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

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ADVISORY BOARD ON RADIATION AND
WORKER HEALTH

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WORK GROUP ON MOUND

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TUESDAY
JANUARY 5, 2010

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The Work Group convened in the Zurich Room of the Cincinnati Airport Marriott, 2395 Progress Drive, Hebron, Kentucky, at 9:30 a.m., Josie Beach, Chair, presiding.

MEMBERS PRESENT:

JOSIE BEACH, Chair
BRADLEY P. CLAWSON, Member
ROBERT W. PRESLEY, Member
PHILLIP SCHOFIELD, Member
PAUL L. ZIEMER, Member

ALSO PRESENT:

TED KATZ, Designated Federal Official
NANCY ADAMS, NIOSH Contractor*
ISAF AL-NABULSI, DOE*
BOB ANIGSTEIN, SC&A*
TERRIE BARRIE, ANWAG*
BOB BISTLINE, SC&A
RON BUCHANAN, SC&A
MEL CHEW, ORAU Team
JOE FITZGERALD, SC&A
STU HINNEFELD, OCAS
EMILY HOWELL, HHS
KARIN JESSEN, ORAU Team
JENNY LIN, HHS
JOYCE LIPSZTEIN, SC&A
ARJUN MAKHIJANI, SC&A
DICK MADDING, Mound worker
JOHN MAURO, SC&A
ROBERT MORRIS, OCAS*
JIM NETON, OCAS
KATHY ROBERTSON-DeMERS, SC&A
WARREN SHEEHAN, Mound worker
DON STEWART, ORAU Team
BRANT ULSH, OCAS
LEW WADE, OCAS*

*Present via telephone

C-O-N-T-E-N-T-S

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

(9:30 a.m.)

MR. KATZ: Good morning, everyone in the room and on the phone. This is the Advisory Board on Radiation and Worker Health. This is the Mound Working Group and we are about to get started.

As usual we begin with roll call, and as I do roll call, please address, for all of the government-related people, address whether you have a conflict of interest as well.

So beginning with Board members in the room.

CHAIR BEACH: Josie Beach, Mound Chair, no conflicts.

MEMBER CLAWSON: Brad Clawson, Work Group member, no conflicts.

MEMBER ZIEMER: Paul Ziemer, Work Group member, no conflicts.

MEMBER SCHOFIELD: Phil Schofield, Work Group member, no conflict.

MEMBER PRESLEY: Robert Presley, Work Group member, no conflict.

MR. KATZ: And let me check on the lines. Are there any Board members attending on the line?

(No audible response.)

MR. KATZ: Okay. Then in the room next: the NIOSH ORAU team.

MR. HINNEFELD: Stu Hinnefeld, Interim Director --

MR. KATZ: No conflict?

MR. HINNEFELD: -- of OCAS, and yes, I have no conflict.

DR. NETON: Jim Neton, OCAS, no conflict at Mound.

DR. ULSH: Brant Ulsh of OCAS, no conflict at Mound.

MR. KATZ: And on the line, NIOSH ORAU team?

MR. MORRIS: Robert Morris, Oak Ridge Associated Universities Team, no conflict.

MR. HINNEFELD: There are other ORAU Team members.

MR. KATZ: Oh, I'm sorry. I said in the room. Sorry.

MR. STEWART: Don Stewart, ORAU Team, no conflict with Mound.

MS. JESSEN: Karin Jessen, ORAU Team, no conflict with Mound.

MR. CHEW: Mel Chew, ORAU Team, no conflict with Mound.

MR. KATZ: Okay. Any other NIOSH or ORAU Team on the line?

(No audible response.)

MR. KATZ: Okay. SC&A in the room?

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

DR. BISTLINE: Bob Bistline, SC&A, no conflict.

MR. FITZGERALD: Joe Fitzgerald, SC&A, no conflict.

DR. BUCHANAN: Ron Buchanan, SC&A, no conflict.

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

MS. ROBERTSON-DeMERS: Kathy Robertson-DeMers, conflicted.

MR. KATZ: And on the telephone, any SC&A members?

DR. ANIGSTEIN: Bob Anigstein, SC&A, no conflict.

MR. KATZ: Okay, and then we have HHS or other government employees or contractors in the room.

MS. HOWELL: Emily Howell, HHS.

MS. LIN: Jenny Lin, HHS.

MR. KATZ: And on the line: HHS, other government employees or government contractors?

DR. WADE: Lew Wade, a contractor to NIOSH.

MS. ADAMS: Nancy Adams, a contractor with NIOSH.

MR. KATZ: That was Lew Wade, by the way, the first one.

MS. AL-NABULSI: Isaf Al-Nabulsi, DOE, no conflict.

MR. KATZ: Okay, and then there are no members of the public in the room, but how about on the line? Any petitioners or other members of the public or staff of congressional offices who want to identify themselves?

MS. BARRIE: This is Terrie Barrie with ANWAG.

MR. KATZ: Welcome, Terrie.

MS. BARRIE: Good morning.

MR. KATZ: You did get the agenda, right, from me?

MS. BARRIE: No, the attachments didn't come through.

MR. KATZ: Let me try that again.

MS. BARRIE: Okay. Thanks.

MR. KATZ: Okay. Other members of the public?

MR. SHEEHAN: Can you hear me?

MR. KATZ: No, but now we can,

yes.

MR. SHEEHAN: You can hear me?

MR. KATZ: Yes.

MR. SHEEHAN: Warren Sheehan, Dick Madding. We're on the way. We should be there in about 20 minutes.

MR. KATZ: Okay. Warren, Sam and Dick Manning, is that what you said?

MR. SHEEHAN: Yes.

MR. KATZ: Warren Sheehan and Dick Madding.

MR. SHEEHAN: And we are ex-Mound employees. I guess that means we're conflicted.

MR. KATZ: Right. You're not conflicted because you're not in the same category.

Okay. That's it then. Josie, it's yours.

Let me just say to everyone on the line, then, please mute your phones. If you don't have a mute button, use *6, and when you want to address the group, use *6 to come back off mute, and please do not put the call on hold. Just disconnect and call back in if you need to leave at some point.

Thank you.

MR. SHEEHAN: You say put it on mute?

MR. KATZ: Yes, please, put it on mute.

CHAIR BEACH: Okay. Thank you, Ted.

For the benefit of those on the line, who don't have an agenda. I'm just going to run through it very quickly. We're going to start off with neutron dose reconstruction with NIOSH this morning.

Breaks are at 11:00, 12:30, 2:30. We're going to go into radon after neutrons. Then we're going to go into stable tritium compounds. If we have time we will go into either high fired Pu-238. That is a change from the printed schedule, and save adequacy, completeness, and internal dose records for tomorrow morning, I believe.

And once we're finished with those two, we'll go into road maps and the remaining closure items at the end of the day tomorrow:, beta/shallow dose, PAAA violations, and the D&D issue.

If -- unless that causes any kind of a conflict for anybody changing that data adequacy until tomorrow.

DR. ULSH: I'll have to check on the plutonium. It's not the data adequacy, it's plutonium-238.

CHAIR BEACH: It may be that they're both tomorrow. It just depends on how long neutron goes. One of our primary speakers is a little hoarse today and can't speak very well on the data adequacy. So that's the only reason for the change.

With that, thank you for everybody being here, and, Brant, if you're ready.

DR. ULSH: Sure.

CHAIR BEACH: We can start neutrons.

DR. ULSH: The latest development on the neutron issue is that we issued a revision of our White Paper dated December 9th. This is an ongoing -- probably the latest iteration in our discussions on this issue.

Basically just to recap, at Mound in the early days like most other places in the complex, neutron doses were measured with NTA film. As the technology evolved, they switched to thermoluminescent dosimetry. That occurred at Mound in the 70s like other places in the complex.

And our White Paper deals with a number of issues all related to neutron dosimetry and neutron dose reconstruction. Some of the fundamental issues are the adequacy of NTA film to measure neutron exposures that were experienced by workers. That's discussed in our White Paper.

The issue there in a nutshell is that in a moderated neutron spectrum like you might encounter in a workplace, a certain fraction of that spectrum falls below the energy threshold of the neutron film. So, in other words, if the neutrons are very low-energy, they're not picked up by the neutron film.

We discussed that issue, and we talked about the correction factors that we make to NTA films to account for that phenomenon.

We also talk about the situation where, at least in the early days of the SM building, where you would find the highest neutron exposures on the Mound site, pretty much the people who were stationed in SM building were assigned both neutron and gamma dosimetry.

A problem comes in though when you have visitors to the SM building, and by visitors, let me define what I mean by that.

Those are people who are not visitors to the Mound site. They work at the Mound site, but not in SM building. So think of plumbers or pipe fitters who might have come up there to do a discrete, short-term job. That's what I mean when I talk about visitors to the SM building.

For those visitors, in the early days they were issued visitor badges, neutron and gamma. But unless the gamma read a certain level, in other words, if it was high enough, then they read the neutron film. But if the gamma measurement was below that, the NTA film was not read.

So we're left with the situation where we have to come up with a way to estimate unmonitored dose essentially for neutrons, and we have an approach put forward in our White Paper. It is different over different periods of time depending on what information we have.

In the early days, we rely on the

Health Physics Progress Reports, where they give the number of films that were measured in that particular time period, and these are mostly quarterly reports. So over the quarter they give the number of films that they read and what dose categories they fall into.

So, for instance, less than 100 millirem, 100 to 150 millirem, and above that. So they give the number of films that fall into each category, and we have used that data to come up with a neutron coworker model, if you will.

The problem is, with those reports, is they are only published in the early days, up into 1960, and after that we don't have those reports anymore, so we have to come up with a different methodology. And we have looked at N/P ratios, which we've used at other sites, and we've also looked at modeling using the MCNP code.

And in the modeling, we've used Mound-specific parameters in terms of neutron energy spectra, and we are also using modeling scenarios that are at least, we believe, reflective of the worst case, reasonable scenarios that you would find at Mound.

And we have discussed this with a number of former workers, and we've taken their input and incorporated that into our modeling approach as well.

So one of the issues, I believe, that we were still discussing the last time we discussed this issue, there were some gaps in our Health Physics Progress Reports in the early years. We have now filled those gaps. So we have a complete set of information up to the 1960s from the Health Physics Progress Reports, and that's reflected in the latest revision of our White Paper.

We also dealt with the issue of NTA film fading. That is particularly an issue when you have long exposure times. So, in other words, if I wear a badge, say, for a month, they did studies at Mound and other places that demonstrated that, particularly low-energy photons, the signal on those NTA films tended to fade up until the time you develop the film.

And I have to take this opportunity to correct something that we said, I think, at one of our previous Working Group meetings. We said that the fading was an issue after the film was developed, and one of the people who is on the way explicitly corrected me on that, and he is correct and we were wrong. The fading occurs before the film is developed. Once the film is developed, the signal is set. So let me take this opportunity to correct that.

But we dealt with the fading issue. Mound had explicit studies on fading, and that's in our White Paper as well. So at this point, I guess, our White Paper is on the table, and we're open to discussing it.

CHAIR BEACH: Any questions? I'm assuming SC&A's ready for --

MR. FITZGERALD: Yes, thank you. Joe Fitzgerald.

We did have an opportunity to review this latest White Paper as we did the one before that and, you know, in general, I think we raised some concerns at the last Work Group meeting that focused fundamentally in two areas. I think some of the issues that Brant mentioned regarding fading and completeness of the progress reports, I think, were, in fact, addressed and resolved.

However, I think we still want to go back to the two larger questions that we raised, one of which was the application of the MCNP, and I know at the time we had this discussion about whether the MCNP, in fact, incorporated site-specific information: something that would tie it back to the actual parameters, geometry, moderators, what have you, at Mound, something that we felt was important that that model be sufficiently accurate.

And the other issue focused on -- that was the coworker approach, and some of the questions that Ron raised which you'll get into again is the year-to-year variability that was acknowledged in the paper, something on the order of 1 in 17. I think we were questioning that database, that the N/P ratios in the comparison were, in fact, valid.

But those are two central questions, and I think what we want to do is go ahead and start talking about the MCNP. That's where we kind of left off at the last meeting, questioning whether, I think we used the term generalized model could be applied with conservative parameters but without explicit workplace measurements that we could discern, and I think that's where I want to give the floor to Ron and perhaps Bob Anigstein as well.

Ron, do you want to start out?

DR. BUCHANAN: Okay. This is Ron Buchanan of SC&A.

Before we get into the details, I know this is like Monday morning, and you want to hear about neutrons, and so I want to lay a little bit of background because it's different than if you're used to working with gamma and beta and alpha.

When the neutron -- you have two situations. You have a bare neutron source which gives off fairly energetic neutrons, and then when it passes through moderating material which contains hydrogen, such as polyethylene or water or plastics, you degrade that energy spectrum, and you say that's good. Okay. We had shielding. That's good for the worker.

That's true. It decreases the magnitude of dose, but it also softens the neutron spectrum, which means you get a larger portion of lower-energy neutrons. And so the problem that we're discussing here, the basic problem is that these lower energy neutrons then would fall below the threshold of the NTA film.

And just so that you have something to hang your hat on here, NTA film is an emulsion, a photographic emulsion, and instead of just reading how dark it gets, you actually count little stars that the neutron interacts with the emulsion, count the number of proton stars created.

And at a certain point, the efficiency in creating these stars decreases, and this is about, agreed upon, about one MeV of neutron energy. You start getting a decrease in the efficiency of creating these stars or recognizable stars. These have to be counted under a microscope, and so if you look at the Los Alamos TBD-6, that gives a pretty good figure in there of the decrease in response as a function of energy, slow 1 MeV.

Now at some point it cuts off. The stars aren't recognizable by the reader, and this is up to debate. It varies between .5 and .7. It depends on how long it sets, how good the reader is. Somewhere between .5 and .7 MeV is the threshold where it just drops off a stair step and you can't read anything below that, and I say that to illustrate the fact that if you have a glove box, just a bare metal glove box or work station of some kind and you have a source inside that's just a bare source, whatever configuration it is in a stainless steel capsule, then the worker is exposed to mainly the full spectrum of that, I'll call it, bare source. That's one just in a stainless steel sealed capsule, all-neutron spectrum.

And so your NTA film is going to see most of those neutrons because they're around 2 to 4 MeV. However, when you put shielding there, polyethylene shielding, that's a good thing, and it decreases the amount of dose the worker receives, but it also softens the spectrum, and your NTA film won't pick it all up.

And so, say that you see half of

it and half of it you don't see. So if you know that's a constant variable, then you can multiply your results by two and come up with the right dose.

Now the problem comes in in that when the Health Physics Group calibrates an NTA film, they usually use a bare source out in the open and try to obtain a scatter-free environment, and they put an NTA film so many meters away, expose a certain amount of time, and get a calibration factor. But then when a worker wears that badge and it doesn't register some of the lower-energy neutrons, then his dose would be less than what he was exposed to actually because the calibration source had a few lower-energy neutrons and the worker was exposed to more lower-energy neutrons.

So in this case shielding would decrease a dose, but actually cause someone not to be registered, and this was found out. Mound used NTA film from 1949, 50, in that area, up until 1977, and then they switched out to TLDs, thermoluminescent dosimeters, which has that better energy response for lower-energy neutrons.

Now during this time, the DOE complex in general and Mound also recognized this problem with the lower-energy neutrons in the mid to late 60s, and so they did some modification to the dose of record between 70 and 76, and the records were multiplied by a factor of two to compensate for this. They felt that Mound used polonium, which is a high-energy neutron source from the beginning up to the 60-63 era, and so they felt that that didn't need adjusted because it was high energy.

And then they started using plutonium neutron sources in the 60s, and they got plutonium sources in in 59, but they really didn't do a large production of plutonium sources until the early 60s.

There was a transition period

between 60 and 65 in which they tried separating workers out and using one calibration factor for plutonium workers and one for the polonium workers, different calibration sources, and they said, well, polonium is going out. We'll just do everybody using the lower-energy plutonium sources in 1965.

And I say all of this because you have to consider that, yes, they did use one major source at one time and one major source of lower energy at a different time, and so the problem SC&A has and also NIOSH recognized is missed dose in the NTA film. The workers that worked with these sources do have dose of records in their files showing a certain dose, say, 100 millirem for exchange or 200, whatever it is, but we feel that this is low because this didn't record all of the dose.

So what NIOSH has done to propose to correct this is to go in and do some correction factors. In other words, you multiply the recorded dose by certain correction factors to bring it up to where it should be, and one of these -- and I'll just get this off the table -- is the fading, is the fading as Brant said between time of exposure and development. The tracks kind of disappear. Temperature, humidity and time have an effect on it. So the reader doesn't see all of the tracks.

And so you can either compensate for that by some calibration factors. We really don't have an SEC issue with fading, perhaps a site profile issue, but not necessarily SEC issues.

NIOSH did do some additional correction factors in the revised paper for fading. This is one correction factor. The other correction factor is for the lower energy neutrons that were actually not registered, and this is the main issue.

And so what NIOSH did, they set up an MCNP, which is a Monte Carlo Neutron

Transport Code, which is a generic code developed mainly at Los Alamos, used throughout the nuclear industry, and essentially what this does; it takes a bare neutron energy spectrum for our case, which I talked about earlier, and it says, how does the neutrons interact, each interaction, and then says what you get out the other side.

So if you put a certain amount of shielding there, certain geometry, then it says, okay, this is your neutron energy spectrum which the worker was exposed to.

And SC&A doesn't have a problem with using the MCNP or with -- retract that. SC&A does not have a problem with the MCNP model in that it has been used in many other places.

Now the input parameters to this model are mainly the bare neutron source, and SC&A does not have a problem particularly with the bare neutron source. These were pretty well characterized in most of the national labs and universities: a polonium, beryllium or a plutonium to provide source is fairly well known, the energy spectrum on the input. That's your bare source.

The other input to the model is the configuration, the geometry. What material is between the bare source and the worker? So that affects the spectrum that the worker is exposed to. What's behind the worker? What's above, and what's below?

And so this is what the model was to do, was to take that information. NIOSH used zero, two, four, and six inches of polyethylene or water in this case, which is similar to polyethylene, shielding and determined the percent of neutrons missed below the -- they used a .5 ratio. SC&A would like to see it done between .5 and 1, but we have to agree that it's an acceptable method before it's worth doing that.

And so what they did is look at all of the scattered neutrons, and they did a silo, concrete silo, put a source in a generic glove box with so many inches of water around it, and then the concrete scattered the neutron, and did it for an operator and an observer at certain distance and found out it ranged from 19 to 36 percent of the neutrons were missed. So you simply take a correction factor of 1.2 or 1.5, whatever it is, and multiply your reading by that.

And so that's -- and correct me if I'm saying anything wrong -- that's on NIOSH's modeling, and so what SC&A would have liked to see was some sort of benchmark that now we say we have this spectrum outside of this glove box that the worker is exposed to. We would have liked to have seen that, yes, at some place in Mound history somebody did either Bonner spheres, which are different sized polyethylene balls with neutron detectors in it to get a neutron energy spectrum so we could verify that. A neutron activation analysis of foils to get a rough idea of the spectrum, or a rem ball and an NTA film together to get a correspondence between those.

Neither NIOSH nor SC&A has really found good information on that at a work location, through the years at different locations and different operations at Mound, and so this is where SC&A -- we don't know if this model is conservative, under-conservative or about right because we don't have anything to benchmark it against.

Now the revised paper did come out with the one measurement that was made in 1966 outside a glove box using a source inside with ten- and 12-inch spheres. This is not a real strong stake in the ground because you really need more than two spheres to get good energy measurements. You can get different neutron spectrum that would satisfy this criteria, and it was at one location in one building at one time. It wasn't any other information other than that one measuring point.

Another issue that we are concerned with is that the modeling does an AP frontal exposure, and the NTA film has a different response depending on whether the neutron is coming right at it the way it's calibrated, from the side, zero degrees from the side right at it, and so NIOSH did propose a correction factor for AP exposure, frontal exposure, and if you have any issues with that, that is a site profile issue.

However, at these production facilities, usually you had rows of work stations or glove boxes. So you had PA, which is from-behind exposure, and the film badge worn on the chest of a worker, the neutrons that have passed through eight or ten inches of hydrogenous material before it was registered.

There has not been any studies --

MR. KATZ: I'm sorry to interrupt, but I'm not worried that we can hear in this room because I have the volume down, but there are a number of people that don't have their phones on mute on the line, and you are probably causing trouble for other people on the line who are trying to listen.

Hello. There are some people talking on the line. Please mute your phones. Use *6 if you don't have a mute button.

Thank you.

I'm sorry, Ron.

DR. BUCHANAN: Okay. So if you have neutron irradiation from the back, then we don't know what correction factor needs to be added to that NTA reading, and we have not --

MR. MORRIS: Excuse me, Ted. This is Bob Morris. Could you ask Ron to back up about three minutes on this conversation?

(Laughter.)

MR. KATZ: I don't know if you have a rewind button.

DR. BUCHANAN: Okay. What was the last thing you heard?

MR. MORRIS: I think it was when you were -- it was just before -- let me see if I've got this right. You didn't have any problems with our angular dependence. I think that's really when I couldn't --

DR. BUCHANAN: Okay, yes.

MR. KATZ: And this is who speaking? Sorry.

MR. MORRIS: It's Bob Morris.

MR. KATZ: Bob Morris. Thank you.

DR. BUCHANAN: Okay. So that's just a short period. That -- won't have any problem with that.

Okay. NIOSH did propose in the White Paper that the AP frontal irradiation had an adjustment factor for it, and so this was taken from some earlier work done by published authors, and if this is a problem, it's more of a site profile issue from frontal irradiation.

However, we're concerned at Mound, like Los Alamos perhaps, had rows of work stations or glove boxes in which the worker was irradiated from the back side with neutrons from a station behind it, and so this would not compensate for the fact that the neutrons had to pass through eight or ten inches of hydrogenous material which would degrade the neutron energy and would not necessarily be registered by the NTA film.

And so there has not been any correction factor proposed for this, and we are not aware of any correction factors readily available in the published literature on this. There might be some. If there is, we'd be glad to hear about it.

And so in summary, there's two main issues here now, the modeling and the coworker model. So I'm going to summarize. The modeling issue is that we, again, don't have a problem with input bare source. We do have a problem in that the output from the modeling may be correct. It may not be correct. We don't have any benchmarks that we can tie it to to say it is correct. So this is an objective decision on the Board whether they accept a model which could be conservative, it could be right on, or it could be under-conservative, which we can't really benchmark. SC&A can't, and we haven't found that it has been to this point sufficiently benchmarked.

Now this is for the modeling. Now the modeling also affects the coworker dose because if you're going to use the data from the coworkers that were batched, the dose of records and say, okay, we're going to apply this to the people that Brant talked about that were not badged for neutrons and should have been badged. Then you have to do the correction factor for that data also.

And that was done. NIOSH did apply the correction -- all the fading, the low-energy response and the angular dependency to the coworker data that was available, and then generated some coworker information.

Now they did this in two parts, and Brant briefly explained it, but I will explain in a little more detail so that you can understand what the SC&A issues are with it. There's two sets of data that they generated. From 1949 to 1977, in Table 4-4, there is the year listed and N, which is the number of paired neutron/photon badge readings they have, and then they have the N/P ratio that they derived from that. N/P ratios are usually used when you do not have neutron data or reliable neutron data, and what that means is that you take the photon dose; you multiply it by a factor, say, two, and that is the neutron dose you assign for that dose reconstruction.

Now this is usually used, such as Rocky Flats, when you had a period of time, say, from 1960 to 1980. You didn't have reliable neutron data, but from 1980 to 1990, you had reliable neutron data because you started using TLDs or some good method of dosimetry. So you take the neutron reading average, photon reading. You get a ratio of, say, two, and then you apply that back to the workers in the earlier periods when you didn't have neutron information, but you had photon information.

Photon badges are usually fairly reliable, and so if a person got 100 millirem of gamma dose, photon, then you'd multiply that by two and assign him 200 millirem of neutron dose.

Now this is what is done in Table 4-4, page 21 of the -- no, that was the earlier issue. Anyway, it's Table 4-4.

DR. ULSH: You're correct. It's page 21.

DR. BUCHANAN: It's still page 21. Okay.

And so this gives you the 50th percentile and the 95th percentile of the N/P ratio. Now I understand NIOSH means this to be a bounding upper estimate of the neutron dose, and they prefer to use another method from 1950 to 1960, which is in Figure 6.2, and the new one has all of the constant quarters filled in. This is from the HP Monthly Reports.

And so they adjust. What happens here is the HP Monthly Report only gave the number of badges that fell within a dose interval. Zero to 100 millirem, 100 to 200, greater than 300 millirem. So you may have had 40 in one, two in another, and none in another or something. And so they applied the adjustment factor to it, and then the adjustment factors to it, and then came out with a median and a 95th percentile dose.

I guess SC&A's question on looking at this, it recently dawned on me, why did we use this method when we had all of the NTA film data available. Usually NTA N/P is used only when you don't have data available, like for 1954, we had 32 NTA films available. Why don't we just make a table of coworker data like we do photons? If we want to do photons, we would just take the badge readings, make a table, and assign that per year.

So Table 4.4 and Table 6.2, I think it's unnecessary. Why don't we just take the NTA data and assign it as coworker with the adjustment factors and such and see how that looks from a statistical basis because SC&A feels that the Table 4.4 has too much variance between years to be valid.

Now we have not seen this data. I could not find it on the O: drive that was used to create Table 4.4. So we couldn't do any statistical analysis on it, but now I understand the White Paper to say that it is correlated, and it appears perhaps to be correlated within a year in that your GSD is not large for each year, we have very much concern about from year to year for a factory-type operation, assembly line-type operation, which should be fairly -- you've got fluctuation as operations change, shielding changes and stuff, but we do have a problem with going like from 1957, we go from 4.9 to 11.9 in 58, and we have a range of .6 to 18.6.

And so we have a problem with that fluctuation. On the 1950 to 1960, the intervals information, you don't have exact data. You don't have real worker data per worker. You only have a number of badges in a certain range, and we feel that this is kind of a hazy area. Is this acceptable or not? And that may be another subjective decision like the modeling is, and we'd like to see what it looked like if we just used the NTA film badge data with correction factors for the coworker.

So this, issues of the -- that I just talked about, is the areas that we have concern whether dose reconstruction can be done. Number one, they're modeling benchmarks, and then we have a lot of correction factors on top of each other, and it spills over into the coworker dose, and so we feel that there remains questions that some are site profile issues, as I alluded to, but some, I think, affect the feasibility of doing reasonable dose reconstruction with neutron data.

Any questions?

CHAIR BEACH: Thank you.

DR. ULSH: Paul, do you have one?

MEMBER ZIEMER: I just had one question for clarification. On the one measurement you mentioned, which looked like an attempt to benchmark or you said perhaps could be used for benchmarking with a two-sphere Bonner system, did NIOSH actually do what Ron talked about with that set of readings?

I mean, you can always argue that, yes, it would be great to have three Bonner balls that are four or five and came down the spectrum, but do we have a rough benchmark? That's what I'm trying to get a feel for when you referred to that one set of measurements.

DR. BUCHANAN: In that particular instance in 1966 with that glove box in that location. Now, I don't know any of the others. I tried to find that reference and could not find that number in the site research database.

DR. ULSH: Do you recall which reference you're referring to? I know that's a tall order.

DR. BUCHANAN: Yes. It's 76610. It's what's listed in the White Paper.

DR. ULSH: Seven, six, six, one, oh, is that the SRDB number?

DR. BUCHANAN: Yes, the SRDB reference, ID 76610.

DR. ULSH: Okay.

DR. BUCHANAN: And I couldn't find it. I asked Joe to alert somebody, but it wasn't time over the holidays.

Okay. To answer your question, I don't know the details of the experiment. Okay? But it would only apply to that one particular --

MEMBER ZIEMER: Right, but it would seem to me that they must have had a lot of Bonner sphere measurements around the facility. Do we know that?

DR. BUCHANAN: No.

MEMBER ZIEMER: They didn't? I mean, they have the equipment.

DR. BUCHANAN: I don't know if they had PNL come in to do all of their measurements or not. I have not found it in the literature. I mean, this is the problem. We cannot find --

MEMBER ZIEMER: That's the only one we know of?

DR. BUCHANAN: -- spectrum. That's the only one.

DR. ULSH: Hold on.

DR. BUCHANAN: I mean that I'm aware of. Maybe Brant has others.

DR. ULSH: Bob, do you have anything to add now? I want to make sure that you have a chance to chime in, but do you have anything to add on that particular issue?

DR. ANIGSTEIN: Are you referring to me, Bob Anigstein?

DR. ULSH: No, sorry. Bob Morris.

MR. MORRIS: First of all, going backwards on the Bonner sphere question, [identifying information redacted] did many Bonner sphere measurements. They were always -- and in fact, as we understand it from the public meetings, every source that was shipped from Mound was measured with a set of Bonner spheres after a certain date when those became in common use. They measured the spectra width before it was shipped out.

Now, the problem with that is that it's only going to be in the bare configuration as they set up for that kind of routine quality measurement on their finished product.

So there were many, many Bonner sphere settings -- energy measurements made, but do they represent the situation we're modeling here, and the answer to that is no. We don't have that recurring measurement in the workplace.

But with regard to benchmarks in general, MCNP has been benchmarked and published in peer-reviewed literature over and over and over again. The question is not, can MCNP reproducibly predict a neutron dose given a certain input energy and a certain shielding configuration. The answer to that is, yes, that's been proven dozens and dozens of times.

The question is do you agree with the input assumptions about the shielding configuration. If you can say, yes, four inches of water is a reasonable assumption about the amount of shielding, concrete below the people's feet, concrete above the people's heads, concrete as wall material, that's reasonable assumptions. If you can come up with those to the point that you agree with those assumptions, then there's not much doubt about the outcome being appropriately predictive of what happened in real life.

DR. ULSH: I would also add that the particular configuration that we modeled, as Ron described earlier, was kind of a generic glove box inside of a concrete silo. Now if you were to go back in time and look for a concrete silo at Mound, I don't think you would find that that's a representative scenario, and we're not presenting that it is.

