we are talking skin dose. And since skin does is not covered, it is essential that we are trying to assign a dose to workers with skin doses during the covered period that would normally not be compensated for skin cancer.

So, getting it right regarding the dose to the skin from beta is going to be very important. And I think the essence of the discussion here is do we use the empirical data to assign skin dose that you have available to you, which might be

limited. I know you had that graph and spreadsheet that was very useful and you actually list all of the measurements by year of the kinds of data you have and you come up with your ratios and your 95th percentiles.

Or is there reason to believe that that curve or that data set is really not representative and you would get, of course, a substantially different result and, I believe, a much higher beta dose -- correct me if I am wrong -- if you went with the Hicks tables?

So what I am trying to do right now is just understand what the issue is we are struggling with and I think that -- I'm trying to characterize that so we are all thinking about it the same way. Did I describe our conversation appropriately, the context of what we are discussing and why we are discussing it?

MEMBER MUNN: I think so. I would like to know the magnitude of exposure that we are talking about. Are we talking about single digit

millirem?

DR. MAKHIJANI: Well, if Bob Barton is right from the numbers that he just quoted --

MR. BARTON: I'm going to throw it up right now.

DR. MAKHIJANI: -- we are talking about non-SEC cancers. This could be very important for skin cancer.

MR. BARTON: Okay, I don't know if everybody can see this spreadsheet up on the Skype meeting.

DR. MAURO: I have it in front of me.

MR. BARTON: Yes, so Column E is your gamma dose and Column D is your beta dose. And then you see it is just a simple ratio. And the maximum observed was a little over 18 in the empirical data.

DR. MAURO: Right now on my screen I have the matrix but not the spreadsheet.

MR. BARTON: Okay, one moment.

DR. MAKHIJANI: Do you want to put the spreadsheet up?

MR. KATZ: Yes, Bob is doing that.

DR. MAKHIJANI: It has the claim numbers on it.

DR. MAURO: Yes.

MR. KATZ: That's okay, Arjun. That's okay. This is just for us internally anyway, this screen.

DR. MAKHIJANI: Okay. Alright.

MR. ROLFES: In dose reconstruction, the first thing that we would use is the empirical data, rather than a calculated model. I mean the dosimetry is always the most important thing for us to consider, the very first step in a dose reconstruction.

DR. MAURO: Mark, this is John. I agree with you. I think the heart of the matter is that we do need to have confidence that the empirical data that you have tabulated here is complete, reliable, and representative. And, as a result, represents the best information available, as opposed to, if that is the case, then

defaulting to what I would consider to be a surrogate approach, which would be using the Hicks tables, which would require, as I understand it, quite an elaborate evaluation, given your starting point is picocuries per meter squared. And the issue that Gene mentioned earlier regarding the degree to which the data might be shielded or not shielded -- anyway, what I am getting to is that I would argue that the issue before us is do we believe this data set is an adequate set of data to build a coworker model for assigning beta dose based on observed gamma dose. I think that is what we are talking about here.

DR. MAKHIJANI: John, that is what we are talking about but it is not a question of measured data versus some hypothetical. It is a question of what does the measured data beta/gamma ratios represent when the vast majority of recorded gamma doses were zero or below detection limit.

DR. MAURO: Oh.

DR. MAKHIJANI: And so whatever the

beta dose might have been at the time is not included in this calculation.

And so, I think that is why we are -- if the majority of badges had positive gamma doses, then it would be a completely different situation but, if I remember correctly from past discussions, well over 90 percent and maybe over 95 percent of the badges recorded zero gamma dose. And so we have got the majority of badges that are not represented in this ratio calculation.

MEMBER MUNN: And it is being postulated that in that scenario you may have beta doses which are significant enough to affect dose reconstruction. Is that right?

DR. MAKHIJANI: It is simply we have very different numbers from the Hicks tables which are, after all, based on some very serious scientific work that was done at the time to analyze the testing with actual data from the weapons or the devices that were being tested. So, it is not an arbitrary set of numbers versus measurements.

And so the question is when most of the badges, the vast majority of the badges are not represented in this particular ratio calculation in the spreadsheet, is it preferable, especially when it would be more claimant favorable to use a number the Hicks tables compared to this particular ratio.

DR. MAURO: Well, Arjun, I would like to add a little to that. My recollection is that I did a lot of work in the Marshall Islands and exposure to people at Rongelap. And if I recall, the beta doses were very, very high relative to a gamma dose in the shorter time periods. I may have flubbed it, I don't know.

DR. MAKHIJANI: No, no, I think you are right. The Marshall Islands data do indicate a very high beta, Karl Morgan's measurements at Bikini indicated there indicated very high beta/gamma ratios, obviously, in the period immediately after the test. Sometimes, if memory serves me right, running into the hundreds.

DR. MAURO: Yes, and I remember using a factor of ten.

Now the reason why this is relevant, and I am just trying to help to get clarity in thinking about the problem, so if we have a data set for measurements made at NTS for gamma and a large fraction of them are zero, can an argument be made that there could be a substantial beta dose there that we are missing? In other words, is it appropriate? Is the presumption here that if the gamma dose is not detected, that it is likely that there is very little, if any, beta dose? And I am sure -- and it is not apparent to me that that is fairly the case.

DR. NETON: Well, I'm looking at the data in -- I don't see the whole spreadsheet but their 1966 doesn't seem to demonstrate that. We have in 1996 -- 1966, a large number of badges that had a lot of beta activity -- beta dose and no gamma dose. I don't think that is true.

What changed between 1965 and '66, '64,

'66?

MR. BARTON: Would it be helpful to the Work Group to see what the Hicks numbers actually are?

DR. NETON: I have seen those recently. They are high.

MR. BARTON: They are very high.

DR. NETON: I'm saying though in '66, the empirical data doesn't support John's argument that there is a substantial portion of beta dose and no gamma, associated gamma dose. At least in '66, that is not true.

DR. MAKHIJANI: How do you conclude that from this table, Jim?

DR. NETON: I'm just looking at -- well, given that the ratios are around one, I don't know how -- I don't see. Do we have a huge amount of beta with no gamma? That would be tremendously high beta/gamma ratios. And I don't see that. I don't have the whole table in front of me but that is my question.

DR. MAKHIJANI: Well, that is the question. That is obviously the question.

DR. NETON: Is that supported by the 1966 data? No.

MEMBER MUNN: Well --

DR. NETON: There is virtually no measurements there that don't have --

MR. BARTON: There wouldn't be any zeros in the --

DR. MAKHIJANI: Yes, this table does not include any measurements where the gamma was zero. That is the whole point I am trying to make.

MEMBER MUNN: And what I am asking is so how many of them are there, since we do not have -- we have significant numbers here.

DR. NETON: Well, yes, if you look at the ones that the gammas are zero, what are the betas? That is the question.

MEMBER MUNN: Exactly.

DR. MAKHIJANI: That is the question. And we know both from tables and from the Marshall

Islands that beta/gamma ratios are -- were often much, much higher than one. The analysis of an Appendix C of the TBD also shows the same results. And you know what was done by Eckermann in 2006. The data from Marshall Islands also show the same thing.

Now, it is obviously sketchy data but the question is, in that context, given the importance of skin cancer, what is the claimant-favorable approach to use? Is it this approach or the numbers derived from or maybe one ratio based on the Hicks tables shown in Appendix C?

DR. NETON: Well, I think one could actually look at the ones where the gamma was non-detectable and look at the betas and, first of all, determine how high those values were. If you have zero gamma and you have ten millirem beta or something to that effect, it is really a nonissue.

DR. MAKHIJANI: I mean it would be useful to see that.

DR. MAURO: I think the conversation we are having is very important. And I think we have to all be comfortable with the ratio that is eventually selected and the issues we are raising here because we are talking about people with skin cancer need to be assigned appropriate beta skin dose.

This is a very interesting issue. And when you think in terms of what the ratios are in other venues that we see, such as the Marshall Islands and in that period of the Hicks tables and then we are looking at your empirical data and we just want it all to ring true. And right now, it doesn't appear to all ring true.

DR. NETON: I'm just having trouble visualizing a scenario where the beta is huge and the measured photon is zero.