Rather, we picked that scenario because it's bounding. It's going to be the worst case that's reasonably consistent with Mound. In other words, the fact that we assumed concrete walls and a concrete -- well, a concrete floor might be reasonable, but concrete walls and ceilings, I don't think that that's necessarily representative of Mound, but doing that is claimant-favorable because it increases the scatter. It increases below the fraction of the neutrons that are below the energy threshold.

So that's why we picked that to be a bounding assumption, rather than get into issues about how many glove boxes were in the line and how far behind the worker was the glove box line.

Ron, you're right. I mean, in terms of glove boxes being in a line and getting some neutron dose from the rear, from the AP -- no, PA geometry, but I would also remind you of the $1/r^2$ rule where basically the dose decreases as the square of the distance. So I can't tell you that the contribution is zero, but it's very, very low.

You know, I think that to say that we don't have benchmarks at Mound, you know, Mound-specific benchmarks, I don't think is true exactly. We did have the meeting with the former workers, and we did run all of this by them. Now, I don't want to present to you that this is their model. It's not. This is our model, but we asked them to point out anything that might be unreasonable in what we're suggesting, and it was a multi-hour meeting and we had a lot of discussions and a lot of suggestions which we at least attempted to incorporate here.

We talked to some of the members of the public about when the shielding was put in, how much shielding was used, and they pointed out that there is a limit on how much shielding you can actually incorporate because you still have to be able to reach through it and do the work inside the glove box. So we incorporated that in how much shielding we assumed. That, you know, in a way is Mound-specific. We talked to Mound workers. We got their input on how much shielding was used at Mound and that's reflected in what we've done here.

Also, I'm pretty sure that this is in the SRDB, though I don't have a number. Just to give you an example -- is it okay for me to say authors' names or is that -- yes, okay.

We have neutron energy surveys of SM building. The one that I'm looking at right now is dated March 2nd, 1964, where a guy named [identifying information redacted] used the TMC double-moderated neutron counter to determine neutron energies in SM building and presents a table with one, two, three different rooms, different hoods, what material was inside, what the ratio was, what the energy in terms of MeV was.

So certainly, I mean, there are a handful of neutron dosimetry experts across the complex, and I don't mean this to be an all-inclusive list. People like [identifying information redacted], Roger Falk, and [identifying information redacted] is certainly among that number in terms of the foremost neutron energy experts in the country, and it would be extremely surprising to me if this issue was not -- they weren't aware of this issue; they didn't take steps to address this issue.

We have examples, although I don't want to say a complete set, that they actually did do neutron energy surveys in SM building and, you know, one can assume multiple other buildings as well.

And I would also point out to you if you look at page 41 of our White Paper, you will see a series of graphs that give the dose-equivalent weighted spectra at the different positions from our modeling, and the important point, I think, to make here, what you're going to see is a series of curves, and the dose equivalent. In other words, when you're trying to calculate millirem or millisieverts neutron dose to a particular person, that would be the area under the curves here, and I know that the people on the phone may not have this at their fingertips, but if you look at the lower energies, and I'm talking about .1 MeV, in particular, the part of the neutron spectrum that is not reliably measured by the NTA film also happens to coincide with the part of the neutron energy spectrum that contributes a trivial amount to the dose equivalent.

So I think we've got a tempest in a teapot here. We're splitting hairs on a fraction of the energy spectrum that is almost negligible in terms of what it contributes in terms of dose equivalent. So even if we didn't measure any of it, I think it's a trivial contribution to the total neutron dose equivalent and that's what we're interested in here. So I don't think that that's a big issue.

In terms of the coworker model, I don't know. It's the first time that I've heard that idea, Ron, about why don't we just use the NTA films themselves.

MR. MORRIS: Brant, before you go into that, can I chirp in one?

DR. ULSH: Yes, sure, jump right in.

MR. MORRIS: The other thing I'd point out is that the tables of 7-3 and 7-4 which showed the neutron dose equivalent missed due to the NTA film energy threshold effect, they effectively define a sensitivity study so that you can say, well, you know, I don't think two inches of water is the right amount. I don't think -- I think six inches may be too much. It may be three, perhaps.

And what these tables will actually show you is what your eye will interpolate between them, and you can see that you can effectively define the most important parameter here, which is the water shielding thickness in the amount of missed dose and understand, well, what if my assumption was completely wrong.

Well, in the case of a plutonium-beryllium source, if you assumed two inches of water shielding, you might miss nine percent of the dose, and if you assume six inches of water shielding, you might miss 11 percent of the dose.

So you can see that it effectively will help you understand how important these assumptions are to the overall conclusion. So whether or not we've got a ground true benchmark in a specific location, it still gives you this ability to understand the importance of the various assumptions that go into especially the shielding thickness assumptions that go into the model.

DR. ANIGSTEIN: This is Bob Anigstein. I'd like to chime in with a couple of comments.

One is, again, this Table 7-3 makes the assumption that there is a sharp cutoff at .5 MeV and that everything above .5 is registered by the NTA film, and that is just not correct. The fact is that there is a gray area that, between .5 and one, some neutrons are registered, some aren't.

I point out that in the Hanford TBD, there was an examination and actually they referred to, I think, a meeting or symposium sponsored by AEC in the late 1960s where they said that the NTA film cannot be used below 700 keV or .7 MeV, and this is not consistent with what is being done here.

DR. ULSH: Well, I haven't seen the particular reference. At least I don't have enough information to determine whether or not I've seen the reference that you're talking about, Bob. I would say to you that NTA film is always used when neutron energy spectrums -- I'm sorry. Let me rephrase.

During the time period when NTA film was the methodology used to measure neutron dose, it was always used in the presence of low-energy neutrons because you always have low-energy neutrons. Unless we're talking about a bare californium source or, you know, a particular bare source, I think part of that spectrum is always going to be below the NTA threshold.

I do agree with you that there is

not a sharp cutoff at .5 MeV. There's a reduced efficiency above that and, Bob Morris, sorry, you may want to talk a little bit about that, but --

MR. MORRIS: Sure. The reason we chose .5 MeV is because that's the OCAS Implementation Guide. I think that's what the title of the document is. It tells us to assume .5 MeV, and so that's what we did in this case.

But I actually ran the calculations with a .7 MeV energy cutoff, and they're not incorporated into this White Paper, but as you would guess, the numbers go up in terms of missed dose, but they don't go up precipitously. They're not a big change.

We could certainly provide that information. It wouldn't be a big deal for us to add those tables into the White Paper, but it really comes back down to those are TBD questions. They're not SEC questions. I mean, whether the correction factor is 31 percent at its worst for the operator location or whether it's 39 percent at its worst for the operator location, it's still a reproducible number in terms of our calculating.

DR. ANIGSTEIN: And the other issue which was talked about but not focused on is the variability from year to year of the N/P ratio and then the grade variability within a given year. There was this box-and-whisker plot which shows that actually at the extremes the ratio can go within a given year -- can be as low as one and as high as 33, and then the fact that the instrumental comparison showed no correlation, you had five-by-five survey meters measuring photons and neutrons, and there was no correlation whatsoever. I mean, the .15 correlation coefficient, which we all agree shows a very poor correlation, calls into question the whole concept of using the N/P ratio for Mound.

DR. ULSH: I kind of disagree with

that, I think, because when you're talking about instrument calibrations, of course, you're measuring a particular point in space, a particular time, and you're measuring neutron dose and gamma dose at that particular point.

Well, that will correlate, you know, assuming that the dosimetry is effective; that will correlate perfectly if a particular worker stands at that point in time 100 percent of the time, and as we know that's not realistic. Workers move around. So they'll move away from the neutron source. They'll get gamma dose somewhere else. That's going to lead to a poor correlation perhaps between what you might estimate from a neutron instrument reading, instrument readings taken at a particular point and what was actually experienced by at least the dosimeter worn by the worker.

I do agree with you that there are some years on the box-and-whisker plot, and by the way, that's Figure 4-2 on page 20, where there is, well, the variability that you have described, but I think we have captured that variability in Table 4-4.

So, yes, I mean, if there is great variability, what's the effect of that? Well, it leads to high estimates of the 95th percentile that's bounding. I mean, that's claimant-favorable.

It's not claimant-favorable?

DR. ANIGSTEIN: The instructions to the dose reconstructor in the White Paper, maybe they're a little vague, but they don't say to give everyone the 95th percentile. My impression is that it implies you can just assign the entire distribution, which in fact is similar to giving the median value rather than the 100 percent.

It is specifically on page 49, the second bullet. It says, when using the NTA, shall multiply the measured photon dose by the log-normal distribution, not by the 95th percentile. Ninety-fifth percentile is certainly more claimant-favorable than just throwing in the entire distribution because with that particular person you don't know where you would be on the distribution. You may very well be at the high end. We don't know.

DR. NETON: We have to think about this, though. I'm not as familiar as I probably should be with this calculation, but would that distribution be multiplied by the photon dose that included all of the missed dose?

DR. ULSH: Yes.

DR. NETON: I think that's where you run into a little bit of a problem. If you start calculating missed dose that wasn't measured and you get this inflated value and then you start multiplying that inflated value and a 95th percentile, you start to get into, I think, some unreasonable values.

We've run into this before with

the NPE corrections in the past.

DR. ULSH: But you also have to keep in mind who this methodology is going to be applied to. If a person was measured by NTA film, that's what we're going to use. The people who this would be applied to is the people who were not measured with NTA film, and those people are, for instance, visitors to SM building that came in for a short period of time and their gamma badge read below the threshold that would trigger them to read the NTA film.

So I would say to you that it is reasonable to assume that these are low exposed workers.

DR. MAKHIJANI: Could I ask a question about that? A little bit of an observer. First of all, these ranges from the Health Physics Report, zero to 100, 100 to 200, they're not measurements for the type of worker you're talking about, say, a construction worker. They're measurements for workers who were badged and whose badges were read and the ranges were put. So we don't actually have measurements for them. We're assuming that what was experienced by those workers is similar to what was experienced by glove-box workers, but I'm not sure. It's a question.

Is that what we're doing?

DR. ULSH: If you look at Figure 6-1 on page 22, it gives an example of an excerpt from the Health Physics Progress Report, and it's typical of what you see, and it has got a section for film meters, beta and gamma, and it has got regular and visitor, and then for neutrons, it gives, the particular example here, it gives the number of films processed, 818, regular and visitor, number of readings, zero to 100 millirem, 100 to 300 millirem, and over 300 millirem, and what it shows you is -- so the regular and the visitor are included here.

DR. MAKHIJANI: But from what

you've said earlier, the reason for the question is from what you said earlier is when visitors were there, say, for a day or a week, and then they did their job and they didn't come back for another six months, in that week, if they didn't get a measurable photon dose, their neutron dose was not read, right?

DR. ULSH: Correct.

DR. MAKHIJANI: And so how do you know whether any visitors are represented in this or whether their badges were read in such a scattered way that -- how do you establish the relationship between the N/P ratios for the people you're talking about and the N/P ratios for whom you actually have data?

DR. ULSH: Well, I think the N/P ratios were calculated based on the type of information that you see here, which includes both regular and visitor. I don't think that we have the ability to separate out the visitors as a discrete group if that's what you're asking.

DR. MAKHIJANI: Yes, and I'll tell you the reason I'm asking so I'm not being mysterious here. I'm working on Savannah River Site construction worker data with our team, and we're compiling these, and we not only see some cases, not all, some cases where construction workers are more exposed or at least have the higher bioassay results in this case, and not compiling neutron data, in some circumstances than sort of process workers, even though they may not have been on the job in a similar pattern.

We also see maybe there might be different distributions. It's so -- I'm just bringing up the issue in this context because we've seen something kind of important emerge in another context. So I give you a little preview of what's happening.

DR. BUCHANAN: I would like to clarify something. Okay? Now, the Table 6-1 and 6-2 is NTA film readings. These are not N/P ratios. Table 4-4 is N/P ratios. The 49 to 77 is N/P ratio calculated from NTA film. Table 6-1 and 6-2 is actually readings, not direct readings, but intervals, dose intervals from NTA film readings. It is not an N/P ratio methodology.

DR. MAURO: I would like to jump in a little bit. It seems that the rock you're standing on is the sense that you could place an upper bound on what the adjustment factor needs to be to the NTA film. In other words, you've got a lot of film badge readings for workers that work with the glove box. You are getting information back. There are tracks that could be counted. You have lots of information, what that looks like when it's bald or naked source. We all understand that, but that has very little relevance. That's more of a quality check that the product you brought in is the thing you think it is.

And then you put this source in a glove box, and here's where I have been thinking a lot about this and we've been talking a lot about this. Here's here I start to say to myself, okay, it's 1963, 64 and I'm a health physicist, and my job is to make sure that the people who are standing by that glove box meet their dose limits for the week, for the quarter, for the year, 5 rem a year, whatever it is, and I know that neutron dosimetry is problematic, especially when it's attenuated.

So I say to myself, okay. If there were -- now, you went through a number of scenarios. Well, let's put one inch, two inches, three inches, four inches of water between the source and the film badge and the worker as if that represented a bounding set of circumstances. That in reality may very well have occurred at Mound.

Now, right now I don't know if that's true. In other words, when speaking to folks that have worked quite a bit at Los Alamos, they were not in a position to say whether or not that's a realistic scenario.

Where I'm headed with this is that the construct -- the construct that you have related to the sensitivity analysis of how does the distribution of energies change as a function of changing fitness of shielding is a construct that inherent in it is a presumption that somehow you've captured the upper end of the amount of shielding that might be there. I'm concerned that if you didn't, in theory you could have a person standing in front of a glove box working and if there's enough shielding there, you're not going to see any neutron exposure, and he's going to be dosed, though. He's going to be getting an exposure to neutrons, all of which might be below .5 MeV. You see nothing on his film badge and so, therefore, there's nothing to apply an adjustment factor to.

So where I'm going back to is, if you're going to use MCNP, which is a great, perhaps one of the best simulations for doing dosimetry, there has to be a connection between the amount of shielding you're saying places an upper bound on the amount of shielding that's plausible.

Now, you intimated something very, very important that I didn't hear before when we were talking about this. You intimated that the physical setting of these glove boxes are such that you really could not fit more than six inches of shielding even if you wanted to.

Now, that's a very important statement because what that does is it places a boundary on the physical reality that you really can't put more than that. So under the worst-case conditions, you're saying for these particular glove boxes, you can't have more than six inches of water or other attenuator between the source and now when you said that it's -- coming into this meeting, I was concerned that if you can't place an upper bound either by process knowledge, understanding the design of the facility, what the practice was, anything in the literature that would establish a plausible upper bound on the amount of shielding, we've got a real SEC issue here. If you can't put a boundary on that, then theoretically the story I just told, you could have someone there being exposed to .5 MeV photons and less, and you're not even going to see anything on the film badges.

I would like to zero in on the level of confidence that you have that when you did your sensitivity analysis and you laid in the six inches of shielding as being your upper bound, that we could hang our hat on that, and that there's evidence that that's a real boundary. Because now we are talking turkey, and not only that. Now we're talking what goes to the heart of Part 83 where you have to use site-specific information because up until now, until I heard that, I didn't hear any site specific information.

DR. ULSH: I don't want to

interrupt.

DR. MAURO: No, I'm done.

DR. ULSH: Okay.

DR. MAURO: You heard my story.

DR. ULSH: I can tell you where we got that information in terms of the ceiling characteristics and the thickness, and that was from workers who worked there first hand, and they raised a good point that don't forget you still have to balance dose reduction versus being able to actually reach inside and do the job.

So that's what made it even more compelling when we heard it straight out of the workers' mouths who were there. Really, I mean, there's a limit on how thick you can put the shielding in place.

DR. MAURO: I was speaking to a fellow that worked for, I think, 20 years at Los Alamos, and I asked him, does that sound reasonable to you?

Unfortunately he's not on the

line. I wish he could be on the line, [identifying information redacted]. I said, [identifying information redacted], this is what you did for your whole career. Could you say with a degree of confidence based on your experience at Los Alamos that six inches of shielding is probably a good number?

He says, well, I could say that probably a pretty good number for Los Alamos, but I have to tell you I really have no way of knowing whether or not that's a good number or not for Mound, and so he said, I couldn't stand by that, that six inch number. I mean, I couldn't today sit down at this table with you and say, yes, in my opinion based on 20 years of doing these calculations and working with these glove boxes because I don't know what those glove boxes look like, and I don't know what the practice was of shielding neutrons.

So we were left in a position of taking on faith that the six inches was, in fact, a good number, but we don't know that, and I don't know if anyone in this room knows that.

DR. ULSH: I know of two people in the room who know it.

DR. MAURO: Okay.

DR. ULSH: Bob, did we use six inches? We keep talking about that number. That's what we used, right?

DR. BUCHANAN: Used two inches for your dose.

DR. MAURO: And then you get a sensitivity analysis, though.

DR. ULSH: Right.

MR. MORRIS: The six-inch number is available. Let's see. So you're asking what did I use for the correction factor calculation?

DR. ULSH: What did we assume for shielding thickness?

MR. MORRIS: We calculated it for zero to six inches thick, and then let me double-check because it has been a while since I wrote that. I'll get back with you in just a second.

DR. ULSH: Okay. Whatever that number is -- I was pausing to see whether I should say it or not. We have a former worker in the room who was one of the primary sources where we got that information. I think that six inches was the number we threw out, but even if it's low, if it's a few inches more, again, that's a TBD issue. I mean, it's not infinite. You still have to be able to reach through it.

DR. MAURO: Well, at some point you're going to block out everything above .7, and then you've got yourself a headache.

DR. ANIGSTEIN: This is Bob Anigstein.

Another issue I'd like to throw in relevant to this is the fact, going back to N/P ratio, the fact that workers with low gamma, low photon doses, they didn't even have their NTA film read. That puts a bias on the ratio because that gets rid of some potentially very high. It's possible to have low photon and high neutron. For instance, if you're sitting in front of a glove box that has a lot of lead shielding, it will effectively stop the photons, but it will have very little effect on the neutrons. So the two measurements, using that ratio is a very soft statistic.

DR. ULSH: Well, I wouldn't necessarily disagree that lead shields shield photons. I don't think lead shielding is realistic for Mound. At least I haven't heard of it.

MEMBER ZIEMER: Well, that's another site-specific issue actually, and unless we know that lead was used -- I mean, you're talking about a very specific thing.

Yes, you can cause that effect as Bob described, but did the Mound glove boxes actually use lead shielding?

MR. STEWART: The answer to that is no. They use hydrogenous material for the new glove boxes.

DR. MAURO: These are two different -- I think in my mind, unless my little model in my head is a little different, we have two questions here. One has to do with the adjustment factor to the neutron dose as reconstructed based on NTA film and whether or not the adjustment factor for the distribution is, in fact, bounding, and this goes to the question of how many inches of hydrogenous material.

The other question, I think, which is separate and equally important, is the neutron to photon ratio, and that is all over the place. So, I mean, if we could zero back, I'd like to -- before we move on, I'd like to hear a little bit more about this.

You see, I am concerned that for all I know there could have been a common practice to insert quite a bit of hydrogenous shielding material where your adjustment factors no longer apply. There may actually be a point where, if only a small percentage -- in fact, Arjun and I discussed this over the weekend -- if only a small percentage of the energy of the neutrons that actually reach the detected film badge, let's say, ten percent, 20 percent, five percent -- I don't know -- that adjustment factor all of a sudden goes through the roof.

DR. MAKHIJANI: Right. I mean, that's the reason I was kind of shaking my head when you said if everything gets cut off --

DR. MAURO: That posted --

DR. MAKHIJANI: Everything doesn't have to be cut off, right.

MEMBER ZIEMER: Well, the mean free path of these neutrons, as you well know, so that you can easily put a bound on what that is --

DR. MAKHIJANI: Well, you can

tell. You know, you can calculate a thickness of helium material where you'd get little or no 5.5 MeV neutrons out, but my point --

MEMBER ZIEMER: No, no. I'm talking about the FAS, the mean free path of the FAS. John is talking about a scenario where you get moderated all the time.

DR. MAURO: Moderated, that's right. That's what I'm saying.

MEMBER ZIEMER: It's very easy to come up with that number. I don't know what it is, but you can come up with that very easily. What would it take?

DR. MAKHIJANI: What it would take.

MEMBER ZIEMER: And it's a certain number --

DR. MAKHIJANI: To shut it down.

MEMBER ZIEMER: -- of paths. If you get about five to seven mean free paths, then they're pretty much gone, but I don't know what that is, but they could easily find that out.

DR. ULSH: Yes, we could. Off the top of my head --

DR. MAURO: But you see the --

DR. MAKHIJANI: But the thing that I just want to finish here is you don't actually have to cut off all the energy neutrons to have a problem with this approach. If you have most of the high energy neutrons attenuated and then the correction factor becomes very sensitive to an exact knowledge of the percentage of high energy neutrons that are getting through and what that energy spectrum actually is, because if it's five percent versus 15 percent, your correction factor is deferred by a factor of three.

MEMBER CLAWSON: I've got a question. This is Brad.

Do we know exactly how much of this hydrogenous or did it vary on these glove boxes?

I guess what I'm used to is I'm

used to being able to pull up a print, and they have got a set boundary of how these things were put together. Do we have --

DR. ULSH: Well, you raise a good question, Brad, because at the beginning it was simply, you know, a sheet of plexiglass per unit, very thin shielding.

MEMBER ZIEMER: Right.

DR. ULSH: Over time as they realized they had a problem, they added more and more shielding. So you're right. We can't reproduce the exact time line of when and where shielding was added, and that's why we have taken the approach of assuming what we consider to be the worst case.

You know, as Ron described earlier, adding shielding is good from the standpoint that it knocks the energy spectrum down, and it shields the worker from the neutron dose, but the problem is that as you add more shielding, more of those neutrons are below the energy threshold of the NTA film.

So for this particular application, what we've assumed is what we consider to be the maximum practicable, the maximum plausible shielding thickness because that's what we would consider to be a bounding scenario in terms of how much neutron dose could we have missed.

And just to give you an idea, I mean, to go back to something John said, you're right, John. I mean, at some point if you add more and more and more and more shielding all the way out to an infinite thickness of shielding, you aren't going to see anything, and I don't know at what point between zero and infinity that happens, but --

DR. MAURO: I'm bringing what I call my common-sense approach. You pick six inches, two inches as being your default value.

DR. BUCHANAN: That's what they used in the tables.

DR. MAURO: In the tables for the

local factor. The basis for that I don't know, and you used six inches -- I think it was up to six inches -- in your sensitivity analysis. I don't know why you stopped there. I mean, to me you've got to bring that back to Mound.

In other words, when those assumptions in the end only are useful to us and only meet the letter intent of part 83, if you could build a bridge between that and Mound, somehow you've got to be able to do that, and I haven't seen that yet.

DR. ULSH: Our bridge to Mound is what we heard from the workers in terms of their input on what shielding was at Mound.

MEMBER SCHOFIELD: I would like to ask these workers a question. This comes from many years' experience working glove boxes myself. A lot of the 238 glove boxes you had thicker lead or I mean not lead either; polyethylene or water shielding below the glove box where you may have instrumentation or pumps or whatever you've got down there, and then at the actual upper level where the workers performed the hands-on stuff, you had a thinner layer of shielding.

From a practical standpoint, I know six inches of additional shielding would make it very difficult for the average worker to really reach in there and do their work.

MR. MADDING: You weren't in there all the time. You might go in, turn a valve --

CHAIR BEACH: If you're going to talk, you have to give your name.

MR. MADDING: Dick Madding.

I started working SM in December 29th, 1963, and was at Mound, working at Mound through 1981. I had various jobs. My job when I started out was final assembly of the product that was attempting to be produced at NSM. So I didn't have to work back in the really production area itself.

And as Brant said, the shielding

varied widely. The boxes I worked in had nothing, practically nothing, because I was on the tail end of the process, and by the time it got there, not very much material.

Down at SM-35 where the production was done, toward the end of the cycle, six inches was the average, and there were some places that were higher, but what you would do, you would go in and you might load a furnace. You might only be in there five or ten minutes, maybe not even that much, and you made do with it.

And your point about the shielding below being heavier, I think in the most cases -- and Warren may have something on this -- it was more or less the opposite. There wasn't a lot of shielding down low. The shielding started, you know, at your waist where the glove box was, and they kept adding and adding until you couldn't -- like you say, you couldn't do anything.

MEMBER CLAWSON: Well, this is

Brad speaking.

MEMBER CLAWSON: This is one of my issues because we got into the history behind this, why they sent me up to Hanford, because they were putting everything -- everything was shielded, but all they were taking care of was the upper portion. All of the bottoms of the glove boxes had zip holes. So they didn't calculate that in because in the calculation they put is that we have a wall of this much shielding, but what we ended up doing was getting back-scatter from it, which increased even more.

MR. MADDING: Now, I don't say there was nothing down below, but you might have two inches, maybe four inches, but the boxes basically were shielded above the waist.

MEMBER CLAWSON: Right, because that's what they were trying to protect.

MR. MADDING: That's what they were trying to protect.

MEMBER CLAWSON: When you were getting up there to that, but it gets on into some other issues.

CHAIR BEACH: So, Brant, do you have any actual engineering drawings or specs for any of these glove boxes to give you the actual --

DR. ULSH: You know, I've looked at so many over these past two years. I don't want to say that we don't. Nothing jumps to my mind.

MR. MATTING: I'm looking for them. There are engineering drawings still in existence, but I've been working on the technical documents, 8,000 of those, and the 77,000 photographs, but I am coming across engineering drawings in the documents, and part of doing the documents is I end up -- in order to be sure optical character recognition is on all of the pages, which was not the case when they were scanned, I am looking at every page of every document, 93,000 pages, and I'm 90 percent through.

But I do run into some engineering

drawings, but I have been sensitive to your question about Benelex and that kind of thing before, and so I've been keeping an eye out, but I have not seen any.

DR. ULSH: So I guess the answer is that they probably do exist somewhere. I don't necessarily have them at my fingertips, I don't think. If you found one in the SRDB, I wouldn't be surprised, but I don't have them at my fingertips.

MEMBER SCHOFIELD: I would assume this varied from one glove box to the next because, based on what the workers had to do at that station as to the level of shielding, they could or could not have or may have existed then. In the early days, like you said, I would have also assumed there wasn't shielding of glove boxes.

MR. SHEEHAN: To that point, can I add something to this gentleman's?

CHAIR BEACH: Could you state your name?

MR. SHEEHAN: Warren Sheehan, Mound, 56 to 89.

In doing a little survey work before I came down here recently to find out about this proton/neutron measurement that the surveyors made, I talked to one of our health monitors and, Dick, you hadn't heard this yet. I talked to Dave Hites, and Dave mentioned that back in 34, 34 was waste recovery, liquid recovery, and he talked about the measurements down there going below box line, and he pointed out how much higher it was down there.

And I don't recall whether they added shielding to it later on, but that exact condition did exist.

MEMBER SCHOFIELD: One other quick question. When they took their measurements, the health physics technicians took their measurements, I assume they were taking them right? I would assume they do two typical measurements, one through the shielding and one through the glove ports or whatever opening the technicians or the craft had to work through.

But did they record both of those measurements or just record only the ones through the shielding?

MR. SHEEHAN: I wish I could answer that. I talked to, I think, four different monitors or five, and I got two different answers. I mean about 50 percent. I think I talked to four, and what I was trying to find out, did they actually -- where did they record them.

Brant talks about they have like 46,000 measurements. I don't know where they came from because I don't believe they came out of SM. I mean, I don't know how many of them did. In my period there, which was before they really got into it, we weren't doing much of anything.

But what I do remember is we recorded it on a plastic card on the box, and the surveyors come along and update it. I asked them, did you ever record it anywhere?

Two of them told me no. Two of them told me they did.

CHAIR BEACH: I think Don has a comment.

MR. STEWART: Yes, I just have a quick question for Dick and/or Warren, and this goes back to what Ron was talking about earlier. Were these glove boxes actually in rows in the SM building?

MR. MADDING: They were in SM-31, and this is Dick Madding. That building, the glove boxes in that room were like the layout of this table without a glove box on the end. So you had a U-shaped configuration completely filling the room. There wasn't -- there wasn't room for two people to pass between the glove boxes and the wall, and by the time they added the shielding, you couldn't go through there in a straight motion. You had to go sideways to get through because they had so much shielding to the boxes.

MR. STEWART: And that was generally true in SM?

MR. MADDING: No. That was SM-35, which is the high-production area.

MR. STEWART: Okay.

MR. MADDING: Now, there were, like my finishing, my final assembly area happened to be in a U also. SM-35, I don't know how many boxes. What: 12, 14 boxes down one side and three or four across the one end -- it was a relatively big room and a lot of boxes.

MR. STEWART: But not a back-to-back configuration.

MR. MADDING: No, there was space in between.

MR. STEWART: Right.

MR. MADDING: You access the back. You access the back of the boxes, and the back of the boxes came off for some types of -- some boxes were that way, but you access the back of the boxes because that's where the filter, all the piping, the filter stuff came out.

MR. STEWART: The maintenance aisle in the back, yes.

MR. MADDING: There was a work aisle in the back about as wide as this table, seven feet, six feet, whatever the table is.

MR. STEWART: Is this also true for the PP Building?

MR. MADDING: Oh, no. The PP Building, I helped design PP Building, and I helped design the glove boxes for PP building, and PP Building was Mound's reaction to the problems at SM, the third level reaction.

The second level reaction was the SM addition where we went to a solid ceiling because the ceiling in the original SM building was a drop ceiling, a floated ceiling with panels in it, and it was deadly. In fact, in one of the big accidents, a person by the name of [identifying information redacted], who is still around, and he was in the analytical in a low area room. It ran into the shower, contaminated the whole building, and he was taking a shower with his respirator on and plutonium oxide was going down his back. He came out of the shower hotter than he went in. He had a stripe down his back.

This was the filter excursion, the worst accident at SM, in my opinion, which was late 1964.

MR. SHEEHAN: On [identifying information redacted] of 64.

MR. MADDING: [identifying information redacted] of 64.

CHAIR BEACH: I think Ron has the floor.

DR. BUCHANAN: This is Ron Buchanan.