DR. MAKHIJANI: Well, what was the threshold of detection back then?

DR. NETON: It is what, 30 millirem, maybe.

MEMBER MUNN: I don't know.

DR. MAKHIJANI: It is pretty high. So, if you have a threshold of detection of 30 millirem and the beta/gamma ratio was ten, you are talking about omitting a pretty significant dose.

DR. NETON: Well, the detection limit for gamma back then is probably ten or so.

MEMBER MUNN: I would think so.

MR. SMITH: This is Matt Smith with the ORAU Team. Just to point out an addition that was made to Revision 3 of the external NTS TBD, in other words, a document that is on the street right now, language was added to Section 6.4.2.1 and that is page 52 out of 135 of the PDF file.

Language was added to I think address this issue. Again, I am trying to refresh my memory tape from several years ago. I guess it would be four years ago. But the language of that is in that section that is here, that a more precise estimate is required for beta to photon ratio. The values in Attachment C can be used.

So, in other words, yes, we have had this discussion before. And I believe that paragraph or that section language was added in to address what is being discussed right now.

MR. BARTON: When you say a more precise estimate --

MR. SMITH: Well, I think it is important to probably read these paragraphs on page 52 in context. The paragraph ahead of it, which obviously will get revised with the updated information on the empirical beta to gamma ratio, states that the regulations in 42 CFR allowed claims to be completed with using efficiency methods. So, to some degree, I believe, the approach was if we have claimant-specific information the dose reconstructor has the professional judgment option of using Attachment C. If we have a claim at hand SEC space and we don't have specific information that would let us know what tests or what activities that worker was doing, then you would use the ratio straight -- the

empirical ratio that we have been discussing, as Gene has pointed out, in order to derive an electron dose.

DR. MAKHIJANI: Well, I actually I have the TBD open in front of me. This is Arjun. And I am looking at the section that you have been citing. I actually don't understand how this is constructed. Because if Attachment C represents the precise estimate, why don't we use that as a first resort, rather than as a second resort?

I would have thought that the direction to the dose reconstructor would be to use the precise estimate first and then if it were, for some reason, not suitable, to resort to some other method as a second option.

MR. SMITH: Well, maybe Gene can jump in and add some background information. But to use Attachment C, I believe we need to know information about what specific tests they might have worked on or been associated with, and whether or not they were out in the field or not. We may have that

information and/or we may not have that information.

DR. MAURO: I think that is an important premise we are operating from, as I think about this and these ratios. We have workers out there where they are badged and we have measurements but we are making an assumption that the exposure scenario that this worker, whatever their values are, was because he was standing on contaminated soil from fallout. That is how the exposures occurred.

DR. MAKHIJANI: Or immersed in. Immersed in.

DR. MAURO: Yes, you are right about that. There is the immersion component also. And we are trying to reconcile what other experience has been with people standing on and I guess immersed in. I would have to go back and look at my experience in Rongelap to reconstruct it. If I recall correctly, it was standing. But nevertheless, nevertheless. All I am trying to

say is that we are operating from the premise that the ratios that we expect to see or are seeing is as a result of individuals outdoors standing in an environment where there is a deposit of radionuclides, some of which are resuspended and that is our scenario, when in fact is it possible that the new data also reflect workers that were doing other types of -- exposed in other types of scenarios where the Hicks tables and the fallout assumptions we are making here don't apply. Or are we really dealing with workers, yes, these are workers that were outdoors and the way that they were exposed is to to those fallout radionuclides that were in the soil?

DR. ANSPAUGH: If somebody is standing there in 1966, that material would have weathered into the soil and certainly the beta/gamma ratio would have decreased dramatically.

DR. MAURO: Yes, that is true, too. Yes.

MR. BARTON: I would like to ask you a

question. Are we planning to use beta/gamma ratio method going back into atmospheric testing?

MR. ROLFES: That is what the entire issue was for, I believe, because the beta not being recorded.

MR. BARTON: So it is not just '63 to '65. It is '66 all the way back and then it is applied to the coworker photon doses as well?

MR. ROLFES: It is applied all the way back, back to 1951 and within Attachment C here, we give the photon ratios at various times for listed tests here.

MR. BARTON: That is Appendix C?

MR. ROLFES: This is H-9 of Attachment C of the TBD and it is on page 121 of 135 of the NTS TBD. And I think it says here, too, if you can't discern what test the employee was involved with that you should use the average beta to photon values for the given test, if the EE isn't directly identified with a specific event.

MR. BARTON: Could the differences in

ratio be simply that a lot of these Hicks table ratios appear to being the results of atmospheric testing? Although they do calculate them out to like 50 years. So, let me just throw it out to the Work Group.

DR. MAURO: Well let me --

DR. NETON: Where are these doses calculated, one meter off the ground or at the ground surface? I mean it makes a big difference.

DR. MAURO: Yes. Well, just let me back up for one minute.

MR. BARTON: I didn't write the TBD.

DR. NETON: No, these are the Hicks table ratios.

MR. BARTON: Well, it is in your TBD.

DR. ANSPAUGH: These tables, they don't do anything with beta. The gamma is calculated at one meter above ground. But you have to remember the Hicks table assumed no decrease with time of the tritium in going in the depth. They only are designed to look at the decay of

radionuclides but not the distribution into the soil with time.

MR. ROLLINS: And this is Gene Rollins again. Something else I think we need to consider that when these weapons were detonated, a lot of non-contaminated dust was taken into the atmosphere and it would fall back down. So, weathering might be almost instantaneous.

DR. MAKHIJANI: Well, on the page that Mark just referred to, Table C-3, you have average beta photon ratios for someone was on-site during the year for particular tests. And for those cases where you have information for the worker as to what test they participated in, I don't see why these numbers can't be used in preference to calculated number that is from the spreadsheet, especially if they are more claimant-favorable and they are already in the TBD.

MR. BARTON: It's kind of a strange situation because it sounds like that the best estimate method is using the ratio of 1.16;

whereas, if you go to -- I'm sorry. For an overestimate you are going to use the 1.16 as an efficiency measure. And if you want a best estimate and more precise dose reconstruction, now you are going to go to Attachment C, which now you are getting up into, in some cases, double digits.

But even in Table C-3, I mean for a year after the test it is 2.2, 3.4, 3.3, you know numbers like that, with an average usually right around 2.0.

So it is just strange that usually for an overestimate you are throwing kind of a larger number at it and then if you want a best estimate, we kind of refine it down based on the data. But in this case we are saying you know if you want an overestimate or underestimate, we are going to use the empirical data and if we need a more refined estimate, we are going to use the appendix. It is almost --

CHAIR CLAWSON: Reversed.

MR. BARTON: -- backwards.

DR. MAKHIJANI: I do think that looking at the language in the section we were just looking at on page 50-something, the instruction is backwards in terms of the priority. It should be this particular thing.

Anyway, the information is available here. I don't see why this particular table shouldn't be used always. I mean, if you don't have data from a particular test, the various tests can be averaged, depending on some judgment about how long the worker was there, during which period, which you always know.

DR. MAURO: There is one more dimension to this I guess I am having a little trouble with. I am looking at the tables right now and this is airborne, submerged in a plume. And we are talking about the time period during above-ground testing. I'm getting myself oriented now.

And any individual that may have been exposed could have experienced some exposure from airborne activity, which would include beta and

gamma, but there is also the accumulated activity in the soil, which goes back to Hicks, which is a different situation. Are we trying to reconstruct the doses to the scan of workers for the entire time period of '51 to '92 from beta emitters from both exposure to an airborne activity and also exposure from deposited activity that might be contributing also?

If that is what we are doing, which would be a good thing to try to capture the whole thing if that is where we are, to give the benefit we can to the folks with skin cancer, then the data that you do have, if we went back to that curve, does that go back -- does that begin in the '50s -- I don't have it in front of me -- and goes all the way, as a function of time, what the reserved ratios are? And the question of zero --

What I am getting at is that I don't feel as if, and this might be because I have not been studying it the way Gene, and maybe Dennis has, and others, but I actually feel off balance right now

and that to understand if in fact we are giving, assigning to the workers during the above-ground testing and then subsequent above-ground testing the benefit of the doubt and giving them the plausible upper bound beta dose to the skin.