At the PP Building, how were the glove boxes arranged at the PP Building?

MR. MADDING: PP Building was --

first, you must know that the glove boxes were served by an overhead conveyor, a fiberglass conveyor, and interspersed in the box lines was a service box which took a large, probably two-foot tall by 18-inch bucket which came down, which went up on an elevator and went up into that conveyor, and the conveyor ran in a big loop over six lines of boxes, each line running the length of the building and 100 feet, you know, maybe 25 boxes in a row.

DR. BUCHANAN: Okay. What were they in, a row of glove boxes? Was there glove boxes behind the person working at a glove box?

MR. MADDING: No, because you had the same situation that you did in SM-35 expanded vertically because you would have a row of glove boxes, and then you would have a service corridor, and that service corridor was almost as wide as this room, 12, 14 feet, and that was the service corridor for two lines of glove boxes. So you would have two lines of glove boxes and the service quarter, and then you would have another two lines of glove boxes and a service quarter and a third one.

DR. BUCHANAN: So you had workers back to back. You'd have a glove box here with a worker on this line. You'd have a row of globe boxes here with a worker facing that way.

MR. MADDING: Thirty foot, 25 feet apart.

DR. BUCHANAN: Twenty-five feet apart.

MR. MADDING: Because you had a row of glove boxes and then you would have the working area, and these were nice working areas. They were -- you had seven feet. These were big rooms, and then you had a corridor which accessed the room. Then you had the work area for the next set of glove boxes, and then the glove boxes themselves.

So you might have from the fronts

of the glove boxes, you would have 30 feet.

CHAIR BEACH: Can I inject here for a second?

Do you want him to draw a little diagram?

What I'd like to do is open that out. If you would sketch that out, and let's take a comfort break for 15. Let's do 15 minutes. Is that okay?

MR. KATZ: So at 12:15 we'll take it off mute again.

Eleven fifteen. Sorry.

(Whereupon, the above-entitled matter went off the record at 11:00 a.m. and resumed at 11:19 a.m.)

MR. KATZ: Okay. We're reconvening after a short break. This is the Mound Working Group of the Advisory Board on Radiation Worker Health, and we're in the middle of a discussion about neutron dose reconstruction and the configuration of the glove box rooms.

CHAIR BEACH: Okay. Where we left off was the glove box discussion as Ted said. Does anybody have any more questions or comments or where are we at on that?

DR. BUCHANAN: Do you want to explain this? Does Dick want to explain this to the rest of the group?

CHAIR BEACH: Sure, that would be great.

DR. BUCHANAN: Would you do that?

MR. MADDING: The PP Building was the third generation of design for handling plutonium-238. The first generation was SM building. The second generation was the SM addition, which was attached to the SM building and had a hard ceiling, which was a huge difference. A lot of the SM building's hazard was due to the drop ceiling. Something that happened at one place in the building would go through the ceiling and come down in another laboratory that the people were doing what they should be and had no idea that it was going to happen.

When I went into SM in December of 1963, you didn't have rad worker training. What you had was a mentor. My mentor was a guy by the name of [identifying information redacted], [identifying information redacted], and he told me. He said, Madding, you may wish you had gone to Vietnam before this is over.

(Laughter.)

MR. MADDING: He said, this is war. He said, and if you want to be safe, he said, you're going to know everything that's going on in that building on the hot side before you go over there. You're going to know who's pulling trash, how competent he is. You're going to know what maintenance is going on. You're going to know everything that you had better know. You had better do your situational awareness and know what's going on before you go over there.

So I made a few trips to the

library and a few trips to the warehouse over on the other hill, and missed a lot of the accidents, but you couldn't miss it all in SM building because they ran 24/7 starting in 1964 through 1965, trying to get this failed design to work.

The building was designed for a liquid, low quantities of liquid. SM building we're talking about. Two people from SM, from Mound, [identifying information redacted] and [identifying information redacted] spent a year at the design agency pre-1961 to try to get this thing to work, and the liquid concept wouldn't work.

And today we know that with the calcining and the self-heating and the gaseous build-up from the nitrogen and the constant nature of plutonium nitrate which will eat through welds and anything else, that it would. So they switched to the oxide, but by that time the building had been designed for liquid, small quantities of liquid, and so they converted. The building went hot in 1961, trying to make this concept that was virtually impossible to make.

In fact, it was impossible to make in the original design, and they had to change the specifications in order to get anything out of it, and then they only got a few percent.

So during that SM era, you created, if you've seen the attachment to our White Paper, in February of 1966 a monthly report talked about 1,788 drums, 1,788 55-gallon drums of trash ranging from nothing way up to--

CHAIR BEACH: Pardon me. You just said attachment to your White Paper?

MR. MADDING: Yes.

CHAIR BEACH: Okay. Which White -- I'm looking at --

MR. MADDING: You're not looking at Brant's White Paper.

CHAIR BEACH: No, no, no. Which

one are you talking about?

MR. MADDING: You're looking at Warren Sheehan. Warren Sheehan and I, the SM paper --

CHAIR BEACH: Okay. So is this the one, the document?

MR. MADDING: That's the one. That's the one.

CHAIR BEACH: Okay. I just wanted to make sure everybody knew that it's the two documents that I sent out that were released from DOE last week. So those are the two he's talking about, and not to be confused with the other White Papers.

Thank you.

MR. MADDING: And by the way, the SM building up until the addition was put on in 1966 was used exclusively for DoD purposes. There was no space, there were no RTGs and that kind of stuff. And this has been declassified, and I have declassified documents which show this, some basic aspects of this.

So the heat source work as far as SM building didn't really come into play until late in the game, 1967, I think there was a kilogram for space applications went out, and then in 1968 we had the SNAP 27, which is higher numbers, but during all of this time from 1963 on through material in kilogram quantities was being shipped into the building. Nothing was going out except as drums of trash and a lot of hold-up in the building.

So the point I want to make is that building was loaded and a lot of neutrons everywhere. In fact, it was so severe that the badge board, dosimeters, that was kept in the corridor outside where you went in the change room, they had a background badge, a background dosimeter which was kept on there, and I remember vividly because it had a metal clip on it like you would use on your gloves, and I skied, and I kept using those metal clips and I always had my eye on that clip on that dosimeter.

But the background, and I don't know whether the background -- how that background information was used. Do you know, Brant, if that background badge board, background information was used in any way?

DR. ULSH: I don't know about the specific instance, but in general, the background is subtracted from the badges that the workers wear, in general.

MR. MADDING: Right. The background got so high that management people who were running the building in 1964 realized that this could be a problem in the dosage calculations and everything. So they moved that badge board 100 feet out to the guard shack to get it out of the area.

So the potential was there. The potential was in the building for high dosage. In fact, at that time the limit was three rem a quarter. Many people, in fact, in the attachment to our White Paper we mention one guy in specific. He was done six weeks into the quarter. He had gotten his three rem. I personally in the three years I worked in there, I ended up with 25 rem -- that's with a big R -- external exposure.

CHAIR BEACH: Once again, can you refer to yours as a document? Otherwise we're going to have people requesting --

MR. MADDING: Right.

CHAIR BEACH: -- White Papers.

MR. MADDING: Right.

CHAIR BEACH: Okay. Thank you.

MR. MADDING: The SM --

CHAIR BEACH: They are both right here.

MR. MADDING: Right. I want to see how it's typed. I just want to see the title so that I get the title right. Special Metallurgical Building, Mound Laboratory, 1961 through 1968, and there is an attachment which is a weekly report from Huddleston, who was a building manager to Bradley, and I don't see that in yours.

CHAIR BEACH: Those are the only two that were cleared and given to me.

MR. MADDING: Okay.

CHAIR BEACH: Do you have a question?

DR. MAURO: Yes, I do.

CHAIR BEACH: Okay.

DR. MAURO: When you had mentioned the dose limit of three rem per quarter and --

MR. MADDING: Nineteen sixty-four.

DR. MAURO: In 1964, I'm familiar with that dose limit. Of course, that dose limit was a combination of both photon and neutron exposures, I presume. Now, when they did the neutron contribution to the dose, obviously they were aware at the time that some of that neutron dose that was experienced was from neutrons that were detected from the track to the NTA film, but also there was a contribution from neutron flux that the energy was relatively low and did not cause tracks on the film.

So health physicists in charge at the time had to use some type of judgment, I'm presuming, that there was an add-in, some contribution. Now, in order to do that, he had to have some knowledge on what he believed was a reasonable distribution of the energies of the neutrons, and that has to be in my mind based on some type of measurements that someone made, and I haven't seen those measurements.

MR. MADDING: I can't really answer your question as to what kind of factors were put in. Brant has a lot of information on that, I believe; is that right?

DR. ULSH: Yes, yes.

MR. MADDING: He has got a lot of information on that. He is far, far more qualified to answer than I am. I do know that the badge reading cycle was compressed as you got closer and closer to the limit. It might be a week, and then it got down to days. They were bringing people from the other hill over, totally unqualified people, Ph.D.s, anybody. You were subject to being assigned to SM, and there was no way out of it. It was a unanimous decision, and you went.

And there were a couple of people who went out the door because they wouldn't go, and there was one person who when he went to lunch or whatever, he taped his badge, he taped his dosimeter to the bottom of the box to get out early. The only problem was he forgot and left it, and all of a sudden they come up with this huge death-dealing reading and forced him to admit what he had done, and he may not have been the only one. He's the only one I know of that was documented.

But this was a bad place to be, and the people on the other hill, on the main hill, they were hearing the evacuation, the sirens go off, and they evacuated SM building, evacuated SM building once a week, you know.

I mean, it's hard. Looking back, of course, I'm fresh out of college. It was an adventure.

CHAIR BEACH: Does anybody have any other questions pertaining to the glove boxes?

DR. MAKHIJANI: Yes, I have one. In the SM building you said a lot of material was coming in and they were trying to transition into a new process and it wasn't working. So everything basically was being stored as trash in the building, and then it was taken out of the building.

MR. MADDING: Yes.

DR. MAKHIJANI: If you look at a typical drum of trash, trash in it --

MR. MADDING: Plutonium, yes.

DR. MAKHIJANI: -- and if you look at a typical drum of trash, what else would be in a typical drum. I mean, would it be solutions? Would it be --

MR. MADDING: No, no solutions, no

liquid, no liquid.

DR. MAKHIJANI: Would it be solid trash, paper?

MR. MADDING: It was paper. It was paper, metal, syringes from the analytical department. In fact, one guy in R&R accidentally injected himself with a syringe when he was cleaning out a box.

CHAIR BEACH: Trash .

MR. MADDING: Yes, a lot of gloves.

MEMBER CLAWSON: Anything that had an attempt to clean up a mess or clean up an operation?

MR. MADDING: No, just normal trash that would come out. This material was run through sieves for sizing, particle sizing. You run it through a sieve stack.

Do you know what a sieve stack is? Okay. Those things eventually didn't work right. They got clogged and everything. Trash.

The big problem was for the first couple of years or first four or five years, there was a no discard policy. Plutonium-238 was hard to come by. I mean, they didn't make it before 1960 or whatever, 58, 60, and it was very difficult to come by. So the AEC had a no-discard policy.

CHAIR BEACH: So one last thing.

MR. MADDING: Yes.

CHAIR BEACH: While you're here; we talked to Warren. Warren Sheehan and Dick Madding are former Mound workers. They supplied documents to the Work Group which everybody has. I have given them out to everybody.

Is there any other comments? I know you were going to summarize possibly or just note that they're here. We have them.

MR. MADDING: Warren has some comments. I have made the comments that he was going to go first and hand it over to me and that I was going to make.

CHAIR BEACH: Okay.

MR. MADDING: Which is the building had a lot of potential. By the time you got the PP Building, all that is gone.

CHAIR BEACH: Okay.

MR. MADDING: Full amount is coming in, shielded, conveyor system so that there was no pulling trash. I don't know of any release in the PP Building. Do you, Warren? No releases, none.

CHAIR BEACH: Okay, and you're going to be here the rest of the day in case anyone has other questions.

MR. MADDING: Here for a while.

CHAIR BEACH: For a while.

MR. MADDING: Warren does have some comments.

CHAIR BEACH: Okay.

MR. KATZ: Before we get to that, Dick, I mean, one of the reasons before we broke, you drew a diagram up there to explain that there was a lot of muddle about how the glove boxes were configured.

MR. MADDING: Okay.

MR. KATZ: And actually, although you clarified it for people in the room while we were on break, but for the record and for the people on the phone, they really haven't heard the discussion of actually how things were configured with the glove boxes.

CHAIR BEACH: Thank you. That's correct.

MR. MADDING: This is PP Building. It consists of six box lines running the entire length of the building, maybe 100 feet. Those boxes --

CHAIR BEACH: Do you have an idea of how big the building was? Forty feet or --

MR. MADDING: Oh, no, no, no. We're talking 150 feet long by 120 feet wide.

CHAIR BEACH: That's the perspective I wanted for the glove box. Thank you.

MR. MADDING: Right, right.

MR. FITZGERALD: You said the glove boxes were very close to the walls or I think you said --

MR. MADDING: No, no. The glove boxes were backed on a service quarter. So there was one service quarter, and I haven't drawn the one over here, but there was one service quarter for each set of boxes. This has -- the two end lines had a service quarter which only served one set, and then the other four boxes had service quarters between them.

MR. FITZGERALD: Was this configuration pretty stable? I would assume it got modified as --

MR. MADDING: Oh, no. There was no mod.

MR. FITZGERALD: No modification.

MR. MADDING: No modification. This building was built with prestressed concrete T beams for the ceiling and the floor. The services were all done from the basement. There was no overhead lines, no overhead services. Everything came up through the floor in the service areas behind the glove boxes. And that's why there were no incidents, no releases, no internal exposures at all out of PP Building, and very little neutron, relatively little neutrons.

DR. MAURO: This is a glove box.

MR. MADDING: That's a glove box. This is a person working.

DR. MAURO: There's the source.

MR. MADDING: Right.

DR. MAURO: And the shielding is between this source and this wall, and flocks of neutrons is coming out and striking --

MR. MADDING: Right, but this shielding in PP Building was relatively small, maybe four inches maximum.

DR. MAURO: Okay. You mentioned something about six inches before.

MR. MADDING: Six inches in SM building and more.

DR. MAURO: So it was different.

Now, then you have the space from here and you have another --

MR. MADDING: Sure. You've got --

DR. MAURO: And the space from here to here is what?

MR. MADDING: Well, it's 25 feet.

DR. MAURO: Twenty feet.

MR. MADDING: To the next one.

DR. MAURO: And now -- but there's 21 here, this neutron flux. There's a person over here, there's a person over here, there's a person over here.

MR. MADDING: Well, generally not.

DR. MAURO: Oh, no?

MR. MADDING: You might have two people in a room, maybe three maximum, but each one of these rooms, you know, each one of these was a room down along here the entire length of the building. There might be four or five rooms down.

CHAIR BEACH: Are you talking rooms with shielding walls?

MR. MADDING: Yes, rooms with shielding walls, hard shielding and painted with a special paint that would clean off, a special glossy paint that was used everywhere, ceilings. The outside of the building was blocked, and all that was coated with this paint, special paint, and very easy to clean up.

DR. BUCHANAN: This orange? Is that concrete or plastic?

MR. MADDING: No, no, no, it's drywall, basically drywall with this special paint on it. And the ceilings were high ceilings because you had this big fiberglass conveyor. You had this big fiberglass conveyor which started here, and it ran here, and it ran here, and it ran here, and it ran here, and it ran here, and then it came across and started again.

DR. MAURO: What is this?

MR. MADDING: That is the typical SM building.

DR. MAURO: Are these glove boxes?
MR. MADDING: Yes, those are glove boxes.
DR. MAURO: And they were dealing with neutron sources in there?
MR. MADDING: No, you were dealing with plutonium-238 dioxide.
DR. MAURO: Okay.
MR. MADDING: Nitrate or dioxide.
DR. MAURO: And so this is a complete different configuration.
MR. MADDING: Completely different thing, completely separate.
DR. MAURO: Are we worried about this, too?
I know we were looking at this one. I was wondering.
MR. MADDING: Yes. We got off onto the difference between PP Building and the SM building.
MR. FITZGERALD: Now, SM and PP, is that it? I'm sure there's other

operations.

MR. MADDING: Well, you had our building. You had a lot of small work going on in our building. Our building had hard ceilings, but our building did get one thing. When the big heat source push comes on and mother nature is calling for the spacecraft to go and you don't have the fuel, they put a plasma torch down in our building, and I worked on that one, too.

And so they had kilogram quantities basically in one room in our building going in and out of a torch box, of a plasma torch box.

Go ahead.

DR. MAURO: So we've got glove boxes here.

MR. MADDING: Right.

DR. MAURO: Six feet from this wall to this wall.

MR. MADDING: Yes.

DR. MAURO: A person standing

here.

MR. MADDING: No, no, no. Everybody is on the outside. This is service.

DR. MAURO: Oh, thank you.

CHAIR BEACH: That's the one, John, where you couldn't walk straight.

MR. MADDING: Yes, you couldn't walk. Once they got the shielding on, you had to go sideways.

DR. MAURO: Okay. Got it.

MR. MADDING: And Building 35, was you know, I mean, SM-35, it was to the SM building what the SM building was to the other hill. Nobody at SM building wanted anything to do with 35 because of the radiation and the potential for release, and there were a number of releases.

DR. ULSH: Dick, just to clarify for people who aren't familiar with it, when you say 35, you're talking about Room 35 in that building.

MR. MADDING: Room 35 in SM

building, right.

MR. FITZGERALD: Now, you did say earlier that they hung Benelex to the point where, you know, a certain proportion of the kind of exposure they were reading. You're saying at the very end there wasn't as much of being hot as far as shielding.

MR. MADDING: No, the PP Building was designed --

MR. FITZGERALD: No, I'm talking about SM. You were saying that --

MR. MADDING: Thirty-five was the most heavily shielded room in the building.

MR. FITZGERALD: It was almost empirical where they were basically putting as much shielding as they could.

MR. MADDING: They put it on there till you couldn't do the work.

DR. MAURO: And how thick was that?

MR. FITZGERALD: Pretty thick.

MR. MADDING: There were some --

there was a couple boxes here across the end of the U that did have close to 12 inches on them. The other major accident happened, I believe this was in 68, Warren?

MR. SHEEHAN: What's that?

MR. MADDING: The Talbert --

MR. SHEEHAN: Oh, Talbert 35. That was 35.

MR. MADDING: Well, I know, but it was at the end of the year here and what they had was the glove box gloves had a special coating. What was that coating, Brant? The coating on the glove box gloves, there's a special name for it.

DR. ULSH: I don't know.

MR. MADDING: I can't --

MR. MORRIS: That would be Hypalon probably, Hypalon probably.

MR. MADDING: That's it, Hypalon, Hypalon-coated gloves. So they did recovery on these Hypalon-coated gloves, and since you don't want to put any kind of moisture, you don't trash out, you don't send for burial anything, any kind of liquid or moisture.

They had some furnaces. They had a big, tall furnace in here that they hung those gloves in to dry them out. Well, what they didn't realize was the Hypalon out-gasses, and the furnace temperature got up a little bit maybe more than it should, and suddenly with three people in that room it exploded and blew the front off and dropped these pieces of shielding which actually knocked the one guy out.

Another guy who lost his respirator, one guy took off. He was out of there. The other little guy about my size handled the 300-pound guy that was knocked out. He dragged him out of there without a respirator on, the most contaminated man in there, and he died a year and a half ago, and he was not a claimant, right? You said he was not a claimant.

DR. ULSH: I can neither confirm

nor deny.

MR. MADDING: Okay. I thought I heard you say he was not a claimant.

CHAIR BEACH: All right. Any other questions.

MR. FITZGERALD: Another question. You had said something to the effect that if you, I guess, were a rad worker at the time of the national production cycle, where 24/7 you had to do your time and ask them to -- I mean, it sounded there was a lot of -- --

MR. MADDING: Not even a rad worker. Any scientist on the other hill, you could be down there doing research in NMR, and you got sent to SM.

MR. FITZGERALD: But just about everybody on the plant were rad workers, not just the --

MR. MADDING: No, they weren't rad workers.

MR. FITZGERALD: They weren't rad workers?

MR. MADDING: I wouldn't call them -- I don't know that I would.

CHAIR BEACH: Anybody that was working on that.

MR. MADDING: Anybody that was capable, anybody that was a body and hands that was capable of going to SM, and this included women as well as men, they went up there.

CHAIR BEACH: I guess I wonder if they would be considered visitors or if they were badged.

MR. MADDING: Oh, no, they were badged. The only visitors were the thing that Brant talked about where you would have a maintenance guy from the other hill come up in an unusual circumstance.

DR. ULSH: I have a question.

MR. MADDING: Yes.

DR. ULSH: The drawing there that you have that represents SM-35 --

MR. MADDING: Right.

DR. ULSH: -- where you've got thick shielding, if I were working in there would I be badged, NTA film badged?

MR. MADDING: Oh, yes. I mean, there was -- anybody who ever says that people weren't badged and un-whatever dose was incurred by people in the SM building, that did not occur.

CHAIR BEACH: When did they badge with NTA?

DR. ULSH: NTA film, from beginning of operations up through --

DR. BUCHANAN: Seventy-six.

CHAIR BEACH: From the beginning.

DR. BUCHANAN: Yes.

CHAIR BEACH: Not backwards.

MR. STEWART: Just to clarify, that's also true for bioassay, including bioassay. That worker would also be monitored for any intake.

MR. MADDING: Yes, absolutely, absolutely. You peed in a jug every so often.

MR. STEWART: It's consistent with the records I've seen. I just wanted to get that on record.

MR. MADDING: Absolutely. There was no slackness on management's part or on the health physics people's part about keeping track, and part of the reason is because in 1964 and the first part of 1965 until the design was changed to a workable design, they had people were running right up against the limit, and they wanted to know everything that was going on. They were very careful.

DR. BUCHANAN: And what years was this that they were calling people off the hill?

MR. MADDING: Sixty-four and the first half of 65, 1964 and the first half of 65. After 65 when they went to the different concept, the throughput completely reversed, and so there was no more really high quantities being put in and nothing coming out the other end.

The problem was that you had all these lines, all these boxes and everything had been heavily contaminated, and that contamination was still there, but it wasn't kilogram quantities. The kilogram quantities were in trash barrels outside the building, and the first place they went was where the PP Building was to be built, and when the PP Building was under construction starting in 1967, they were moved out by the thorium storage building to pads out there.

One of my other jobs -- I love jobs -- was testing DOT containers.

CHAIR BEACH: Anybody else have any other questions?

DR. ULSH: Yes.

CHAIR BEACH: Okay.

DR. ULSH: Think now in terms of N/P ratio. Do you know what I mean, neutron to photon ratio?

MR. MADDING: Yes.

DR. ULSH: People who were badged,

and let's just take that SM-35, do you have an opinion as to whether or not the N/P ratio that would have been seen from those workers, do you have an opinion about whether it would have been higher or lower than other people, like visitors?

MR. MADDING: Well, what you had, what you had in the SM-35 was a process with plutonium dioxide, and it was essentially all plutonium dioxide. The outside contaminant to that was nothing that was going to change the neutron to photon ratio.

So as far as the neutron to photon ratio, I mean the whole building was plutonium-238 oxide. There wasn't anything else in there, and there wasn't anything added on that would, in my opinion, change anything. The only thing was the shielding was quite different in different parts of the building.

For example, right next to SM-35 was SM-39, which was a manipulator operation, which was the original place they made the plutonium-238 metal sources, and they kept it to milk the cow for [identifying information redacted] and other people to get the U-234 and --

CHAIR BEACH: Okay.

MR. FITZGERALD: Let me ask you another question. You indicate that really in that time period, about a year, whatever --

MR. MADDING: Sixty-four to mid-65.

MR. FITZGERALD: -- that really they brought people in. Based on the film badge, they dosed them up to, I guess, three rem a quarter or close to it. Well, they actually used more readings to make sure they didn't cross the line, but they kind of came up to the line.

MR. MADDING: Absolutely.

MR. FITZGERALD: And then they'd annotate.

MR. MADDING: They shortened the badge reading time as you got closer to your limit.

MR. FITZGERALD: Right.

MR. MADDING: They started out with like two weeks, and as you got close it came down and--

MR. FITZGERALD: And they rotated a group of workers in --

MR. MADDING: Yes.

MR. FITZGERALD: -- and did the same.

MR. MADDING: Yes. Everybody got through during the time in the barrel. Everybody got their shot in the barrel.

MR. FITZGERALD: You say everybody. I mean it was really almost everybody, and you're talking about --

MR. MADDING: They went over and you were doing some innocuous research on NMR and you were a Ph.D. and they thought you were capable of doing this, you were up there until you burned out, and then you went back to your regular job, and this went on for a year and a half.

And it was three shifts a day, actually two shifts, really, of really hard work.

The other incident that I was talking about where they were boiling off the ethanol and the glove box blew off as one of the guys was walking by and blew him through the double doors into the SM addition. He was fine. In fact, I just recently saw him here last year.

Well, he was -- he was a QC guy, but he was assigned the third shift up there, and he just happened to be making his rounds and happened to be walking by that box when it let go, and bang.

And of course, the consequence was that they decided, hey, while the building is all contaminated, now is a good time to change the filters in this Unit 35, and that's when the November 11th actually --

CHAIR BEACH: Anybody, any other

questions for Dick?

Thank you very much.

MR. MADDING: Okay.

CHAIR BEACH: All right. So where are we now?

MR. MADDING: Warren has shot the barrel on our SM?

CHAIR BEACH: Yes. Warren, do you want to just go now or do you want to wait till we're finished? Go ahead.

MR. MADDING: It ties together with what I said.

CHAIR BEACH: Why don't you go ahead, Warren?

MR. SHEEHAN: My time in the barrel?

CHAIR BEACH: Do you want to sit?

MR. SHEEHAN: No, no.

CHAIR BEACH: So Warren Sheehan.

MR. SHEEHAN: I think most of you know who I am by now, but anyhow, Warren Sheehan. I worked April 56 to June of 89, through June of 89.

So I had experience in the SM building as the initial health physics supervisor when the place went hot, and then I was taken back out of there to -- I guess the best words I ever heard from [identifying information redacted] was he called me one night and he said, I'm going to split the dosimetry group and bring you back and head up the bioassay group, and he said he proposed it to his boss and he said, [identifying information redacted] wants to put that like a duck to water.

Well, not only he did; I did. So that was my ticket out, but I soon went back when the second shift went on because when they put the second shift on, then I had to go back, and I never could remember when I did actually get myself extracted from the place.

Nevertheless, I had some experience not during the period Dick was there, at least some of the period.

Anyhow, one remark I would say about the location of the facility there, that had the AEC's thought process that was prevalent in 69 and 70 been prevalent in 59 and 60, that project would have never been located there. It would never have been there at all. The SM stack was only 200 feet from a public road, and there was the state park from which the lab got its name, Mound Lab. It was across the road. There's a five-acre state park there. That's where the mound is, and that whole park was within 1,200 feet of the SM stacks.

So we didn't have miles to dilute our emission sends. They were right across the road, very, very close. So eventually they decided they didn't want us to work open plutonium.

Well, anyhow, let's move on then to dosimetry, which I think is probably what you people are primarily interested in, and when I first approached Chairperson Josie here about putting together a paper, I promised her I didn't want to get into a technical discussion, and I still don't because I don't believe I could match wits with these people. My history is four decades old, and three decades as far as health physics. So you guys have moved down the road quite a ways from where I was.

But I was a, I want to say bystander, not a bystander; a spectator at the time this took place. So these are observations that I want to make regarding dosimetry and on the evaluation process.

As I say here, it's been 40 years since I worked in the field, but that said, the state of the art that has been employed 40 years ago has not changed. Only the evaluation processes have, and here are some of the major issues I see with bounding the dose.

Number one, only yearly dose summaries are available, and I picked that up out of your TIB documents. Without individual badges on, badge cycles are only a guess, and the time of exposures, counting first as acute, also are not know. The time interval between exposure and the film being developed are a major concern.

I'd like to, if you will allow me, to read a letter I got from a fellow by the name of [identifying information redacted]. Now, [identifying information redacted] was a certified health physicist that worked at Mound from 61 to 70, and he related this story to me the other night over the phone, and I said, well, [identifying information redacted], would you write that up and send it to me?

He and his coworker were doing neutron modification studies using the incoming shipment from Savannah River, and this is a story he wrote about that.

The thing that I remember, I did most of the handling of the material and [identifying information redacted] ran the counter. Of course, I wore my regular film dosimeter, which as I remember was monthly issue. It may have been bi-weekly, and [identifying information redacted] wore a visitor film badge.

At that time [identifying information redacted] was assigned to, he was working in, I think, Advanced Devises. So he was a member of the Criticality Committee, but he wasn't assigned to a radiation building. So he wore a visitor badge.

I was interested in just how much neutron exposure dose I accumulated on this project. The thing that sticks in my mind is that most visitor December film was developed soon after the work was accomplished, and my dosimeter developed at the end of the dosimeter wearing period, which I think was monthly.

[identifying information redacted] dosimeter result was a relatively high reading, and mine much, much lower reading, just the opposite of what was expected. The light came on that the neutron backfeeding was much greater than ever anticipated. As a result of this knowledge, I had some experiments done where neutron film was exposed and then developed, read at different intervals afterwards to get a handle on the fading problem.

As a result of this study, the neutron calibration film at Mound Lab was exposed to a neutron source in the middle of the week instead of immediately prior to developing the film, which would more closely assimilate the worker's exposure and dose conditions.

So that's sort of how that little affair with his own personal experience resolved with them changing the calibration procedures that were used.

MR. STEWART: Question, Warren.

MR. SHEEHAN: Yes.

MR. STEWART: What time frame was that statement?

MR. SHEEHAN: Well, this, I gather from what tells me was 6/24. If I'm correct, it's MLM-1340, and this was June of 66.

MR. STEWART: Right, and 1968 is when they implemented the protocol to do step-wise calibrations.

MR. SHEEHAN: Right.

MR. STEWART: So that work resulted in change.