So, right now I am in a place where I can't say to myself that I think that that is being done here. I'm not. You know the conversation we are having I think is trying to get to that point but it seems to be pretty complicated.

DR. NETON: Well, John, I think we have beta/gamma numbers for all workers after '66. So all we are talking about here is they didn't measure beta prior to '66. So, we are talking about '51 to '65.

DR. MAURO: Okay.

DR. NETON: And we have photon measurements on those folks but we don't have beta measurements. So, it is trying to assign a beta dose to people who worked there prior to 1976 only.

DR. MAURO: Okay so that includes the

period of time where there was above-ground testing?

DR. NETON: Oh, yes.

DR. MAURO: And when there wasn't above-ground testing and when there was venting and when there wasn't venting.

DR. NETON: Correct.

DR. MAURO: So we have the full mix of scenarios and we are looking for a way to get a ratio that could be used for those workers, where do have some data on both beta and gamma. And that ratio that you observed for that person for that year would be very much a function of where he was and when he was wherever he was doing it, and whether he was exposed to the deposit activity, whether he was exposed to the submerging activity.

So, it seems to me we are in a circumstance where the data that we do have has to be fully understood as to what we have captured there or didn't capture and whether or not it is fair to say that this particular ratio would be

applied across the board. I assume with regard to the actual photon exposures, every worker is going to be assigned some photon exposure, whether he was measured or not. Am I understanding?

In other words, what you are claiming here is we can reconstruct the photon exposures to everyone and using this ratio, we can reconstruct the beta exposures to everyone and those doses will be assigned to everyone who is not compensated under the SEC. Is that what we are trying to do here?

DR. NETON: Yes, I think so.

DR. MAURO: Okay, good. So and we do have some limited amount, it sounds like a small percentage of the workers, were badged where we have photon exposures and where we also have photon and beta exposures. And somehow that data, as a function of year, and somehow that data --

PARTICIPANT: I can't hear.

DR. MAURO: Can you hear me now?

CHAIR CLAWSON: Yes.

DR. MAURO: Somehow that data, we feel is sufficient, where we can reconstruct doses with sufficient accuracy. I guess right now, from looking at the data and understanding these ratios, I have to say I am uncomfortable saying we have got a handle on this problem.

CHAIR CLAWSON: Well, John, let's talk a path forward then to be able to take care of that, then. What do you feel we need? If NIOSH has given us this, it looks like to me it is in SC&A's hands to evaluate this, make sure we know where we are going at.

You know I am going to be honest. This has been a lot of years and, as all of us have been going through the days, we are trying to remember how we got to where this point is at.

So, I agree that we need to come to resolution with this but it looks like to this part of it, for me, this is in SC&A's hands to do this evaluation. Am I correct?

DR. MAURO: Yes, we already went

through this in advance and closed it out but I don't recall.

MR. ROLLINS: No, no, we have never closed this out, John.

CHAIR CLAWSON: No, this hasn't been closed. This has been one that has been hanging on for quite a while.

MEMBER MUNN: But the thing that still bothers me -- there are a couple of things that bother me. In all the discussion, I still haven't had anybody give me an answer to my original question, which is, essentially, what is the magnitude of exposure that we are talking about as a possibility in the scenario that has been painted for us?

The other thing that I have a question about is whether or not Marshall Island data is particularly applicable in this particular case. The kind of exposure that I would expect from Islanders is not the same kind of exposure I would -- for beta is not the same kind of exposure that

I would expect from a worker on a Nevada Test Site. You know clothing alone would make a difference.

MR. STIVER: Wanda, this is John Stiver. I might be able to help out a little bit here. About ten plus years ago, when I was working at SAIC, we were dealing with this very same issue with dose at PPG and the Nevada Test Site and we used the Hicks tables to generate beta/gamma ratios for various geometries of the most prominent one applicable to the test participant that is kind of standing politically for a claimant, puts in a claim of one of his fission products and activation products and actinides. And I just pulled out from the Health Physics Society website the paper by Barss and Weitz in 2006, which I believe I had sent to Jim Neton about a year ago when we were kind of grappling with this idea of skin dose. And on page 385 you guys could probably -- I just sent it to Bob Barton. It is available on the Health Physics Society website and I pulled it down. And you want to know kind of what the beta/gamma ratios might

be for different distances from a flame source with different times after detonation. And Table 2 on page 385 we have got the Nevada Test Site is well based on the Hicks tables actually with the classified information included, the shot specific radiochemistry, although in this case the actinide is really not going to make much difference.

For times of detonation from half an hour to two years, distances from the flame source for bare skin exposures. That was just one table of many with distances from one to 200 centimeters.

And say just for taking 100 centimeters at one meter above the flame source at 12 hours post-detonation, we are looking at a beta/gamma ratio of 16.

And as you go our further in time, this is not including weathering and so forth, this is just the results on the flame, they go from 7.8 at half an hour up to 96 at two years. So, we are talking about pretty serious beta exposures.

MEMBER MUNN: But John, my concern is

the scenario that has been painted for us here is when we have zero gamma from which to begin our questioning.

MR. STIVER: Right. Zero, I don't know. You know I would assume for a dose reconstruction you would be looking like half the MDL, 20 millirem or whatever it might be. So, in that case, you might, in the worst-case scenario two years post-detonation, you would be looking at 100 times that.

I don't know how that would be done procedurally, but I know it would be a question for NIOSH. You know just to get an idea of what these beta/gamma ratios might be I know John brought up the PPG information and we talked about open and closed window dosimetry. But based on this work, he put a lot of blood, sweat, and tears into this report way back when and it might be worth us going back and taking a look at that in comparison to some of these other data sources. Maybe we could have a technical call or at the next meeting, get

together and talk about this in a little more detail.

DR. MAURO: And I agree, John.

And Wanda, I understand your question. Could we go back to that spreadsheet where the curve is showing the ratios that was compiled? Because there are measurements there, I think reported, of what type of gamma doses were observed, what the scale was. And we are talking about a very low one. We have some gamma doses -- it is hard for me to read. Any way to make it a little bigger? If everybody is in the same position I am, I'm having a little trouble looking at it.

MR. BARTON: Just get closer, John.

DR. MAURO: I can't get closer. I am about an inch away.

All right, so we are talking about -- oh, thank you. And the column that has the gamma is -- okay, here we go.

So, we are talking about -- these are millirems, I presume. We are talking about a few

hundred millirems, tens to hundreds of millirems that were observed in a given year. And I assume that the numbers we are looking at, these are individual claims. So, you can see the variability, which is good, the fact we had individual claims. We don't know their duration, where they were, what they were doing, that sort of thing. But we are seeing numbers that are not insignificant, if in fact the ratio for the beta/gamma, in some circumstances, could have been ten to one.

Now what we are saying is we are looking at say the very first line in 1966, we are seeing only a modest beta/gamma ratio.

MEMBER MUNN: There's Bob's 1.6.

DR. MAURO: Yes, it is very low. And to go back, so this is a real number. We don't know what that person was doing in 1966 that this is what he experienced. And in theory the argument could be made well, obviously, he wasn't standing on contaminated soil being exposed that way because

his beta dose would have been much higher, right, I mean in theory. So, he probably was doing something else. Is that possible?

MEMBER MUNN: I'm not arguing the source of the exposures. I'm not arguing any of those things. My point is this table that we are looking at, John, shows us an enormous range in the beta/gamma ratio and I'm not arguing that. What I am saying is this table does not show us zero gamma exposures. And that is what this discussion originated from is the fact that there are some people, I don't know how many people, but there are people who have badges that does not show a gamma ratio -- does not show a gamma exposure. And the argument here is how do you identify the beta exposure and what that was.

So, my question is, it is simple mathematics. If you multiply a zero by anything you get zero. So, if we are going to --

DR. MAURO: I hear what you are saying but I am assuming then you see zero, you are going

to assume that he as at one-half the MDL for his change out period.

MEMBER MUNN: Exactly.