MR. SHEEHAN: Right, right. There was a couple papers that were put out, 1490, which was a plutonium fluoride source which showed 33 percent in one week, 56 percent in two weeks. Then that was followed later on by another study which involved a plutonium, moderated plutonium oxide source, which is 16 and 30 percent. Then the combination of those two papers was reported in Health Physics in Volume 17, 1969.

Anyhow, I lifted one statement out

of there. Contrary to what was expected, the results indicated less fading in the Pu-2 moderated source, which was considered to have the lesser energy, but they were confused on that.

Well, anyhow, that kind of is the story, the introduction of the track fading. It was known to exist, but not to exist to the extent that it did because all of our experience heretofore in the higher energy, PuBe or POBE -- I'm sorry -- POBE, which was around four and a half MeV and gave you a nice track to read, but this stuff is different.

While we're on tracks, I lifted something out of [identifying information redacted] and [identifying information redacted], the only thing I could find. I wish you people could see an SM film of proton recoil tracks that came out of our SM field failed exposed people. It wasn't anything like this. It was nothing like that. I mean, that just doesn't give it. That gives you a false impression of what you're looking at.

Now, a polonium beryllium track will look more like that. I mean not even that extensive, but the tracks that we were seeing, not only were the tracks with a slimmer -- slimmer? -- short; they were occluded, if you will, by a photon fault. Those films had an awful lot of trash on them, and I think one of the ladies best expressed it, and I'll clean it up. She said it was like trying to pick fly dung out of pepper, and I couldn't have found a better term, although she used a more earthy term.

So anyhow, okay. Track failure. So we have conflicting results even on that, and then we have faults, which was a real problem, and no consistent neutron/photon ratio. I don't know. Brant has a better handle on it than I have by far, I'm sure.

Calibration methods cannot duplicate all field conditions, and I think Ron Buchanan addressed that a few meetings ago about that, and I read the account in the transcript, and I thought Ron was right on target with that.

I don't know how you do your calibration under field conditions, but the question is still there. If you're going to get the scatter and all of these other things, it's hard to go into a sterile situation, such as normally is done in the calibration.

So finally, what corrections were made at various time periods, that is, track fading, and flux quality factors applied are not clear.

One other thing is I think we're right at the inflection point on these neutrons at SM building, inflection point meaning around one MeV, and you know, it's an inflection point not only so far as dose relationship, but it's also an inflection point so far as film response.

So we've got both of those things working against us, and the more shielding you put up, the more you force that in that direction. So I really think that's one of the pivotal problems we have in bounding the dose, is not only the dose, say, flux, the quality factor, while I tend to build -- they used to be called RVE, but quality factor -- and I assume the fading.

Well, I'll leave it there on the external.

CHAIR BEACH: Warren, I think you have a question.

DR. BISTLINE: We've been talking about shielding. Did you have shielding over the glove ports in the well? Because at Rocky we actually had doors that you, Benelex doors that you could shut.

MR. SHEEHAN: I don't think so, but Dick?

MR. MADDING: They had plugs. They stuck a Benelex plug with a big handle on it at the end of the glove ports.

DR. BISTLINE: When did that --

MR. MADDING: As they added more shielding, the plug went right along with the shielding and the plugs got thicker as the shielding got thicker.

CHAIR BEACH: How thick were the plugs? Do you have any idea?

MR. MADDING: The same as the shielding. If you had six inches of shielding, you had a six-inch plug.

CHAIR BEACH: Okay.

MR. FITZGERALD: And you made the point that even though it got cumbersome, and you're talking about 12 inches, I mean, that's hard to imagine how you would reach, but you said that you went ahead and you just went with it anyway.

MR. MADDING: It depended on what you were doing. If you were doing the furnace like drying the gloves, I mean, the only thing you did, the furnace stuck out from the back wall a foot and a half or so or you had it positioned where you could reach it, and you went in and opened the door and took some gloves out and put some other gloves in, and closed the door and you went on.

This was not -- except in the production days of 35 where they were actually processing the material, you were not standing in front of a glove box working six hours a day.

DR. BUCHANAN: In PP Building you didn't?

MR. MADDING: PP Building was probably even less, except for the analytical workers who worked with very low quantities, because the different recovery operations they did you would digest material or you would put it in an ultrasonic cleaner and turn the ultrasonic cleaner on to clean the material off of it.

The different processes that you did in recovery did not really require you to stand there constantly eight hours a day or six hours a day.

CHAIR BEACH: You had mentioned shielding some times more than 12 inches. Did you --

MR. MADDING: No, no, no. I didn't say more than 12 inches.

CHAIR BEACH: So just 12 inches at the --

MR. MADDING: I don't know of anything -- I don't know everything about it because I stay out of there.

CHAIR BEACH: Okay.

MR. MADDING: But I as in there, and like I said, I can recall specifically going through that U and I didn't see how the 300-pound guy ever got in there and worked.

CHAIR BEACH: Okay.

MR. MADDING: He had to be pushing up against it, seriously.

CHAIR BEACH: -- to manipulate if you had that situation where you had 12 inches?

MR. MADDING: Probably not. For

the most part you set your equipment up inside the boxes to suit, but it wasn't -- you know, that was the recovery operation beginning in mid-'65 through mid-'68 when the building was shut down. A lot of the boxes were converted for recovery operations. So more or less in line recovery trying to clean up boxes which had been contaminated in the high process period, 64 through mid-65.

CHAIR BEACH: Okay. Thanks.

Any other questions? Okay.

DR. MAKHIJANI: When did you do this experiment to determine the amount of fading and you found it was more than spots?

MR. SHEEHAN: Well, that, I think, was six -- the reports that came out, and I'm going from that, one of them was 67 and the other one was 69.

DR. MAKHIJANI: And then did anybody go back and correct the old number?

MR. SHEEHAN: I'm not aware of it.

MR. MADDING: It's hard to correct

something that's been published and 700 copies been sent out.

DR. MAKHIJANI: Okay.

MR. SHEEHAN: You're just asking whether or not the doses were.

MR. MADDING: And we have copies of all those papers if you'd like. We also have them on a flash drive if you'd like them that way.

MR. STEWART: The SRDB as well.

DR. ULSH: Actually I was wondering if you'd like the Health Physics citation. I can give it to you.

DR. MAKHIJANI: Oh, great.

DR. ULSH: I'm looking at it right now, at least the 1969 one.

DR. MAKHIJANI: Why don't you just send me an email.

DR. ULSH: Sure.

CHAIR BEACH: Okay, Warren. You had some other comments?

MR. SHEEHAN: Well, before I leave

that, I will say that Room 39, they did have a manipulator box and done with water shielding. That was a metal reduction where the middle one was made. The 4X bond in that process, it gave off a pretty high level of neutrons, but that was the only manipulator operation in the building that I know of.

MR. MADDING: That's right.

CHAIR BEACH: I just know how operators are. We tend to create tools that we need to do our job. So I'm sure Brant knows what I'm talking about.

MR. MADDING: There was some of that being done. I'm not saying -- you know.

CHAIR BEACH: Well, that was what I was asking.

MR. MADDING: And in the manipulator line particularly. I worked in there also.

CHAIR BEACH: Well, I didn't mean --

MR. MADDING: They had all kinds

of --

CHAIR BEACH: I just -- anyway, that's okay.

MR. SHEEHAN: Okay. Well, on internal, on this subject I feel a lot better. Maybe that's because I'm a bioassay man, but I feel much better about the data that goes into trying to bound the dose here, but what your major problem is here is the modeling process, and when you get into the modeling process, then you have to concern the mode of entry, the particle size, distribution, the chemical nature, type S and so forth, again, chronic or acute exposure, and the end effect of chelating therapy on the internal organ distribution, and finally, last but not least, the fact that we did not have a lung counter at the time that we were in operation and we really could have used it certainly.

And we used to have to send people out to Bob here and have him count it, but different places. So we didn't get a lot of lung counts done unless we had a real crisis, and then we would send them also.

And the lung counter, body counter, well, I don't know that in the earliest years that the science had been developed that far, but at least with the low energy stuff that you had to look at relative to P-38, that was the problem there. I mean, if you had high energy things, you know, they were around. People were doing it, and it was being reported on.

But I go back to a paper that [identifying information redacted] -- I don't know if many of you people know [identifying information redacted].

If you knew [identifying information redacted], you knew [identifying information redacted]. He published a paper. He was at National Reactor Test Site, and in 1962 he gave a paper at Savannah River, and it was titled What Value Urinalysis, and he based it on the fact that if you had insoluble material that was in the lung then you'd never see it in the urine. So he was campaigning for routine fecal samples. Well, that went over like a lead balloon. There was a lot of controversy about that, but he had a point. No doubt about that. So anyhow.

Well, then finally, and I'll wrap it up, our remote location did not help us much there. It created problems, but in summary, to talk about the dose situation, and I don't want Brant to take offense to this, but --

DR. ULSH: Don't worry. I won't.

MR. SHEEHAN: -- to bound the neutron dose when using 45-year old NTA film data is like, as the old saying goes, trying to make a silk purse out of a sow's ear. The data was flawed to begin with, and with all of the reworking of this data 45 years later, with all of the best intentions does not change these facts. In today's computer language we say garbage in, garbage out.

While we did have a credible monitoring program, it was what it was, flawed method. So you can't make much more of it out of it.

Finally, Congress signaled a strong desire to rectify damages by stipulating claimant-favorable handling of claims. Employees in the SM building endured exceptional hardship and risk while working with kilogram quantities of the most hazardous isotope known not in caves, but in glove boxes. They deserve no less than SEC status.

CHAIR BEACH: Thank you.

Any other questions for Warren?

I do appreciate you braving the weather to come out and join us today. Thank you.

MR. SHEEHAN: Very good.

CHAIR BEACH: Thank you very much.

MR. SHEEHAN: I hope we didn't torpedo your meeting.

CHAIR BEACH: No, no, no. You're

fine.

MEMBER CLAWSON: Actually this meeting is about you as workers and so forth. So we greatly appreciate what you have to say to us.

MR. SHEEHAN: Well, you know, one thing, Brad, I think Dick and I probably share the same thing. Once you worked at SM you were branded. It was sort of like it was in your brain and you can't get it out, you know, and so we were both branded.

MEMBER CLAWSON: Appreciate it.

MR. SHEEHAN: Thank you.

CHAIR BEACH: It's ten after 12. It would be nice if we could wrap up neutrons before lunch, but I don't know if SC&A has some further comments.

DR. ULSH: Well, I would just like to make one comment. Brad asked me at the break if Warren and Dick worked for ORAU or NIOSH. Just to clarify, they don't. I mean, they're here of their own accord. In fact, I didn't even know they were coming today.

But I would like to take the opportunity to thank both Warren and Dick not just for coming down today, but in the past couple of years, they've been extremely helpful, especially these two and a handful of other former workers. So these are the guys who were there on the front lines, and I would encourage you, the working group, to give what they say a lot of credibility in terms of they were there. They were there first hand. So to be honest, none of us were.

CHAIR BEACH: Okay. Work Group, what's your -- NIOSH, are you considering bringing anything else to the table on this issue, neutrons, the issues raised by SC&A?

DR. ULSH: Well, at this point, I think the latest revision of our White Paper stands. I mean, we don't envision any changes to it. If I interpret where we are in the process correctly, I don't know that there would be time to make any further changes anyway, but it kind of depends on the deliberations of the working group, whether or not you want to see something in addition.

But I think for where we are now, we're comfortable with what we've got out there.

CHAIR BEACH: Okay. Paul?

MEMBER ZIEMER: We've heard a lot of comments from SC&A today, but we've seen none of that in writing. I understand that it may be in the process or is there an intent to formalize any of that in terms of what your bottom line is?

I mean, we've talked around a lot of things. To some extent there appears to be some agreement on approaches, but maybe some disagreements on what the inputs to the system look like particularly under modeling. So it's not completely clear to me how close or how far apart our contractor and NIOSH are and what we have available as work group members to sort of come to a bottom line on this neutron issue.

MR. FITZGERALD: Yes, I guess I would respond that we did get -- well, first off, we had a Work Group meeting. When was that? May, I think it was.

CHAIR BEACH: May.

MR. FITZGERALD: Right. Where we discussed these issues. We didn't have a paper, White Paper, from NIOSH and we did discuss the MCNP issue. We discussed some of these issues, and based on that discussion, NIOSH did give us a slightly revised White Paper December 9th.

And I can only tell you we did review the White Paper and had discussions amongst ourselves in preparation for this meeting, and there is a lot of material that could go into a White Paper, but I think what we wanted to do is be in a position to respond to this revised White Paper from December 9th and take the guidance of the Work Group.

If it appears that it would be

beneficial to provide what we've said in writing, we could do that. However, I think we have actually provided most of it already.

I mean this to me is a refinement of what we said in the summer, our concerns over the MCNP input data not reflecting actual work place parameters.

MEMBER ZIEMER: Right. That's what John was emphasizing, but it appeared, John, that you're feeling now that there is more work place specificity, although you may have some concerns about, for example, do we have the right numbers for the shielding and so on.

DR. MAURO: As I mentioned before, it really comes down in the sensitivity analysis which attempted to show how much of an adjustment factor is needed. My concern was that there was no connection from where I looked at between the two inches and six inches of shielding and mount. Is there reason to believe that that is -- we heard just now something I didn't know, that, well, maybe a foot is the most it really could have been.

Now, having that kind of feedback, it really now becomes a judgment call on whether or not that kind of information makes it Mound specific. Has a bridge been built now?

Before today, I have to tell you there was no bridge. When I read that they took these numbers because someone felt they were reasonable, but our feedback from our people was they could not tell from looking at everything whether that was unique, whether or not we could say that was applicable to Mound.

We heard a little bit of information today from first-hand experience, and it really becomes a judgment call now: does that build a bridge or not or can more be done to build a better bridge?

We also heard that there may be some drawings; there may be other information that may be out there. In my mind this one -- now, I'm not talking neutron/photon now. I'm talking just simply this idea of the adjustment factor, and the use of the film badge to try the NTA film as the rock you're going to stand on.

To me that rock has to be solid, and right now that rock seems to be a little shaky because we're not quite sure whether a bridge has been built between the MCNP runs and the assumptions used in the MCNP runs in order to come up with the adjustment factor. To me that's the rock that this house is built on, and it's really a matter of, you know, your own personal sense. Do you feel comfortable with the basis for that assumption? Is there enough to build on?

DR. NETON: I just would add remember that this model value was only used for those visitor badges that had non-detectable photon. That's the only time it's used.

DR. MAURO: Well, that brings us to the N/P at the model. The model is to fix the N/P, and to me what we really -- unfortunately what we only really covered was the NTA film adjustment factors because of the uncertainty in the distribution. I think there are other subjects we really haven't delved into.

I think this issue of the N/P ratio and the fact that it's all over the place, I mean when I heard what I heard, when I heard numbers of ratios, it went from 18 to one to 18 to less than one, and that there's a presumption that there is a relationship between the two.

They may be stochastically independent. They may not be any relationship between a given photon reading and a given neutron dose because on each application where there's a change-out. The design of the shielding may have been so different that there is no correlation, and that means that there is no reason to believe there is a correlation between the neutron dose and the photon dose. You can't use neutron to photon ratios. It becomes like stochastically independent of each other, and then that goes out the window, and we haven't talked about this yet.

So I'm concerned about that. Now that becomes the place where when you don't have NTA film and you want to --

DR. NETON: Well, it's going to be on the correction factor maybe.

DR. MAURO: Oh, no, I'm just saying right now you heard where we are on the correction factor.

DR. NETON: Well, I don't know.

DR. MAKHIJANI: I had a comment. We've heard, at least I heard new information. I don't know if it's new to the veterans

MR. FITZGERALD: No, it's new to us.

DR. MAKHIJANI: That as I

understand the model that's on the table, it's a bare source that is giving high energy neutrons. It's moderated by some kind of shielding.

Now, we see that because a lot of stuff was going in starting around 59 or 60 and nothing was coming out, you've got a lot of trash. You've got a lot of trash. You've got a lot of hydrogenous material that constitutes the matrix for storing the stuff in the building. So now you've got barrels with paper, booties, gloves that have kilograms and kilograms of plutonium-238 whose characterization is not known, but you can infer that the neutrons would be much more heavily moderated than just or possibly more. They may not.

DR. NETON: I don’t know Arjun, that’s a stretch.

DR. MAKHIJANI: No, it is a stretch. I agree. I say maybe.

MR. MADDING: May I inject? This

is Dick Madding again.

May I inject that the drums went out of the building.

DR. MAKHIJANI: So the drums were not in the building.

MR. MADDING: Oh, absolutely not. They were stored outside alongside the building, and when PP Building was built in that space, they went 500 feet --

DR. MAKHIJANI: So then you've got a new place outside the building.

MR. MADDING: Yes, but it was down alongside the building and not in the regular path of people going by.

DR. MAKHIJANI: Right, but I just heard something that caused me to sit up to say that the whole source issue is more complicated. Whether it's an issue of significance for those or not I don't know, but I just want to put that on the table because it was a new issue.

MR. FITZGERALD: Well, I think

certainly the information on the --

DR. ANIGSTEIN: This is Bob Anigstein. I'd like to make one comment based on the workers' description of how crowded the rooms were where the glove boxes were.

So this idea that we had brought up earlier, it sounds the exposure would have been PA, becomes much more realistic now, and it sounds as if the worker might have been just as close to the glove box behind him or almost as close as the one in front.

MR. KATZ: Bob, you couldn't see the drawings. So it's understandable that that's not clear to you, but actually they're in separate rooms, and there's hallways between the rooms. So they really aren't back to back as you imagined.

DR. ANIGSTEIN: Oh, okay.

MR. FITZGERALD: But, Paul, to answer your original question, sure, I mean, we could take our notes from the last week or so and put them into a White Paper. I don't see the value on this particular issue, but I think what we discussed today is pretty much what we discussed last summer. You know, what we're saying, again, is that -- and I think, you know, some of the -- I would think it's a revelation to me. I think the new information is not only the material that was in the source material, but also the shielding thickness being up to 12 inches.

MR. MADDING: I do have a number of photographs of SM boxes and PP boxes with shielding on them, if those are of interest to you.

MR. FITZGERALD: Well, I think that is what we were saying last summer and now, that without building this bridge -- let's just keep the analogy going -- without building this bridge to actual parameters, engineering drawings, something that would bring this down better, I think the confidence in the adjustment factor is still not there, and that's what we kind of said this summer and what we said in our White Paper.

I think this discussion underscores it even more. So I don't think we can say more than what's been said at the table today.

MEMBER ZIEMER: Just a follow-up question, though. As I understood it, it is to me a very different picture of the operation as well. Many of the glove box people were working in different locations.

MR. MADDING: Oh, absolutely.

MEMBER ZIEMER: And so, you know, you might have a 12 inch, but you weren't working with the 12 inch shielding 24-7.

MR. MADDING: No.

MEMBER ZIEMER: You might be in there for ten minutes and then you were at a different place with different amounts. So there is a kind of built in I don't know if I want to call it averaging procedure, but I think you could make the argument that some kind of reasonable distribution of that shielding thickness would not be a bad approach.

I mean, yes, you could bond it and say let's take the maximum shielding, but it seems to me that's also unreasonable in terms of what they're doing.

MR. MADDING: And I'm not in conflict with what Brant has found out from other workers because it depends on when you were there as to what the thickness of the shielding was. If you were there --

MEMBER ZIEMER: For any given worker, it's likely to be changing --

MR. MADDING: Right, oh, absolutely.

MEMBER ZIEMER: -- from time to time.

MR. MADDING: Nobody could -- SM-35 was a respirator operation practically from the start, 100 percent respirator operation, 100 percent of the time. Nobody can work in respirators six hours of the day. It's not just --

CHAIR BEACH: Okay. Warren has been waiting patiently to add something.

MR. SHEEHAN: I want to interject. You talk a lot about this neutron/proton ratio and being somewhat box related, how much shielding and all of that. I want to point out something that I think you haven't maybe considered. I think it is more job oriented.

The reason I bring that out, there was a lot of trash bagging, and if you had seen trash bagging, you're hugging that trash bag. You have no shielding. So in this case there is no shielding. You can talk about six inch, 12 inch or whatever, and you can talk about pi r squared. There's not much there, you know, when you're hugging that bag and there was an awful lot of trashing out done and you can come out through the glove ports.

So I think maybe the doses were more job-related than box related in the end.

That's my point.

CHAIR BEACH: Thank you.

DR. MAKHIJANI: Just to throw a number in here, we found a number doing a little Google search. Scattering mean free path in water of neutron decreases from six and a half centimeters to 11.25 centimeters and the neutron energy decreased from 5 MeV to .22 MeV, probably a couple of centimeters of mean tree path is what we're talking about, and then decreasing. That's to start with, and then after the first collision would decrease.

DR. ULSH: Sorry.

DR. MAKHIJANI: I'm done. This is right. I mean, I'm just doing it.

DR. ULSH: Again, I would remind you what the purpose of this White Paper is, and it is to estimate the unmonitored neutron dose from visitors. When you're talking about SM-35 with the shielding where some workers could have been in there for short periods of time, those workers wore NTA film. Those are not the people that this White Paper is going to be applied to.

The people that this is going to be applied to are the visitors to the building that got low photon doses so that they didn't read the neutron dose.

DR. MAKHIJANI: I was just talking about the correction factor. I'm not talking about duration.

DR. ULSH: I'm not addressing what you said yet.

DR. MAKHIJANI: Oh, okay.

DR. ULSH: I'm talking about the past ten minutes of discussion.

DR. MAKHIJANI: Okay.

DR. ULSH: I would also direct you to page 41 of our White Paper where it clearly shows the fraction of the dose equivalent that is contributed by low energy neutrons, and it is trivial down below the neutron energy spectrum.

DR. MAURO: That's unshielded. That's a naked source.

DR. ULSH: No.

DR. MAURO: What is that? That's a shielded source? That dose --

DR. ULSH: Two inches of water here, but the point is --

DR. MAURO: Okay. It's the relative dose?

DR. ULSH: In terms of the relative dose equivalent. If you look at the area under the curve, it will give you the dose equivalent.

DR. MAURO: And this would be for -- all of these represent two inches of water.

DR. ANIGSTEIN: Which page are you on?

DR. ULSH: These particular pictures do. Forty-one of 53 in our White Paper.

Now, if you're interested -- I'll

let John finish looking before I go forward.

DR. MAURO: So it looks like about half or less. The area under the curve?

DR. ULSH: Right, the area under the curve if you look at -- Bob Morris, you can help me out. We've got a table here that shows -- but that's in there.

DR. MAURO: Okay.

DR. ULSH: And the last thing that I would add is far from being an unknown factor here, if you look at a paper by [identifying information redacted] in Health Physics in 1980, and if you're interested I can give you the complete citation, it gives you the effect in terms of the fraction of neutrons with energy less than one MeV in this case, and it has different shielding thicknesses. It goes from zero, a bare source, up to 30.5 centimeters. So about a foot coincidentally, and what you see is you get to a point of diminishing returns; that you add more and more shielding, the effect, it flattens off. It's less and less.

So I mean, this is not an unknown thing that we have just discovered here. Mound was well aware of this problem, and the reason that we picked the scenario that we did is not because Mound was full of concrete silos. It's because it's meant to be a bounding scenario.

I would present to you that the scenario that we picked with the glove box in the middle, concrete floors, concrete walls is the claimant favorable upper bound on what you would see that Dick drew up on the board.

CHAIR BEACH: But you're saying its scenario is not site specific data, which is what we're looking for.

DR. ULSH: It's not. The reason that we picked that scenario is to be a worst case so that we didn't have to model 14, 15 different configurations. We picked the one that would be the claimant-favorable bound.

In other words, if we went in in

MCNP and modeled specifically each of these scenarios and you could go on ad infinitum, it would not be worse than what we've got in this paper. That's why we picked it.

CHAIR BEACH: But the law says that it has to start with site specific.

DR. NETON: We knew it was plutonium dioxide. We knew, you know, the material it was starting with, the glove box configuration, the use of shielding. There was a lot of site specific information that was used.

DR. BUCHANAN: I'd like to make one clarification. The adjustment factors from the modeling apply to all NTA film readings, the workers and the visitors and the coworkers. Okay. All of these adjustment factors, each worker that has a dose of record of NTA film and then these adjustment factors applied to his record to assign a dose, and then they are coworkers' data to assign a person that wasn't badged. Okay?

So this modeling affects everybody's dose reconstruction.

DR. MAURO: That's why I kept saying the lock we're going to stand on it, and we've got to make sure --

DR. BUCHANAN: Not just coworkers.

DR. MAURO: Yes, I'm with you. So this is where it starts.

MEMBER SCHOFIELD: On the waste materials, I assume they had some type of NTA instrumentation to verify what was in those drums.

CHAIR BEACH: Can I stop for just a second. Phil, I don't want to interrupt that thought, but shall we take an hour lunch break and continue this afternoon? Because we're right at 12:30, and then some of those, maybe you can ask those questions. Will that work for everybody?

You think from 12:30 till 1:30?

MR. KATZ: Okay. For everyone on the phone then, we're going to disconnect the line, and we'll be back up at 1:30.

Thank you all.

(Whereupon, the above-entitled matter went off the record at 12:30 p.m. and resumed at 1:30 p.m.)

MR. KATZ: So good afternoon. This is the Advisory Board on Radiation and Worker Health, Mound Working Group, and we are reconvening after a lunch break.

Before we get started, I just want to check in. Joyce Lipsztein, are you with us on the phone?

DR. LIPSZTEIN: Yes, I am.

MR. KATZ: Joyce, would it be possible -- the Work Group is still deliberating about neutron dose reconstruction, and there's probably up to 30 minutes to go on that topic. Would it be possible for you to call in in 30 minutes?

DR. LIPSZTEIN: Oh, yes, of course.

MR. KATZ: I mean, you're welcome

to stay on the line. I just mean that you don't need to be hogtied to it if you have other things you need to do.

DR. LIPSZTEIN: Okay. I'll call back in 30 minutes.

MR. KATZ: Okay.

CHAIR BEACH: Thanks, Joyce.

DR. LIPSZTEIN: Okay. Bye-bye.

CHAIR BEACH: Okay. What we're going to do is we're going to switch the tritium with the radon. So we'll talk about the tritium after we finish up with neutron, and then we'll go into radon at the end of the day.

What I'd like to do is ask SC&A if they would tee up all the issues, if there's any other issues for neutrons, and then give NIOSH a chance to say whether there's anything more they want to provide, and then see how the Work Group feels at that point with what we would like to see from either SC&A or NIOSH.

If that's okay with everybody, we'll move forward.

MR. FITZGERALD: You want to re-summarize. I think it's summarized pretty well, but we have a new chapter and new information.

DR. BUCHANAN: Okay. SC&A's position at this point is what I'd like to recap, and we have said this previously, but at this point our position essentially is the same as it was last May and June and when we came into the meeting with the additional fact that we felt that we see stronger issues with the working conditions that existed at Mound. With what has been presented today, we do not feel that the generic model of a glove box represents or can be tied to the many working conditions as we've seen on the board here in PP and SM and SM 35, without knowing more about what the working conditions were there to tie it to the model or the model tied to the working condition.

In addition, we have not found that the results of the modeling can be tied to actual dose data or spectrums taken to -- at the Mound site to verify it. It's actually a combination of both, but we do not see either, and so we don't feel that the MCNP model, while we agree with their input parameters for the source, we agree that the model can be used to do these calculations, to do calculations from the nuclear industry. We don't feel that it has been proven beyond a reasonable doubt that it would provide correct dose reconstruction for the workers.

We're not sure if two or six inches or ten or 12 inches would be adequate. It may be. It may not be. I'd like to emphasize that the MCNP modeling will be applied to all dose of record. This is for the everyday worker and for the visitors and coworkers, anything. It all has to be adjusted, and the dose of record is in their folder, reads a certain millirem, and it will have to be adjusted by applying a factor of 1.2 or two or whatever it is. This is derived from the model and the other fading factors and such, and so this applies to all doses, neutron doses.

That's the number one issue. Number two issue is the coworker model. We feel that the variance in coworker data taken from the NTA data for the neutron to photon ratio shows a large amount of variance. We do not have the data available to do the analysis. We cannot just say looking at Table 6-2 tell whether it's correlated or not.

That correlation needs to be done within the years and between the years because they vary quite a bit, and the intervals in the second map that I'm using, the dose intervals, zero to 100, 100 to 200, and greater than 300, is not based on actual individual dose measurements, but on categories. This does not really stand on solid ground as far as dose reconstruction, we don't feel.

The other item is we do not understand why the NTA values themselves, since you have some for each year, from 49 to 77, was not looked into to see if those could be used for coworker dose assignments because this would be less complicated. It wouldn't be adding a factor of photons or intervals to it, and it would be of interest to see if that would provide coworker dose data as the adjustment factors were applied.

So I think that summarizes our position on neutrons at Mound at this time.

CHAIR BEACH: Anybody have questions for SC&A?

And then NIOSH, Brant, do you have a counter there?

DR. ULSH: Sure. Bob, do you want to weigh in before I do? Bob Morris.

MR. MORRIS: No, you go ahead.

DR. ULSH: Well, I tried.

MR. MORRIS: Please.

DR. ULSH: On the first point that Ron raised about the generic model, I guess the issues are clearly on the table for the Work Group to consider. We have laid out the bases for why we think our model is a plausible upper bound for what we're going to apply. We do not agree that it is not tied to Mound-specific data. There is a lot of Mound-specific data in it in terms of the source term, in terms of an estimate on the shielding, although if you want to argue that the -- let me get my tongue back in my mouth here -- the shielding should be thicker, that is in what John in past meetings has called a tractable issue. It can't go on forever. Your human arm is only so long.

If you want to argue that it should be a foot or ten inches or eight inches, fine. You know, that's a tractable issue, and I would say that it's a TBD issue and not an SEC issue.

Ron stated that the modeling would

be applied to all doses of record. I don't agree with that. First of all, it would be applied to unadjusted NTA films. I don't know. I think that you probably wouldn't say that it would be applied to TLD data.