DR. MAURO: And that will give you some dose. Let's say it is 10 or 20 per change out presuming the change out was monthly. In theory, the dose that you would assign to gamma for that worker would not be insignificant.

And then if you assume, you would have to assume, of course, some ratio to that to get the beta. So, we are not talking about small doses.

MEMBER MUNN: My first question was what is the magnitude. If it is not small, how large are we talking about? That is my question.

DR. MAURO: Let's try one out. Let's say we are at ten. We will go with the assignment of one-half the MDL being ten but in DCS it is probably closer to 20 and it is monthly change out. So, we are talking 200 millirem per year --

DR. MAKHIJANI: Per month.

DR. MAURO: The monthly change out and

you are not seeing anything and the MDL is 20. So over the course of the year, I am just multiplying by ten months, should be multiplied by 12, so it is 120 millirem, right?

DR. ANSPAUGH: No, 240.

(Simultaneous speaking.)

DR. MAKHIJANI: So there are two separate issues involved. One is if you -- the issue that I raised wasn't the question of what you would do to apply a beta/gamma ratio, even though in situations where the photon dose was zero in a dose reconstruction.

The issue that I raised was given that 95 plus percent or whatever the actual number is is very high in the 90 percents of recorded gamma doses are below MDL. So, zero was below the measurement limit, the measurement threshold.

What were the missed beta doses in those circumstances? And if those missed beta doses were representative of the high beta doses relative to gamma that are calculated and measures in

situations like the Marshall Islands, I wasn't referring to doses received by the Islanders, Wanda. I was referring to the measurements made in the working situations by the health physics section of the Marshall Islands tests.

So this has nothing to do with dose calculations for the Islanders. These were field measurements that were made of beta/gamma ratios at the time but I recall the ones made by Karl Morgan and published later.

So, we are talking about whether it is appropriate to use this approach, given that the vast majority of doses are recorded and gamma was zero. And if not, then secondarily the question arises so what is the appropriate way to assign a beta dose in situations where the recorded gamma dose is zero. And obviously, you could use LOD over two or something like that.

But whatever method you use, it would apply to whether you use a ratio of 1.16, or 10, or 5, or 50 or whatever. You would use exactly the

same thing. So, the method for assigning dose in a situation where the gamma dose is zero would be exactly the same, independently of whatever approach you use to calculate the ratio.

DR. MAURO: And I don't think it is insignificant. I mean we are talking rems per year.

DR. MAKHIJANI: Yes, we are talking rems.

DR. MAURO: It is rems per year.

MEMBER MUNN: Thank you.

DR. NETON: Right, my original point a while ago was if you looked at the badges in '66 that had zero dose, what is the magnitude of the measured beta dose.

DR. MAURO: Right, a couple of rem.

DR. NETON: Is it within range? So you are arguing it could be in the rem range but I am saying if you have a lot of badges in '66 that have zero recorded gamma and very low recorded beta, then I don't understand how you can assign 2 rem

doses.

DR. MAKHIJANI: Well, we haven't seen the recorded beta doses.

DR. NETON: Well that is why I say we need to look at that, Arjun.

DR. MAKHIJANI: Yes. So, I think there are two sort of questions for investigation. One is you know the values in Appendix C and the kind of values that John Stiver was just referring to. And whether that is the most claimant-favorable way or -- and the second thing is to look at the recorded beta doses in 1966 for those cases where the recorded gamma dose was zero.

DR. MAURO: I think this issue that we are discussing right now is one of the most important issues that we have been discussing, simply because we are talking probably about a very large number of workers that were working in that time period who may have developed skin cancer. So, this has the potential to have -- and I think we are talking about rems per year, even when the

doses to the gamma were low, relatively low, or below the MDL level. We are still talking now -- Jim, you probably have a feel for this but when you are talking about a rem or two or three per year to the skin to workers, now I don't know how many years they have been there, are we in a realm where we have the very real possibility of compensation?

DR. NETON: Oh, yes, for certain skin cancers.

DR. MAURO: Yes. So, this particular --

DR. NETON: Certain skin cancers are very sensitive.

DR. MAURO: So we have been talking about this for quite some time and my reaction is we have got to really nail this and everyone has got to be very comfortable with the methodology, with the ratios that are being applied, and interpreting the validity representativeness of the data that you do have and what it means.

So, yes, I feel very strongly. And I

don't get too excited about things but I think this is a big one and we have got to get really comfortable with this.

CHAIR CLAWSON: Okay. Well, that being said, let's talk about the path forward, then.

MR. KATZ: Right. Who wants to look at those empirical data?

DR. NETON: Well, I think we need to look at the data that had zero recorded gamma and look at how much -- the magnitude of beta to start with.

I also think it seems like we need to look at -- I am a little confused from what I have heard about how we are actually assigning beta dose in those periods, based on Table C-3 versus C-1 and versus the ratio. I'm not quite clear how that is working. So I think we need to begin looking at that.

MR. ROLFES: Yes, this has been -- this was put into the TBD years ago and we haven't

received kind of written comment on it. So, we have proposed this method, of which I would have to look back and review myself, but we haven't received any written comments on that revision. So, I don't know if that is something that the Advisory Board would like to task SC&A to do.

DR. NETON: You mean on this specific issue or the whole TBD?

MR. ROLFES: I guess that particular issue, that particular piece that was added.

DR. NETON: I think it is very clear to me what the comment is, at this point. I think we know.

DR. MAKHIJANI: Well, Jim, you know when we were looking at that section on page 50-something earlier in the TBD and then at Appendix C, I made the comment that the instruction seemed to be a little bit backward in the sense that if the precise estimate is from Table C-3, then that should be the method of choice, just leaving all other issues aside for the moment. And that if for

some reason Table C-3 cannot be used, then some other approach should be developed.

So, it would seem if NIOSH believes that Table C-3 is the precise method, and we generally know what tests people were in, I don't see why we should be resorting to the kind of ratios that are presented in that spreadsheet.

DR. NETON: Well I mean if C-3 is the sort of default if you know the values, then we don't have any argument here, right? I mean is there an argument that if we know where the guy was positioned and we can use C-3, those values appropriate, those ratios?

DR. MAKHIJANI: I don't think we have reviewed those ratios.

DR. NETON: But they are much higher than one, obviously, and they seem to be in the certain ballpark that we are talking.

DR. MAKHIJANI: Yes.

DR. NETON: But let's say for example if you agree with those ratios, then that is okay.

Then the second issue then is what do we do if we don't know where this person worked. And it is not clear to me what we are doing.

MR. ROLFES: Okay, there is averages in Table C-3 that can be used by year.

DR. NETON: So, I guess what is the usage for the 1.1 ratio, if we are not using it? I mean where are we using it?

MR. ROLFES: Gene, I am going to have ask you for help on that. When would the beta/gamma ratio of 1.1/1.0 come into play in comparison to Attachment C?

DR. NETON: Three.

MR. ROLLINS: Well, we are very -- I'm sorry, my battery is running down. Hang on just a second.

That is the third telephone I have been through.

MEMBER MUNN: You need a charger.

MR. ROLLINS: I have got chargers all over the house. I just to have keep moving phones

around.

Personally, I have never used Table C-3 and the reason being is that those ratios were not supported by the empirical data. And I understand there are genuine concerns about what that empirical data may actually represent, as opposed to earlier times. But I just always used the 1.04/1.16 when I applied the beta doses prior to the period when they were measured.

And in my experience, and I have done probably hundreds of NTS cases, I really haven't seen that many where it was applicable to do that. Either they weren't there during that time period or for whatever reason. Maybe it was a prostate and it didn't matter.

DR. NETON: Right. It was specifically skin cancer is the big issue here.

CHAIR CLAWSON: Nevada Test Site has got a lot of that.

DR. NETON: Yes, I think I would like that. NIOSH, we need to have an internal

discussion on our part a little bit on this before -- I'm not confused, I guess. I just need to know a little more about the background behind this. I haven't looked at this in a long time and we have got the John Stiver paper that you provided, which I don't recall getting but I'm sure he sent it to me.