DR. BUCHANAN: No, no. I wasn't talking at 49-77.

DR. ULSH: Right, but I would also point out to you that, well, like I say, it's only during the NTA era.

Models are just that. They're models. They are meant to be an approximation of what you see. They're not meant to be a one-to-one representation, and the reason is you can envision just looking up on the board that we have here in the room; you can envision any number of scenarios. It's just not possible to model every single scenario.

That's why we picked the one that we did, because it represents the worst case that would still be reasonable for Mound in terms of concrete walls, concrete floor, concrete ceiling. That is meant to be claimant-favorable because it maximizes the scattering. It maximizes the moderation that would occur to the neutron spectrum.

So, quite frankly, our White Paper stands. I mean, that's our position in terms of modeling.

On point number two, the coworker model, Ron raised a concern that the N/P ratio was too variable from year to year. I would say to you that that represents the reality of the situation at Mound, and it's very understandable why you would expect to see variable N/P ratios. I think Dick alluded to it earlier. If you start at one end, if you follow the plutonium-238 material through, for instance, the SM building, you go across a number of different glove box lines. You go through an entire process that is operated on on plutonium-238, and you would expect to have different N/P ratios throughout that process.

Yes, it's variable, but I think

that captures the reality at Mound. I didn't know that being variable was a disqualification. It's variable because it was variable.

In terms of categorical data being somehow not sufficient for dose reconstruction, I simply disagree. We have the summary data from the Health Physics Progress Reports. What it shows by and large is that of the multitude of film badges that were read, and there were hundreds, in some quarters thousands, they all fall into the lower dose category. So we're talking about at least in terms of these badges them being low.

Yes, they are influenced by the badge exchange cycle, and they're influenced in a claimant-favorable way. In terms of when you have a more frequent exchange, the missed dose is higher and that's claimant-favorable. I don't see a problem with that.

Yes, it is categorical data. I

don't know. I'm not ready to commit and say that we haven't done that anywhere else. I can't think of a specific example. So I'll just be silent on that issue, but I haven't heard a real technical basis for why that is unacceptable.

So there again, I think we're going to stand on that unless the Working Group has another specific request for us, and we will gladly try to accommodate that, but in terms of what we've heard so far, I think our position is well on the table.

Bob, do you want to add anything now?

MR. MORRIS: I really do think that you've said it very appropriately. We do have a model that is capable of finding a bounding exposure, and we have a model that is arguable about whether we've got the right assumptions in it for the technical basis document, but those can be changed if we need to.

If you want to say our judgment is four inches or five inches of water shielding to make the adjustment factors, that's very doable. Nevertheless, it can be done, and so I think it's not a question of can we do it in terms of the SEC. It can be done.

If you don't like the assumptions, we can change the assumptions.

DR. ULSH: So I guess at this point I would put it in the Working Group's hands. You've heard both sides of the issues, and I guess it's really kind of up to you guys now.

CHAIR BEACH: Joe, do you have anything else to add?

MR. FITZGERALD: No. I think, you know, again, we've pretty much reiterated our position since last summer. So I think everything is on the table.

There is, I would add though, some new wrinkles that came up in today's discussion, which you know we weren't aware of in terms of the actual workplace conditions that could bear some, you know, review in terms of augmenting what we've done already, but our central position is nothing has changed from that standpoint. We don't think the -- if I can use the analogy of the bridge -- we don't think the bridge to the site parameters would make this, in our view, a valid model has been built in terms of parameters.

CHAIR BEACH: Well, I guess I'd like to ask the Work Group. Paul, you brought up earlier that you'd like to see something in writing from SC&A based on --

MEMBER ZIEMER: No, I was really asking that as a question as to whether there was an intent to add anything to the record based on the discussion today or whether everything that they have already given us is pretty much their position. It sounded like there was perhaps some new wrinkles, and I thought that John was saying that he was perhaps interpreting things a little bit differently based on the information about the sort of limits on how much shielding you could put in.

So it wasn't clear to me where SC&A was ending up. So that was the only question I was raising as to whether something else was going to be in the record that we don't already have.

CHAIR BEACH: Okay. So as a Work Group, are we ready to decide on this issue or, John, do you have something?

DR. MAURO: I did have a question. To go back for a moment to the coworker model, the neutron to photon ratio where you're going to reconstruct doses to people who don't have NTA film but do have photon, and you're going to use that ratio, now, I have to say it's one of these brain teasers. If the two parameters are stochastically independent of each other, I don't mean this facetiously, but it's almost like saying I'm going to take the person's height and correlate it with another parameter.

I mean, there's no relationship between them, and you have a real person. You have a real person showing up and say, I want to reconstruct this person's neutron dose. I have his photon dose, and I'm going to use a neutron to photon ratio, but if there's no relationship between the two --

DR. NETON: Am I missing something?

DR. MAURO: -- that's when you do --

DR. NETON: But a lot of these people had NTA badges. They just weren't read. So these people were monitored. Their photon badges were non-detectable, and therefore they didn't read the neutron back.

DR. MAURO: Okay. So that's why I'm asking the question.

DR. NETON: The only class of people -- and I think I'm right -- does this one apply to.

DR. MAURO: So you've got a group of people.

DR. NETON: Visitor badges.

DR. MAURO: These are visitors. They had --

DR. NETON: They are workers, but they have visitor badges.

DR. MAURO: They have visitor badges, and they have both neutron and photon record.

DR. NETON: They were badged.

DR. MAURO: They were badged, and now what was it that you -- now, you're going to be using a neutron to photon ratio. Now, you have results back that are photons.

DR. NETON: No.

DR. MAURO: No, you don't have that.

DR. ULSH: Wait, wait, wait.

DR. MAURO: Help me here. My fault because I haven't delved in -- I was listing to the problem. Let me hear some more about this.

DR. ULSH: Maybe I can clarify. The group of people that we're talking about are what in Mound terms were called visitors. They were Mound workers, but they weren't permanently stationed in, for instance, SM building.

DR. MAURO: Okay.

DR. ULSH: So they were, you know, the crafts or the trades people that came up to do a specific job. When they did that, they were termed visitors. They were given a visitor neutron and photon badge. They went in and they did their job, and then they turned in their badge. They read the photon badge, and I can't remember exactly what the cut point was, but there was a photon dose, and if the photon dose was above that cut point, then they went ahead and read the NTA film. If the photon dose was below that cut point, they didn't read the NTA film.

So essentially if the photon dose is below the cut point, you could think of them in terms of being unmonitored for neutrons.

DR. MAURO: Okay.

DR. ULSH: Because they wore badges, but they weren't read. So they're essentially unmonitored.

DR. MAURO: So you've got the photon reading, and based on that reading, you may or may not have a neutron. If you don't have the neutron, the neutron dose was not read out, then you're going to apply neutron to photon ratio to somehow assign that.

DR. ULSH: During a certain time period.

DR. MAURO: During that certain time. Okay. Now, the neutron to photon ratio that is going to be used to make that assignment is based on data where you have both measurements, paired measurements that are in detectable range, and it's my understanding that those paired measurements do not show -- in other words, there was no correlation done.

Has any analysis been done to show that, yes, when the photon doses are higher the neutron doses are higher and there's a relationship?

And I understand that that wasn't done. It seems to me without having that, looking at the actual date and see the correlation coefficient between your paired numbers where you do have positive readings, I can't see how you could do this.

DR. ULSH: Bob Morris, I'm not going to give you an option now. Jump in and help. Tell what we have or have not done in terms of correlating neutron to photon.

MR. MORRIS: That's true. We did not make a correlation there.

DR. ANIGSTEIN: This is Bob Anigstein.

Identifying with what John was

saying, I did -- one year on the box and whisker plot in the 70s where the ratio goes from, just reading it off the plot, it seems to go from about one to 33. So with Brant's point, as Brant said yes, the neutron to photon ratio -- is variable. We're not disputing that. We're not saying there's something wrong about the calculation. But that's the whole point. It is so variable that where does this particular individual who may have had a blank -- whose neutron dose wasn't read, and I'll just make up a number. Let's say the threshold was 100 millirem on the photon dose. Well, he could be assigned 100 millirem for a neutron dose or he could be assigned 3 rem for a neutron dose depending where on this range you pick off that ratio.

And there's a huge amount of variability which does not sound like it meets the standard of sufficient accuracy in dose reconstruction.

DR. MAURO: But, Bob, I'll go a

step further. If there's no correlation between those two parameters, even if you were to pick the worst one, the 33, it doesn't mean anything anyway.

DR. MAKHIJANI: No, I think that that is the most fundamental point now. This is from memory of a couple of years ago. Maybe Brant remembers it better than me, but I think this was an issue at Rocky Flats where part of the problem with the use of the N/P ratio is that it did not seem to be -- and in that place it's a different kind of N/P ratio we're talking about admittedly because that was building N/P ratios. There didn't seem to be a correlation between the N and the P.

But this is from memory, but I think this is a very important issue.

DR. MAURO: I think it has to make sense to me. You know, even if you were to pick 33, the worst one, if there's no relationship, that doesn't mean -- it's just arbitrary that 33 happened to show up. If there's no correlation between the two, that just happens to be the number that showed up.

Do you see what I'm saying?

DR. NETON: There are universal ratios that are out there, right? Think of it as a universal ratio, and if you pick the highest one, then you have a sampling of all these universal ratios and you pick the highest one. I think that does say something. That's not a happenstance.

DR. MAURO: But there's no reason to believe the next one -- if they're stochastic independent, there's no reason to believe the next one is going to fall within that spread.

DR. NETON: Oh, but you have -- I don't know.

DR. MAURO: Do you see what I'm saying?

DR. NETON: I'm just saying if you had a legitimate sampling of all the work activities that are out there that have been done and you have a population, a distribution of ratios, that is a valid upper limit of a ratio of the monitor population.

DR. ULSH: John, let's take for the sake of discussion what you're saying is true. The neutron dose and the photon dose are completely uncorrelated. What would be the effect of the estimated neutron dose if you took that measured N/P ratio, given that has nothing to do with photon dose whatsoever, and multiplied it by the photon dose?

Well, at worst, if you picked a high ratio you would overestimate the neutron dose.

DR. MAURO: See, I would agree if we knew that as the neutron dose went up, there was a correlation coefficient, and then you knew that there was a relationship between the two and you say, okay. We know there's a relationship between the two. There's a variability, and you operate and you make a bunch of slopes and you picked the 95th percentile slope. Do you see what I mean?

DR. NETON: If there's not a one-to-one correlation, then what you have is a distribution of ratios. That's what you have.

DR. ANIGSTEIN: If I could, Bob Anigstein again.

The point is what is the effect even if there is no correlation? What is the effect of assuming, one, you have a scientifically invalid dose reconstruction which does not meet the letter of the law.

DR. NETON: No, no, no. I'm not saying assume one, Bob. I'm saying if you have a distribution of ratios and you take the highest ratio, that is an empirical sampling of that work activity. There are only so many different activities that can generate these ratios.

DR. ANIGSTEIN: But John's point was that if it's really uncorrelated, this is just a spurious number, and the one that wasn't measured could have higher.

DR. MAURO: It's almost as if I took a random number generator and I picked 100 numbers randomly.

DR. NETON: I don't think so, John. You just sample the work force population, and I have all of these ratios of work activities that were ongoing at that time at that facility, and of all those ratios, here's what I have as the worst case activity.

I'm not saying it couldn't be higher. I'm just saying that based on that empirical sampling this is what you have. You have data to indicate that under some condition it can be 30-to-one.

DR. BUCHANAN: But next year it's four, you know. In the production or, say, like, it doesn't change that rapidly from year to year.

DR. NETON: One work activity that occurred that could have been 30-to-one.

DR. ANIGSTEIN: But you also have a biased sample in the sense that the one that would give you the highest ratio by definition or by implication would be the ones with the lower photon doses, and the ones with the lower photon doses they didn't even bother measuring reading the NTA film.

So you could have 100-to-one, but because the photon dose was low, they were never read. So we have no record of it.

DR. ULSH: No, I could also turn that around and say by definition we have a biased sample because the ones that give you the highest ratio are the ones with the highest neutron doses, and if they had high neutron doses, they were measured.

DR. ANIGSTEIN: I know, but we don't know that because if they had a low photon dose, they never read the NTA film. So they don't know what's on it.

DR. MAKHIJANI: Actually, I think in this very narrow point, Bob is right. If there was a high neutron dose and a low photon dose, you don't know what it was, but you know, suppose one takes what Jim Neton was saying at face value, that you sample the universe of people and then you use the highest number and you have a range of work activity that is sampled, my question actually would supplement. I'm not saying that this -- you know, I'm not taking sides on the earlier argument. I think there's another question. Have you sampled? You have a sample of the nature of activities that were being done, and that's why I asked the question before the break. Are we sampling the right population?

We're taking an N/P ratio from one population, which is dominated by the process workers and applying it to the visitor population. I think Warren was actually saying something very interesting about visitors to us after the break, which I think deserves to be heard, you know, about the way visitor badges were used, and with the indulgence of the Chair, I think it would be important for people to know.

MEMBER CLAWSON: It would be nice for all of us to hear it.

CHAIR BEACH: Sure.

MR. SHEEHAN: I think the question was, again, the usage. One of the things, if workmen were, say, assigned to a job that went on for several days, some of them would keep the badge in their locker or wherever, shoe box, and reuse it. Many times they're thrown in from one visit, one badge, one visit, one badge. So you could never correlate the project.

DR. MAKHIJANI: So we don't know this universe of monitoring in terms of either photon or neutron dose because if it was more frequent that people kept their badges for the job, it will be more likely they got a photon dose that would be above a threshold and a neutron dose. But if they were changing their badges every day, then it would be much more likely that they got -- I think we don't have enough information about the universe of workers to whom we're trying to apply this N/P ratio other than this problem that John was talking about in terms of lack of correlation, which is also a problem.

DR. NETON: I need to know more than about -- it seems to me that these were short duration exposures to these workers. They ran out. They didn't have much exposure. Probably most of them, I'm assuming right now, are probably non-detectable.

DR. MAKHIJANI: And the reason I put this issue on the table, you know, having not gotten the read in Mound is that we started with this assumption at Savannah River site, and what we're finding at Savannah River site is that that assumption is not correct.

Now, I don't know whether -- I think the lesson from that is that is that it bears looking into and that we shouldn't be assuming a prior that because it was maintenance workers, that their exposure potential was somewhat less because they were in there for shorter periods of time.

MR. MADDING: Dick Madding.

I'd like to comment on that. A lot of times the maintenance workers came up there to do really high level maintenance without shielding because they were removing glove box fronts and changing glove box fronts, putting new equipment in and taking equipment out without shielding and were working in areas, in service areas which were intentionally not shielded because it was not intended to be worked in.

So, for example, behind the lines changing filters or working on things in the SM Room 35, and so those workers did have -- the visitor workers did have a potential for a high number.

DR. MAKHIJANI: I think Savannah River is not likely to be an isolated case. It just bears looking into. That's all.

DR. ULSH: So keep in mind what would have happened in a situation like Dick just described. That badge with a high dose, by definition you would have exceeded the LOD, and you'd get a positive reading on that badge.

DR. MAKHIJANI: Well, do you know the photon threshold for reading the neutron dose? It isn't above detectable limit with some other threshold.

DR. NETON: I'm trying to envision the scenarios which alter these neutron-photon ratios. You know, you almost have to look at the specific activities.

DR. MAURO: So there's this subgroup of workers who experienced a photon dose that's below some cutoff that you didn't measure the neutron. We're working with this subgroup by definition at least that had relatively low photon dose.

DR. NETON: Exactly.

DR. MAURO: So that's important. So we know that category, and they didn't read the neutron dose, I presume, because they presumed at the time that it would be low. So it now all seems ring true until I heard that no attempt was made to see if there is a correlation between the two. In fact, what I'm hearing is that there's very little correlation between for each one of these campaigns or operations, the ring which they set up the shielding, whether they shielded the gamma or shielded the neutron, it was dealt with in a way -- and I don't know what the rationale was -- but I could imagine in some cases they set up the shielding and eliminated the neutron dose and not the photon. More likely they eliminated the photon, not the neutron, and all of a sudden this relationship that they were operating on, in other words, the thought process that they had at the time may have been fundamentally flawed, and until you do the correlation, which shows, no, no, they were right; there is correlation, and we know if we cut off at 100 millirems for the photon, we know that the dose for a neutron really can't be much higher than this.

Right now I don't see where that analysis has been done to provide a compelling argument that, yes, that is reasonable and not only reasonable, but also claimant favorable, and I think that's where we're having trouble.

MR. MORRIS: Well, John, this is Bob Morris.

It's true that we haven't done that, but it's also true that this is the first time that anyone has ever brought it up as an issue with the year's worth of consideration of it, will you? When we didn't think to do that, it didn't make our thought process when we set up the analysis, but it's not that we couldn't do that analysis. It's just a matter of doing it.

DR. NETON: Yes, well, this is Jim.

I think after, you know, going through this discussion, I think there is an open issue there for us to review. It is true that these probably lower-exposed type individuals, there must be some upper limit though on this.

DR. MAURO: That's what I'm looking for. That's what I'm looking for.

DR. NETON: And whether there's the universe that we sampled, if that's the appropriate one, or if one can make some correlation with that, maybe that would be true. I don't know at this point. I certainly couldn't make that judgment sitting at this stage

CHAIR BEACH: So can I wrap this up a little bit? NIOSH, you're agreeing to go back and review the correlation between the -- and I probably won't say this totally correct -- the N/P ratio and give us what, a memo on that?

DR. BUCHANAN: Within the year, between years.

DR. NETON: Well, just in general

the use of the N/P ratio for visitor badges.

DR. MAKHIJANI: I think that probably will cover the universe.

DR. NETON: Just in general the applicability N/P ratio with the visitor badges, the validity of that ratio to bound visitor badge.

DR. MAKHIJANI: That's the only group that --

DR. NETON: I think that's what Brant said, and I think that's true, that it's only the visitor badges that weren't read that were below a certain threshold.

And I'd also like to get a handle on how many workers are we talking about here. Is this a few people, thousands of people? I really don't know. I have not looked into this very closely.

CHAIR BEACH: Okay. So to that end, I believe that's the only action item for NIOSH, and I would like to ask SC&A if they -- and if you have more after this -- I'd like to ask SC&A to come up with their final version of what we've learned today, what we've heard today, and deliver that to the Work Group also.

DR. ULSH: Josie, can I ask at least for our item what time frame are you looking?

CHAIR BEACH: Well, I think that we should probably wait until after the transcripts come out and give people the opportunity to go back and look at transcripts and then -- I don't really want to give you a hard fast right now.

MEMBER CLAWSON: It would be nice tomorrow.

(Laughter.)

DR. NETON: Close of business today maybe.

CHAIR BEACH: There has been a lot of discussion for a lot of hours, and it just might be something that you'd want to go back and look at before you finish that.

MR. MORRIS: Josie, this is Bob Morris.

CHAIR BEACH: Hi, Bob.

MR. MORRIS: With regard to the question that SC&A has on the table, I'm curious if you could come to a position about whether there's anything that we could say short of having measurements that we've never found that would satisfy your need for the bridge from modeling to the work floor at Mound.

I mean, is there anything you can imagine that would ever satisfy your concern?

CHAIR BEACH: Well --

MR. FITZGERALD: Yes, I was going to say I think back in the summer when we first raised this issue, we were pointing to whether there would be any characterization information on the -- I just felt there must be some engineering drawings or something that would give you some of the spatial information that we talked about today for the first time.

MR. MORRIS: Last summer you said a sensitivity analysis.

MR. FITZGERALD: Yes.

MR. MORRIS: Which is essentially what Figure 7-15 is.

DR. ULSH: Repeat that, Bob.

MR. MORRIS: Last transcript, I think you said that you could be satisfied by a sensitivity analysis, and if you look at Figure I think it's 7 -- I'm going by memory -- 7-15, the picture of the -- I'll find it in just a second -- that effectively does give you the sensitivity of the shielding values to the changing --

DR. ULSH: But we're in the context, if I recall what was in there, in the context of the bare source in one location, not certainly a sensitivity analysis against the operations that took place at Mound, but I think we're looking for some -- we're calling it bridge now, but some way to link this to the conditions at Mound itself.

DR. ULSH: But a sensitivity analysis, in my understanding in terms of modeling, is you vary one parameter, hold all the others constant, and you see how much of an impact it has.

MR. FITZGERALD: Right, but to what extent is that one parameter, in fact, a bounded parameter. I don't know whether, again, this notion of the shielding versus the sources, I think the answer to his question, some sense of whether or not you can demonstrate that bounding amount, I think that's where we started this thing; some evidence that, in fact, it is the most conservative condition that would have existed.

DR. MAURO: You see, one of the difficulties in doing everything that we're doing is that we're now looking at the average. We want to make sure that each time we do a dose reconstruction for someone that we feel confident that we have given him the benefit of the doubt.

Now, so then within that context, each person -- because we're so used to thinking in groups, averages, you know, collective dose, but when you're dealing with each person, you have to walk away with a sense of, listen, I think I'm doing the right thing by this guy.

Now, within that, within that kind of mindset I say, okay, a judgment has been made that we're going to use two inches of shielding to come up with a distribution of the neutron energies, and that will define the adjustment factor, whatever it was, 1.3. I forget what number, whatever the number was that you were going to multiply your film badge reading by.

Whatever dose it was, let's say it's 100 millirem. You'd multiply by 1.3, and I think a sensitivity analysis is certainly a good thing to do, and you say, okay, well, let's see how much it changes if we didn't use two but we used three, four, five, six, and we got a sense of how it changed. It didn't change that much. I think it went to 1.5 or whatever. In other words, it started to change, and I think that adds value because you want to know that.

But in the end, but in the end, to me, if I was the worker, I'd want a level of assurance there because I know I spent a lot of time maybe working out of a glove box where the neutron shielding was, you know, pretty heavy. I don't know. I'm making this up now. I'm putting myself in the mind of this worker who is going to have his dose reconstructed.

And let's say he knows and we should know that he knows it's really unlikely that there was any worker that spent 2,000 hours per year in front of, working on one of these sources where the thickness of the hydrogenous material just couldn't have been more than eight inches, ten inches. It just physically couldn't be more than that.

Now, I would say that in keeping with Part 83, you would like to be able to say, and that applies to Mound and here's why it applies to Mound, because we have a pretty good idea of the kind of glove boxes they use. You may have some drawings. You may have whatever it is, but I realize on a case-by-case basis they may have added different kinds of shielding. In other words, sometimes they use a little bit, sometimes. I don't know.

But here's where the bridge comes in, is that you want to be able to say with a degree of confidence that for every worker that you're going to reconstruct that neutron dose for using this adjustment factor, you'll want to be confident that you have given him the benefit of the doubt.

So it puts you in this funny place where you have to maybe make an assumption that may not be applicable to 90 percent of the workers, but you've got to capture them all.

Now, right now the two inches doesn't sound like it does that. Now, whether the three inches, the four inches does nor not I don't know, but I --

MR. MORRIS: Well, if you would, look at Figure 7-15, please.

DR. ULSH: That's on page 45, I believe. Bob, will you explain what the Y axis is real quick?

MR. MORRIS: Sure. This is the graph showing the percentage of neutron dose when at the observer position; is that correct?

DR. ULSH: Yes.

MR. MORRIS: Okay. So the Y axis is the percentage of the missed dose equivalent for the observer, who it turns out gets lower dose than the operator position, but because there's more scattering at that position farther away from the end shield, of course, the missed doses by percentage is higher.

So we have three different materials in consideration so that we've got the sensitivity of higher to the lower energy of first material, and then we've got different shielding thicknesses from no shielding up to six inches of shielding.

DR. MAURO: I think this is a great figure. I mean, it's a very useful figure. What it shows you is a great sensitivity analysis. It's a sensitivity analysis, and you make a case without a doubt that if you had six inches of shielding and you were working with I guess it's plutonium fluoride, you might have missed about 50 percent of its dose, 50 percent of the neutron. That's what this says, about 50 percent.

DR. MAKHIJANI: I have a question about this calculation. The shape of the curve seems wrong because if you have enough thickness of water, you're going to wind up with thermal neutrons and miss everything, whereas these curves seem to be asymptotic to something less than 100 percent.

You have to have curves where a certain thickness of water you'll miss 100 percent of the dose because you're going to be --

DR. MAURO: But that's not -- you'll never reach 100 percent.

DR. MAKHIJANI: Right. These curves don't look right to me because none of them will ever reach 100 percent. There's something wrong with this calculation in my opinion.

MR. MORRIS: The calculation has been available for review for a year.

DR. MAKHIJANI: Well, I'm just -- get inside a nuclear reactor. You have .0253 electron volts in a tank of water. The starting electron volts in a nuclear reaction are about 5 MeV, so a little bit more than what we're talking about potentially. Very soon before it reaches from one fuel rod, which is very close to another fuel rod, you've got .0253 eV, total neutrons.

So this calculation, I believe, is highly unlikely to be correct.

DR. ULSH: There's an important factor that is not being considered here, and that is that as you thermalize or collide the neutrons, more and more of the neutron energy is being absorbed by the moderator and less and less is contributing to the dose that is received.

DR. MAKHIJANI: That's right.

DR. ULSH: That's why it's going asymptotic because, yes, it's true if you look at the amount that's below the .5 MeV threshold, at some point it's going to be 100 percent.

DR. MAKHIJANI: The dose received, not the dose registered.

DR. ULSH: This is the dose equivalent received.

DR. MAKHIJANI: Well, this is not

the chart you want because this is not the adjustment factor. The adjustment factor has to come from the -- the adjustment factor is the dose received divided by the dose registered. This chart tells you nothing about the relationship of the thickness of water to the actual attenuation factor. At six, eight inches of water, you're not going to have any .5 MeV neutrons. You're going to have essentially all thermal neutrons.

DR. ULSH: Exactly.

DR. MAKHIJANI: So whatever neutron dose you have, I'm not saying that the neutron dose is high or low. I'm saying neutron dose can be very small. My question is: do you have an ability to calculate it by assuming a six inch water moderation between the source and the person?

I'm saying that because you're measuring only high energy neutrons, on the other side of six inches you've got pretty much something close to thermal neutrons.

DR. ULSH: But the point of this graph --

DR. MAKHIJANI: All you're doing, the calculation.

DR. ULSH: The point of this graph is to show that the more and more shielding you add, the less and less dose you get because all of the energy is being absorbed by the moderator.

DR. MAKHIJANI: I totally agree with that. I have no problem with that.

DR. ULSH: Well, that's the only point of this graph.

DR. MAKHIJANI: Well, I'm saying this graph cannot help you with adjustment factor because the thermalization of the neutrons at a certain thickness of water will just cut off your ability to measure any dose with this badge.

DR. ULSH: But the question is how much of the dose is missing. That's the question.

DR. MAKHIJANI: No, no, no. The question is at what thickness of shielding is your badge useless to register a dose. That's the number I think you want without prejudice to the idea as to whether doses were high or low. You want to know that number because that's the most important number.

You've got workers saying that you have four inches, six inches, eight inches, 12 inches. If you can't register a dose beyond four inches or polyethylene shielding, I don't see what good it does you to say, well, you know, the dose is attenuated by 50 percent by the time it gets on the other side because you don't really know. You don't have a measurement with which you can validate that.

DR. MAURO: See, it presumes -- this goes back -- it presumes that you are going to get some dose registered on the film badge.

MR. MORRIS: Yes, this is still a significant amount of the neutron energy that's above half an MeV. You know, the half value layer for neutrons is sort of what, four inches or something like that?

MR. CHEW: The tenth value layers is about what?

DR. MAURO: The tenth value.

DR. BUCHANAN: And that value is about four inches.

MR. CHEW: In what, in air?

DR. BUCHANAN: Water.

DR. NETON: We've done this before at several other sites, and I know we had moderators around this dimension and I don't recall this being an issue. We use for assessment a factor of two, for instance, I think, at Y-12 where I'm not sure --

DR. MAKHIJANI: I just think that you have a pretty thick shielding of water and you need to know what portion of the .5 --

DR. NETON: Oh, no, I appreciate if SC&A feels that way they'd do a calculation to document it because this ad hoc sort of calculation gets us nowhere.

CHAIR BEACH: Right.

MEMBER ZIEMER: Are you saying that you think they are missing 100 percent of the dose just to go --

DR. MAKHIJANI: I think we need to know that. I think we need to know the number at which you're missing a very large fraction of the dose, not just --

MEMBER ZIEMER: Well, if you believe the curves, it says that you reach an equilibrium.

DR. MAURO: Yes, that's what it says.

MEMBER ZIEMER: And if you put in more shielding, you're not reducing the dose further. You're not missing more.

DR. MAKHIJANI: Well, I can show you a Los Alamos paper right here where a 30-centimeter radius of polyethylene attenuates the neutron dose by a factor of 1,000. That's what it shows here.

That may be starting neutrons and outside of a 30-centimeter sphere, at least that's how I read the chart.

DR. ULSH: Well, I haven't seen this plot.

DR. MAKHIJANI: I'm not sure. I just pulled it up on Google.

MEMBER ZIEMER: That's different. You're looking at a shielding equation.

DR. MAKHIJANI: Yes.

MEMBER ZIEMER: This is a percent of the dose missed, which conceptually is a different thing.

DR. MAKHIJANI: Not exactly the same thing, yes. I think the amount of points --

MEMBER ZIEMER: It's a gamma simulation you'd get what you're talking about, but when you go way out, see, what percent of the dose is being missed way out?

DR. MAKHIJANI: Yes, it is a different question. You're right.

CHAIR BEACH: What I'd like to do is not interrupt, but to bring this to a close. The first thing I do want to say is we haven't had a meeting in eight months, and so a lot of these issues were just brought up at the last meeting, and so this is the first time we've gotten together and actually talked about them.

Second, NIOSH, I think, we're going to probably need your paper before SC&A can give us that final. So whenever you think that the review and the correlation between the MCNP, whenever that would be available, you can tell me today or we can get that at the end of the meeting tomorrow.

DR. ULSH: Well, in order to give you that answer, I guess I would like to go back to Bob Morris' question. I don't want to get into a situation where it's a bring me a rock thing and we bring you what we think you want and it turns out not to be what you want. So if you could maybe crystallize a little more discussion among the Work Group: here's what it would take to satisfy our concerns. I think then we would have a better picture on either we can't satisfy that or we can and here's how much effort it's going to take.

At this point I'm a little unclear as to what it is that you guys need to see to satisfy your concerns.

MEMBER ZIEMER: Well, I need to get clarity on John's point. In my mind, if you sampled gamma and neutron at some point for some process and did that repeatedly, you'd see that correlation. Go to a different place and a different time and a different process you'll get a number.

So I'm thinking that the correlations are there for each point, but now you're sampling a body of correlations.

DR. MAURO: Different situations.

MEMBER ZIEMER: In my mind, the only outstanding question I had because I agree with Jim; I think here's the body, and I think the only question is have we missed something, and you said maybe at the really low end where the gamma dose was so low --

DR. MAURO: That you didn't worry about those.

MEMBER ZIEMER: -- that you didn't worry about the neutrons, or do we have enough data to know? And you should be able to cap that, too.

I don't know of any case where there's neutrons without gammas. If someone can point one out to me, I always teach my students this. There are always gammas where there are neutrons, and there will be a correlation for that particular scenario.

DR. MAURO: That situation.

MEMBER ZIEMER: Now, if that ratio is so great that we're missing some assigned dose, I think we could pick that out, but with all the sampling you have, there can't be many cases; it would have to be really unusual, but there may be one other ratio out there, but it's not going to be like ten, 20, 30 times that distribution in my mind. It may be just --

DR. MAURO: Okay.

MEMBER ZIEMER: I want to know what you're talking about.

DR. MAURO: This is good. This is good. So you're saying that the reason there's such a variability --

MEMBER ZIEMER: Yes, it's not that there's no correlation between gamma-neutron.

DR. MAURO: There is.

MEMBER ZIEMER: It's that there's a distribution correlation.

DR. MAURO: The design, the physical setup that was being dealt with in case number one was a particular situation.

MEMBER ZIEMER: Right.

DR. MAURO: Was generating certain neutrons.

MEMBER ZIEMER: And you agree.

DR. MAURO: I agree with that.

MEMBER ZIEMER: You could reproduce all of the parameters. We should get that same ratio every time.

DR. MAURO: I agree.

MEMBER ZIEMER: Otherwise the universe has no logic.

(Laughter.)

DR. MAURO: This is a good thought.

DR. LIPSZTEIN: When a miracle occurs.

DR. MAURO: No, no, no. The high variability in the neutron to photon ratio that was observed, 31 or whatever it is is an artifact or an outcome that --

MEMBER ZIEMER: I would say plus the real situation.

DR. MAURO: Because the conditions under which they were measured was so different.

MEMBER ZIEMER: Right.

DR. MAURO: Now, so the argument

is that there's enough of those measurements where they measured this situation.

MEMBER ZIEMER: Or is there some upper limit beyond that?

DR. MAURO: Well, you could say --

MEMBER ZIEMER: Which you could identify, and I think Jim sort of --

DR. NETON: Here's what I thought the issue was. We have this universe of ratios, this distribution of ratios. The question is though for the people that we're applying those ratios, that is, the visitor badges who were apparently to a large extent maintenance workers, are those ratios valid to apply to -- these are glove box workers primarily, to these maintenance crafts? Is there something unique about the maintenance craft that those visitor people that had a unique -- is there a plausible, unique neutron/photon ratio that would not be captured in this universe of N/P ratios that we have from the worker.

MEMBER ZIEMER: And if not, why not?

DR. NETON: And if not, why not, and that could either be explained empirically or logically or a number of different ways.

DR. ULSH: But keep in mind that what we do have is not only the workers stationed in, for instance, for SM, but also the visitors that were high enough to be part of that.

DR. MAURO: What is very important is that I was wrong about the idea that there should be a correlation. No, no, no. I was thinking there should be a correlation. You've got a high neutron dose. You've got a high -- but no. Each one of these are unique situations that have their own neutrons and photon relationships.

MEMBER ZIEMER: And their own correlation and that limited --

DR. MAURO: And that limited space. So to plot the individual neutron dose, the photon dose for all of these different circumstances, you're going to get a scattered graph graph. It's not going to be a relationship, and the question is: is that okay?

MEMBER ZIEMER: But it doesn't tell you what to do with it.

DR. MAURO: Yes. So is that okay? That means that there's a way to come to grips with it.

DR. NETON: And that's why I think that we suggest that we're going to apply the distribution of those ratios to the adjustment factors. The question then was whether it was the distribution of the 95th percentile. That's where we ended up.

DR. MAURO: Well, it seems to me that there's a mechanistic issue here. What we have is a class of workers that have only had -- and may represent a lot of different kinds of job categories as Arjun pointed out -- there could be a lot of different kind of job categories where their full-time doses were always less than some number.

So these are a group of workers. It's a subclass of workers. Now, right?

DR. NETON: I don't want to put is in a position to prove the negative, I mean, prove that this didn't happen.

DR. MAURO: Well, no, no.

DR. NETON: You have to come up with some plausible scenario.

DR. MAURO: I'm putting myself in the shoes of the worker. I'm a worker that is one of the workers that worked there this year and my photon during my change-outs, I guess each one of my change-outs, I was always below this cutoff, whatever. What is the cutoff you guys --

DR. NETON: We're not sure.

DR. MAURO: All right. Let's say it's 100 millirems. Okay? Now, what you're saying in my case every one of those cutoffs was less than 100 millirems, and let's say I know that. I worked in a number of different situations.

And now what you're going to say is that, well, for you we're going to assign a multiplier, and maybe a distribution of the multipliers. You take the dose that you receive and assign this distribution of multipliers to that person.

Now, that distribution of multipliers is somehow going to come out of this array of values which is going to be one to 33. Now, I guess there has to be some -- now, there has to be a degree of comfort that you're not short-selling this guy.

And what happens if this circumstance was one where the nature of the operation was that there was a lot of shielding to reduce this photon exposure, and that was a problem, and the neutron, we don't know where the neutron exposure was. It may have been at the high end.

You see?

DR. NETON: I understand. You're speculating and making up these scenarios, but the point is --

DR. MAURO: But it's a real -- you have to go through these things.

DR. NETON: I understand, but the question is, can a plausible scenario be envisioned in this class of visitor badge workers that had a neutron ratio that was higher than we would propose to assign based on the known universe of that.

DR. MAURO: Well, based upon what you understand about the operation of the facility.

CHAIR BEACH: Can I ask? Do you understand that all I'm asking for, what John is asking for is to be able to put something in writing?

DR. NETON: We just have to demonstrate that these ratios are valid to apply to the visitor badge workers, why those ratios would not plausibly be different for that class of workers than what was experienced in the general plant environment.

DR. ULSH: And do you have a picture of what that evidence would be?

DR. NETON: I think there's a few things to look at. One is to look at the visitor badges that did have positive readings that were read. I don't know how robust that's going to come out, you know, what percentage, but that's one place to look.

Another place is to do sort of a theoretical analysis and you know, you get a 30-to-one ratio of photons to neutrons. Based on what we know about what happened in this plant, what scenarios could you envision where there would be 30 times more neutrons than any measure of photon dose out there greater than that?

And, frankly, I don't know the answer to that, but that's one.

CHAIR BEACH: I'm not going to give you a time frame. We'll talk about that later.

And then on the other side, SC&A, give us their final piece, their final to the Work Group. Is everybody okay with that?

MR. FITZGERALD: Yes, with maybe the supplement of making sure that we have access to the data, the neutron/photon data. It may be there. We just could not locate it.

DR. BUCHANAN: I couldn't find it on the O: drive.

DR. ULSH: In terms of N/P ratio, I think that's MESH, isn't it, Bob?

CHAIR BEACH: Was that your earlier --

MR. MORRIS: Yes, that was MESH. I can certainly take a look at my spreadsheets that we've got that data stored in and see if I got something that I could post for you to review.

CHAIR BEACH: Is that the one we mentioned 76-610 or was that a different effort?

DR. BUCHANAN: No, that's a different.

Bob, if you could send us the spreadsheet you've worked off of to create those N/P ratios, it would save us a lot of hunting time.

MR. MORRIS: Ron, I'll certainly promise that I'll look for them hard and try to get them on the O: drive for you.

CHAIR BEACH: And could you send a notice out to Ted or somebody to let us know that you've done that, Bob?

MR. MORRIS: Yes, of course. We'll go through normal chain of communication on that.

CHAIR BEACH: Okay. Is everybody ready to stop, to move on? Okay.

We're actually at a break time. Shall we take a ten-minute break?

DR. MAURO: Did Joyce call in?

CHAIR BEACH: I'm sure she did a half hour ago.

(Laughter.)

CHAIR BEACH: Let's take ten minutes and then we'll start on the table of tritium compounds.

MR. KATZ: I am just putting the phone on mute. I'm not disconnecting.

(Whereupon, the above-entitled matter went off the record at 2:34 p.m. and resumed at 2:47 p.m.)

MR. KATZ: Okay. Then this is the Mound Working Group. We're reconvening after a short comfort break, and we're finished for the day with discussing neutron dose reconstruction, and we're moving on.

CHAIR BEACH: Okay. Could you check and see if Joyce was back on the line?

MR. KATZ: Joyce, are you with us on the line, Joyce Lipsztein? Joyce?

(No audible response.)

MR. KATZ: Perhaps you're on mute? I'll last call Joyce. Joyce, are you with us on the line right now?

If not, I guess we could just do another topic before.

That sounded something like a fax. Okay. Last call. Joyce Lipsztein, are you with us?

(No audible response.)

MR. KATZ: Is there anyone on the line?

MS. AL-NABULSI: I'm here.

MR. KATZ: Oh, is that you, Joyce?

MS. AL-NABULSI: Isaf.

MR. KATZ: Oh, that's Isaf. Okay. So we can be heard. Okay. Well, maybe, Josie, do you want to do a different topic?

CHAIR BEACH: Actually I think we're going to go ahead and go with stable tritium compounds. We did say we were going to start and hopefully Joyce will join us in a few minutes, and I believe, SC&A, you're going to take the lead on this. John is going to call Joyce.

DR. ANIGSTEIN: Yes. We went

ahead and put together a reset of talking points. The reason we did that, we had some challenges discussing this topic because it does relate to the weapons program, and there are some sensitivities, and this is a review for those sensitivities. That's one reason we wanted to go ahead and use the talking points, you know, as a starting point.

We did have a work group meeting in July where this was discussed, and we raised concerns at that time on a proposal by NIOSH to use -- I'm sorry. I'm looking at something else here. Let me back up a little bit.

In April, you know, we sent a White Paper certainly to the Work Group and NIOSH that dealt with some of the issues we felt revolved around the handling of special tritium compounds, tritides, and a lot of these issues revolved around, you know, the application of site specific information to the model OTIB-0066 that would be used, and also with some concerns expressed over whether or not you could define a worker population exposed to the most insoluble tritides.

We did have a number of questions regarding whether or not you could, you know, use hafnium tritide as, you know, the sort of insoluble tritide that was in active use at the site. But certainly that was the thrust of that.

In NIOSH's October 2009 response, and again, this was relatively recent, we did have a chance to review it though in some detail. I think the contention that was, you know, whereas intermediate solubility compounds present at least a theoretical exposure potential to a large number of workers, exposure to the very insoluble tritides -- in this case we're talking hafnium tritide -- was, in fact, limited to a very small, discrete group of workers that NIOSH had a roster for.

And it was emphasized and, again,

we had addressed this issue in our White Paper, but I think the concern that NIOSH has raised that perhaps we would conflating the worker population at Mound that were, in fact, exposed to the so-called more intermediate solubility tritides with what was being defined as a much smaller group, much more discrete group that would have been exposed to the hafnium tritide.

I think it was emphasized that the fact that these workers were identifiable by name was a key difference and, you know, certainly made dose reconstruction more manageable in terms of assignment of dose.

And, in fact, the concern, I think we expressed some concern that maybe there was a wider exposure potential to the Mound worker population to the more insoluble tritide, that that wasn't true; in fact, that there was a very constrained potential exposure to the insoluble tritides.

Okay. Essentially what we did and

what I think Brant and NIOSH has done is just try and pin down a little better what the operational use of hafnium tritide was historically at Mound, and that required certainly a number of trips to OSTI to look at classified information.

So what I'm going to go through is basically a summary of what we have found, and I know Brant has looked as well, and we had the benefit of looking at some of the documentation that was at OSTI that he had looked at. So this is our basic summation of this.

And, again, I'm reading from our three-pager. While SC&A acknowledges that the handling of hafnium tritide began at Mound as a confined, discrete operation, as described by NIOSH, with a select number of workers, based on this review I just referred to, the historical records indicate we believe otherwise that the number of locations and workers handling this compound, again, hafnium tritide, expanded over time.

In other words, essentially in the beginning, in the 60s, you had a discrete operation, set number of workers, a specific geographical location, whether it's one or two or three rooms, whatever, and you certainly could define it by workers. You probably, again, as has been done, you could tie it to identities.

But that changed over time. You get into the 70s and 80s, and as with most operations, it matured and changes took place. What we identified were a much more extensive handling of hafnium tritide that went beyond this initial fabrication and included as a minimum, and this is what we could determine in a relatively short period of time, that as a minimum you had storage operations that took place and make sense in terms of material. You had a destructive testing quality assurance program at Mound that extended over a number of years. You had a major scrap metal recovery operation involving hafnium tritide as well as other materials where the quantities were actually substantial.

And ultimately, of course, you had D&D that would have taken place for all of these specific activities.

So the picture I'd want to paint is that, yes, you had a very confined operation involving a set number of workers that started in the 60s, but as time went on, you had a number of other activities that got underway as the program matured, and these other activities and programs involved additional workers, new categories of workers and involved operations that were at the tail end of the process, involving things like scrap metal recovery and what have you.

So a different picture, I think, than what we've been playing with beforehand, and you know, again, there's documentation albeit a lot of it is classified, that gets into time frames, gets into specific locations, and whatnot, but so far it is not clear there's any specific information about the workers that may or may not have frequented those activities during those time frames.

And from this documentation, these programs weren't, you know, sort of brief, one time only programs. They were longstanding programs, and the significance of the quantities, these weren't trace quantities. These were substantial quantities of hafnium tritide that were handled, processed and stored.

And so, you know, again, without going into the long history of looking at this question, I think this question of whether one could identify a specific work force that one could dose reconstruct based on information on hafnium tritide, if one assumes that to be most insoluble tritide at Mound, we find that problematic at this point because I think it's clear there were many more activities and certainly more workers that would have been involved in this.

Now, that's sort of the key issue that we kind of belted around and certainly figured in NIOSH's response. There's other questions, and part of the reason we want to have Joyce available is there's other questions of whether or not, you know, even with the intermediate sources of tritides, diffusion, rust, what have you, you know, this was reflected, I think, for the first time in the last NIOSH White Paper that you had an additional source of exposure to a larger group of workers, and so there was an acknowledgment that certainly NIOSH would consider how that would be addressed with that recognition.

But I think, you know, sort of bottom line, we don't see at this point, not to say it couldn't be done, at this point we can't see how you would distinguish those workers that might have been exposed to, say, the highly insoluble hafnium to those workers that were exposed to the more ubiquitous intermediate solubility tritides, and I think that's essential for dose reconstruction.

And the other point that we raised in that three-pager was, you know, if you can't distinguish those worker cohorts, then it sort of pushes you on a direction of perhaps considering how to assign an upper bound with the worst case of maybe hafnium gets you into a space of maybe implausibility because clearly hafnium was constrained in terms of exposure pathways, but not as much as I think was originally thought by NIOSH.

MEMBER ZIEMER: Could I ask a question? Joe, this may have been covered in the past and I just don't remember. I don't even know if I'm asking a question that can be answered in terms of security things, but is the hafnium tritide in a liquid or solid or what form is it in? Are you allowed to say that?

I'm trying to envision --

MR. FITZGERALD: I can't.

MEMBER ZIEMER: You can't say that.

MR. FITZGERALD: I'd rather not get into --

MEMBER ZIEMER: All right. I wasn't sure, but because it has to do with routes of ingestion and where does it go in the bottom?

And let me ask another question, if you can answer it. Is it a compound or is it an occlusion? Can you answer that?

MR. FITZGERALD: It's a metal substrate compound.

DR. BISTLINE: It's a compound, and this has been cleared through headquarters.

MEMBER ZIEMER: It is a compound.

DR. BISTLINE: It is a compound, and it's a stable metal tritide.

MEMBER ZIEMER: Because there's

mention in here of the tritium diffusing out of containers, and if it's a true compound as opposed to an occlusion, like nickel tritide is really tritium occluded on nickel and it diffuses off.

DR. BISTLINE: Well, but the diffusion is when it's in a gaseous form, and it's diffusing through the metal, but there is reactivity which creates tritides, which creates solid metal tritides, forms.

CHAIR BEACH: And, Paul, that is on the last page of the three-pager.

MEMBER ZIEMER: Oh, is it?

CHAIR BEACH: And I believe, Kathy, you had your hand up. Did you have a question or comment?

MS. ROBERTSON-DeMERS: I was just going to point out that the confusion issue is separate.

DR. BISTLINE: And I need to point out that there are a number of different tritide forms, stable metal tritides that have been handled throughout the DOE complex, and there's a list of these, hafnium tritide, uranium tritide, palladium tritide, titanium tritide, zirconium tritide, erbium, scandium tritides, those to name some of the ones that are listed, and these are not -- I cannot elaborate on how they were used or where they were used, but these have been used throughout the DOE complex, and all of these are forms which were also used at Mound Laboratories.

And these were in levels that were in production processes. So we're not talking about insignificant amounts that have been used throughout the complex. So it becomes more than just an issue with Mound, but we are talking about Mound at this point.

MR. FITZGERALD: But it's a form that's readily dispersible and available for inhalation. We certainly can say that without getting into the makeup of the compound.

MEMBER ZIEMER: And there are some cases where the gas is leaving the metal matrix, maybe even by recoils.

DR. BISTLINE: Right. One of the problems that has been found especially on the site return type situations is a diffusion through metal containment and through gaskets and other.

MEMBER ZIEMER: That would be diffusion of the tritium, not of the tritide.

DR. BISTLINE: Yes, but it forms tritide, where you've got reactivity which creates tritide forms, and then you also get into the rust issue with iron tritides and glove box situations, gaskets, gloves and glove ports, where if you've got tritium gas in an enclosed environment, with time -- and it's accentuated by temperature and pressure. Any time you get temperature and pressure, why then that just increases the amount of diffusion and reactivity that will occur.

But it's not a trivial amount that's involved here.

MS. ROBERTSON-DeMERS: And the

temperature and pressure will also indicate how deeply into the matrix it goes and how readily it will be released later.

MEMBER ZIEMER: Right.

MR. FITZGERALD: You sort of have several facets to the issue. Certainly one is, you know, what represents sort of the bounding prevalent type S insoluble compound that would be applied to certain groups of workers and what represents the more soluble forms of tritide.

Mound handled a lot of different types of tritide, some of which were only handled in small bench scale research. So it didn't represent anything that was substantial. As a part of what I think we jointly were looking for is what represents the most insoluble form of tritide and represents an exposure pathway, you know, tangible exposure pathway to workers, and certainly what was in the White Paper that we received from NIOSH was hafnium represented certainly that species of tritide.

Now, there's some debate and I don't think we've settled it out completely about whether there's others that represent a substantial source or not of a different form of tritide, but certainly we've keyed in on hafnium because I think that was the one that was identified as NIOSH's concern and also, not to put words in Brant's mouth, but in terms of the White Paper was the one for which specific workers were identified, and I think we've been having this discussion for some time, and specific activities were defined.

At this point what we're raising is some questions or concerns about whether or not that sharply defined fence around a specific operation and specific main set of workers is, in fact, that constraint. We don't think so, based on what we've looked at. That's what we're bringing to the table that is new.

I think the issues that we've

raised on the fusion we've raised before, and I think NIOSH did respond to some of those issues. At this point I think it is probably the new one, the going-in proposition.

MEMBER ZIEMER: Another question on solubility, and maybe, Kathy, you could answer this, but does the -- when you're talking about solubility, let's say, of hafnium tritide or tritide, as some say tomatoes and some say --

(Laughter.)

MEMBER ZIEMER: -- is the hydrogen, the tritium atom readily exchangeable with the hydrogen in solution or is it truly -- what's insoluble? Is it the whole compound or is it just the hafnium metal?

CHAIR BEACH: You need to come to the table, Kathy. I'm sorry. He can't hear you.

MEMBER ZIEMER: You understand what I'm saying. In other words --

MS. ROBERTSON-DeMERS: It's the actual compound.

MEMBER ZIEMER: The compound. So it's not exchanging freely.

MS. ROBERTSON-DeMERS: In other words, the exchange is totally bounded to --

MEMBER ZIEMER: It's not exchanging freely with other hydrogens like many tritium compounds.

MS. ROBERTSON-DeMERS: Right. Some of them are soluble.

MEMBER ZIEMER: Got you. So they are talking solubility.

MS. ROBERTSON-DeMERS: That's why Mound refers to them as stable metal tritides.

MEMBER ZIEMER: Yes, thank you.

MR. FITZGERALD: Now, the dilemma, of course, is we can't go too much further because the operations in this case define sort of the presence of the compounds we're talking about and the workers that may have been associated with those compounds, and that's about all we can really talk to at this point.

MEMBER ZIEMER: Can we talk about bioassays?

DR. BISTLINE: Well, yes, I think that's an issue.

MR. FITZGERALD: Let's stay within the body. Then what? How would you bioassay it?

MS. ROBERTSON-DeMERS: And Joyce might have a more extensive knowledge of this. There's about seven of the compounds that were handled at Mound where a solubility determination has been made either through in vivo or in vitro studies, solubility studies that are based upon the DOE standard tritium compounds.

DR. BISTLINE: Let me read a couple of statements from a Pantex metal tritides Technical Basis Document that was published by Pantex. It states, no special swipe techniques have yet been identified to apply to metal tritide surveys. Savannah River site has investigated the use of mass spectroscopy but found it only identifies the base metal and does not tell if it is a tritide or not. No special bioassay techniques have yet been identified, and this is a 2004 publication, have yet been identified, but a longer than expected biological half-life provides an indication of the presence of metal tritides, and D.M. Taylor, radiation doses from some tritium labeled organic compounds states it has concluded that although the ICRP OBT model may underestimate doses for specific compounds by up to an order of magnitude, it can still be applied with caution for prospective radiological protection purposes, but it should not be applied for interpretation of bioassay data.

I think those are important statements.

MS. ROBERTSON-DeMERS: Can I add

something to that? The DOE handbook actually goes and states the physical and chemical behavior of STCs common to the bioassay methods implemented at Mound for the measurement of HTO intakes and the subsequent internal dose calculation models can be ineffective.

DR. ULSH: Is it our turn yet?

(Laughter.)

CHAIR BEACH: I'm checking to see if there's any other questions.

MR. FITZGERALD: Joyce, did you have anything before turning the table to Brant?

DR. LIPSZTEIN: The amount that comes out that you expect to come is very, very small, and if a worker is exposed at the same time to, let's say, hafnium tritide and he is exposed also to tritium, tritiated water, then you can't really distinguish what comes from the hafnium and what comes from tritiated water because what comes from the tritiated water will dominate the excretion rate. What comes out in urine is very, very small.

And if you consider that everything that comes out in urine comes from the bypass of hafnium tritide, then you get an unbelievable high dose of 10,000 higher than the dose if you consider it tritiated water. So that's a really big problem.

OTIB-0066 talks about this problem a little bit, and the example that was done for us also shows an unbelievable high dose if everything is considered type S.

DR. MAURO: Joyce, that would be for the respiratory tract, but for the other organs?

MS. ROBERTSON-DeMERS: That's for the lung, yes. That's for the lung, and we don't know exactly what's happening on the GI tract, but if we apply the ICRP GI tract model, it's also going to be a very big dose to the GI tract, but to the other organs it's more or less the same things. It doesn't matter. A little bit depends -- a little bit? No, a lot depends on how long after an intake you collect the urine sample.

MEMBER ZIEMER: The GI tract, shouldn't it pass right through?

MS. ROBERTSON-DeMERS: Yes, but very little would pass through the GI tract if you consider type S.

MEMBER ZIEMER: Oh, no, I'm talking about ingestion, not inhalation.

MS. ROBERTSON-DeMERS: Oh, no, no, no. I was talking about inhalation, yes.

MEMBER ZIEMER: Oh, oh.

MS. ROBERTSON-DeMERS: I'm thinking about the inhalation. So it's very difficult to self-discover, especially if someone is -- you don't know who was exposed and you expect so little in the urine sample.

So because it's best not to match a urine sample, then when you go back to the dose to the lungs, it comes out to a very,

very high dose. And unless you know exactly who was exposed and when, it's very difficult to calculate the dose and distinguish what comes out really from the hafnium tritide and what is expected in urine because of exposure to tritiated water.

CHAIR BEACH: Anybody have any more questions for Joyce?

I would like to say, Joyce, thanks for your patience, and I believe Brant is ready to speak now.

DR. ULSH: All right. As had been mentioned, this discussion has been ongoing for quite some time, like most of the Mound issues, and that includes some discussions that took place in the cone of silence.

We have interviewed three workers with hands-on experience in the tritium program at Mound. They had responsibility for that program, and they have all told us, all three of them; they gave us a list of workers who could have possibly been exposed to hafnium tritide. They named them by name, and we have provided those interview notes and provided those names to the Working Group and SC&A.

Now, we also, to verify that -- Joe, I would like to get a clarification. In terms of the documentation that you're using to conclude that a wider group of workers could have been exposed, are these the references that are cited in your notes from your August 18th visit to OSTI?

MR. FITZGERALD: No, these are documents that were collected during your visit in November that DOE shared because these were all tied up in classification review. So these were essentially your documents, your notes that are under classification review at DOE and will be for a while, I guess, at this point.

DR. ULSH: Okay. To clarify the chain of events, on October 5th, Joe faxed me a copy of these notes from a visit to OSTI that occurred on August 18th.

MR. FITZGERALD: Right.

DR. ULSH: And then in November -- I don't remember the exact date -- I went to OSTI to look at the references that were cited in Joe's notes. Just like always we bootstrap. We take a look at the report and then we look at what's referenced. Then we went beyond that.

So I looked at all of the references that were cited in Joe's notes, and I'm bumping up against the exact same problem that we've been having since the beginning, and that is none of those references specifically mention hafnium tritide. They mention tritides, and it's my interpretation that SC&A has interpreted that to mean hafnium tritide.

Work with other more soluble tritides, for instance uranium tritide is commonly used as a tritium storage bed. Lithium tritide is another one that was used widely at Mound. Those were much more extensive operations, and I think if you look at least at the references that were cited in your notes, Joe, and I would say any of the ones that I saw down there, nowhere did I see any indication whatsoever that the work with hafnium tritide was larger than what we have represented.

In fact, we found an explicit document that gave a month and year and a location where hafnium tritide operations began, and I would be happy to discuss that under the appropriate circumstances, and it exactly supported what the workers told us in terms of the scale of the operations involved.

I still come back to I think the mistake that's being made is a conflation of hafnium tritide with other tritides. For instance, in your notes, Joe, you go into a lot of depth about the square footage dedicated to tritide operations, the extent of the physical infrastructure that was used and the manpower that was used in tritide operations. I looked at those documents. They do nowhere that I saw specifically relate to hafnium tritide. And in fact, it's consistent. I know what the uses were. It's consistent with Mound's work with other tritides, uranium and lithium primarily, that were used for other purposes.

I have seen no evidence whatsoever to indicate that hafnium tritide was more extensive. If you feel that such evidence exists, I would love to see it, and maybe we can discuss that under the appropriate circumstances.

MR. FITZGERALD: Well, I think we can talk in general terms. Kathy is trying to prime me here, but essentially we did find certainly some indications in Appendix B of the [identifying information redacted] report that hafnium by name existed in a number of named and numbered rooms, other than the ones that were cited in your original assessment.

That was a starting point, but not necessarily the basis for our conclusion at this point.

I call it reconnaissance survey at OSTI essentially because I think as we understood your point, the so-called confined, discrete nature of hafnium operations, what we want to do is go beyond. We recognize the expertise of the three individuals who talked to you. That's not in question, but we wanted to see if there's any corroboration on paper, meaning, you know, what can we find in records that would substantiate that conclusion.

And so my visit to OSTI was essentially in response to maybe some concerns that despite the expertise of the individuals we're hoping for some corroboration in reports of documentation.

Also on the classified database, meaning that certainly we looked at the open literature, but we wanted to look at the classified as well because of the nature of these operations much of it would be in that area.

So my visit -- I guess it was August. In August was to see what was there, and we did some search terms, and what we had found on tritides, I think, was essentially as you alluded. You know, square footage, basically in S and RW, you know, just areas that were identified as being devoted to tritide operations, and I don't disagree that it wasn't clear what types of tritides. It just was tritide operations in the many square feet.

And as I recall when I sent the notes into NIOSH, I did not try to draw any conclusions. I just sort of said, well, you know, here's the data. You know, it's too early to know whether the data suggests one thing or another, and I think you actually acknowledged that I did not do that. I did not say one way or the other, except the fact that this was a large area being devoted to tritide operation, much larger than I would have thought, but you know, again, it wasn't clear what types, and it's not clear we could even talk about it anyway.

So anyway, that was sort of the -- you know, again, based on the reconnaissance, you know, you parachute in for a day or so, go through dusty records. That's the best I could come up with.

Now, that's where it's at. I think you made a return visit, which you mentioned to me at Savannah River, and at that time indicated that you had found something that perhaps corroborated better the notion, I think, that you have come up with that this was confined, discrete operation.

And when I checked into seeing whether or not we could be privy to this material because, again, I think it makes the meeting much better if everybody has the same documentation; I think DOE's response -- remember this was right before Christmas -- was, no, actually it didn't appear that any of this material would be available as declassified, what have you.