We have got the disconnect between the theoretical calculations versus the empirical and I am concerned, I guess, about going back in time where there is active testing going on, applying '62 ratios where the testing has stopped. I mean I do have some concern about that.

So, I think we need to look at that a little closer.

CHAIR CLAWSON: Okay, so I believe that it is going to be in NIOSH's hands but I also want to make sure that SC&A provides you with what their issue is. I want to make sure that we are all good with the same issue.

DR. NETON: That is a good point, Brad.

I think maybe it would be good if SC&A would summarize the concerns they have or not. I mean we are not hitting a verbal target. We are hitting a written target.

CHAIR CLAWSON: And also, we are all onboard -- we are looking at the same thing for the same process. Because we have gone around the table here several times and we were going in different directions.

So, Arjun, I think that will basically come down to SC&A just to make sure that we have clarified what our issue is with it. NIOSH will have the action.

MR. KATZ: Right. Just a memo is fine, Arjun, summarizing the points of concern.

DR. MAKHIJANI: Yes, in that memo, if I might suggest, you know we have not reviewed the section in question of the external dose TBD nor the numbers in Appendix C. And I don't know what order you want to take them in, Brad, but at some point I think -- or if NIOSH is going to move from

that, maybe we ought to just wait until NIOSH gets back to us.

MR. KATZ: Yes, I think it makes sense to first sort out this first order of questions and then, if necessary, you can then review those actual values.

DR. MAKHIJANI: Sure, so we can lay out our position in a memorandum as to what we think the issues are and then NIOSH will have that in their reconsideration.

MR. KATZ: Yes, that sounds good.

DR. MAURO: This is John. I just want to point something out. What is going to happen here is I guess we will just go on to reiterate we are looking at arrays of numbers right now and we have raised a number of questions. So, in effect, the transcript of this meeting -- I think we have articulated all the different aspects to the data sets and why we have some questions and concerns.

MR. KATZ: Right.

DR. MAURO: So, I think it has already

been done. So, what I am hearing is you would like a memo from us to try to put into one place the conversation we had in some cogent way.

MR. KATZ: Yes and, John, you don't need to reiterate the conversation really. Just crystalize the bullets of concern, basically, that they can then address.

DR. NETON: I agree it is in the transcript but it is very hard to start citing transcript pages and stuff. That gets kind of messy.

MR. KATZ: Yes, so just a very brief synopsis of the concerns is good.

DR. MAURO: Yes, well I think we at SC&A need to collect our thoughts also. You will notice that this has unfolded in front of us as we spoke. And I think John Stiver brought to the table some important experience, as has Arjun and mine. So, to get our story together, it might not -- I would like to have an opportunity for us at SC&A to collaborate a little bit more and get our arguments

down a little clearer regarding why we have concerns. So, I would like to do a little bit of work on this.

MEMBER MUNN: We would like that, too.

MR. KATZ: That is fine, John, but you don't have to build a big mountain here because until they get to those initial questions, you don't know where you are. They may answer the problem.

DR. MAURO: Sure, okay.

MEMBER MUNN: In 500 words or less.

CHAIR CLAWSON: Don't put that on them.

DR. MAURO: We won't get carried away.

MEMBER MUNN: That's great. And John Stiver, I would really appreciate having a copy of your paper.

MR. BARTON: I can send you the one he was talking about.

MEMBER MUNN: Okay, that will be fine.

CHAIR CLAWSON: Okay, should we break for lunch?

MR. KATZ: I think so.

MR. STIVER: Wanda, I can definitely provide that to you.

MEMBER MUNN: Thanks, John.

MR. KATZ: Yes, thanks, John.

So, let's break for an hour. I guess you can't really get a lunch in less than an hour here, especially since I'm not sure we can drive anywhere with the snow coming down.

(Simultaneous speaking.)

MR. KATZ: Okay, so anyway it is 12:45 and we can reconvene at 1:45 for everybody. Okay, thanks and thanks for hanging in.

(Whereupon, the above-entitled matter went off the record at 12:45 p.m. and resumed at 1:47 p.m.)

MR. KATZ: Welcome back, everyone. I think we have our room assembled here. Let me check on the line. This is the Advisory Board of Radiation Worker Health NTS Work Group.

(Roll call.)

MR. KATZ: All right.

CHAIR CLAWSON: I guess back to Arjun and the matrix.

DR. MAKHIJANI: Yes, thank you, Brad. So we dealt with item 11 before.

MR. KATZ: Yes.

DR. MAKHIJANI: Item 12 is sort of open. We can go to the May 2015 version of NIOSH of the matrix and they proposed to revise TBD to take radon doses into account in this particular way. I think it looks okay to me but it is for the Work Group to discuss.

MR. BARTON: Arjun, I do -- Arjun, this is Bob. I do have one question which is kind of universal to this site. It kind of has the caveat in here that when records indicate the claimant made entries, how do we know when the claimant made entries into the Gravel Gertie?

MR. ROLFES: How do we know when a person made entry?

MR. BARTON: Yes.

MR. ROLFES: There probably were not very few, I mean with the exception of the Device Assembly Facility. There were some tests on the Gravel Gertie design. We would have to have some sort of indication that an individual had entered the Gravel Gertie and was involved in assembly or disassembly operations inside the Gertie.

MR. BARTON: Well this also involves the period after the SEC.

MR. ROLFES: Correct. Yes, the Device Assembly Facility would be after the SEC time period. So, we need some sort of information to tie that individual. If an individual identified that they entered the Gravel Gertie, then we would assign radon and thoron exposures.

MR. BARTON: So this would be kind of dependent on the additional records section of DOE files.

MR. ROLFES: We would have to have some sort of notification in the DOE files or in the telephone interview or in the initial claim of DOL

and giving the benefit of the doubt to someone if they were in a job category that would involve such an entry, then -- it really only comes down to the importance for lung cancer or respiratory tri-cancer claims. So, in those cases, for the benefit of the doubt we would probably assume, as an overestimating approach, that they did if they had a potential job that fit the bill.

MR. BARTON: So for instance say like a scientist or something like that and you were doing a best estimate, you would assume that they were? Because if you don't know who went in, it kind of begs the question.

MR. ROLFES: Right, you would have to have some details. But that is my initial thoughts. I don't know that that is written anywhere, though.

MR. BARTON: All right. I mean I just say this because it is kind of a universal concern in the program is when you start to try to place workers in specific areas, I mean it has gotten

dicey even in places where the dosimeter badge had an area code associated with it and I don't think they really did that in NTS. Maybe they did that in later years.

MR. ROLFES: And the other would be tunnel workers as well, if an individual was involved in tunnel work and kind of assembly work, we would assume that person could have been exposed to radon and thoron.

MR. BARTON: Okay, so there would be a sort of, I don't want to say cohort, but a group of job categories.

MR. ROLFES: If there is a person that says they were a miner, you know, obviously, they would have likely. But without a specific example, I think you would have to probably take a look into all the pieces of information that we get with the claim.

MEMBER MUNN: Well and then there is another item, too, certainly for unusual activities like Gravel Gerties, it would be highly

likely that any worker who is making a claim themselves would mention that in their CATI.

MR. BARTON: I agree with that when the CATI is with the worker.

MEMBER MUNN: Yes.

MR. BARTON: But there is a lot of claims that it is with the survivor and there are security concerns.

MEMBER MUNN: Absolutely.

MR. BARTON: So obviously, I mean I think we all want the CATI to always benefit the worker. I would be a little concerned if we were using a CATI with a survivor and it didn't say Gravel Gerties but they had a job title that might have entered them. But if the plan is to assign it to those job categories that could have gone in there, and I know it is pretty much a small group because there weren't that many people at the site going in there, that is reasonable. I think that might want to be defined a little bit as to what instructions there will be as far as -- and I know

it is case by case to a certain extent but this is another one where I think that the intent is to assign this radon dose, which I agree with the approach, it is just a question of how it is going to get implemented on a practical basis.

DR. MAKHIJANI: This is Arjun. I agree with Bob. It is kind of jogging my mind -- I have been away for a while -- the fact that this question has been a difficult one.

And so if the NIOSH could add the job categories and say "job categories such as" would be assigned this radon, that would be a good thing.

DR. NETON: So I guess this will be in abeyance.

MR. KATZ: Yes. It is probably a may and not a would because it probably depends on the totality of the information.

DR. NETON: Right.

DR. MAKHIJANI: So, NIOSH will propose some language for the Work Group to look at?

MR. KATZ: Yes.

DR. MAURO: This is John related to the matter. Do we have information -- I haven't looked at the Site Profile -- on the levels so like default concentrations of radon and its progeny? They are assumed inside Gravel Gerties and inside the puddles or wherever there might be this concern for elevated radon.

MR. BARTON: I thought we were using Pantex, right?

DR. MAURO: Whether you went in or not, if you didn't go in, what would be assumed?

MR. ROLLINS: This is Gene Rollins. We have those concentrations in the TBD.

DR. MAURO: It's there already. Okay, very good. Thank you.

DR. MAKHIJANI: So, do we move on to the next one, Brad?

MR. KATZ: Yes, Arjun.

CHAIR CLAWSON: Yes.

DR. MAKHIJANI: Okay. So the next one is about the environmental I-131 doses. I think

we discussed this earlier. I will live it up to Lynn Anspaugh to say if we have anything more to say here.

DR. ANSPAUGH: I don't think we have anything additional that we didn't discuss this morning.

DR. MAKHIJANI: Okay.

MR. BARTON: Yes, it was 13. We closed 13 this morning.

DR. MAKHIJANI: Number 14 is closed.

Number 15 was included in the discussion of 5 that we started the day with.

Lynn, did you want to say anything more about that?

DR. ANSPAUGH: No.

DR. MAKHIJANI: I think we covered fractionation earlier on.

DR. ANSPAUGH: Yes, we discussed that. We have a path forward, I think --

DR. MAKHIJANI: Right.

DR. ANSPAUGH: -- in order to resolve

it.

DR. MAKHIJANI: Right. So, it is open but it is part of something we already discussed.

Sixteen is closed.

Seventeen I think is -- let's see what the NIOSH response is. Yes, so this is covered as part of 5. I don't know, Lynn, if you wanted to say something about 17.

DR. ANSPAUGH: Well, I believe that 17 will be closed if we close number 5.

DR. MAKHIJANI: Yes, I believe so, too, but I am deferring to you.

MR. KATZ: All right, so it is open, still, until we finish up.

DR. MAKHIJANI: Yes, right.

Eighteen is closed.

Nineteen I think review of beta/gamma doses we discussed already, pre-1966 beta dose. This 19 and the earlier one should have been consolidated really. I think we have done so today.

So, I think we have a path forward on that.

Twenty is closed. I apologize for my error, which I said in the beginning at the time of the start, I said that was an error. It should be 20 should be marked closed.

Twenty-one is open and we should go to the NIOSH description and I give it over to Mark.

MR. ROLFES: All right. Let's see. This was about whether individuals had extremity dosimetry. And I will just read our response here. Let's see.

Well, I think our previous response is above. And following the Work Group meeting that was held in December of 2014, we had completed another review of claims that had been submitted since 2012, which identified 12 NTS employees, two Sandia Albuquerque employees and one Sandia Livermore employee that had skin cancer claims on the [identifying information redacted]. One of these individuals was a [identifying information

redacted]. The individual was -- well, I don't want to go into the details of his case here.

But let's see -- I don't know. I am going to be discussing specifics of one claim and it is probably not appropriate to discuss the one claim.

MR. BARTON: Your response is right on the meetings and people could read it.

MR. ROLFES: Right but not out loud for members of the public on the line.

MR. KATZ: You can mention things that just can't be put together on a person. But I mean like you can't talk about the years he was employed, stuff like that.

MR. ROLFES: Okay. It is giving him specific dosimetry results and I don't know -- I don't want to identify the individual but don't know exactly how far I should proceed.

MR. KATZ: I think, again, if you mention details that can't be -- that no one can pin to an individual, they are fine, the details

are fine.

MR. ROLFES: Okay. So anyway, this individual was never issued extremity badging at the NTS but during the late 1950s and '60s, he was issued wrist badges by Sandia Albuquerque.

While wearing the wrist badges over nine quarters, all of the wrist badges were below the detection limit. Over the same period, his whole body badges measured 40 millirem. His entire career -- I will leave out the dates -- he was assigned 70 millirem from Sandia Albuquerque, 25 millirem from NTS and 290 from Pacific Proving Ground. He was a -- what about a job title?

MR. KATZ: Don't. Don't go there.

MR. ROLFES: Okay. This case was compensable using the external dose from NTS and PPG and the individual had several skin cancers.

DR. MAURO: Hey, Mark, this is John. I see in the response, a little hard to read here because it is small, but I read it before but is there a standard procedure for when you encounter

these cases? In this case you had a particular case and you came up with a strategy, how to deal with that person, but is there guidance in general when you are dealing with an extremity? Unless this is the only case where you have to do with OSHA construction for extremities cancers and this is what you do and that is the end of the story. Or do you have the need for a more descriptive prescriptive approach for reconstructing doses to extremities for workers in general?

MR. ROLFES: Let's see here. I am looking back in our previous response. Let's see. Our previous response here is that we would evaluate extremity dosimetry to determine the appropriateness of the application of a glove box factor, such as dose to the prostate or gonads. And to determine claimant-favorable doses applicable to skin cancers appearing on the hands and forearms, in cases where extremity monitoring included other parts of the body, such as the head, an evaluation would be made to determine

appropriate adjustments necessary to cancers appearing above the shoulders.

When cases of device assembly workers require dose reconstruction, extremity doses will be evaluated for application to cancers appearing on the extremities.

DR. NETON: This is Jim. I don't think there is any generic model for dealing with extremities. It depends on the specific situation. I know like at Fernald, we had a ratio extremities, the whole body. But if you are standing in a uniform plane, extremity dose is equal to the whole body dose.

DR. MAURO: Yes.

DR. NETON: So, it really depends on the specific job or task.

DR. MAURO: And I agree that that is a reasonable approach, case by case. What is upsetting was -- and I guess is that already in the Site Profile? So, does it require any additional language or do you feel it is addressed

sufficiently or do you think we need to talk a little bit more about that?

MR. ROLFES: I would have to take a look at the context of the TBD but this was for one individual out of the thousands of claims that we had received. And I think we agreed that we would keep this in mind, in the event that an individual with a cancer on an extremity was referred to us for dose reconstruction by the Department of Labor. And as of the review that we had completed in 2012, we had identified those 12 employees.

But this is something also that if an individual indicates that they were issued extremity dosimeters for something, then we would certainly also look into that. I would have to look into that.

DR. MAURO: I understand that. I was only asking whether or not there needs to be some revisions or editing of the TBD. Because clearly, the TBD is going to require some revision as we go through this process. And this is one of the areas

that I am only asking is that whether or not there is an action that you think needs to be taken to clarify or expand upon whatever discussions are there already or do you feel that there is really no action item?

MR. ROLFES: Let me just search the TBD while we are sitting here and see if I can find anything about the discussion of the extremity dosimetry.

DR. MAKHIJANI: This is Arjun. I think I agree with what Jim Neton just said. I mean the ratio of whole body to extremity can be one or it can be very high. It could be an order of magnitude, depending on the job.

I remember the higher ratios arise when people are handling materials and maybe some guidance can be included in the TBD for typical cases, not kind of an actual instruction in terms of a number, but typical ratios that the dose reconstructor could consider and then apply in the specific instance, like the plane situation would

be one and handling uranium would be a completely different situation.

DR. MAURO: Is there a generic procedure on this? I mean well over 100 procedures.

DR. NETON: There are some specific geometry corrections procedures. I can't remember the exact numbers anymore but we have got one on machining uranium metal. We have got the glove box one that corrects for extremity dose. So, there is a couple of them out there but they are sort of generically written that we think could be referred to in this situation.

DR. MAKHIJANI: But Jim, would you agree that some kind of guidance would be appropriate that these ratios could vary a lot from one upwards and some reference to --

DR. NETON: Yes, I mean I am looking through the TBD, the TBD right now, as Mark is doing, and I don't see anything in here that speaks to that. I wouldn't be against adding some

language in there about reviewing the exposure-specific situation to assign extremity dose.