So, you know, at my request they did make arrangement to go down and actually, you know, view the documentation, you know, your notes and the documentation that you collected, not only the references that I had identified as part of my August review, but I guess, you know, again, new documents that you might have identified in the course of your review, and that's pretty much what I reviewed at OSTI the second time, and you know, my notes are still actually in declassification, too. So in a sense, for this meeting the best I could do -- and this, again, is the two or three pager -- was to see what DOE would do in terms of allowing something to be said on this subject based on that review.

So the source of this information is essentially the new documents that you identified in your November review, which I think pointed to certain activities that existed at Mound historically that, again, involved things like scrap metal handling, destructive testing, QA programs, and I picked up on storage the first visit, where there was certainly some inventories of hafnium being stored by me, but that, you know, is essentially it.

Again, this is a thin read, but it certainly raises some questions about how discrete and how confined as a hypothesis, whether or not it's true. And I can almost see where if you had three experts that were, you know, sort of handling the fabrication side of the house, focused on that particular operation where it wouldn't necessarily know about other workers in other places and other times that were going on.

You know, if you think of the evolution, this is the front end. It wouldn't really be familiar with those who were complementing the activities at the back end of a cycle, you know, that took place at

Mound.

DR. ULSH: That's not an accurate characterization of the three workers that we interviewed. They were involved in radiation protection in terms of the tritium program. So they didn't have just a limited perspective in terms of their particular discrete part of the operation. They were involved, like I said; they were responsible for radiation protection for that program.

All I can tell you is that the account that was given to us by those three workers was corroborated by everything that I reviewed at OSTI, which included everything that was cited in your report or your notes, plus other documents that were referenced in there that we pulled.

I could give you a specific reference that is especially helpful. I'll do that off line if you'd like.

MR. FITZGERALD: Okay..

DR. ULSH: That's the one you need

to look at. It's on microfiche at OSTI. That would be very convenient, I think, for Bob to go look at. I could even inquire at OSTI if they can have a copy sent up to --

MR. FITZGERALD: Actually I've reviewed it.

DR. ULSH: Okay. So what that document does, if we're talking about the same document, it tells the exact month and year that the operations started in terms of renovating facilities to house this operation. I believe it was two rooms. If you look at the size of the lathe that was involved, you had to look at it under a 4X microscope to even see it. So it's not consistent with the thousands of square feet that are in your notes.

MR. FITZGERALD: Well, you know, let me interject. I am not disagreeing at all with what was in the microfiche. I think it describes very accurately the initiation of the program in the 60s and the fact it was in two rooms and sort of discrete work force, everything, no dispute there at all. I think that was very true. It began as a very discrete operation at that point in time.

What I'm saying and what I think is corroborated by these documents is that over the next ten, 20 years -- this actually makes a lot of sense in DOE land -- you know, as you produced whatever you were producing, you had to support QA programs. You had to support recovery programs. You had to support waste management.

So you know, as you go down the life cycle of the thing that you're making, it's the rest of that cycle where you have handling at Mound where additional workers would have been involved, and that's corroborated by the documents that were reviewed at OSTI.

And, again, it does not negate the premise that it started that way, very discrete, very specific, but it does suggest that that wasn't the case as time went on. I think that's pretty much the position that we're taking.

DR. ULSH: I think I understand the operations that you're talking about, and again, I saw nothing that indicated that those operations were not handled by the very same people who worked with the material in the first place, which makes sense. The weight of the evidence, again is the workers who were there and told us, and it also makes sense if you just think about it from a logical perspective. If you've got a highly sensitive, highly secure operation and you take great pains to make it discrete and well classified, you don't want every worker knowing about this; what sense would it make to at some point later in the operation to expand it?

So I know that there's some sensitivity in going further. If you could provide me the documents that you think show a wider operation, I would be happy to go back to OSTI and take a look, but --

CHAIR BEACH: Before you start, Kathy has been patient.

MS. ROBERTSON-DeMERS: So are you saying that all of this operation has to be in one building?

DR. ULSH: I believe, well, no, because they did nuclear magnetic resonance on some samples -- but, again, those were contained. They were in sealed glass vessels. There would be no exposure potential there. That's all I can think of off the top of my head.

MS. ROBERTSON-DeMERS: Okay. I'm just going to talk generically, okay, about Appendix B of the [identifying information redacted] document, which is a characterization by room.

First of all, lithium, uranium, and hafnium are not the only tritides that are listed in that.

Second of all, I believe we found, identified tritides in at least five buildings.

CHAIR BEACH: I was with Kathy when we made the list.

DR. ULSH: Yes, that wouldn't surprise me: tritides.

CHAIR BEACH: No, they were named specifically.

DR. ULSH: Right. You're saying there was hafnium tritide at Mound in five different buildings?

CHAIR BEACH: At least.

MS. ROBERTSON-DeMERS: I'm not going to specify any further. I can tell you off line.

DR. ULSH: All right.

MS. ROBERTSON-DeMERS: But the document does exist, and it seems to contradict your position.

DR. ULSH: I'll reserve judgment until I see the document myself.

MS. ROBERTSON-DeMERS: And I know that you guys have a copy of it.

MR. CHEW: We need to also know the basis of the [identifying information redacted] document, that the [identifying information redacted] document was written, and so to give some people some opportunity making sure when you come back and do D&D, what potentially was put at those facilities. The qualification is that if there was any potential speculation that that tritide might have been in that particular room was mentioned but not confirmed. I think if I remember correctly, [identifying information redacted] did not have a clearance; is that correct?

DR. ULSH: I believe that's correct, and in fact, two of the three workers that we interviewed served as the technical experts with the clearance to assist [identifying information redacted]. So when they say this is it --

CHAIR BEACH: And these are the rooms.

DR. ULSH: Right, I think that carries --

MR. CHEW: I'll just make one more statement then. If you look at the list of the tritides that was mentioned, there was only potentially speculated that maybe even a small quantity might have shown up, but it was not confirmed. That's what I'm saying. I don't recognize it as in the [identifying information redacted] document.

MS. ROBERTSON-DeMERS: It seems to me that we have conflicting sources, and there has to be some resolution

DR. BISTLINE: And I think it needs to be stated that the paper by [identifying information redacted] and [identifying information redacted], which is published by Mound, classifies eight such stable metal tritides, and it gives a list of the eight stable tritides, and we keep coming back and narrowing it down to hafnium tritide, but there are eight that were listed by Mound themselves as stable metal tritides, which raises an issue with me as to why we keep limiting it to one stable tritide when there are at least eight, and some of those were forms of tritides which, as I mentioned were used throughout the complex.

And I'm not going to go any further than that, except to say that these were used at production levels, and I'm not stating anything that hasn't been cleared by headquarters as far as classification of these. I could give you a list of the eight. That's not classified.

DR. ULSH: Without getting into that, I'm aware of other common -- common is a relative term -- other tritides that were used in I guess a significant scale, I would say, of different sites, and they are the ones that were examined by a researcher named Yang from Lovelace, another researcher name Zhou --

it's Chinese. I assume I'm saying it right -- at Lovelace, and they're cited in our report. Since they're now physics literature, I don't think it's going to be less. Things like erbium, things like titanium, things like zirconium, these are the ones that --

MR. FITZGERALD: Scandium, uranium, lithium, these are the ones that are all in the literature.

There were some others, if you recall, I don't know if you were at the meeting, but I know you were.

MR. FITZGERALD: I actually think he was. You were at the Germantown meeting, right? Germantown or Savannah River, yes.

MR. CHEW: Well, I don't think he was at the Germantown meeting though.

DR. BISTLINE: Yes, I was in Germantown.

DR. ULSH: Okay. There were some specific ones you asked us about, and we went back and checked with one of the guys that we interviewed about the scale of the operation. They were listed in [identifying information redacted], and he confirmed for us that they were, to quote loosely, like science-fair type project scale operations, not production operations. We investigated a couple of those.

MR. FITZGERALD: Yes, they were all bench scale operations, right.

DR. ULSH: The articles that we have cited by Zhou, Chang, even SC&A's OTIB-0066 review all say that hafnium tritide is type S, and the workers that we interviewed to a man say that hafnium tritide is the limiting case.

Now, we could speculate. Maybe there's worse ones out there. Fine, but that's speculation. If there's any evidence of that, I would gladly review and entertain it, but I haven't seen it.

DR. LIPSZTEIN: The problem is that there are some tritides that were not studied and there are some forms of that nobody -- you know, there is one paper that says that it's type S. Other papers have classified them as type M.

DR. ULSH: Not hafnium tritide. I have not seen a paper that classified --

DR. LIPSZTEIN: No, no, no. I'm not talking about hafnium tritide. I'm talking about other kinds of --

DR. ULSH: Right. They range anywhere --

DR. LIPSZTEIN: And say that people might be exposed to other kinds of tritides. They would not be classified as type M, and we don't know.

DR. NETON: This is Jim.

I've let this go on for a while. I may want to take this conversation in a slightly different direction, and that is what I thought I heard was the sort of implication that if we were to assign this to a larger group of people, let's say, for instance, it were true that there were these other types of tritides out there that also exhibited type S behavior, and if we didn't know, we would assign the more conservative or claimant-favorable dose.

I'm not sure why that's an issue here. I mean, we've heard that there is no valid bioassay technique for identifying tritide.

DR. ULSH: Can I jump in?

DR. NETON: Yes.

DR. ULSH: This relates to the quote that Kathy gave from the DOE handbook and, I think, some of the other ones that say that tritium bioassay is ineffective for tritide loosely. You have to keep in mind the context of that document. The DOE requirements at the time were you had to be able to detect 100 millirem or less exposures.

It is true that the urinalysis in place at the time were ineffective in detecting doses from hafnium tritide less than

100 millirem. That does not mean -- I mean, all that means is that the missed dose is high. We grant that.

DR. NETON: No, I didn't mean to imply that the bioassay techniques that were used to analyze the tritium were invalid, but there's no way to identify a tritide exposure by looking at the urine. You just can't tell. What comes out in the urine is not useful.

But, secondly, I'm not hearing any fundamental objections to our type S model for vary insoluble forms of tritides. So given that, and I don't know why it would be improper for NIOSH to use type S in cases where we didn't know if in every insoluble tritide exposure occurred to bound the exposure for those workers.

I'm not buying the argument that just because they come up in these very large doses makes it wrong. I mean, it is what it is. If the model is valid and you get a very large lung dose from inhaling a highly insoluble form of a tritide, so be it. But if we don't know, and I'll read right from our regulation, 82.18, Paragraph DB, when NIOSH cannot establish exposure conditions with sufficient specificity, the dose calculation will assume exposure conditions that maximize the dose to the organ under consideration.

And that's exactly what we would do. I see this no different than how we're treating Super S across the DOE complex currently. So I'm not sure, you know, why SC&A seems to be going down this path.

MR. FITZGERALD: Well, I guess it's not a path per se. It's just reacting to certainly the approach where for Super S or type S, the proposal is to focus on a small group of workers that would have been potentially exposed and everybody else is not covered.

So I think we're not proposing any particular approach. We're reacting to --

DR. NETON: Well, I've heard two

issues here. One is that there's a small group of workers, and SC&A is arguing that that's not necessarily true. There's a larger group of workers, and I'm saying if what you're saying is true, why is it not valid for us to assign super or type S to those forms of tritium where we don't know. You can't make a value judgment on the person's exposure conditions.

MS. ROBERTSON-DeMERS: Because the only bioassay that you have was for HTO insoluble forms of tritium.

DR. NETON: But we have already agreed, and I think Joyce would agree that TIB-0066 is a valid way to calculate the lung dose for highly insoluble materials.

DR. ULSH: And that's what comes out in the urinalysis, tritiated water. The tritium comes off the hafnium, enters the body and comes out in the urine. It's created by --

DR. NETON: That's what makes it

type S. It very slowly decouples from the metal form, comes out in the urine, and we're accounting for that by using the very insoluble lung model.

DR. ULSH: And McConville and Woods even described the exact pattern that you see in these insoluble tritides.

DR. MAURO: I think I see where this is going, and it goes like this. Let's say in the process of doing dose reconstruction according to the way you just described it, you find that, well, we really don't have the records or the information that lets us put a boundary around them. Who might have spent the say or two working with -- as I understand it, it doesn't take very much tritide, hafnium tritide, inhaled to deliver a very large dose. We're talking about a 10,000-fold difference between chronic exposure to hafnium tritide and chronic exposure.

So it may turn out that even if a

person only worked a few days with, let's say, a volatile or aerosol, a form of hafnium tritide or stable tritide that could be inhaled. You'd be in this very difficult situation. From a practical sense, one could argue that we really can't rule anybody out. Anybody that worked at Mound that was involved in handling tritium may at some point -- let me go through the line and then show me where it breaks down.

We may have to assign hafnium tritide models to everybody, everybody with a bioassay, except for maybe -- maybe the easiest question is we probably can identify those that certainly were not, and that may turn out to be only a small percentage of the population of workers.

So what then we're confronted with, let's say we go out with that. Okay. Let's agree that, yes, I think we could all agree that this group of people in no way ever came in contact with a stable metal tritide.

But the rest of it, the rest of that group of workers -- and I don't know how many there are -- maybe some time they could have been exposed.

DR. NETON: I would suspect that you would limit the population of people you are monitoring for tritium exposure.

DR. MAURO: We're getting there.

DR. NETON: This one bioassay has the potential to be --

DR. MAURO: Okay. So look. You have this whole population. Then you say there's a subpopulation that has been monitored for tritium, whatever that group is. I don't know if that's 100 people or 10,000 people. Whatever, I'm with you.

Then they say, okay, in that group, one thing. We probably could sit around the table maybe under a classified setting and say we know these people could not have been exposed to that form. Okay?

And let's say we can actually do

that. That leaves the rest. I don't know what that rest is, but now we're at a point -- and that could be a lot of people. I don't know, and they were going to assign the worst case. So we'll be assigning doses to some relatively large number of people that are on the order -- some very high doses to the respiratory tract, and there may be a relatively large number of people.

Which brings me to the end of the story. Do we have a plausibility argument here?

DR. NETON: Why is that any different than Super S where everyone in the complex that was potentially exposed to plutonium in the DOE weapons complex now is essentially getting the Super S plutonium intake exposure, whether that existed at all of those facilities or not.

So I fail to see the difference. I think we're splitting hairs here. It's a large dose, but so is Super S, I mean, and we would definitely limit it to people who were on bioassays or should have been bioassayed.

I don't know. I don't know how it's going to come out. Dose reconstruction is not done in aggregate. We don't take and apply this to everybody and say, there's your dose.

You take the case; you look at the file; you look at the exposure potentials, monitoring history. All those things come into play in a dose reconstruction. So I don't see why this is an issue, whether it's ten people or 100 people or the entire site. I really don't see it.

CHAIR BEACH: I think because Brant was limiting it --

DR. NETON: Well, no, that's not what I'm hearing.

MR. FITZGERALD: What I was pointing out was certainly the proposal that NIOSH has on the table is to distinguish two populations of tritium monitored workers, those that were definitely exposed, potentially exposed to hafnium, and those, the balance, that may or may not have been exposed. Intermediate, that's kind of a yet to be decided issue.

DR. NETON: Let me pose this question then. Is SC&A okay with NIOSH assigning on a claimant-favorable basis type S tritide exposures to --

MR. FITZGERALD: Well, this is certainly a completely new proposal.

DR. ULSH: Whoa, whoa, whoa. Wait a minute, wait a minute.

MR. FITZGERALD: No, no. I mean what was being proposed was certainly --

DR. NETON: If I read the talking notes that were prepared for this meeting that you provided. That was your fundamental argument, was that it was inappropriate to apply Super S tritide exposure to Mound workers because it was an implausibly high dose. That's what I read.

MR. FITZGERALD: The concern was the implausibility question, I think, that John just raised. Is it implausible to assign every tritium exposed worker a factor of 10,000 in terms of the dose?

DR. NETON: I would say we would apply it as we can given, if we can't, we have two equally plausible scenarios or two plausible scenarios, we'll assign the higher dose.

DR. ULSH: But I want to make it clear. I want to go on record and make it clear that we're saying even for the sake of discussion, if everything you say is true and it's greater than these ten, and by the way, I vehemently disagree with that, but for the sake of discussion, even if we applied it to everyone, you can't say on the one hand, it's a bigger group than these ten, and then we say, okay, well, then we could apply it to everybody.

And then you say, no, no, no, but

that's not plausible. You can't have your cake and eat it, too, here.

MR. FITZGERALD: Well, I guess, you know, by its nature it's a policy interpretation question. I think you framed it that way, that, you know, certainly the regs allow you to make that call, and in this case, is the call -- does it present an implausible situation? Is 10,000 times more dose to the lung a plausible condition that would exist in some circumstances?

I think that's the kind of question that you come up against, which is sort of the test, and it is --

DR. NETON: I find that dose is irrelevant. If the model is valid and the dose is 10,000 times higher, that's what it is. I mean, that's off the table.

MR. FITZGERALD: Is that true?

DR. NETON: Well, the magnitude of the dose is irrelevant if you buy that the model is technically accurate. It is what it is. That's the exposure incurred by a person who inhaled an insoluble tritide.

DR. MAURO: What would have to happen for a person to get that dose? There is sort of a dilemma here, and it lies in the regulation, the way the regulation is. We have to live within that regulation. On the one hand, we have a situation. Well, listen. Since we don't really know for sure whether this person was exposed to hafnium tritide or one of the more insoluble tritides.

We have no choice but to assign the worst case assumption, which means that this person would be inhaling the tritium that we're observing in his urine, that week after week after week after week after week in a given year. We're going to assume that that tritium that we're observing in his urine was all due to hafnium tritide.

DR. NETON: If we cannot tell.

DR. MAURO: If you can't tell, and you may very well be in that situation.

DR. NETON: Well, first of all, it would have to be a lung cancer.

DR. MAURO: Yes.

DR. NETON: A small -- not small, but like 20 percent of the cases.

DR. LIPSZTEIN: Jim.

DR. NETON: Yes.

DR. LIPSZTEIN: We were discussing this yesterday with Rich Leggett, and he pointed out to us the very interesting thing is that when you calculated dose using OTIB-0066, you don't take into account the self-absorption of the tritium beta particles within the particle, and these would reduce the dose by around -- he made a very quick calculation, but around one to five percent. Very quickly it was a particle of five microns.

DR. NETON: If it's one to five percent, it's not worth doing.

DR. LIPSZTEIN: No, no. The dose, one to five percent, the only fraction of beta energy that would escape was in the range of one percent. So the dose to the lungs would be 100 times lower.

DR. NETON: And we looked at that, Joyce, and I'd be happy to entertain any calculations that could accurately come up with that value because anything that I've seen is a best guess.

DR. ULSH: No, in fact not.

DR. NETON: There is a paper out there.

DR. ULSH: -- published it and he explicitly calculated the self-absorption factor.

DR. NETON: No.

DR. MAURO: Along the order that Joyce is saying.

DR. LIPSZTEIN: Yes.

DR. NETON: But we looked at that early on, and I can't remember why. I made the decision to discount that correction, and there must have been a good reason for it or otherwise --

(Laughter.)

DR. NETON: I remember dealing with this issue because this issue was brought to the table, and we didn't include it.

But nonetheless, okay. So the doses are --

DR. MAURO: Whether it is ten thousand or a thousand.

DR. NETON: That's a modeling issue.

DR. MAURO: But now, see, I'm going to go back to something that Paul pointed out to me, which I think is important for us to entertain. Now, let's say it turns out when you implement it the way you just described. Let's say there are 300 workers. We're going to end up treating them as if all their exposure was to hafnium tritide, and so every bioassay sample from them that they --

DR. NETON: Only the lung cancers.

DR. MAURO: No, no. I'm with you.

They all --

(Simultaneous speaking.)

DR. MAURO: -- it turns out the other organs as Joyce calculated, it's only a factor too high. In other words, the lung is going to be 1,000 to maybe 10,000 times higher and the other organs would be maybe a factor or two higher.

But let's say we decide to do that. I think that's what --

DR. NETON: They're not the same.

DR. MAURO: Well, I think she said it was -- well, anyway, now I'm going through a line of thought. For that to happen, we know that's impossible because the amount of tritium that moved through the facility compared to the amount of hafnium that -- so but Paul made a very good point when we were talking about thorium. The question becomes that person, that person. Is it plausible that that person, in theory, could have spent two, three years full time -- that's all they worked with, nothing else -- hafnium tritide and therefore, every -- now, it comes to the plausibility issue. Now, if that's plausible, that is, we have a person that says, well, listen. It's plausible that that guy could have worked with highly insoluble forms of tritium his entire time he worked at that facility.

And if the answer to that is yes, then it becomes plausible. If it turns out it's really not plausible, but the reality is that when you work at Mound, only an extremely small fraction of the time are you actually working with hafnium tritide. The rest of the time you're working with something else.

So I think this all does come down to a plausibility question, notwithstanding the fact that Brant, you know, may want to resist that and say no, we could really define the population. Because I could see the population growing, the number of workers, to the point where are we now in the realm where it's now considered no longer plausible, and I'm hearing that the interpretation of the regulations -- and we don't interpret regulations. I know that --

(Laughter.)

DR. MAURO: -- is, okay, you know, I guess if NIOSH decides that it's all -- we're going to make a very big tent. We have to. We have no choice because of the information you're limited by.

DR. NETON: I'm not saying we've made that decision, but --

MR. KATZ: Before you go on, let me just -- I'm hearing a lot of static on the line, and I'm afraid that maybe the people on the line can't hear. Someone on the line or maybe more than one has their phone off of mute, and we're hearing a lot of static -- and it went away. So maybe that solved the problem.

Thank you. Sorry.

DR. MAURO: I'm done. I guess I'm

saying I've been struggling for a long time with this plausibility issue. You know, when does the assigning some conservative assumption, you know, to cure the lack of information get to the point where it borders on the absurd?

DR. NETON: I think you have a very good point, and quoting Dr. Ziemer or I paraphrase Dr. Ziemer, which is dose reconstruction is for individuals. When you look at that individual case, is it plausible that that particular person that you're looking at worked with hafnium tritide? Yes or no, or can I tell?

I've got two exposure scenarios. One says no; one says yes. The other one gives me a higher lung dose. I'm going with it. I'll go back to Bethlehem Steel right now, the poster child for this.

Every worker who we did a dose reconstruction for at Bethlehem Steel after extensive review by a lot of people received the same dose whether it was the secretary the pipefitter, the parking lot paver or the guy working with uranium himself. So I'm not sure why this argument is now coming into being.

CHAIR BEACH: Well, I think because it was limited to those ten people, and so we found proof that there was more than ten people and maybe we need to go back, sit down and --

MR. FITZGERALD: The situation was different.

DR. NETON: I've heard two issues from us here. That's what's confusing me. One is there's more than ten. It's more than ten, but you can't apply it to everybody, and I'm saying, yes, you can because to find everybody who was plausibly exposed to that scenario.

MEMBER ZIEMER: Well, if it's more than ten and you can't identify who the rest of them are, you've got a problem. It's not unlike what we had, say, at Oak Ridge Hospital where intuitively you say not everybody could have gotten that maximum dose, but you can't tell who it was or who it wasn't, and so is that plausible? Well, on an individual basis it becomes plausible only in the sense that you can't tell from that worker.

DR. NETON: Well, I'd say it was a little different at Oak Ridge Hospital. We didn't know what the maximum dose was.

MEMBER ZIEMER: All right. That's a different thing in that sense, but it's still the issue of you're treating everybody the same because you can't define what the restrictions were.

You know, if there are some other places that you agree if Brant said, yes, okay. Here's another spot and there's 20 more people, that's not an issue per se. It's just a matter of -- on the other hand, if there's evidence that it's all over the place and we can't tell who, that's a completely different issue, I think.

It would be awfully surprising, based on what I'm hearing about this material and its use, that it was just all over the place for anybody to work with.

DR. ULSH: That would be completely contrary to everything that we've seen.

MEMBER SCHOFIELD: The other side of that is I would be willing to wager that if you look at that, you may have ten people who are, quote, assigned as tritium workers, but then you have crafts, guards, you name it, come through there, and they're not going to be on a bioassay necessarily for tritium. They may even be --

DR. ULSH: They probably will be on a bioassay for tritium. You cannot go into these buildings without being on a bioassay for tritium. They will be.

MEMBER SCHOFIELD: At Mound?

DR. ULSH: Absolutely.

MEMBER SCHOFIELD: Okay. I would

disagree with you at other facilities.

DR. ULSH: I'm just saying Mound.

MEMBER SCHOFIELD: Maybe at Mound. Mound might be a different horse.

DR. ULSH: You don't just wander into these double security-padlocked rooms or even the other tritide areas that you've described unless you're --

MEMBER SCHOFIELD: These people may have been escorted in there a lot of times, too, to do a job. They weren't told what was necessarily they were working with. They were in there to do a job, get the job done, and get out of there.

DR. ULSH: If you visited that building, you peed in a bottle before you went in and after. I mean, well, definitely after. You definitely left one on the way out.

MR. FITZGERALD: We reviewed that. I think we're pretty secure on that issue.

Maybe what should have -- you know, again, this was framed up in a way which should have been a question of the plausibility. Of course, the interpretation of plausibility for the regs is not -- yes, I think we posited that thing because we didn't see anywhere you could go if you could define.

We saw the intent of defining the population, but if the population could not be defined, then, you know, the question was is there a plausible way to go.

DR. NETON: Either way we can do it.

MR. FITZGERALD: Yes.

DR. NETON: It's either ten or it's more, and if it's more, we're going to be claimant-favorable.

DR. BISTLINE: How about the waste from that facility and D&D of the facility? Were other people exposed? I don't know. I'm just asking.

DR. ULSH: We asked that question specifically of the workers that we interviewed, and due to security concerns, they didn't want anyone to even know that that stuff was there. Due to security concerns, the lab people, meaning the people on the list of ten that we were given, cleaned up their own labs. Well, in terms of the equipment that was used in this operation, they cleaned it down to clean standards because you don't want, if you've got highly secret materials, you don't want to release free release equipment that has that material in it. You want to protect the confidentiality of it.

So if you read the notes from the interview that we provided, we asked specifically what about D&D workers; what about people climbing around in the rafters, and they all three of them answered in the negative. That wouldn't be a significant exposure potential.

Now, if you read some Mound documents, in particular, related to tritides during the D&D era, they were concerned about being able to detect. They didn't have a bioassay in place, as Kathy indicated in that quote that she gave earlier.

DOE requires you to have a bioassay method in place to be able to detect doses as low as 100 millirem. And, indeed, they did not. That was the source of a lot of heartburn with relation to this material.

DR. NETON: So it sounds to me that there is some work to be done in the background related to these other documents that indicate that this material could be elsewhere.

DR. BISTLINE: How about the other tritides that are stable tritides, not intermediate tritides, but considered stable tritides besides --

DR. NETON: It would default to the class that would give the highest dose to the organ being constructed just like I read out of our regulation.

DR. ULSH: Well, and again, I go back to --

DR. NETON: There's Type M or --

DR. ULSH: Right. You have to also not only consider the solubility in isolation, but the exposure potential, and that's why we specifically looked at the ones that we were asked to look at in the Germantown meeting in terms of what was the exposure potential, how extensive the scale was, and to a man all three of them said hafnium tritide was the worst. If you look at the articles from Zhou and Chang, they clearly indicate that hafnium is the worst, and again, I go back to what I said earlier. If there's a worst one out there, I can't prove a negative. I can't prove --

DR. BISTLINE: No, I'm not saying worst, but I'm saying that's close to that, scandium, iron oxide, et cetera --

DR. ULSH: Nothing that I've seen. Everything that I've seen indicates that hafnium tritide is in a class by itself, and you have to keep in mind what the purpose of working with these compounds was.

DR. BISTLINE: Oh, yes.

DR. ULSH: It was to tightly bind tritium. You didn't want this stuff floating off.

DR. BISTLINE: Well, but there were others that were used for the same purpose.

DR. ULSH: Well, exactly. I mean, uranium tritide is --

DR. BISTLINE: No, I'm talking about other ones that are classified as stable tritides.

DR. ULSH: Okay. Well, obviously I'm not going to go into that.

DR. BISTLINE: Okay.

MS. ROBERTSON-DeMERS: Bob mentioned that Mound had eight stable metal tritides that they identified. Just for your benefit, why we brought up the fusion is two of them are byproducts of handling a lot of tritium gas and HTO, and that's where it comes into play in D&D and in evasive maintenance operations and stuff.

And there's also insoluble organically bound tritium that exists and is produced through diffusion of tritium.

MEMBER CLAWSON: Well, I guess I go back to what we got into in Savannah River, which reverts even up to Mound, and that is that these people may have been escorted into these facilities and so forth, but any of the maintenance on any kind of pumps, any kind of -- how it dispersed through everything, and you're telling me that all of this stuff was taken care of by these people. I'll bet you any of these pumps or oil or anything else like that went out, I think you'd be pretty hard pressed.

You can say that, but I'll bet you're pretty hard pressed.

DR. ULSH: Well, certainly there is an issue of organically bound tritium in pump oils. I wouldn't dispute that. I would also not represent to you -- in fact, I made this mistake in Germantown and Bob called me on it -- I won't tell you that these people cleaned their own trash cans. They didn't. They were escorted in. They were on tritium bioassay, but hafnium tritide was always handled doubly contained.

They did have two incidents that I'm aware of where you would call it a spill, two discrete incidents that we know about. The doses from those have been estimated. So I don't think that that's an SEC issue, but under normal operating circumstances that you're describing, Brant, day to day operations where people come in to do maintenance, to clean the floors, whatever, first of all, they didn't handle this material outside of a containment environment under normal operating circumstances, and certainly, number one, you would never just take it out and work on a bench and, number two, even if you did, you wouldn't then open the door and let some guy in to change the trash.

MEMBER CLAWSON: Well, you've got different people going in there though. This is what I'm trying to say. You'd have instrument techs. You'd have everything else like that.

The way I do right now, we bring lots of people into our area there, but they don't know what we're doing and they don't need to. They've got to change. They've got to change a parameter or they've got to change something on our --

DR. NETON: So they have a need to know.

MEMBER CLAWSON: Yes.