MR. BARTON: Would you end up using like a surrogate from another site?

DR. NETON: No, I don't think we put values in there. I think you could talk about these unique -- not unique but specific exposure geometries where extremity dose could be much higher than one, a ratio of whole body, that is, and deal with that in a case by case basis.

DR. MAURO: Yes, a little qualitative guidance.

DR. NETON: There is not much you can do quantitatively here, I don't think.

DR. MAURO: Yes, I agree.

MR. BARTON: I'm just wondering because we are talking about bomb assembly workers with that kind of activity that happened at Sandia or something like that.

DR. NETON: We ran into this extremity

issue at other facilities.

DR. MAKHIJANI: Have you run into it at ☺Pantex? How was it resolved over there?

DR. NETON: I don't recall, Arjun. Of course, Pantex is an SEC for its entire time period, too.

MR. ROLFES: There is discussion in the TBD. My search function was not working earlier. There is a discussion of the extremity monitoring conducted at NTS. It is section 6.3.2.3.

DR. NETON: Right. It really just talks about the type of monitoring that was done.

MR. ROLFES: Okay, page 34.

DR. NETON: Yes, I kind of saw that. But it doesn't really get into the -- if people were -- if extremity monitoring was conducted, I guess one could look at some of the values that we have if we have some extremity monitoring. But again, it is specific. You know a photographer walking around the site taking pictures is going to be different than a guy who is doing weapons assembly

work.

MR. BARTON: One other question I had and it might just how it is worded. Under the middle column that says status it says according to NIOSH there were no claims of device assembly workers involving extremity cancers as of 2007. And then the most recent one is a review of claims submitted since 2012. From 2008 to 2011, were those claims looked at as well or --

MR. ROLFES: The second review was done in 2012. Let's see. Maybe it was done after. It says a review of the claims submitted since 2012. So this was probably done after the Work Group meeting in December of 2014. So I would have to look to see what date that was done but there were additional claimants that fell into this category.

MR. BARTON: I'm just wondering because we looked before 2007 and then we looked 2012 on.

MR. ROLFES: Correct.

MR. BARTON: So there is another period

that might have another example in there to kind of help.

MR. ROLFES: Yes, there could be another claimant, since we are talking about 12 employees at this time or at the time that this analysis was completed.

MR. BARTON: And this is kind of -- I just happened to be looking at this claim last night. But one of the discussions that we had had previously is that a lot of the bomb assemblers were not necessarily just NTS employees --

MR. ROLFES: Correct.

MR. BARTON: -- but were actually coming from the sites.

MR. ROLFES: Assigned laboratories.

MR. BARTON: And that one case, and it was from the '80s, you did an extremity monitoring. He wasn't even a chemist but he was involved in the drill backs and doing their own testing. You know what the nuclear material was that happened in underground tests. And he had that extremity

monitoring and actually was associated with NTS, even though he was an employee of one of those other sites.

MR. ROLFES: Sure.

MR. BARTON: So, that is kind of a mitigating -- again, it is just by happenstance I came across it. It was actually in the list of claimants about how you find out if they were in the NRDS or NRDL or not for that study you did.

MR. ROLFES: Got you.

MR. KATZ: Okay, so NIOSH is going to propose some additional language, qualitative guidance. So, that is in abeyance.

Arjun?

DR. MAKHIJANI: Yes, so we are in more?

MR. KATZ: Yes, sure. That one is in abeyance.

DR. MAKHIJANI: So we were on 21.

MR. KATZ: Yes.

DR. MAKHIJANI: So 22 is still open and that was pending the resolution of Pantex, the same

question at Pantex on neutron/photon ratios. And so I will hand it over to Mark to update us.

MR. ROLFES: Yes, this is Mark. And we had previously proposed neutron to photon ratio for Pantex. However, as a result of many deliberations with the Work Group we ended up using the data that we had and made some adjustments to the recorded neutron doses during certain time periods. And we have developed, essentially, a coworker external dose approach for photons, neutrons, and electrons and have proposed to use that in lieu of the n/p ratios.

So, let's see if there is an individual that we believe that would fit the bill at NTS as being involved in a job where neutron dose would be possible and they were not monitored for neutrons, then we could assign a coworker neutron dose from Pantex.

MEMBER MUNN: So, if that's incorporated, we can close it, right?

DR. MAKHIJANI: Well before we had

discussed an approach of using an n/p ratio from Pantex but now the approach is being changed. So, I don't know how Brad wants to consider this.

Brad, you are also --

CHAIR CLAWSON: Well, if you remember, we were back there and the way NIOSH was looking at this and, Jim, correct me if I am wrong but we were looking at one that we could use throughout all the sites but come to find out that each one of the sites had their unique differences. So each site was then going to have its own ratio, if I am correct.

DR. NETON: Well, we found out we couldn't use a ratio at Pantex and, as Mark said, we ended up developing a distribution of monitored neutron doses and apply it over the years. And I am looking up here. The neutron doses were pretty small for Pantex. I can tell you that if I can find them.

MR. ROLFES: This approach has been approved. It is in the Pantex TBD as well. I just

wanted to add that, since we said that this was pending the approval of the Pantex external coworker model.

CHAIR CLAWSON: How are we applying this at NTS?

MR. ROLFES: If an individual was involved in a job category where they had an exposure or a potential exposure to neutrons, such as handling fissile material, for example, then if they had no dosimetry for neutrons, we would apply a coworker neutron dose based upon the Pantex.

DR. NETON: I can tell you the Pantex doses, the highest annual dose at Pantex, 50th percentile, is 43 millirem and it goes down from there. It is no higher -- it is 4 millirem in recent -- it is very small.

DR. MAKHIJANI: This approach seems a little strange to me. This is Arjun.

It is one thing to take an n/p ratio and apply it from Pantex to NTS. But the use of coworker model distribution from Pantex for a

different work population at NTS doesn't --

DR. NETON: Well, I would say --

MR. ROLFES: The work population isn't too different. The work is slightly different in that there are different types of devices handled but the work, the hands-on work is essentially using the same fissile materials at Pantex, as they are at NTS.

MR. BARTON: Can we argue that it fits the surrogate criteria? Because that is essentially what we are doing.

MR. ROLFES: I would think so. It is essentially an assembly worker.

DR. NETON: And these guys were probably doing this more full-time than the ones at NTS. Is that right?

MR. ROLFES: Oh, yes. Yes.

MEMBER MUNN: Oh, yes, absolutely.

DR. NETON: These guys were part-time production. And again, the highest total was 43 millirem.

MR. ROLFES: You are talking the majority of the work involving fissile material handling would be done at Pantex in configurations that would likely maximize the dose rates and doses that the employees received. NTS was an intermittent. There wasn't a lot of time spent directly handling fissile materials in advance of a test. You know these operations that were conducted at NTS would have lasted a short amount of time, not 40 hours a week, as would at Pantex.

DR. MAKHIJANI: I would agree.

MR. BARTON: And they might be from Pantex.

MR. ROLFES: Right.

CHAIR CLAWSON: And this is what we got into is that most of the people that would fit this bill were NTS or from other sites, Sandia, Livermore, Pantex, whatever else. People were actually doing it and most of them had their own badges in their facilities, too.

So this was kind of the uniqueness of

what NTS was. So, I guess you know Arjun, I really don't see anybody that would really fit this but I think that they have taken the appropriate steps to be able to take care of it. But unless there is a real big outstanding issue --

DR. MAKHIJANI: Well, Brad, I think I understand Jim and Mark's point, now that they have explained it and I have no problem in proceeding in the way they have proposed.

CHAIR CLAWSON: Okay, so we could close this one or put it in abeyance?

DR. MAKHIJANI: Well, there would be some pending change to the TBD.

CHAIR CLAWSON: Okay, so this will be in abeyance.

DR. MAKHIJANI: Right.

MR. KATZ: Mark, did you say this is already in the TBD or not?

MR. ROLFES: The Pantex TBD has been approved.

MR. KATZ: Oh, the Pantex TBD.