MS. ROBERTSON-DeMERS: Can I make a clarification here? There are really two sets of people here. There's the individuals who produced material on purpose.

MEMBER CLAWSON: Right.

MS. ROBERTSON-DeMERS: Then there's the individuals who are potentially exposed to tritides since tritium gas and HTO diffuse into material, and as D&D and intrusive maintenance came, this was re-suspended. Okay? So this side is non-production. This side was not directly involved with processing of a product.

DR. ULSH: Okay. I don't necessarily disagree. I think we have to differentiate between hafnium tritide and other tritides. No one, I would contend to you, was accidentally making or exposed to hafnium tritide that way. You can envision other tritides, for instance, iron oxide, rust, that kind of thing, and that's exactly why we're saying with the exception of this group of ten people that we know were exposed to hafnium tritide, for everyone else on the tritium bioassay program at Mound, we're assuming that they could have possibly been exposed to some of these intermediate tritides, intermediate compounds, intermediate solubility tritides like the kinds you are describing.

MS. ROBERTSON-DeMERS: But where is the solubility study on iron oxide tritide? Because I haven't been able to locate one, and like I said, Mound calls it a stable metal tritide.

DR. ULSH: It is specifically addressed. I specifically asked about rust in terms of in the interview that we did with three workers, and they all to a man told me that it is less soluble than hafnium tritide. It is certainly delimiting or more soluble.

I mean, keep in mind what we're going to do with this. You've got on the one end of the spectrum hafnium tritide. On the other end of the spectrum you've got tritiated water. Hafnium tritide is highly insoluble. Tritiated water is highly soluble. For lung and I would say maybe even respiratory tract organs, hafnium tritide will be the limiting case.

At the other end of the spectrum

every other organ that I can think of tritiated water will be the limiting case. We're getting all balled up in these intermediate things, but these are the two that we're going to use in practice because that's what's claimant favorable.

We are not proposing -- with the exception of the ten people possibly exposed to hafnium tritide, we are not proposing to make any kind of a differentiation for the rest. We're going to say if it's claimant-favorable, we're going to assume hafnium tritide. If it's claimant-favorable, we're going to assume exposure to some of these intermediate solubility tritides, and if it's claimant-favorable, we're going to assume tritiated water. These are the bounding; these are the ends of the spectrum.

I think that the proposed approach that we're putting out there accurately reflects what we know about Mound, and considers the claimant-favorable application to this approach.

DR. LIPSZTEIN: So did I understand correctly if you don't know the solubility of the tritide, you will assume Type S?

DR. ULSH: No. Well, I don't agree with the premise of your question. If we truly don't know the solubility, then assume type S? Sure, but I would say to you that we do know that hafnium tritide is the worst of --

DR. LIPSZTEIN: Yes, because it has even a longer half-time in lung than type S.

DR. ULSH: No, it doesn't.

DR. LIPSZTEIN: -- the ones that were studied but not all of them were studied.

DR. ULSH: Well, again, we're getting into prove a negative. Prove to me that there is not something worse than hafnium. If you've got any evidence to suggest that, I would gladly entertain it.

DR. LIPSZTEIN: I don't know. I don't know because if you don't have literature based on some kinds of tritides, you don't have literature data. You cannot prove anything either way.

DR. ULSH: But, Joyce, we know what tritides were in use and what the scale was, and everyone that has researched this that has at least published results on it and everyone that we interviewed to a person said hafnium tritide is the worst. They didn't say, oh, but then there's this other one that's even worse.

There's no reason to think --

DR. LIPSZTEIN: Well, yes, but those were not studied.

DR. ULSH: I can't prove a negative.

DR. LIPSZTEIN: The ones that have been studied, the worst is hafnium tritide, but there are others that have been studied also and could be type S like carbon tritide, for example.

DR. ULSH: Carbon tritide was not -- it was not in wide use in the complex.

DR. NETON: Well, Joyce, are you saying that there's a potential compound out there that's more insoluble than S?

DR. LIPSZTEIN: I don't know because there are some tritides that were used at Mound from which we don't have papers on it.

DR. ULSH: They were not in wide use at Mound. I can tell you that. The ones that were in wide use at Mound were lithium tritide, uranium tritide. Hafnium tritide wasn't in wide use. It was very discrete. I might be missing one or two, but all of the ones that I've just mentioned have been studied, and they know the solubility is very well determined.

There are some exotic ones that were like I said one-off science fair type experiments, but those don't present an exposure potential.

DR. NETON: Well, I think everything -- we were sort of talking around in circles.

MR. FITZGERALD: Yes, we have a path forward.

DR. NETON: Yes, we have a path forward, I think.

(Laughter.)

DR. NETON: I think in my mind we need to get together with those who are in the know and look at these other potential sources of hafnium tritide exposures at Mound outside of what you believe the universe to be.

DR. ULSH: Joe, if you can --

MR. FITZGERALD: Yes, we need to have another cone of silence looking at these actual references and locations cited, including Appendix B, and just everything.

CHAIR BEACH: Well, and I think they all need to be delivered to one location so everybody can be there at the same time.

MR. FITZGERALD: Well, yes, that's the idea.

DR. NETON: And wherever we agree that that may or may not-- that potential exposure may have occurred, we will use --

MR. FITZGERALD: Let the chips fall where they may on that.

DR. MAURO: So what I'm hearing then it may turn out at the end of the whole process that no one will be assigned tritiated water vapor. I can see you ending up there.

DR. NETON: Not likely, John, because this only applies to lung cancers that we're talking about.

DR. MAURO: No, no. What I'm saying is, yes, the purpose of maximizing those.

DR. LIPSZTEIN: -- the doses to the colon are about 100 times higher if you come to the type S. The doses to the lower intestinal wall --

DR. NETON: GI tract.

DR. LIPSZTEIN: -- are 1,000 times higher.

DR. NETON: Yes, I can agree with GI tract because of the swallowing.

DR. MAURO: But there are other organs where tritiated water is higher than hafnium.

DR. NETON: Well, because it's systemic. You've got to maximize your systemic organ. You're not going to maximize the systemic organs by having an insoluble material in your lungs.

So basically, as Joyce corrected me, it's GI tract plus lung cancers, that subpopulation of those workers who were on a bioassay monitoring program who we can maybe all agree worked in areas where hafnium tritide existed.

I think that's it. I mean, Brant right now says it's ten people and SC&A has some evidence that might speak to some other locations, and we need to look at that. Don't you agree with that, Brant?

DR. ULSH: I agree that I would like to see what papers it is that you feel indicates a wider, and I'll reserve judgment until I see that.

DR. NETON: Well, yes. I'm not saying that we agree that there were locations, but we --

CHAIR BEACH: And Mel would like that to take place at Livermore.

(Laughter.)

MR. FITZGERALD: This time of year I won't argue with you.

CHAIR BEACH: As soon as possible. Okay. We can talk about that probably later.

MR. FITZGERALD: Right.

CHAIR BEACH: So what do you guys think? Do you want to move on to radon or have you had enough for one day?

We can take a break. What's your pleasure?

MR. KATZ: You guys want a ten-minute break? A ten-minute break for folks on the phone, too. So we'll start back up at 20 after four.

(Whereupon, the above-entitled matter went off the record at 4:10 p.m. and resumed at 4:21 p.m.)

MR. KATZ: Okay. We are reconvening after a brief break.

We have just concluded, I believe, discussions of stable tritium compounds on the agenda, and moving on from there.

CHAIR BEACH: And we are going to move on to radon, and this is the last topic of discussion this evening. Let's give me some ideas. Is there anybody that needs to be done by any certain time or are we all good for another hour or so?

MEMBER PRESLEY: It's my nap time.

(Laughter.)

CHAIR BEACH: I was thinking about the local folks that have problems with family. The rest of us are stuck.

Okay. So we'll just finish radon or not finish radon for the next hour or so, and I guess, NIOSH, we're going to throw it into your court.

DR. ULSH: Okay. Radon is one of those issues that has been hanging out there for a while, and I guess we have to go back to the currently established SEC class, and that is 1949 to 59, and that is based on radium separation operations which occurred in the SW cave. That operation commenced in about the mid-50s and was D&Ded in about 1959, and by D&D what I mean is they removed as much of the source term as they could, removed as much of the equipment as they could feasibly remove, and basically they poured concrete on top of the old cave facility.

And let me describe just briefly what the old cave facility was. Basically think of a hot cell, only not quite so confined. The basis of our SEC class was that the documented contamination that spread not only throughout the old cave, but throughout the R and SW Buildings from this very, well, I have to call it a messy operation.

And we determined that we could not reliably say who might have been exposed to this material. People could have traveled in and out of these buildings. So it was everyone on site. That's the currently defined SEC class.

Those operations by and large ceased in 1959 and the full cave was concreted in. When they concreted it in, they made some office space up on top of this facility in Room SW-19, I believe.

And fast forward now about 20 years, and there was a particular individual that went in for a body count and came up with a very strange result, and that led to an investigation, and what they discovered was that there was a tunnel, an access tunnel. Now, this is not a tunnel that people frequented. It was only like two and a half by two and a half approximately, I think.

Yes, Don?

MR. STEWART: Two foot, three inches tall.

DR. ULSH: Okay. Two foot, three inches tall.

Right by this guy's desk in SW-19 there was a square hole cut in the floor for reasons that aren't clear to me, and basically what you had was in this access tunnel, you had radon gas, and when I say radon, I'm using the term loosely. Not just 222, but also thoron and actinon. This is left over contamination from the radium separations operation.

So this tunnel provided pretty much an ideal environment to build up extremely high concentrations of the three radon isotopes, and then, of course, it came out in this hole right by this guy's desk.

Now, I'm not going to mention the guy by name for Privacy Act reasons. I can tell you that he's not a claimant. To my knowledge, he doesn't have lung cancer, but so what happened when this guy came up with a high body count, they did an investigation and they discovered this tunnel.

Apparently the investigator at the time, I am inferring that he didn't have historical knowledge of the operations that occurred, and he measured a very low radon-222 concentration, but just screaming-high daughter product concentrations, and it was a mystery to him why there was such a disequilibrium.

Well, of course, it's because you didn't just have a radon-222 source. You had these other radon isotopes as well, and so at the time it didn't occur to him what he was actually observing.

He sampled. They put a person in a bubble suit, went down into the tunnel and sampled, and discovered these very high concentrations. They measured at the outlet, the square hole in the floor, and discovered a high concentration, and then they measured in the breathing zone and discovered a reduction by a factor of ten.

So what I am telling you is I think there are enough uncertainties with that investigation in terms of issues of instrument calibration that were used in terms of possible played out of the material in the Tygon Tubing that was used. He mentioned that. For that guy sitting there, I can't really put a plausible upper bound on his lung dose. The other organs, I think it's going to be trivial. I mean, it's radon. I can't say that it's zero, but it's trivial.

The weight of the evidence to me suggests that, due to the factor-of-ten reduction he saw simply between the outlet and the breathing zone, if you dilute first out further into the room and then out further into the building, I can tell you that it's my best scientific judgment that the doses, the concentrations that you would see are low, but I can't put a number on it. All I can say is it's low.

Given that, I also have to tell you that I know that this guy was sitting here in 1979. I don't know who might have been sitting there prior to that, no idea.

What happened as a result of this, they sealed cracks around that room and sealed off that access tunnel, and then they stacked it. So they vented off the radon that was in there.

So from 1959 to 79, we have an issue certainly for this guy and anyone else who might have been sitting there. We've discussed this matter with DOL in terms of their administration of a class, and it's their input, their view that if we were even to say that it was only this one room, SW-19, they couldn't really administer that because they don't know who frequented that room, who went in, who went out.

I think it's certainly true; at least I have not seen any evidence nor do I have any reason to believe that this would be an issue, well, number one, outside of SW-19, but number two, outside of R and SW Building. That's where this was. R was connected to SW.

And so I can't envision that this source term would present significant exposure potential outside of these two buildings. So given that, what I'm telling you is I can't accurately reconstruct lung dose for this guy or anyone else who might have been sitting there, and nor can I reconstruct or put a plausible upper bound on lung dose for anyone else who spent a significant amount of time in there.

So that's where we are with radon. We struggled with this for weeks and months, and basically we don't have a solution for that.

CHAIR BEACH: I'm sorry to make you, but can you give me the exact years again and the room?

DR. ULSH: The current SEC period ended in 1959. This issue was discovered in 1979.

CHAIR BEACH: Okay.

DR. ULSH: The room number involved, I believe, is SW-19. Don, can you?

MR. STEWART: That's correct. SW-19 was actually built over the old cave area, and due to plate drain tiles there was significant amount of that processed material that had diffused through the soil. So we've got an ongoing generation of all three isotopes of radon.

MR. FITZGERALD: And the remedial action, wasn't that in 1980? These measurements were 79, I thought. I thought the remedial action was 80.

MR. STEWART: Yes, I believe that was 1980.

DR. ULSH: Yes, it might have been the end of 79 where they were investigating again back in '80. That's possible.

DR. NETON: It's bad, but what Brant said is exactly right. This is really not trivial levels of radon. I mean, hundreds of thousands of picocuries per liter in this tunnel and more importantly, to back-calculate 20 years for the thoron exposure, they actually started with a thorium-228 source term. I thought it was originally thorium-232, but it was thorium-227. That has something like a couple of year half-life. So you back-calculate those concentrations 20 years and you have some enormous potential amounts of thoron gas in that tunnel, and it ended up with some plausibly high doses.

MR. STEWART: An example, you can illustrate what Jim is saying. We've calculated an ET-1 dose from thoron, almost 18,000 rem in the year 1959.

DR. NETON: That would have been our best estimate, and who knows? Given all of the compounding factors that Brant mentioned, sort of the lack of understanding of really what was in that tunnel at the time led to some very confused scientists for a while. There are about eight pages or so of handwritten notes that are kind of interesting to read in the SRDB on this.

MR. FITZGERALD: Now -- oh, go ahead. I'm sorry.

DR. ULSH: No, go ahead.

MR. FITZGERALD: Well, a couple of other considerations. We've talked about this before. When we did the site profile review, it seems like eons ago now, but you know, we interviewed folks on this issue, and the question of where this tunnel underlaid -- is that the right word -- was underneath, you know, and the R and SW Buildings are contiguous buildings. They were, you know, built together. They weren't separate, and the tunnel, you know, was not only under -- well, certainly the capped old cave was under SW-19, but the tunnel itself was under several other rooms or whatever in SW and may have extended to R, although I think -- I'm not sure if we heard definitive answers to what the length of the tunnel was.

But the one thing that sticks in my mind was the interview we had with rad techs, who sort of similar to what [identifying information redacted] -- similar to this individual who monitored in SW-19, also volunteered that in terms of the alpha monitors over tracks in our building and actually R-218 was the room that specified in the interview notes. They, too, pegged out, and their response was that, yes, they were getting a lot of influx of what they thought was radon.

Now, they didn't do an analysis to pin down the constituents, but, again, it was a very high influx of gaseous alpha emitting, which they considered radon. And so to sort of compound the situation, it's not clear, you know. This was certainly a major source, a major opening, if you may, but it's not clear from the breadth of that tunnel and where it was located where else the R/SW complex was getting its influx of radon, but certainly in that one data point, the rad tech acknowledge that they were seeing a high level radon coming through in R-218.

So it's difficult. There aren't that many measurements, but the ones that do exist suggest the very high level radon and an implication that may have existed wherever this tunnel might have provided that source term.

DR. ULSH: Well, given the input that we've had from DOL that it's not feasible to limit it to just SW-19, that may be an academic question anyway because I think, you know, I can't speak for anyone making SEC classes or anything like that, but one can logically conclude perhaps that DOL would be inclined to make it all of R/SW Building.

CHAIR BEACH: And then possibly

expand the years? Because I know there was some -- the same interview he's talking about, Joe was, they were talking about wells that they found up until the late 80s, and didn't really do a lot of sampling until the 90s for radon.

DR. ULSH: Wells? Are you talking drinking wells?

CHAIR BEACH: No. I'm sorry. Let me find it. You want to go ahead. I'll find it.

DR. ULSH: Okay.

DR. NETON: The other fact was that any gas measurement that was made of the radon detector built to measure gas would give you an inappropriate reading because of the unique mixture of the three radioisotopes. They're calibrated usually to radon-222, which had a certain diffusion consonant across the membrane, and you're measuring radon-219 and radon-220, and so that's what really threw the original measurements off.

They were measuring, I think, several hundred picocuries per liter, but not seeing any daughter activity. They were like, well, what's going on. There's a huge disequilibrium here, and it really was they were measuring these other radon gases.

CHAIR BEACH: Oh, manholes. I'm sorry. Yes, they talked about down in the manholes. That's the same one.

MR. FITZGERALD: That's the same analysis.

CHAIR BEACH: Sorry. I thought it was a separate one.

DR. ULSH: So this may be a short discussion. I don't know.

MR. FITZGERALD: No. I mean, I know where we're coming from. There are certainly uncertainties and the level of radon was such that we thought it was a concern.

CHAIR BEACH: So at this point it's up to Department of Labor to give us --

MR. FITZGERALD: I'll let them

speak to the process.

DR. ULSH: No, we would have to revise our evaluation report to conclude this. We can't bound to an additional class.

MEMBER ZIEMER: Were those buildings themselves restricted? In other words, can you define who had access to those buildings?

DR. ULSH: Yes, they were tritium buildings. Paul, again, we're relying on if you went into those buildings, you --

MEMBER ZIEMER: If there was a record of it, you can tell which workers it would apply to.

DR. ULSH: Well, basically, if you did not have tritium bioassay, you didn't go into those buildings because if you did, you gave tritium bioassay.

CHAIR BEACH: What about maintenance workers?

DR. ULSH: If maintenance workers went into our R/SW Building, they gave tritium bioassay.

MS. ROBERTSON-DeMERS: I can tell you for a fact that I went into many rooms in R building and never submitted a tritium bioassay because I was in R Building.

DR. ULSH: What years?

MS. ROBERTSON-DeMERS: Nineties.

MEMBER ZIEMER: Well, what's the implication of this? Anyone who got tritium bioassay could have been in that building; is that what you're saying?

DR. ULSH: Yes.

MEMBER ZIEMER: And therefore, could have gotten high radon exposure.

DR. NETON: Right. This would be a class, the 250-day requirement.

MEMBER ZIEMER: Yes. I'm just trying to get a feel for who it applies to. That's a fairly large group.

DR. ULSH: I can understand why you might not have had tritium bioassay in the 90s, because of DOE Order whatever it was that was in place at the time.

DR. NETON: Eight thirty-five?

DR. ULSH: Yes, that said, if you didn't have 100 millirem exposure potential, you didn't have to give a bioassay. But we're talking about 1959 to 1979, before that order was in effect.

MR. FITZGERALD: Now, the only -- not to throw a fly in this fine ointment at this point -- you know, the 1980 venting, you know, you've read the same things that I've read. You know, it got much better, but as far as what I would call a decent survey of radon, that didn't happen until, I think, the early 90s. They did a DOE-wide survey, including a lot of these buildings, and one thing that I don't think has been nailed very well, I just sort of accept it on faith based on, you know, the memos I've read after they vented.

They said, well, it went down appreciably, blah, blah, blah, blah, but you didn't see what you would like to have seen, which is some subsequent surveys.

There was one interviewee who I won't name who was a health physicist that, you know, said that, you know, there was just a lot of radon amount, a lot of, you know, I think it was attributed maybe wrongly to a local coal burning plant or inversions, but he said, you know, they have alarms going off a lot.

So I guess the only thing that might be useful to do is the punctuation point in 1980. You know, even though there was a memo or two that said, hurrah, the problem is solved, in 1980, given the source term and the levels involved, you know, we certainly did not find any more documentation, but it would be probably helpful just to firm that up that there wasn't this unsubstantiated radon exposure occurring at levels that would pose a problem after '80.

I think we assume that's the case

because the memo suggested that made a big difference, but I'm not sure what a big difference translates into.

DR. ULSH: Yes, I can't pull the exact dates off the top of my head, Joe, but I do remember that after they remediated, in other words, sealed, you know, the cracks and the holes and everything --

MR. FITZGERALD: The cracks.

DR. ULSH: -- put the stack in, they did go back and re-survey. I don't know what the levels they measured were, but I have the same recollection that you do, that they concluded that, okay, problem fixed.

MR. FITZGERALD: Right. You probably would want to -- you just, given where we are at this point, that would be the only question I'd have.

DR. NETON: You also have that radon cup data that was taken in the 90s that you mentioned, and I don't know how widely distributed those cups were in Mound, but I know at Fernald they were -- what the numbers of the public buildings were. So there might be some SW cup data that was taken in 1990.

MEMBER ZIEMER: But the surveys made after the remediation, they had taken care of the issues on correctly characterizing the equilibrium factors in the mix. I mean, how -- do we have a reliable survey after the remediation that will give you confidence that that's the cutoff?

DR. NETON: Well, I think the radon levels went down dramatically.

MEMBER ZIEMER: Okay.

DR. NETON: The radon levels that were measured themselves were an overstatement of the radon-222 because they were responding to the whole three gases.

MEMBER ZIEMER: Right.

DR. NETON: And so they went down by I forget how much, but it was --

MEMBER ZIEMER: It should be orders of magnitude I would think, yes.

DR. NETON: Order of magnitude. So it was certainly much better.

MR. FITZGERALD: But you know, in terms of the additional dose, was it good enough to not give you a problem? I don't know because we didn't go any further and neither, I think, did Brant. So that would be the only question. If you were going to drive a stake in 1980, that would probably be the only question.

MS. ROBERTSON-DeMERS: I can help you guys out with about 1994 forward in our samples about initial counts and decay counts. So if you needed to get a concentration.

MEMBER ZIEMER: Yes, but you still have 1980 as, I guess, the cutoff point for a proposed class, right? And you're talking about 94.

MS. ROBERTSON-DeMERS: Yes, I'm just eliminating those years.

MR. FITZGERALD: Brant, we have the same recollection, that really what 1980 represented based on the memos we read and the measurements they took, and again, I think the measurements were no less primitive than the original ones they took to find the problem. So the only question is can one have confidence that it wasn't residual issues beyond 80. That's all.

DR. NETON: They eventually did take working level measurements, which was specific for the progeny of radon-222. I hadn't read those in a while.

DR. ULSH: And another piece of information, it doesn't definitively answer the question, but the same person who investigated this incident then became the head of a radon group at Mound, and it was based at Mound, but they studied radon throughout the DOE complex and even other places, too.

MEMBER CLAWSON: He's still doing it.

DR. ULSH: Yes, even from coal

plants.

MEMBER CLAWSON: And he's been interviewed.

DR. ULSH: Well, by both of us.

MR. FITZGERALD: And I think it may be a matter of going back and just making sure that, you know, that's still the truncation point, and that's a clean truncation point. You know, again, I remember it being said that way.

CHAIR BEACH: And I don't know the process on that. Jim, will you come back to us and tell us what you've decided or will it go straight to Labor?

MR. KATZ: No, no, no, it will come to the Board.

CHAIR BEACH: Well, no, I meant -- go ahead.

DR. NETON: See, we have an SEC Evaluation Report on the table, correct? That's what we're discussing here. So we would have to amend the SEC Evaluation Report to include this class. Since the Board hasn't heretofore voted on that, I think we can just issued an amended report, but I guess a decision has to be made as to whether it would be more expeditious to like finish all of the debate on Mound and then issue one report or just issue this amendment to get that on the table and then move forward from there. I don't know how.

MR. HINNEFELD: I think we'll need to have that discussion internally.

CHAIR BEACH: Well, I was just curious about how that all would work, if it would come back to us because I wasn't sure.

MEMBER ZIEMER: Could you clarify for me, Brant? At some point this survey was done. That was when they discovered the tunnel or when was the initial survey that led to the concern?

DR. ULSH: That happened in 1979 when this particular individual went for a whole body count.

MEMBER ZIEMER: Okay. It's sort of like the watchers thing.

DR. ULSH: Exactly.

MEMBER ZIEMER: Where did it come from?

Okay. So they went back and surveyed the room, but you indicated an inadequacy in that survey and some uncertainties, but were those uncertainties so bad that you can't use that data? Because we're dealing with uncertainties all the time.

It's more than just the characterization of the levels at that point because who knows what they were for 20 years as part of that, I suppose.

DR. NETON: That's part of it. First you have basically one measurement or a series of measurements taken over a very short period of time in 1979. That's the only thing you had to hang your hat on, and then there's technical issues with the relative contribution of three different isotopes of radon. They went into the tunnel as Brant said and pulled a sample, but they had to pull it through this Tygon Tubing.

So trying to come up with this equilibrium, the equilibrium of the different radon isotopes, and there's documentation that says, well, we probably had some wall losses in tubing and we don't know. It was pretty hard to decipher.

I looked at this thing for --

MEMBER ZIEMER: Well, I was trying to get a feel. I mean, those kind of problems we do deal with.

DR. NETON: We do.

MEMBER ZIEMER: But it's more than just that. It's the fact that you have 20 years. We know radon values bounce all over the place by orders of magnitude.

DR. NETON: Exactly.

MEMBER ZIEMER: You'd like to have it extended through the -- actually you'd like a whole integrated year and so on, but I'm sort of pressing. You guys are sufficiently convinced that you can't bound what the doses would have been, you know, integrated over a year based on samples and knowing how radon -- I mean, there's all kinds of data in the literature about how radon fluctuates, and you could take the worst kind of fluctuation.

I'm just trying for us to make sure that we --

DR. ULSH: There are a lot of uncertainties associated with the measurements that were taken in 1979 which was described.

MEMBER ZIEMER: Right.

DR. ULSH: But even if you took them at face value and said, these are golden. These are great, the problem is -- and even this researcher admitted this or investigator -- that for that particular guy sitting at that desk it would be difficult to estimate, put an upper bound on his lung dose because we don't know how long he was exposed to it.

And like Jim said, if you go back

20 years and decay correct for what was it, thoron? I mean, you get and I think Don said like 18,000 rem.

MR. STEWART: Yes, that's to the --

DR. ULSH: I think we might have a lot of spirited discussions about what sufficiently accurate is and what a plausible upper bound is, but I think that that clearly exceeds it.

DR. NETON: You've got this sort of age old issue with radon. Is the building ventilation rates over time going back 20 years and was that hole there recently or was there other holes and bigger holes.

Given this 100,000 picocuries per liter source term directly underneath this room or this building, we found that it's just too uncertain to be able to assign not only the dose of this particular individual, but anyone else that frequented that office or other area, where there might have been holes.

It's hard to say.

On face value, the original measure might think -- the highest I saw was 200 picocuries per liter, but there's an uncertain mixture of the three different isotopes, and I'm not even sure that 200 was valid because it was sort of a EPERM type detector. It was calibrated for radon-222. So how much radon-219 could have been there, that's what caused the original alarm in his mind, was that 200 picocuries but I'm seeing no progeny for radon-222, almost none. So where is this gas coming from?

Well, we've got some sort of unquantified concentration of thoron and actinon in there. So it gets to be quite a messy problem.

DR. ULSH: And the frustrating thing is that we're really talking about lung and respiratory tract doses primarily. We're talking about a guy who's not even a claimant at this point in time, and furthermore, even if he was, plutonium missed dose would probably be sufficient to compensate him, but there's no remedy available to us for that, to address those things.

MEMBER ZIEMER: Were there other measurements made in the rest of the building at that time? I mean, you're sort of assuming.

DR. ULSH: If there were, I'm not aware of them, but I can't say there weren't.

DR. NETON: Well, we're not assuming is there's a problem throughout the entire building. I think originally, as Brant said, in discussions with Department of Labor, if it was even only that one room, they wouldn't be able to administer a class based on a one-room class definition.

MEMBER ZIEMER: Well, I mean --

DR. NETON: I mean, you could define it that way, but --

MEMBER ZIEMER: You could define it that way and say you have to be in that room 250 days a year.

DR. NETON: That's true, yes.

MEMBER ZIEMER: I mean, someone casually going into an area like that, it's --

DR. ULSH: It's another point of frustration.

MEMBER ZIEMER: I know.

MEMBER CLAWSON: You said this was a hole they cut in it? I understood it was a crack in the floor.

DR. ULSH: Don, do you have any insight on that? I thought it was a square hole.

MR. STEWART: I don't remember whether it was --

MEMBER CLAWSON: Well, in the interview that we had with this individual, he said that the person came back with this and so they went in there to check this, and it was an actual crack in the concrete, was what I remember.

DR. ULSH: Could have been.

MEMBER CLAWSON: And he put it down that this is where he got all of this from. We asked him the same thing about his instrumentation and stuff. He says, you know, this was very crude. We're just starting out everything else like that.

When you were saying this hole, that's what I was wondering because as one of the things about these facilities that I understood, it was that they were kind of stacked one on top of another, kind of -- how could I politely put this? -- they were kind of of put together in a hasty way.

The issue that came up was that there was lots -- the way that the facility was built, that there ended up to be several cracks, and this is what pushed them into actually opening up the end of that and putting the tunnel on there, because it made a perfect breeding ground for radon and everything else like that.

DR. ULSH: Could very well be,

Brad. I can't tell you if it was a square hole why it was there. It could have been a crack. I don't know.

I do remember when they discussed the remediation that they talked about ceilings and cracks. So maybe that's the case.

MEMBER CLAWSON: Right. Well, this pushed them over the edge, and there were several other cracks, but what they wanted to do was get to the root of the problem. What was it? And that's when they found the passageway underneath it, and they had sealed off both ends.

So by venting it, you're -- you know, years earlier they had vented it, and that took care of the issue of being able to move that, plus sealing up the holes.

CHAIR BEACH: Okay. Is there any other discussion? There's nothing really for us to do at this point for radon.

Tomorrow I'd like to propose that

we move data adequacy and completeness to the first thing in the morning, unless there is some reason to start with high-fired based on other schedules.

DR. ULSH: I don't have an issue with it.

CHAIR BEACH: So we'll just start with the last one first thing in the morning, 9:30.

MR. KATZ: Okay. Are we adjourned?

CHAIR BEACH: We're adjourned.

(Whereupon, the above-entitled matter went off the record at 4:52 p.m.)