MR. ROLFES: But the NTS TBD, I don't believe has been updated to include a statement to use the Pantex external dose coworker model.

MR. KATZ: Okay, good. Thanks.

DR. MAKHIJANI: Okay, can we move on, Brad?

CHAIR CLAWSON: Yes, go ahead.

DR. MAKHIJANI: Okay, 23 we have discussed, unless Lynn has something more to say.

DR. ANSPAUGH: No, I think our conclusion has been what the soil data are and we felt that they were adequate for the purpose for which they have been used.

DR. MAKHIJANI: Right.

DR. ANSPAUGH: So I think this one, in particular, probably can be closed.

DR. MAKHIJANI: Okay, so it will be closed in the mix of things to resolve number 5.

The next issue -- sorry.

MR. KATZ: No, no. I'm just trying to understand whether -- if that is something that

there is nothing more to do with, then we can actually close it.

DR. ANSPAUGH: I think this one could be closed.

DR. MAKHIJANI: Okay.

MR. KATZ: Then let's do that, if the Work Group Members are in concurrence.

CHAIR CLAWSON: Phil, do you have any problems with it?

MEMBER SCHOFIELD: I don't have any comments. I agree, let's just close it.

CHAIR CLAWSON: Okay, Gen?

MEMBER ROESSLER: Am I off mute?

MR. KATZ: Yes.

CHAIR CLAWSON: You are off mute now.

MEMBER ROESSLER: I agree, let's close it.

MR. KATZ: Okay.

CHAIR CLAWSON: Alright, Wanda?

MEMBER MUNN: Oh, yes.

CHAIR CLAWSON: Great.

MEMBER MUNN: Yes, I was ready this morning.

CHAIR CLAWSON: You shouldn't be so overzealous on stuff there.

MR. KATZ: Okay, thanks, Arjun.

DR. MAKHIJANI: Yes, okay, number 24 is closed. Number 25 was transferred sometime back to the Worker Outreach Group, so no longer discussed in this Work Group.

And Number 26, the grab bag of things originally in 2005 in the TBD review we raised quite a few issues around waste handling and related activities. And so I will hand this over. This is kind of a lot of it is post-1992 but I will hand it over to Mark to have a response. It's complicated.

MR. ROLFES: The concern was about the monitoring practices, primarily for individuals under 10 CFR 835 in the more modern era after 1992.

We have put a significant amount of information into the TBD that describes the

monitoring practices and can see the response is pretty lengthy here. I don't know that you want me to read this monotonously.

Are there specific questions or is there anything that sticks out that you have questions about what we have added to the TBD?

DR. MAKHIJANI: No, I just want to point out to Brad that this is a significant change in the TBD and that we have not reviewed it and I don't know whether you wanted it reviewed.

CHAIR CLAWSON: Well, yes, I did. But I thought yes, that was part of the thing that we wanted you to take a look at because we have had a lot of changes to the TBD and I just wanted to make sure that we agreed to how it finally got in there.

DR. MAKHIJANI: Okay.

CHAIR CLAWSON: So, I guess that will be in abeyance.

MR. KATZ: No, that one is just SC&A needs to review it. That's it.

CHAIR CLAWSON: Right.

DR. MAKHIJANI: So are you tasking us to do that?

MR. KATZ: Yes.

CHAIR CLAWSON: Yes.

DR. MAKHIJANI: All right.

MR. BARTON: I feel a little bit ignorant on this. Can someone tell me what an orphan source is?

MR. ROLFES: A lost source.

MR. BARTON: A lost source?

MR. ROLFES: Yes, a source that industry have -- an industrial facility uses a source and loses it and it is discovered. You know it is sent for disposal.

MR. BARTON: Okay.

DR. MAKHIJANI: Not in its proper home.

MR. BARTON: That's the end.

DR. MAKHIJANI: Yes, that is the end.

CHAIR CLAWSON: Okay. So do we want to go over what --

MR. KATZ: Do we really want to go over it?

CHAIR CLAWSON: -- the TIB as well? We had better make sure that everybody understands or -- well, just to make sure that everybody is in agreement.

MEMBER MUNN: I have one question before we start that and that is on number 25. Do we know if anything at all is going on with the Work Group?

MR. KATZ: No, nothing is going on with that Work Group. And moreover, this is really a little odd to transfer it even to another Work Group. I didn't go back and look at the transcript to see what had been said at the time about why this would be transferred.

MEMBER MUNN: But you know since December 2014, I haven't heard anything about it.

MR. KATZ: No. Well, the Worker Outreach Work Group is not going to meet over this little thing here, anyway.

MEMBER MUNN: I didn't think so.

MR. KATZ: But Arjun, do you want to talk more about that item 25?

DR. MAKHIJANI: Well you know this dates back to quite a long time ago when NIOSH and us and the Work Group and the Board had extensive discussions about how worker outreach should be approached and specifically on documentation of interviews and things like that.

And since that time, as noted here and as you all know, NIOSH has changed its documentation approach and a lot has happened and there is much more documentation now.

I mean I, if you want to close it here, that would be okay with me. I just don't know if it is not being handled in the Worker Outreach and there is no intent to do it, we could close it. It doesn't matter to me.

MR. KATZ: Yes I think, Brad and company, this is -- I mean the only other thing specific that I recall from what you brought up

there was about whether the appropriateness or the completeness of the interview conducted. But that is water under the bridge. And SC&A and NIOSH do these interviews together these days and have been for the past eight years or nine years. So it is really I think the whole matter can just be closed.

DR. NETON: Yes, and the issue is now it is an SEC for the entire period. It is not necessarily relevant.

MR. KATZ: Yes.

DR. NETON: I mean as important as it might have been if the interviews were used to establish dose reconstruction for presumptive cancers.

MEMBER MUNN: Yes, I think -- can't we just say that NIOSH is --

CHAIR CLAWSON: I think we ought close this. Just close it. A lot of things have changed since --

MR. ROLFES: Our notes say that the matter was closed in the December 2014 meeting.

MR. KATZ: Oh, okay.

DR. MAKHIJANI: No, no, no. That is a little bit of misleading. If I am remembering the transcript of the meeting, it was closed for the purposes of this Work Group because it was transferred.

MR. KATZ: I see.

DR. MAKHIJANI: But what we are discussing now is that it should be closed, period.

MR. KATZ: Okay. Okay well, we probably could have closed it then but --

MR. BARTON: We basically had the same conversation back in 2014.

MR. KATZ: Yes. So let's consider it closed now.

MEMBER MUNN: NIOSH has changed its documentation process and it is now closed.

DR. MAKHIJANI: Okay.

CHAIR CLAWSON: It will state in this transcript that it is closed.

I don't know about anybody else but I

go back and read those transcripts and I think to myself, wow, do I really sound that stupid?

You know, you stop in the middle of sentences and stuff. Oh, my God. So you guys do a great job on transcribing this, maybe help us a little bit.

MEMBER MUNN: This is a rhetorical question, right?

CHAIR CLAWSON: Yes. What can we say? Okay, I guess it is all yours, Ted.

Wrap-up and Adjourn

MR. KATZ: That's all. We are all finished, I think. And we can't schedule another meeting until we have a new TBD and/or the follow-up when Lynn has his discussions and so on.

So, I think that is all we have. Oh, you want me to run through all we have.

CHAIR CLAWSON: Yes.

MR. KATZ: The only problem is that part of my -- I lost my internet connectivity. So, I have two -- so like the early part, I don't have

those items because they are online but they are not on my current document.

CHAIR CLAWSON: Maybe after you get back, maybe you could just send out to everybody and we could just --

MR. KATZ: I would be happy to do it. I will send out a very brief thumbnail on what the action items were and who has them. So, I will do that when I --

MR. BARTON: I have some notes but I don't know how complete they are.

MR. KATZ: Sorry?

MR. BARTON: I have some notes but I don't know how complete they are.

MR. KATZ: Yes, so I will send it to the Group and people can correct or add to it as necessary.

Okay and thanks, everyone, for all the work and diligence, attention and good luck with your weather in various places.

(Whereupon, the above-entitled matter

went off the record at 2:27 p.m.)